

Mycological Nomenclature: Reflections on its Future in the Light of its Past

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Résumé. Recension de l'histoire de la nomenclature des Champignons dans le Code de Nomenclature Botanique. La promulgation de lois d'exception conduit à une complexité sans fin et cela sans avantages compensatoires. Nous postulons que la nomenclature mycologique y gagnerait en stabilité, simplicité et facilité d'application si on réduisait le nombre de lois d'exception.

Introduction

For many years now, it has been argued that fungi do not belong in the plant kingdom and should not be treated as plants. Not only mycologists (e. g. BROOKS 1924, ROGER 1948b, MARTIN 1955, AINSWORTH 1960, et al.) but also other evolutionists (e. g. COPELAND 1956, WHITTAKER 1969, MARGULIS 1974, et al.) have expressed and expounded this point of view. Now, through promotion by VOSS (1975), the several-kingdom hypothesis has become respectable enough to be recognized in the International Code of Botanical Nomenclature, although as yet on a somewhat equivocal basis: "it seems unwise for the Botanical Code to rule, in effect, that all plants must be placed in the plant kingdom". Students of fungi, of certain monerans and protists can look forward to a Nomenclatural Code that will govern the naming of their organisms eventually without requiring that these be "treated as plants" It is indeed wise for changes of kind to begin to enter the Code, lest some of those who consider fungi a separate kingdom also imagine they must therefore have a separate system of nomenclature.

When AINSWORTH (1960) wrote that "the possibly more logical solution to the blurred dichotomy between plants and animals" was "to federate all 'kingdoms' of living things as a single unit", he seemed to me to have expressed the soundest possible approach — not to taxonomy, but to nomenclature. The need for separate Codes and separate individual rules for the nomenclature of taxonomically different recent (i. e. not fossil) organisms is frequently asserted for reasons of taxonomy. But rules of nomenclature do not affect taxonomic treatments. Dealing with the Slime Moulds (Table 1), zoologists under the Zoological Code (LOEBLICH & TAPPAN 1964) and mycologists under

the Botanical Code (MARTIN & ALEXOPOULOS 1969) produce practically identical classifications. Furthermore, they even use practically the same names. It is comforting to discover that the independence of botanical nomenclature from zoological nomenclature (Principle I in the Code since 1956) is not so aggressive as to confuse those who must treat organisms governed by the rules of both.

My purpose here is not to press for a unified Code of Biological Nomenclature, for, I am told, "it is now too late" (BRUMMIT & CHATER 1974: 852). What I shall attempt is to forestall among mycologists any "sense of the crisis which led bacteriologists to develop their own Code of Nomenclature" (AINSWORTH 1960), and to lay a foundation for arresting the non-essential coinage of exclusive rules of nomenclature for fungi. Subcommittees have been organized by the Nomenclatural Secretariat of the International Mycological Association. They are re-examining such long-established customs as the observance of later starting-points and such continuing sources of confusion as the separate nomenclature for Fungi Imperfecti. At the Second International Mycological Congress, these matters will be discussed in open forum and, it is to be hoped, proposals readied then for changes in the Code. In preparation for these open discussions, I present below an abbreviated review of our association with the Botanical Code, speculating on what might have happened to the names of fungi had certain changes not been made at certain points in our nomenclatural history.

Our nomenclatural past

In what is called the First Edition of the Rules, the Vienna Code (BRIQUET 1906), the mycologist who considered his nomenclatural needs different from those of the phanerogamist could be comforted by Art. 9. It stated that "the rules and recommendations of botanical nomenclature apply to all classes of the plant kingdom, reserving special arrangements for fossil plants and non-vascular plants". However, a footnote cautioned that "these special arrangements have been reserved for the Congress of 1910" The mycologist had to wait for the Second Edition of the Rules, the Brussels Code (BRIQUET 1912). There, fungi (unless lichenized or myxomycete) were favoured with nomenclatural starting-dates later than 1753. And fungi that are pleomorphic were allowed separate names for their imperfect states; but priority was vested in the name "given the state containing the form which it has been agreed to call the perfect form" With that, however, there was an explicit reminder that "the nomenclature of fungi which have not a pleomorphic life-cycle follows the ordinary rules"

Consider what would have happened to mycological nomenclature if the footnote to Art. 9 of the Vienna Code had stated: "These special

arrangements have been reserved for the Congress of 1981" In other words, what if mycologists had not rushed to put forth their nomenclatural problems as being so different from those of other botanists (e. g. FARLOW 1910) that they could not allow the nomenclature of all fungi to "follow the ordinary rules"?

Certainly, if no special arrangements had been made for fungus nomenclature in 1910 or since, we should by now have had 75 years' experience with the Botanical Code as it is used by students of vascular plants. If the special arrangements for fungi were essential, mycology would, presumably, be in a deplorable state today. Let us look at one name as a simple example of the consequences, the name *Monilia* as analyzed by DONK (1963). *Monilia* HILL ex WIGGERS of 1780 included *M. crustacea* (L.) WIGGERS and *M. aspergillus* (SCOP.) WIGGERS. We are informed by DONK that the first of these is based on *Mucor crustaceus* MICHELI ex L., a *nomen dubium*, and the second belongs to *Sporodinia* LINK (quod est *Syzygites* EHRENB. ex FR. in present-day nomenclature). As type of *Monilia*, DONK selected *Mucor glaucus* L. — his reason for doing so need not be discussed here — and he might have convinced us of the wisdom of his choice. This would have made *Monilia* HILL ex WIGGERS an earlier synonym of *Aspergillus* MICH. ex LINK.

Would we therefore now be using *Monilia* instead of *Sporodinia* (or *Syzygites*) or *Aspergillus*? Perhaps — and in 75 years, the sting would have gone out of it. Either that, or — had we found it unacceptable — we could have used the then Art. 20 (BRIQUET 1906; now Art. 14) to reject *Monilia*, conserving whatever generic names were most conducive to stability in accordance with mycological literature before 1910. We might have lost a few now-familiar species-epithets along the way, as we have lost *A. glaucus*, shelved as a *nomen dubium* or *ambiguum* (vide RAPER & FENNELL 1965: 148). But many of these names would not have become so familiar if later starting-points had not come into use. And, in any event, no matter how we change the rules, there will always be some species-names that can be saved only by conservation — a convenience so far rejected by all Congresses (but see DEMOULIN 1975: 6).

If, then, no special arrangements had been made for the naming of fungi, we should probably now have a conserved name *Aspergillus* MICH. ex LINK. And, since MALLOCH & CAIN (1972: 2618) have found it feasible to neotypify *Mucor herbariorum* WIGGERS (the ascospore state of *A. glaucus*, fide RAPER & FENNELL 1965: 148), it might also have been possible to neotypify *A. glaucus* (L.) LINK. With that, there would be a usable lectotype for the name *Aspergillus* with application to a genus of ascogenous fungi producing aspergilloid conidia of the *A. glaucus* group, exactly the treatment to be found in

RAPER & FENNEL. Undoubtedly, over the years, innumerable fungi introduced into *Aspergillus* because of similar conidial or ascogenous states would have had to be removed to other genera. But taxonomically justified transfers are unavoidable whatever the rules of nomenclature. And meanwhile, a huge load of controversy would have been prevented, innumerable pages of discussion on names and their application would never have been published, and who knows how many other problems might have been solved instead. So it might have been if (i) priority of fungus names had followed a 1753-starting-point, and (ii) there had never been a legalization of what DONK (1960a: 103) has termed "a conventional system of nomenclature, with pseudo-specific and pseudo-generic names", i. e. *nomina anamorphosium* (DONK 1960b) for Fungi Imperfecti.

But the footnote to Art. 9 of the Vienna Code postponed special arrangements for non-vascular plants — not for 75 years, but for five. Let us look first at starting points.

Art. 19 (BRIQUET 1912) left the nomenclature of lichens and myxomycetes to begin with LINNAEUS, Sp. Pl., ed. 1, 1753, but moved rusts, smuts and gasteromycetes to an 1801 birth with PERSOON, Syn. Meth. Fung., and the rest of the fungi to begin with FRIES, Syst. Myc. 1821—1832. Between the First World War and the Second, there was time for only one clarification. This appeared in the Cambridge Rules (BRIQUET 1935), where "Botanical nomenclature begins " was changed to "Legitimate botanical nomenclature begins "; in other words, pre-startingpoint names were ruled illegitimate. But what did 'illegitimate' mean in this context? It was clear what it meant with reference to pre-1753 names: they were ruled out of consideration because they were published in works in which the Linnaean system of binary nomenclature for species was not consistently employed. But how illegitimate were names published between 1753 and later starting-dates? Apparently WAKEFIELD (1940: 283) und ROGERS (1941: 570) took 'illegitimate' to mean 'invalid'. But when the first line of the Article was changed (LANJOUW et al. 1952) to "Valid publication is treated as beginning ", DONK (1957: 246) interpreted this to mean that, before 1952, pre-starting-point names had been considered validly published though illegitimate [and thereby, earlier homonyms of post-startingpoint names]; they had then been devalidated by the changed phrase, with the provision for revalidation by publication on or after the starting-date. There was still no agreement on the status of these names. Only one thing was clear: rejecting 1753 as a starting-date and LINNAEUS's Species Plantarum as a starting-point book had flung mycologists into a special set of uncertainties.

Among the problems was FRIES's Systema itself. Because it is in

several parts (for dates, see ROGERS 1952), a decision had to be made on the status of names that were published between the date of *Systema I* in 1821 and that of *Systema III* (2) in 1832/33 (BISBY & MASON 1940, WAKEFIELD & BISBY 1941, BISBY et al. 1942). Furthermore, it was necessary to fix the precise date in 1821 when *Systema I* appeared, since its publication date would decide the validity of names in at least half-a-dozen other books (see ROGER 1941, 1944, 1951, 1952). And so, an attempt to deal with these problems came out in the Stockholm Code (LANJOUW et al. 1952). *Systema I* was arbitrarily dated January first 1821. This settled the validity of names published in other 1821 books, and presumably the chronological priority of all names published after the first of January 1821. However, the *Systema* as a whole retained its standing as a starting-point book, its names shielded from competition with homonyms and synonyms.

Starting-point matters for Fungi Caeteri have remained in this state since 1952. Are we now comfortable with Art. 13? Apparently not. Questions continue about the typification of validated pre-startingpoint names (DONK 1957, DOSTÁL et al. 1958: 278, SINGER 1965), which is a problem of the interaction between Art. 7 and Art. 13. Different interpretations continue, proposals for changes in dates, and discussions of them have not stopped (DIEHL et al. 1954, HUGHES 1959, MARTIN 1960, SINGER 1960, VASSILKOV 1960, ECKBLAD 1968: 7—10, HOLM 1974). And so, a subcommittee has been established by the I. M. A. Nomenclatural Secretariat to restudy the matter of starting-points, considering — among other proposals — a return to 1753 (PETERSEN 1975). It may be that (KOMÁREK et al. 1959) “many of the unsatisfactory consequences of a ‘later starting point’ experienced by the mycologists are due to the fact that their ‘later starting points’ were not chosen properly in many cases” But whatever the reason, it seems to me that, if the footnote in the Vienna Code had postponed special arrangements for fungi until 1981, later starting-points would not be on the agenda.

Now, how about the rules for ‘plants’ with a pleomorphic life-cycle?

If no special arrangements had been made at the Brussels Congress of 1910 or since, the names of pleomorphic ascomycetes and basidiomycetes would have followed the ‘ordinary’ rules that regulate the nomenclature of phycomycetes, lichens, myxomycetes and all recent chlorophyllaceous monerans, protists and plants. There would have been problems enough to keep us busy. But by this time, most of the generic names of importance would have been conserved if otherwise unavailable, and typified to suit our purpose. We should have become accustomed, after 75 years, to the use of a single botanical system (see HENNEBERT 1971) for naming our fungi. And in fact the logic of

it would have become second nature and perhaps overcome our masochistic tendency to make nomenclatural matters ever more complicated.

Nevertheless, special arrangements did enter the Brussels Code (BRIQUET 1912), giving formal recognition to two nomenclatural series for pleomorphic fungi, a perfect-state series composed of names comparable to 'ordinary' botanical names for whole organisms, covering "the different successive states of the same species", and a conventional series (DONK 1960a) of *nomina anamorphosium* (DONK 1960b) available for imperfect states but not interfering with the botanical series, having "only a temporary value" because they represent "inferior" states. As soon as the type method was formulated (Art. 18, Cambridge Rules, BRIQUET et al. 1935), mycologists seized it as a sound rationalization and basis for the two-series system (see BISBY 1944, ROGERS 1948a). They incorporated it in a rephrasing of the Article in the Stockholm Code (LANJOUW et al. 1952), strengthening both the type method and the two systems of naming pleomorphic fungi, and yet leaving the article "in essence, unchanged" (see ROGERS 1948a: 252).

The changes made, however, were significant. First, phycomycetes and lichens were explicitly excluded from the jurisdiction of the Article, their nomenclature traditionally having followed the botanical system (see BISBY 1942). But to mycologists working with non-lichenized pleomorphic ascomycetes and basidiomycetes, it was explained that, for a name to be considered as applying to one state or another, i. e. as applicable to the perfect state or only an imperfect state, "the type specimen must bear that state". And "the author who first describes a perfect state [presumably in accordance with the rules for valid publication of a name] may adopt the specific epithet of the corresponding imperfect state, but his binomial . . . is not to be regarded as a transfer"

It was a most acceptable solution to the demand for a separate nomenclatural series for the imperfect states of pleomorphic fungi. A name being tied to its type, and now (as in our present Art. 13), the group to which a name is assigned being determined by the accepted taxonomic position of the type of the name — if the Article had remained unchanged and interpreted literally, the examples in our current Art. 59 would be disposed as follows:

- (i) *Ravenelia cubensis* ARTH. & JOHNSTON, based on a specimen bearing only uredinia and therefore a *nomen anamorphosis* in spite of the use of *Ravenelia* (a perfect-state genus), would be appropriately transferred, in accordance with its type specimen, to *Uredo cubensis* (A. & J.) CUMMINS.
- (ii) Re: *Mycosphaerella aleuritidis*, published by OU as a *comb. nov.* with the *nomen anamorphosis* *Cercospora aleuritidis* as basionym: Ou was permitted by the Article to adopt *aleuritidis* as the epithet for the perfect state, but he

was not permitted to do so by means of a transfer from the *nomen anamorphosis*. Having done so, since a new combination is tied to the type of its basionym, OÜ has made *M. aleuritidis* (MIYAKE) OÜ a *nomen anamorphosis* in spite of fulfilling all the requirements for valid publication of a new perfect-state species.

- (iii) So, too, with *Corticium microsclerotium* (MATZ) WEBER of 1939: being an explicit new combination for *Rhizoctonia microsclerotia* Matz, it remains a fully legitimate new combination and *nomen anamorphosis*, while *C. microsclerotium* WEBER of 1951, validly described for the perfect state, becomes a later homonym.

As is obvious, I agree with DONK (1960b: 173) on the interpretation of this part of the Article and the example he gave of correct application:

- (iv) *Candelospora ilicicola* HAWLEY, a nom. anam., was transferred to another form-genus as *Cylindrocladium ilicicola* (HAWLEY) BOEDIJN & REITSMA at the same time as a new species was described, *Calonectria ilicicola* BOEDIJN & REITSMA, for the perfect state.

Another example of what I consider to be correct interpretation of this Article is borrowed from BISBY (1953: 97):

- (v) *Uredo dianthi* PERS., based on material bearing teliospores whose presence (though not identity) was noted in PERSON'S description, is properly transferred to a perfect-state genus as *Uromyces dianthi* (PERS.) NIELSS.

This is how it might have been, had Art. 69 in the Stockholm Code been taken at its word.

Instead, as BISBY (1953: 117) demonstrated, the Article could be taken as meaning more than it said. Although no reference was made in it either to valid publication or legitimacy, and it was not among the Articles cited as consigning names to illegitimacy as punishment for not conforming, yet Bisby interpreted the Article to rule on both validity and legitimacy when transfers were made from *nomina anamorphosium* to perfect-state genera. He said: "the common practice of transferring names is now legitimate only when " And he treated example (ii) above in these words: "A perfect state described as '*Mycosphaerella aleuritidis* (MIYAKE) OÜ nov. comb., syn. *Cercospora aleuritidis* MIYAKE' is now to be cited as a new species *M. aleuritidis* OÜ".

The controversy has been adequately discussed by DONK (1960b) and DEIGHTON (1960). In spite of the fact that Art. 69 in the Stockholm Code (LANJOUW et al. 1952) apparently did no more than strengthen the recognition of the two nomenclatural series for pleomorphic ascomycetes and basidiomycetes by tying each series to the type specimen of a name, and although the Article remained fundamentally unchanged through three Codes, it was obvious almost before it was published that it would have to be rephrased to eliminate the possibility of differing interpretations. DEIGHTON'S (1960: 240–241) proposed revision (a) brought back a clear statement that the perfect-

state name is, unlike a *nomen anamorphosis*, a name in the sense of all botanical names, meant to cover the whole organism; (b) ruled explicitly illegitimate a name published for an imperfect state but placed in a perfect-state genus; (c) ruled invalid (and therefore non-existent) the transfer from a *nomen anamorphosis* to a perfect-state genus, and (d) until 1967, transformed this explicit new combination into a new name for a perfect-state species, as long as it satisfied the requirements for publication of a new species and was based on perfect-state material.

Many of us who were at the Edinburgh Congress of 1964 hailed DEIGHTON's proposal as a most adroit handling of some very knotty problems. It was accepted with little change and is the basis for Art. 59 as we know it now (LANJOUW et al. 1966, STAFLEU et al. 1972). We who were so delighted with it were soon made aware that it did not solve all problems (see HENNEBERT 1971, RAO 1973); but certainly we failed to notice wherein real danger lay, in its effect on other, sometimes very fundamental, parts of the Code. Nor did we anticipate how it might be used in attempts to divorce mycological nomenclature from the type method (see below).

The current Code in the light of Art. 59

In the section on **TIPIFICATION**, ARTS. 7—10, the Code makes no attempt to specify what kind of material is to be used as type, except that it must be permanently preserved or at least in the form of a description or figure. Art. 59 does make a specification. It specifies what characteristics must pertain to the type specimen of a name in our perfect-state series and our conventional series. This seems harmless, no more than a re-statement of the ruling that a name is applied in accordance with its type — a necessary re-statement, apparently, being something of a definition of what is meant by the two nomenclatural systems.

In this section, emphasis is given to application in accordance with the type, whether the type of a name or of the basionym of a combination. Art. 59 has its own view of these matters (see below, under Arts. 33 and 55).

In the sections on **PRIORITY**, ARTS. 11—15, the correct name for every plant is established in accordance with priority in one uninterrupted chronological series. The fact that Art. 59 splits fungus nomenclature, into the perfect-state series like the botanical one and a separate conventional one for imperfect states, creates no problem for other users of the Code. Interestingly enough, however, this matter is so little understood in the Code that Art. 11 cites Art. 59 as permitting more than one correct name for a taxon (see WERESUB et al. 1974). Yet, a perfect-state taxon has a circumscription (i. e. the species in

all its states), position and rank (within the perfect-state series) different from those of a form-taxon with a circumscription limited to a single imperfect-state within the conventional series. The taxon that is called *Sclerotium durum* PERS. ex FR., and the one called *Botrytis cinerea* PERS. ex PERS., both belong to one species that is called *Botryotinia fuckeliana* (DEBARY) WHETZEL (see HENNEBERT & GROVES 1963: 342); but these are taxa with different circumscriptions, position and rank, *B. fuckeliana* being what DONK (1960b: 172) calls the true species, *Botrytis cinerea* and *S. durum* being pseudo-species, i. e. form-species. Although one of these includes the other two, their names are not synonyms, for they do not conform in application, they are not interchangeable and do not challenge each other's priority. Art. 59 does not sanction several names for a single taxon.

It is when we come to the CONDITIONS and DATE of VALID PUBLICATION of NAMES (Arts. 32—50) that we begin to set up our own rules for what is supposed to be the same as the botanical system of nomenclature. Many of the conditions of valid publication for the names of new taxa are tied in with specified dates (see also LEUSSINK 1975): as of 1908, 1935, 1953, 1958, additional requirements progressively restrict the conditions for validation, the first a description or diagnosis rather than just "an illustration with analysis showing essential characters", then this diagnosis in Latin, with clear indication of rank, and finally with the nomenclatural type indicated. Another date is added by Art. 59, paragraph 4: until 1965, as long as the Code's requirements are fulfilled as of the dates specified, validity as the name of a new species is conferred on the new combination of an epithet from a *nomen anamorphosis* to a perfect-state genus if accompanied by the description of material characterizing the perfect state. But after 1967, such new combinations may no longer be considered validly published names for new species. This was a ruling introduced to deal with a few controversial problems and strictly limited to avoid further uncomfortable illogicalities of the same kind. It was certainly not foreseen that anyone could consider perverting this provision from its emphasis on the type and protologue as deciding whether a name is a *nomen anamorphosis* or a perfect-state name, to a total dependence on the decisiveness of the choice of a generic name (HAWKSWORTH & SUTTON 1974a) "according to principles already inherent in the Article" (HAWKSWORTH & SUTTON 1974b).

Art. 33 rules on valid publication of a new combination, requiring no more than the definite use of it and, as of 1953, a full and direct reference to the basionym. Art. 59 adds that, if this new combination is of the kind referred to above, namely, transferring an epithet based on an imperfect state to a genus of perfect-state fungi, it is not validly published as a new combination. No notice is given in Art. 33 to this

restriction added by Art. 59. Nor was it ever anticipated that its purpose of preventing incorporation of imperfect-state types in perfect-state genera might be misrepresented as implicitly condoning an extension to the prevention of transfers of perfect-state types from form-genera to perfect-state genera (HAWKSWORTH & SUTTON 1974a & b).

Art. 34 rules, among other things, that a name is not validly published "when it is proposed in anticipation . . . of a particular circumscription, position or rank of the group (so-called provisional name)". This is the ruling that was used by CUMMINS (1956) and by GROVES & ELLIOTT (1969) to exclude from validity such names as *Coleosporium eupatorii* ARTHUR of 1906 and *Sclerotinia alni* MAUL of 1894, respectively. Both names were based on imperfect-state material but published in perfect-state genera in anticipation of the discovery of fruiting material that would confirm their authors' understanding of the taxonomy of these *anamorphoses*. Art. 59, paragraph 3, treats names of this kind differently (see below under Rejection of Names). Therefore, HAWKSWORTH & SUTTON (1974a: 567) proposed an addition to Art. 34 excluding such names from its force, instead of eliminating the more arbitrary and less logical ruling in Art. 59.

Another part of Art. 34 is pertinent, its final paragraph referring to Art. 59 as somehow permitting alternative names for the same taxon. As mentioned above, in the discussion on Art. 11, the several names permitted by Art. 59 are not "alternative names for the same taxon", but more accurately "separate names for the perfect and the imperfect states of Fungi" (LEUSSINK 1975: 198).

In CHAPTER V of the Code, ARTS. 51—61 cover RETENTION and CHOICE of NAMES and EPITHETS. Here we must look at Art. 55, last paragraph, the result of a long, hard fight at the 1935 Congress in Amsterdam (SIRKS 1936: 347—354). It irrevocably fixes a *comb. nov.* to the type of its basionym, no matter how obvious the error made by the author of the new combination. According to Art. 59, however, the kind of *comb. nov.* referred to above (under Art. 33) is different; so different that, until 1967 it is not to be accepted as tied to its basionym (a *nomen anamorphosis*) because the author of the new combination obviously applied it to the perfect state. This appears to disregard Art. 55.

Yet it may be argued that it does not. The precept in Art. 55 is expressed as bearing on a case where the "epithet has been applied erroneously to a different species". In the Art. 59 case, it may be said, there is no erroneous application to a different species because the perfect-state name (typified by the perfect form) covers the whole species, including the imperfect state given the *nomen anamorphosis* that was used as basionym. Nevertheless, as DONK (1960b: 173) points out, technically the recombination is a misapplication, for,

although a form-species may belong to a perfect-state species, it is a different taxon in a separate series, and its name is differently typified. In any case, the ruling in Art. 7, paragraph 10, is clearly applicable to the new combinations in dispute: "A new name formed from a previously published legitimate name or epithet (*stat. nov.*, *comb. nov.*) is, in all circumstances, typified by the type of the basionym".

Therefore it can be said that Art. 59 requires exception for special cases from both Art. 7, paragraph 10, and Art. 55, last paragraph. It is, however, an exception of a particular kind. It does not actually divorce the *comb. nov.* in the perfect-state genus from the imperfect-state type of its basionym; instead, it first rules this kind of new combination invalid (see above, under Art. 33) and therefore non-existent. No threat to the simultaneous or later publication of the identical binomial for a perfect-state species.

Finally, the section on REJECTION OF NAMES AND EPITHETS: With the deletion of Arts. 70 and 71 at the Leningrad Congress, the number of Articles that force illegitimacy on validly published names has dropped. The feeling seems to be that, as long as a name is validly published and typifiable, applying it in terms of its type should be unavoidable if practicable. Art. 59, paragraph 3, demands an exception. When a taxon is described and placed in a perfect-state genus, but the description covers only an imperfect-state, its name — though validly published — is ruled illegitimate. Mycologists could have denied validity to such names as provisional or anticipatory (see above, Art. 34). Instead, Art. 59 rules these names validly published but not to be applied in accordance with their types because of the incorrect choice of a generic name (see WERESUB et al. 1974). And HAWKSWORTH & SUTTON (1974a) have proposed to extend illegitimacy to validly published names typified by perfect-state material but placed in form-genera.

Concluding Comments

Such is the International Code of Botanical Nomenclature as viewed through the mist created by the "special arrangements" that mycologists have considered essential to the stabilization of the nomenclature of fungi. Every concession to a particular problem becomes a precedent that serves as basis for the demand for further concessions. And every bit of stop-gap legislation requires a complex formulation to keep it from encroaching on other rules. We have forgotten that, as MARTIN (1960) pointed out, originally (BRIQUET 1906) it was a 'leading principle' that "the rules of nomenclature should neither be arbitrary nor imposed by authority. They must be simple and founded on considerations clear and forcible enough for everyone to comprehend and be disposed to accept."

Table I

Zoological Code: LOEBLICH & TAPPAN (1964)		Botanical Code: MARTIN & ALEXOPOULOS (1969)	
Order	MYCETOZOIDA	Subdivision	MYXOMYCOTINA
Subord.	EUMYCETOZOINA	Class	MYXOMYCETES
		Subclass	CERATIOMYXOMYCETIDAE
Superfam.	CERATIOMYXACEA	Order	CERATIOMYXALES
Family	Ceratiomyxidae		Ceratiomyxaceae
	<i>Ceratiomyxa</i>		<i>Ceratiomyxa</i>
		Subclass	MYXOGASTROMYCETIDAE
Superfam.	TRICHIACEA	Order	LICEALES
Fam.	Liceidae		Liceaceae
Subfam.	Liceinae		
	<i>Licea</i>	Family	<i>Licea</i>
S. f.	Reticulariinae		Reticulariaceae
	<i>Reticularia</i>		<i>Reticularia</i>
S. f.	Tubiferinae		
	<i>Tubifera</i>		<i>Tubifera</i>
S. f.	Lycogalinae		
	<i>Lycogala</i>	Family	<i>Lycogala</i>
			Cribrariaceae
S. f.	Cribrariinae		
	<i>Cribraria</i>		<i>Cribraria</i>
		Order	TRICHIALES
Fam.	Trichiidae		Trichiaceae
S. f.	Trichiinae		
	<i>Trichia</i>		<i>Trichia</i>
S. f.	Arcyriinae		
	<i>Arcyria</i>		<i>Arcyria</i>
S. f.	Prototrichiinae		
	<i>Prototrichia</i>	Family	<i>Prototrichia</i>
			Dianemaceae
	<i>Dianemina</i>		<i>Dianema</i>
Superfam.	STEMONITACEA	Order	STEMONITALES
Fam.	Stemonitidae	Family	Stemonitaceae
S. f.	Stemonitidae		
	<i>Stemonitis</i>		<i>Stemonitis</i>
S. f.	Colloderminae		
	<i>Colloderma</i>		<i>Colloderma</i>
S. f.	Amaurochaetinae		
	<i>Amaurochaete</i>		<i>Amaurochaete</i>
		Order	PHYSARALES
Fam.	Physaridae	Family	Physaraceae
S. f.	Physarinae		
	<i>Physarum</i>	<i>Physarum</i>
		Family	Didymiaceae
S. f.	Didymiinae		
	<i>Didymium</i>		<i>Didymium</i>

That the Code is not simple, not clear or comprehensible, that it is becoming more and more authoritarian and arbitrary is due not only to the demands of the mycologist. SILVA (1960: 5), an algologist, has written that "basically, the Code is beautifully simple and provides a stable, yet flexible, nomenclature to accommodate any taxonomic arrangement. Unfortunately, exceptions and needless elaboration, coupled with failure to complete the change-over from the circumscription to the type method, have resulted in a Code which is verbose and in certain respects difficult to interpret and apply". Problems exist for all followers of the Botanical Code. Witness the composition of Subcommittee F of the I. M. A. Nomenclatural Secretariat: there are more non-mycologists than mycologists now involved in analyzing the difficulties of interpreting the apparently simple statement in Art. 10 that "the type of a name of a genus is a species" And a special committee to study this problem has been established also by the I. A. P. T. General Committee (Voss 1976: 174).

Undoubtedly, there are nomenclatural problems that are genuinely peculiar to mycology, such as those under discussion by other I. M. A. Subcommittees — the need for living types when dried specimens convey little or no usable diagnostic information, and the fungus taxa (*formae speciales*) whose names are not now under the jurisdiction of the Code. But the Subcommittees on Art. 59 and on starting-points have discovered, it seems to me, that the emphasis on the different nomenclatural needs of mycologists has been misplaced. We could have had a simpler, clearer, more comprehensible and less arbitrary Code if we had joined our fellow users of the Code in perfecting the rules followed by all, and had demanded exceptions only after at least a generation or two of use.

This is the lesson of our past. Different rules of nomenclature do not change taxonomic treatments: the taxonomy of the Slime Moulds remains the same under the Botanical and the Zoological Codes. Nor does there appear to be any evidence for the frequently repeated contention that taxonomic differences demand different rules of nomenclature: under the "International Rules of Botanical Nomenclature, chiefly of vascular plants" (BRIQUET 1906), *Monilia* or a conserved *Aspergillus* might have been the name we used for today's *Eurotium*, but the taxonomy of ascomycetous aspergilli of the *glaucus* group would not change.

In this year of the Second International Mycological Congress, when the Nomenclatural Committees bring in their reports, we shall have a chance to weigh what has happened and what might have happened against what is proposed for the future of mycological nomenclature. There will be proposals for additional "special considerations" for fungi in the Code. Other submissions will urge deleting

or reducing the numbers of these exceptions. I am convinced that the fewer the exceptions, the easier the application of the rules, and therefore the sooner we will achieve nomenclatural stability. Should there be arguments against change on the basis that it is now too late, I remind you of MARTIN's (1960) wise words: "if a considerable number of mycologists believe the adoption of the present rules was unwise and makes for undue complication and confusion, it is highly desirable that a change in the direction of simplicity and clarity be seriously considered before it becomes even later".

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