

# Zoologischer Anzeiger

herausgegeben

von Prof. J. Victor Carus in Leipzig.

Zugleich

Organ der Deutschen Zoologischen Gesellschaft.

Verlag von Wilhelm Engelmann in Leipzig.

**XXII. Band.**

**3. Juli 1899.**

**No. 591.**

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## I. Wissenschaftliche Mittheilungen.

### 1. J. Beard on the Sexual Phases of *Myzostoma*.

By William Morton Wheeler.

eingeg. 12. Mai 1899.

The 13th volume of the Naples »Mittheilungen« contains an interesting rejoinder<sup>1</sup> to my paper on the sexual phases of *Myzostoma* published in the 12th volume of the same periodical<sup>2</sup>. Since this renewed attack contains nothing essentially different from Beard's former note<sup>3</sup>, which, I still believe was condignly treated in my paper, I should not feel bound to consider it, were it not that my work has been garbled and misrepresented by Beard. Further justification for replying, if any were needed, might be sought in the consideration that continued controversy may induce some conscientious student who has an opportunity of working at the Naples Station or at the French or Japanese sea-side laboratories to undertake a renewed study of the reproductive organs of the various species of *Myzostoma*.

<sup>1</sup> J. Beard, The Sexual Conditions of *Myzostoma glabrum* (F. S. Leuckart). Mitth. a. d. Zool. Stat. z. Neapel, 13. Bd. 1898. p. 293—324. Taf. 10.

<sup>2</sup> W. M. Wheeler, The Sexual Phases of *Myzostoma*. Ibid. 12. Bd. 1896. p. 227—302. Taf. 10—12.

<sup>3</sup> John Beard, The Nature of the Hermaphroditism of *Myzostoma*. Zool. Anzeig. 17. Jahrg. 1894. p. 399—404.

The matter in dispute may be very briefly stated. Beard holds that *M. glabrum* is dimorphic, the species being represented by hermaphrodite individuals and by dwarf "complemental males". The latter are "dorsicolous", i.e. they are attached to the dorsal surface of the large hermaphrodite individuals which in turn adhere to the peristome of *Antedon rosacea*. From a comparative study of several species representing the morphological extremes of the genus *Myzostoma* I concluded that *M. glabrum* is monomorphic, each individual of the species being from the first hermaphrodite, i.e. possessing both ovaries and testes, and being like other members of the genus (notably *M. cirri-ferum* and *M. alatum*) protandric, then hermaphrodite and ultimately more or less hysterogynic. In other words the functional male phase (Beard's "complemental male") passes into the functional hermaphrodite phase as soon as the first ova mature, and the functional female phase begins with the atrophy or disappearance of the testes<sup>4</sup>. The cysticolous and entoparasitic species of the genus tend towards a condition in which the functional male and female phases overlap but little, thus exhibiting only a brief functional hermaphrodite phase (*M. eremita*), or these phases no longer overlap and thus present two well-marked periods of sexual maturity, one male and the other female (*M. pulvinar*). This I designated and still designate as "a simpler—and I trust, also—a more satisfactory explanation of the sexual peculiarities of *Myzostoma*" than has been offered by Beard or any other author.

Beard begins his paper with an attempt to reply to Friedtjof Nansen, who was the first to suspect that the "complemental males" were not what Beard had represented them to be<sup>5</sup>. Nansen's defence need not be taken up by me. That gentleman is quite able to defend himself—since events have shown that he is undaunted in the face of difficulties far greater than those presented by the sexual phases of a little parasite like *Myzostoma*. Then, too, my own defence virtually includes that of Nansen, whom Beard supposes to have been restrained from going to such extremes as myself by certain "hard facts of the anatomy of the complemental males". The consistency of these adamantine "facts" may be tested in the course of this paper.

In proof of the assertion that Beard has garbled my statements many passages in his paper could be cited. I select the following good example. Singling out one of my figures, fig. 56, he proceeds to charge upon it thus (p. 308): "From the largest of the dorsicolous forms he

<sup>4</sup> The female phase has not yet been seen in *M. alatum*, but very probably occurs.

<sup>5</sup> Bidrag til Myzostomernes Anatomi og Histologi. Bergen, 1885. 9. pls. With English résumé.

figures in fig. 56 what is apparently an egg, lying in one of the 'ovaries'. From my own examination of dorsicolous specimens it can be stated that search has been long and laboriously made for any such appearance. One swallow does not make a summer, and the picture of one egg, even if there be no mistake as to the dorsicolous nature of the form from which it was taken, does not make an hermaphrodite." And again at p. 309: "In fig. 56 he figures a large egg, lying on the outer side of one of these structures", and on p. 241 he remarks: — "specimens in this stage (6a, 1 mm long) are found, like those of the five preceding stages, attached to the backs of older individuals . . . In some of the specimens a few of the oocytes have begun their growth while still in the ovary (compare pl. 12. fig. 56 o)."

"In this passage and in the description of the plate it is not distinctly stated, that this figure is taken from a true dorsicolous specimen of *M. glabrum*. The reader is left to infer that this was really the case and the author leaves a loop-hole to doubt, where the observation, which of all others would decide the matter, is concerned. A precise statement as to the place, from which the specimen was taken, as to the number of such apparently hermaphrodite dorsicolous forms in the author's possession, and as to the number of obvious eggs in each of these, might have settled the point for good. I would submit that Wheeler may have been mistaken in referring this figure to a true dorsicolous specimen of *M. glabrum*. It may be suspected that it was taken from the side wall of a hermaphrodite, and, as I recognized in 1884, such specimens are never true males, but always contain eggs, as well as spermatozoa," etc. This unfortunate fig. 56 again annoys Beard on p. 313, but by the time he reaches p. 315 he has made up his mind concerning it: "I hold, until the contrary has been proved, that the specimen of fig. 56 was, in all probability, not a true dorsicole, but a young disc-form, which in early life had chanced to attach itself to the side wall of a larger hermaphrodite."

After reading these tirades one turns to the incriminated fig. 56 to find that it has nothing whatever to do with the question at issue! Had Beard taken the pains to read my paper he would have found that fig. 56 is adequately described in the text on p. 256 and that it was merely introduced into the plate to show the appearance of a couple of amoeboid parasites in the ovary of a large *M. glabrum* distended "with young and nearly full grown ova!" Incidentally a couple of young oocytes, which Beard overlooks, notwithstanding the *o* of the reference refers to them rather than to the very large ovum, are mentioned in connection with the description

of the ovary of the dorsicole (1 mm long). Hence the reference was introduced with the word "compare".

Besides presenting the spectacle of a tilt with a wind-mill, the above quoted passages with Beard's interpretation of my fig. 56, are replete with information which they were never intended to convey. First they show that Beard either did not take the pains to read my account of the triplet ovarian cells and their migration—since recognized and confirmed by Prouho<sup>6</sup>—or that he has purposely ignored this most vital portion of my paper. In either case he has egregiously misrepresented the actual conditions. Second, his interpretation shows that he is inadequately acquainted with *Myzostome* material, or he never could have postulated the existence of such an egg as the large one in fig. 56 in a young side-wall hermaphrodite, nor have supposed that I could regard it as a young oocyte. And third, we have a fair sample of the facile and superficial explanation which runs riot through more than one page of Beard's paper.

Even the admission that my reference was slightly obscure does not excuse Beard, for I was justified in not extending my account and introducing more figures of *M. glabrum* because this species is in all essential morphological characters like *M. cirriferum* which had been previously considered at some length. But that there might be no doubt concerning the agreement between the two species, I introduced a frontal section (fig. 24) which Beard does not deign to notice. It is true that it happens to be taken from a discicole 1,25 mm long, but it shows, nevertheless, conditions of the very same nature as those represented for *M. cirriferum*, viz. the origin, migration, fixation and incipient growth of the oocytes with their attendant "Nährzellen". This fig. 24 which covers the conditions described for my stages 6a and 6b might have been studied with profit by Beard. It would have shown him that the peritoneal epithelium—and this is true even in far earlier stages—is much flattened as I have represented it and not cuboidal or of the impossible shape shown in his figures. Beard certainly makes strong claims when he asks us to believe that these figures demonstrate the origin of the oocytes and "sperm-mother cells" from the general peritoneal epithelium, for the lithographer's art was never more vainly employed.

The nine stages of *M. glabrum* described by me were selected from a large amount of material, several hundred specimens, in fact, of very different sizes ranging from 3—4 mm. My sectioned material of stages 6a (dorsicoles), 6b and 7 (discicoles) are all in the neighbor-

<sup>6</sup> Henri Prouho, Dioïcité et Hermaphroditisme chez les Myzostomes. Zoolog. Anzeig. 1895. No. 486. p. 4.



hood of 1 mm in length. They present essentially the same conditions, whether dorsicoles or discicoles, excepting in the extent of the body cavity and the migration of the oocytes. But there are among my material a few discicoles about 1 mm long in which the oocytes have not yet left the ovaries (these must be "complemental males" according to Beard!) while in several dorsicoles of about the same size the migration is well under way through the somewhat more extensive body cavity. Indeed, no hard and fast line can be drawn either in internal or in external structure between the larger dorsicoles and the smaller discicoles. The only morphological distinction which Beard is able to find is the unimportant and by no means constant greater gibbosity of the dorsicole<sup>7</sup>.

The admission that the ovaries ("Nansen's organs") are really the ovaries of *Myzostoma* would at once lignify Beard's "hard facts". It pleases him, therefore, while admitting that there are "no reasons for terming them rudimentary" to seek refuge in the assertion (p. 319) that they "are probably the original sexual organs, which existed prior to the adoption of the parasitic mode of life. They still function, but not as the sole sexual organs. In the hermaphrodite they represent ovaries, in the males testes" (sic!). These remarks, like many others in Beard's paper, will be appreciated at their true value by any one familiar with vermian morphology. The value of his opinion on matters testicular is well shown by his figures of what he calls "sperm-mother cells" (figs. 11, 13 and 15; each cell (?) containing four little dots!), and his worthless fig. 14 shows the extent of his analysis of the ovaries, even if this were not apparent from the magnification.

Beard complains of my remarks concerning his much-vaunted table based on old and (as his figures show) wretchedly preserved material. Since it is Beard and not myself who is unable to recognize young oocytes (he failed to see them even in my fig. 56!) one may judge of the truth of his remark "no eggs" opposite the list of dorsicoles on p. 295. Since, moreover, ovaries full of oogonia or young oocytes are present in all these forms, eggs must be present, and the animals cannot be males "complementary" or otherwise. As to his trivial insistence on the overlapping of the stages indicated in his table, I may state that I am still of my former opinion on such methods of observing and reasoning. His statements in regard to uniformity of contraction are false as proved by

<sup>7</sup> My figs. 19 and 25 show that this difference did not escape me, but it was recognized as inconstant and therefore omitted from my paper.

a glance at my material killed and fixed in a dozen different ways. Specimens show a tendency to curl or contract, especially in the direction of the median longitudinal axis, and this curling and contraction varies greatly in amount and may be present or entirely absent in specimens of all ages, whether dorsicoles or discicoles, large or small, carried through the fluids together. In large specimens measuring 4 mm in life, I find that this amount of inconstant folding or contraction may shorten the longitudinal axis (for cross-sections!) as much as 2 mm in exceptional cases! Even when the dorsicoles are hardened without being detached their free edges may show a variable folding, which makes the counting of sections of a limited number of specimens like those in Beard's table, especially the counting of sections made 15 years ago, a very flimsy bolster for the assumption of a "complemental male". In answer to Beard's remark that it was I who was guilty of this method, I may be permitted to state, that my specimens were at least compared and measured before they were embedded and sectioned, that they were in a better state of preservation and far more numerous than Beard's, and above all I did not base my views on a few deceptive measurements but on the carefully studied sequence of certain morphological peculiarities in the reproductive organs. These organs, I had every occasion to know, are quite as variable in *Myzostoma* as they are in other animals<sup>8</sup>. In my opinion Beard's insistence on the importance of the overlapping in his table is about as reasonable as the statement that every lad becomes sexually mature when he is exactly 5 feet 4½ inches high.

In his latest paper Beard displays another peculiarity of method which, emanating from one engaged in teaching comparative embryology (emphasis on "comparative") is no less remarkable than his peculiar use of measurements in a morphological discussion. He writes (p. 315): "I have never felt myself under the least obligation to investigate *M. cirriferum* for I have never made or desired to make any statements about it. If dorsicolous forms had occurred in this species, it would not have been "quietly ignored"<sup>9</sup>. And at p. 308 he says: "I feel under no obligations to investigate this form, for as Prouho shrewdly (sic!) observes, there exists no reason a priori for supposing that what obtains for one species of the genus will hold for all. Wheeler it may be observed, appears to overlook (sic!) one dif-

<sup>8</sup> Even Beard's table shows variation, for one of his discicoles 1,14 mm long is mentioned as having smaller eggs than another discicole only 1,05 mm in length. He even calls attention (p. 305) to the great variation in the color of *M. glabrum*.

<sup>9</sup> No, *M. cirriferum* is only useful in connection with such unwarrantable statements as those on p. 301 and p. 322 bis.

ference in the life history of *M. cirriferum* and that of *M. glabrum*, that in the former what he believes to be the young forms are never dorsicolous like certain small ones in the latter." Now the reader will doubtless infer that Prouho's "shrewd" remark was made in connection with *M. cirriferum*, but it was hardly even as "shrewd" as this, for it was made with reference to *M. alatum*, a form which, as Prouho himself observed, has dorsicoles, that become hermaphrodite while still attached to the larger individuals<sup>10</sup>! And, of course, Beard would not quietly have ignored *M. alatum*. — No, indeed! He mentions this to him extremely inconvenient species, so very closely related to *M. glabrum*, once at the very end of his latest paper, in a misleading phrase as having dorsicolous males "afterwards becoming female". According to the principles adopted by Beard and Prouho, the man who has dissected a horse would be right in regarding it as a dioecious animal, but if he should for the first time in his life see a zebra passing in a circus procession, he might be justified in writing all his friends that the latter animal was hermaphrodite. or a "complemental male", or even an asexual mammal!

It is unnecessary to consider at length Beard's remarks concerning the cysticolous and entoparasitic species. In obedience to his non-comparative method Beard makes no observations of his own on any of these species but spends much time distorting and vilifying the observations of others in his desperate attempt to save his pet, the "complemental male" of *M. glabrum*. My work on *M. pulvinar* is dismissed with a misrepresentation (p. 320) which may pass without comment, and the following remark: "apparently since Wheeler's manuscript left his hands Prouho has directly challenged his conclusions". The fact that the manuscript had already left my hands before Prouho's article appeared, proves, of course, that I was mistaken in regard to *M. pulvinar*! To my knowledge, Prouho has not reverted to this subject since my paper was published, and I take this to mean, till that gentleman makes statements to the contrary, that he either acquiesces in my view, or does not wish to insist on his interpretation of *M. pulvinar* as a dioecious species. Till I am proved to be wrong I shall continue to regard Beard's remarks on this subject as so many gratuitous assertions.

The sexual phases of *Myzostoma* are hardly of sufficient general importance to justify more extended comment on Beard's paper, abounding as it does in misunderstanding, misrepresentation and futile

<sup>10</sup> Like the "side wall" hermaphrodites of *M. glabrum*, which should be a much greater source of discomfort to Beard than they appear to be.

speculation. Beard's unwarrantable inversion of the natural sequence of the degrees of parasitism within the genus *Myzostoma*; his gratuitous and confused hypothesis of a choice on the part of a dioecious animal between the Scylla of hermaphroditism and the Charybdis of parthenogenesis; his one-sided interpretation of the observations of Nansen, v. Graff and myself, who have devoted far more attention to these parasites than he has been able to give; his depreciation of the theories of others while demanding belief in his own speculations; — all these matters might be considered at great length, but Beard is not alone in having "more congenial and more important work in hand". I trust that enough has been said in this and my previous papers to convince any fair-minded zoologist that the "complemental male" of *M. glabrum* is one of those tenuous and fanciful creations for which one could have wished that euthanasia, that silent death so becoming to pet speculations when they have ceased to afford either amusement to their originators or edification to their readers.

Hull Zoological Laboratory, the University of Chicago, April 20th 1899.

## 2. Sulla omologia dell' organo assile dei Crinoidi e su altre quistioni riguardanti la morfologia degli Echinodermi.

Nota di Achille Russo, Prof. di Zoologia presso la R. Università di Cagliari.

(Con 3 figure.)

eingeg. 25. Mai 1899.

Avendo esteo ai *Crinoidi* le mie ricerche, rendo noti preventivamente alcuni nuovi risultati, sperando di potere quanto prima pubblicare un lavoro d'insieme.

Dagli studi di E. Perrier<sup>1</sup> sullo sviluppo della *Comatula mediterranea* (*Antedon rosacea* Linck) risulta che l'organo assile dei *Crinoidi* sia una omologa formazione della glandola ovoide delle Asterie ed Ophiure e degli Echini. Tale organo, prolungandosi nell' asse peritoneale del peduncolo della larva per la moltiplicazione delle cellule dà origine a tanti ramj che, nel corso dello sviluppo, vanno a situarsi nelle braccia e poi nelle pinnule. Queste vedute furono seguite in gran parte dal Cuén ot<sup>2</sup>, il quale generalizzò i risultati, asserendo che gli elementi germinali degli Echinodermi, fatta eccezione delle Synapte ed Oloturie, si formano per una proliferazione delle cellule della glandola ovoide. Il Prouho<sup>3</sup> però nel 1887 veniva a contraddire gli studi

<sup>1</sup> Mémoire sur l'organisation et le développement de la Comatule de la Méditerranée. Nouv. Arch. du Mus. d'Hist. Nat. de Paris. 1886—1889—1890.

<sup>2</sup> Études morphologiques sur les Echinodermes. Arch. de Biologie, T. XI. 1891.

<sup>3</sup> Recherches sur le Dorocidaris papillata et quelques autres Echinides de la Méditerranée. Arch. de Zool. exp. 1888.



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Zeitschrift/Journal: [Zoologischer Anzeiger](#)

Jahr/Year: 1899

Band/Volume: [22](#)

Autor(en)/Author(s): Wheeler William Morton

Artikel/Article: [J. Beard on the Sexual Phases of Myzostoma. 281-288](#)