

The changing paradigms of biological systematics: new challenges to the principles and practice of biological nomenclature

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'Biological nomenclature is supposed to deal with names alone, not with concepts, but historical examples show how wrong this idea can be' (Stevens, 1994, p. 488).

In all departments of science it is sometimes profitable to distance oneself from the day-to-day practice, and to reflect upon the nature and the theoretical foundations of our work. This is also true of biological nomenclature, and in this field these are times of change. The draft of the fourth edition of the *International Code of Zoological Nomenclature* has been circulated and is being discussed. The debate is also lively in botany, as may be seen from the last few volumes of *Taxon*, while a forthcoming *Code of Bionomenclature* is on the horizon (Hawksworth et al., 1994). Therefore, some refreshed (or refreshing) thought on the basic aspects of bionomenclature seems to be timely.

'It is hardly surprising that major changes in nomenclature tend to occur when there are major changes within systematics' (Stevens, 1991, p. 164). Is there any correspondence, or agreement, between the major approaching changes in nomenclature and the current wide-ranging labour affecting all aspects, both theoretical and practical, of biological systematics (Minelli, 1993)?

How far is nomenclature truly independent from taxonomy?

In this most general question I do not refer to the nomenclatural consequences of the most usual kinds of taxonomic decisions, e.g. whether to regard two nominal species as distinct or not, or whether to place them in the same genus rather than in two different genera. In this area, things are mostly running in a satisfactory way, and some of the revised provisions in the proposed new Codes will certainly help smooth out a lot of residual difficulties.

I do not refer here either to operationally large, but theoretically minor, problems such as the nomenclature of ambireginal organisms. For these (some thousands of taxa) it is a matter of taxonomic decision whether to put them under the Zoological Code or to treat them as 'plants', thus referring them to the provisions of the Botanical Code. (For a recent assessment of this aspect see Corliss, 1995).

There are, instead, much more basic questions. 'A purely nomenclatural argument may be much less common than we think, but the concepts brought to bear in such arguments are diverse. Species concepts are only one set of them, and possibly not even the most important. The whole systematic discipline, what systematists should do, and how the discipline should be organised, may also be at issue. That is surely the case now' (Stevens, 1991, p. 166).

Biological nomenclature aims to provide a universal, consistent, stable and user-friendly system of names. The question for what kinds of users these names are produced has been recently debated at length and from very different points of view (see, for example, Hawksworth & Bisby, 1988; Haskell & Morgan, 1988; Hawksworth, 1991). Much less debated, however, is what kind of objects, or

concepts, we are providing names for. But this is really the point where taxonomy stands out as the real, although seldom acknowledged, ruler of biological nomenclature.

We are accustomed to take it for granted that the (only) names with which formal biological nomenclature is concerned are those for species taxa and supraspecific taxa (genera and families at least), corresponding to the individual taxa in a hierarchical classification, but it could, or perhaps should, be otherwise.

Species names

Within the present Zoological Code there is no place for animals not obviously belonging to 'species'.

Take, for instance, hybrids. Until recently, natural hybrids were regarded as a peculiarity of the plant world, their very rare occurrence among animals being so exceptional as to be better ignored from the viewpoint of nomenclature. As for artificial hybrids, these could always be described as such by listing together the names of the parental species, thus obtaining a more definitive nomenclatural treatment than the still uncertain names we use for some domestic animals (Groves, 1995). However, our traditional view of natural animal hybrids has changed as a consequence of the progress of cytogenetics, more recently complemented by biochemical and molecular studies. There are, naturally occurring, many hybrid forms which are at least as stable and well-circumscribed as many conventional species. In terms of nomenclature, these forms are often denoted by formulae, rather than by Linnaean names, but there is no universality of attitude towards them. Echelle (1990a, b), for instance, argues that the 'non-Mendelian species' of hybridogenetic fishes and reptiles should be treated, from the point of view of nomenclature, as are the usual 'Mendelian' species.

Besides hybridogens, there are several other classes of uniparentally reproducing animals (and plants) which are usually given conventional species names. They are quietly listed in catalogues, or keyed out in monographs, in a way not different from that for the other named 'species'. The potential dangers of this uniform taxonomic treatment (Minelli, 1993) are hardly lessened by the fact that these uniparental 'species' are sometimes called — in some groups at least — agamospecies or microspecies, rather than species. According to several authorities (e.g. Dobzhansky, 1937; Mayr, 1969; Hull, 1980; Ghiselin, 1987), however, these organisms *do not form species*. If we agree with this view, how can we accept that they are named as if they *are* species?

At a recent (April 1995) workshop in Cardiff, sponsored by the Systematics Association, some two dozen taxonomists gathered to discuss 'The Species in Practice', as experienced by specialists working with organisms as different as viruses and bacteria, flowering plants and insects, mammals and freshwater fishes. The conclusion (Claridge & Dawah, in press) was that the entities uniformly treated as species under the nomenclatural principles of the Codes are extremely heterogeneous. This heterogeneity is only partly dependent on the different personal attitudes of the specialists or the different taxonomic traditions prevailing in the different groups. To be sure, substantial differences in attitude and tradition are there, but these are of minor importance when compared with the real differences in the ontological status of the basic taxonomic units we call species in the very diverse groups of organisms.

This means that many statistics involving species numbers must be looked at very cautiously, and sometimes even be rejected as nonsense. For instance, in discussions dealing with biodiversity and the current state of our inventory of the living world, we are accustomed to offer, or to read, such estimates as 'vertebrates are 2.5% of the living species named to date', as if 'species' were 'the same' [whatever this expression may biologically mean] in birds and bacteria, rotifers and reptiles!

The same with fossils. We are seldom ready to vigorously react, as we indeed should do, when somebody tells us that the named fossil species are, say, one in 10,000 of the cumulative number of species the Earth has generated since the primeval past. This 1:10,000 ratio is just a ratio between the size of an actual list of names and the size of another potential list. However, these two lists would deal with two different kinds of entity, quite apart from the objections we could easily raise as to the nature, or the homogeneity, of the entities within each one of them.

In the face of such current examples of lack of critical attitude I do not need to develop much theoretical argument. There is, however, the need to stress that such basic misconceptions stem largely from a less than critical attitude towards biological nomenclature. We cannot rightly blame the users of nomenclature for adding together apples and cherries when we, the producers of taxonomy and taxonomic nomenclature, knowingly conceal the amazing and still problematic diversity of objects and concepts under the obscuring veil of one and the same kind of names (Linnaean binomina).

To be sure, the scientific literature already abounds with evidence that Linnaean binomina are not always the best way of unambiguously conveying our appreciation of the taxonomic identity and status of the organisms we deal with. Formulae where a generic name is followed by an accession number or a locality name are not at all rare in papers dealing with molecular systematics or cytogenetics of some critical species groups. In many cases, the use of formulae rather than formal species names is not an expression of contempt towards traditional systematics and nomenclature, but the confessed perception that not everything in the living world fits neatly into our traditional taxonomic schemes.

Supraspecific taxa and hierarchical classifications

It is a matter of opinion whether the views of present systematists are more diverse concerning the nature and concept(s) of species, or of supraspecific taxa. At least to some the two problems are quite one and the same (e.g. Nelson, 1989, 1994; Cracraft, 1992). I will only add two points about the links between nomenclature and our views of supraspecific taxa.

The first is that the 'genus' is likely to have a unique status, in our minds, just because of its traditional role in our binominal nomenclature: 'A quite serious shortcoming of Linnaean nomenclature is that the generic name forms the foundation for the species name. The problem is, that genera are more arbitrary and more variable than species' (de Candolle, (1813) 1844, p. 216; my translation).

The second point is that the Linnaean hierarchy does not appear to be compatible with some attitudes, less conventional but nonetheless legitimate, towards biological systematics. Voices of discomfort have been raised many times, ever since Linnaeus, but these days such voices are distinctly more frequent and loud. This is not the place to critically review this kind of literature. I just point here to one of the theoretical

positions that induces some systematists to advocate something other than the Linnaean hierarchy and nomenclature. This position corresponds to the claim that classification in the traditional sense is a legitimate, but not necessarily the only or the primary, way of representing the outcome of systematic research. Griffiths (1974) was the first, to my knowledge, to argue that application of the Hennigian phylogenetic principles necessarily leads to the production of a *system* of hierarchically branching monophyletic units. The ontological status of the system is not the same as that of the traditional *classification*. A classification is a set of hierarchically nested subsets, or classes, whereas the system is a whole, of which the branches (from the major ones down to the terminal tips) are parts, or parts of parts. I refer to Griffiths (1974), Hennig (1975), Ax (1984), de Queiroz (1988) and Minelli (1991) for more details. It is enough, here, to refer to Griffiths's (1976) conclusion that adopting a phylogenetic approach to biological systematics means discontinuing the use of the formal Linnaean ranks.

From a palaeontological perspective, Willmann (1988, p. 901) has clearly expressed a concurrent idea: 'Neontologists as well as palaeontologists have been trapped by one aspect of the current classification of organisms, namely the ranking of taxa. Following Linné, the neontologists used to deal with ranks such as orders, suborders, classes, etc. Essential in ranking is the extent of the differences between the (recent members of) the groups. Fossils have often narrowed these gaps, and according to the theory of evolution originally no such gap ever existed. The categorial ranks, however, remained. From this resulted the problem of the origin of 'classes' and 'orders'. There are however no 'orders' or 'classes', 'genera', 'families' or 'suborders' as real units of Nature, these are artificial mental constructs dating from pre-evolutionary times. They are of no use in modern biology, mere anachronisms, not even necessary for the systematization of life ... It thus seems medieval when Stanley wrote as late as 1978 (p. 36) 'if genera typically arise by quantum speciation ... then families, orders, and classes must arise in the same manner, normally by several discrete steps'.

de Queiroz & Gauthier (1990, 1992, 1994; see also de Queiroz, 1992) have provided a well-argued discussion of a possible 'phylogenetic system of biological nomenclature'. Their proposal does not affect the nomenclature of species (the terminal taxa in the phylogenetic system), but requires a completely new way of dealing with supraspecific taxa. The problem is that 'under evolutionary interpretations of higher taxa and their names, the current system fails to accomplish its own stated purposes' of providing explicit, universal and stable taxon names (de Queiroz & Gauthier, 1994, p. 27). These authors claim that the definition of supraspecific taxon names should follow rules other than the current reference to Linnaean categories and nomenclatural types (e.g. to the genus *Agama* as the type genus of the family Agamidae). Instead, they suggest (pp. 28–29) a 'phylogenetic definition of taxon names ... in terms of common descent and the phylogenetic entities deriving their existence from that process. For example, the name 'Agamidae' might be defined as the clade stemming from the most recent common ancestor of *Agama* and *Leiolepis*'. They are sensible enough to consider whether such phylogenetically defined taxa would accord with the principle of preserving freedom of taxonomic thought and action. Happily, they offer an affirmative answer to this question, pointing to the fact that taxonomists must still subjectively determine the contents and diagnostic characters of taxa.

This radical approach to the nomenclature of supraspecific taxa would probably answer many problems recently raised from within the ranks of cladists. For instance, Meier & Richter (1992) have argued that the current usage of taxon names is ambiguous, because the same name is sometimes used for taxa including the stem lineage and sometimes for the crown taxa only.

We must expect that the developments of cladistics will increasingly ask for a revised biological nomenclature. Other, not necessarily overlapping, requests can be expected from other corners of the wide world of systematists. Science generates concepts, concepts need names and names are very effective in shaping (and concealing) ideas. To be sure, all these unconventional propositions are unlikely to generate in the near future a viable reformulation of the current Codes or a self-consistent alternative to them. History shows that the transition from the pre-Linnaean polynomial nomenclature to the Linnaean binomina was not accomplished overnight. It took decades from the pioneering efforts of Strickland and de Candolle to establish full-fledged International Codes of nomenclature. Therefore, all these critical attitudes towards the Linnaean (pre-evolutionary) nature of biological nomenclature must be approached without anxiety and with a constructive attitude. To be sure, we should never throw away two centuries of names, with all the associated information, taxonomic and other! I am delighted to see that most recent advances in phylogenetic systematics have been accomplished without much nomenclatural trouble, at the species level at least. Things are different, however, at the higher levels, where the use of rank terms such as order or class is quite often abandoned: what is left is just place for an open sequence of relative ranks, such as the 15 levels recognised by Ehlers (1985, p. 168) between Plathelminthes (traditionally, a phylum) and the two sister groups Caryophyllidea and Eucestoda (traditionally, two subclasses or the like).

Nomenclature in the service of science

To sum up, I welcome change. By this I do *not* mean changes of animal and plant names for purely 'nomenclatural' reasons; so far as these are concerned, I cannot but side with the users of nomenclature. These are a very wide community to which we, producers of names included, all belong. Systematists are something more than mere name producers, steadily struggling with, or sometimes shaking hands with, the users of names. We systematists are people involved in the development of an old but today very lively science. Change, therefore, is by necessity the stuff of our professional life: change of concepts, of paradigms, of goals to achieve. Looking at the century-long stability of angiosperm taxonomy which followed Antoine-Laurent de Jussieu and Augustin-Pyramus de Candolle, Stevens (1994, p. 221) has recently commented: 'I will suggest that a distrust of theory, a system of instruction that is similar to an apprenticeship and a tendency to look to past masters of the discipline for justification are interconnected factors leading to stasis.'

Things are perhaps a little bit better now, but we have on our shoulders, as systematists, the full responsibility of looking for sound policy in taxonomy as well as in nomenclature. We must be well aware of the deep interconnections between science and names. We face the formidable task of improving the independence and creativity of our discipline while, at the same time, promoting and improving the stability of names.

Haskell & Morgan (1988), Minelli (1991, 1993) and Bogan & Spamer (1995) see, either with approval or with anxiety, that a possible outcome of current trends in bionomenclature could be the development of a 'systematic bipartisan nomenclature', one side of it being for the specialists (systematists), the other side for the users. Whatever the future, we shall need a complex nomenclatural machinery. With this prospect, we should wholeheartedly welcome proposals such as the principle of registration of new names and the production of species lists that are being offered as operational improvements of the current Codes, i.e. as the keystones of the biological nomenclature of the (near) future.

But that is not the end of the story. With the launch of a fourth edition of the International Code of Zoological Nomenclature and with an active involvement in the production of a Code of Bionomenclature, the International Commission on Zoological Nomenclature, and the zoological community at large, are making a more substantial step forward than ever in the past. The next steps, however, will really need to be a jump or several jumps. To avoid breaking our old bones it would be prudent to start studying all the aspects of the landscape we have to go through.

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