

## Appendix A \*

### ON SOME REQUIREMENTS FOR VALID PUBLICATION OF NAMES

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In the early days of post-Linnaean botany there were few recognised restrictions on the way in which new names for plants could be brought into general use. Names published without a description of the plant, and even unpublished manuscript names, could frequently find their way into common acceptance among botanists. With the establishment and development in the present century of a Code of Nomenclature, internationally recognised and accepted, successive restrictions have been imposed on botanists proposing new names, to the effect that unless certain requirements are fulfilled a name will not be accepted as validly published under the Code. The intention of these restrictions has been to force botanists to provide certain basic items of information so that there will be as little doubt as possible as to the correct application of the name, and to rule out those names which are introduced in a casual or slipshod manner. Undoubtedly, the overall effect of these has been beneficial to nomenclature. Unfortunately, however, when one attempts to plug a hole in a leaking bucket one may find that the main flow is stopped but a number of smaller leaks appear at the periphery. So it seems to be in nomenclature. When new rules are introduced into the Code it is not always easy to foresee exactly how they will be applied in practice, and the precise interpretation and application of some of the rules under Chapter 4, Section 2 of the Code is certainly open to differing opinions.

The problem of invalid publication of names in botany today is a serious one. Over the last three to four years I have had to make decisions on whether over 5,000 names proposed in the same period for plants from Europe and neighbouring areas were validly published or not. Some 13–16%, or about one in every seven, I have adjudged not validly published (or, in a few cases, illegitimate) — see figures quoted in *Taxon* 16(2): 100 (1967), 17(1): 13 (1968), and 18(1) (in press). But in addition there have been many marginal cases in which the Code has not given a clear ruling and in which I have given the benefit of the doubt to the publishing author and accepted the name as validly published. In other cases I have felt that a name should not be accepted as validly published but have had to accept it under the present wording of the Code. My colleagues at Kew working on compilation of *Index Kewensis* are similarly repeatedly beset by difficulties of interpretation of the Code. I hope the present discussion and proposals will clarify and improve the relevant Articles.

#### *Publication of new combinations and 'nomina nova' — Article 33*

The second paragraph of Art. 33 reads "A new combination or a new name for a previously recognised taxon published on or after 1 Jan. 1953 is not validly published unless its basionym (name-bringing or epithet-bringing synonym) or the replaced synonym (when a new name or epithet is proposed) is clearly indicated and a full and direct reference given to its author and original publication with page or plate

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\* Proposals sent but not received before deadline.

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reference and date." The intention of this is excellent but its actual application is very frequently in doubt.

In the Paris (1956) Code the final part of this sentence (then under Art. 32) read simply "... is clearly indicated with a full reference to its author and original publication". As a result of a proposal by Deighton (*Taxon* 7 (9): 263–264, 1958) the specific reference to "page or plate reference and date" was added in the Montreal Code in order to clarify the meaning of "a full reference". But this immediately raises doubts about other items of information not specifically mentioned. It is common practice with some authors not to give the volume number in a reference, simply citing the year of publication. Thus, to quote one of many examples, Soó in *Acta Bot. Acad. Sci. Hung.* 11 (1–2): 246 (1965) gave the place of publication of the basionym of *Nonnea pulla* (L.) DC. subsp. *rossica* (Stev.) Soó as "Stev. Bull. Soc. Nat. Mosc. 1851: 572" without mention of the volume number 24 which was clearly given on the volume concerned. This to my mind is not a full reference, but since the Code does not specifically mention that the volume number must be included in the reference, while it does specifically mention page number and date, I have accepted the name as validly published. Indeed it might be difficult to include volume number in the stated requirements, for a number of journals appearing only annually have only a date and no volume number, as for example *Novit. Bot. (Praha)* and *Nov. Sist. Vyšs. Rast. (Leningrad)* among current journals. Borhidi in *Bot. Közlem.* 54 (3): 151 (1967) gave the basionym of the new combination *Alexitoxicum pannonicum* (Borhidi) Borhidi as "*Cynanchum pannonicum* Borhidi 1966, *Acta Bot. Hung.* 1966: 242" where the date 1966, given twice in the reference, seems to be clearly preferred to the volume number 12 which actually appears on the volume in question. But where a date is used as a volume number it may not always coincide with the actual year of publication. Thus the name *Rhinanthus angustifolius* Gmelin subsp. *bosniacus* (Behrendsen) Soó in *Acta Bot. Acad. Sci. Hung.* 14 (1–2): 151 (1968) certainly cannot be regarded as validly published; the place of publication of the basionym was given as "Behrendsen Verh. Bot. Ver. Brandenburg 1903: 211" but the volume concerned, designated 'Fünfundvierzigster Jahrgang 1903', is clearly given as actually published in 1904, and the reference thus has no date of publication. Furthermore, in such a case if there should have been two or more volumes published in the same year and no indication is given as to which is intended then such a reference is not acceptable.

However, one may well ask where one draws the line between a full reference and one which is not full? Nobody, I hope, would argue that commonly accepted abbreviations of journals, such as *Kew Bull.* for *Kew Bulletin*, should be ruled out on the grounds that they were not "full references". However, I find myself unwilling to accept "MBL" or "ÖBZ" as used by Soó in *Acta Bot. Acad. Sci. Hung.* 12 (3–4): 358 (1966) for (presumably) *Magyar Botanikai Lapok* and *Österreichische Botanische Zeitschrift* respectively. To the majority of botanists these abbreviations would be meaningless, and they surely cannot be regarded as full references for the purposes of the Code. The same can probably be said for "A. & G. Syn." used by Soó & Borsos in *Ann. Univ. Sci. Budapest Rolando Eötvös (Biol.)* 8: 318 (1966) for "Ascherson & Graebner, Synopsis der Mitteleuropäischen Flora", but it must be admitted that single letter abbreviations are commonly accepted for certain authors, particularly "L." for Linnaeus and "DC." for De Candolle, and many European botanists would know who A. & G. were. Can one accept such an abbreviated reference? Precise legislation to cover such cases is probably impossible

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to achieve, and an arbitrary subjective decision seems inevitable in some such borderline instances.

An apparently clear-cut case, however, is presented by the name *Mentha aquatica* L. f. *incana* (Boiss.) Zefirov in Wulff E.V., ed., Fl. Kryma 3 (2): 228 (1966) where the place of publication of the basionym *M. aquatica*  $\beta$  *incana* is given as "Boissier IV (1879). 544". The complete absence of the name of Boissier's publication surely prevents acceptance of this as a full reference, even though most botanists coming across it would correctly assume that the work concerned is the Flora Orientalis.

Further difficulties occur over citation of the page reference. I assume that the page required is that on which the name of the basionym appears and not the complete pagination of the book or paper in which this occurs. Thus the name *Dactylorhiza majalis* (Reichb.) P. F. Hunt & Summerhayes subsp. *cambrensis* (R. H. Roberts) R. H. Roberts in *Watsonia* 6(4): 261 (1966) is considered not validly published since the place of publication of the basionym is given as "R. H. Roberts, *Watsonia* 5, 37–42 (1961)" i.e. the complete pagination of the paper in which the basionym appeared but without any indication of which of these pages (actually p. 41) the basionym itself was published on. Similarly when Soó in *Acta Bot. Acad. Sci. Hung.* 13(3–4): 309 (1967) published three names — f. *subpallida*, l. *albida* and l. *purpurea* — under *Centaurea rhenana* with a combined reference for all three given as "Stoj. et Achtaroff *Centaurea* Bulgariens 1935: 48–49" none can be regarded as validly published; the basionym of the first actually appeared on p. 48 and of the second and third on p. 49, but for none of them is the exact page given. However, in other superficially similar cases where more than one page reference is given it seems that valid publication may be accepted. S. Pawłowska in Pawłowski, B., ed., Fl. Polska 11: 89 (1967) published the name *Teucrium montanum* var. *praemontanum* (Klok.) S. Pawł. giving the basionym as "*T. praemontanum* Klok. Fl. URSS 20: 67–68, 506–507 (1954)"; in fact the first two page numbers refer to the description in the Russian text (though actually 66–67) while the last two refer to the latin description validating the basionym, the name actually appearing on p. 506. A similar situation is found in publication of the name *Allium nigrum* var. *dumetorum* (Feinbrun & Szélub.) Mouterde, *Nouv. Fl. Liban Syrie*: 281 (1966) where a very full reference to the paper in which the basionym appeared was given, with the particular reference to "pp. 146–147"; the basionym *A. dumetorum* actually appeared on p. 146 but the accompanying description continued on to p. 147. I do not wish to rule these new combinations out on the grounds that more than one page number was given (a proposal by G. F. Laundon in *Taxon* 17(4): 461 (1968) actually recommends for practical reasons that references to places of publication of names should include the full pagination of the whole protologue), but if in such a case the description ran over a number of pages, and all of these were included in the reference, it might be difficult to accept this and still not accept the *Dactylorhiza* example quoted above.

Similar doubts may even arise over dates of publication. Although it is not clear whether or not a new combination was intended, I have noted that Danert apud Mansfeld in *Kulturpflanze Beih.* 2: 373 (1959) made a reference to "*Mentha spicata* L. var. *crispa* Benth. Lab. gen. et spec. (1832/36) 174 (sub *M. viridis*)". The date given covers the five years over which the whole of Bentham's work appeared, but the actual year in which the name concerned appeared is not indicated, no doubt because of the difficulty of determining when individual parts were actually published. Similar difficulties may occur where other undated publications are concerned,

perhaps particularly in the case of names validly published by the distribution of exsiccatae bearing printed labels where the date of distribution may now be impossible to ascertain. In such cases a reference without an exact date (i.e. to the nearest year) may be acceptable if it is clear that this cannot be determined, but if the exact year of publication is readily available and not given then the reference should be considered unacceptable in the context of Art. 33. Thus the combination *Chronanthus orientalis* (Loisel.) Frodin & Heywood published by Heywood in *Feddes Repert.* 79(1-2): 21 (1968) with the basionym cited as "*Cytisus orientalis* Loisel., *Nouv. Duham. Arb. ed. nov.* 156 (1801-1819)" is not validly published; at least in the Kew copy, the 'Nouveau Duhamel' comprises vol. 5 only of the 7 volumes of the second edition (1800-1819) of Duhamel's 'Traité des Arbres' and is clearly dated 1812. (It seems in fact that there is variation between copies in the use of the title 'Nouveau Duhamel' - c.f. Stafleu F. A., *Taxonomic Literature* 114. 1967 - and the reference would be best given as 'Loisel. in Duham., *Traité Arbres Arbustes ed. 2*, 5: 156 (1812)).

In a considerable number of cases when it is not immediately clear that the requirements of this Article have been fulfilled, further examination of the paper concerned may show that the full reference is in fact given but in an obscure place. It seems, in fact, to be common for some or all of the necessary details to be given in a list of bibliographic references at the end of a paper, well removed from the place in which a new combination is proposed. Thus the basionym of *Erysimum arbuscula* (Lowe) Snogerup in *Op. Bot. (Lund)* 13: 9 (1967) is given simply as "*Cheiranthus arbuscula* Lowe 1856"; at first it appears that no full reference is given, but in the bibliography at the end of the paper, on p. 69, one finds full details of Lowe's 1856 publication with a precise page reference, p. 289, stated. Among many other similar examples I may quote publication of the names *Potentilla libanotica* Boiss. var. *isaurica* (P. H. Davis) B. Pawłowski in *Fragm. Fl. Geobot.* 11(1): 81 (1965), *Sanguisorba diandra* (Hook.) Nordborg in *Op. Bot. (Lund)* 11(2): 60 (1966) and *Sideritis catillaris* Juz. var. *chlorostegia* (Juz.) Zefirov in Wulff E. V., ed., *Fl. Kryma* 3(2): 101 (1966). Now although such practice certainly causes difficulty, or at least extra work, for anybody wishing to determine whether names are validly published or not (and particularly for those who compile indexes of such names and have not time to peruse every paper at length), it must be admitted that all the details *are* provided, even though they may be scattered from one end of a publication to the other. It seems, therefore, that such names must be accepted as validly published.

But how far must one be expected to search through a paper in quest of a full reference? The name *Tamarix africana* Poir. var. *fluminensis* (Maire) Baum appears in Baum, *Monogr. Rev. Gen. Tamarix* (1966) on p. 97 without any further indication of what the basionym is or where it was published. It thus appears at first to be not validly published there. If, however, one does some detective work, finding that among the citation of specimens seen by the author is one annotated as the type of *T. brachystylis* var. *fluminensis* Maire and then checking this in the index at the back, one may eventually discover that on p. 94 of the same work this name, apparently the basionym though this is not stated, appears with a full reference to author and place of publication. Can this reference, three pages removed from the new combination and with no cross reference, be said to allow acceptance of the combination as validly published? Again, the new combination *Salix arctica* Pall. subsp. *crassijulis* (Trautv.) Skvortsov in Tolmatchev, *Arkt. Fl. SSSR* 5: 59 (1966) is published with the basionym given as "*S. crassijulis* Trautv. *Salic. frigid.* 308", i.e. with all necessary details except that the date is omitted (apparently accidentally). How-



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ever, quite by chance, a reference on the previous page (p. 58) to a synonym of *S. arctica* reads "*S. diplodictya* Trautv. Salic. Frigid. (1832) 307" and it may be argued that since the date of Trautvetter's work is given *somewhere* in the same paper the name *S. arctica* subsp. *crassijulis* is here validly published. (Possibly Skvortsov himself does not agree with this argument since he has in fact correctly made the combination again elsewhere, also in 1966). A very similar situation is found with *Mentha spicata* L. var. *eisensteiniana* (Opiz) Tacik in Pawłowski, Fl. Polska 11: 217 (1967) where the basionym is cited without date of publication — "*M. eisensteiniana* Opiz in Naturalient.: 301" — but two pages earlier, p. 215, Tacik himself makes another combination *M. spicata* var. *walteriana* with the basionym "*M. walteriana* Opiz in Naturalient. 9: 132. 1825" i.e. the same publication with the date here given. Another example is the name *Euphorbia balsamifera* Ait. subsp. *adenensis* (Deflers) Bally in *Candollea* 20: 34 and 36 (1965) where the combination is actually made in the captions to an illustration and a map respectively, with the basionym and full details given (somewhat incidentally) in the discussion on p. 31. In all of these four examples there is no reference given from the place where the combination is made to the place where the details of the basionym are given, and it is only by luck or laborious effort that the conditions for valid publication can be found to have been fulfilled. Although I have reluctantly accepted valid publication in these instances I would prefer that some ruling should be made in the Code to enable such cases to be dismissed as not validly published.

The use of back-references such as '*loc. cit.*', '*op. cit.*' and '*tom. cit.*' are of course quite permissible in citing the place of publication of a basionym, so long as it is clear where the place referred to is and all the necessary details are in fact given there. In some cases, unfortunately, it may require a lengthy search to find where the '*loc.*', '*op.*' or '*tom.*' is actually cited, and if there should be any doubt then the reference should not be accepted.

The examples quoted in the discussion above are just a few of the many difficult cases on which I have had to come to some decision about a "full reference" over the last three or four years. As I have commented already it is probably impossible to cover every contingency in the Code and some subjective decision as to what constitutes "a full and direct reference" seems inevitable. Nonetheless, I hope that the following proposals arising from the above discussion may clarify Article 33 without themselves raising any further difficulties, or unnecessarily complicating the Article.

*Proposal 254.* Include the existing Note 1 of Art. 33 in a new Note 1 as follows: "Note 1. a) Omission of the volume number does not render a reference incomplete if it is clear from the year given which volume is intended. b) Abbreviations of names of journals or books in a generally accepted or comprehensible form are permissible; abbreviation of such to single initial letters throughout does not constitute a full reference. c) A page reference must be to the page on which the basionym is validly published, not to the complete pagination of the work concerned, but additional inclusion of pages on which the whole protologue is published does not preclude acceptance of the reference. d) A date must be given to at least the nearest year unless this is not generally accurately ascertainable. e) The full details of the reference must be clearly associated with the new combination or new name at the place of publication, or may be given in the collected bibliographic references for the whole work, or may be clearly referred to within the same work by means of '*loc. cit.*' etc., but incidental inclusion of the details in the same work

without a cross reference is not acceptable. Mere reference to the *Index Kewensis*, the *Index of Fungi*, or any work other than that in which the name was validly published does not constitute a full and direct reference to the original publication of a name." Examples to illustrate this Note may be drawn from the above discussion.

A further problem arises from the unfortunate word 'indicate', which in paragraph 2 of Art. 33, and elsewhere in the Code, is open to different interpretations. How may we legitimately interpret the ruling that a new combination "is not validly published unless its basionym... is clearly indicated..."? Conflicting views have already been given by Laínz in *Taxon* 10(9): 268 (1961) and Heywood in *Taxon* 11(3): 68-69 (1962), the former asserting that it is not necessary to actually *state* the basionym or even say what the exact form of it is, and the latter insisting that such a statement is necessary. The case in point was the publication by Laínz in 1960 of "*Leptogramma pozoi* (Lag., *Genera et species*...: 33. 1816) Laínz, comb. nova" where the basionym *Hemionitis pozoi* Lag. was not stated but the full reference to its place of publication was given. Now while I agree with Heywood that it is highly desirable that the basionym itself should be actually stated, I cannot find that Laínz's practice is contrary to the actual wording of the Article. There is a big difference between a name being "stated" and a name being "indicated". The Oxford English Dictionary includes as meanings of 'indicate' the following "1. To point out, point to, make known, show (more or less distinctly): 2. To point to or towards the presence, existence, or reality of". If one is buying a book listed in a catalogue one can clearly *indicate* the name of the book, by pointing a finger or asking for the third book on the fifth page, *without stating* the name. Similarly Laínz has undoubtedly clearly indicated the basionym without actually stating it; there is absolutely no doubt that the basionym of his *Leptogramma pozoi* is *Hemionitis pozoi* Lag. The assertion of Deighton in *Taxon* 7: 264 (1958) that "The wording of the Stockholm Code... made it clear that the work in which a new combination appears must contain a statement of what the basionym is", quoted by Heywood (*loc. cit.*), is incorrect if taken as meaning that the basionym itself must be stated. However desirable this is, it has never been written in to the Code. As it stands at present the Code does not demand explicit citation of the basionym in valid publication of a new combination.

I have recently accepted as validly published large numbers (i.e. literally hundreds) of new combinations without actual statement of the basionym, particularly those published by Soó in his series of papers in *Acta Bot. Acad. Sci. Hung.* and those in Maire's *Flore de l'Afrique du Nord*. The great majority of these have in fact been in the form such as "*Bupleurum longifolium* L. ssp. *aureum* (Fischer in Hoffm. *Genera Umbell.* 115, 1814 p. sp.) Soó stat. n." (in *Acta Bot. Acad. Sci. Hung.* 12 (1-2): 116 (1966)) where the actual name of the basionym can be deduced from the information given (presumably *Bupleurum aureum* Fischer in the example quoted). Although this form of publication is perhaps acceptable to everybody (see Heywood *loc. cit.*, p. 69, para. 4) these names, all validly published under the Code operating at the time, would have to be regarded as not validly published if the proposal of Heywood (*loc. cit.*, final paragraph) were adopted retroactively in its present form. If direct citation in full of the basionym is to be made compulsory it should only be made so from a future date. While asking the Seattle Congress to consider the following proposal as an alternative to that earlier suggested by Heywood I must admit that I have some reluctance about introducing yet another date into the Code (the second in one paragraph) and wonder whether continued acceptance of publication

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of new combinations proposed in the same way as *Leptogramma pozoi* (Lag.) Lainz and *Bupleurum longifolium* subsp. *aureum* (Fischer) Soó might be preferable.

*Proposal 255.* Add to Art. 33, para. 2, the following:  
“After 1 Jan. 1971 such a reference must include the explicit citation of the basionym itself”.

This leads on to consideration of the first paragraph of Art. 33, in which the actual wording seems to me to be in fact contrary to what I believe is probably more or less universal practice in nomenclature today. Current practice is to accept a new combination only if it is actually made, i.e. unless “an epithet is associated with a particular generic or other name” (see end of first Example paragraph). Thus if an author writes that *Bellis perennis* is to be transferred to *Taraxacum* he does not validly publish the name *Taraxacum perennis* unless he actually makes the combination, i.e. juxtaposes the name *Taraxacum* and the epithet *perennis*. However, what Art. 33 actually states is that “A combination is not validly published unless the author definitely *indicates* that the epithet or epithets concerned are *to be used* in that particular combination” (my italics). Again we have the word ‘indicate’ which may or may not mean ‘explicitly state’, but worse still the phrase ‘are to be used’ is clearly in a *future* tense. In the *Blephilia* example quoted as a combination not definitely indicated I would suggest that Rafinesque gave fairly clear indication that the name *Blephilia ciliata* was *to be used* in the future. He did not, however, actually use the name himself. I propose amendment of the Article to fit in with accepted practice.

*Proposal 256.* Amend the first paragraph of Art. 33 to read: “A combination is not validly published unless the author actually uses the epithet or epithets concerned in that particular combination”.

I am not sure how to interpret “bibliographic errors”, which according to Note 2 of Art. 33 “do not invalidate the publication of a new combination”. Presumably if one merely gives the wrong page reference or wrong volume number by mistake this does not matter, but I wonder how much latitude one can allow. In publishing the combination *Acetosa cordata* (Desf.) Á. Löve & Kapoor in *Taxon* 16(6): 520 (1967) cited the basionym as “*Rumex cordatus* Desfontaines in Cat. Hort. Paris ed. 2 (1829), p. 40”, but in fact the basionym, *R. cordatus*, was validly published on p. 389 of ‘Cat. Hort. Paris’ (1829) which is the third, not second, edition of a work of which the first two editions were called ‘Tableau de l’École de Botanique’. The reference to p. 40 apparently refers to the publication of the name as a *nomen nudum* in the first edition of Tab. École Bot. in 1804. Is the combination validly published? I assume that if one gives a reference only to a *nomen nudum* in publishing a new combination this cannot be accepted as being merely a bibliographic error. Thus the name *Lycopodium annotinum* L. subsp. *pungens* (La Pyl.) Hultén in *Ark. Bot. (Stockholm)* 7(1): 7 (1968) is not a validly published name since the basionym was given as “*Lycopodium pungens* La Pylais in Desv. in Mém. Soc. Linn. Paris 6, 1827 p. 182” where the name was a *nomen nudum*. (Flora Europaea gives the name as *L. pungens* La Pylaie ex Komarov).

### *Gradual fulfilment of requirements for valid publication*

As the second sentence of Art. 45 recognises, the requirements for valid publication may not all be met simultaneously. One may publish a *nomen nudum* first, then later supply a latin description of the taxon, and later again designate the type

of the name, valid publication being effected only at the last instance. Or in publishing a new name one may refer back to a previously published description instead of actually giving it at the same time as publication of the name. Such cases seem to be closely analogous to publication of new combinations, for which, as discussed at length above, a "full and direct reference" is required. However, in the case of a new name validated by reference to an earlier published description the reference to it may be direct *or indirect*, as stated in Art. 32. In 1965 Quézel & Contandriopoulos published a number of names of new species and subspecies from Greece in *Candollea* 20: 51–90, but omitted to designate a type of any of them. In order to rectify the situation they repeated these names without descriptions and indicated the type of each in a note in *Taxon* 16 (3): 239–240 (1967). For none of these names did they supply a page reference to the place of publication of the latin description of the taxon given in their earlier paper, but as Art. 32 allows direct *or indirect* reference to a description the names may be regarded as validly published. It seems to me, however, that if the Code demands a full and direct reference to the place of publication of the basionym of a new combination or *nomen novum* then it should demand the same in cases such as these. In the absence of such a requirement anybody may at the moment publish a new name with only the vaguest reference to a latin description, perhaps simply an author's name with no mention of place or date of publication. The tendency in the Code is clearly to demand precision of authors publishing names, and I therefore make appropriate proposals below.

This point raises the question of a most unfortunate loop-hole in the application of Art. 33 paragraph 2. A new combination proposed with an incomplete reference is not validly published *as a new combination*. There may, however, be an indirect reference (i.e. "... indication by citation of the author's name or in some other way that a previously and effectively published description or diagnosis applies to the taxon . . ." — Art. 32, Note 1), to a description which can be accepted under Art. 32, so that the intended new combination might in fact be validly published *as a name of a 'new taxon'*. Thus in the case of *Mentha aquatica* L. f. *incana* (Boiss.) Zefirov, quoted above (p. 44) as an example of a new combination not validly published, it can quite justifiably be claimed that there is a reference to a description (and Boissier's description was in Latin) which is acceptable under Art. 32. The name *M. aquatica* f. *incana* Zefirov — but not (Boiss.) Zefirov — would therefore be validly published as the name of a 'new taxon' if Zefirov had designated a type, which, however, he did not. (The name would have been illegitimate as a later homonym, but this would not have been so if the proposed new combination had been a transfer from one species to another instead of a change of rank under the same species). It seems clear, therefore, that a full and direct reference should have been demanded in Art. 32 since the Stockholm Congress for reference after 1 Jan. 1953 to an earlier description, in order to eliminate this loop-hole and avoid these unfortunate complications. If such a requirement should be approved by the Seattle Congress there is a choice between dating it back to 1953 or from a future date. As the former would retroactively invalidate names such as those of Quézel & Contandriopoulos quoted above I make the proposals to date from 1971.

*Proposal 257.* In Art. 32 paragraph 1 delete "(direct or indirect)" and add at the end of the paragraph "From 1 Jan. 1971 such a reference must be full and direct as defined in Art. 33".

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*Proposal 258.* Make the same addition as in the previous proposal to Art. 36.

*Proposal 259.* Add to the first paragraph of Art. 45 "From 1 Jan. 1971 valid publication of a name for which relevant requirements have been fulfilled at different times is effected only when a full and direct reference (as defined in Art. 33) is given to the place or places where these requirements have been previously published". This covers the case of an author who first of all publishes a name with designation of its type and only later provides a latin description of the taxon. It requires that on the second occasion a full and direct reference be given to the place of publication of the type, which is not covered by Arts. 32 or 36.

### *Indication of the type of a name — Article 37*

Art. 37 states that "Publication on or after 1 Jan. 1958 of the name of a new taxon of the rank of family or below is valid only when the nomenclatural type is indicated (see Arts. 7–10)". Here we are again faced with interpretation of the meaning of the word 'indicated', and with a complete absence of any guidance as to how much information about the type is required.

In consideration first of all of names of species and lower ranks, it seems that current practice in 'indicating' the type is as diverse as it could possibly be, varying from simple mention of a locality or the briefest mention of a type to citation of the fullest details with perhaps publication of a photograph of the specimen. In a normal indication of a type specimen one might expect to find a statement of at least the place of origin of the specimen, the date of collection, the collector's name and the number of the collection if there is one, the herbarium or other institution in which the specimen is preserved, and a statement, including the word 'type' or 'typus' or preferably 'holotypus', that this is the type. In practice it seems that any number or combination of these items may in fact be given. Thus, for example, *Quercus* × *polycarpoides* Georgescu & Ciobanu in *Stud. Cerc. Biol., Bot. (București)* 18(1): 6 (1966) was described in Latin and a locality was given "In Munții Pirini, deasupra satului Katunci, la 650 m altitudine" with no further information or suggestion that a specimen was collected from the locality. Such examples are very common. In other similar cases the locality may be cited as '*locus classicus*' implying that it is the 'type locality' but still without mention of a specimen. *Ornithogalum amblyocarpum* Zahariadi in *Rev. Roum. Biol. (Bot.)* 10(4): 290 (1965) similarly had a single locality given, but here the name of a collector was also mentioned, suggesting at least that there is, or was, a specimen in existence. For *Potentilla chrysantha* Trev. var. *karakoramensis* Hartmann in *Bot. Jahrb.* 85(2): 313 (1966) the locality was given "alpine Wiese . . . 4130 m im westlichen Seitental bei Mango" with a list of associated species followed by "Blütezeit: Juli — 16.7. 1962"; no actual mention of a type was made, but as the paper concerned is an account of the collections of an expedition to the Karakoram in 1962 there is presumably a specimen collected on 16 July preserved somewhere. But again Pázmány in *Cat. Sem. Note Bot. (Cluj)* 1967: 63–64 (1968) described a number of new taxa, giving a single locality and a date for each, but in one case the plant was given as frequent; to my mind this suggests either that the citing of the locality was here not an indication of an herbarium specimen or that more than one specimen had been collected. In other cases a specimen is actually cited by mention of an author's name and collecting number but without indication of where the specimen was collected or where it has been deposited. In the case of *Malva* × *inodora* Ponert in *Feddes Reptert.* 73(2): 93 (1966) a locality was given "Velke Losiny prope castellum . . ."

followed by "Typus: Praha" which presumably refers to the place where the specimen is preserved, but no other details of a specimen were given and there may well be several specimens in Praha from this locality. For *Fraxinus* × *borzae* Georgescu & Tutunaru in *Contrib. Bot. (Cluj)* 1967: 128 (1967), although the place where the type is deposited was stated by "Holotypus in herbario Institutii Biologici Academiae Romaniaae conservatur", no locality of origin or any other details were given; apparently one can only discover these when one actually sees the specimen. A further stage, where the details are more or less adequate, is found in the case of *Stipa iberica* Martinovsky in *Feddes Repert.* 73(3): 150 (1966) where the locality was given as "Locus classicus: Herdam in Hispania; legit R. Agelet" followed by "Typus in herbario Institutii botanici universitati Florentinae conservatur".

For which of such names can it be said that "the nomenclatural type is indicated"? As I have pointed out above the word 'indicate' can cover a very wide range of practice. My own feeling is that a simple citation of a locality, even if it is designated as *locus classicus*, cannot be interpreted as an indication of a type specimen. If any other detail suggesting existence of a specimen is given, such as a precise date or a collector's name, I have accepted that Art. 37 has been complied with, on account of the breadth of possible interpretation of the word 'indication' used by the Code. Again, if the word 'type' or its equivalent is actually used I have accepted this, even if little or no useful information about the type is given, for at least there is an indication that a type exists which is all that Art. 37 demands.

In all the examples given above there was only one locality or specimen cited. In cases where more than one locality or specimen is mentioned without a statement that one or other refers to or is the type it seems clear that under Art. 37 the name is not validly published, for only one can be "the nomenclatural type" (singular). (It is in fact desirable for the Code to refer to "the holotype" instead of to "the nomenclatural type" and this is proposed below). But I wonder to what extent one can assume that a single specimen cited is the type if no statement to that effect is given. *Athyrium filix-femina* Roth subforma *imbricata* Papp in *Comun. Bot. (București)* 4: 98 (1967) was published with simply a Latin description, a single locality and a date, whereas f. *laxissima* of the same species published by the same author on the previous page had two localities and two associated dates. I have taken the first as validly published (a type being 'indicated') but the second not so. But it seems that the author probably had little thought of types when he published these and other names in the same paper, and just happened to cite one locality for some new taxa and more than one for others. In the absence of an explicit statement from the author the only practical course to follow seems to be to accept citation of a single specimen as indication of the type and to rule out citation of more than one. A proposal to add an explanatory note to this effect, with similar allowance for names of taxa above specific rank, is made below.

I remark in passing that I applaud the honesty with which a recent author describing a new species stated that there were *no* specimens preserved thus "Exs.: nulla / v.v.", but regret that this clearly condemns the name proposed as not validly published.

But I find that many cases where it seems that valid publication must be accepted under the present vague ruling of the Code are highly unsatisfactory. The type should not be merely 'indicated' in any way however vague; it should be actually designated by citation of certain minimal details of the specimen. Furthermore it is of fundamental importance to other taxonomists that the place where the type is deposited should be made known, for if nobody but the publishing author knows where it is nobody else can see it, and in many cases the type might just as well not exist at all.



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Recommendation 37B does advise that "When the nomenclatural type of a new taxon is a specimen, the place where it is permanently conserved should be indicated", but as only a Recommendation with no legislative power this does not seem good enough. (I have commented elsewhere on the unfortunate wording of the "type of a new taxon".) Many authors at the moment just ignore the Recommendation and literally hundreds of new names are being validly published every year *without* any indication at all of where the type is conserved. This is unnecessarily creating difficulties which future generations will inherit. I therefore make a proposal to demand explicit citation of the type with a statement of where it is deposited.

*Proposal 260.* In Art. 37 replace "the nomenclatural type" by "the holotype of the name". This proposal supersedes an earlier proposal by myself and A. O. Chater to substitute "the type of the name". As an example of practice contrary to this I quote the name *Ornithogalum* sect. *Galactea* Zahariadi in *Rev. Roum. Biol. (Bot.)* 10(4): 277 (1965) where two types, *O. ponticum* and *O. brevistylum*, were designated as lectotypes.

*Proposal 261.* After Art. 37 add the two following Notes: "Note 1. Citation of a single element (specimen, species or genus where appropriate) may be assumed to be an indication of the holotype even without a direct statement that this is so. / Note 2. In the case of a name of the rank of species or below a mere citation of a locality without further reference to a specimen does not constitute indication of a type. Citation of a collector's name or collecting number, or a date of collection, or any other reference to a detail of a specimen, may be assumed to be an indication of a type".

*Proposal 262.* i) Add a further paragraph to Art. 37, as follows: "On or after 1 Jan. 1971 for purposes of valid publication (1) indication of the holotype must include explicit use of the word 'holotypus' or 'typus' or their direct equivalent; (2) for a name of a new taxon of the rank of species or below the type specimen must be actually designated by citation of the locality of its origin and/or some details of the collector, or date of collection, or the collecting number; and (3) the herbarium or other institution in which a type specimen is permanently conserved must be stated. Such a statement of an herbarium or other institution may be in an abbreviated form such as is recommended in *Index Herbariorum* or similar work." ii) Delete accordingly the part of Recommendation 37A after the word 'diagnosis' and all of Recommendation 37B.

The direct equivalents of the words 'holotypus' and 'typus' would include their direct translation into any other language and also words such as 'holonomenifer' (sometimes spelled hyphenated as 'holo-nomenifer') and 'nomenifer' adopted by Traub in *Plant Life* 23: 67 (1967) and elsewhere. A work similar to *Index Herbariorum* would include *British Herbaria* by D. H. Kent et al. (1957) in which additional similar abbreviations are given. Difficulty might arise over the word 'locality', and probably any geographical location would have to be accepted.

A further difficulty over application of Art. 37 arises from the fourth paragraph of Art. 22 which was introduced into the 1966 Code following the Edinburgh Congress. It states that "When the epithet of a subdivision of a genus is identical with or derived from the epithet of one of its constituent species, this species is the nomenclatural type of the epithet unless the original author of the subdivision of the genus has designated another type". In proposing an insertion to this effect Prokhanov in *Taxon* 13(1): 25-26 (1964) seems to have been thinking primarily of names proposed

earlier than 1958 and not to application of Art. 37. But if after 1958 somebody proposes such a name *without* designation of a type, can the type be inferred from the epithet chosen? For example, the name *Actaea* ser. *Spicatae* Kemularia-Nathadze, Ranal. Kavk. Takson. 50 (1966) was proposed without designation of a type, but it might be argued that under Art. 22 para. 4 the type is 'indicated' as *Actaea spicata* L. by choice of the epithet *Spicatae*. However, it is not always so simple as that. Thus the name *Narcissus* subsect. *Angustifolii* A. Fernandes in *Bol. Soc. Brot.* ser. 2, 40: 241 (1966) was also published without designation of a type, but when one consults *Index Kewensis* one finds that there are four different publications of the name *Narcissus angustifolius*, so which is the type? In fact no *N. angustifolius* is recognised under this subsection by Fernandes, and it seems that the epithet was chosen simply because the species in it are characterised by narrow leaves! (I am very grateful to Professor Fernandes for discussion of this example). The same arguments of course apply to names of families and subdivisions of families. Art. 10 states that "The nomenclatural type of a . . . family or of any taxon between family and genus is the genus on whose present or former name that of the taxon concerned is based". Again there is a possibility of homonyms within the same family (and conservation of generic names from a later date with a different type frequently gives rise to this) and to avoid any doubt an explicit statement by the author of a name of a family or subdivision of a family as to the type of this name is desirable.

*Proposal 263.* Add to Art. 37 a new paragraph "For purposes of valid publication the type of a name is not indicated merely by the choice of the epithet adopted".

I make no mention here of other problems of indication of a type species or type genus for names above the rank of species. The problems involved in defining what the type of a generic name actually is are complex and beyond the scope of the present notes.

#### *Incidental and unintentional publication of names*

The first paragraph of Art. 34 includes the statement that "A name is not validly published . . . when it is merely mentioned incidentally". I confess that I have never understood what this means (incidentally to what?) or how, if at all, it is applied in practice, and I suggest that in fact very many names which can be regarded as "merely mentioned incidentally" are currently universally accepted as validly published. The only example of this given in the Code is the generic name *Jollya*, "mentioned incidentally by Pierre (Notes Bot. Sapot. 7. 1890) in the discussion of the stamens of another genus". The name *Jollya* certainly is not validly published in the place cited, but this is simply because no description or diagnosis was given; it is in fact a *nomen nudum*. If a description or diagnosis had been given the name would surely have been generally accepted as validly published, despite the fact that it appeared incidentally in a passage discussing another genus. Thousands of names have been validly published in footnotes or in observations under discussions of other taxa. As just one example I quote valid publication of the new combination *Calystegia soldanella* by Robert Brown in a note "Facies *C. soldanellae* L. diversa . . ." after the description of a new species *C. reniformis* R. Br., Prodr. Fl. Nov. Holland.: 484 (1810).

The Article explains, however, in Note 2 that "By 'incidental mention' of a new name or combination is meant mention by an author who does not intend to introduce

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the new name or combination concerned". But this leads us into very deep water. How do we know the *intentions* of an author? And what is meant by 'introduce' in this context? In the *Jollya* example given the author, Pierre, goes on in the next sentence, referring to *Jollya* and another genus, by saying "Ces deux derniers genres néo-calédoniens sont des Chrysophyllées dont je parlerai plus loin", so perhaps after all he did actually intend to introduce a new name! But how are we to know now? Is evidence of an author's intention, perhaps by the words '*comb. nov.*', '*sp. nov.*' etc., to be demanded before valid publication of a name is accepted? Such evidence of the author's intention very often is not given, and it would be impossible to legislate in the Code on this point.

In my opinion new names can in fact be validly published by accident. For example, in their account of Vitaceae in *Flora Zambesiaca* 2(2): 439–492 (1966) Wild and R. B. Drummond gave many species of *Cyphostemma* with names cited as having been published as new combinations by Descoings in 1960, not realising that Descoings had failed to cite full details of the place of publication of the basionym and so had not validly published the names; but as in every case Wild and Drummond have cited, according to normal practice in *Flora Zambesiaca*, the basionym with full reference to its place of publication they have in fact themselves effected valid publication in the Flora. It is clear that new combinations were not intended, and indeed the authors have intimated to me privately that if they had realised that Descoings had not validly published the names they might never have done so themselves. But although these *Cyphostemma* names were not intended as new combinations in *Flora Zambesiaca* they are certainly not "merely mentioned incidentally"; they were names clearly enumerated for taxa recognised in the Flora, and are validly published there. Indeed, one questions why 'incidental mention' should ever have been defined in terms of lack of intention to introduce a name, for the two are by no means the same thing

It thus seems that little or no notice is taken in practice of the literal meaning of item (3) of Art. 34, while strict application of the interpretation of it suggested by Note 2 would be both undesirable and impossible to achieve. I therefore propose deletion of both from the Code.

*Proposal 261* (see also 264a). Delete item (3) from the first paragraph of Art. 34; delete Note 2; and delete Example (3).

### *Simultaneous publication of more than one name for a taxon*

The last paragraph of Art. 34 reads "When, on or after 1 Jan. 1953, two or more different names (so-called alternative names) are proposed simultaneously for the same taxon by the same author, none of them is validly published . . .". I have invoked this ruling in the peculiar case of *Campanula acarnanica* Damboldt in *Bot. Jahrb.* 84(3): 341 (1965). Having fulfilled the requirements for publication of this specific name in the text of his paper, the author then added an afterthought at the end (published at the same time) in which he said that further consideration had led him to believe that the taxon should be regarded as only a subspecies of *C. garganica*. If he had then stated clearly that for *C. acarnanica* in the main text one should read *C. garganica* subsp. *acarnanica* Damboldt I would accept this as valid publication of the subspecific name (though I would still think it an unfortunate way to do it).

But instead of this the author *accepted the specific name* and published a new combination "*C. garganica* Ten. ssp. *acarnanica* (Damboldt) Damboldt comb. nov." (loc. cit. p. 358). This seems to be even more undesirable than the alternative names for the same species under different genera given in the examples in the Code, for in this case the implication is clearly that the epithet *acarnanica* for this taxon is to be dated for purposes of priority from 1965 at both subspecific and specific rank. I find no alternative but to regard both as not validly published under Art. 34, and have given them so in *Regnum Vegetabile* 45: 47 (1966). (Damboldt has later cast doubt on the correctness of my decision in *Bot. Jahrb.* 88(2): 200–203 (1968), but for the sake of safety has republished the combination *C. garganica* subsp. *acarnanica* (Damboldt) Damboldt (loc. cit. p. 201). But it is clear that if the name *Campanula acarnanica* was not validly published in 1965 one cannot make a new combination based upon this, so the combination is still not validly published! However, in 1968 subspecific rank alone is definitely accepted, and a reference is given to a Latin description of the taxon and the type of the name, so the new name *C. garganica* subsp. *acarnanica* Damboldt, not (Damboldt) Damboldt, is validly published in 1968).

I have also invoked this Article in the case of the two names *Oenothera* × *drawertii* and *O.* × *polgari* published by Rostanski in *Acta Bot. Acad. Sci. Hung.* 12(3–4): 341 and 347 respectively (1966). Both were avowedly published for hybrids between the same two species, *O. suaveolens* and *O. depressa*, and so the same hybrid specific name should therefore be applied to both taxa, one being given as a nothomorph of the other. As two names at specific rank were simultaneously proposed for the same interspecific hybrid, both are not validly published under Art. 34.

*Proposal 264a.* Include the two above cases as Examples of the final paragraph of Art. 34.

#### *Names proposed with an incorrect ending*

Art. 32 states that "In order to be validly published, a name of a taxon must . . . have a form which complies with the provisions of Arts. 16–27 (but see Art. 18, notes 1, 2 and 3)". The second Note of Art. 18 allows that "When a name of a family has been published with an improper Latin termination, the ending must be changed to accord with the rule, without change of the author's name". But no comparable statement is anywhere made for names of any ranks other than that of family. Now Professor A. Fernandes has generously drawn my attention to the fact that he published the name *Narcissus* subsect. *Angustifoliae* A. Fernandes in *Bol. Soc. Brot.* ser. 2, 40: 307 (1966) in the wrong form by mistake; to comply with Art. 21 paragraph 2 it should have agreed in gender with the generic name and been spelled subsect. *Angustifolii*. Now according to Art. 32 quoted above this could not be simply corrected to *Angustifolii* but must be regarded as not validly published. (In this actual example there is a further complication, as discussed above, p. 53). Similarly, apparently, a specific name in which the epithet does not agree in gender with the generic name, and which is therefore contrary to Art. 23 paragraph 4, is not validly published. I believe, however, that this is not generally accepted in practice, and propose the following to bring the Code into line with established custom.

*Proposal 265.* Add a second Note to Art. 32 as follows: "Note 2. Names published with an incorrect Latin termination but otherwise in accordance with this Code may be changed to accord with Arts. 16–27, without change of the author's

name, and regarded as validly published." Note 2 of Art. 18 may then be deleted.

### *Names of hybrid taxa*

According to Art. H5 different hybrid groups derived from the same parent species "are termed nothomorphs". This, however, is an over-optimistic statement, for in actual practice some authors have applied to such groups the infraspecific ranks used for non-hybrid taxa. Thus, for examples, we have *Mentha* × *villosa* subsp. *amaurophylla* (Timb.) Tacik in Pawłowski B., ed., Fl. Polska 11: 218 (1967), *Rorippa* × *armoracioides* (Tausch) Fuss var. *commelinicarpa* (Froelich ex Abromeit) Soó in *Acta Bot. Acad. Sci. Hung.* 13 (3-4): 305 (1967) and *Carduus* × *orthocephalus* Wallr. f. *mulliganii* Boivin in *Nat. Canad.* 94 (5): 646 (1967), all of which should evidently have been published as nothomorphs. What is the status of these names for which no allowance is made in the Code? I suggest that the situation is somewhat analogous to that of 'misplaced terms', such as a form divided into varieties, which under the next to last paragraph of Art. 33 are not validly published. It seems therefore that such incorrect names under hybrid species should also be regarded as not validly published.

*Proposal 266.* Add to Art. H5 the following: "A name published subordinate to a specific hybrid name but at any rank other than nothomorph is not validly published. / Example: The name *Carduus* × *orthocephalus* Wallr. forma *mulliganii* Boivin in *Nat. Canad.* 94(5): 646 (1967) is not validly published; the taxon ought to have been designated a nothomorph and not a forma".

The case of two hybrid specific names proposed simultaneously for variants resulting from the hybridisation of the same two species is dealt with above, p. 55.

The final point covers intergeneric hybrid names of the wrong form. Art. H3 states that "The 'generic name' of a bigeneric hybrid . . . is formed by combining the names of the two parent genera". What then is the status of such a name which is not so formed? The name × *Maltea* Boivin in *Nat. Canad.* 94(4): 526 (1967) was proposed for a hybrid between the genera *Puccinellia* and *Phippsia* which should have been called *Phippsinellia*, *Puccinippsia* or something similar. This is not covered by Art. 32, which states that "In order to be validly published a name of a taxon must . . . have a form which complies with Arts. 16-27" but makes no reference to names of hybrids. By analogy it would seem that names such as × *Maltea* should be regarded as not validly published, but this is not yet stated in the Code. Similar cases could well arise in names of trigeneric hybrids and of hybrids of four or more genera, and the following proposal should be inserted as the final paragraph of the Article to cover all cases.

*Proposal 267.* Add to Art. H3 the following: "Names proposed for intergeneric hybrids and not in accordance with the above statements are not validly published". An example is given above.

In conclusion I would stress again that the examples quoted in the various discussions above are mostly not rare exceptions which are never likely to recur but represent just a few of the many instances in which I or my colleagues have been confronted with apparent deficiencies in the present Code. It is hoped that the Seattle Congress will approve the proposed improvements of the Code and so for the future reduce, if not completely eliminate, the number of cases where it is not clear whether

or not a name has been validly published. I am grateful to Mrs. L. Pinner of the *Index Kewensis* staff at Kew for discussion of many of the points raised, for her sympathy over the difficulties outlined in interpreting the Code, and for her support of the above proposals.

#### Miscellaneous proposals by R. K. Brummitt (Kew)

*Proposal 268.* Add to Art. 7 the following Note: "When the type of the name of a taxon is known but insufficient information about it is available to enable it to be assigned with certainty to either or any of the subordinate taxa recognised, a *neotype*<sup>1</sup> for which the required information is available is to be arbitrarily chosen". This is an alternative to Proposal 125 of Bullock in *Taxon* 17(4): 455–456 (1968). I have amicably discussed this situation with Mr Bullock and, since we are agreed that some coverage in the Code is required but differ as to what is the best solution to the problem, he has invited me to submit an alternative proposal. The problem arises in the case of *Mimosa pudica* L. for which the type is a sterile specimen. Three varieties of the species were recognised by Brenan in 1955, but the type is so poor that it cannot be assigned with certainty to any of them. The practice adopted by Brenan, and which Bullock's proposal seeks to regularise, is that the 'typical' variety, var. *pudica*, is restricted to the type specimen itself, while the three varieties recognised are named var. *hispida*, var. *tetrandra* and var. *unijuga*. But this solution seems to me to be an unfortunate sacrifice of taxonomy to nomenclatural expediency. Faced in a Flora with three varieties of *M. pudica*, none of which is called var. *pudica*, I would immediately assume that there is at least one more variety in some other region and want to know where the typical variety occurs. Even the possibility of being able to immediately refer all sterile specimens of *M. pudica* in the herbarium to a folder labelled 'var. *pudica*' does not recommend this solution of the problem to me! I would prefer that a good specimen should be arbitrarily chosen as a 'neotype' and one of the three varieties of *M. pudica* named as var. *pudica* accordingly.

A very similar example has arisen in the case of *Butyrospermum paradoxum* (Gaertn. f.) Hepper (or unfortunately now correctly *Vitellaria paradoxa* Gaertn. f.) discussed by Hepper in *Taxon* 11(7): 227 (1962), where the type of the specific name is a seed of unknown origin which could belong to either of the two subspecies recognised. Rather than regard the seed as a third subspecies of no geographical distribution it seems desirable to make an arbitrary decision and choose a neotype to decide which subspecies shall be known as subsp. *paradoxum* (or *paradoxa*). I am myself faced with a similar situation in typification of the generic name *Reineria* Moench. Although as a generic name this is a *nomen rejiciendum* rejected in favour of the later synonym *Tephrosia*, the earliest subgeneric name available in this genus appears to be *Tephrosia* subgen. *Reineria* (Moench) Bak. However, as I have pointed out in *Taxon* 16(1): 73 (1967), although the type species of *Reineria* can be said to be *R. reflexa* Moench, the only species included in the original circumscription of the genus, there seems to be no means now of identifying which species that name actually applied to. Certainly I have no idea which of the two subgenera I would like to recognise in *Tephrosia* this species would fall into. I would like therefore to make an arbitrary decision and designate some well-known species as a 'neotype' of the name *Reineria* in order to fix its application; the alternative, according to Bullock's proposal, seems to be to recognise a third subgenus including only *Reineria reflexa* which nobody can identify.

<sup>1</sup> I recognise that use of the word 'neotype' in this sense would not be quite the same as its established use and present definition in the Code, and if anybody could invent a better term to fit the situation I would be happy to accept that.



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*Proposal 269.* In two places in the first paragraph and in three places in the third paragraph of Art. 7 Note 4 delete "or epithet". As pointed out by myself and A. O. Chater elsewhere, and by McVaugh in *Taxon* 17(4): 460 (1968), an epithet cannot have a type; only a name can have a type.

*Proposal 270.* Similarly correct the third paragraph of Art. 11 to read "... is the combination of the epithet of the earliest legitimate name in the same rank...". An epithet cannot stand alone and cannot be legitimate or illegitimate.

*Proposal 271.* Add to Art. 14 Note 4 as amended by the Proposal 121 of Bullock in *Taxon* 17(4): 454 (1968) the following sentence: "Similarly if a name of a genus has been conserved against an earlier homonym but the two genera so named are later considered to be congeneric, the earlier homonym is to be restored for the combined genus". This is in fact the opposite of Proposal 122 of Bullock (loc. cit.) with whom I have again amicably discussed the problem and agreed to differ. The present Note 4 deals in fact with what happens when a taxonomic opinion which has necessitated conservation of a name in order to retain established usage is reversed. But it is clear that when Note 4 was inserted into the Code no thought was given to the case of a conserved homonym — it is intended to apply only to conserved synonyms. Thus, according to the present unfortunate wording, as soon as for example the name *Conyza* was conserved in the sense of Lessing 1832 over the original *Conyza* of Linnaeus 1753 the latter name must immediately be restored again since "it is considered the name of a genus distinct from that of the *nomen conservandum*"! However, Mr Bullock's Proposal 121, which I strongly support, will now correct this situation. But in the case of a conserved homonym, when the taxonomic opinion requiring conservation in the first place is reversed (i.e. when one now lumps two genera designated by homonyms which had earlier been regarded as taxonomically distinct) the same should apply as in the case of other conserved names, i.e. the rejected name should be resuscitated. Thus in the example quoted, if as a result of a broadened generic concept in the Compositae *Conyza* Lessing and *Conyza* L. are regarded as congeneric after all, then *Conyza* L. should be restored for the combined genus. In some recent discussions on whether a name should be conserved from a later date with a different type in order to preserve established usage, the argument has been used against it that if this were done it would mean that somebody taking a broad generic concept and wanting to use the name in its original sense could not do so; it might happen in fact that another synonym later than the rejected homonym but earlier than the conserved homonym would have to be adopted. My proposal above would overcome this difficulty and would also avoid the situation of having an earlier homonym as a synonym of an accepted name.

*Proposal 272.* Insert as an example of Art. 40 paragraph 1 the following: "*Eryngium* sect.  $\times$  *Alpestria* Burdet & Miège in *Candollea* 23: 116 (1968) published with a statement of its parentage *Eryngium* sect. *Alpina*  $\times$  sect. *Campestris*". No example of a hybrid group at the rank of a subdivision of a genus is so far given. This recently published name seems to offer an excellent example.

*Proposal 273.* In Art. 42 item (1) insert "or reference to" after the words "the provision of". This has been discussed by Bullock in *Kew Bull.* 15(3): 391–392 (1962) and by myself in a proposal for amendment of the conservation of the name *Pterolobium* in *Taxon* 17(5): 598–600 (1968). The proposal is simply to make Art.

42 comparable with Art. 41 in which a reference to a previously published description is allowed.

*Proposal 274.* Under Recommendation 46C delete the example of "*Havetia flexilis* Spruce ex Planch. et Triana". This is a most unfortunate choice of an example since Planchon & Triana did not validly publish the name *Havetia flexilis*. The name they did validly publish was *Havetiopsis flexilis*, with *Havetia flexilis* Spruce in synonymy. No place of valid publication of the name *Havetia flexilis* is given in Index Kewensis.

*Proposal 275.* Add to Art. 73, after the examples of *Dioscorea lecardi* and *Berberis wilsonae* corrected to *D. lecardii* and *B. wilsoniae* respectively, the example of *Artemisia verlotorum* corrected to *A. verlotiorum*. It seems to be commonly overlooked that genitive plural epithets should be corrected in the same way as genitive singulars, and in fact I am unaware of any case in which this has ever been done.

*Proposal 276.* Amend the wording of the Note to Art. 63 to read "The inclusion of a type (see Art. 7) is here understood to mean the citation of a type specimen or type species or type genus, the citation of an illustration of a type specimen, or the citation of a name unless its type is at the same time excluded". The present wording is rather confusing since it specifies both "citation of a type specimen" and "citation of the type of a name". As far as specific and infraspecific names are concerned these are exactly the same thing, for a type specimen is always the type of a name and the type of a name is always a specimen. The only advantage of saying "citation of the type of a name" is that this does cover names of taxa above the rank of species, which of course must be covered in this Article. Since the Note particularly mentions "citation of a type specimen" it ought also to mention "type species" and "type genus", and if these are included then "citation of the type of a name" is quite superfluous. Alternatively one could simply delete "citation of a type specimen", but I feel that the wording proposed above is preferable since it is more explicit. I find "its type" clearer than "the type" in the last clause. The word "itself" after "name" is superfluous.

*Proposal 277.* In the first example of Art. 63 replace the part following the semicolon by "the type of the name *Chrysophyllum* L. was included in the original circumscription of *Cainito* Adans.". The present wording, that "the two genera had precisely the same circumscription", is highly misleading and most unfortunate, for it suggests that when any two taxa have the *same circumscription* the name of the second is illegitimate. This of course is certainly not always so, and is not what the Article says at all. Two varieties may have been described under the same species and have the same circumscription, perhaps "floribus albis" with no specimens cited, but this does not make the name of the second illegitimate. A new genus may have a description identical with that of an earlier-named one, but unless the type of the name of the first is included in the circumscription of the second, the name of the latter is perfectly legitimate.

I am grateful to Dr. C. V. Morton for discussion of the last two proposals and for his invitation to publish them, though they may not necessarily represent his views or the views of the Superfluous Names Committee.