

LETTER TO THE EDITOR

Am I a bony fish?

It has been interesting to read the responses to the 150 signatories of the statement on paraphyly presented by Nordal & Stedje in *Taxon* 54: 5–6 (2005). The comments by Williams, Ebach & Wheeler on p. 858, by Potter & Freudenstein on pp. 1033–1035, by Monsch on pp. 1036–1038, and by Dias, Assis & Udulutsch on pp. 1039–1040, all in *Taxon* 54(4) (2005 publ. 2006), are opposed to the views expressed, while those of Alexander in *Taxon* 55: 4 (2006) seem broadly but apparently not completely sympathetic. The question of paraphyly is, I feel, the most important issue under debate in taxonomy today. The matter can only be resolved by open debate, so all comments are welcome. At the risk of seeming boringly repetitive to readers of this journal, I would like to focus attention on key issues which have not been fully addressed in these responses.

In a much quoted paper which might be said to have started this discussion, de Queiroz & Gauthier in *Ann. Rev. Ecol. Syst.* 23: 449–480 (1992) noted that the Linnaean system of classification (i.e., a system recognising ranked taxa such as families, genera and species) is incompatible with a system which recognises only monophyletic taxa (clades). This is immediately obvious to many people, but apparently not to others. In *Taxon* 46: 723–734 (1997) I tried to emphasise this difference, and in *Taxon* 51: 32–33 (2001) I tried to explain in simple terms why this is. As soon as we apply ranked taxa to the full phylogeny of any monophyletic group, we create paraphyletic taxa. If we do not accept paraphyletic taxa, the entire classification will collapse into the original family, genus and ultimately species. Do those favouring cladistic classification accept these points or not?

Putting it another way, I have twice (*Taxon* 51: 34–35 and 52: 803–804) asked opponents how it can be that when evolution is creating wider and wider variation, they favour a classification which must give lower and lower ranks as evolution progresses. But I have had no answer. Surely there is something wrong with this idea. A good example illustrating the point is afforded by the Cactaceae case mentioned below. It seems to me, and apparently to many others, that there is a logical fallacy in the theory behind such a classification. Do those favouring cladistic classification agree that the nearer we come to having a complete phylogeny, the less acceptable their position becomes, and that if we have all the facts their position is a logical impossibility?

It is argued by some (Freudenstein in *Taxon* 47: 95–104 (1998); Potter & Freudenstein loc. cit. supra) that since we do not have the ancestors of our present-day taxa we need to consider only extant taxa included in a cladogram and

need not take ancestors into consideration. But if a paraphyletic taxon is defined in terms of ancestry, i.e., one which includes some but not all of the descendants of a common ancestor, it does not make any sense to say that in determining monophyly or paraphyly we should consider only extant taxa and ancestors are not relevant. They are essential to the definition of paraphyly. And how do we take fossils into account? Nobody has proposed (as far as I know) having a system of classification for fossils different from that for extant organisms. Furthermore, ancestral groups do not always die out. I understand that mammals have evolved from bony fishes. If we recognise the bony fishes as a taxon Osteichthyes at the rank of class or subclass or any other rank, do I have to agree to being a member of that taxon? And presumably the mammal line has been descended from within one group of bony fishes. Is it sensible to have a classification in which some bony fishes are placed in a taxon including the amphibians, reptiles, birds, mammals and so on but excluding many other fishes? This is what cladistic classification would require.

To most taxonomists, classification depends on characters. If one knows the phylogeny but cannot accept paraphyletic taxa, one has to either sink derived taxa even though they may have very distinctive characters (like mammals) or split the ancestral group (bony fishes) even if there are no appropriate characters to do so. This has been accepted and explicitly stated by those adopting a cladistic broad concept of the genus *Veronica* (Albach & al. in *Taxon* 53: 429, 2004). But can this principle really produce good taxonomy? Again, surely something is wrong if we are producing a classification rather than a phylogeny. It seems that characters are no longer important. Do the opponents of the 150 signatures argue that characters are no longer relevant in classification?

When it was found that the Cactaceae nested within the broad concept of the genus *Talinum* in Portulacaceae, I was amused to receive a message from a sympathiser saying (tongue in cheek—I think) that he was going to propose 3500 new combinations in *Talinum* for all the species of the Cactaceae. The theory of cladistic classification is so wrong that distinctive groups which are sunk into another family or genus can usually no longer be recognised even at subfamily or subgeneric rank because they would just make another subfamily or subgenus paraphyletic. Sometimes the ancestral group is split into two (or more) in the hope that the derived group will fall through an imagined gap between them (as in the case of *Talinum*), but the derived group must always have arisen from something. Even if

you split *Talinum*, the whole of the family Cactaceae, already including various subfamilies and tribes, would have to be included within one subtribe of the Portulacaceae. And ultimately, every monophyletic group has apparently arisen from one ancestral species.

The statement of Nordal & Stedje noted that cladistic classification is causing chaos in taxonomy, but this has been denied in the responses. It depends on how you perceive chaos. The recent disintegration of the Scrophulariaceae may seem like chaos to some. If we have to sink Hydrostachyaceae into Hydrangeaceae, Podostemaceae into Clusiaceae, Hippuridaceae (flowers consisting of an inferior ovary and a single stamen) into Scrophulariaceae, the whole of the Juncaceae into *Juncus*, many distinctive genera into *Lobelia*, and many other cases, we are moving towards a generally chaotic situation in my opinion. I try not to be critical of my fellow botanists, but the Primulaceae presents another relevant case. Molecular data have very interestingly demonstrated that the Myrsinaceae nest within the traditional Primulaceae (rather than the other way round which many might have expected). However, transferring a large part of the Primulaceae (including *Lysimachia* and *Cyclamen*) to the Myrsinaceae (see interesting treatment in Kubitzky's *Families and Genera of Vascular Plants*, vol. 6, 2004) does not seem useful to me, particularly when no formal recognition can be given to that group because it would still be paraphyletic with respect to the rest of the Myrsinaceae. Furthermore, we find that *Dodecatheon*, *Dionysia*, *Soldanella*, *Omphalogramma*, *Kaufmannia* and *Cortusa* all make *Primula* paraphyletic, *Douglasiana*, *Vitaliana* and *Potamosace* make *Androsace* paraphyletic, and *Anagallis* and *Glauca* make *Lysimachia* paraphyletic. To me, sinking these derived genera or splitting the ancestral ones would not be helpful, would not be good taxonomy, and would create a fair amount of chaos in the classification of the Primulaceae. At the time of writing, all parties concerned seem to have wisely refrained from making the formal changes.

Taxonomy should be able to recognise distinctive features which have evolved in a group. If we cannot do this, we cannot reflect evolution. If we sink very distinctive genera into a paraphyletic parental taxon (*Veronica*, *Lobelia*, *Juncus*, *Primula*, etc.), we are simply merging everything into one amorphous plesiomorphic soup. To me, this is not a reflection of evolution, it is a denial of it. Contrary to what Potter & Freudenstein (loc. cit., p. 1035) have said, it is cladistic classification which emphasises plesiomorphic characters at the expense of apomorphic characters. Traditional taxonomy permits recognition of taxa based on apomorphic characters. The cladistic approach fails because it does not permit recognition of newly evolved taxa.

The thinking behind cladistic classification is based on a confusion between a phylogeny and a taxonomy. A

phylogeny has no classes, as noted by Knox recently, while a taxonomy itself has no phylogeny although it may take phylogenetic data into consideration. They have different methodologies, different structures, different properties and different uses. Some people try to treat them as the same thing, and try to attribute the properties and uses of a phylogeny to a classification. But if we already have the phylogeny, why is it necessary to try to derive the phylogenetic information from the classification (see also *Taxon* 52: 804, 2003)? If we have a horse and a cow, we would not go to the horse if we want milk and would not normally choose the cow if we want to go riding. The basic incompatibility between a phylogeny and a classification will always create problems if we confuse them.

The response by Williams, Ebach & Wheeler (loc. cit.) has stated that the 150-signature statement makes a false connection between cladistics and the *PhyloCode*. But it seems to me that these things are intimately linked and we need to take a broad perspective. The *PhyloCode* is perfectly logical in promoting a system of monophyletic groups (clades) which make an almost infinite successively interesting series going right back to the year dot.

It is also perfectly logical in not imposing ranked taxa on this, because you cannot do that without creating paraphyletic taxa. I think this, rather than the reasons attributed by Potter & Freudenstein (loc. cit.), is the basic reason why the *PhyloCode* abandons ranks. The *PhyloCode* is logical (if we really must name clades) but completely impractical for general purpose communication, while traditional 'Linnaean' ranked classification with paraphyletic taxa is logical and practical, but ranked classification without paraphyletic taxa is illogical and impractical. We need to consider all three positions together, as Potter & Freudenstein (loc. cit.) have indeed very appropriately done.

But the matter is not of merely theoretical interest. Our taxonomy must be very practical since it has very many users. Both the theoretical and practical issues have been very nicely reviewed by Diggs & Lipscomb in *Sida* 20: 647–674 (2002). No doubt the arguments will go on, and we need further exchange of views to resolve the matter. I look forward to responses to the questions I have posed above. But I feel very confident that future generations will thank Inger Nordal and Brita Stedje for raising the profile of the discussion and showing that many taxonomists have serious objections to the theory and practice of cladistic classification. It is good that many who are part of the silent majority have at last been persuaded to express their view.

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