

LETTER FROM THE EDITOR

Advocacy dressed up as scientific critique

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While we certainly appreciate the concerns of Martin (2006) about the quality of papers published in our journal as well as the quality and integrity of our peer-review process, we would like to reassure readers that his concerns about our review process are unfounded, and in doing so mention also that concerns about quality of analyses are ubiquitous, regardless of the methods used. Martin's (2006) first objection to the Ramey *et al.* (2005) publication is with the perceived advocacy in the *absence of peer review*. While scientific objectivity in conservation biology, in particular, is difficult (many of us become biologists *because* of our love of nature), Martin is absolutely correct that the peer-review system should, among other things, guard against blatant advocacy, especially in the face of conflicting data. Yet unlike Martin's letter, the Ramey *et al.* (2005) paper was passed through a standard peer-review process. Indeed, the first submission was rejected due to two reviewers who were unconvinced by both the analyses and the data. Both reviewers, however, recommended that the authors be encouraged to resubmit if their concerns could be addressed. As the editor assigned to this paper, I concurred with this assessment and rejected the paper with encouragement to resubmit. The Ramey team then collected the additional data (microsatellite data as a nuclear complement to their mtDNA sequence data) and performed additional analyses as suggested by the reviewers, and resubmitted their paper. This revised draft went out for a second round of full review (a more rigorous standard than in many journals). I sent this paper back to one of the original reviewers (the harshest – as is my own personal policy) as well as a 'fresh' reviewer (which is also a standard of mine to make sure that the paper still has good flow and consistency). These reviewers were more favourable to this revised draft, but still had concerns which were subsequently addressed by the authors in a final submission. As the editor, I then examined the final paper and found that the concerns of the reviewers had been addressed. While we obviously cannot disclose the identity of the reviewers, they were, contrary to Martin's speculation, experts in population genetics and conservation genetics. Thus, the peer-review process of this paper was of at least standard rigour.

Martin offers two lines of evidence of 'advocacy' by Ramey. The first is citing Ramey as someone who 'is a self-proclaimed advocate for changing the Endangered Species Act'. I would argue that most conservation biologists would be in this same camp. There are many scientific difficulties

with the Act as it stands; however, opening up that for review opens up many doors for political input that could even further reduce the scientific input into such questions. Of course, as a journal, we do not solicit opinions about national conservation policy from authors before we make decisions on manuscripts. The decisions are based on peer review (as described above). Martin's second line of evidence is the following quote from Ramey *et al.* (2005): 'If defensible data are lacking and a protected organism is not distinguishable with a high degree of certainty from neighbouring, non-threatened relatives, considerable financial and logistical conservation effort may be misallocated at the expense of other endangered organisms.' In my view (as an alternative peer reviewer), this is not advocacy but straightforward common sense. Funds for endangered species are very limited. Why would you want to spend these precious resources on taxa that are originally based on weak data and do not hold up to scientific scrutiny. It seems as though Martin is advocating to ignore the science altogether whereas Ramey *et al.* (2005) clearly state 'If defensible data are lacking . . .'. Rather than making 'the task of maintaining biological diversity less expensive', Ramey seems simply to be arguing for those precious resources to be more carefully allocated to species of truly high need for conservation.

Finally, Martin critiques *his perception* of the 'peer review process orchestrated by the journal *Animal Conservation*' and rates it miserable. I am the one who 'orchestrated' the peer review along with what I thought (and still think) are standard peer-review practices (as described above). In addition to our own peer review, the Ramey work has also been extensively reviewed by 14 peers via the US Forest Service and these reviews are available online (<http://mountain-prairie.fws.gov/preble/>). As one can quickly see from Martin's critique, advocacy is alive and well in the science of conservation biology and advocates fall out on both sides of the issues. In this, as in many other cases, conclusions are debatable, and *Animal Conservation*, like other scientific journals, is the correct place for those scientific uncertainties to be expounded and debated. In this same issue, Ramey *et al.* respond to the scientific critique of their article. While I disagree with the recommendation of retraction, certainly standard scientific practice would be to strongly support Martin's recommendation to acquire independent replication of the study for the strongest conclusion. It is through the preponderance of evidence that these legal

issues are solved, not through some magical biological statistic.

References

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