Miscellaneous Nomenclatural Notes

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1. **ROBERT BROWN’S PAPER “On the Asclepiadaceae”.** Mr D. P. Rogers’s argument (Taxon, 2:181-2) concerning the date of publication of Robert Brown’s paper on the Asclepiadaceae is clearly a reply to R. de Litardière’s request (Taxon, 2:34) for guidance on the matter.

Whilst admitting the cogency of Mr Rogers’s analysis of the first volume of the Memoirs of the Wernerian Society, it is evident that he has overlooked the fact that Brown quoted his paper, with volume pagination, in his *Prodromus* published in 1810. I drew attention to this in a note in the Kew Bulletin, 1953:64-5 and concluded that for practical purposes it would be convenient to adopt the date 1809 for Brown’s paper, since this date has been used in the *Index Kewensis* and indeed in most of the relevant literature. The existence of the undated, separately paginated, reprint must not be ignored (See Art. 39, Examples).

2. **CONSERVATION OF GENERIC NAMES.** In Kew Bulletin 1953:65 I recorded my acceptance of the spelling *Huernia* for the generic name commemorating J. Heurnius, in accordance with Sprague’s arguments concerning this and other of Robert Brown’s generic names (Kew Bull. 1929:242). This name appears, however, among others proposed for conservation by E. Janchen in Appendix V B of the International Code (1952), with the “corrected” spelling *Heurnia*. I wish to record my objection to the conservation of this name, and to make the general observation that proposals for conservation should not be made by botanists who are not actively engaged in work on the taxonomy of the genera concerned. In this connection I should like to draw particular attention to the remarks on this subject by Dr Sprague and Dr Fosberg (Taxon, 1:14-15).

3. **REJECTION OF IMPLIED NEW COMBINATIONS.** A general point (also relevant to Robert Brown’s paper on the Asclepiadaceae), concerns the plea by M. B. Raizada (Taxon, 2:185-7) for the admission of implied new combinations. Article 42 may be imperfect, and H. St John (Taxon, 3:8) has shown that some of the examples quoted are bad ones, but the effect of invalidating implied new combinations is a good one. It should not be annulled. I can assure Mr Raizada that the compilers of the *Index Kewensis* are very patient indeed and Article 42 has eased their task considerably. Would Mr Raizada ask that they be obliged to read in detail to discover whether an author in mentioning, for instance, *Asclepias undulata*, really meant to read *Xyasmalobium undulatum*? (See Kew Bull. 1953:64). By comparison, the task of correcting the entries wrongly attributed to authors who did not in fact publish the names concerned is not too difficult or extensive.

It must be remembered that although the great majority of the entries in the *Index Kewensis* are correct, they cannot be accepted for nomenclatural purposes without checking against the publications cited, any more than the taxonomic opinions expressed in the original volumes and the first supplement can be accepted without question. As an example, *Index Kewenses* cites *Albizia isenbergiana* Benth. as a synonym of *A. julibrissin* (Wild.) Durazz. But Bentham did not in fact publish the combination *Albizia isenbergiana*, — he merely cited the invalidly published name *Acacia isenbergiana* Schimp. ex A. Rich. in a list of species to be referred to the genus *Albizia*. Later, he published his opinion that *Acacia isenbergiana* (i.e. *Inga isenbergiana* A. Rich.) is a synonym of *Albizia julibrissin*. (See Hook. Lond. Journ. Bot. 1:327, 1841, and 3:91, 1844).

4. **CONSERVATION OF GENERIC NAMES AND THE YEAR 1890.** Mr C. V. Morton’s proposal no. 89 (Taxon, 3:22) seems to give undue weight to an unfortunate phrase in Article 24. It would surely be better to delete the words “up to the year 1890”. Experience has shown that neither 1890 nor 1907 has any significance whatever in this matter, which is purely one of convenience.

5. **NAMES OF INFRASPECIFIC TAXA.** There can be little doubt that proposal no. 90 by H. Hara (Taxon, 3:23) will be rejected by the Paris Congress. The application of the proposal depends entirely on matters of opinion and the modification of the rules envisaged will do nothing to resolve the difficulties quoted. Who, for instance, is to decide what is a “slight or unessential”
modification? This departs from the realm of nomenclature into genetics and evolutionary theory. The proposal is completely opposed to the rules expressed in Articles 61 and 72 and the arguments brought forward do nothing to show that the consequential modifications of those and other rules would be desirable.

In the examples quoted the correct procedure would be to reject any name the application of which is shown to be indeterminable. The difficulties encountered because of the lack of an index of infraspecific names are, although serious, not the concern of the Code, and the proposal would not, as Dr Hara seems to think, ease those difficulties.

6. NAMES OF SUPRASPECIFIC TAXA. R. H. Lambert's proposal no. 86 (Taxon, 3:19) and his proposal no. 152 (Taxon, 3:64) are unnecessary and show some misunderstanding. Clapham, Tutin and Warburg (Fl. Brit. Isles) did not make the combination Alchemilla vulgaris and they certainly had no intention of making a taxon of higher rank than species when they added the abbreviation "agg." One may object to the form of presentation, but the nomenclature is sound. All that the authors in fact say is that if species 3-14 (of Alchemilla) are united into one taxon, the (aggregate) species so formed must be called A. vulgaris.

Mr Lambert's second example (Proposal no. 86) is equally irrelevant and one might ask why he has selected Gigantabies taxifolia for special consideration? All the binary names in J. Nelson (Senilis), Pinaceae (1866) are illegitimate under Articles 12-15, since he subdivides the Pinaceae (or Coniferae) in a manner which excludes (among others) the category "genus" and further makes recognition of that category impossible; in other words, he does not use the nomenclature of the Code, although he does use binary names for one of his categories of taxa.

7. ARTICLE 34. The first two paragraphs of this article are wrongly placed in the section entitled "Names of taxa below the rank of species", since they apply to a much wider range of categories, up to Order. The first paragraph reads, "For nomenclatural purposes, a species or any taxon below the rank of a species is regarded as the sum of its lower taxa, if any". The last two words are in any case meaningless, but also any taxon is, by definition, the sum of its lower taxa, for all purposes, not merely nomenclatural. This paragraph, suitably reworded, would be better placed following the last paragraph of Article 8.

The second paragraph conflicts to some extent with Articles 42-54, concerning valid publication. It reads, "The description of a subordinate taxon which does not include the nomenclatural type of the higher taxon automatically creates a second subordinated taxon of the same rank which has as its nomenclatural type the type of the higher taxon." In this paragraph there is no direct reference to which categories are to be included in the word "taxon" and in spite of the sectional heading it can and does apply to all categories up to the rank of Order. The date of publication of the name of the "type" subordinate taxon is clearly indicated as the date of publication of the first "non-typical" subordinate taxon. This is also dealt with to some extent in M. S. Doty's proposal to amend Article 32 (Taxon, 3:16).

The valid publication of the name of a taxon of any category does not require the citation of either a type or of any of the lower categories included in it. But the Code and the type method which it embodies demand (See Art. 18-21) that all such taxa must have a type and it therefore follows that the name of the type subordinate taxa are automatically created when that of the higher taxon is published.

It is admitted that the unfortunate exclusion of tautonymic specific epithets (Articles 65, 79) prevents the application of this principle when a genus is described without the citation of species; but there is no bar, so far as I can see, to regarding Diplacorchis Schltr. Sect. Diplacorchis as having been published by Schlechter in Beih. Bot. Centrabl. 38(2):127 (1921), and not by Summerhayes, who first divided Diplacorchis into sections by the description of Sect. Brachycorythoides Summerh. in Kew Bull. 1931:579 (1931) and separated it from the "type" section.

8. ARTICLE 35. In the same way as in para. 7 above, the application of the provisions of this article cover a much wider range than is indicated in the article, but the particular point I wish to make is in connection with the last sentence, "This epithet can no longer be used when that of the next higher taxon is changed". If the change is on taxonomic grounds only then the epithet must stand under the normal rules governing legitimacy.
It is in such instances that the date of publication of "type" subordinate taxa becomes significant and the argument of para. 7 above applies.

I cannot support de Litardière's argument (Taxon, 3: 41) for the suppression of this article. The examples cited by him involve the classification of the taxa concerned, but their nomenclature does not, so far as I can see, involve any serious difficulty. Any author who uses a name such as Festuca rubra L. subsp. rubra var. rubra subvar. rubra (or the corresponding ternary name indicated in Article 34) is saying, in effect, "I have identified the subvarietal taxon which includes the type of Festuca rubra L.". If such typification is impossible, or the author is in error, the subvarietal name amounts to a misidentification. But this is not a fault of the Code.

9. THE CAPE BELLADONNA LILY. Dr Dyer's excellent review of the opinions expressed on this topic (Taxon, 3: 72-74) is a timely plea for reasonable action. My colleague Mr J. R. Sealy was, perhaps, in a better position geographically than his somewhat vituperative American critics to assess the available evidence and this fact alone, — apart from his obviously careful and unbiased presentation, — should have given them cause to reconsider their earlier opinions. I am convinced that Mr Sealy's conclusions were correct.

The special case, as is usual, calls to mind others in which differences of nomenclatural opinion have led to confusion, and this is nowhere more apparent than in Mesembryanthemum L. for which no fewer than three species, each representing a distinct genus, have been proposed as lectotype and each has gained a number of adherents. A review of this controversy was given by Rothmaler in Notizbl. Bot. Cart. Berlin, 15: 410-414: 1941, who concluded that N. E. Brown should be supported.

The citation of type species in the list of nomina conservanda gives a means of settling such questions by international agreement. At present the list is limited to the conservation of generic names against earlier synonyms or homonyms; it could be extended to the conservation of names in a particular sense.

10. NEW ARTICLE 70bis. Mr L. Benson's proposal no. 142 (Taxon, 3: 60) is a further illustration of confusion between nomenclature and taxonomy. The facts of inconsistency and apparent carelessness of authors in their use of the categories "variety" and "subspecies" do not constitute reasons for continuing or encouraging such inconsistencies and carelessness. Most modern taxonomists have a fairly clear idea of their own interpretation of the limits of the various categories of taxa and there is a most remarkable degree of unanimity amongst them. The introduction of the idea that it does not matter whether an author calls a taxon "variety" or "subspecies" or indeed any other infraspecific category — particularly into a Code which aims at precision — would be definitely retrograde.

11. RECOMMENDATION 82G. The arguments for and against decapitalization of specific epithets have been repeated ad nauseam. But taxonomic nomenclature is a means to an end, not an end in itself or a field to be given up to classicists, historians and others who wish to display their erudition to the discomfiture of users of plant names.

For my part, I am very pleased to be relieved of the extra burden of being obliged to check the origin of doubtful epithets to decide whether I should write them with a capital or a lower case letter and would welcome the incorporation of decapitalization of specific and infraspecific epithets into Article 82 with the force of a rule. The arguments supporting Proposal no. 154 (Taxon, 3: 64) by Gleason and Rogers seem to be merely facetious. I have never heard any suggestion of decapitalizing unitary names; by definition they are substantives. Specific epithets, however, even if they happen to be substantives, are in effect adjectival and the absence of an initial capital letter tends to emphasize this fact (See also L. Benson, Proposal no. 2, in Taxon, 1: 113).

12. ARTICLES 31 AND 32. H. St John has made useful and necessary proposals for the amendment of these articles in Taxon, 3: 7, but further amendment seems desirable. Article 31, para. 3 suggests that the epithets of section and lower infrageneric categories should be plural adjectives, but Article 32 as amended by St John insists that all such epithets must be substantives, at least for that taxon which includes the type. These two dicta taken together are at variance with Recommendation 32A which points out that
a mixture of substantives and adjectives for infrageneric epithets in the same rank should be avoided. This is perhaps a point which the editorial committee might bear in mind.

13. ARTICLE 39. The amendment proposed by H. St John to define more accurately the field of literature in which valid publication of new names may take place will find considerable favour with the compilers of the Index Kewensis. It should, I think, be the business of the Congress to consider whether it is practicable to restrict valid publication to journals devoted to taxonomic botany, on a national or even an international scale. St John’s amendment is an improvement on the article as at present worded, but in answer to his question — I would exclude the publications he mentions, together with ecological and agricultural journals, the journals published by the numerous specialist horticultural societies and the vast majority of local natural history society journals. All of these are to a greater or less extent scientific journals, some of them exclusively so; but their business is not primarily taxonomic botany.

This matter is also raised by L. C. Wheeler in his proposal no. 140 (Taxon, 3: 58). He points out the difficulty of defining tradesmen’s catalogues and newspapers, but then makes the startling assertion, — "Restriction of avenues of publication is the first step toward the creation of more Rafinesques and Marcus E. Joneses." This seems to be a non sequitur, as indeed also is his next paragraph which states that the necessity for providing a Latin diagnosis is a sound preventive. No Latin diagnosis is required for new names which can be referred to previously published descriptions. In this connection I would like to mention that the editor of the Journal of Ecology does not permit publication of new names in his journal, nor do the editors of the Flora of Tropical East Africa and the new edition of the Flora of West Tropical Africa.

14. ARTICLE 75. I am in full agreement with H. St John’s vigorous attack on this article in Taxon, 3: 10. The provision for the rejection of nomina confusa is being taken care of by a modification of the proposal for nomina rejicienda given in Taxon 1: 78-80 put forward by some members of the Kew staff to the Geneva Symposium (Jan. 25-30, 1954). The necessity for this provision is underlined by St John’s phrase, “The name of any taxon that is capable of typification….”; there are so very many names that are doubtful or incapable of typification.


FUNGAL SYMBIOTIC LOT LICHENS

by

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In Art. 76 of the I.C.B.N. it is stated: “For nomenclatural purposes names given to lichens shall be considered as applying to their fungal components — —.”

This does not mean that “lichens are to be considered only with regard to the fungus, leaving the lichenic complex out of the picture (even from a biological point of view), nor that “a lichen is considered as a fungal parasite of an alga” (Ciferri & Tomaselli, loc. cit.).

Following the scheme suggested by Thomas (1939), Ciferri & Tomaselli propose the establishment of two different classifications and two nomenclatural systems, one for the lichens and one for the fungal components of the lichens.

This means an addition of about 20,000 new names to botanical nomenclature. Is this drastic step really necessary and can it be scientifically defended?

As generally admitted, also by Ciferri & Tomaselli, lichens are composite organisms consisting of fungi and algae. It has been clearly proved that neither the lichen-fungi nor the lichen-algae form inter se coherent taxonomic groups, but that they are related (or belong) to different groups of non-lichenized fungi and algae. Instead, lichens form a biological group. This group is not sharply circumscribed, and in many cases it...