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TYPIFICATION AND LATER STARTING-POINTS

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The present "International Code of Botanical Nomenclature" (1956) contains a Principle (III) stating that, "The naming of taxonomic groups is based on priority of publication." This sentence is to be amplified in the mind by the clause, "except when expressly limited." That the priority principle is a heavily restricted one, is well known, and mycology, certain sections of algology, and other groups of cryptogams are involved in this connection by starting-points of a later date than May 1, 1753 (Linnaeus, "Species Plantarum") as specified in Art. 13. Thus, for the Hymenomycetes (belonging to the 'Fungi caeteri' of Art. 13) the starting-point *date* is January 1, 1821 and the corresponding starting-point *book* is Fries's "Systema mycologicum" (1821—1832). According to Art. 13 valid publication of names is treated as beginning with the specified dates, and this implies that all names before the later starting-point dates are not validly published, even those published between the originally general starting-point (Linn., Sp. Pl. 1753) and the later ones. Here, then, we are confronted with a whole-sale restriction of the priority-principle.

The selection of Linnaeus's "Species Plantarum" as the originally general starting-point for the binomial system of nomenclature for all plants was an obvious choice that needs no discussion here. The reason for deviating from it and for introducing later starting-points for many groups of cryptogams, is less evident now. It is a well known fact that the organizers of the International Botanical Congress at Vienna (1905) wanted to neutralize as much as possible of Otto Kuntze's sweeping nomenclatural reforms and they had an ample majority of votes to that purpose at hand. One of the means to dam the flood was a revised formulation of A. de Candolle's "Lois" (Paris, 1867). To protect the *phanerogams* additional action was taken by means of a long list (about 400 names) of nomina conservanda drawn up by Dr. H. Harms. The list passed by a huge majority (118 to 37). This amounted to the rejection of more than 400 specified and many additional, but not especially mentioned, generic names. Rejection, in this case, meant being completely or conditionally excluded from priority. But Kuntze had also dealt with the *cryptogams*; and, moreover, the menace of the rival "American Code of Botanical Nomenclature" (1907), as exemplified for instance by the work of Underwood, Earle, and Murrill,

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dawned upon the legislators, so they took drastic action in favour of the cryptogams at the Brussels Congress (1910) by fixing later starting-points. This coup did not restrict the rejection of names to generic ones: all names of the binomial system and Tournefortian generic names published after 1753 of certain specified groups published during certain periods were rejected, inclusive of specific ones. In addition, lists of *nomina conservanda* for cryptogams were opened. It is quite plain that the acceptance of the later starting-points ruled out at one stroke quite a good many embarrassing old names in a similar manner as the *nomina rejicienda* were ruled out. It is interesting to note that the principle to which all Congresses from Vienna onwards have so persistently clung, viz. that of not admitting *nomina specifica conservanda* (*rejicienda*), was severely violated in this way!

EXAMPLE 1.—When Fries in 1821 preferred the name *Polyporus lucidus* (Leyss.) Fr. he made a choice from a handful of available metonyms, but he did not choose the earliest of these. When Murrill (1902) called the same fungus species *Ganoderma pseudoboletus* (Jacq.) Murrill he was after all quite correct: the basynym *Agaricus pseudoboletus* Jacq. (1773) is of an earlier date than *Boletus lucidus* Leyss. (1783). The introduction of the later starting-points changed this situation by giving precedence to Fries's names in the "Systema" and later names dating from after the appearance of the first volume of the "Systema."

What became the status of the rejected names? Due to the notorious confusion in connection with expressions like 'validly published,' 'legitimate,' and 'correct,' and their antonyms (and others) the answer was not clear until the Stockholm Code (1952: Art. 23) was published. The Brussels Rules (1912: Art. 19) merely state that 'botanical nomenclature' begins for different groups at different, specified, dates. In the Cambridge Rules (1935: Art. 20) one reads instead, 'legitimate nomenclature.' In the Stockholm Code and in the present one (Art. 13), 'legitimate nomenclature' has been changed into 'valid publication of names.' During the Stockholm Congress it still could be defended that the pre-starting-point names were 'illegitimate' (validly published, but *impriorable*); at present these names appear to be 'not validly published.' The status of the generic names rejected in favour of individually conserved names have taken another course of evolution: they are now, undoubtedly, to be taken as validly published.

The following historical recapitulation may be of some use. Names of the binomial system and Tournefortian generic names published after 1753 and before 1910 were validly published if they complied with the provisions of the rules of nomenclature. Then, in 1910 (and at still later dates) all those belonging to certain groups of cryptogams with special later starting-points and published before the specified dates, were retroactively rejected and in 1950 it was decided that they became not validly published names. Not until 1930 was the type-method incorporated among the Rules, but it was *never* made clear how this would affect the typification of pre-startingpoint names that were taken up in the starting-point book or after the starting-point date.

At this point of the discussion a comparison may be introduced between the 'natural' starting-point, and the later ones that might be contrasted as the 'artificial' starting-points. On a whole our Code is one that governs the legacy of the Linnean system of binomial nomenclature and it was quite logical that originally one general starting-point was taken for all plants: Linnaeus's "Species Plantarum." That book not only introduced the binomial method, it also introduced a complete application of the method to the whole plant kingdom. Moreover, it completely altered in an astonishingly short time the course of botany; the overflow from the pre-Linnean area was small and stopped surprisingly soon. The "Species Plantarum" was spon-

taneously accepted at once as the starting-point book for both the nomenclature of plants as well as for their taxonomy.

The situation is quite different indeed as regards the later starting-points. First, when the earliest of the future starting-point books was published, the binomial system had already taken its course and had been in general use for about 50 years; and when the most recent one of them was published, about 150 years had lapsed. Secondly, the impact of these later works may have been considerable, yet it never was so enormous as that of the "Species Plantarum." Thirdly, none of these later works rung in a new era of a new system of nomenclature, although they were important from a taxonomic point of view. Fourthly, they were not spontaneously and unanimously accepted as new starting-point books at their appearance; it was only some time after their publication (from more than 100 to 10 years later) that they were given their new importance; and it can not even be claimed that they have even now universal support. Fifthly, what was ruled out was a considerable portion of the results of the application of the binomial system itself: quite a different situation from that when the "Species Plantarum" appeared, for up to that time the Linnean binomial system did not exist.

To simplify the discussion a few terms may now be defined. I call *devalidated names* all names that would have been considered validly published if no later starting-points had been introduced (Donk 1951: 202, and earlier publications). Devalidated names that were validly published afterwards may be termed *revalidated names*. Since devalidated names are no longer validly published ones, they do not render illegitimate (impriorable) later homonyms, but it should be remembered that the introduction of a later starting-point (previously and in the future) may bring in its train considerable changes in the nomenclature of *all* plants by restoring legitimacy to those later homonyms that have their earlier counterparts among the names that become devalidated.

Opinions on typification of revalidated names

As to the typification of revalidated names, a search through recent mycological literature will show that two different opinions exist. Most authors who have cared to motivate their views start from the following premise. Since a revalidated name acquires its status of a validly published one by virtue of the accompanying description, the latter is decisive when a choice of type has to be made. The original description of the corresponding devalidated name is comparable to one pasted on a herbarium sheet. If the revalidating description was based on a different species from that originally included, then the type of the revalidated name will be the species that was incorrectly named by the revalidating author.

Compare Rogers & Jackson (1943: 283) in a discussion of a species belonging to the fungi that have 1821 as their starting-point date:

"What is more important is that it is not allowable to define *T[helephora] granulosa* merely from Persoon's [original] concept and his specimens; that species was 'validated' (as some would have it) by publication in Fries's 'Systema,' and must forever be seen through Fries's eyes."

Some authors even go much further and would only select the type from the specimens actually seen (if any) by the author when he drew up the revalidating description. They go so far as to exclude a priori the original specimen(s) of the

devalidated name if the revalidating author is known to have seen material, even in cases in which the revalidating description fully covers the original specimens or is merely a copy of the original description! This amounts to excluding all pre-starting-point specimens from consideration in all cases, for instance, where Fries added "v.v." (*vide vivo*) to his descriptions in the "Systema mycologicum." Compare Singer (1951: 122-125).

EXAMPLE 2.—When the name *Clavaria abietina* Pers. 1794 was revalidated by Fries in the starting-point book ("Systema") in 1821 he described a different species from the original one. The fungus he had in mind at that occasion has also been called *Clavaria invalii* Cotton & Wakef. 1919. Because of Fries's description, Donk (1933) felt bound to apply the name *Clavaria abietina* Fr. "(non Pers.)" to *C. invalii*. The Persoonian species then required another name and hence *Clavaria ochraceo-viridis* Jungh. 1830 was taken up for it.

EXAMPLE 3.—The name *Hydnum farinaceum* Pers. 1801 was revalidated by Fries in the starting-point book ("Systema") in 1821 with an accompanying diagnostic phrase which (except for a slight unimportant alteration) included the whole of Persoon's original phrase. Lundell (1953) concluded from a re-examination of "the authentic material" from Femsjö in Fries's herbarium that the name *Hydnum farinaceum* Pers. ex Fr. had to be rejected, since none of the specimens agreed with the current interpretation of *Hydnum farinaceum* Pers., though specimens of the latter fungus exist so named by Persoon himself (Herb. Persoon, Leyden).

EXAMPLE 4.—"*A[rmillariella] mellea* was described from Denmark and validated by Fries in 1821 on the basis of a description drawn from Swedish material. The species as it occurs in Sweden should therefore be considered as the typical *A. mellea* . . ."—Singer (1956: 176).

It will be evident that the above developed principle of typification depends on the assumption that the revalidation of a name needs an accompanying description. The Rules previous to the Stockholm Congress (1950) sharply distinguished between two categories of new names, viz. (i) new names for new taxa (to be accompanied by a description), and (ii) new names for already named taxa, viz. name changes (merely a reference to the description under the basonym, thus *under another name*, being required). Starting from this basis, one proceeded to conclude that a taxon with a devalidated name (that is, with a not validly published name) was nameless under the Rules and had to comply with the requirements necessary for valid publication of the name of a new taxon: the revalidation, therefore, required an accompanying description. In many instances the reference to the devalidated name would have been ruled out anyhow also because it was to a description under the 'same' name. The conclusions appeared logical and consistent with the then prevailing Rules. Yet, after carefully giving these questions renewed consideration, I now believe that the Rules from before 1950 (Brussels, Cambridge, Amsterdam) as a whole were not formulated with the later starting-points in mind.

To day many mycologists still take the above views on typification for granted and in no need of elaborate defence. Moreover, they act as if no important technical difficulties at all have arisen and that everything runs smoothly. This is certainly not the case and I have earlier offered some suggestions for improvements (Donk 1949: 273-287).

Miss E. M. Wakefield (1951: 139) criticized the above procedure:

"Fries took up the name *Clavaria abietina* Pers. in Syst. Myc. I (the starting-point for the nomenclature of the group), but unfortunately identified it with a common Swedish species which does not turn green (= *C. Invalii* Cotton & Wakef.). Persoon's greening species he thought to be a variety only. Persoon's specimen of *C. abietina* exists at Leiden, and the [author] is strongly of the opinion that this should be the type specimen for *C. abietina* Pers. ex Fr., and not Fries's misidentification."

Here, then, is a quite different guiding principle. It considers a devaluated name and its revaluated counterpart as both based on the original, pre-startingpoint, type. This opinion became defensible when at the Stockholm Congress (1950) references to descriptions irrespective of names were admitted. (Code 1952: Arts. 32, 33, 39). Previous to this the mere reference to a description accompanying a devaluated name was not admissible as a means of revalidation (in the opinion of many mycologists), the devaluated name not being a 'name' under the Rules (not validly published; Cambridge Rules: Art. 19).

In all cases where the revalidation of a devaluated name is accompanied by both a reference and a description, the present Code seemingly allows us to consider the revaluated name (retroactively) validly published in two different manners of equal standing at once: first, by the reference to the description under the devaluated name, and, secondly, by the accompanying description. However, it should be remembered that the Stockholm Congress *intentionally* admitted valid publication by means of a reference ("direct or indirect") to a pre-startingpoint description. This shows that pre-startingpoint taxa, when they receive a validly published name, do not come under Art. 34 (dealing with names of 'new' taxa), but are to be considered as legally already existing taxa.¹ Hence, one would conclude, it is the reference to the description that has to prevail: the description of the revalidating author, not being essential, is purely supplementary.

Proposal and supporting arguments

I would now propose that the principle for the typification of revaluated names defended by Miss Wakefield be definitely accepted as the 'legal' guiding one.

One reaction against any change from his point of view has been published in advance by Singer (1951: 123).

"A [further] problem is very difficult. It concerns the habit of all modern taxonomists who follow the rules at all, to consider a pre-Friesian name, validated according to Art. [13 (e) and (f)] by a post-Friesian author as based on the specimen or description of the latter. It is, as has been pointed out to me by M. A. Donk in a very interesting discussion on the subject, rather questionable whether or not this customary procedure conforms with the intention of those who voted the original rules in 1910. Nevertheless, it would be grossly unfair and detrimental to the general acceptance of nomenclatorial rules, if those who have adhered to them as they best understood them, were now penalized by a revision of the interpretation or, if one wants to express it so, by a reconstitution of the original intentions."

There is some exaggeration here. The situation, then and now, is that Singer's conception in its extreme form has never been accepted by a fair proportion of mycologists (out of the few who have voiced their opinion on these matters). Moreover, we should take into consideration the opinion of botanists working on the other groups of cryptogams with later starting-points. Finally, there are even mycologists who completely reject Singer's views. It should also be pointed out that at Stockholm Congress his conception lost much of its foundations, real or supposed: some readjustment in this regard has become necessary but has not yet been supplied.² In my opinion, a radical change is needed, if one does not want, in the long run,

¹ The main argument for the suppression of the words 'under another name' was to be able to associate names with pre-startingpoint descriptions (cf. Lanjouw 1953: 505). As Dandy expressed it: "When a name is based on a pre-Linnean name the essential point is that the new name is associated with the description accompanying the earlier name . . ."

² At the Stockholm Congress it was briefly pointed out that some of the proposed (and accepted) alterations would have far-reaching consequences in connection with current views on typification of revaluated names (cf. Lanjouw, 1953: 505).

to alienate mycologists and other cryptogamists from the Code, if it really supports the views defended by Singer.

Let us continue to quote Singer:

“From a practical point of view, the admission of the original pre-Friesian author’s concept as the type of a (re-) validated name would contribute toward a better documentation only in the case of Persoon (and even here not in all cases); in the case of other authors, especially Scopoli, Schaeffer, Bulliard, Withering, Linnaeus, Batsch, Bolton, etc., it would open the door for futile discussions and a variance of interpretation which would be especially dangerous and detrimental in cases where the Friesian name is already fixed by a more methodical description, or by unanimous tradition.”

I beg to disagree once more. Of the authors mentioned, Schaeffer, Bulliard, Batsch, and Bolton published their new species with descriptions accompanied by often excellent plates which in most cases permit us to recognize their species with precision. This is more than can be stated of Fries’s descriptions in the starting-point book which has no figures and plates, and which can often be interpreted with some measure of precision only through the plates of the pre-starting-point authors he cites. Many futile discussions could be saved by leaving Fries’s interpretations, and those of other revalidating authors, out of account. The number of cases in which Friesian *misinterpretations* are already ‘fixed’ by unanimous tradition is small.

The following are the main arguments to support the proposal.

(i). It was evidently not the intention of the members of the Brussels Congress (which introduced the later starting-points) to pave the road for changing the types of an indefinite number of taxa, but, rather, to get rid of numerous names, names that would have become a nuisance if the principle of priority had not been restricted. Why link a useful purpose to a cumbersome anomaly, if there is no overwhelming advantage to be gained by doing so? Moreover, it is an anomaly that was never explicitly enjoined: it has been deduced from the Rules as they stood, without any clear indication that the legislators ever took it into serious consideration at Cambridge when the type-method was incorporated in the Rules. The aim was and has been stability in the use of names, not a shifting of taxa to a different footing.

(ii). The most important argument, in my opinion, is that truth is not distorted when the second principle is accepted. Later starting-points should mean, merely, a restriction of the principle of priority: certain names that were never revalidated lost their priorability completely, others that were, had their priorability reduced by a certain span of time. The intention has been to privilege the names applied by the starting-point-book authors, an intention that was once more recognized at Stockholm when the present Art. 13 (f) was formulated anew. Miss E. M. Wakefield, who took part in the enactment of later starting-points at Brussels, has authorized me to state that only a suspension of the priority principle was aimed at. Dr. J. Ramsbottom also expressed this opinion in a recent discussion I had with him on the subject.

The preferable course is the one that does not distort the historical evolution of taxa (provided perhaps that this does not clash with deliberate intentions of the revalidating authors: see below). Bresadola, Quélet, and many American authors never bowed for Fries’s authority in taxonomic matters. If Fries erred, they thought, he should be corrected. Many mycologists still share this attitude. Why not go on to correct the errors of starting-points authors? Accepting an error like a mis-application as correct creates an anomalous situation from which there is no way out except by a crooked exit.

Another distortion of facts is made by ascribing to revalidated names the status of names published for new taxa. If we assume for the moment that they really

acquired that status, then it must be remembered that they attained it retroactively: the revalidating author was not aware of doing anything else but applying an available name. He would certainly have risen to the occasion if he had been a seer. Fries himself considered a name he took up (without intentionally excluding the type) as based on the original material.

EXAMPLE 5.—The name *Merulius vastator* Tode [= *Merulius lacrimans* (Wulf.) ex Fr.] was misapplied by Fries in the starting-point book (Syst. mycol. 1: 329. 1821). When he became aware of his error he introduced a new name for his erroneous conception, *Merulius aureus* Fr. (Elench. 1: 62. 1828).

(iii). In many instances a revalidated name cannot be typified otherwise than by the type of the devalidated name which so many mycologists now leave out of account to such an extent that they even do not cite it in synonymy. When a name is revalidated merely by a reference there can be no escape from going back to the original publication to learn which specimen should be accepted as type. When the revalidating author merely copies or re-edits the original phrase or description the type will still be that of the devalidated name. Moreover, I believe that the present wording of the Code leaves anyone free to typify a name by an original specimen in all cases where the revalidating description does not positively exclude it, even in those cases where the revalidating description was drawn up exclusively from additional specimens, a circumstance usually difficult to verify. Opinions diverge at the moment principally I believe in connection with those cases in which the revalidating description was based on specimens of a different taxon, that is, in cases where a misapplication occurred.³

(iv). It may well be that the here proposed method of typification of revalidated names was really implied in the Rules (Code) all the time, although in a vague manner; compare Recommendation 50D. It recommends the form '*Boletus piperatus* [Bull.] Fr.' or '*Boletus piperatus* Bull. ex Fr.' for revalidated names; in this example Bulliard stands for the author of the devalidated name and Fries for the author who revalidated that fungus name. It hardly needs to be pointed out that a nomenclatural type is permanently attached to a name (Art. 7) and that the author's citation is part of the plant name (Art. 46: "For the indication of the name of a taxon to be accurate and complete . . ."). From this it follows that the author's citation stands for the same type as the plant name to which it is appended. If 'Bull. ex Fr.' means that the original name and the revalidated one have the same type, the latter must be Bulliard's. This view is also consistent with authors' citations as in names as '*Onnia tomentosa* (Fr.) P. Karst.' The latter is a telescoped form of the statement that the recombination *Onnia tomentosa* P. Karst. is an isonym of [*Polyporus*] *tomentosus* Fr., and that both names are based on the same type, that is, Fries's, even if Karsten misapplied the recombination to a different fungus from the original one at the time he published the recombination (Art. 55).

It would be misleading to write '*Boletus piperatus* Bull. ex Fr.' if the name had to be typified by a Swedish specimen seen by Fries (but not kept) when he drew up the revalidating description; or to write '*Clavaria abietina* Pers. ex Fr.' if one really meant '*Clavaria abietina* Fr. (non Pers.).'

Recently Holm (1957: 12) came to a diametrically different interpretation: "... comment typifier ces espèces? Prenons par ex. *Sphaeria seminuda* Pers. ex Fr., nom que les deux auteurs ont employé dans un sens différent. Allons-nous adopter le sens de Fries ou celui de Persoon? Les règles adoptées ne permettent pas de conclure. Pourtant, la rec. 50D pourrait nous mettre sur la voie; il n'est pas nécessaire, y est-il dit, d'indiquer l'auteur antérieur à

³ Leaving out of account such an extreme view as given in Examples 3 and 4. The present Code hardly favours it.

Fries. *Sphaeria seminuda* Pers. ex Fr. devient donc *Sphaeria seminuda* Fr. Cela semble indiquer que le nom doit être pris sensu Fries; sinon, on serait obligé d'écrire *Sphaeria seminuda* Fr. non sensu Fr."

When adding authors' citations to names, mycologists almost universally add the citations that are consistent with the here proposed principle, although they often typify the names in agreement with the other principle. An authors' citation as in '*Amanita muscaria* (L. ex Fr.) S. F. Gray'⁴ is correct only when the type is chosen from Linnaeus's syntypes. It is faulty if one typifies *Agaricus muscarius* L. and *A. muscarius* Fr. by different specimens and it would become still more faulty if one added 'S. F. Gray,' for Gray never recombined the epithet 'muscarius Fr.' because he did not know of the existence of Fries's "Systema." If we typify the name in accordance with Linnaeus's original publication and consider the mention of Fries only as indicating the date on which the name became validly re-published, then the author's citation '(L. ex Fr.) S. F. Gray' is not objectionable anymore, except for the use of 'ex' (see below).

Singer's comment (1951: 124) on this situation is as follows.

"This complication . . . , by the tacit consensus of those concerned, has thus far been handled in a way suggesting the existence of an explanatory note supplementing Art. [13] saying that "transfers made after the starting data in the different groups (or: in 'Fungi caeteri') but regarding pre-Friesian names revalidated in the sense of a post-Friesian author must be understood as transfers of the unit concerned in the post-Friesian concept rather than in the original pre-Friesian concept, unless the transferring author makes a definite statement excluding the Friesian or post-Friesian concept such as 'non Fries', and 'nec Fries' . . ."

Not only would the explanatory note appear quite unsatisfactorily in practice, it also would distort the facts in a manner that few people will find acceptable when they will start probing more carefully into the matter. It has been labelled, "évidemment un anachronisme."

(v). The systematics of Fungi as well as many other groups of cryptogams are seriously handicapped by the lack of an index similar to the "Index kewensis." Indexing of this kind will be greatly simplified if the present proposal is adopted. I have had the opportunity to test the two principles of typification under discussion while indexing certain groups of Fungi and have reached the conclusion that (short of abolishing later starting-points altogether) there is only one reasonable way out.

(vi). It often occurs that one is obliged to change the citation of the revalidating author. An instructive example is found in S. F. Gray's "Natural Arrangement of British Plants" which for some time had been considered as of uncertain date of publication: it was not definitely known to have appeared after Fries's "Systema mycologicum," Volume 1, and, therefore, some mycologists considered it pre-Friesian. Rogers (1941) concluded that it was ostensibly issued November 1, 1821, after Fries's Volume 1. (Now that it has also been arbitrarily fixed that Fries's first volume of the "Systema" was issued on January 1, 1821, Gray's work is definitely in.) This new situation necessitated many changes in author's citations. Rogers (1951; 1952) also drew attention to Hooker's "Flora scotica" (May 1821) and Mérat's "Nouvelle Flore des environs de Paris" (June 1821), which once more necessitated the change of many author's citations; this time 'S. F. Gray' had to be replaced, because both

⁴ One even will find the form "*A. muscaria* (L. ex Fr.) Pers. ex Gray," which reflects the existence of the devaluated name, *Amanita muscaria* (L.) Pers. — The citation 'S. F. Gray' is now to be replaced by 'Hook.' or perhaps even by still another author's citation.

works are of an earlier date. Recently I have been able to establish that Saint-Amans's "Flore agénaise" was published at a still earlier date (April 1821) and again it appeared necessary to change many author's citations that had been replaced already a few times. Perhaps, we are not yet at the end. I have not yet been able to fix the precise or relative dates of Nocca & Balbis's "Flora ticinensis," Volume 2, and a few other books also published in 1821. If we had to change the type of the names simultaneously every time the new revalidating author had studied specimens of his own, stability would only be reached very gradually.

The above account only deals with the year 1821, a very short span of time. I am now in the position to state that at least among the 'Aphylophorales' currently accepted 'late' author's citations have to be replaced in numerous instances: 'revalidations' ascribed to J. Schroeter, Saccardo, Murrill, and others appear more often than not to have been performed at previous occasions, and I have a still growing list of publications that will have to be scanned in this respect, works that are often difficult to get on loan for critical examination. Every visit to a large library now brings to light additions to the list.

(vii). One of the most powerful arguments against the rejected guiding principle for typification discussed above, in its most moderate interpretation (exclusive of Examples 3 and 4), is in the needless complexity it usually raises. For instance it creates a multiplicity of homonyms. Examples may tell much of the story.

EXAMPLE 2 (cont'd).—The name *Clavaria abietina* Pers. was once more revalidated (after Fries) for instance by Persoon himself. He re-published his fungus name in 1822 by adding an almost unchanged description taken from his pre-startingpoint work and which excludes Fries's species, so it cannot be typified by Fries's (lost) specimen 'adopted' in the first part of the present example. The type of the devalidated name clearly is also the type to be chosen for *Clavaria abietina* Pers. ex Pers. 1822. Here we have got at least three names, all written the same except for their author's citations, but with different taxonomical or nomenclatural meaning, viz. (a) *Clavaria abietina* Pers. 1794 (type, specimen x), a devalidated name, (b) *Clavaria abietina* Fr. (non Pers., although ascribed by Fries to Persoon; type, specimen y), and (c) *Clavaria abietina* Pers. ex Pers. (non Fr.; type, specimen x). The latter two are validly published homonyms and, as all homonyms of the Code, based on different types.

I have introduced for such names as (b) and (c) the term monadelphous homonyms: both are historically tied to a single devalidated name, (a). Of the two homonyms, (c) is the later one. When still adopting the Rules from before the Stockholm Congress as a basis, I suggested to follow this principle: "All 'later' monadelphous homonyms . . . are simply [to be] reduced to the status of mere applications (often 'misapplications') of the corresponding earliest validly published homonym. They are not [to be] listed in synonymy as 'different' names" (Donk 1949: 287). It would be much better to get rid of this kind of complication altogether.

But this is not all and still more complications are in store.

EXAMPLE 2 (cont'd).—Both species, *Clavaria abietina* Fr. (non Pers.) and *C. abietina* Pers. ex Pers. = *C. ochraceo-viridis* Jungh. belong to the genus *Ramaria* (Holmskj. ex Fr.) Bonord. according to several authors. Before the introduction of later starting-points, *Clavaria abietina* Pers. was renamed *Ramaria abietina* (Pers.) Quél. 1888. However, this name was not available, because its basynym was a later homonym, and before Quélet made the recombination (or any other re-combination was made) *Clavaria ochraceo-viridis* Jungh. was validly published, and Persoon's species (under *Ramaria*) had to be called *Ramaria ochraceo-viridis* (Jungh.) Donk 1933. The transfer of *Clavaria abietina* Fr. (non Pers.) encountered another inherent difficulty: it could not be called *Ramaria abietina* (Fr.) John Doe because that combination was already preoccupied by *R. abietina* (Pers.) Quél., and so it was necessary to provide for

the recombination *Ramaria invalii* (Cotton & Wakef.) Donk 1933, basonym, *Clavaria invalii* Cotton & Wakef. 1919, introduced for the same species as *C. abietina* Fr. (non Pers.).

It need not be demonstrated in the form of a table that the course followed in this Example is much more complicated than the one upheld in the present communication.

Holm (1957: 13) closed a Chapter on "Nomenclature" with the following sigh:

"Etant donné ces difficultés et puisque Fries n'était évidemment pas un spécialiste des Pyrénomycètes, je crois qu'on peut sans témérité mettre en doute l'opportunité de prendre le Systema Mycologicum comme 'point de départ' pour la nomenclature des Pyrénomycètes (et des *Fungi imperfecti*). Il est malheureusement trop tard aujourd'hui pour introduire une réforme radicale et supprimer la référence à cet auteur."

Yet, improvement of the situation, even without abolishing the later starting-points, seems possible after all if we stop to attach disproportionate and presumed importance to the revalidating authors. Let us go back to the intention of the Brussels Congress: to limit the *priorability* of names which originated before the starting-point dates -- and nothing else.

Additional remarks

In the above discussion only misapplications 'without exclusion of type' have been taken into consideration. It remains to consider applications of devalidated names of which the types were excluded when they were revalidated.

EXAMPLE 6.—*Hydnum cyathiforme* Schaeff. 1774 was misapplied by Bulliard 1783, who called a quite different fungus "*Hydnum cyathiforme* Schaeff." When Fries (in the starting-point book) revalidated the name *Hydnum cyathiforme*, he ascribed it to Bulliard and listed *H. cyathiforme* Schaeff. as a synonym under *Hydnum tomentosum* L. ex Fr. Thus, Fries actually recognized two homonyms, one, *Hydnum cyathiforme* Fr. ("Bull.;" non Schaeff.) which he applied as if it were a correct name, and one, *H. cyathiforme* Schaeff., which he listed as a synonym of a different species.

EXAMPLE 7.—As mentioned in the foregoing Example, Bulliard misapplied the name *Hydnum cyathiforme* Schaeff. without actually excluding Schaeffer's type. The first author who after January 1, 1821 (the starting-point date and the arbitrarily accepted date of the first volume of the starting-point book of the group concerned) took up *Hydnum cyathiforme* without explicitly excluding Schaeffer's species, was Saint-Amans (April 1821). Saint-Amans cited Bulliard, but did not write "Bull. (non Schaeff.);" or something equivalent. In my opinion, his reference has to be corrected, in agreement with, and through, Bulliard's own account, from 'Bull.' into 'Schaeff.,' and he should be credited as having revalidated the original name: *Hydnum cyathiforme* Schaeff. ex St.-Amans (non *H. cyathiforme* Fr.), although he misapplied it in Bulliard's sense.

After considerable hesitation I have come to the conclusion that names 'revalidated with *explicit* exclusion of the type' should be assessed as 'new' names rather than as 'revalidated' ones that were misapplied. In normal cases, in which no later starting-points are involved, the misapplication of a name with exclusion of the type does not create a new name (except when the author expressly indicates the later as 'nom. nov.,' or similarly). It would perhaps be preferable, to follow exactly the same course in cases of the type of Example 6, but after all devalidated names have been stripped from their priorability and do not count as potential homonyms.

When typifying revalidated names, and before being completely converted to the opinions here defended, I nevertheless have always tried as much as possible not to

severe the connections between the devaluated name and its revaluated counterpart. This line of conduct has been objected to by some authors who believe that there existed no moral obligation at all to take into account what has been published before the starting-points. The present Code explicitly permits us to go back to the original publication, at least if there is a reference, and I have now changed my views accordingly.

EXAMPLE 8.—*Odontia* Pers. 1794 was introduced with two original species, *O. ferruginea* Pers. and *O. nivea* Pers., the first of which was indicated as the type by Banker (1902), which makes *Odontia* a synonym of *Caldesiella* Sacc. 1877. When the name was revaluated as *Odontia* Pers. ex S. F. Gray 1821, Gray cited "Persoon," gave an accompanying description adapted from Persoon [Syn. Fung. 560. 1801, as *Hydnum* sect. *Odontia* (Pers.) Pers.], and treated two species both of which were originally not included by Persoon. The reason for this latter situation is quite plain: Gray listed only the British species which by change were not the two original species. Until a short time ago I reasoned that we did not know for certain if Gray would have included the two original species if they had been reported for the British Isles, and, therefore, that they were not available for selection to the type (Donk 1956: 105, which see for details). The present Code, however, permits us to associate the name *Odontia* Pers. ex S. F. Gray with Persoon's original description and, thus, also with the original species which were not explicitly excluded by Gray. Therefore, I now consider Banker's earlier typification of Persoon's name by *Odontia ferruginea* as also applicable to *Odontia* Pers. ex S. F. Gray and reject my later choice of one of the two species Gray treated, viz. of *Hydnum obtusum* Schrad., selected for *Odontia* 'S. F. Gray' in particular.

If the proposals made in the present paper are accepted, the author of a devaluated name should not be suppressed at will, a course left open by Recommendation 50D, for he is the true author of the name. The additional author merely stands for the date from which the name begins to count in priority considerations. This is a quite different situation from the one covered by Recommendation 46A, where another use of 'ex' is recommended.⁵ As the same manner of expression for two different situations is likely to lead to confusion and is undesirable, it is proposed to substitute 'ex' by 'per' in the case of revaluated names and to write *Boletus piperatus* 'Bull. per Fr.' instead of *Boletus piperatus* 'Bull. ex Fr.'^{6, 7}

PROPOSALS.8—Add to Art. 13 the following Note.

18. Note 5. The starting-points for Fungi later than 1 May 1753 merely affect the dates of publication of names that would have been validly published if no later starting-points had been introduced: they do not change the type of these names, except when it can be shown that the type was definitely excluded on purpose by the author who validly published a name dating from before its starting-point in accordance to the present Article.

19. Recommendation 50D to be split up and emended as follows.

Recommendation 50D.—The citation of an author who published the name before the introduction to the binomial system of Linnaeus is indicated, when considered useful or desirable, preferably between square brackets.

⁵ The use of 'ex' in Recommendation 50B is again different from both the uses in Recs. 46A and 50D and should be withdrawn.

⁶ The word 'per' was suggested to me by Dr. R. A. Maas Geesteranus.

⁷ Recommendation 50D offers two ways of citing the 'original' author. I would suggest reserving the square brackets for citing non-binomial authors from before the general starting-point 1753: *Lupinus* [Tourn.] L.

⁸ Proposals nos. 18 and 19 submitted to the 9th International Botanical Congress Montreal 1959.

Example: *Lupinus* [Tourn. Inst. 392, *pl.* 213. 1719] L. Sp. Pl. 721. 1753, Gen. Pl. ed. 5. 322. 1754, or *Lupinus* [Tourn.] L.

Recommendation 50D (bis).—The citation of an author who would validly have published the name if the starting point of a later date than 1 May 1753 of the group concerned and listed in Art. 13 had not been adopted, is connected by the word *per* with the citation of the author who actually validly published the name.

Example: *Boletus piperatus* Bull. Herb. France *pl.* 451, *f.* 2. 1789 *per* Fr. Syst. Myc. 1: 388. 1821, or *Boletus piperatus* Bull. *per* Fr.

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