Zoologischer Anzeiger

herausgegeben

von Prof. **Eugen Korschelt** in Marburg.

Zugleich

Organ der Deutschen Zoologischen Gesellschaft.

Bibliographia zoologica

bearbeitet von Dr. **H. H. Field** (Concilium bibliographicum) in Zürich.

Verlag von Wilhelm Engelmann in Leipzig.

XXVI. Band. 18. Mai 1903. No. 700.

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I. Wissenschaftliche Mittheilungen.

1. On Rules of Nomenclature.

By Fredk. Pickard-Cambridge, B.A.; F.Z.S.

eingeg. 29. Januar 1903.

The paper published by Dr. Friedr. Dahl in the "Zoologischer Anzeiger" Bd. XXV, No. 683/684, Oct. 13th, 1902 entitled "One word more about the rules of nomenclature", needs some reply from me, because otherwise a wrong impression may be gained as to my attitude towards the questions under consideration.

The rule of Priority.

In the first place it seems to me that Dr. Dahl is somewhat confused about two of my rules which he regards as quite contradictory the one to the other. They cannot, he says, stand side by side if logical and consistent rules are desired. If, he holds, I regard the first name given to a species as the correct one, then I must also regard the first species referred to a genus as the type. Certainly, in the case of the determination of names, it is my first rule that the name

which occurs earliest, even with respect to a page or line, is the name we must use, not by any compulsion, but simply if we are to be consistent with this rule.

But in the case of "types" we are dealing with a totally different problem, which has to be solved in the presence of its own peculiar circumstances by a different mode of proceedure and hence by different rules. Though, none the less, the rule of priority would still be followed in the case of the fixation of types, when for instance two species were cited as typical, then, if these be no other method applicable, since we are determined that one species shall be the type, we select the first, simply for the sake of convenience. There is no special merit in the Law of Priority in itself; we do not bow the knee before a mystic "Firstness" and make a "Mumbo Jumbo" of it! Does Dr. Dahl suppose that we apply this rule of priority to every problem in systematic zoology? If so we shall be compelled to accept the Linnaean classification of the Araneae and recognise one genus only Aranea. We simply adopt the rule of priority for the purpose of avoiding confusion, not because there is any particular virtue in the name first given or credit to the author who gave it. We must recognise one name, and one only, and we agree to take the first given. As for the credit, in a great many cases the real credit is due to the men who years after have all the thankless labour of determining to what forms these names should apply.

I will make my position however perfectly clear on this question of names and types.

A. The first rule applied to the problem of the settlement of the names of species, is, that the name which was first given to a species shall serve.

B. The first rule applied to the problem of the fixation of the types of genera is that one of the species originally included in the genus shall serve as the type, and that this shall be either the last left in by elimination, or the first definitely cited as the type.

In the second case we are dealing with a totally different set of circumstances and we start confronted by the fact that many species, very often including the first, have been already removed to new genera. We recognise the right of this removal; and it follows that none of the species removed can serve as the type of the original genus; making this proviso — that one at least must be left in. If all have been removed, then the last removed, being really the last left in, serves as the type. We do not worship "Lastness" or attach

any special importance to it, we choose the last species left in simply because it is the only one left to us.

As a matter of fact, the groups are in themselves often of no systematic importance whatever. — But we have to use names, we agree that the old ones will answer the purpose very well; and all we want to do is to definitely fix some single species on to each name so that we shall know what we are talking about when we use these names.

We have to clear our minds of "Bogies"; and simply adopt some method which our present immediate necessities suggest and the past action of systematists renders possible.

If Dr. Dahl cannot discriminate between the two processes, and holds that consistency in applying rule (A) in the matter of "names" involves a fundamental logical inconsistency when we apply rule (B) in the case of "types", one can only suggest that we do not move on the same intellectual plane.

Definition of "Type".

Dr. Dahl has made a great discovery — "For Mr. Cambridge the sole aim of a type is to make the subdivision of a group very easy for an Author". If by this he means that for me the object of a type is that we may more readily classify under recognised names the material which comes before us, well and good. Incidentally of course subdivision may become less difficult when one has the generic characters made definite and distinct; just as the identification of new species is rendered more easy by the publication of excellent tables and figures. But what does Dr. Dahl suppose the object of systematic zoology to be? to assist the student who wishes to classify his material, or to throw dust in his eyes and effectually prevent him from making head or tail of the matter? There are I believe systematists who avoid giving the only characters which are of any value in separating species, in order to choke off other workers from the study. This at least is the only interpretation one can put upon the phenomenon of the utterly inadequate diagnoses of genera and species which appear. But as far as I am myself concerned I at least endeavour, however unsuccessfully, to make the classification clear and distinct. If this action makes it easier for other authors to make new genera and species, that is an incidental result; but not the main object held in view when one desipes definite types — we do not care whether it is easy or difficult for authors to subdivide genera; what we do want is that the characters of the genera and species already founded shall be as clear as possible.

But the "type" says Dr. Dahl plays a far more important part in nearly all the sciences and is understood to be "the fundamental form of a group of things". This form "exists only in a transcendental sense, not in reality but only in our minds, and it is customary to choose out from that group an example which most nearly approaches this fundamental form". — And this species we are to regard as the type.

Now I have no intention at present of entering into subtle arguments as to the nature of the ultimate Type in Itself, and when an author chooses out an example from a group, it does not matter to me by what metaphysical considerations he fixes upon this or that species to represent the form which did not exist in reality, so long as he selects one which does exist in reality, namely one of the species originally referred to the genus.

This species Dahl regards as the type, and one would ask why, when the original author at any time cites a species as type, this species is not to be regarded as that which more nearly approaches the fundamental form he had in his mind? And why then are not Latreille's citations in 1810 so to be regarded?

When however the original generic group has been broken up before a type has been cited, then if the original author wishes to cite the type, he has to confine himself to the remainder, and from these to select the one which more nearly approaches his ideal type form. So too with a later author. And what is this citation of a type but the limitation of the genus, originally allowable, as it then exists? The action is for practical purposes exceedingly advantageous because it definitely attaches the generic name to a single species which serves as "type" or standard of comparison.

For in practical systematics, as in every other science with which I can acquainted, a type is always used in the sense of a standard of comparison by which we may know the characteristics of any group of phenomena referred to, and classify then accordingly.

Whatever subtle differences then may be in the senses in which the term is used; in actual practice, whether used in a Biblical, Medical, Architectural or Phylogenetic or any other sense, the type always carries with it the idea of a standard, a fixed and definite form with which we may compare others and so determine their affinities. But whether or no, at present we are engaged in systematic zoology and not in Biblical exegesis, and for us a type is used in the sense of a standard of comparison, either for a single species or for a generic group, so that we may compare our material with it and decide with which group it has the strongest affinities. Of what earthly value

is a type if it has not this function? And if it has no value, how comes it that no modern authors of any ability ever fail to cite their types?

One can readily understand and sympathise with the type of mind which has some delicacy in tampering with the original generic groups of honoured authors a hundred years ago, and would desire to leave then intact. But when once we have agreed to accept the logic of the stricken field and realize that long ago these groups have been broken up, and again divided by the sword of the analyst, and have admitted the validity of such action; then I fail to understand the kind of intellect which afterwards refuses to permit either the original, or other author, from carrying out a further limitation by definitely citing a "type". Still less sympathy has one for the temperament which, having once agreed that certain processes are practically necessary, declines to apply them whenever he finds it inconvenient to do so.

However, as I ventured to observe in a former paper, one has no desire to take one's stand as a pope in any matter, nor would one ever seek to compel anyone to conform to any of these rules, except so far as they can be brought under compulsion by the dictates of their own common-sense, in view of a practical problem to be solved.

In case any systematists may wish to have a clear idea of the object we have in view and the methods by which we propose to attain it, I add a brief summary of the rules I am following.

The determination of the Type of a genus.

In the interests of practical systematic zoology it is held to be absolutely necessary, in order to put an end to the prevailing confusion in the use of generic names, that a single species be fixed upon as the Type or Standard of Comparison for each genus, so that we may know exactly what characteristics we refer to when we use any particular generic name.

Definition of a genus.

A genus consists of one or more species, either definitely cited by name, or indicated by reference to some publication, and designated by a special generic name.

A valid genus.

A genus is held to be valid, when the generic name is accompanied by one or more established species, cited by name or indicated by reference, with or without a generic diagnosis.

Note 1. In all other cases the generic name is regarded as "nomen nudum".

Note 2. The fact that a generic name is "nomen praeoccupatum" is held not to affect the validity of the generic group with reference to the proceedure indicated below for the determination of types. Experience shows that confusion is best avoided by this method.

The object of fixing the type of a genus and of the methods of determining the type, (A) by elimination, (B) by citation; is to restrict, as early as possible in the history of a genus, the area within which the type need be sought for.

Definition of Type.

The type of a genus consists of a single species which serves as a Standard of Comparison for determining the characters which are to be connoted by a particular generic name.

a) By Elimination or Exhaustion and by Restriction.

Rule 1. The type must be represented by one of the species originally included in the genus when the generic name was first bestowed.

- 2. A species once removed from a genus to a new generic group is no longer available as the type.
- 3. A generic group may be restricted to one or more species, at any time when a new genus is being founded which includes one or more of the original species.
- 4. A generic group may be restricted by the quotation of the generic name accompanied by one or more of the original species, with the term "sensu restricto", and the species left in by the process of elimination, or isolated by restriction, are alone available for service as the type.
- 5. When a single species alone has been left in, or isolated, this is regarded as the type.

b) By definite citation.

1) At the time when the genus was first founded.

2) At a time subsequently to the founding of the genus.

1) Rule 6. When the founder of the genus has definitely cited any single species under the terms, "typ", "typus", "type" or "typical" — the species thus referred to is regarded as the type of that genus.

- 7. When the founder of the genus specially refers to the figure of a species in connection with the generic diagnosis, this is also regarded as a definite citation of the type.

2) Rule 8. When the generic and specific names of a species are identical, this species is held to be the type of the genus.

- 9. When any author, the originator of the genus or otherwise, has at any time definitely referred to any single available species under the terms noted in rule 6; the species thus cited is regarded as the type of the genus.

Note 1. The actions of Elimination or Restriction and that of Citation do not clash, but mutually assist in the determination of the type. The first limits the area within which the type can be cited; while the citation puts an end to any further action of elimination or restriction so far as the settlement of the type is concerned.

Note 2. If an author, either consciously or unconsciously, removes all the remaining species in a genus to a new generic name; then this name becomes simply a synonym, since the two groups are conterminous.

2. Bemerkungen zu den Amphipoda Hyperiidea der deutschen Tiefsee-Expedition.

I. Thaumatopsidae.

Von Privatdoc. Dr. Woltereck, Leipzig.

(Mit 1 Tafel und 2 Figuren.)

eingeg. am 24. Februar 1903.

Die erstaunliche Vielseitigkeit im Bauplan der Hyperiiden, welche wie keine andere Thiergruppe den formenumbildenden Einfluß rein pelagischen Lebens erkennen lassen, wird am besten durch die beiden Gegensätze *Oxycephalus* und *Rhabdosoma* auf der einen, *Thaumatops* und *Mimonectes* auf der anderen Seite illustriert. Denn hier haben wir im letzteren Fall als angestrebte und schließlich fast erreichte Idealform die Hohlkugel, im ersteren dagegen die gerade Linie, resp. den wagerechten Stab vor uns, also keine geringe Divergenz im Bereich einer Unterordnung!

Während *Mimonectes* die Kugelform durch gemeinsame Auftreibung von Kopf und Brust — mit Ausnahme der flach bleibenden Ventralfläche — in der That beinahe erreicht, während ferner die neu aufzustellende Gattung *Sphaeronectes* auch noch die Bauchseite halbkugelig vorwölbt, zeigt die Familie und Gattung *Thaumatops* andererseits gesonderte und daher viel weniger die Urform entstellende Aufblähung von Kopf, Peraeon, Pleon und Urus, wobei ersterer durch die enorme Ausbildung der (bei *Mimonectes* und *Sphaeronectes* stark rückgebildeten) Augen am meisten auffällt.

Außerdem überragt diese Gruppe durch ihre Körpergröße so sehr