

## POINTS OF VIEW

## Further dogged defense of paraphyletic taxa

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I was invited to defend my views on cladistic classification at a symposium on Linnaean taxonomy at the Smithsonian Institution in 2001. When the paper was published (Brummitt, 2002) I received very many messages of support from people from all around the world. But in this business it seems one never convinces everyone, and I know that others are probably still critical of my views. We need discussion to resolve what I believe are some very important issues in systematics, but my opponents usually prefer to ignore what I hope is the logic of my case. So I was pleased and interested to see the reply to my paper by such prominent opponents as Nelson, Murphy & Ladiges (2003) in which they have credited me with dogged defense of paraphyly over a period of eight years.

They have taken up the challenge originally given by me and my co-author Marc Sosef (Brummitt & Sosef, 1998) to anyone to draw a phylogenetic tree (as distinct from a cladogram) divided fully into Linnaean taxa without any of them being paraphyletic. This is the first response to that challenge that we have received. But even if I ignore the requirement that contestants should draw their own tree and actually mark the taxa on it, I do not find that the solution now presented qualifies for the prize on offer. A paraphyletic group is one which includes a single common ancestor but not all its descendants. In my opponents' first scenario based on the Rosa diagram, they have divided it into genera (AB)e, A, B, (CD)e, C and D. In this case the genus (AB)e is paraphyletic in respect to genera A and B, and genus (CD)e is paraphyletic in respect to genera C and D. The genus (ABCD)e, mentioned on p. 296 but omitted (presumably accidentally) from the table between pp. 296 and 297, would be paraphyletic in respect to all other genera recognised. In their second scenario (p. 297), genus A would be paraphyletic in respect to B (or vice versa), and genus C would be paraphyletic in relation to D (or vice versa).

Thus, while I am grateful to the authors for putting their solution forward, they seem to merely emphasise the point I was making. As soon as you put the apex of the tree (or indeed the apex of any sector of the whole tree) into a genus or family, this must be paraphyletic in relation to any other genus or family recognised among

its progeny. If we are classifying all the products of evolution, every taxon we recognise (apart from the original one) must make another taxon paraphyletic. That is why, as has been pointed out repeatedly recently, traditional taxonomy is incompatible with a system of only monophyletic taxa. Every monophyletic group would collapse into its original family, genus and species. If the theory is nonsensical, then we can be sure that the resulting taxonomy will also be nonsensical somewhere. As others have noted, extinction is the saviour of cladistic classification, but it is incomplete and haphazard, and can only be a partial saviour. Inevitably situations will be found where traditional taxonomy is rendered unacceptable to most by strict adherence to the cladistic principle of monophyly. Sinking Podostemaceae into Clusiaceae does not seem to be good taxonomy to most people, and an increasing number of such absurdities is coming to light as the molecular information increases.

In comparing my original figure with that of Rosa which they placed in almost mirror image of it, my opponents have failed to comment on a most important difference between the two. In my figure, there is a major change in characters marked, but this has been ignored. The Rosa figure has no such feature, and so is easily divisible into four equal clades. If anyone likes to call these four genera, they are welcome to do so, though most people would like to know how the four genera differ from each other first. But to apply the same thinking to my figure, which the authors seem to do, would in my opinion be nonsensical. Three of the four clades would differ little from each other or show no differences, while the fourth would be very different. To make four genera in this case would not be good taxonomy. We cannot ignore characters in classification.

I believe taxonomy must depend on characters related to lines of descent, not simply on lines of descent alone. If we liken my opponents' Myrtaceous example to their Rosa figure, then of course it is very easy to recognise the four genera *Corymbia*, *Blakella*, *Angophora* and *Eucalyptus* because characters are irrelevant. Genera are then recognised solely according to lines of descent. But if the scenario should be nearer to my figure, and the differences between *Angophora* and the rest are much greater than those between the other three groups, then

the wisdom of recognising four genera is called into question. If, indeed, there were virtually no characters to separate three of the clades, then it would seem unwise to recognise more than two genera altogether. I have no views on whether there should actually be two genera, *Angophora* and *Eucalyptus*, or one genus *Eucalyptus*, or four genera, but I object to being told that the two-genus taxonomy is not a possibility simply because *Eucalyptus* would be paraphyletic. Such extreme views are currently creating nonsensical taxonomy in other cases where the scenario equates more with my Fig. 1.

So now, if my opponents are up for another little challenge, I invite them to say what they would do in the scenario of my Fig. 1. Let us say that the circles represent populations. There are virtually no characters to distinguish the open circles one from another (let us say that the characters are at best those on which closely related species might be based), whereas the dark circles differ from the open circles in characters normally treated as worthy of generic rank in the group concerned. The left hand eight terminal units are more closely related phylogenetically to the eight dark circles, but are morphologically much closer to the 16 right-hand terminal units. How should we treat this taxonomically if we cannot recognise paraphyletic taxa?

My opponents have asked "...what credibility is there in a few circles and interconnecting lines?" I am happy to express some sympathy with them on this, though I think these diagrams can have some relevance in making visual presentations of different scenarios. As Stuessy (1997) has said, there must be more to classification than just branching patterns of evolution. And when the branching patterns are cladograms and not even phylogenetic trees, the doubts become considerably greater. I commented in my 2002 paper (p. 38) that I think it is because many people think only in terms of cladograms that they cannot see what I regard as the absurdity of what they are saying. I would prefer to apply simple common sense. On pp. 34–35 I asked why it is that when evolution is creating greater and greater diversity, the cladistic view is that as it progresses we have to give lower and lower ranks to its products. I asked whether this is not a silly idea and invited answers (p. 35), an invitation I now extend again to my present opponents.

Those who argue for all taxa being monophyletic will say that this is necessary in order to make evolutionary deductions in fields such as phytogeography possible. But this is certainly not necessary. If one knows that a taxon is paraphyletic, one can group it with its derived or epiphyletic taxa to make a clade, or make one's deductions direct from a cladogram (Brummitt, 1996: 382). But it is not necessary to unite them into one taxon. There is a major difference between a phylogeny

and a classification, which convey different sorts of information, but the two concepts are often hopelessly confused.

If I have had any disappointment in my decade of defending paraphyletic taxa, it is that although literally hundreds of fellow taxonomists have told me that they agree with what I say, I have persuaded very few to say the same things in public. They are always too busy, or say that somebody else should do it, or that all the arguments have already been put forward. One comment which made me particularly sad came from a systematic botany graduate research school. Although everybody there secretly believed with everything I said in the 2002 paper, they all knew that if they said so publicly they would never get a job in systematics. However, Vicki Funk (2001), no stranger to cladistic theory, has questioned the use of monophyly as the Holy Grail of classification, seeing it as as one of three basic tenets of cladistics that are now in serious doubt. An excellent balanced comprehensive review of the arguments by Diggs & Lipscomb (2002) has argued for acceptance of paraphyletic taxa on both theoretical and practical grounds. And I have been privileged to see a copy of a text by J. Cullen & S. M. Walters, for publication hopefully in 2004, in which they say that the value of monophyly as a principle in classification has been shown to be zero (quoted here with permission). But until significantly more taxonomists publicly express such views, the journals will go on publishing taxonomies based on very shaky grounds. So here is another challenge—when is the silent majority in the taxonomic community going to raise its voice in favour of common sense?

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