



Article 59

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P. radiata might be given the collective name *Attenuradiata* pine, *Attenuradiata* Hybrid pine, or *Pinus Attenuradiata* Hybrid instead of the binomial *Pinus* × *attenuradiata*.

Some interspecific hybrids have been given Latin names honoring persons. Similar names in modern languages without the Latin endings are more appropriate, for example, *Sonderegger* pine rather than *Pinus* × *sondereggeri*.

Any tree hybrids produced in quantities or commercially should be named further as varieties (cultivars) for greater precision, for example, hybrid clones in *Populus* and varieties of hybrids in *Pinus*. Under the Code (Art. 45), when a variety (cultivar) of an interspecific hybrid is introduced into cultivation, it must be given a variety (cultivar) name in addition to the collective name or formula, even if only one variety is concerned. Otherwise, the first variety would be nameless and not readily distinguishable when a second variety (different and possibly inferior) from the same two parental species is produced and named elsewhere later.

ARTICLE 59

F. C. Deighton (Kew)

The wording of the present Art. 59 (Code, 1956) was drawn up at the Seventh International Botanical Congress, Stockholm, 1950, as a result of a proposal by Drs. G. R. Bisby and J. A. Stevenson. References to previous discussions on the subject are given by Rogers in *Mycologia* 40: 241-254. 1948. A number of recent publications have, however, shown that there is more than one possible interpretation of the provisions of the Article.

Most difficulty has arisen in interpreting the following sentences from Art. 59:

“The type specimen of a name applied to a particular state must show the characteristics of that state.”

“The author who first describes a perfect state may adopt the specific epithet of the corresponding imperfect state, but his binomial for the perfect state is to be attributed to him alone, and is not to be regarded as a transfer [i.e., new combination].”

Yet these two sentences were undoubtedly intended, when the Article was first drafted, to be of fundamental importance in clearing up earlier confusion.

Dr. M. A. Donk (*Taxon* 9: 171-174. 1960) has discussed certain aspects of the Article. He stresses the Rule that a name which is validly published makes a later homonym illegitimate [it must be noted that this still applies even if the earlier homonym is illegitimate: Art. 64 (2)]; and there is no precise provision in Art. 59 to amend this rule. In this connexion he discusses *Corticium microsclerotia* (Matz) Weber.

He also cites Bisby's remarks (An introduction to the taxonomy and nomenclature of fungi, I.M.I., Kew, ed. 1: 88. 1945) about *Mycosphaerella aleuritidis* (Miyake) Ou. These early remarks by Bisby were, however, made before the present Art. 59 was framed, and Bisby later (An introduction to the taxonomy and nomenclature of fungi, C.M.I., Kew, ed. 2: 117. 1953) remarked “Article 69 [= Art. 59 of Code, 1956] now eliminates most of the uncertainties which existed in its former wording as Art. 57. . . . A perfect state described as “*Mycosphaerella aleuritidis* (Miyake) Ou n. comb., syn. *Cercospora aleuritidis* Miyake” is now to be cited as a new species “*M. aleuritidis* Ou (as “(Miyake) Ou”).” Bisby presumably interpreted the last sentence of para. 1 of Art. 59 (the second of the two sentences which I have quoted

above) as constituting, in effect, an amendment to Art. 64(2). Donk has shown that the wording of Art. 59 does not exactly sanction this interpretation, though I believe this was what was intended when the Article was framed.

We must always remember that the Articles of the Code are not meant to result in legal quibbles. Nobody should find himself in the position of having to remark "the current interpretation of the International Rules appears to be", as does Savile (Canad. J. Bot. 33: 490. 1955). That such a remark should be found necessary points to the need for rewording of Art. 59, and I have therefore tried to draft, for consideration, a rewording that reflects what I believe the majority of taxonomic mycologists want it to mean, and what I believe its original sponsors intended. I have also tried to ensure that as few changes as possible in names of fungi will be needed. These are two of the objects indicated by the Preamble to the 1956 Code. It may be thought unfortunate that the wording of Art. 3 of the 1952 Code has now been omitted: this stated "The Code of nomenclature should be simple and founded on considerations sufficiently clear and forcible for everyone to comprehend and be disposed to accept".

A series of examples will be cited, with comments, in an endeavour to discover what has become established custom in the interpretation of Art. 59. Since this Article has perhaps its most important and difficult application in the Uredinales, most examples will be drawn from this group of fungi, but it is advisable first to consider some more simple examples, of the kind discussed by Donk.

Example 1

Mycosphaerella caryigena (Ell. & Ev.) Demaree & Cole in J. agric. Res., 44: 145. 1932, was published with an English diagnosis for the *Mycosphaerella* state drawn up from material collected in (?) southern Georgia, U.S.A. (type not designated), not from the type of *Cylindrosporium caryigenum* Ell. & Ev. on which the "new combination" was based.

Example 2

Mycosphaerella cruenta (Sacc.) Latham in Mycologia 26: 525(1934), was published with diagnosis in English and Latin for the *Mycosphaerella* state drawn up from material collected in N. Carolina, U.S.A. (type not designated), not from the type of *Cercospora cruenta* Sacc. on which the "new combination" was based.

Example 3

Elsinoë ampelina Shear in Phytopathology 19: 677. 1929. Shear gave a description in English of the asci and ascospores of the fungus which he obtained on overwintered cankers caused by *Sphaceloma ampelina* De Bary. In one and the same paragraph on p. 677 he says "we would therefore refer the ascogenous form of the grape anthracnose organism to the genus *Elsinoë* as *E. ampelina* n. sp." ... "and it is possible as Jenkins and Horsfall suggest that *E. viticola* Rac. is a synonym of *E. ampelina* (de By.) Shear, n. comb.". The name is cited in later literature sometimes as *Elsinoë ampelina* Shear and sometimes as *E. ampelina* (De Bary) Shear.

Comments. Examples 1 and 2 are similar to *Mycosphaerella aleuritidis* (Miyake) Ou which was cited by Bisby (Mycologia 36: 282. 1944) and referred to in para. 4 above. Bisby was quite sure about the intentions of the present Art. 59 in regard to such an example, though it is also clear that he did not advocate the publication of a name for the perfect state of a fungus in this way. It follows that Bisby would have regarded examples 1 and 2 as being the valid publication of legitimate names "*Mycosphaerella caryigena* Demaree & Cole, sp. n. (as '(Ell. & Ev.)')" and "*Mycosphaerella cruenta* Latham, sp. n. (as '(Sacc.) Latham')", based on types from

southern Georgia and from N. Carolina respectively. It is of interest to note that Petrak (Just's bot. Jber. 63 (1935), Abt. 2: 941. 1944) listed these names as "*Mycosphaerella caryigena* Demaree & Cole. — Stat. conid. *Cercospora caryigena* (Ell. & Ev.) v. Höhn." and "*M. cruenta* Latham. — Stat. ascif. *Cercosporae cruentae* Sacc."

Example 3 is not an example of alternative names (Art. 33), since the name *Elsinoë ampelina* was published by Shear both as a new species (based on Shear's type) and as a new combination (based on De Bary's type). If we follow Bisby's interpretation, the validly published and legitimate name was *E. ampelina* Shear sp. n. (Note that should it be considered that Raciborski's earlier-named species is identical with Shear's, the correct name of the fungus will be *E. viticola* Rac.).

There are, I believe, only a few similar examples, the latest to my knowledge being *Leptosphaeria taiwanensis* Yen & Chi (as '(Mats. & Yam.) Yen & Chi') in J. Sugar Cane Res., 4: 213. 1950. A good reason for accepting these names as the validly published and legitimate names of new species, based on the type containing the perfect state, is that the perfect state of many such fungi is rarely seen. If we are to follow Donk's argument, *Mycosphaerella caryigena* (Ell. & Ev.) Demaree & Cole must be based on the type of the basionym (*Cylindrosporium caryigena* Ell. & Ev.) and a new name will have to be published for the *Mycosphaerella* state based on material seen by Demaree & Cole (or, if this is no longer available, on a new type) and accompanied by a Latin diagnosis. This, however, would probably not prove to be an insuperable difficulty.

I am personally not averse (as is Dr. Donk) to the use of the same epithet for both the perfect and the imperfect states of the same fungus. In large genera such as *Mycosphaerella* and *Cercospora*, in which there is bound to be some duplication of specific epithets, I find it easier to remember (for example) that *Mycosphaerella arachidicola* W. A. Jenkins is the perfect state of *Cercospora arachidicola* Hori, than that *Mycosphaerella personata* Higgins is the perfect state of *Cercospora vitis* (Lév.) Sacc. while the perfect state of *Cercospora personata* (Berk. & Curt.) Ell. & Ev. is *Mycosphaerella berkeleyi* W. A. Jenkins. The practice of using the epithet of the name of the imperfect state in the name of the perfect state is at least 60 years old.

APPLICATION OF ART. 59 TO UREDINALES

Whereas, in Ascomycetes, the imperfect state has been assigned to a genus of the Fungi Imperfecti, in the Uredinales the imperfect states are recognised as still belonging to the Uredinales. In fact, the uredia of *Hemileia* (to give an example) are as easily recognisable as belonging to that genus as are the telia; and because of this, a number of new taxa have been described in *Hemileia* though based on types bearing uredia only. But I believe nobody has described a species of *Mycosphaerella* based on a type without perithecia and showing only an imperfect state.

So far as I am aware, recombinations from a "perfect" to an "imperfect" genus (in modern concept) have only been made in the Uredinales: e.g., Arthur's transfers from *Melampsora* into *Uredo* (which he at that time regarded as a "perfect" genus), in Result. Sci. Congr. int. Bot. Vienne, 1905: 338. 1906. I see no reason why these should not be regarded as validly published and legitimate; and though nobody now uses these combinations, the binomials must have priority over later homonyms in the genus *Uredo*.

Most of the following examples are taken from recent papers by Professor G. B. Cummins, though similar examples in Uredinales can be found in recent works by other authors. Cummins (*Mycologia* 48: 601. 1956) explains clearly his interpretation of Art. 59 by these remarks:

“The following nomenclatural changes are necessary to bring the names of certain species of North American rust fungi into accordance with the International Code. Most of the changes involve either: 1, the provision of Latin descriptions for telial stages which were previously, but subsequent to January 1, 1935, published in English where a specific epithet based on an imperfect stage was treated as a transfer (technically *nomina nuda*) or 2, names used under a perfect genus but based on the aecial or uredial stage (anticipatory names). In the first case Latin descriptions are necessary to validate the names and in the second case transfers of the specific epithet to the appropriate form genus is required.”

Example 4

Achrotelium lucumae Cummins, sp. nov. in *Mycologia* **48**: 601. 1956 [Latin description of telia: type from Florida, collected in 1939].

Syn.: *Uredo lucumae* Arth. & J. R. Johnston in *Mem. Torrey bot. Cl.* **17**: 169. 1918; *Uraecium lucumae* (Arth. & J. R. Johnston) Arth. in *Bull. Torrey bot. Cl.* **60**: 467. 1933; *Achrotelium lucumae* (Arth. & J. R. Johnston), *Cumm.* in *Bull. Torrey bot. Cl.* **67**: 70. 1940, *nom. nud.* [i.e. telia described, but not in Latin].

Comments. Numerous other examples could be cited, from all of which it is clear that it is the wish of several taxonomists that the third sentence of Art. 59 (the first of the two sentences I have quoted in para. 2 above) shall imply that when a specific epithet based on an imperfect state is transferred to a “perfect” genus without the provision of a description (in Latin, on or after 1 Jan. 1935) of the perfect state, such “new combination” shall be treated as not validly published and thus shall have no status under the Code. Presumably the full formal citation of the correct name could be *Achrotelium lucumae* Cummins ex Cummins (1956) which would normally be cited as *A. lucumae* Cummins (Rec. 46A). It will be seen that the type of *Achrotelium lucumae* Cummins, 1956, is not the type of *Uredo lucumae* Arth. & J. R. Johnston, 1918.

This is essentially similar to the example of *Corticium microsclerotia* (Matz) Weber discussed by Donk (*Taxon* **9**: 172-3. 1960). In some respects it is similar to nos. 1, 2 and 3, above.

Example 5

Uredo cubensis (Arth. & J. R. Johnston) Cummins in *Mycologia* **48**: 607. 1956.

Syn.: *Ravenelia cubensis* Arth. & J. R. Johnston in *Mem. Torrey bot. Cl.* **17**: 118. 1918, based on uredia.

Comments. Numerous similar examples could be cited from recent publications. The name *Ravenelia cubensis*, though validly published, is regarded as illegitimate because it was published in contravention of the third sentence of Art. 59 and must therefore be rejected. (If this is the general wish, another paragraph ought to be added to Art. 64, to read “when it is published in contravention of Art. 59”.) How can it therefore be held that the name *Uredo cubensis* is legitimate, since it is based on the illegitimate name *Ravenelia cubensis*? The note to Art. 72 seems pertinent: “when a new epithet is required, an author may, if he wishes, adopt an epithet previously given to the taxon in an illegitimate name, if there is no obstacle to its employment in the new position or sense; the epithet in the resultant combination is treated as new”. The example to Art. 72, Note, is similar to example 5 and I suggest that it can properly be held that the new name should be written *Uredo cubensis* Cummins, not *U. cubensis* (Arth. & J. R. Johnston) Cummins, and the epithet *cubensis* in this combination must be treated as new, dating from 1956. The type remains the same for both the new and the old (illegitimate) name.

Example 6.

“*Coleosporium eupatorii* Arth. sp. nov.” [ex Cummins] in *Mycologia* 48: 603. 1956 [cited as “Arth. ex Cumm.” by Cummins & Stevenson in *Plant Dis. Repr.*, Suppl. 240: 119. 1956].

Syn.: *Coleosporium eupatorii* Arthur in *Bull. Torrey bot. Cl.* 33: 31. 1906, based on uredia.

Cummins supplies a Latin description of the telia and designates Cheo 1240, collected in 1932 as type. He mentions that Tai (*Farlowia* 3: 101. 1947) first described the telia [under the name ‘*Coleosporium eupatorii* Arth.’] but not in Latin.

Example 7

“*Hemileia mussaendae* Viennot-Bourgin sp. nov.” [ex Cummins] in *Bull. Torrey bot. Cl.* 87: 36. 1960. “Status uredosporiferus. *Hemileia mussaendae* Viennot-Bourgin”. [Latin description of telia and teliospores.] “Type: on *Mussaenda* sp., Wenchi, Ashanti, Ghana, Nov. 25, 1955, C. Piening, C.B. 2129 (IMI., PUR).”

Cummins remarks: “Since Viennot-Bourgin’s fungus” [*Bull. Soc. mycol. Fr.* 67: 431. 1952] “on *Mussaenda erythrophylla* Schum. & Thonn. from Ivory Coast had no teliospores the name, *Hemileia mussaendae*, dates from the present publication and Piening’s specimen is the type”. Presumably Cummins would cite the author as “Viennot-Bourgin ex Cummins”.

Example 8

Uredo ricini Biv.-Bern., 1815, was described on *Ricinus*. The name was changed to *Caeoma ricini* by Schlechtendahl, 1826, to *Melampsora ricini* by Passerini (Erb. Critt. Ital., Ser. II, fasc. XIV, no. 684. 1878) and to *Melamporella ricini* by De Toni (Saccardo, *Sylloge Fung.* 7: 596. 1888). Though only uredia were known till 1952, the names *Melampsora ricini* and *Melamporella ricini* have been widely used for the rust on *Ricinus* in spite of the fact that *Uredo ricini* Biv.-Bern., was presumably the correct name according to Art. 59 as interpreted by Cummins and others.

Noronha (*Agric. lusit.* 14: 229, 242. 1952) described, with a Latin diagnosis and under the name *Melampsora ricini* Passerini, telia on *Euphorbia marginata* artificially inoculated with urediospores obtained from *Ricinus communis* in Portugal.

The late Dr. G. R. Bisby, as is evident from his notes in *Herb. I.M.I.*, thought it correct to cite the name of this taxon as *Melampsora ricini* Passerini ex Noronha (1952), and presumably he would have regarded the telial material on *Euphorbia marginata* from Portugal as the type, and not the uredial material on *Ricinus* with which Passerini’s name is associated.

Comments. In examples 6 and 7, it will be noted that there is a wish to retain an established (and validly published) name for a taxon by the use of ‘ex’ between the name of the original and the later author. But this particular use of ‘ex’ is not provided for in the Code: Rec. 46A only applies “when a name has been proposed but not validly published by one author and is subsequently validly published and ascribed to him by another author”. Yet if *Ravenelia cubensis* Arth. & J. R. Johnston (example 5) is held to be validly published, though illegitimate, it is illogical to maintain that *Coleosporium eupatorii* Arthur (example 6), and *Hemileia mussaendae* Viennot-Bourgin (example 7), likewise based on uredia and hence regarded as illegitimate, are not validly published; and I do not see how the Code could be amended so as to distinguish between the validity of publication of these names.

But examples 6 and 7 are not quite the same as example 5 because the telial type is not the same as the uredial type on which the illegitimate name was based: and Art. 72 is only applicable when the type of the new name is the same as that of the old and illegitimate name (as in example 5). The names *Coleosporium eupatorii* Arth. and *Hemileia mussaendae* Vienn.-Bourg., which (like *Ravenelia cubensis* Arth. & J. R. Johnston, example 5) we are considering for this argument to be validly published but illegitimate, are undoubtedly permanently attached (Art. 7) to Arthur’s and Viennot-Bourgin’s types, respectively, and to no others.

I can see no reasonable alternative to the necessity for the publication of new names to be associated with the telial types in examples 6 and 7, since Art. 64(2)

makes it quite clear that the names published by Cummins are later homonyms and must be rejected: the same argument applies to the more extreme example 8. The alternative of amending Art. 64(2) by the addition of the words "except when the earlier homonym is illegitimate because it was published in contravention of Art. 59" is, I think, unlikely to be generally acceptable.

To date, there are relatively few examples similar to 6 and 7, but more and more are likely to arise as telial material is discovered. It is therefore urgently necessary that agreement should be reached about the wording of the Code so that there shall be no doubt about legitimacy and validity of publication of such names, their correct citations and their types.

APPLICATION OF ART. 59 TO TYPES IN UREDINALES

Several Uredinologists have given their views on the interpretation of paragraph 3 of Art. 59, in regard to its application to types.

Bisby (An introduction to the taxonomy and nomenclature of fungi, C.M.I., Kew, Ed. 2: 117. 1953) says "the common practice of transferring *Uredo* names" [to genera based on perfect states] "is now legitimate only when the description or the type specimen of the *Uredo* shows that teliospores were present."

Savile (Canad. J. Bot. 33: 490. 1955) says "The current interpretation of Art. 69 of the International Rules" [Code, 1952: = Art. 59, Code, 1956] "appears to be that if a specimen described as a *Uredo* also bears telia, unnoticed or unmentioned by the author, this specimen must be regarded as the type when the fungus is assigned to its appropriate genus; and the reassigning author must be regarded as making a new combination. If telia are lacking in the original specimen, the reassigning author is interpreted as making a new species based on his telial specimen." (Note, incidentally, that Savile would presumably regard examples 6, 7 and 8 as the publication of the names of new species based on the telial specimens, but does not explain how the question of homonyms is to be overcome.)

Jørstad (Blumea 9: 1. 1958) follows Savile, saying "owing to the type method now being established (Code, Art. 18 [1952: = Art. 7. 1956]) it seems natural to consider a *Uredo* name valid if the type contains teleuto, even if this stage has not been mentioned in the author's description of the species in question." This view was held by Rogers (Mycologia 40: 250-251. 1948), and Cummins is evidently of the same opinion. But this argument, if based on sentence 3 of Art. 59, seems to me to be somewhat of a *non sequitur*. It is to be noted that Hylander, Jørstad and Nannfeldt (Opera bot. 1, 1: 4. 1953) said "the rust names considered as correct are those founded on *descriptions* of the teleuto state" [my italics]: Jørstad and Nannfeldt evidently modified their views later (Bot. Notiser 3, 1: 306. 1958).

It is essential that Art. 59 shall be worded so that there can be no two opinions about its meaning in this respect.

Example 9

Puccinia jalapensis (Holw.) Barth. in N. Am. Ured. no. 2549. 1922 [Latin description of teliospores by Cummins in Mycologia 48: 605 (1956); cited as '(Holw.) Barth. ex Cummin.' by Cummins and Stevenson in Plant Dis. Repr., Suppl. 240: 153. 1956].

Syn.: *Aecidium jalapense* Holw. in Ann. mycol., Berl. 2: 392. 1904, with undescribed telia; *Dicaeoma jalapense* (Holw.) Arth. in N. Am. Flora 7: 420. 1920.

Cummins remarks: "Both N. Am. Ured. Nos. 1501 and 2549 are from Holway's original collection No. 3076 on *Ipomoea jalapa* (L.) Pursh, Jalapa, Mexico, Oct. 5, 1898. A few teliospores are present, hence Holway's *Aecidium jalapense* is based on a type bearing the perfect state although not described. A description of the teliospores is given above."

Comment. It is accepted that Holway did not see the teliospores, and though these were present on part of the type collection, they may not have been present (or were, at least, not recognised) on the part which Holway examined.

One of the problems in connexion with micro-fungi lies in the fact that a "type" is a collection usually consisting of more than one individual plant of the same species (and frequently of other species as well). In this sense, it is a mixture from which a later worker must choose a "lectotype" as nearly as possible conforming with the original diagnosis, since the actual plant individual which was examined by the original author was very likely removed by him and not preserved (or if it is still present it will be in most cases virtually impossible to locate again). What Holway saw in his type were aecia, and his name, *Aecidium jalapense*, must be permanently attached to the aecia on the type collection.

The first person to discover teliospores (though he did not see telia) on this material was Cummins, who described them in Latin and cited the same collection as the type. But it is surely incorrect to say that the basionym is *Aecidium jalapense* Holway, though the teliospores are presumably a state of the same fungus species.

The first publication of a name for this taxon in *Puccinia* was by Bartholomew. Cummins regards Bartholomew's binomial (and likewise Arthur's, in *Dicaeoma*) as not validly published. I have discussed this point under my comments to example 4 and it may well be desirable to make clear provision in the Code so that Bartholomew's combination into *Puccinia* may be rejected in such a way as to give it no status under the Code. But the portion of the specimen (a collection of plant individuals, though doubtless belonging to the same species) on which Cummins has found teliospores is not the portion associated permanently with the name *Aecidium jalapense* Holway, and (if Bartholomew's binomial can be rejected as invalidly published) the name *Puccinia jalapensis* should in my opinion be attributed to Cummins (1956) and to him alone, since the epithet '*jalapensis*' would no longer be preoccupied in *Puccinia*.

Example 10

Jørstad (*Blumea* 9: 1-29. 1958) examined Persoon's collection of rusts now preserved at Leiden, and listed those on which telia are present. In reference to the present discussion, the following categories are pertinent:

(1) Rust taxa of which only telia are known. In this case, Persoon must have described telia. Example: *Uredo scutellata* Pers.

(2) Rust taxa by the description of which Persoon clearly indicated that he had seen telia. Example: *Uredo appendiculata* Pers. and *U. tussilaginis* Pers.

(3) Rust taxa represented by specimens in Persoon's collection showing both uredia and telia, but the telia of which were undoubtedly not described by Persoon. Examples: *Uredo betae* Pers. and *U. viciaefabae* Pers.

Comments. Category (3) is the point of interest in this discussion. Jørstad says, of *U. betae*, that one of the two collections consists of "a beet leaf with plenty of uredosori, and a few teleutosori not easily seen", while the other is "a beet leaf with uredosori only". If he had seen teliospores, Persoon would surely have seen and described the short persistent pedicels which are as evident in *Uromyces betae* as in *U. appendiculatus*. Still more obvious are the longer persistent pedicels of the teliospores of *U. viciaefabae* (*U. fabae*), which Persoon does not mention, though Jørstad says that telia are present on the collection which he takes as lectotype.

There is also the vexed question of *Milesia* White and *Milesina* Magnus, and it is time that it is decided once and for all which name is to be accepted for the telial state. Cummins & Stevenson (*Plant Dis. Reptr., Suppl.* 240: 130. 1956) say 'In con-

sidering the problem of *Milesina* it has been decided that the former more nearly conforms to the spirit of the present Code.' Jørstad & Nannfeldt (Bot. Notiser 3, 1: 306. 1958) say "It has been thought necessary to use *Milesia* instead of *Milesina*"; thus reversing their views in Opera bot. 1, 1. 1953. On the other hand, Hiratsuka (Revision of Taxonomy of the Pucciniastreae, Tokyo, 1958) used *Milesina* instead of *Milesia* for the genus characterised by telia, and transfers to *Uredo* all species names validly published in *Milesia* and *Milesina* which are based on uredia only: i.e., he regards *Milesia* as a synonym of *Uredo*. Mayor (Mém. Soc. neuchâtel. Sci. nat. 9, 1. 1958) also uses *Milesina*.

The basis of the decision to use *Milesia* instead of *Milesina* is Faull in Contr. Arnold Arbor. 2, 1932, where he states that the type of *Milesia polypodii* White (the type of *Milesia* White) contains teliospores which were unnoticed and unmentioned by White, and (p. 10) remarks "To particularize, I have found few species of *Milesia*, out of the twenty-two species and varieties whose teliospores are known, in which the teliospore characters are of the slightest taxonomic service in the recognition of species. The uredospores of *Milesia*, on the other hand, exhibit unusual, distinctive, morphological versatility, so much that species can ordinarily be recognized from them alone without reference to other organs or structures."

Faull continued (p. 10) "That the framers of the accepted rules anticipated situations such as are presented by *Milesia*, that they recognized the spirit of the principles underlying the Rules as something not requiring illogical adherence to defective formulae under all circumstances, and that they favored or anticipated adaptations of the Rules if such arose, are more than presumptive opinions."

"Therefore", Faull said (p. 11), "I have accepted *Milesia* White as the proper name for the genus In my judgement sufficient latitude is permitted by the Rules to sanction these usages, but if not, I anticipate that either the Rules will be amended so as to extend recognition to uredinal characters as a criterion of priority, or at the very least that *Milesia* White will be listed among the *nomina conservanda*."

The case of *Milesia* and *Milesina* is complicated by the fact that the teliospores are formed within the epidermal cells of the host plant and were not noticed by White who was preoccupied with the conspicuous uredia. In a similar way, Berkeley in 1837 described *Cylindrosporium ficariae* Berk., which is now known to be conidial *Entyloma ficariae* Fisch. v. Waldh., 1877, but failed to notice that chlamydo-spores were present in his type material though Berkeley & Broome in 1875 reported their presence (see Ainsworth & Sampson, The British Smut Fungi, C.M.I., Kew: 106, 1950, and Lindeberg in Symb. bot. upsaliens. 16(2): 41. 1959).

In a somewhat different category comes *Chrysomyxa*. Savile (Canad. J. Bot. 33: 490. 1955), in reference to *C. ledi* var. *cassandrae* (Peck & Clint.) Savile, says "the specimen from Center bears both stages" [i.e., telia and uredia] "in abundance and is the first one cited by Peck. It is accordingly hereby designated as the lectotype of *U[redo] cassandrae* It may be noted that under a hand lens or the lowest power of a dissecting microscope the germinating telia look much like uredinia. Under magnification sufficient to show the urediniospores clearly they are readily distinguished."

Savile's observations seem to me to point to a logical solution of this difficulty. As Nannfeldt remarks (Symb. bot. upsaliens. 16(2): 152. 1959), "The acceptance of the type method certainly does not mean that the descriptions should be totally neglected!" I suggest that if the possibility cannot be excluded that the original author of the name of a taxon in Uredinales did actually see and attempt to describe the telia, his type must be accepted as the type of the telial state. This is the case with *Uredo cassandrae* Peck & Clint., and with *Uredo scutellata* Pers., *U. appendiculata* Pers. and *U. tussilaginis* Pers. But in the case of *Uredo viciae-fabae* Pers., *U. betae*

Pers., *Aecidium jalapense* Holw. and *Milesia* White, the original authors of these names definitely did not see telia.

I can myself see no good reason for conserving the name *Milesia* against *Milesina*. I think there are names available in both genera for all species so far described. In this connexion, Arts. 7 and 62 are pertinent.

CONCLUSIONS

In framing a rewording of Art. 59, we must bear in mind the following:

- (1) The Code deals with nomenclature and not with taxonomy.
- (2) A nomenclatural type is not necessarily the most typical or representative element of a taxon: it is merely that element with which the name is permanently associated (Art. 7, Note 1).
- (3) In fungi, other than Uredinales, the imperfect state is assigned to a genus of the Fungi Imperfecti (Deuteromycetes); whereas, in the Uredinales, the imperfect states are still regarded as belonging to the Uredinales. Nevertheless, in order to provide one code of nomenclature for all fungi, names of genera of Uredinales which are typified by a state which consists of spores which do not give rise to basidia are treated as names of imperfect states and are governed by the same rules of nomenclature as apply to Fungi Imperfecti.
- (4) It is a paradox that in fungi (perhaps one should say other than Uredinales) we recognise that a taxon (which we say has one correct name which is the earliest legitimate name typified by its perfect state) may be made up of one or more additional taxa each typified by one of its imperfect states, and that these, though they are allowed to be regarded as distinct, are not infraspecific taxa but facets of one and the same taxon. This paradox must, however, be accepted for convenience of nomenclature.

I have tried to provide for what I believe were the intentions of the original sponsors of the Article and to follow as far as possible Cummins's interpretation of its application to the Uredinales which is, I believe, the interpretation of other modern uredinologists. In particular I have kept in mind the following objects:

(a) To provide for the rejection of 'anticipatory names' (as defined by Cummins) such as *Ravenelia cubensis* Arth. & Johnst. (example 5) and *Coleosporium eupatorii* Arth. (example 6) by making them illegitimate. (Para. 3 of reworded Article.)

(b) To provide that recombinations from an imperfect to a perfect genus, such as *Achrotelium lucumae* (Arth. & Johnst.) Cummins, 1940 (example 6) and *Corticium microsclerotia* (Matz) Weber, 1939 (which again are 'anticipatory names'), shall be considered not validly published and hence without status. This must be done in order to provide that the names *Achrotelium lucumae* Cummins, 1956, and *Corticium microsclerotia* Weber, 1951, shall not be regarded as later homonyms. (Para. 4 of reworded Article.)

(c) To provide that names published in the manner of *Mycosphaerella aleuritidis* (Miyake) Ou and the examples 1 and 2, while being considered not validly published as new combinations (see (b) above) shall be considered as the validly published new names of new taxa if the other requirements for valid publication are fulfilled, as they are in the examples cited. (Para. 4 of reworded Article.) I agree with Dr. Donk that this cumbersome method of publishing names for new species in perfect genera should be discouraged, and at his suggestion I have added a note prohibiting such a method on or after 1 Jan. 1966.

I find myself opposed to the interpretation of Cummins, Jørstad and Savile of Art. 59 in its application to types in the Uredinales (see examples 9 and 10), and

have suggested what is to my mind the only logical solution. (Para. 3 of reworded Article.)

I have been unable to think of a reasonable formula which would sanction the practice followed in examples 6, 7 and 8, and can see no alternative to the necessity in such cases for the publication of new names to be associated with the telial types.

I therefore suggest, for consideration, the following rewording of the Article.

Article 59

In Ascomycetes and Basidiomycetes (inclusive of Ustilaginales) with two or more states in the life cycle (except those which are lichen-fungi), the correct name of all states which are states of any one species is the earliest legitimate name typified by the perfect state. The perfect state is that which bears asci in the Ascomycetes, which consists of the spores giving rise to basidia in the Uredinales and of the chlamydo-spores in the Ustilaginales, or which bears basidia in the remaining Basidiomycetes. However, the provision of this Article shall not be construed as preventing the use of names of imperfect states in works referring to such states; and in the case of imperfect states, a name refers only to the state represented by its type.

When not already available, specific or infraspecific names for imperfect states may be proposed at the time of publication of the name for a perfect state or later, and may contain either the specific epithet applied to the perfect state or any other epithet available.

The nomenclatural type of a taxon of which the name has been ascribed to a genus characterised by a perfect state must be one of which the original description includes a description of the perfect state or of which the possibility cannot be excluded that the original author included the perfect state in his description. If these requirements are not fulfilled the name, although validly published, shall be considered illegitimate.

The recombination of the specific or infraspecific epithet of a name typified by an imperfect state with a generic name characterised by a perfect state shall be considered not validly published as a new combination; but shall be considered the validly published name of a new taxon if the recombining author provides a description (in Latin, on or after 1 Jan. 1935) of the perfect state and indicates a type (on or after 1 Jan. 1958) for the perfect state, and shall be attributed to the author of that name and to him alone. However, publication on or after 1 Jan. 1966 of a recombination based on an imperfect state and applied inclusive of the perfect state shall not be considered the valid publication of a new name for the perfect state.

Examples: (a) *Ravenelia cubensis* Arth. & J. R. Johnston (Mem. Torrey bot. Cl. **17**: 118. 1918), based on a specimen bearing only uredia (an imperfect state), was validly published but is considered illegitimate because it was described in a genus characterised by a perfect state. The correct name is *Uredo cubensis* Cummins (Mycologia **48**: 607. 1956), published as '(Arth. & J. R. Johnston) Cummins' (see Art. 72).

(b) The perfect state described (Sinensia **11**: 183. 1940) as "*Mycosphaerella aleuritidis* (Miyake) Ou n. comb., syn. *Cercospora aleuritidis* Miyake", accompanied by a Latin diagnosis of the perfect state, shall be considered not validly published as a new combination (since the type of the basionym does not bear the perfect state) but validly published as a new name of a new species, which must be cited as "*M. aleuritidis* Ou", based on the material examined by Ou which bore the perfect state. Since this is an undesirable method of publishing the name of new taxon, a name published in this manner on or after 1 Jan. 1966 will be considered not validly published either as a new combination or as a new name of a new taxon. The correct method of publication of this name would be "*Mycosphaerella aleuritidis* Ou, syn. *Cercospora aleuritidis* Miyake", though it is not essential (for the purposes of nomenclature) that the synonymy should be mentioned, and Ou could equally well have chosen any available epithet other than *aleuritidis*.

(c) *Corticium microsclerotia* (Matz) Weber, n. comb., syn. *Rhizoctonia microsclerotia* Matz, was published (Phytopathology 29: 565. 1939) with a description in English of the perfect state drawn up from specimen different from the type of *Rhizoctonia microsclerotia* Matz. Weber's recombination must nevertheless be considered to be based on Matz's type of *Rhizoctonia sclerotia* and is considered not validly published because this type does not show the characteristics of a perfect state genus. The name is likewise not validly published as a new name of a new taxon based on Weber's material, because no Latin diagnosis was provided. The correct name for this species is *Corticium microsclerotia* Weber in *Mycologia* 43: 728. 1951, where a Latin diagnosis was supplied for the perfect state: the epithet *microsclerotia* was not legitimately preoccupied in *Corticium*.

(d) A recombination from a genus typified by a perfect state into one typified by an imperfect state is legitimate (e.g. Arthur's recombinations from *Melampsora* into *Uredo* in *Result. Sci. Congr. int. Bot. Vienne, 1905: 338. 1906*), though this practice is not recommended.

Article 64

The following amendment will be necessary to Art. 64:

The present section (3) should be renumbered (4) and the following inserted as section (3):

“(3) when it was published in contravention of Art. 59.”

In conclusion, I wish to express my thanks to Dr. M. A. Donk for very helpful criticism.

DAS PROBLEM DER NOMENKLATUR CHEMISCHER TAXA

P. Tétényi (Budapest)

VENT beschäftigt sich (Taxon, vol. 9, p. 53) mit dem Problem der Nomenklatur chemischer Taxa und bestimmt: “es handelt sich auch bei solchen Sippen in allen Fällen um *Pflanzen*, die Unterschiede liegen *nur in der Anwendung verschiedener Methoden*”. Die Wissenschaft kommt dementsprechend “an Quantität und Qualität zunehmenden Unternehmungsmethoden” zu immer weiteren Kenntnissen, aber “an unseren Pflanzen stellen wir doch schliesslich *keine anderen* als morphologische Merkmale im weitesten Sinne fest”. Den Unterschied an morphologischen Merkmalen kann man mit verschiedenartigen Instrumenten erkennen und “auch aus dieser Betrachtung ergibt sich eine stärkere Akzentuierung *der Objekte* als der Methoden”. Bei der Einführung eines neuen Instrumentes müsste man charakterisierte Taxa entsprechend besonders bezeichnen, was eine weitere Komplizierung unserer Nomenklatur bedeuten würde.

Der Gedankengang ist wirklich interessant und die Schlussforderung, dass eine weitere Komplizierung der Nomenklatur zu meiden wäre, besonders anziehend.

Ist es aber auch tatsächlich so, dass sich die Differenzen in den Pflanzengruppen nur aus der Anwendung von unterschiedlichen Forschungsmethoden und -mitteln ergeben?

Nein, die Differenzen bestehen in den einzelnen Pflanzengruppen auch unabhängig von den angewandten Mitteln und Methoden und auch unabhängig davon, ob wir sie bereits wahrgenommen haben, oder ob ihre Entdeckung einer späteren Zukunft über-