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Reply to Santesson's Criticism on Taxonomy of Fungal Symbionts of Lichens

Author(s): R. Ciferri and R. Tomaselli

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a warning of possible further uncertainty as to identity, is found in some of the citations. For example, Donk named a subgenus *Ramariopsis*. Corner in his text says he is elevating this to the rank of genus, and he uses the name as the name for one of his generic entities as "*Ramariopsis* Donk emend." To quote Article 59 of the Code: "When a genus or a taxon of lower rank is altered in rank but retains its name or epithet, the original author... must be cited in parenthesis, followed by the name of the author who effected the alteration."

It is hoped that the above discussion combined with this review will point out some of the distinctions between the systematic, taxonomic and nomenclatural phases of a monographic study. It is hoped that this

expression of opinion as to better nomenclatural procedures will call attention to the constant necessity of distinguishing between *fact* and *opinion* and, in the case of taxonomy and nomenclature, between *obligate* and *facultative* relationships. It is, of course, Corner's choice whether he cares to follow the Code, the type method and priority, or not. The lack of a nomenclatural treatment that follows the Code does not necessarily alter the magnitude of the systematic and taxonomic contributions. Perhaps it is Corner's almost complete freedom from such conventions as adherence to the nomenclatural Code that has enabled him to treat this group of fleshy fungi strictly morphologically and develop such a sparklingly fresh taxonomic treatment.

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## REPLY TO SANTESSON'S CRITICISM ON TAXONOMY OF FUNGAL SYMBIONTS OF LICHENS

by

R. Ciferri and R. Tomaselli (Pavia)

Santesson (Taxon 3 (5): 147-148. 1954), with more ill-will than good reasoning is anxious, although he is a pure lichenologist, to rush to the defence of mycology. The examples of "Endocyanosen" and "Syn-cyanosen" are irrelevant, and even more so are the other symbionts cited by Santesson, which are brought up only for the sake of argument. If we were to follow him along this track we could quote cases to the contrary which concern the taxonomy of the Protozoa associated with the digestive apparatus of termites or bovines; but these are outside our argument.

Strange to say it is really Santesson, a pure lichenologist, who protests against the proposed system, which is in fact mycological and not lichenological. If one were to protest against the great mass of the "twenty thousand" new binomials dreaded by Santesson, one would need to be, above all, a mycologist and not a lichenologist who should take no interest in what should be the system and the method of nomenclature of fungal symbionts of Lichens! But Santesson's alarm is ill-founded; the new binomials, so far from being 20,000, will not number even 2,000. The study of fungal species so far completed and published has established hardly more than 200 species!

We should like to ask Santesson if our supposition is really mistaken: why did not

Santesson, or any other mycologist (or, naturally, any lichenologist) protest when Thomas in 1939 opened the way to such a system of fungal symbionts of Lichens, by proposing 51 genera and 112 species? Our work is merely the continuation of this line of study and the extension of the list proposed by Thomas.

Let us examine in detail Santesson's main criticisms:

1. The argument that fungal symbionts cultivated in the laboratory do not fructify is really valid because most Ascomycetes fructify when grown freely under natural conditions and when they are cultivated in the laboratory under the same conditions, while this is not so for Ascomycete symbionts of Lichens. This plainly means that the Ascomycete symbionts of Lichens, if cultivated in isolation, are deficient in something that they obtain from their association with their algal partners. Even if future laboratory technique should supply this deficiency, there would always be a biological distinction between free Ascomycetes and Ascomycete symbionts.

2. Once again we affirm that nobody has been able to reproduce with certainty lichenic symbionts under experimental conditions in a laboratory. And in fact the case mentioned by Santesson of Thomas's *Cl-*

*donia pyxidata*, 1939, is the proof because this was a mixture of the fungus with the alga (and the podetia produced are characteristic of the fungus, even when cultivated), but this was not a complete lichen since no spores were produced! Only when we have obtained a fructified lichen will it be possible to say that we have attained complete synthesis of lichens under experimental conditions.

3. According to Santesson Art. 76 would be applied only for the purpose of nomenclature, without any reference to the biological conditions of fungal symbionts of Lichens or to their taxonomic position. But it is a fact that this is very different from lichenological practice in classification which cannot be considered apart from its appropriate nomenclature.

4. Santesson is surprised because a number of fungus genera are common to one or more morphologically very different lichen genera and some lichen genera include two or more different fungus genera. Why then is he not surprised that the same alga may be found in very different Lichens? The lichenologist, after all, is interested in the whole fungus-alga association, in which the two components have equal value. If it were not so it would be better to abolish the Lichens, and to treat the two symbionts separately.

5. He says besides that it is sensational to learn that the lichen fungi of *Usnea* and *Lichina* etc. would belong to the same genus. We agree entirely in this matter that it may be a surprise for a lichenologist (accustomed to examine all the thallus in its

macroscopical form which is different from one genus to another when the fungus is in symbiosis with the alga, but not always, it seems, when it is isolated in a culture), but this is no surprise for the mycologist.

For the same reason it is not surprising for the mycologist that the fungal components of *Cladonia rangiferina* and *Cl. silvatica* should represent different fungus genera. The difference arises only from a difference in the colour of the apothecium, but this is a factor sufficiently distinct from a mycologist's point of view. But, taking for granted that in such a case doubts may arise, founded on different points of view, this is not a sufficient reason for rejecting either the general conception or the entirety of the work as it has been put forward.

The final statement that we intend "to make fun of lichenology" is not only incorrect and lacking in courtesy but it discloses in Santesson a particular "forma mentis" not only as a polemicist, but also as a scholar on which, since we are colleagues, we prefer not to dwell. We say once again: lichenologists and lichenology are outside the scope of what we propose which considers only the opportunity of classifying the fungal symbionts of Lichens in mycological, not lichenological lists, and as Fungi — not as Lichens.

In confirmation of this is the fact that the genera proposed by us have been enumerated in "Index of Fungi" (Comm. Mycol. Inst. 2 (7): 115-141. 1953) and even reported in the last edition of "A dictionary of the Fungi" (Ainsworth, G. C. and Bisby, G. R., 4th ed., 1954, Kew, Surrey).

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## The type of *Amaryllis bella-donna* L.

by

J. E. Dandy (London) and F. R. Fosberg (Falls Church, Va.)

There has been complete disagreement among current interpretations of the typification of the genus *Amaryllis* L. beginning with Uphof's attempt to typify it in 1938 (3) and culminating in articles in *Taxon* by Dyer (8) and Traub (9). This led to the calling of a special meeting of those interested in the question on June 30, 1954, by the International Bureau of Plant Taxonomy in connection with the Nomenclature Section of the Paris Congress. This was attended

by a small number of botanists and the matter was discussed in detail. It was decided that, after examination of the material in the Hortus Siccus Cliffortianus, the present authors should prepare a succinct statement of the case for publication in *Taxon*.

It may be stated at the outset that the facts in the case seem fully in accord with the careful and complete discussion by Sealy (4). The reason for a further article is