

## PROPOSALS TO AMEND THE CODE

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(175–176) Proposals to modify the provisions in the *Code* for naming fossil plantsRichard M. Bateman<sup>1,2</sup> & Jason Hilton<sup>2</sup><sup>1</sup> Jodrell Laboratory, Royal Botanic Gardens Kew, Richmond, Surrey, TW9 3DS, U.K.<sup>2</sup> School of Geography Earth and Environmental Sciences, University of Birmingham, Edgbaston, Birmingham, B15 2TT, U.K.

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The nomenclatural treatment of fossil plants (and thus of plant fossils) has been subject to two recent reviews in the pages of this journal (Bateman & Hilton in *Taxon* 58: 1254–1280. 2009; Cleal & Thomas in *Taxon* 59: 261–268, 312–313. 2010). In the more recent of these two contributions, Cleal & Thomas (l.c.) made explicit a distinction between plant fossils—fragmentary remains of extinct plants—and fossil plants—the individual extinct plant as it appeared in life or, more accurately, as it is now perceived to have appeared in life as a result of painstaking conceptual reconstruction work undertaken by generations of palaeobotanists.

Cleal & Thomas then proceeded to critique the two main modifications to the nomenclatural treatment of fossil plants made in the two most recent versions of the *Code*: the introduction of ‘morphotaxa’ into the *St Louis Code* (Greuter & al. in *Regnum Veg.* 138. 2000) and of ‘fossil taxa’ into the *Vienna Code* (McNeill & al. in *Regnum Veg.* 146. 2006). Whereas ‘fossil taxon’ is defined as “any taxon whose type is a fossil” (footnote to Preamble 7), ‘morphotaxon’ is a category within the broader category of fossil taxon “that can only include fossils of the same plant-part, life-history stage or preservational state as represented by the type” (Art. 1.2). Given this definition, any taxon for which greater knowledge subsequently becomes available (e.g., two formally named “plant-parts” are shown to be conjoined) is no longer a morphotaxon. However, the *Vienna Code* does not stipulate the category complementary to ‘morphotaxon’ that is needed to accommodate such an expanded taxonomic circumscription encompassing the two newly correlated “plant-parts”.

In a set of three explicit proposals to amend the *Code*, Cleal & Thomas (l.c.) argued for a more explicit use of the terms ‘fossil plant’ versus ‘plant fossil’, focusing on the latter, and for the hyphenation of the term ‘fossil’ with the intended taxonomic rank (e.g., fossil-species, fossil-genus). They also advocated elimination of the term and concept of ‘morphotaxon’. However, they did not argue for any replacement for this term or concept, presumably believing that the undeniably tortured 75-year history of special provision for plant fossils in the *Code* militated against further ‘judicial’ experimentation. Although we concur with most of the arguments made by Cleal & Thomas (l.c.) against the existing provision for plant fossils in the *Code*, and therefore support their three formal recommendations, we believe that the resulting conceptual vacuum would prove detrimental to palaeobotanical taxonomy.

Our own recent review of palaeobotanical taxonomy (Bateman & Hilton, l.c.) presented detailed arguments that it is primarily the **disarticulation** of previously intact fossil plants into plant fossils that were typically single organs (e.g., flower, seed, pollen—each approximately equivalent to the much-criticised ‘morphospecies’) that necessitates special provision in the *Code* for fossil plants. This problem is of course reinforced by additional constraints, notably the recalcitrance of plant fossils to investigation by DNA sequencing or

direct observation of autecology. Using lessons learned from developing explicit protocols for reconstructing disarticulated organs into conceptual whole-plants, we advanced three critical definitions that took concepts rooted in the valuable ‘Palaeobotanical Appendix’ that last appeared in the *Paris Code* (Lanjouw & al. in *Regnum Veg.* 8. 1956) and reconfigured them in the modern framework of phylogenetic systematics. Specifically:

“**Organ-species:** Any morphologically delimited category of disarticulated organ.

**Form-species (shared organ-species):** Morphologically delimited category of disarticulated organ that, within the context of a particular comparative study, is perceived to lack unique character states (and thus is assignable to multiple whole-plant species).

**Autapo-species (unshared organ-species):** Morphologically delimited category of disarticulated organ that, within the context of a particular comparative study, is perceived to possess at least one unique character state (and thus is assignable to a single whole-plant species).

**Template:** All of the categories of organ-species that are necessary to complete the conceptual re-assembly of a whole-plant species.

**Whole-plant species:** Morphologically delimited conceptual aggregate of all of the organ-species required to complete the relevant template, including at least one autapo-species. This concept contrasts with an **intact plant**, which has never been disarticulated but nonetheless does not necessarily possess every organ in the relevant template (e.g., because it was not in reproductive mode at the time of preservation)” (Bateman & Hilton, l.c.: 1256–1257).

We recognise that taphonomic constraints mean that most attempts to reconstruct a conceptual whole-plant actually result in only a partial reconstruction, consisting of some but not all of the requisite organ-species.

These five key definitions are born of our conviction that previous discussions of palaeobotanical taxonomy (including those of Cleal & Thomas, l.c.) have focused on the relationship between the Linnaean binomial and the underlying type(s) but have largely ignored the third essential element of a protologue, specifically the character-based formal description that dictates diagnosability. Our carefully defined categories of fossils to which binomials can be assigned recognise the disarticulated organ (organ-species) as the basic unit of palaeobotanical taxonomy, and distinguish between organs that appear to bear at least one character state unique to the source whole-plant (autapo-species) from those that demonstrably do not (form-species). These distinctions are not dictated by the mode of fossil preservation per se (contra Cleal & Thomas, l.c.) but rather by the kind and number of taxonomic characters presented by the fossil. We also explained how the limited pool of character states inherent in morphological taxonomy inevitably identifies the majority of organ-species as being form-species.

Lastly, we acknowledged that our terms ‘organ-species’ and ‘form-species’ were rooted in the traditional, previously *Code*-specified terms ‘organ-genus’ and ‘form-genus’, noting that past decisions to elevate from the species to the genus level the problems posed by fossil disarticulation merely transfer already difficult decisions to a level where, by definition, even fewer diagnostic characters are available to the taxonomist. We also note that the concept of form-taxa was described as “quite indispensable” in the *Paris Code* (Lanjouw & al., l.c.: 56), and both form-taxa and organ-taxa were deemed “vitaly important to palaeobotany” by Faegri (in *Taxon* 12: 20–28. 1963).

The main difference between our position and that adopted by Cleal & Thomas (l.c.: 264) is epitomised by their statements that the motivation for defining ‘morphotaxa’ in the *St Louis Code* as ‘those parts, life-history stages, or preservation states of organisms that are represented by the corresponding types’ “may have originated from views expressed earlier [in the] ... preamble to the *Draft BioCode* (Greuter & Nicolson, 1996: 348): Palaeobotanists may find it desirable to prevent the use of names of organ taxa ... for different fossil organs, once the link is established. We doubt that palaeobotanists would want to take up, say, a name typified by pollen organs for a fossil leaf or wood sample when both pertain to the same organism.” The authority on which this perceived opinion of palaeobotanists was based was not given. The quoted example is, of course, an extreme situation, and it is difficult to imagine any scenario where palaeobotanists would want to unite taxa of pollen and leaves in this way. However, if the wording of the example is changed slightly, the situation becomes less clear-cut: “We doubt that palaeobotanists would want to take up, say, a name typified by stems for a fossil leaf when both pertain to the same organism. Surely, if the evidence pointed beyond reasonable doubt to all stems of a particular form having borne leaves of a particular form, *and that those leaves were only found on that form of stem* [our italics], what would be the point of having separate names?” And later (p. 265): “If we accept *Lyginopteris* as a morphotaxon simply because its type is an anatomically preserved [pteridosperm] stem, then we need a separate generic name for the foliage, assuming that we did not merely subsume it into the highly ‘artificial’ and systematically meaningless *Sphenopteris*” (a famously problematic genus of fern-like foliage).

In the Bateman & Hilton (l.c.) system, even a conclusive demonstration that stem and leaf were borne by the same individual plant could not be taken as evidence that they were borne by that whole-plant species **alone**. Either the stem or the leaf or, more likely, both categories of organ would probably be indistinguishable when found in more than one whole-plant species and so would be categorised as form-species. In our perception, within the category of organ-taxa, form-taxa will predominate over autapo-taxa; form-taxa are not the “small subset” of organ-taxa envisaged by Cleal & Thomas (l.c.) and many previous authors. Moreover, the morphologies circumscribed by the stem-species and leaf-species postulated by Cleal & Thomas can be demonstrated to overlap but they cannot be demonstrated to precisely coincide; there will always be a serious risk that one organ-species will be more taxonomically inclusive than the other. Thus, both binomials should be allowed to survive in parallel, while noting their observed or inferred relationship as a more substantial aggregate component of a conceptual whole-plant species. This statement certainly applies to the relationship between the stem-species *Lyginopteris* and the frond-species *Sphenopteris*—or rather, more precisely, the one of the many less ‘artificial’ frond-species segregated from within the traditional ‘*Sphenopteris*’ by several recent authors.

Hence, to us, the concepts of organ-species and form-species, and thereby the terms that we routinely use as short-hand for those concepts, are essential elements of palaeobotanical taxonomy. Unfortunately, in our experience, editors of many scientific journals have used the stepwise deletion of these terms and concepts from the *Code* (detailed, albeit with contrasting emphases, by Chaloner in Jones & Rowe, *Foss. Pl. Spores*: 179–183. 1999; Bateman & Hilton, l.c.; Cleal & Thomas, l.c.) as an excuse to prevent usage of these or similar phrases in subsequent taxonomic treatments. Many authors, including Cleal & Thomas (l.c.), have argued that it is important to divorce nomenclatural from taxonomic decision-making, yet a (in our opinion highly counter-productive) series of decisions taken by Nomenclature Sections of successive International Botanical Congresses have made it far more difficult for us to pursue palaeobotanical systematics in the way that we would wish. We do share with Cleal & Thomas a desire to avoid using the prescriptions of the *Code* to constrain other palaeobotanical systematists into operating within what we regard as an optimal conceptual framework. But we ask that the forthcoming *Code* from the 2011 (Melbourne) Botanical Congress should expressly permit us to employ this now carefully explained system of overlapping binomials, which differs from all previous systems in being both internally consistent, symmetrical and comprehensive.

Thus, we seek to fill the conceptual vacuum that would be left by what we hope will be Cleal & Thomas’s (l.c.) successful modifications of palaeobotanical aspects of the *Code* (101–103) not with new prescriptions but rather by the addition of a permissive but not obligatory article, plus an associated footnote and example. Our proposals are intended to supplement, rather than replace, those put forward by Cleal & Thomas (l.c.):

**(175) Insert a new Art. 1.3 and associated note to read:**

“1.3. As in the case of form-taxa for asexual forms (anamorphs) of certain pleomorphic fungi (Art. 59), the provisions of this *Code* authorise the publication and simultaneous use of names for fossil-taxa, irrespective of the extent of disarticulation into component organs, and hence of whether the fossil-taxa are perceived as ‘organ-taxa’, ‘form-taxa’, ‘autapo-taxa’, or conceptual whole-plant taxa.<sup>1</sup>

<sup>1</sup> Because most fossil plants have been disarticulated into their component organs, the basic unit of palaeobotanical taxonomy is of necessity a single morphologically circumscribed organ, termed an ‘organ-species’. Where two or more partially or completely reconstructed conceptual whole-plant species based on fossil evidence are shown to contain indistinguishable copies of the same category of organ, that shared organ can be described as a ‘form-species’. Where an organ-species exhibits at least one character-state that also appears to be diagnostic of a partially or completely reconstructed conceptual whole-plant species, this organ-species can be described as an ‘autapo-species’. These three prefixes (organ-, form-, autapo-) can also be employed at any higher taxonomic rank.”

**(176) Insert immediately after the new Art. 1.3 the following example:**

“*Ex. 5.* Conceptual reconstructions have been achieved for at least 13 whole-plant species of Pennsylvanian tree-lycopsids assignable to seven whole-plant genera, based largely on anatomically preserved plant fossils (Bateman & al. in *Ann. Missouri Bot. Gard.* 79: 506–507. 1992). All three whole-plant species assigned to the whole-plant genus *Lepidophloios* include the root+rootlet organ-species *Stigmaria ficoides* (Sternb.) Brongn., which also occurs throughout the remaining six genera. The microsporangiate cone *Lepidostrobus*

*oldhamianus* Williamson occurs in all *Lepidophloios* species plus one additional whole-plant genus, whereas the megasporangiate cone *Lepidocarpon lomaxi* Scott is confined to the whole-plant genus *Lepidophloios*. The megaspore *Cystosporites giganteus* (Zerndt) Schopf is found throughout *Lepidophloios* and two other whole-plant genera. Although contrasting organ-species of both *Lycospora* Schopf, Wilson & Bentall microspores and *Lepidophloios* Sternb. stems+branches are unique to each of the three whole-plant species recognised within the whole-plant genus *Lepidophloios*, the stems reliably provide more diagnostic characters than the microspores. Thus, the organ-species of root+rootlet, microsporangiate cone, megasporangiate cone and megaspores are perceived as form-species of varying degrees of taxonomic generality, whereas the organ-species representing stem+branch and microspores are perceived as autapo-species that are genuinely diagnostic of the source fossil plant.”

### Postscript

Readers will note that, prior to making the above recommendations, we have not discussed the complex issues that surround selecting a formal name to represent a reconstructed whole-plant species (e.g., Chaloner in Spicer & Thomas: 67–78. 1986; l.c. 1999). This is not an accidental omission; we have chosen in this article to concentrate on nomenclatural issues that, we believe, pertain across all palaeobotanical activities; our interests and recommendations are informed by, rather than confined to, the more specialised discipline of whole-plant reconstruction.

For the sake of completeness, we will state that we agree in principle with Cleal & Thomas that a conceptually reconstructed plant can be referred to as ‘the *Bilignea solida* plant’ rather than ‘*Bilignea solida*

Scott’. However, this supposedly informal designation (“outside of a formally codified system of nomenclature”: Cleal & Thomas, l.c.: 262) still employs a formal Linnaean binomial. Thus, the choice of a binomial to represent the whole-plant species, selected from among the available organ-species binomials, inevitably becomes vulnerable to application of the law of priority. If this law were rigorously applied in such cases, it is unlikely that the oldest names for two sister-species would represent the same category of organ (e.g., leaf), and even less likely that the first-described organ(s) would happen to bear the autapomorphic character state that in practice diagnoses the whole-plant species. Nor is the oldest name likely to represent the organ that bears the largest number of taxonomically informative character states within the relevant genus, family and so forth.

In our opinion, a maximally informative and utilitarian nomenclature for conceptual whole-plants will require suspension of the law of priority in the majority of cases, potentially precipitating a plethora of recommendations to conserve maximally informative younger names against suboptimally informative oldest names. The irony of the resulting paradox is not lost on us; the main driver to painstakingly reconstruct fossil plants from plant fossils *sensu* Cleal & Thomas (l.c.) is to approach the degree of morphological knowledge available to systematists for extant plants. Yet, in doing so, the nomenclatural rules in the present *Code*, developed primarily for extant plants, become increasingly difficult to apply.

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## (177–182) Proposals concerning names of taxa above the rank of family

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The *Code* (McNeill & al. in *Regnum Veg.* 146. 2006) makes a very important difference between names above family rank and those at family rank and below. The latter are subject to the principle of priority, and every taxon (with the exception of eight families listed in Art. 18.5) can have only one correct name in any one circumscription, position and rank. In contrast, those above the rank of family are not subject to priority, and one taxon may have numerous different names which are all correct. At ranks such as order, one can choose any name previously published at that rank, or, if one likes, choose a new name. This difference impinges on author citations and valid publication of higher names.

If in a taxonomic publication one accepts a taxon above the rank of family, such as an order, one is generally not very interested in who first published the name that one adopts. Of much greater significance is whether any previous author or authors have adopted the name in the same sense. For instance, if one is referring to the order *Ericales*, it is relevant to know whether one is using the name in the sense of Lindley (who first published the name), or Bentham and Hooker, or Engler, or Cronquist, or APG or anyone else. So why do we write *Ericales* Lindley? Nobody worries if the names *Magnoliophyta* or *Pinophyta* are written without author names following them, and to

do so would seem to have no benefit. In many contexts, it would be preferable to encourage authors to write, wherever relevant, “*sensu*” followed by an earlier author who has used the name in the same or very similar sense, or, if the name has been used before but only in a significantly different sense, to add *sensu novo*.

Valid publication at ranks of family and below determines the date from which priority operates. But at ranks above family there is no priority, and the concept of valid publication is meaningless. There is no penalty for publishing a name invalidly. If a name is published without a Latin description, it does not matter, because anybody in future can use that name or any other name they like. So why do we insist on having a (Latin) description for names of orders etc.? It serves no function. We can operate perfectly well without the concept of valid publication above family rank. Below is a package (Prop. 177–180) aimed at removing the citation of author names and the concept of valid publication for names above the rank of family.

There are also some concerns about the rules governing the orthography of names at these higher ranks. Recommendation 16A appears to merely *recommend* which terminations are to be adopted at various ranks, which suggests they are not obligatory. But Art. 16.3