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LXII.—*Mr. Dendy on the Chalininæ.*

By R. VON LENDENFELD.

My friend Mr. A. Dendy has recently published a paper entitled "The New System of Chalininæ, with some Brief Observations upon Zoological Nomenclature" (Ann. & Mag. Nat. Hist. November 1887, p. 326). Every reader of it will probably have been as surprised as I was to find that this paper is simply a review of an account of the Australian Chalininæ recently published by me ('Zoologische Jahrbücher,' vol. ii. 1887), as my name does not occur in the title.

Mr. Dendy draws attention to some mistakes in my paper, for which I am much obliged, as his review will in this way partly serve as a list of errata to my original essay, and thus add to its utility. I hope that he has pretty well exhausted the mistakes contained in it, as it produces the impression that he looked for them with much trouble and as logomachy is evidently not among Mr. Dendy's faults.

A number of his statements, although made in a very confident, dogmatic style, are highly controversial, and I should be glad to make a few remarks upon these parts of Mr. Dendy's review.

Concerning the canal-system Mr. Dendy raises a doubt as to the correctness of my drawing, because I do not describe in the text every detail contained in the drawing. These details are not essential, and I omitted to describe them (1) because they are by no means common to all Chalinids, and (2) because they are in the drawing. I hope he will reexamine the type to which the drawing relates, which is under his care at the British Museum, to settle this doubt of his.

As to the apparent inconsistency involved in placing the Gelliodinæ and Ridley's *Toxochalina* in the Chalininæ, and therefore in the family Homorhaphidæ, I can only say that such inconsistencies are unavoidable and are met with also in the families Heterorhaphidæ and Desmacidonidæ as established—and, I think, with good reason, for it would be a mistake to attach too much importance to the shape of the spicules—by Ridley and Dendy (Report on the 'Challenger' Monaxonida). The Heterorhaphidæ are characterized as possessing differentiated microsclera, and yet Ridley and Dendy have (*l. c.* p. 32) placed *Rhizochalina* in that family, although there are no differentiated microsclera in this genus. The Desmacidonidæ are characterized by the possession of chelæ, and yet Ridley and Dendy place sponges in this family which

have no chelæ. In their Report the following are described as forms of Desmacidonidæ without chelæ: *Echinoclathria glabra*, *Agelas*, and *Echinodictyum*. As the authors no doubt knew very well, there are a number of such besides those three contained in the 'Challenger' collection. No doubt these are true Desmacidonidæ.

To make these important exceptions appear less conspicuous Ridley and Dendy say (*l. c.* p. 62) in a little footnote—nothing is said about any exceptions in the diagnosis of the genus—that they have "included *one or two* species without chelæ, on the supposition that they have had them, and subsequently lost them," in the family Desmacidonidæ. This method of shelving *one species and two genera* described, besides many more not in the 'Challenger' collection, which appear inconvenient to a preconceived idea as "one or two" species, "gives us," to use Mr. Dendy's own words (p. 336), "some insight into his method of working."

The inconsistency is in all cases apparent, but it is no fault of the authors. The method of arranging organisms in families, genera, species, &c. is, as carried out by us, unnatural, and the faults which result from it must be ascribed to the method employed, and not to the biologists who use it.

I do not see why the Homorrhaphidæ should not comprise sponges *with* differentiated microsclera, as the other two families comprise forms *without* them. The arrangement is, in consequence of this want of precision, open to objection; but it was not I who established it, but Ridley and Dendy. The precision of the other families would not be increased if the sponges referred to were removed from this family.

It is all very well to say that the amount of spongin has only very little systematic significance, and that this has been "demonstrated again and again." I also agree with Mr. Dendy that the amount of spongin has no great significance; but it must be borne in mind that F. E. Schulze ('Challenger' Report on the Hexactinellida, p. 497) attaches a good deal of importance to it; and Schulze probably knows more about sponges, and certainly has a much more matured judgment, than my friend Mr. Dendy and myself put together. It gives me the impression that Mr. Dendy has allowed himself to be carried away by his own strong convictions, and he inveighs against my arrangement—which, like all similar arrangements, is a subjective idea, and doubtless faulty and bad—with a force worthy of a better cause.

Mr. Dendy attacks my method of nomenclature most unmercifully. He persistently closes his eyes, however, to the logical principle which I maintain in it, and thinks it

sufficient to show that my nomenclature is, according to the method to which he has accustomed himself, untenable and peculiar. This is certainly the case; but it could equally well be shown that the method employed by Mr. Dendy is illogical, unreasonable, and bad. I do not find that the logical correctness of my system is questioned, but Mr. Dendy takes it upon himself to say that it cannot be right. The principles on which I name the species &c. are very different from the method employed by him; and to enable the reader to form a judgment on the method of nomenclature employed by me, I give an abstract of it here.

If a species is described and I redescribe it without altering its limits and definition, and without placing it in another genus, I attach to it the author's name, because I retain his species.

If a species is retained by me in its original scope, but placed in another genus, I retain the specific name and replace the name of the original author by my own, because the name under which I describe it is different from that used by the original author. F. E. Schulze ('Challenger' Report on the Hexactinellida) gives in such cases the name of the original author in brackets, and sometimes adds another, as, for instance, in the case of *Farrea occa* (Bowerbank), Carter (p. 277).

If I retain a previously described species but alter its scope, I retain the old name, unless it is objectionable, and affix my name to it, because it is in this sense a *new* species.

If a species has been described under various specific names I take the oldest of those which apply to the species *in my sense*, and not to the oldest of all irrespective of this.

If I combine a number of *different* sponges previously described to form one species, I give it a new name, because none of the previous names applies to the species *in my sense*, which is *new*.

If sponges belonging to different species have been described as one and the same species (this does not occur often), I of course establish new species for the parts thereof.

If I raise varieties to the rank of species, I retain the name of the variety if suitable and attach my name to it, because as a species it is *new*.

If I distribute a number of sponges previously described among varieties of one species, I of course give these as synonyms of the varieties, and establish a new name for the species, under which the old names do not appear as synonyms.

I hold strong opinions on this point, and am radically

averse to the certainly illogical, although perhaps easy, way of naming things in vogue among many naturalists, including Mr. Dendy. I do, however, by no means imagine that I have established this rational system of nomenclature, as I will call it. It was established long ago, and has been most carefully utilized for practical work by Hæckel in his 'System der Medusen.' I do not think that any difference at all exists between Hæckel's method and mine, and I am further not aware that I have made any mistakes in the nomenclature either in my paper on the Chalinids or elsewhere.

It seems to me as if Mr. Dendy wished to veil his real attack on the principle involved by the feigned polemic against my Chalinid nomenclature. Well, I suppose everything is fair in love and in war! But somehow I do not like this perversion of the real issue.

To show the fallacies of the system of nomenclature advocated and employed by Mr. Dendy one need only look in the Report on the 'Challenger' Monaxonida by Ridley and Dendy. The volume opens at page 117. There a species is described as *Iophon Pattersoni*, Bowerbank. On the following page a variety "*Pattersoni*" is mentioned. A comparison of Bowerbank's original diagnosis shows that *his* species *Halichondria Pattersoni* is nearly identical with the variety *Pattersoni*. The species *Iophon Pattersoni* as established by Ridley and Dendy differs entirely from Bowerbank's sponge, and it is simply untrue that the species described by Ridley and Dendy as *Iophon Pattersoni* is synonymous with *Halichondria Pattersoni*, or was ever established or conceived by Bowerbank. The species has been established by Ridley and Dendy, and their names should be attached to the specific name. *Halichondria Pattersoni*, Bowerbank, should be given as a synonym of the variety, *Iophon Pattersoni*, var. *Pattersoni* (sic!).

As none of the previously described species coincide with the *Iophon Pattersoni* of Ridley and Dendy, a new specific name ought to have been given. Any one of the old names would give a wrong idea, and it would be illogical to use it, as it is obviously illogical to replace the whole by a part.

Such tamperings with the laws of thought have already brought our science into the contempt of mathematicians and philosophers. Logic must be sternly established. I regret that my friend Mr. Dendy and I hold such diametrically opposite views; and I can only answer to his statement (p. 337) "Whatever may be the real name of this comprehensive species, it certainly cannot be '*Ceraochalina papil-*

lata, n. sp.!' " that this name is perfectly logical and must be upheld ; and, further, concerning Ridley and Dendy's *Iophon Pattersoni*, that whatever may be the real name of this comprehensive species, it certainly cannot be "*Iophon Pattersoni*, Bowerbank"!

Mr. Dendy cites the name of another of my species, and criticizes it as "beyond comment." I regret that my mental faculties are so feeble that I cannot see even the slightest objection to it!

Mr. Dendy enters a protest against the "free-and-easy system of nomenclature" used by me. It is not necessary to waste any words on such a phrase ; but I must enter a protest against the cramped and illogical, although easier, method employed by Mr. Dendy.

I have been able to meet all Mr. Dendy's objections in the same objective and friendly manner in which no doubt they were meant, however forcible Mr. Dendy's language may be.

There is, however, one assertion which I am sorry to say cannot be viewed in this light. Mr. Dendy says (p. 336) that by my method I was "as it were capturing all stray species and taking forcible possession of them." Trusting that Mr. Dendy will regret the wording of that passage, I feel that it is incumbent on me to express my thanks to him for exposing the mistakes contained in my paper, and for showing me on what points a clearer expression of my views was desirable.

LXIII.—*Descriptions of six new Species of Butterflies captured by Mr. John Whitehead at Kina Balu Mountain, North Borneo, in the Collection of Mr. H. Grose Smith.* By H. GROSE SMITH.

Papilio acheron.

Male.—*Upperside.* Both wings bluish black, slightly suffused with purple ; margins between the nervures narrowly white ; the posterior wings slightly irrorated with grey in the anal area.

Underside. Both wings with markings as in my *P. Forbesi*, except that the grey rays between the nervules on the anterior wings are less marked, the ochraceous band on the posterior wings does not extend beyond the upper median nervule, and the three blue spots near the exterior margin between the costal nervure and the discoidal nervule are less