



LETTERS

edited by Jennifer Sills

In Search of Peer Reviewers

AS A PAST EDITOR OF *MARINE MAMMAL SCIENCE* and a present associate editor of the *Journal of Mammalogy*, I have had great difficulty in lining up reviewers. Sometimes it takes 8 or 10 tries to find someone who will agree to review a paper. The typical excuse is "I'm too busy."

First I try the people who have published the most relevant and recent papers on the topic in question. Then I move down the range of choices. The temptation, and sometimes the need, is to turn to potential reviewers in less-related fields or those who are not so "busy" (i.e., are not producing much themselves). This inevitably leads to less-knowledgeable reviewers and often reviews of lesser quality, which of course complicates the editor's job and sometimes enrages the authors.

If an average acceptance rate of 50% is assumed, and if each paper needs at least two reviews, then each paper published represents at least four reviews. Following this logic, if you publish three or four papers a year, you should be doing at least 12 to 16 reviews. Anything less means that you are sloughing off the work to others who are perhaps less knowledgeable and capable than you in your specialty, and you should not be upset when someone reviewing a paper of yours "doesn't have a clue."

Doing a fair share of peer reviews should be a recognized and expected part of the job for scientific professionals; it should be written into the job descriptions of salaried scientists and be considered in evaluating junior faculty for tenure. The caution should be "Publish and review, or perish."

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A Peer Review How-To

AS A MEMBER OF THREE EDITORIAL BOARDS, author of 90-some scientific papers, and reviewer of over 900 manuscripts in the past 30 years, I have seen my share of scientific reviews. Reviews have become increasingly critical and demanding. This trend has doubtless contributed to high standards, but carried too far it can become counterpro-

ductive and frustrating for authors, editors, and reviewers alike.

A reviewer's chief responsibility is to advise the editors on whether a manuscript with suggested revisions would be acceptable for publication. Reviewers should highlight a paper's strengths and weaknesses, but they need not delineate strengths in very weak papers nor stress minor weaknesses in strong papers. Reviews should be prompt

and thorough and should avoid sharp language and invective.

Reviewers make two common mistakes. The first mistake is to reflexively demand that more be done. Do not require experiments beyond the scope of the paper, unless the scope is too narrow. Avoid demanding that further work apply new techniques and approaches, unless the approaches and techniques used are insufficient to support the conclusions. There is no need to require more tests of conclusions that have been demonstrated beyond reasonable doubt, and conversely, authors need not exclude every possible explanation for their results. Suggest an additional experiment, further analysis, or altered interpretation, but do not make publication contingent on these changes. If the conclusions cannot stand without additional work or if the evidence does not distinguish between reasonably likely alternatives, recommend that the editor reject the manuscript.

The second mistake often made by reviewers is failing to consider all of the journal's goals and requirements, including standards and guidelines stated in the editorial policy and gleaned from its articles. Do not reject a manuscript simply because its ideas are not original, if it offers the first strong evidence for an old but important idea. Do not reject a paper with a brilliant new idea simply because the evidence was not as comprehensive as could be imagined. Do not reject a paper simply because it is not of the highest significance, if it is beautifully executed and offers fresh ideas with strong evidence. Seek a balance among criteria in making a recommendation.

Finally, step back from your own scientific prejudices in order to judge each paper on its merits and in the context of the journal that has solicited your advice.

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Climate Change Goals: Where to Begin?

COLIN CHALLEN'S EDITORIAL "PLAYING CLIMATE change poker" (20 July 2007, p. 295) has a very apt title. As a British parliamentarian who chairs the All Party Parliamentary Climate Change Group, he obviously feels very good about the impressive goals set in the recent UK Draft Climate Change Bill. But he also has the equivalent of an ace in the hole which, like any good poker player, he carefully does not show.

This ace is the use of a 1990 baseline year for calculation of the targeted 60% reduction in British CO₂ production. The year 1990 was during the period when the UK was switching away from heavy reliance on coal. This switch occurred because of the increasing production of North Sea oil and because of Prime Minister Thatcher's aversion to the then-powerful and militant coal miner's union.

The UK is not alone in trying to take advantage of the vagaries of history by using 1990 as a baseline year. Germany's recent proposal for a 50% reduction in carbon dioxide emissions, presented by Prime Minister Merkel at the G8 meeting in early June, takes advantage of the 1989 German reunification that added highly inefficient eastern German industry to their 1990 baseline.

Rather than using a baseline of 17 years ago, the all-important goal-setting process to mitigate global climate effects should start with a clear understanding of where we are now and where we want to be in the future. Global climate change is too serious to be treated as a game. It is time to turn over every player's hole cards.

BERNARD D. GOLDSTEIN

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Response

BERNARD GOLDSTEIN HAS HIT UPON A VERY important point. It is where we want to be, not where we've been, that matters. We need to achieve a safe and sustainable concentration of greenhouse gases in the atmosphere,

and we should "backcast" all our calculations from that overriding objective and not obsess about 1990 as if that year had some magical quality.

Taking a baseline year does have one important quality: It provides an absolute that prevents the relativism of those politicians, notably in the administrations of the United States and China, of merely seeking to reduce the carbon intensity per unit of GDP. That is a recipe for emissions growth.

COLIN CHALLEN

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Beyond Bed Nets

DESPITE THE NOBLE EFFORTS OF ECONOMIST Jeffrey Sachs to address the malaria problems of Africa by supplying bed nets to 80% of the sleeping sites in the sub-Saharan ("Battling over bed nets," News Focus, 26 October 2007, p. 556), he might benefit from words written over 20 years ago by parasitologist J. D. Gillet: "The behaviour of the pathogens responsible for tropical disease and the behaviour of the hosts other than man are both studied in great detail, but the behaviour of man, the third component in these cycles of transmission, is for the most part totally and inexplicably disregarded" (1). Carefully monitored studies have shown that bed nets can be very effective against malaria (2), but will communities be able to maintain the 70 to 80% coverage level needed to have a dramatic effect on disease transmission? Condoms serve as an analogous strategy in the prevention of

Letters to the Editor

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AIDS; they are less penetrable to HIV than bed nets are to mosquitoes, and yet AIDS continues to spread. The behavior of man may once again be the missing factor in our calculations. We need bed nets, but what we need more is a long-term partnership with Africans that includes education, land reform, and the mutual development of local anti-malaria strategies.

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References

1. J. D. Gillet, *Trans. R. Soc. Trop. Med. Hyg.* **79**, 12 (1985).
2. C. Curtis, *Trends Parasitol.* **21**, 504 (2005).

TECHNICAL COMMENT ABSTRACTS

COMMENT ON "Protein Sequences from Mastodon and *Tyrannosaurus rex* Revealed by Mass Spectrometry"

Mike Buckley, Angela Walker, Simon Y. W. Ho, Yue Yang, Colin Smith, Peter Ashton, Jane Thomas Oates, Enrico Cappellini, Hannah Koon, Kirsty Penkman, Ben Elsworth, Dave Ashford, Caroline Solazzo, Phillip Andrews, John Strahler, Beth Shapiro, Peggy Ostrom, Hasand Gandhi, Webb Miller, Brian Raney, Maria Ines Zylber, M. Thomas P. Gilbert, Richard V. Prigodich, Michael Ryan, Kenneth F. Rijdsdijk, Anwar Janoo, Matthew J. Collins

We used authentication tests developed for ancient DNA to evaluate claims by Asara *et al.* (Reports, 13 April 2007, p. 280) of collagen peptide sequences recovered from mastodon and *Tyrannosaurus rex* fossils. Although the mastodon samples pass these tests, absence of amino acid composition data, lack of evidence for peptide deamidation, and association of $\alpha 1(I)$ collagen sequences with amphibians rather than birds suggest that *T. rex* does not.

Full text at www.sciencemag.org/cgi/content/full/319/5859/33c

RESPONSE TO COMMENT ON "Protein Sequences from Mastodon and *Tyrannosaurus rex* Revealed by Mass Spectrometry"

John M. Asara and Mary H. Schweitzer

We sequenced six endogenous collagen peptides from *Tyrannosaurus rex* bone fragments using mass spectrometry. Five sequences match birds, but only two match amphibians, supporting dinosaur-bird relationships. Buckley *et al.* reinterpret and misinterpret our data and question sequence authenticity, but they used a suboptimal phylogenetic algorithm to analyze only a subset of reported sequences and they suggest analyses that are less sensitive and less specific than mass spectrometry. We disagree and use data to explain.

Full text at www.sciencemag.org/cgi/content/full/319/5859/33d