______, and M. C. BONDELLO. 1983. Effects of off-road vehicle noise on desert vertebrates. *In* R. H. Webb and H. G. Wilshire (eds.), Environmental Effects of Off-road Vehicles: Impacts and Management in Arid Regions, pp. 167–206. New York, Heidelberg, and Berlin: Springer.

BRAUTIGAM, A. 1991. Preliminary 1991 list of CITES Appendix II animal species subject to significant levels of trade. Species: Newsletter of the Species Survival Commission IUCN—The World Conservation Union 17:26–30.

BRINGSØE H. 1992. The adoption of the poison-arrow frogs of the genera *Dendrobates* and *Phyllobates* in Appendix II of CITES. Herpetol. Rev. 23:16–17.

BUSACK, S. D. 1974. Amphibians and reptiles imported into the United States. U.S. Dept. Interior, Fish and Wildlife Serv., Wildlife Leaflet 506: 36 pp.

DODD, C. K., JR. 1986. Importation of live snakes and snake products into the United States, 1977–1983. Herpetol. Rev. 17:76–79.

ENDERSON, J. 1992. Peregrines and the Endangered Species Act. The Peregrine Fund Newsl. 22:6–7.

FITZGERALD, L. A., J. M. CHANI, and O. E. DONADIO. 1991. *Tupinambis* lizards in Argentina: Implementing management of a traditionally exploited resource. *In* J. G. Robinson and K. H. Redford (eds.), Neotropical Wildlife Use and Conservation, pp. 303–316. Univ. Chicago Press.

GANS, C. 1992. The status of herpetology. *In* K. Adler (ed.), Herpetology: Current Research on the Biology of Amphibians and Reptiles, pp. 7–20. Proc. First World Cong. Herpetol. and Soc. Stud. Amph. Rept., Oxford, Ohio.

GREENE, H. W., and J. B. LOSOS. 1988. Systematics, natural history, and conservation. BioScience 38:458–462.

HEDGES, S. B., and R. THOMAS. 1992. The importance of systematic research in the conservation of amphibian and reptile populations. *In* J. A. Moreno (ed.), Status y Distribución de los Reptiles y Anfibios de la Región de Puerto Rico, pp. 56–61. Publ. Cientif. Misc. No. 1, Depto. Recursos Nat. Puerto Rico.

HILLIS, D. B., and J. S. FROST. 1985. Three new species of leopard frogs (*Rana pipiens* complex) from the Mexican Plateau. Occas. Pap. Mus. Nat. Hist. Univ. Kansas 117:14 pp.

INGRAM, G. 1991. The earliest record of the ?extinct platypus frog. Mem. Queensland Mus. 30:454.

KATZ, G. 1992. Mexican system faces test. Ecological, legal issues key in mayor's slaying. Dallas Morning News, Sunday, Feb. 2, 1992: IA, 10A.

MROSOVSKY, N. 1988. The CITES conservation circus. Nature 331:563.

WAKE, D. B., E. R. PIANKA, R. G. ZWEIFEL, G. B. RABB, H. C. DESSAUER, R. RUIBAL, G. W. NACE, and J. W. WRIGHT. 1975. Collections of preserved amphibians and reptiles in the United States. Misc. Publ. Soc. Study Amph. Rept. 3:22 pp.

FOOTNOTES

8. The plethora of overlapping U.S. state and federal wildlife laws, as well as visa policies, make it effectively impossible for Mexican nationals (as well as other non-U.S. citizens) to pursue collecting-based research in the United States.

9. Indeed, the state of California has opened state parks to off-road vehicles, providing that they remain on pre-existing trails, a restriction of dubious practical value.

10. The conclusion that this one specimen was the "last of its species," killed off by a scientific collector, would be almost certainly incorrect. El Salvador is well-known for its leadership in habitat destruction in Central America, an ongoing process well advanced in the region of the Hacienda Monte Cristo.

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Anonymous Reviews: A Response To Wilson And McCranie

Wilson and McCranie (1993) have provided a generally good series of arguments for and against the anonymous peer review system, and both authors certainly are familiar enough with the system to qualify as knowledgeable critics. However, I was surprised to read several of their opinions, qualified as "... incendiary...," against the system, especially #2 which alleges that "... the editor and such a [vindictive] reviewer form a secret pact against the author."

While my publications experience is not as extensive as that of the authors, I have served as editor of various publications series, have had peer-reviewed manuscripts and grant proposals accepted, and have had some rejected. All of the editors it has been my good fortune to have known regarded their role not as part of any pact, but rather as a sort of interface between prospective authors and science. Some even went so far as to be a publication facilitator for novices or persons not fluent in English. Only once, for a co-authored funding proposal, have I received a rejection based in large part upon the snide commentary of a review committee member who implied a former association with my department, and whose identity I can guess. Happily, the work is proceeding with very adequate and diverse corporate support, privately solicited; sometimes success is the best revenge!

I can accept the thought that there are editors who are less conscientious about their role, and it seems that Wilson and McCranie (and persons with whom they discussed their stance) have encountered them. After all, editors, like other scientists, are human first and scientists/editors second, with all the room for ego abuse and lack of character that implies. While locating persons qualified and willing to serve as editors is not at all easy, neither is it impossible. Wilson and McCranie's criticisms, therefore, seem less appropriate to the system than to publishers who fail to replace dishonest or vindictive editors. I write this with the full realization that an author may pin an additional target on their derriere by making such an accusation against an editor, but not to speak perpetuates injustice—ultimately, we're entitled to all the rights and justice we're willing to stand up and defend. And several authors leveling such charges at an abusive editor would be, I feel, a considerable threat to the status of a journal and hence to that of its publisher.

Selection of reviewers and disposition of manuscripts after review together constitute a unique art form. It definitely is time consuming, but I am not at all sure it would be more expedient for authors and reviewers to correspond directly—in fact, I can easily imagine this process requiring **more** time than the present system! Direct correspondence might enhance cooperation, but I wonder at what point a cooperative known reviewer recommending constructive revisions would feel they ought to be a co-author? The editor's time per manuscript would be reduced, but this interface is a necessary part of editorial functioning.

For an anonymous reviewer to use the system to suppress competition is to my mind utterly contemptible, and is another damning indictment of the bucks-grabbing, overhead-generating nature of much research-mill science. An editor receiving one poor review and two good ones clearly needs to examine critically their choice of reviewers as well as the science involved, and failure to do so is editorial cowardice. But again, this is addressable through the publisher. Yes, there are economic decisions involved, and it's certainly possible to keep costly journal pages filled with papers which have received unanimously good reviews. But does this approach best serve science? I doubt it, and I expect Wilson and McCranie would agree. Reading Bob Jaeger's example of "academic revenge" saddened me—it really did—but it did not surprise me. If that's the logic with which tenure decisions routinely are made everywhere, then academia ought to be about ready to implode. Why would a graduate student want to spend years developing in order to enter a system that routinely eats its young?

All of the editors interviewed by Wilson and McCranie seemed to agree (as do I) that the system is far from perfect, though it seems ultimately better than most alternatives. However, in the course of informal discussion with a mammalogy graduate student here (Mike Stokes, pers. comm.), I learned of an interesting and logical variant under discussion among KU's mammalogists (and, doubtless, elsewhere)—TOTALLY anonymous peer review, wherein the author is unknown to the reviewer, who remains anonymous to the author. This is easy for editors to implement, preserves the valuable parts of the present system, and reviewers who wish to identify themselves could continue to do so.

In conclusion, I thank Wilson and McCranie for initiating what I expect will be extensive, and I hope productive, discussion.

LITERATURE CITED

WILSON, L. D., and J. R. MCCRANIE. 1993. Anonymous reviews: Necessary convention or outmoded obscurantism? Herpetol. Rev. 24(4):137–138.

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Cladism: The Great Delusion

"What a lovely day. Let's go and dig up some nodes."
—What palaeontologists don't say.

"Although the thrills of novel thoughts are to be nurtured, and all students to be introduced to them, there is an inherent danger that should be recognized, namely that a new concept be not mistaken through the thrill it bestows as a sort of 'divine revelation' of THE TRUTH."—H. M. Smith (1972)

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Once again, traditional or evolutionary systematics has clashed with the "new science" of cladistics. Lazell (1992) has questioned the results of the application of the cladistic method to the classification of iguanine lizards (Frost and Etheridge 1989), and has been firmly rebuffed (Frost and Etheridge 1993).

But has he? Lazell may be a reactionary, but he is a very well-informed and thoughtful one; he deserved better than the patronizing "most people think as we do nowadays" condescension of Frost and Etheridge's reply. The truth is that cladism, while taking on the overtones of religious dogma at times amongst adherents who casually dismiss the wisdom of earlier great minds, is neither unanimously accepted amongst biologists, nor is it the "hard science" or "objective approach" that its supporters insist.

To the claim that cladism is the consensual philosophy of systematists of the nineties, I would observe that the many writings of S. J. Gould (a useful sample reprinted in Gould 1980) summarize some of the problems with the cladistic approach, and give examples of some of the implications of rigorous application of the rules of cladism (and I know of no cladist who *demands* less than rigorous application, though he may wisely stop short of *applying* such rigor). Gould has argued that not only does cladism force one to abandon such commonsense vernacular concepts as "fish," "zebras," or "apes," but it even prohibits one from considering dinosaurs to be reptiles unless one is also prepared to consider that birds are reptiles. Gould also notes that cladism relies upon inference and hypothesis, ignoring the fossil record almost entirely, not only in its details as regards the past history of particular groups, but also in its overall suggestions and hints as to how the evolutionary process has historically proceeded.

Foolish, too, would be the cladist who casually discounts the contributions to systematics and evolutionary biology of Ernst Mayr, who, in a recent updating of his 1969 work *Principles of Systematic Zoology* (Mayr and Ashlock 1991), went to some length (over sixty pages) to present a refutation of the cladistic philosophy, an approach that clearly disappointed an otherwise respectful cladist reviewer (de Queiroz 1992).

Other noteworthy doubters include the late A. d'A. Bellairs, probably the most prestigious British herpetologist of recent decades, who wrote in a book review (Bellairs 1989): "The emphasis is on characters of systematic interest rather than upon functional anatomy, a concomitant of the cladistic approach which will lead morphology into even greater disrepute than it currently 'enjoys' among other kinds of biologists. Furthermore, the work makes minimal concessions to the general herpetologist; for example, one looks in vain for any summary of the cladistic method or for a glossary of its wretched jargon. Elitism! One is just supposed to know, though one doubts if the subject is taught in any university in the United Kingdom."

More recently, the veteran herpetologist R. F. Laurent (1992) has presented a thoughtful essay on the shortcomings of cladism, noting some of the absurdities that can result from rigorous application of the Rules, and observing that any system that uses discontinuous categories (e.g., species) cannot accurately reflect true (presumably continuous) phylogeny. Other systems, he observed, are not truly phylogenetic either, but they do not pretend to be.

But I prefer not merely to appeal to inventories of prestigious nay-sayers and "magister dixit" arguments alone. I will try to categorize and analyze the problems with cladism in as objective a way as possible, keeping "ad hominem" arguments to the bare minimum (an ideal perhaps not achieved by Frost and Etheridge). Nonetheless, comments about practitioners of alternative approaches that can only be classified as "snide" do seem to be a stock-in-trade of cladists (note, e.g., the comments at the foot of page 99 in Farris 1990), so the reader is asked to forgive my occasional descent into a similar spirit in the arguments below.

The problems with cladism may be summarized as follows:

i) "Straw man" arguments that misrepresent the actual nature of competing approaches to systematics.

ii) The absence of new concepts behind the new and opaque jargon.

iii) Deliberate rejection of "hard" data, and incompatibility with the fossil record.

iv) The erroneous axiom that evolutionary history has been simple and overwhelmingly divergent (the principle of parsimony).

v) Subjectivity masquerading as objectivity throughout the discipline of cladistics, such subjectivity being evident in a) determination of character polarity; b) selection of sister groups; c) determination of which shared characters represent parallelism or convergence, and which represent evidence of common ancestry; d) arbitrary (or prejudicial) wording utilized to force complexly varying characters into "A or B" dichotomies.

vi) A concerted drive to assert with finality (or at least until the next hypothesis) that which one does not actually know.

These objections may be further elaborated as follows.

i) "Straw Man" arguments. Cladists (e.g., Frost and Etheridge) equate their procedure with the "recovery" of natural, phylogenetic groupings, and portray the primary opposing approach (evolutionary or traditional systematics) as defending absurdities such as the concept of the group that includes "my car and three geese." Yet traditional systematists have long sought to identify "natural" rather than "artificial" groups, primarily by examination of all aspects of the morphology (and fossil history) of a group