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TAXON

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WHAT IS A TAXON?

H. J. Lam (Leiden)

The fact that Mr. Morton's remarks on the term taxon (Rhodora 59: 43—44. 1957) have been reprinted in the present journal (Taxon 6 (5): 155. 1957) — thank you both for the advertising! — seems to call for a short reply, and I trust that the name of the present "News Bulletin" is not considered another misuse of the term.

It may be of some historical interest to first recall the origin of the term. It was first, semi-subconsciously, conceived when I wrote my paper on "Phylogenetic Symbols, Past and Present" (Acta Biotheoretica ser. A. 2 (3): 153—194. 1936). I had, a few years earlier, returned from a longish stay in the tropics, and the new task of a professorship in Europe entailed new contacts and new interests, and inspired me to new thoughts. I had at that time been impressed by some papers of Prof. A. Meyer (now Meyer-Abich) of Hamburg and when preparing my paper of 1936, came across the term taxon (-a) and phylon (-a) which were introduced in Meyer's "Logik der Morphologie im Rahmen einer Logik der gesamten Biologie", 1926 (pp. 127, 133, 241).

However, Meyer's concept of the term phylon being of a purely philosophical and theoretical nature rather than referring to what we in the "Code" use to call a phylum (divisio), I wrote in my paper of 1936 in a note on p. 180 (referring to the words "taxonomic units" in the text): "I have not checked the matter thoroughly, but it seems to me that Meyer's taxa (and phyla) are more valuable concepts in theory than in practice". As a matter of fact I must confess that I have never clearly understood what exactly Meyer meant with these terms. But feeling a need for a dynamic phylogenetic term to complement the static term "taxon" I suggested a "genorheithrum" for a "flow of coherent genes", as "phylon" would obviously be ambiguous. Genorheithrum is akin to Zimmermann's "hologeny" and Rensch' "Rassenkreise" in a phylogenetical sense (cf. W. Zimmermann, Flora Allg. Bot. Ztg 33: 419. 1939; and B. Rensch, Kurze Anw. f. Zool.-syst. Studien, Leipzig 1934: 102, fig. reproduced in my paper of 1936, p. 190) [1].

In the meantime, on account of my uncertainty about Meyer's meaning, I felt rather at a loss when Prof. G. H. M. Lawrence of Cornell University, who then prepared his "Taxonomy of Vascular Plants" (1951) wrote me for information about the origin of the term. The result may be found in a footnote on p. 53 of his book.

In 1936, however, I did not yet think of the term taxon in the sense we now use (and misuse) it in: and I suppose that I more or less forgot it as a point of little importance, and that, when Dutch taxonomists were invited by Prof. Lanjouw

[1]) Cf. also my paper (in Dutch) in "Vakblad voor Biologen" 19(11): 202. (1938), in which I stated that the term taxon (-a) stands for what, since Naegeli, is called "*Sippe*" in German.

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to prepare proposals for what is known as the "Utrecht Conference" of 1948, I subconsciously reinvented the term in its present significance. As every reader of recent taxonomic literature knows, it was accepted by the Stockholm Congress and now appears in the "Code". It seems to have been proposed at the proper moment for its use has spread with lightning rapidity and even seems to have infested the zoological mind.

Sofar regarding the history. Before concluding with a few words in reply to the question which serves as a title to these lines I think I may be permitted to mention the origin of the name of the present journal. I remember that, on one of the last days of the Utrecht Conference (which was a stimulating though rather trying affair), we went for dinner in a local restaurant. Lanjouw and I were seated first; we were tired, but our minds were working on with the problems dealt with (among which the term taxon as well as the idea of establishing an International Association with a Bulletin of its own); and playing with the menu Lanjouw sighed: "Now what shall we call that Bulletin. Do you have a suggestion?" Half jestingly, I grinned: "Why not Taxon"? Lanjouw smiled, but he did not seem to like the idea too well. The subject was dropped then and there and we never discussed it again, but the Bulletin appeared to be called "Taxon" allright.

And now Mr. Morton laments that the term, so enthousiastically received, is occasionally being badly misused. That of course, falls to the discredit of the culprits and it is certainly useful to point out cases of evident abuse, though I think there is no reason to regret its introduction as Mr. Morton seems to fear on my behalf. But when we come to think of it, it is not so easy to give a proper definition of the term. At first sight it seems obvious that (to quote Morton's examples) a taxon, being a collective concept, cannot be "short-haired" or "growing in swamps". On second thoughts, however, it may be considered that any group of organisms with some common character (of whatever kind) has some right to be called a natural group. It may be that such a natural group is extremely ephemerous, such as a gathering of taxonomists with the purpose of discussing the proper use of the term taxon. The people involved are clearly united by a "character", viz. the purpose just mentioned, and I cannot think of a reasonable argument not to call such a gathering a natural group. The same applies to organisms which are evidently united by a common feature, obviously originating from a common cause. We all know groups of organisms bound together by ecological, biological, ethological, and psychological factors. In this light an expression like "ecological taxa" (Van der Pijl, Vakblad voor Biologen 37 (6): 69. 1957) is comprehensible, if not desirable, and it becomes clear that "short-haired taxa" is an (admittedly inadmissible) abbreviation of the expression "taxa consisting of short-haired specimina".

However, if we recognize these as more or less "natural groups" (a surely extremely vague designation), we are not easily inclined to refer to them as taxa.

This takes us to our point: what exactly is, or should be, a taxon? It is undoubtedly meant to cover a natural group or natural groups, but this does of course not mean that every natural group deserves the name taxon. The Rules do not give a definition of course, the term having just been substituting what was formerly called "a taxonomic group" or a similar expression, which was not defined either, and for comprehensible reasons: as Morton correctly states, the taxon concept is an abstraction, which can no more be unambiguously defined than any of the categories it covers.

But, as is the case with any of the categories, taxon has inevitably two significations: as an abstraction, and as a reference to a particular case. In the latter the term may be used, and in fact is used for stylistic reasons, so as to avoid too frequent repetition of the name of the category in question, and if a definition should be required, this

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definition coincides with that of the category and might generally be given as a group of organisms which a "good taxonomist" recognizes as (a species, genus, family... etc.).

As an abstraction the term taxon is rather sharply definable if the condition is added that it should denote any nomenclatural category and that by nomenclatural categories we exclusively understand those enumerated in Art. 13 of the Code.

Even so, however, we must be aware that a botanist may be seriously tempted to identify "taxon" and "any natural group", the latter term being applicable to any group of organisms which have some "natural feature" in common.

In conclusion we may state, that taxon is a term which is unloosenably connected with the Code and should not be used in any other context.

THE COMPULSORY LATIN DIAGNOSIS

A. A. Bullock (Kew)

The Paris Code, most painstakingly edited by an international committee under the very able chairmanship of Prof. Lanjouw and astute guidance of Dr. Stafleu, is a vast improvement on all previous editions but there remain some few anomalies, and this note is intended to point out one of them.

Art. 34 of the Paris Code corresponds to Art. 44 of the Stockholm Code, but an attempt was made to close a loophole for escaping the compulsory Latin diagnosis. The Stockholm Code brought forward the effective date to 1 Jan. 1935 for all plants except bacteria and algae; the Paris Code fixed the date for those two groups at 1 Jan. 1958. But there is an addition in the Paris Code which goes far beyond the intention of the Congress and probably beyond the intention of the editorial committee. This is, ".... or by a reference to a previously and effectively published Latin diagnosis". The intention, clearly, is to cover the period since 1 Jan. 1935, prior to which descriptions in modern languages are sufficient for valid publication, but the effect of the wording of the article is to invalidate all new names published since 1 Jan. 1935 which depend upon a reference back to any previously published description, unless the latter was in Latin! It is impossible to say how many names will be affected, but the number must be large.

The following amended wording of Art. 34 will perhaps avoid this difficulty, and bring it into line with Art. 32, which also refers to previously published descriptions [1]:

"*Art. 34.* In order to be validly published, a new name for a taxon of recent plants, the bacteria and algae excepted, published on or after 1 Jan. 1935 must be accompanied by a Latin description, or by a reference to a previously published description in accordance with Art. 32, provided only that such previously published description must be in Latin if it was published on or after 1 Jan. 1935.

The same considerations apply to new names for recent taxa of Algae, except that the effective date is 1 Jan. 1958."

It will be noted that I have substituted the word "description" for "diagnosis", as has been done already in Art. 32 and elsewhere in the Code. Although the word "diagnosis" includes perhaps more than a simple description, in the jargon of

[1]) Proposal no. 11 submitted to the 9th International Botanical Congress, Montreal 1959.

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