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Brooks' Response to Holmes and Price

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POINTS OF VIEW

Brooks' Response to Holmes and Price

Holmes and Price's (*Syst. Zool.*, 29(2), 1980) response to my views on the evolution of non-interactive parasite communities mostly missed the mark I intended to discuss. Therefore, I present this brief rejoinder indicating that I do not dispute the validity of their judgements, but neither do I consider them valid criticisms of my point.

It was gratifying to note that Holmes and Price were able to use the method I proposed successfully in the case I presented. I would suggest that a stringent application of parsimony criteria would have supported an interpretation that (1) one species exhibited co-speciation and the other was an invader, and (2) their present site segregation should be ascribed to competitive exclusion. That is a minor methodological point, and I applaud Holmes and Price for being perhaps the first ecologists to use phylogenetic analysis in their work.

The bulk of their response deals with exposing and demolishing two statements which they consider critical assumptions of my view. Recognizing the nature of this forum, I at first was inclined to ascribe their argument to an informed attempt to draw contrasts. Because any person reading both my view and their response could discover that the two critical assumptions did not appear in my paper, I initially intended not to respond. In the event that some readers would not recognize this fact and assume that Holmes and Price's rebuttal constituted valid criticism, I decided to try and determine from where the assumptions emanated.

I recently presented a discussion of coevolution (Brooks, *Syst. Zool.*, 28: 299-307, 1979) in which I differentiated degree of host-specificity (*co-accommodation*) from concomitant phylogenesis of hosts and parasites (*co-speciation*). I further presented an argument that the two phenomena were decoupled—one did not predict the other. In the course

of preparing this paper, I sent Holmes and Price reprints of my paper—I had cited it in the current manuscript. They did not cite that paper in their response. So, their critical assumptions attributed to me not only do not appear in the manuscript under discussion, they are directly contradicted by a paper I published less than a year earlier!

In short, I discussed *co-speciation* of parasites constituting a community: could it happen and what implications would that have for the synthetic theory of evolution? Holmes and Price responded by discussing *co-accommodation* among contemporary taxa. Had I espoused the two assumptions Holmes and Price attributed to me, their rebuttal would have been correct and justified. I never embraced those concepts and am violently opposed to them. On that score, Holmes and Price and I are in agreement.

So, from where did those assumptions come? I can only surmise, as I did in a letter to Holmes, that they were based on an assumption (contradicted by my coevolution paper) that I espoused some sort of resource-tracking model of coevolution. I espouse no model—I let no theory dictate my experience. As a phylogeneticist, I test hypotheses or theories against experience or observations. Thus, because the assumptions Holmes and Price present are refuted by experience, and because I do not and never have espoused them, Holmes and Price's argument is untrue and irrelevant.

If Holmes and Price define communities as ecological aggregations which are always in a state of flux, we have no point of common ground for discussion. If they espouse another definition, I refer them to the works of Croizat and others in biogeography and Boucat and others in paleontology for some idea that community structure may persist for long periods of time. If they really believe that all the systematic work in parasitol-

ogy has failed to produce evidence of co-speciation among members of parasite communities, I hope they will produce evidence of an empirical nature defending the view that more than half of the traits exhibited by parasites are homoplastic, giving false estimates of phylogenetic relationships.

In closing, I would like to state that Holmes and Price are correct that any model of coevolution based on narrow co-accommodation and on the notion that all components of parasite communities result from co-speciation is contradicted by

observation. Fortunately, I do not espouse either of those views.

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