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COMMENTARY 3:
Two Codes in a Dual System? No Thanks.

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Abstract

With respect to the problems caused by strict application of the principle of priority, I think a solution can be found by formulating a rule to suppress certain names. I agree that mycologists especially have severe problems with illegitimacy and typification. Nevertheless from discussion of those topics, only one conclusion can be drawn: We do not need harmonization or unification, rather we require a further diversification, with the *International Code of Botanical Nomenclature* providing more nomenclatural flexibility in mycology and likely in algology.

None of the problems can be solved by a *Code of Biological Nomenclature*, since the problems relate to names published before the year 2000 and such names are not covered by the *BioCode*. Harmonization does not offer any solution for problems that some of us have to deal with. Moreover I reject the idea as a goal in itself. Why make things unnecessarily complicated?

INTRODUCTION

The idea of an ***International Code of Biological Nomenclature*** or ***BioCode*** has taken shape, and we now have a draft version published in ***Taxon*** (Greuter et al., 1996). In this commentary, I present arguments against its necessity and provide some alternatives. My main guideline will be Hawksworth's (1992) plea for increased harmony of the codes. The title of his article is a more emphatic version of the title of the Mini-Symposium: "The need for a more effective biological nomenclature for the 21st century". In the Abstracts, we have a new approach: To have a nomenclature that takes into account the results of phylogenetic research. This point is discussed in the Epilogue.

Hawksworth began illustrating the need for change by reporting the results of an inquiry among 80 plant taxonomists in the United Kingdom (60 persons answering), asking them to tell (anonymously) how they spend their research time. Having thus demonstrated the need for more effective nomenclature, he discussed six problem fields: Priority, effective and valid publication, illegitimacy, types, ambiregnal organisms, and decision-making bodies. He mentioned four procedures towards a more effective nomenclature: Lists of protected names, registration of names, lists of standard names, and harmonization of the existing codes of nomenclature.

Hawksworth asked the taxonomists to indicate the proportion of the research time they devoted to nomenclatural as opposed to taxonomic studies. It appeared that about half spent 10-75 % of their research time on nomenclatural matters. Extrapolating this to the U.K. as a whole, Hawksworth calculated that this is the equivalent of 52 full-time posts. This waste of time should stop.

Before addressing Hawksworth's other remarks, allow me to comment on his finding about the time spent by a taxonomist on nomenclatural issues:

1) If so much time appears to be necessary for nomenclatural elucidation of about 60 small sets of taxa, then how could one imagine that reliable lists of Names in Current Use (NCU, one of the proposed solutions) could be produced within a reasonable period of time? If one wishes to do more than merely copy existing indices and floras, it is necessary to do a thorough search of the existing literature. Such searches can be never-ending so that the value of NCU lists is that once they are approved, searches for additional or earlier names are unnecessary. For further comments on NCU lists, see paragraph A below.

2)Disturbing is Hawksworth's comment at the end of the time-spending paragraph. When a modern revision has been published, the names in synonymy "remain validly published to compete with other names. Conscientious subsequent monographers consequently feel obliged to re-work and check much of this `morass', not infrequently finding reasons to amend the decisions of their predecessors. Is this once again a cost-effective use of a taxonomist's time?" What does Hawksworth mean? If a later monographer decides to split a species, should he propose a new name instead of adopting an older name (that has been in use in the past, e.g. in plant sociological literature)?

I. PRIORITY

Under the ***International Code for Botanical Nomenclature*** (ICBN), `priority of publication' is one of the main principles (Greuter et al., 1994: 3: Principle III). There is a problem indeed, and the Reveal (1991) example referred to by Hawksworth is a good one. One is also reminded of the articles by Mabberley (e.g., 1980) in which he demonstrated that many names, traditionally ascribed to author X on date Y had already been published by another author at an earlier date (and often in an `obscure' journal that seldom if ever was consulted by botanists).

Even more serious is the replacement of well-known names by names that never have been used. An example: Burtt & Smith (1986) replaced ***Nicolaia*** Horan. (an 1862 name conserved since 1987, the proposal was by Burtt, 1981) by ***Etlingera*** (which is a rejected name against ***Amomum***, of which it had so far been considered as a taxonomic synonym). In this case, name changes are necessary for taxonomic reasons. It would have been preferable if this could have been done under ***Nicolaia***, with the addition of 44 new combinations. There is, however, an older name that has priority. Now for 58 species, new combinations have been made, mainly by Smith (1986), under the completely unknown 1792 name of ***Etlingera*** Giseke (Burtt & Smith, 1986: 235: "The name has not been taken up hitherto"). One of them is ***E. elatior***, the name for an important ornamental species, until then known as ***N. elatior***. (A better solution under the present Code would have been to put ***Etlingera*** at another place on the list of conserved and rejected names, specifically against ***Nicolaia***.)

There is another category of nomenclatural acts that occasionally poses problems: The picking up of lectotypifications that have been neglected until now (see Margadant & Geissler, 1995: 613).

The proposed remedy for those problems is the production of lists of "Names in Current Use" (NCU). Some lists have been published already: e.g., Hoogland et al. (1993) for family names, Greuter et al. (1993) for generic names, and Pitt et al. (1993) for species names in four families.

I believe that another kind of solution is preferable, comparable to the one found in the ***International Code of Zoological Nomenclature*** (ICZN): Suppression of an older synonym on the grounds that it has not been used as a valid name in the preceding 50 years, whereas the younger synonym has been applied to the taxon by at least five different authors and in at least 10 publications during the same period (Ride et al., 1985: Art. 79(c)). It must be possible to make some comparable rules for the ICBN, working within its different organizational framework. An analogical rule also could be made to suppress overlooked earlier lectotypifications. Such a rule under the ICBN might be less rigid. Nonetheless, it would be a step toward the suppression of names (or authorships or lectotypifications) which would allow the retention of established bibliographic information regardless of priority.

II. EFFECTIVE AND VALID PUBLICATION

Problems addressed in this field: a) doubts on validity, b) the danger of publication of names in journals that have not had a normal peer review, c) determination of the date of publication, and d) the ICBN requirement that (for non-fossil plants) Latin should be used for diagnoses. Only the last mentioned point touches the BioCode, although it is imaginable that even in the ICBN the requirement of Latin would be restricted to certain groups of organisms, and with the addition of a date after which it is no longer necessary. For the rest, Hawksworth considers registration as a remedy (see below, paragraph B).

III. ILLEGITIMACY

This is a concept of the ICBN present from its beginning (even though modified over the years). Hawksworth states that "cases of apparent illegitimacy in cryptogamic groups particularly repeatedly come to light". I have the impression that this might be true for algae and fungi (lichens inclusive). Hawksworth's presentation of the problem, however, is too simple. Using an example from Acharius, Hawksworth mentions two possibilities: Either that Acharius proposed a new name because he had lost respect for Linnaeus, or that he was uncertain whether the lichen before him was really the same as that of Linnaeus. Under the actual example, the second possibility can be cancelled: in an "Obs.", Acharius explicitly stated that he had investigated a specimen, received from Linnaeus himself. Moreover, the second possibility is often indicated by the addition of a question mark or a comment expressing doubt (and in such cases I do not say the "synonym" makes illegitimate the new combination). There are, however, more possibilities:

a) The author who makes the new (illegitimate) combination chooses a new epithet because he considers the previous one as inappropriate or disagreeable, or because another one is preferable or better known, or because the original epithet has lost its original meaning, or (in pleomorphic fungi) because the generic name does not accord with the morph represented by its type. This enumeration is from Art. 51 of the ICBN, in which rejection of epithets for any of these reasons is forbidden. The ***BioCode*** does not have such a comparable rule, not surprizingly, because it does not recognize illegitimacy.

b) The author chooses a new name or new epithet because he is splitting up a taxon and he wishes two new names for what formerly was united, instead of retaining one name for what nowadays is called the type. Again a procedure that is rejected by the ICBN, even though since `Tokyo' it is no longer explicitly stated. Before `Tokyo', Art. 52 and 53 covered these categories of cases. In `Tokyo', the Editorial Committee was empowered to remodel Chapter V, putting a few fragments in Art. 11. Those clear articles were considered to be superfluous and deleted - I now understand, why. Of course the ***BioCode*** does not have those provisions.

c) The author may have chosen the oldest epithet, even though it was invalid (for zoologists: not available), e.g. a nomen nudum, or a herbarium name, or a manuscript name.

d) For generic names, there often is still another possibility: The author (not knowing what later on would be fixed as the starting-point of nomenclature) adopted a pre-Linnaean name.

Thus there is more than one situation that might have led an author to publish a superfluous name. The illegitimacy concept is also in the bacteriological, virological

and horticultural codes, but only the ICZN fails to include it. Why should such an important concept be disposed of in the ***BioCode***?

I started this paragraph with admitting that the illegitimacy concept presents problems for the nomenclature of certain groups of plants. The ***BioCode*** does not solve problems that exists with pre-2000 literature, so mycologists (and algologists?) will likely continue trying to amend the ICBN so as to enter additional exceptions for them. Names of spermatophytes and bryophytes which have been recognized as illegitimate were replaced long ago.

IV. TYPES

Hawksworth mentions several serious problems, some of which mainly occur in research of micro-organisms. Above all else, the reinterpretation of existing type material with the use of new characters or techniques is difficult if the old specimens do not show the features necessary to make an identification. For such cases (only in mycology and perhaps also in algology), I suggest that the adoption of additional type material for taxa (even beyond that of epitype [Art. 9.7]) be legalized under the ICBN. Moreover, the rule that only dead specimens can constitute types is outdated in a period wherein living, but metabolically inactive, material of fungi can also serve as types.

When reading this paragraph, I realized that it certainly is not by accident that a mycologist is one of the great advocates of a new ***BioCode***. Their problems are still not solved by this new ***BioCode*** and I urge them to try to change the application of the ICBN in these fields by making exceptions for mycology (and algology) as might be appropriate.

V. AMBIREGNAL ORGANISMS

This term is applied to all organisms that have or could be treated under more than one of the current codes. The theme is thoroughly discussed by Hawksworth et al., (1994), and I can agree with most of their conclusions. By agreement it could be fixed which code (botanical or zoological) should be applied for names of ***Cyanophyceae/Cyanobacteria***, ***Myxomycota/Mycetozoa***, ***Dinophyceae/Dinoflagellates***, etc. with specific modifications added to that code to accommodate those taxa named under the other code in the past.

For protists it could be decided that the oldest legitimate name (ICBN term or potentially valid name, the ICZN term) is the correct name for the taxon.

There are inter-code homonyms. There could be a rule in all codes that in the future the publication of such homonyms are inadmissible. With respect to existing names, botanists could consider amendation of Art. 56, to enable the formal rejection of names from that category. If a certain name is in use in zoology, whereas it is an

unquestionable synonym in botany, the botanical community could render a service to the zoologists by rejecting the plant name.

I agree with Hawksworth et al. (1994): All problems in this field can be solved without a ***BioCode***.

VI. DECISION-MAKING BODIES

The ICBN differs fundamentally from the other codes in the democratic way it is adjusted every six years. I agree that something should be done to reduce the ever-growing flow of proposals to amend this ***Code***. Maybe the threshold for acceptance of a proposal should be increased? Would it be useful to rule that no change of the ***Code*** takes effect immediately, only the next Congress could decide that a proposed new rule will be definitive? (For some of the new ex/in provisions, I would have appreciated such a policy.)

A. NCU Lists

Greuter (1996) admits that NCU lists "are essential for the long-term usefulness of the new concept" (of the ***BioCode***). I feel ambivalent towards NCU lists. Working on ***Index Nominum Genericorum (Plantarum)*** (ING) (Farr et al. 1979, 1986; Farr & Zijlstra, 1996-onward), I know how unreliable ING (which was used as the basis for the NCU generic names list) is, not in the last instance because of changes to the ICBN since the preparation of many records.

Some examples: a) new rules on the use of `ex' and `in' (`Tokyo', 1993), b) the change of what constitutes the type of a generic name (it should be a specimen, instead of a species; `Sydney', 1981), c) a provision for generic names of fossil plants which was deleted from Art. 42 (`Leningrad', 1975), etc. Farr & Zijlstra (1991) list several more problems encountered with the types as listed in ING. Even though improvements have been made, as for example by the addition of missing lectotypes, many cases requiring correction have not yet been noticed.

Despite these problems, I am not against the NCU principle, provided that a sound and scholarly basis for preparation of the lists is created. The most important criterion should be a restriction in time: To start with, one could rule that only names published before 1851 are included. This would be a big step forward, since most of the excavated names that cause problems, are older. Later on, one could add names from later periods.

B. Registration of Names

This principle, intended for names published on or after 1 January 2000, was accepted in `Tokyo', subject to the approval of the next International Botanical Congress (1999).

C. Lists of Standard Names

This concept endeavours to fix taxonomy as well as nomenclature. I suppose it is the wish to create this category of names that induced people to say that the NCU lists prepared a few years ago should cover the period up to and including 1990.

D. Harmonization of the Codes

According to Hawksworth (1992), harmonization could serve two purposes: a) removing unnecessary differences in terminology and conventions, and b) adopting common strategies to confront various problems, either new or old.

If people already have problems using nomenclatural terms correctly (see Greuter (1996: 292) on misuse of the term `valid') would not that problem be magnified with two codes being used simultaneously? For example, are not the terms `established', `acceptable' and `accepted' (Greuter et al. 1996: 350) also candidates for wrong application?

I am one of the world's few full-time nomenclaturalists. I have some experience with applying two codes. When adding names of fossil algae to ING's database that originally were described as animals, I must consult the ICZN (those names are valid only (Art. 45.5) if they are available under the ICZN). If one would force plant taxonomists, who devote most of their time to taxonomy (and thus are less familiar with nomenclature) to use two codes from 2000 on, I assert that the time they spend on their nomenclature would increase. I am afraid that endless problems would arise. For instance, which term should they use for a 2005 publication in which no new names or combinations are published? Could one still talk of `valid publication', of `correct names', etc.? In my view, "no thanks" - we do not need this problem.

In section III-V above, it was argued that harmonization supposedly reached by the establishment of a ***BioCode*** that only applies to names published from 2000 on does not offer a solution. For botanists (in a broad sense, including also mycologists) a solution can be sought only in amendation of the ICBN.

EPILOGUE

Kevin de Queiroz (1996) argues that a phylogenetic approach in taxonomy requires an entirely different kind of classification, as an alternative to the Linnean hierarchical categories that we are familiar with. When I try to imagine this, I think this must require a flexible system able to incorporate newer ideas on ancestry and descent as soon as the data are available. It will be a challenge to produce a nomenclatural system that meets the requirements of such an alternative approach. Such a new system should grow under the auspices of the researchers in this field, as it cannot be realized by the imposition of a set of rules. And as long as many organisms are not yet covered by any new system proposed by de Queiroz, their nomenclature should be regulated by the present codes, updated where necessary.

References:

1. Burtt, B.L. 1981. Proposal to conserve *Nicolaia* Horan. (1862) against *Diracodes* Bl. (1827). *Taxon* 30: 361.
2. Burtt, B.L. & R.M. Smith. 1986. *Etlingera*, the inclusive name for *Achasma*, *Geanthus* and *Nicolaia* (*Zingiberaceae*). *Notes Roy. Bot. Gard. Edinburgh* 43: 235-241.
3. Farr, E.R, J.A. Leussink, & F.A. Stafleu (eds.). 1979. Index nominum genericorum (plantarum). *Regnum Veg.* 100-102.
4. Farr, E.R., J.A. Leussink & G. Zijlstra (eds.) 1986. Index nominum genericorum (plantarum) supplementum 1. *Regnum Veg.* 113.
5. Farr, E. & G. Zijlstra. 1991. The need to cite types. *Regnum Veg.* 123: 225-229.
6. Farr, E. &. G. Zijlstra. 1996 and onward. *Index nominum genericorum (plantarum).* Smithsonian Institution: <"http://www.nmnh.si.edu/ing/">
7. Greuter, W. 1996. On a new *BioCode*, harmony, and expediency. *Taxon* 45: 291-294.
8. Greuter, W., R.K. Brummitt, E. Farr, N. Kilian, P.M. Kirk & P.C. Silva (compls. & eds.). 1993. NCU - 3. Names in current use for extant plant genera. *Regnum Veg.* 129.
9. Greuter, W., F.R. Barrie, H.M. Burdet, W.G. Chaloner, V. Demoulin, D.L. Hawksworth, P.M. Jørgensen, D.H. Nicolson, P.C. Silva, P. Trehane & J. McNeill (eds.). 1994. International code of botanical nomenclature. *Regnum Veg.* 131.
10. Greuter, W., D.L. Hawksworth, J. McNeill, M.A. Mayo, A. Minelli, P.H.A. Sneath, B.J. Tindall, P. Trehane & P. Tubbs (eds.). 1996. Draft *BioCode*: The prospective international rules for the scientific names of organisms. *Taxon* 45: 349-372.
11. Hawksworth, D. L. 1992. The need for a more effective biological nomenclature for the 21st century. *Bot. J. Linn. Soc.* 109: 543-567.
12. Hawksworth, D.L, J. McNeill, P.H.A. Sneath, R.P. Trehane & P.K. Tubbs. 1994. Towards a harmonized bionomenclature for life on earth. *Biol. Int., Special Issue* 30: 1-44.
13. Hoogland, R.D., J.L. Reveal, M.J. Crosby, R. Grolle, G. Zijlstra & J.C. David. 1993. NCU-1. Family names in current use for vascular plants, bryophytes, and fungi. *Regnum Veg.* 126.
14. Mabberley, D. 1980. Generic names published in Salisbury's reviews of Robert Brown's works. *Taxon*29: 597-606.
15. Margadant, W.D. & P. Geissler. 1995. Seventeen proposals concerning nomina conservanda for generic names of *Musci*. *Taxon* 44: 613-624.
16. Pitt, J.I., R.A. Samson, T. Ahti, A. Farjon & E. Landolt. 1993. NCU-2. Names in current use in the families *Trichocomaceae*, *Cladoniaceae*, *Pinaceae* and *Lemnaceae*. *Regnum Veg.* 128.
17. Reveal, J. L. 1991. Two previously unnoticed sources of generic names published by John Hill in 1753 and 1754-1755. *Bull. Mus. Natl. Hist. Nat., B, Adansonia* 13: 197-239.
18. Ride, W.D.L., C.W. Sabrosky, G. Bernardi, & R.V. Melville (eds.). 1985. International code of zoological nomenclature. Ed. 3. London.
19. Smith, R.M. 1986. New combinations in *Etlingera* Giseke (*Zingiberaceae*). *Notes Roy. Bot. Gard. Edinburgh* 43: 243-254.