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PROPOSED CHANGES IN THE INTERNATIONAL CODE GOVERNING THE NOMENCLATURE OF LICHENS

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> It is the duty of the nomenclator to supply names for natural objects. And lichens are natural objects which come within the sphere of botanical investigation and must be adequately dealt with under the Rules of Botanical Nomenclature. — W. H. Camp, 1947.

Since 1950 the International Code has stipulated that "for nomenclatural purposes names given to lichens shall be considered as applying to their fungal components" (Art. 13, Note 4). Although the intent in incorporating this rule into the Code is sound, the rule as it stands has profound implications that exceed nomenclature and enter taxonomy. A really strict adherence to this rule is not even possible for nonexperimental lichen taxonomists (*i.e.*, all lichen taxonomists) and its acceptance is tantamount to accepting suppositions of which at least some are scientifically questionable or plainly wrong. Still many botanists doubtless find that "... the procedure though arbitrary, is perfectly harmless" (Nannenga 1939). It is discouraging to find how widespread this taxonomically irresponsible attitude is. As the nomenclatural rule for the lichens is now stated, certain valuable taxonomic criteria may actually be outlawed because they may (we do not know) concern both components. The lichenologist should be further disturbed by this rule because he knows that he cannot always differentiate with absolute certainty between some fungal and algal manifestations in the lichens that he studies. The purpose of the present paper is to propose a simple solution to this whole problem.

Lichenologists agree, I think, that a classification expressing the relationships of all fungi — including those in lichens — would be the desirable taxonomy. The old polyphyletic "Lichenes" must go and the fungi of the lichens must be intercalated into the mycological arrangement. This noble aim may someday be fulfilled through the hard work of lichenologists and mycologists, but it will never be achieved by tinkering with the rules of nomenclature. Yet it was to advance this taxonomy that the Code was amended to the version quoted above.

If we accept the application of lichen names as now prescribed by the Code, we must also accept the following:

1. Since the name of a lichen refers only to the component fungus, the taxonomy which that name expresses must be based solely upon mycological considerations.
2. Each "lichen species" contains only one species of lichen fungus.
3. All different "lichen species" contain different species of lichen fungi.
4. The morphology of the lichen fungus is never significantly altered in the lichenized state under which we study it; *i.e.*, the same taxonomic conclusions (leading ultimately to a nomenclature) will be reached regardless of whether the lichen or only the lichen fungus is studied.
5. Any modifications produced in the lichen by the alga — even if these modifications are constant for the lichen — must not be considered taxonomically.

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6. The lichens themselves should not have a nomenclature governed by the Code.
7. If the lichens should not have an official nomenclature, the starting point for the nomenclature of the fungi in lichens should be 1821 instead of 1753.

* * *

Under the present Code, the lichen taxonomist is directed to determine which traits of the lichen are purely fungal and to confine his taxonomic attention to them and to the classification of lichen fungi. Regardless of how desirable all this may be, it is a large order because the lichenologist almost never studies the lichen's fungus in the absence of its alga. In fact, the exact influence of the alga on the lichen thallus — especially in the macrolichens — cannot possibly be assessed by herbarium observations; it can be learned only from properly designed experiments. When the fungus is isolated in pure culture, it is found (or it has been up to now) that the unique morphology of the lichen is not reproduced. Is this the failure of technique? Perhaps, but one line of evidence may indicate the contrary. Hale (1957) isolated the fungal component of the common crustose lichen *Buellia stillingiana* Stein. The formless mass of hyphae that developed in culture produced conidia and was identified by Dr. Stanley Hughes as the Fungus Imperfectus *Sporidesmium folliculatum* (Corda) Mason & Hughes, a common wood-inhabiting saprophyte of eastern North America. This discovery suggests a whole array of speculations and may even substantiate the view that the lichen's vegetative morphology (so important in taxonomy at the species level) is largely a product of lichenization. To exactly what extent then is the vegetative morphology of a lichen an intrinsic trait of the lichen fungus?

Now consider two situations: (1) It would come as a great surprise to lichenologists if it were shown that the fungal component of a lichen species is not uniform (does not belong to the same species) throughout the range of that lichen. The Code tacitly and probably rightly assumes the uniformity of the fungal component in "lichen species." Nevertheless it remains that this supposition has never been proved. Asahina (1937) has even suggested that the macroscopically indistinguishable lichens *Lobaria pulmonaria* (L.) Hoffm. (with a green alga) and *L. retigera* (Bory) Trev. (with a blue-green alga) might in reality contain the same fungus and that the different chemistries associated with these lichens might result from the influence of the different symbiont algae. — (2) On the other hand, suppose that two "different" lichens are found to contain the same fungus. This is certainly not all together improbable. In fact, such a report has already been made. Tomaselli (1957) claims that his experiments show that the common, obviously different lichens *Xanthoria parietina* (L.) Th. Fr. and *Caloplaca elegans* (Link) Th. Fr. really contain the same fungal symbiont: "*Xanthoriomyces parietinae.*" In view of the notorious difficulties in the culture of lichen fungi and Tomaselli's (Ciferri & Tomaselli, 1952, 1954) involvement in the publication of more than 150 illegitimate names of lichen fungi, it seems desirable to postpone evaluating his report until the experiments have been repeated by others.

A consideration of the two possible aspects of lichen fungi just mentioned is prerequisite to achieving a realistic nomenclatural rule for the lichen fungi. Yet there is no indication that any real consideration of these matters preceeded the adoption of the present rule.

Some lichenologists now think that to conform to the Code (as well as to produce a truly modern taxonomy), lichenology must be purged of every vestige of phycology. Santesson (1953, 1954a), for example, urges the unification of lichen genera differing only in the alga which they contain [*e.g.*, *Allarthonia* having *Palmella*

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(Tetrasporales) should be united with *Arthonia* having *Trentepohlia* (Ulotrichales)] and the supression of the time-honored considerations of the distribution of algae in apothecia (*i.e.*, lecideoid *vs.* lecanoroid apothecia) (Santesson, 1954b). It is not impossible that these views are taxonomically sound and if they are they will ultimately prevail regardless of what the Code may say. Yet I think that no one has pointed out that if the taxonomist regards symbiont specificity and symbiont distribution within the lichen as physiological attributes of the fungal component and accords them high significance, algal considerations will continue to be reflected in the taxonomy and in the subsequent nomenclature. In fact, those lichenologists who claim that lichens are simply parasitized algae could even point to Art. 4, Recommendation 4A (an impertinent recommendation that invades taxonomy) and say that all lichens with different algae should be designated as *formae speciales*!

But the most elusive aspect — and for taxonomy perhaps the most important aspect — of the algae has not been widely considered by lichenologists. It concerns the lichen substances, water-insoluble, predominately aromatic compounds that accumulate in the lichen thallus (summary: Asahina & Shibata, 1954). The crude chemical information that lichen taxonomists since Nylander have elicited by spot tests on thalli is now giving way to the precise determination of the lichen substances themselves. Specific tests of some 75 substances can now be made on fragments of even ancient herbarium specimens by Asahina's crystallographic methods or by the more recent innovations of paper chromatography. Although there is a great diversity of opinion concerning the taxonomic weight that should be accorded to these biochemical data, few workers seem to doubt that biochemistry can substantially advance lichenology. A small but increasing number of lichenologists are even coming to agree — with Yasuhiko Asahina and the late Alexander Evans — that interpreting those lichens with certain kinds of chemical variations as distinct species has more to recommend it than ignoring them all together or even as treating them as infraspecific variations (*cf.* Culberson, 1961). And a few lichenologists have already produced subtle, elaborate, and highly significant classifications which intimately involve chemistry as a taxonomic criterion.

But now a new problem plagues lichen taxonomists. With the nomenclatural provisions of the Code in mind, we must find out whether the production of these substances is a trait of the fungus, the alga, or the lichen. There are a few reports that lichen fungi grown *in vitro* produce the same lichen substances that the corresponding lichens produce (*e.g.*, Thomas, 1939; Castle & Kubsch, 1949) while others claim that the isolated fungi do not produce lichen substances (*e.g.*, Quispel, 1945; Hess, 1959). These seemingly contradictory reports may reflect no more than the diversity of plants studied or the cultural conditions.

It has been suggested that the alga may indirectly determine the type of lichen substances produced by the particular organic nutrients that it furnishes to the fungus (Asahina, 1937). More recently, Hess (1959) claims that the alga may even participate directly in the biosynthesis of lichen substances. In either circumstance the alga itself would determine the lichen substance(s) produced. It seems most probable that in at least some lichens, one or the other of these situations is actually operative and, consequently, under the Code's remarkable provision for the application of the lichen names, there would be no justification for the use of lichen chemistry in taxonomy. But is the development of lichen chemistry — the most significant achievement in lichenology since Schwendener's discovery of the nature of the lichen — to be casually tossed aside as a taxonomic tool? In fact, the ideal of intercalating the lichen fungi into the general fungal arrangement may in the end be advanced by lichen chemistry. Although almost all lichen substances are thought to be restricted to lichens, there are similar compounds in some nonlichenized

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fungi and recently the only depsidone ever reported outside the lichens was found in a free-living fungus (Dean *et al.*, 1954). It is conceivable that someday a consideration of biochemistry may be more important than some aspects of morphology for achieving the correct placement of the lichen fungi in the mycological system.

A curious sidelight to the problem of legislating the naming of lichen fungi was pointed out by Ahlner (1953): "The nomenclatural problems deriving from the different starting points for lichens [1753] and for nonlichenized ascomycetes [1821] may be more easily resolved with a common starting point for all generic names (1753)." Presumably the mycologists did not select Fries' *Systema* rather than Linnaeus' *Species Plantarum* as their starting point out of mere capriciousness. Shall we ask the mycologists to select a new starting point for the fungi just because we lichenologists have decided to insist that the lichen fungus itself must become the nomenclatural type of lichen names? Are all these disruptions to botany really necessary?

As I have discussed, we do not know to what extent chemistry or certain aspects of the vegetative morphology of lichens are due to the fungus alone. As our knowledge grows we are sure to become more and more aware that the lichen is a whole that exceeds the sum of its parts. Those lichenologists who equate the subtle relationship expressed as a lichen to that of a parasitic fungus producing a leaf-spot disease on a phanerogamic host grossly underestimate the complexity of lichenization. Yet lichen taxonomists have no choice but to study the whole lichen. Insofar as a nomenclature for the lichen fungus itself is concerned, we have only the vaguest ideas of the problems we shall meet. But we need not be driven to hasty nomenclatural legislation through ignorance. The Code now appropriately excludes, for example, such names as "typicus," "genuinus," etc. for the infraspecific taxon containing the nomenclatural type of the species. A clear precedent for "automatic nomenclature" already exists. To indicate the lichen fungus alone, we could simply add "l. f." ("lichenis fungus") or some other qualifier to the lichen name. I do not now formally make this proposal because someone else may have a better one. Furthermore if any truly different lichens are found to contain the same fungal component, additional nomenclatural provisions will obviously have to be devised.

I know that some lichenologists will fear that changing the Code will invite orgies of name-making for lichen fungi. The possibility exists and, considering the irresponsibility of some botanists, will probably be fulfilled. But we do not need a revolution — "an addition of about 20,000 new names to botanical nomenclature" as Santesson (1954a) fears — to solve the problem. We should work towards formulating a realistic provision for designating the existing lichen names for reuse in referring to the lichen fungi, incorporate our decision into the Code, and make it retroactive. To prepare the way for a new rule, I make the following proposals:

Proposal 1. Art. 13. Delete Note 4 (quoted in the first paragraph of this paper).

Proposal 2. Art. 66. Add as Note 1: "The algal and fungal components of a lichen shall not be considered discordant elements."

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NOMENCLATURE AND TYPIFICATION OF TWO BOTRYCHIUMS OF THE SOUTHEASTERN UNITED STATES

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During investigations on the evergreen grapeferns (*Botrychium* subg. *Sceptridium*) of the eastern United States, I have noticed some facts which will necessitate important changes in nomenclature. In checking the original publications of the taxa involved, I came to the conclusion that Savigny's description of *Osmunda biternata* (in Lamarck's *Encyclopédie méthodique: Botanique*, and based upon a plant collected by Michaux in the Carolinas prior to 1800) did not seem to pertain at all to the species traditionally referred to as *Botrychium biternatum* (Sav.) Underwood. The holotype, later located at Geneva, confirms this conclusion. Accordingly, the revised synonymy of the two species involved will be presented below, and the evidence in support of it will follow.

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