# Linnaeus and the *PhyloCode*: where are the differences?

Max C. Langer[1]

## On stability

Phylogenetic nomenclature (PN) was introduced in a series of papers by de Queiroz & Gauthier (1990, 1992, 1994), and has developed into an unofficial code of biological nomenclature, the *PhyloCode* (Cantino & de Queiroz, 2000). Yet, PN has been recently criticised by some authors (Benton, 2000; Nixon & Carpenter, 2000; see also Forey, 2001), because of its supposed instability if compared to the traditional ("Linnean") system. Indeed, when "stability" is measured in terms of "taxa included in a group referred to by a name" (Nixon & Carpenter, 2000: 301), PN is highly unstable, since a taxon whose name is defined under the PN system can present great differences in inclusiveness depending on which phylogenetic hypothesis is adopted as a template (see examples in Dominguez & Wheeler, 1997; Benton, 2000). However, for de Queiroz & Gauthier (1994), "stability" means that "a name should not designate different taxa, nor a taxon be designated by different names".

Central to the differences between Nixon & Carpenter's and de Queiroz & Gauthier's concepts of stability are the notions of "taxon name definition" and "taxon circumscription" (de Queiroz, 1992; Stuessy, 2000). As stated by de Queiroz (1992), the definition of a taxon name simply serves the purpose of "specifying the meaning of a symbol" (see also Ghiselin, 1966; Løvtrup, 1987; Härlin & Sundberg, 1998). Taxonomic circumscription, on the other hand, is the determination of which biological entities are included in a given taxon (de Queiroz, 2000; Stuessy, 2000). Accordingly, a taxon is first given its name (which is typified and given a definition), followed by circumscription based on aggregation of the entities that conform to that definition. In the present paper, following de Queiroz (1992), taxa (or taxonomic) definitions are used solely to describe the first of those procedures.

The "stability" of de Queiroz & Gauthier refers to the definition of taxon names, whereas that of Nixon & Carpenter refers to the circumscription of a taxon. Certainly, Nixon & Carpenter dismissed the stability of de Queiroz & Gauthier as metaphysical, and so it is. It refers to definitions, which have "nothing whatever to do with the practical problem of identifying the things that might happen to fit the definition" (Ghiselin, 1966). In fact, as discussed by Bryant (1997), only in the presence of a classification hypothesis does the definition of a taxon name specify the composition of that taxon. Accordingly, because they "say nothing empirical" (Ghiselin, 1966), taxonomic definitions are *per se* always stable. The correlation between a taxon and its name is always that which was defined, regardless of which class of definition that is. Using the recurrent example of birds, the concept of *Aves* is stable, whether it is defined by the presence of contour feathers or as "the clade

[1] Department of Earth Sciences, University of Bristol, Wills Memorial Building, Queens Road, BS81RJ Bristol, U.K. E-mail: max.langer@bris.ac.uk.

stemming from the most recent common ancestor of *Archaeopteryx* and the sparrow" (see Benton, 2000).

PN and traditional taxonomy are, therefore, neither more nor less stable than one another regarding the definition of names. Yet, Nixon & Carpenter (2000) claim for the instability of PN in terms of circumscription of taxa, the "stability in practice" of Benton (2000). Indeed, as already mentioned, taxa whose names were defined under the PN system are unstable when it comes to their contents. But is traditional taxonomy more "stable in practice" than this?

Surely, the differences between PN and traditional taxonomy in terms of the stability of their taxonomic circumscriptions reflect how stable are the classification parameters each system uses to circumscribe taxa. In PN, the tree-structure of phylogenies is the main parameter, whereas traditional taxonomy usually relies on sets of organismal traits (de Queiroz, 1992; Benton, 2000). Accordingly, to belong to a defined clade, in the case of PN, and to possess a determined organismal trait, in the case of traditional taxonomy, are the criteria based on which the inclusion of "taxa of lower rank and individual organisms" (Ride & al., 1999) in a taxon is determined. If the definition of these parameters is not stable, there is variation in the inclusiveness of the circumscribed taxa.

In the case of a taxon whose name is defined by PN, the stability of its circumscription depends on the agreement among different authors concerning its phylogenetic relationships (see Benton, 2000). For example, the circumscription of *Aves* defined as "the clade stemming from the most recent common ancestor of *Archaeopteryx* and the sparrow" varies depending on how closely related are *Archaeopteryx* and the sparrow in relation to other putative avian taxa in different phylogenetic frameworks. Similarly, the circumscription of taxa whose name was defined by traditional taxonomic procedures is also unstable when the definition of the organismal trait that it represents is not itself stable. Indeed, if authors start to disagree about what a contour feather is, the inclusiveness of *Aves* (as defined by the presence of that organismal trait) would depend on which definition of contour feather is used. This is as much instability in practice as when there is variation in the definition of which "taxa of lower rank and individual organisms" are included in the clade formed by "the most recent common ancestor of *Archaeopteryx* and the sparrow".

As pointed out by Benton (2000) the bird example "is so familiar that it could be said to be unfair". Indeed, circumscription criteria of traditional taxonomy are often not so clear as that of birds (in fact that of birds is not so clear itself), relying on more obscure morphological features, as well as on the traditional understanding of the group relationships as advocated, and strongly influenced, by some authority. Whatever the parameter these authorities use, they do not seem to change as frequently as phylogenetic hypotheses do. Indeed, a taxon circumscribed based on such parameters is usually somewhat stable in terms of its content.

In conclusion, PN and traditional taxonomy are not significantly distinct from one another in terms of their taxonomic definitions. Yet, the two systems differ regarding the parameters used for taxonomic circumscription. Besides, as already discussed, lack of consensus in the definition of those parameters leads to instability in the content of a taxon. Indeed, because the parameters used in PN (the

phylogenies themselves) have changed more dynamically than those used in traditional taxonomy (the definition of organismal traits), the taxonomic circumscriptions of PN are less stable. It follows that stability in taxon circumscription is a direct correlate of how settled concepts are in science. Accordingly, it should be considered whether the stability in the definition of the parameters that leads to stability in practice reflects "accuracy and depth of understanding" or just "ignorance and lack of work" (quotes from Gaffney, 1979: 103.

**On explicitness**

The frequency with which authority judgements occur in traditional taxonomy, and the sometimes inscrutable parameters by which a taxon is circumscribed, led the proponents of PN to claim that their system is more "explicit" or "clear". In the next paragraphs it is suggested that this is not always the case. Although PN circumscriptions are often clearer, those of traditional taxonomy can theoretically have the same level of explicitness. For this discussion, I do not follow the definition of explicitness given by de Queiroz & Gauthier (1994), as clearness in "the association between a name and a taxon". This refers to the definition of a taxon name, which is always precise (see Ghiselin, 1966). Instead, explicitness is here analysed in terms of taxonomic circumscription, i.e., clearness in the association between a taxon referred to by a name and its composition.

In practice, there are two reasons why traditional taxonomy is said to be less explicit than PN: (1) because of the inscrutability of the parameters that often govern traditional taxonomic circumscription; and (2) because traditional taxonomic circumscriptions often do not mirror phylogeny, i.e., they refer to paraphyletic assemblies of organisms. The first reason is obviously justified, and to illustrate this problem, an example involving basal dinosaurs is presented. Benedetto (1973) erected the family *Herrerasauridae* to include two genera, *Herrerasaurus* and *Staurikosaurus*. Later, Galton (1977) stated that: "I consider that *Herrerasaurus* and *Staurikosaurus* are sufficiently different (see diagnosis given below) to warrant erection of a new family *Staurikosauridae*". Such a statement is symptomatic; for Benedetto (1973), *Herrerasauridae* includes *Herrerasaurus* and *Staurikosaurus*; for Galton (1977), it includes only *Herrerasaurus*. This is instability in practice (in the taxon circumscription) promoted by instability of the parameters involving taxonomic circumscription, i.e., those related to "I consider" and to the "diagnosis given below". Again, authority followed by a complex combination of morphological features (see Galton, 1977), that in no way resemble clear statements like the possession of contour feathers. Indeed, this sort of taxonomic circumscription is as unstable as, and surely less explicit than, those of PN. Besides, this is surely not an isolated and especially faulty example of taxonomic practice, but represents typical and regular procedures, seen in most traditional taxonomic works.

The second reason why classical taxonomy is said to be less explicit than PN is not justified. That a particular taxon is not monophyletic does not mean that its circumscription is not explicit. It is important, at this point, to divorce taxonomic explicitness from phylogenetic adequacy. Coming back to the bird example, with the same degree of explicitness, all organisms nested within the clade defined by

*Archaeopteryx* and the sparrow are birds, and all organisms sharing the presence of contour feathers are birds. Indeed, if birds are defined by the possession of contour feathers, a supposed featherless taxon phylogenetically nested within the group is not considered a bird.

As an aside, it is important to stress that PN taxonomic definitions can only be applied in the presence of a phylogenetic hypothesis. However mainstream the construction of classification schemes based on common descent might be, and has been for a long time (Gaffney, 1979), other taxonomic parameters exist (e.g., overall similarity), or may evolve (Jørgensen, 2000; Hull, in Funk, 2001). Even under an evolutionary framework, the validity of non-genealogy-based classifications is supported by their utilitarian nature (Benton, 2000), and by the definition of taxa as abstract human constructs (Muir, 1968; Løvtrup, 1987).

One of the most significant features of the existing biological codes (Lapage & al., 1992; Ride & al., 1999; Greuter & al., 2000) is that they govern taxonomic practice regardless of which classification parameters are applied—they are theory-independent as asserted by Lidén & al. (1997; see also Moore, 1998). Accordingly, under "definition" the ICZN (Ride & al., 1999) simply means "a statement in words that purports to give those characters that, in combination, uniquely distinguish a taxon" (note that the word character is not restricted to morphological character). PN taxon definitions, on the other hand, can not be applied to classification systems other than those utterly connected to theories of common descent. The *PhyloCode*, therefore, has no legitimacy to govern the entire taxonomic practice, which is not restricted to the unification of classification schemes and hypotheses of genealogy.

Summing up, traditional taxonomy circumscriptions are not always less explicit than those of PN, but the latter are the only ones that strictly reflect phylogenetic hypotheses. Indeed, to explicitly circumscribe a taxon based on phylogeny is the ultimate goal of PN, and it achieves this aim. Yet, in this enterprise, because of the instability of phylogenetic hypotheses, the stability of PN in terms of taxonomic circumscriptions was sacrificed. Clearly, the lack of stability and the explicitness of PN circumscriptions are intimately correlated. They constitute, at the same time, the great weakness and the most important characteristic of that nomenclatural system.

## Why the *PhyloCode*?

Why should taxonomy reflect phylogeny since, as accurately asserted by Benton (2000), "phylogenies are 'real', classifications are not" (internal quotes mine). In fact, classifications do not need to reflect phylogeny. Yet, most authors would agree that it is not enough simply to regard taxon names as utilitarian, and that part of the scientific enterprise is to provide definitions which are backed up by empirical knowledge. In the case of authors working under a phylogenetic perception, this knowledge is represented by the genealogical reconstructions as given by phylogenies. Indeed, a direct consequence of the so-called "cladistic revolution" was that there were several attempts to merge phylogenetic hypotheses with "Linnaean" nomenclature (Hennig, 1966; Nelson 1972; Patterson & Rosen, 1977; among others). Yet, only PN has been simple and revolutionary enough to spread as a real alternative to traditional taxonomy. This is exactly because PN did not attempt to merge the Linnean nomenclature with phylogeny. Instead, it represents a completely

independent system, which can be used not only instead of traditional taxonomy, but also alongside it.

Forey (2001: 89) mentions that the abolition of ranks (as advocated by the *PhyloCode*) would have undesirable consequences for biodiversity studies, specially for those adopting a "taxic approach" (see Smith, 1994). Yet, as discussed above, the utilitarian aspect of classifications are subsidiary to their reliability. If the taxic approach is refuted because ranks have no biological meaning, methods should be devised in order to incorporate new taxonomic approaches into biodiversity studies.

In addition, Benton (2000) accused the *PhyloCode* of being "draconian" and claims that because classifications are utilitarian, they should be worked out by the "systematists, and users of classifications". These are supposedly those authorities mentioned earlier, making use of their often-obscure parameters for taxonomic circumscription. This vision is not endorsed here! However authoritarian the *PhyloCode* might be—and what code of nomenclature is not authoritarian (viz., Lapage & al., 1992; Ride & al., 1999; Greuter & al., 2000)—it is explicit, and defined "by law" rather than "by people". Surely, in the particular case of the *PhyloCode*, the law is still in the process of being created, and this is done by people. Indeed, now is the right moment to discuss not only the provisions of the *PhyloCode*, but also what should be its role in taxonomy in general, i.e., which practices (if any) it is supposed to govern.

**Literature cited**

Benedetto, J. L. 1973. *Herrerasauridae*, nueva familia de Saurísquios Triásicos. *Ameghiniana* 10: 89–102.

Benton, M. J. 2000. Stems, nodes, crown clades, and rank-free lists: is Linnaeus dead? *Biol. Rev.* 75: 633–648.

Bryant, H. N. 1997. Cladistic information in phylogenetic definitions and designated phylogenetic contexts for the use of taxon names. *Biol. J. Linn. Soc*. 62: 495–503.

– Cantino, P. D. & de Queiroz, K. 2000. *PhyloCode: a phylogenetic code of biological nomenclature*. [http://www.ohiou.edu/phylocode/printable.html]

de Queiroz, K. 1992. Phylogenetic definitions and taxonomic philosophy. *Biol. & Philos.* 7: 295–313.

– 2000. The definition of taxon names: a reply to Stuessy. *Taxon* 49: 533–536.

– & Gauthier, J. A. 1990. Phylogeny as a central principle in taxonomy: Phylogenetic definitions of taxon names. *Syst. Zool.* 39: 307–322.

– & – 1992. Phylogenetic taxonomy. *Ann. Rev. Ecol. Syst.* 23: 449–480.

– & – 1994. Towards a phylogenetic system of biological nomenclature. *Trends Ecol. Evol.* 9: 27–31.

Dominguez, E. & Wheeler, Q. D. 1997. Taxonomic stability is ignorance. *Cladistics* 13: 367–372.

Forey, P. L. 2001. The PhyloCode: description and comentary. *Bull. Zool. Nomencl*. 58: 81–96.

Funk, V. A. 2001. SSZ 1970–1989: A view of the years of conflict. *Syst. Biol*. 50: 153–155.

Gaffney, E. S. 1979. An introduction to the logic of phylogenetic reconstruction. Pp. 79–111 *in:* Cracraft, J. & Eldredge, N. (eds.), *Phylogenetic analysis and paleontology*. New York.

Galton, P. M. 1977. On *Staurikosaurus pricei*, an early saurischian dinosaur from the Triassic of Brazil, with notes on the *Herrerasauridae* and *Poposauridae*. *Paläontol. Z.* 51: 234–245.

Ghiselin, M. T. 1966. An application of the theory of definitions to systematic principles. *Syst. Zool.* 15: 127–130.

Greuter, W., McNeill, J., Barrie, F. R., Burdet, H. M., Demoulin, V., Filgueiras, T. S., Nicolson, D. H., Silva, P. C., Skog, J. E., Trehane, P., Turland, N. J. & Hawksworth, D. L. 2000. International Code of Botanical Nomenclature (Saint Louis Code). *Regnum Veg*. 138: 1–474.

Härlin, M. & Sundberg, P. 1998. Taxonomy and philosophy of names. *Biol. & Philos.* 13: 233–244.

Hennig, W. [Translated by D. D. Davis & R. Zangerl]. 1966. *Phylogenetic systematics*. Urbana.

Jørgensen, P. M. 2000. Names are defined, but not as taxa. *Taxon* 49: 779.

Lapage, S. P., Sneath, P. H. A., Lessel, E. F., Skerman, V. B. D., Seeliger, H. P. R. & Clarke, W. A. 1992. *International code of nomenclature of bacteria and statuses of the bacteriology and Applied Microbiology Section of the International Union of Microbiological Societies*. Washington.

Lidén, M., Oxelman, B., Backlund, A., Anderson, L., Bremer, B., Eriksson, R., Moberg, R., Nordal, I., Persson, K., Thulin, M. & Zimmer, B. 1997. Charlie is our darling. *Taxon* 46: 735–738.

Løvtrup, S. 1987. On species and other taxa. *Cladistics* 3: 157–177.

Moore, G. 1998. A comparison of traditional and phylogenetic nomenclature. *Taxon* 47: 31–34.

Muir, J. W. 1968. The definition of taxa. *Syst. Zool.* 17: 345.

Nelson, G. L. 1972. Phylogenetic relationships and classification. *Syst. Zool.* 21: 227–231.

Nixon, K. C. & Carpenter, M. J. 2000. On the other "phylogenetic systematics". *Cladistics* 16: 298–318.

Patterson, C. & Rosen, D. E. 1977. Review of ichthyodectiform and other Mesozoic teleost fishes and the theory and practice of classifying fossils. *Bull. Amer. Mus. Nat. Hist.* 158: 81–172.

Ride, W. D. L., Cogger, H. G., Dupuis, C., Minelli, A., Thompson, F. C. & Tubbs, P. K. 1999. *International code of zoological nomenclature*. London.

Smith, A. B. 1994. *Systematics and the fossil record*. Oxford.

Stuessy, T. F. 2000. Taxon names are *not* defined. *Taxon* 49: 231–233.