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Review

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algorithm is not appropriate for non-metric distance data. Immunological distances, and electrophoretic and DNA annealing data are often non-metric.

The last 3 chapters of the book, "Specimens and Curation," "Characters and Quantitative Character Analysis," and "Publication and the Rules of Nomenclature" seem to be out of place in relation to the theoretical approach presented in the first part of the book. However, the subjects are certainly applicable to systematics, and perhaps they belong in an introductory text of this nature. They contain very little that is special to the phylogenetic system and not applicable also to other approaches to systematics.

My disagreements with some of Wiley's ideas and statements are trivial in comparison to the strengths of the book. I think this volume is an extraordinarily timely and valuable contribution to systematics. It is exceptionally clearly written and easy to read, and will inevitably become a major reference for students of systematics.—*Larry E. Watrous, Division of Insects, Field Museum of Natural History, Chicago, IL 60605.*

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## II

It is extremely difficult to review a book when one disagrees with its basic, essential premise. We might imagine Milton Friedman attempting an honest review of the collected works of Lenin. My case is, perhaps, not so desperate; but it makes it very hard to find, and praise, points that are no doubt praiseworthy. It is likewise hard to avoid nit-picking in a work perceived to be seething with the offspring of one stem-nit. I refer there, of course, to the fact that the author is writing on phylogenetics and (naturally) operates from its basic premise: that not only can (some aspects of) phylogenies be reconstructed in cladograms, but we are also, as scientists, bound to make our classifications rigorously isomorphic with those cladograms. In fact, many of us continue to deny the obligation without feeling at all diminished as scientists. As lucidly discussed

by Martin (1981), we recognize a clear distinction, and only an indirect connection, between phylogenetic reconstruction and classification. As a member of that group, I shall shortly offer some restrained comment on the matter, and then try to forget it.

For the most part I must proceed by critique—which may seem churlish, but I am well known as a churl. In any case, I hope to criticize more fairly than has sometimes been the case in these columns. I shall select for attention a number of themes that should be of general interest, and also, just for fun, point out a few howlers. However, many points and topics will perforce be ignored because of, inter alia, lack of space or their current irrelevance. I am grateful that Wiley's critique of phenetics falls in the latter group. He relies heavily on "results" by Mickevich and Johnson (1976), Mickevich (1978), and Farris (1977; as empirically supported in his 1979a): studies that have all been discredited (Colless, 1980 and in press) to an extent that makes them, at best, very dubious evidence. I am therefore spared the task of defending a large thesis in a small arena.

Wiley's title proclaims from the outset his role as a (relatively) conservative disciple of Hennig (he explicitly rejects the term 'cladism,' and I shall be happy to humor him here). And if Hennig's "Grundzüge," along with his (1965) and (1966), constitute the Gospels of phylogenetics, then Wiley fills the role of Augustine, or Aquinas: analyzing, defining, elucidating, tracing implications, and at every possible point, *proving* that the doctrine is sound. His first six chapters are given to this task. The development is orderly, from definitions of basic terms; through a chapter on species and speciation (his best, I thought); then others on taxonomic and phylogenetic structure and ways of inferring same; to the theory and practice of drawing up classifications. The treatment is very thorough; no concept or term escapes scrutiny; indeed, I found myself recalling the old advertising slogan about the vacuum-cleaner that 'beats and sweeps as it cleans.' The misprints are rather too numerous (but I liked "staring point"; it started a hyperfantasy on what the late James Thurber might have built on it). There are also nonsenses such as "convergent similarities are actually very similar in structure" (p. 120); and mistakes such as 'meld' for (?) 'combine' (it means 'announce'). But despite the defects, these chapters constitute a most impressive exercise in advocacy. They also provide the most detailed exposition of phylogenetics yet available; although I would not say the clearest. Without taking sides, the rather simpler, and I think better written, exposition by Eldredge and Cracraft (1980) is a strong candidate for that title.

Of the remaining five chapters, Wiley gives one to belaboring the opposition, and one to biogeography, an area where detailed phyletic reconstructions are very useful (but are cladistic classifications really required?). His final three chapters provide a short training manual for budding taxonomists. Of

that section I shall simply say that it seems adequate and probably useful for course work; but somewhat out of place as an appendix to the earlier chapters. Many students will be unable, or not concerned, to read the first seven chapters critically; and there is some danger of their accepting Wiley's assertions as received Truth (which God forbid!). Indeed, there is a pretty obvious, evangelical flavor about the whole book. That does not damn it, of course; but it needs to be noted.

Another, related flavor that pervades that book stems, I guess, from a bad attack of philosophitis (caught, perhaps, on Central Park West). It is true enough that, as Sir Karl Popper said somewhere, "genuine philosophical problems are always rooted in urgent problems outside philosophy" (and John of Salisbury said it 800 years earlier: on philosophy, "a sterile science, unless it conceive without"). No doubt philosophers can find genuine and interesting problems in the methodology of systematics; but I am becoming increasingly unsure about what they can offer in return, to taxonomists *qua* taxonomists rather than lay philosophers. Be that as it may, Wiley asserts that a scientist should declare his philosophical bases; and, taking his own advice, he offers a black and white sketch of available philosophies, plus the rather touching admissions that Hempel and Popper are his "favorite philosophers," and that he prefers realism because "it assumes that there are real patterns to be discovered in nature which can be used to study real processes." Really? No doubt it beats pursuing phantoms, as (we are told later) all non-cladists do. Wiley does make an impressive job of presenting his arguments systematically. Definitions, assumptions, etc., are paraded, implications examined, and the one and only true conclusion demonstrated via impeccable deduction. But I must say that, in a proclaimed Popperian, it all smacks a little of scholastic essentialism. The trouble is, one has only to reject, say, his definition of "natural" and the enterprise is wasted. However, I suspect that the arguments are not directed at the sceptic; rather, they are intended to fish for souls amongst the uncommitted. That is no crime, of course; but what comes close to one is constantly to belabor the reader with the jargon term "level of universality," when on most occasions 'rank' would have sufficed! The peak is reached in p. 146: "synapomorphic at the level of universality of actinopterygian interrelationships," meaning 'synapomorphic within Actinopterygii'!

Granted this approach, one might also expect fewer lapses in common or garden logic. Let us note just a few:

(1) Cladogenesis, by definition on p. 8, can generate entities (infraspecific taxa) that are by definition (p. 7) not clades!

(2) Despite all the stress on definition, the much-used key-word "deme" is nowhere defined!

(3) Category switches are common; e.g., on p. 14, "a category is a class prefix name," but 29 lines down "categories are classes." They abound in Wi-

ley's usage of "character," which may denote (on p. 8 alone) a feature or a part or an attribute; as examples we find (p. 117 alone) "presence of neural arch" (attribute), "cycloid scales" (part), and "number of teeth" (variable), all called characters. Perhaps I am overpedantic, but I find these irritating.

(4) On p. 21 we find "*the* species is a naturally occurring group of . . . organisms" (my italics). I should have expected the taxon-category distinction to be more carefully respected, than in the word italicised! Indeed, I get the feeling that Wiley is confused on the matter. How else does one explain his remarks about the "basic difference" between the "taxon species" and the "category species" causing "problems in . . . classification" (I didn't, by the way, find the promised discussion of these in Chapter 6). Certainly, there is a category that we call 'species'; but there is no taxon that we call 'species'.

(5) On pp. 113 and 159, Wiley has terrible trouble with the notions of Type I and Type II error, not realizing that they do not apply when choosing among *alternatives*, such as trees. His descriptions of the two types on p. 159 are in fact identical! And on p. 113 he seems a little puzzled by the fact that Type I seems to imply Type II as well. With his subtypes, on p. 159, he finally gets it right, since a proposed synapomorphy can be accepted or rejected *simpliciter*.

Related to the foregoing is a feature, highly visible in this book, but also common in the recent literature on cladistics: the liberal, vague use of the Popperian buzz-word, 'hypothesis' (and cognates). To me (and, I think, Popper) the word denotes a statement that sets out a *possible state of affairs*, and generally one that someone is proposing as a candidate for actuality. According to the extent of its support (by evidence, or survival of testing or whatever criterion you favor), it may possess greater or lesser credibility; and its credibility is a *crucial appendage*. Likewise, I have long accepted that so-called "facts," "observations," etc. are best construed as hypotheses of high, sometimes incontrovertible credibility. Wiley, however, seems to regard it as legitimate to use 'hypothesis' ad lib without any reference to the associated degree of credibility. The consequent abuse of the term is best seen in his convoluted explanation of the process of testing. The genius of Hennig's notion, of demonstrating phylogeny through inferred synapomorphy, lay precisely in its limpid simplicity. It can be set out in a couple of paragraphs that a high school student could understand. Unfortunately, he introduced the term 'reciprocal illumination,' at best metaphorical, at worst meaning 'positive feedback' (Colless, 1969). It seems to have opened the way for those who could not or would not accept the classical logic (*sens. lat.*) of disciplines such as physics, and yearned for a nice, clean, separate logic of systematics. By building upon fairly naive versions of Popperian falsificationism, certain folk (Wiley included) have erected an elaborate, obscurantist structure which they assert (and seem-

ingly believe) accurately describes their processes of inference. I should hope not!

Let us look at this process, in Wiley's words: "the 'problem of homology' is broken by simply realizing that homologies can be treated as hypotheses which are tested by other hypotheses of homology and their associated phylogenetic hypotheses." Thus, "truth is approached asymptotically, . . . in a system of reciprocal illumination." Here is a good example of the ambiguous 'hypothesis' (and a utopian 'truth'!). For goodness sake, how can you test anything against a (mere) hypothesis? Surely, one must test against something that has, or approaches, the status of a "fact": i.e., if you wish, some preexisting hypothesis that has been *tested* and accepted as credible. If the hypothesis under test has as a logical consequence some such "fact," it passes the test and *may* grow in credibility at the expense of its competitors. But how far would a prosecutor get in a court of law if he sought to corroborate the hypothesis of X's guilt with a (mere) hypothesis that X had been at the scene of the crime? Admittedly, the analogy is not exact: what Wiley (and others) seem to be saying is that an hypothesis of homology is one (ultimately) of synapomorphy; such an hypothesis can be tested (rather weakly) by, principally, the out-group criterion; and surviving hypotheses that consistently imply the same phylogeny thus accumulate credibility for that phylogeny. It may thus become significantly more credible than one or more of its competitors. In a way, we have here a sort of miniature, Lakatosian "research programme," complete (I fear) with positive and negative heuristics. But I fail to discern any Big Deal, of hypotheses tested against hypotheses. Ultimately, one is just counting the score: the number of consistent hypotheses! One certainly cannot test an hypothesis of phylogeny by means of one of synapomorphy; the former would need to entail the latter, whereas precisely the reverse is true! Perhaps cladists might consider Rozeboom's (1980) reference to "the 'hypothetico-deductive' model of scientific inference, the pernicious vacuity of which diverts attention from the operational problems of theory adjudication."

Turning as promised to the 'basic premise,' the logical equivalence of cladogram and classification, I should point out that I have spent 35 years in full- or part-time taxonomy, and always in close association with projects in pure or applied entomology. Perhaps for that reason I see classifications as providing "a precise *lingua franca* that will be as independent as possible of conflicting theories"; as concerned with "the practical problems of establishing consistent professional communication" (the quotations are from Gray, 1980). Moreover, it seems true that our classifications are used mainly in everyday biology not (as some authors seem to believe) biogeography, and that is where they are most rigorously *tested*—not for their truth, but their utility. Users require an efficient labelling system for the pigeonholes in which they will file the facts about organisms; but not only that: they want a sys-

tem of names that is stable, economical, and maximally predictive. By 'predictive,' I do *not* imply that the system in any sense "contains" information; rather, it has *predictive import*. To place an organism in a taxon on the basis of a few "key" attributes can allow us to predict that it has certain other attributes, not yet observed and perhaps observable only with difficulty. The prediction falls short of certainty, but can have a useful degree of probability, and quite enough for a working hypothesis. Moreover, the taxa I have in mind are not just read off from a fully resolved phenogram. Rather, they are (in practice or principle) carefully, albeit intuitively, *abstracted*, from a phenogram, with due regard for robustness and the size of the moats that surround them in character space. From that viewpoint they are just as "real" as tables and chairs; and, for that matter, as "natural," in the perfectly respectable usage of Whewell (*teste* Ruse, 1979). For those who may be interested, I offer the finely honed, current classification of the Culicidae (mosquitoes) as a model for all other groups (and I've reason to suspect that the family is paraphyletic!).

I am, of course, aware that Wiley is a little old-fashioned: he urges that we *should* follow his principles, rather than those of the pheneticists or phylis (sensu Holmes, 1980). A more subtle school (*vide* e.g., Platnick, 1979; Farris, 1977, 1979), which we can safely call cladism, asserts that the virtues listed above (as those of a "good" classification) are quite precisely those generated in a "true" cladistic (phylogenetic) classification. Indeed, in the "transformed cladistics" of Platnick (1979) and Patterson (1980), evolutionary theory itself is scarcely relevant. However, of arguments offered to date (and *pace* Steve Farris), I find some specious and none compelling. I should add that although Wiley hews to the good Hennigian line on these matters, he is not unsympathetic to the "transformed" viewpoint. He just prefers to interpret "patterns of relationship . . . as phylogenies," so that they "can be used to test . . . postulated evolutionary mechanisms." I give him full marks for good intentions; but I find it all a bit overoptimistic. The credibility of a fully resolved, *purely* cladistic reconstruction (i.e., unsupported by, and perhaps opposed by, a parallel phenetic one) is just too fragile. In conflict with some "postulated mechanisms," I fear that the latter would survive and the cladogram would be sent back for revision!

Wiley nods to the transformed cladistics in, for instance, his remark (p. 22): (on order that exists in the world of organisms) "It is not necessary to understand the phenomena or process to recover the ordering." This recent insight of the cladists is extremely interesting; and it turns up elsewhere: e.g., Finch (1981) asserts that the "characteristic of interest" that we may look for in a data set is a "model-free concept." The problem is one of "*describing the data*" (his italics). The point (as I intend it, anyway) is not that the characteristic of interest—in this case a clustering pattern—be theory-free; just that it is gratuitous to inject the theory that we will

use to explain why it is of interest. If, for instance, we carefully draw our taxonomic patterns according to some presumed evolutionary ordering, then it becomes vacuous to "explain" those patterns as "due to" an evolutionary process. If, on the other hand, we read them as non-random clustering in character-space, or messages written in negentropy, or what you will, it is then open to us to interpret them in terms drawn from evolutionary theory. In such a fashion, transformed cladistics has come full circle to join phenetics, especially in the numerical area. And a group of pure pheneticists was there to meet them: the numerical psychologists who have for years been studying 'additive trees,' and who even invented the term 'loose clustering' (Sattath and Tversky, 1977) for drawing taxa off a Wagner tree! I take some pleasure in pointing out that years ago (Colless, 1969) I noted that Hennigian methods were akin to phenetics of the "positive matches only" kind. I now have a fantasy in which transformed cladists run against all the old problems and evolve, inexorably and convergently, to become indistinguishable from pheneticists! They might claim that, according to cladistic theory, they really weren't pheneticists; but could they prove it? What is more likely in the real world is, I think, a synthesis, including a thorough reexamination of phenetic dogma and some of its awful, unsolved problems: e.g., in unit characters and dependent characters. I would like to believe that "monothetic thinking" will not prevail; although the concept of a polythetic group is a pretty subtle one (Wiley, on pp. 79-82, gets it all wrong; but so did Sokal and Sneath!).

Let me conclude by asking several questions, as food for thought:

(1) As Wiley says, "In all phylogenetic analyses the beginning premise is that the group analyzed is, indeed, monophyletic." Otherwise a supposed out-group may be in fact an in-group, with horrendous results. One should, therefore, take one's study-group from some well-analyzed, more inclusive group. But where does all this end? It reminds one of that mythical cosmology, which had the world held up by an elephant, who stood on an elephant, who stood on another, and so on. But I can't see us here accepting the explanation that 'It's just elephants all the way down!'

(2) If phylogeny A is supported by an inferred synapomorphy involving the presence of two bristles instead of three at some position, plus another five synapomorphies of similar "weight"; and alternative phylogeny B is supported by a single synapomorphy in a fairly complex structure; how does one choose between them? And how is it that this question seems never to come up in cladistics?

(3) Cladists usually draw hypothetical cladograms as highly asymmetrical, "pectinate" dendrograms; perhaps because actual cladograms, inferred by Hennigian methods, tend strongly to show such asymmetry (and the much-touted device of "phyletic sequencing" would be useless otherwise). Of real cladograms used as examples by Wiley, 8 out

of 13 are completely pectinate. One can compute an index of asymmetry by summing, over  $n - 1$  forks, the difference in numbers of terminal descendants on each side of the fork, and then normalizing by dividing by  $(n(n - 3) + 1)/2$  (the score for complete asymmetry). For Wiley's Hennigian diagrams the average score is 0.75 (pulled down by a zero for Fig. 6.2). By contrast, his two non-Hennigian diagrams (Figs. 10.7 and 10.8) score 0.67 and 0.45. And, taking at random a paper by Fox et al. (1980), who infer phylogenies via phenograms, we find six figures scoring 0.24-0.88, average 0.47. To a numerical pheneticist highly asymmetrical dendrograms are a fairly well-understood phenomenon, reflecting group-size dependance in the clustering algorithm. What are they reflecting in Hennigian diagrams?

(4) What might happen if an honest cladist (and I'm sure there are some), while drawing up his argumentation scheme pondered the words of Galileo: "you may be sure that Pythagoras, long before he discovered the proof for which he sacrificed a hecatomb, was sure that the square on the side opposite to the right angle . . . was equal to the squares on the other two sides. The certainty of a conclusion assists not a little in the discovery of its proof."—Donald H. Colless, *Division of Entomology, CSIRO, P.O. Box 1700, Canberra City, Australia 2601*.

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**Evolution: Genesis and Revelations, with Readings from Empedocles to Wilson.**—C. Leon Harris. 1981. State University of New York Press, Albany. 339 pp. \$29.50 (cloth), \$9.95 (paper).

In this volume, C. Leon Harris has attempted to portray "the development of theories of the origin of species, emphasizing the creativity of the individual scientist under the influence of his culture" (p. 262). He has succeeded admirably, producing an account that from its dedication ("To my father and mother, and to all my other ancestors, whatever their species") to its final commentary ("Getting Bad Marx in Science") conveys more of the true spirit of inquiry than any writings on evolution I'm familiar with. Most undergraduate courses on evolution would benefit tremendously from its use as a supplementary text.

The book contains nine chapters on, respectively, the Biblical account of creation, the Greeks, Rome and the Middle Ages, the Renaissance, French pre-Darwinians, British pre-Darwinians, Darwin and Wallace, neo-Darwinism, and modern times; its scope is thus similar to the little-known history by M. and J. Gaudant (1971). Each chapter contains a lengthy introduction, some selections from the primary literature, and a brief final commentary. Surprisingly, the selections are often the least appealing pages. Partly this is because they are usually very short, and not always ideally indicative of the tenor of an individual's thought; Aristotle, for example, is represented by an anecdotal section of the *History of Animals* rather than the far more general *Parts of Animals*, Book I, and only Wallace's Linnean Society paper (published together with Darwin's), and no part of his fascinating 1855 piece, is included. But more often the original writings seem merely redundant after Harris' perceptive analyses of their contents and knowledgeable accounts of the history, philosophy, and biology of the times during which they were written.

Given the current tumult within evolutionary

theory, Harris has faced the difficult task of balancing an appreciation of the accomplishments of the past with sensitivity to their weaknesses, and he teeters a bit toward the conservative side. For example, although he is careful not to confute the inappropriately named "synthetic" theory with evolution as a phenomenon ("May my typing finger wither and drop off if I use the phrase 'the theory of evolution,'" p. 3), he provides a definition of evolution ("a change in the genetic composition of a population which may gradually lead to a transformation of the population from one species to another," p. 2) that can appear sufficiently broad only to the myopic gaze of a neo-Darwinian gradualist.

Harris does not fail, however, to discuss the deficiencies of neo-Darwinism. He is fierce in pursuing the tautology of Spencer's "survival of the fittest" in its modern population genetics context, rejecting the arguments that defining fitness in terms of "improved design" (Domning, 1978; Gould, 1977; Lewontin, 1978) or as "only a greater probability of surviving" (Ghiselin, 1969; Hull, 1969; Mills and Beatty, 1979; Ruse, 1971) avoids circularity: "The circumference is increased, but the circle is unbroken" (p. 191). Harris accepts Popper's use of falsifiability as a criterion of demarcation for science, and acknowledges that explanations invoking selection are so buffered by *ad hoc* assumptions as to be untestable in practice.

He attempts to justify retention of a Darwinian paradigm by elaborating his (1975) claim that natural selection is (and functions in neo-Darwinism as) an axiom rather than a testable theory. Although others (such as Cracraft, 1978) have reached similar assessments, Harris goes further in suggesting three criteria by which *both* axioms and theories can be judged: they must be canonical (i.e., they "must not directly contradict accepted facts," p. 229), productive, and "neat" (by which Harris means both broad in scope and simple in structure). The first criterion seems redundant for axioms, since any axiom that can be empirically contradicted is obviously testable. Harris argues that neo-Darwinism *has* been productive (not least "for the employment situation of biologists with mathematical inclinations," p. 229); indeed, he calls selection "An Axiom to Grind" (p. 188).

Whether or not one agrees with Harris in these judgements, the erudition he displays in the book demands respect; the breadth of literature handled in the 35 pages of footnotes (which I wish could have been interpolated into the main text) and 30 page bibliography testify that he has heeded his own advice (p. 53) and avoided the "inbreeding" of modern specialist science. Harris makes history come alive as few biologists have, not least through his attempts to show something of the spirit of the scientists he discusses. He reports, for example, that when one of Cuvier's students, dressed in a red costume complete with horns and hooves, burst into the paleontologist's bedchamber shouting "Wake up, thou man of catastrophes. I am the devil. I have