Theoretical Issues and "Pattern Cladistics"

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Beatty (1982) makes two important points about a party of cladists he terms "pattern cladists." These are: (1) that by searching for characters that define a hierarchical pattern of groups those researchers reduce their groups to Aristotelian classes, and (2) that this position is not "theory-neutral" with regard to current evolutionary theory, but actively antagonistic, even though it does not advance counter-explanations of evolution. When submitted to close reading, however, Beatty's discussion of the implications does not produce the clarification he intended. An unfortunate result, since the issues raised are part of an ongoing discussion and need review. As it is, Beatty's treatment of this matter is not fully informed.

With respect to the first point above, Beatty, following Ghiselin (1966, 1969, 1974) and Hull (1976, 1978), argues that the class interpretation of species does not facilitate the application of evolutionary theory to the same, and adds the refinement that "unless we interpret species entirely genealogically, we are forced into the position that species cannot evolve with regard to their defining properties . . ." This conclusion follows from the nature of Aristotelian class-definition if by "evolve" we mean gradual drift rather than saltation. Let us suppose, for example, that we choose to model species-evolution by the gradual increase in frequency of new traits within a given population. Once we have chosen this as our model, we shall be frustrated by the cladistic practice of defining species by unique sets of characters, for the new species will then be present the moment even one organism possesses the defining traits and we need not talk of frequency at all. Beatty's most detailed discussion of this problem runs as follows. If we assume that species "B" is descended from species "A" where both species are identified by defining characters, then we must be able to say what "B" possesses that "A" does not. Whatever this property is, whether a single character or a set of characters, let us call it "x". Beatty continues (p. 26):

> What is perturbing about all this is our inability to account for the increase in frequency of the trait x, from the time when it was infrequent among members of A to the time when it was frequent in members of B. We cannot explain its increase in frequency in species B because, *by definition*, all or a high proportion of the members of B have x. But x also could not have increased to a high frequency among members of A; its role among the defining properties of B is to distinguish B from A, in which it does not occur frequently. At best we can say that x increased in frequency instantaneously as B was born, even though we don't believe it. Thus, it seems, the very selection of defining properties places constraints upon the traits whose evolutionary histories we can describe and understand. These constraints are not natural — not part of the way the world is — but are simply man made constraints upon what we can possibly know.

This argument is enough to show, according to its author, that the class definition creates a "mess." However, it was not the class definition that created the conflict above, but Beatty's insistence upon using the term "species" in two contradictory senses. If we speak of a species which remains itself while its morphology drifts — certain traits increase in frequency — then by definition we are not speaking of a morphologically defined group (defining characters). A population can fit this requirement, however, and Beatty models his species concept on populations. But since a population concept is not consistent with a morphological concept, the mixture of the two in one account creates a "mess" — i.e., a contradiction. This particular mess is logically generated and logically solved — we have but to remove one of the conflicting species definitions. Beatty argues that the one to be discarded should be the class definition, yet within the space of this argument his only criterion of choice seems to be its contradiction with the population definition. The argument looks to be nothing more than the discovery that the "pattern cladists" are guilty of disagreeing with established opinion, at least when taken by itself.

Beatty continues, however, to his second point, which will add a new aspect to the entire discussion. (In between he gives a historical account of the rise of pattern cladism which is best answered by the individuals he is speaking of, so I pass over it without comment.) The pattern cladists, according to Beatty, claim that their position is more "theory-neutral" than the positions from which they have departed, and treat this as a positive aspect, even a justification of their approach. But as Beatty's first argument has "demonstrated," these methods, which model species upon Aristotelian classes, *are not neutral at all* but are actually "*at odds* with current evolutionary theorizing." Now this looks like more than mere disagreement. If a position is justified through the claim that it is theory-neutral, but its implications are actually at odds with current theory, then the justification fails. This neutrality, Beatty remarks, is a "myth."

But if the approach cannot boast of neutrality, then it must show the same credentials that we ask from any other theory or research program: "All evaluations of research traditions and theories must be made *within a comparative context*. What matters is not, in some absolute sense, how effective and progressive a tradition or theory is, but, rather, how its effectiveness or progressiveness compares with its rivals" (Laudan, 1977:120). On the basis of this view, Beatty argues: "our best theories about patterns of nature are evolutionary theories. What non-evolutionary rivals better explain geographic distribution, the fossil record, developmental similarities and differences, as well as adaptive and nonadaptive similarities and differences?" When asked how they explain the observations listed above, however, the pattern cladists show little or no interest in doing so. Pattern cladism, they say, is not a theory of evolution, and does not need such explanations. Once the "myth" of "theory-neutrality" has been exploded, however, it is obvious that the position does have theoretical implications, and should be judged in comparison to the theories it contradicts. Yet how can such a judgment be made in default of explanations?

Beatty is aware of the standard rhetoric of his targets and is able to anticipate their reply. His own response (p. 31) argues that there is no way around the demand of theoretical comparison:

> The usual rejoinder to this sort of criticism is that classifications should be based on the world, not on theories. A system of classification based on evolutionary theory would tell us more about that theory than it would tell us about the world. This sort of reasoning has, I think, unreasonable appeal. In the first place, the rejoinder is irrelevant in this case. The question at issue here is not so much whether systematics should be theory neutral, but whether systematics should be theory antagonistic. In the second place, the rejoinder seems to suggest that we can build classifications on the basis of the world in the same manner that we build museums on the surface of the earth. We cannot build classifications "on the world," but only on what we know about the world. And what we purport to know about the world is contained in our best theories.

And so we are brought back again to the demand for comparison and judgment. If the pattern cladists are going to question our best explanations of the world, they must offer their counter-explanations. Since, as they admit, they have none, neither can they justify their question.

When putting this much into the mouths of others, it is prudent to check with them before going to print. The target that Beatty has cut up above was an obvious straw-man and the individuals associated with such a position would be naive indeed. But the “myth” of theory-neutrality that Beatty has exploded is a myth without any devotees. No member of the camp under attack has ever suggested that classification was without theoretical *implications* — those implications are part of the motivation for constructing classifications. What they have suggested, as Beatty himself admits (p. 33) in the last paragraph, is that cladistics “is supposedly better methodologically because it does not rely on the supposedly unfalsifiable assumptions of evolutionary biology. And it is supposedly better empirically because it does not require as many assumptions that might not, after all, be true.” This is more like it, but Beatty immediately obscures the issue by adding, in his next sentence: “But pattern cladistics is not, after all, evolutionarily neutral.” No, of course it is not, for if its results were without theoretical implications they would tell us nothing of interest. But the pattern cladists claim that while their results are at odds with some theories, their *method* is without theoretical pre-judgement. This is essentially the point made by Nelson and Platnick (1981:324) as they are quoted by Beatty:

> To state that a cladogram is a synapomorphy scheme invites the rejoinder that a cladogram must, therefore, be a phyletic concept. Not so, for by “synapomorphy” we mean “defining character” of an inclusive taxon. True, all defining characters, in the phyletic context, may be assumed to be evolutionary novelties. But making that assumption does not render it automatically true; nor does it change the characters, the observations on which the characters are based, or the structure of the branching diagram that expresses the general sense of the characters: i.e., that there exist certain inclusive taxa . . . that have defining characters.

The point being that if the cladogram is made necessary by the characters and the characters by observation, there seems very little in the way of theoretical assumption here.

And now we come to a turning point, because any attempt to bring Beatty’s argument into greater focus will result in collapse. The argument is salvageable only by putting aside cladistic *results* and pointing to the assumptions inherent in the *method*: namely, that homologies define hierarchies of groups in nature (Brady, 1982) and that these hierarchies are evidence of causal necessity (which would mean that they are “natural”). Since the second point here is an interpretation of the results and cannot prejudice the investigation, the question turns upon the first point. Are the pattern cladists justified in making the assumption that homologies define hierarchies of groups? This is the actual bone of contention. Let us see what happens when we examine Beatty’s discussion for enlightenment.

First of all, Beatty holds that the assumption is not justified; his argument that the constraints which follow from the use of defining characters “are not natural — not part of the way the world is — but simply man-made constraints” is clear on this point. The world is not that way, so the pattern cladists are not justified in treating the world as if it were. But the target has been missed, for since we are now talking about what is inherent in the *method* rather than interpretations of results (no one has claimed that the latter are “theory-neutral”), Beatty is not speaking of what differentiates the pattern cladists from other brands, but what unifies them. He gives Wiley as an example of a cladist more in line with current thought, but Wiley’s *method* of constructing cladograms is identical with that used by the pattern cladists — it is his interpretation of the results that differs. Hennig himself is also caught in the same net. If homologies do not define hierarchies of groups we can give up cladism altogether, and Beatty argues either: (1) that they do not, or (2) that if they do, the fact is not causally significant. (I am not certain which of these alternatives he means to advance.) If either one of these claims were to be accepted, there would be no basis for cladistics.

Defending now, not pattern cladism, but cladism in general, I would reiterate what has already been said elsewhere (Nelson and Platnick, 1981:165): if the hierarchies did not exist in nature, we should not expect independent workers to find them or to corroborate them with other patterns. In this sense the assumption above is merely heuristic and self-justifying. If Beatty cares to deny this argument he should speak to it. But until he explains how the *method* of cladism can create the appearance of hierarchy where there is none I shall have to assume that he is unable to do so. And once the hierarchy is admitted, the notion that such order could be accidental rather than causally significant is simply without foundation. *Any* general pattern in nature is causally significant at some level, and we need only find the proper level to understand the significance. I doubt that Beatty ever meant to deny this last point, but he has evidently allowed his own opinions to prejudice his judgment. After all, he has no way of demonstrating that such a pattern of Aristotelian groups does not occur in nature, but such was the force of his conviction that he was willing to assume a thesis for which he had no argument.

Returning to the pattern cladists, I am in some difficulty as to the nature of Beatty's complaint. Ostensibly the paper argues that the pattern cladists have gone wrong somewhere, but if the crucial point is the detection of hierarchical groups by synapomorphies, I cannot see that they differ in this practice from any other cladist. If, on the other hand, the complaint is simply that they refuse to interpret their results according to current theory, this undemocratic attitude on their part need not be injurious to the pursuit of science. A reluctance to interpret pattern beyond a description which traces its linkage to other patterns is not objectionable in itself. If may produce fewer claims than more interpretive approaches, but whatever *errors* may be incorporated here will also be found within other versions of cladistics, for they would be artifacts of a common method.

I have examined Beatty's discussion thus far without deciding whether his argument on the opposition between classes and individuals has been correctly applied to biological problems, for it would seem that the truth of this application is irrelevant. The position of any cladist must rest on the empirical claim that the hierarchy of groups is discovered in nature, and the argument that some hypotheses contradict this conclusion, even if correct, is immaterial. But that statement is made from the point of view of cladistics, and if the focus is now shifted, it will become apparent that Beatty's argument does have substantial consequences for other viewpoints. Two of these seem worthy of attention.

If Beatty is correct about the the opposition between current theories and cladistics, then the proponents of such theories must find a means to discredit the empirical claim that cladistic patterns are found in nature or explain how they can justify their own theories in the face of contradictory evidence. Those who suppose that nature actually contains very different groups should certainly worry about how cladists can go about finding patterns which are not there.

Whether he is correct or not, *agreement* with his position would seem to preclude the use of cladistic methods, if inconsistency is to be avoided. Beatty seems to defend cladists who are willing to interpret their results according to "evolutionary perspectives," but if the strategy of defining characters contradicts those perspectives — and my reading of his argument suggests this result — any attempt to combine this research strategy with those interpretations would build in a contradiction. We cannot have it both ways. Either the search for defining characters is in opposition to the theorized structure of nature or it is not. If Beatty thinks that it is, then he should admit that cladistics and current evolutionary theory are mutually exclusive. Given this conclusion, the pattern cladists, by discarding all such explanations, may have the only cladistic position which remains free from internal contradiction. Contrary to his intention, Beatty's argument seems to justify the position he attacks.

The ironic result of the last paragraph is an indication that the issues involved in Beatty's discussion were inadequately addressed. This is disappointing, since Beatty was quite correct in his view that these issues needed clarification. But these same issues have more implications than Beatty allows. A much fuller discussion will be necessary, and it seems almost presumptuous to treat the matter in such a truncated format. Future examinations will, I hope, spend more time in researching the position of the pattern cladists. Many of the implications I brought out above have already found articulation there, and this fact should be recognized by critics.

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