a form of "nihilism." In the model we used, these principles are imbedded in the model's assumptions—among others, that speciation events are essentially instantaneous, that they are independent, even that speciation occurs at all. Any of the model's assumptions are open for inspection of how violations of them would affect our interpretation of species richness in sister clades. But null models should not be dismissed out of hand.

Null model assessment is increasingly recognized as a critical first step in analyses of species richness. Bond and Opell (1998; araneoid spiders), Pearson (1998; planktonic foraminifera, nannofossils, and graptoloids), Purvis et al. (1995; primates), and Wollenberg et al. (1996; columbine plants, cranes, and *Drosophila*) all have used a null model as a starting point to investigate patterns of species richness. Sometimes null models are sufficient to explain the diversity of sister groups (e.g., *Drosophila virilis* species group; Wollenberg et al. 1996). In other instances, they are not (e.g., cercopithecid primates; Purvis et al. 1995). Regardless, all of these authors have started with a null model analysis, recognizing, as did Slowinski and Guyer in their seminal paper (1989, p. 190), that "a random branching pattern *inherently produces sister taxa of disparate size*" (their emphasis).

We would be more enthusiastic had Olson mounted serious potential objections to our analysis: that the assumptions of the null model might be incorrect, that violations of these assumptions might seriously undermine our result, that the Sibley-Ahlquist phylogeny might be flawed and hence that we are making an inappropriate comparison, to name a few. But Olson mounts no such serious objections. Furthermore, we would be delighted to see a rigorous, compelling demonstration that the possession of an adaptation like nest-building capabilities causally explains passerine diversity. But that demonstration has not occurred. Far from consisting of "disheartened sorties into semantics and probability statistics," our analysis, we hope, has introduced an important consideration into the debate about passerine diversity.

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Pricing Biodiversity and Ecosystem Services

I agree with Marino Gatto and Giulio de Leo: It would be desirable to base estimates of the benefits of biodiversity and ecosystem services (environmental assessment) on the knowledge of an interdisciplinary team that could "investigate all possible environmental, social, and economic consequences of a proposed activity" (Gatto and de Leo 2000, p. 353). However, this ideal-sounding approach invites some practical questions. For example, how large and diverse should such an interdisciplinary team be? Also, is it possible for any team, however large and diverse, to investigate *all* the possible environmental, social, and economic consequences of an activity?

Eight years ago, my colleagues and I assessed the environmental and economic costs incurred for bird losses caused by pesticide use, taking into account the different value of birds to different observers. Our review (Pimentel et al. 1993) suggested that individual birds were valued at 40¢ per bird by bird watchers, $216 per bird by hunters, and $800 per bird based on the costs of replacement. We estimated the value of an adult bird, then, to be approximately $30. Some reviewers thought this amount too low, and others too high. Data in the literature suggest that about 67 million birds are killed in the field with pesticides, and this number does not even include the young birds that die in the nest because their parents are killed by pesticides or the young birds that were killed when they were fed pesticide-contaminated insects.

Gatto and de Leo say that a contingent valuation method—whereby respondents to a questionnaire state how much they would be willing to pay for some environmental resource—is the only pricing technique "capable of providing an estimate of existent values" (Gatto and de Leo 2000, p. 348). In my view, however, it would be a mistake to use willingness to pay to assess the value of birds killed by pesticides. The general public simply does not know how many birds are killed by pesticides, so any valuation based on the public's willingness to pay must be suspect.

Our papers (Pimentel et al. 1992, 1993) give details and sources and describe how we arrived at our estimates of the value of species and ecosystems. Scientists, decisionmakers, and the public are very much interested in these environmental and economic data, because this type of accounting gives them some idea of the magnitude of a particular ecological and economic problem. Certainly, we all hope that better data will be forthcoming, supplied through the type of interdisciplinary investigations suggested by the authors. But until those investigations are achieved, let us use current data to inform scientists, decisionmakers, and the public of the magnitude of ecosystem problems.

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Response from Gatto and De Leo:

Professor Pimentel's comments on our article provide us with a further opportunity to clarify our viewpoints on valuing biodiversity and ecosystem services. In particular, he raises four points that we would like to address.

First, he asks how large and diverse an interdisciplinary team should be to conduct an evaluation that meets the standards set forth in our article—that is, an evaluation that accounts for the full range of environmental, social, and economic consequences of an activity. The answer, simply put, is that the size of an interdisciplinary team depends on the importance of the proposed project, plan, or policy. The requirements for environmental impact assessments (EIAs) usually vary according to expected impacts, which can be determined roughly by looking at the size of the proposed project (as noted in European Directive 97/11/EC; European Union 1997) or by conducting a preliminary study (as called for in US regulations; Council on Environmental Quality 1978). Then, if a comprehensive study is deemed necessary, the so-called scoping process (Council on Environmental Quality 1978, Canter 1996, European Union 1997) will identify the critical social and environmental problems to be analyzed, depending on the vulnerability of the territory and the frequency, type, and magnitude of the potential impacts. The size of the interdisciplinary team and the types of expertise it should encompass are calibrated on the basis of these findings. According to Canter (1996, p. 50), "The number of members of an interdisciplinary team can vary from as few as 2 to perhaps as many as 8 or 10 individuals, depending on the size and complexity of the study. Typically a team comprises three to four members."

In any case, the question is not posed properly in the context of our article. The right question would be "Given a limited budget to evaluate a proposed activity, would it be better to spend the money on an interdisciplinary study that explicitly incorporates multiple evaluation criteria or on a cost–benefit analysis conducted by economic consultants who employ monetary pricing techniques?"

Suppose Professor Pimentel is granted $100,000 to assess whether, and in what amount, a new pesticide can be introduced into our environment. A first option would be for him to use those dollars to hire an interdisciplinary team consisting of, for example, an ecotoxicologist, an agronomist, and an economist, who will provide independent evaluations from three different perspectives: The ecotoxicologist will estimate how different amounts of pesticide per unit of crop area (measured, e.g., as kg of pesticide per square km) will translate into different increases of mortality (measured, e.g., as percentage mortality per year) in birds and mammals; the agronomist will suggest how these amounts translate into different increases of harvested biomass (measured, e.g., as tons per square km); and the economist will figure how the farmer's budget (measured as thousands dollars per square km) is affected. Then Professor Pimentel can use multicriteria analysis to assess the tradeoff between the monetary net benefit to the farmer and the mortality of birds and mammals, an analysis that will help the decisionmaker and the citizens reach an informed conclusion about how many kilograms of pesticide per square kilometer can be allowed.

A second option is to use the grant money to hire economists to prepare questionnaires asking some citizens the dollar value they attach to one unit of animal mortality. Because the economists calculate that a good deal of the grant money must go toward preparing, printing, and mailing the questionnaires—and toward paying themselves—they have to behave in the following way: They do not consult an agronomist, but make their own rough estimate of increased harvests based on previous similar case studies. They give a little money to an ecotoxicologist to conduct a study and estimate the pesticide effects on just one charismatic species of bird. Thus the questionnaire recipients learn nothing of the pesticide effects on mammals and other birds, and the returns in terms of increased crop production are not known precisely. The economists collect the questionnaires and, for each unit of pesticide sprayed per square kilometer, compute the difference between benefits and costs.

Even though the costs include only the noxious effects on one charismatic species, not the many other animal species that will undoubtedly be affected, and the farmer's production is estimated only imprecisely, Professor Pimentel would nonetheless calculate the amount of pesticide that corresponds to the maximum estimated net monetary benefit and communicate this figure to decisionmakers. Honestly, we believe that it would be better for the citizens and taxpayers for Professor Pimentel to use the first option rather than the second—that is, he should hire an interdisciplinary team and conduct a multicriteria analysis.

Second, Professor Pimentel wonders whether it is possible to investigate all the possible consequences of a proposed activity. We did not mean to imply that *all* the consequences could be investigated; what we meant was that the interdisciplinary team should do its best to forget no significant consequences of a proposed activity. How to ensure that nothing significant is omitted in environmental impact assessments (EIAs) has been the subject of much debate. The simple solution is to employ "checklists" for different categories of projects or plans (Canter 1996, pp. 86 ff). Checklists compiled on the basis of accumulated experience have been extensively reported in, for example, the *EIA Guidelines* published in 1995 by the European Union Centre for EIA in Manchester, United Kingdom (*www.art.man.ac.uk/eia/lf12.htm#lf12*). Of course, humans are fallible and cannot forecast everything, but we can try to do our best within reasonable constraints set by time and resources. One of the advantages of undertaking this effort is that the possible conflicts between environmentalists and developers over debated questions will emerge before a decision is taken, not afterward. Thus less time and money are spent on the whole decision process.

Third, Pimentel says we claim that contingent evaluation is the only technique capable of providing an estimate of existence values, and such techniques would be inappropriate for assessing, for instance, the value of birds killed by pesticides. Let us point out that we did not say a contingent evaluation method is the *only* technique capable of providing an estimate of existence values. We said that *among pricing techniques*, the contingent evaluation methods approach is considered by economists to be the only one capable of providing an estimate of existence values, and we added that other problems undermined the effectiveness of any pricing technique. Indeed, a good deal of our paper is devoted to convincing the reader that existence values cannot be consistently estimated via pricing techniques. Professor Pimentel's example on the value of the existence of birds (40¢ or $216 or $800 per bird) is a beautiful demonstration of this fact.

Fourth, until data collected through the type of interdisciplinary investigations we suggest are available, Professor Pimental comments, let us use current data—presumably gathered through traditional accounting techniques—to give the public some idea of the magnitude of ecosystem problems. This point seems to contradict Professor Pimentel's third point, that "it would be a mistake to use willingness to pay to assess the value of birds killed by pesticides," or, in general terms, to assess the existence value of a species. Why does Professor Pimentel claim, at the end of his letter, that these accounting exercises are useful, while in the previous paragraph he maintained that they are not? Moreover, it must be considered that even pricing techniques, if properly used (Portney 1994), require interdisciplinary investigation. In fact, respondents to questionnaires that are used to derive estimates of willingness to pay must be properly informed about the significant consequences of a proposed activity. Such information can be provided only after an investigation that explores the different social, environmental, technical, and economic aspects of the problem. As we said above, pricing techniques and EIA-like techniques must be compared *ceteris paribus*. There is no doubt that we must "use current data to inform scientists, decision-makers, and the public of the magnitude of ecosystems problems." But should we convey information on the magnitude of an ecosystem problem as a single number (net monetary benefit) or as a more inclusive array of information? We believe that the monetary solution is simplistically attractive but very misleading in most cases.

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