

Other Methodological Problems Remain

Development of common reference materials resolves only some of the BC methodological uncertainties, however. Next, we must work together to analyze these materials. To achieve this goal, we propose a comparative analysis project using these reference materials to gauge how different methods can be used to interpret BC components in aerosol, soils, and sediments. The intention of the study is not to advocate a single technique; rather, such a comparative analysis will help immensely to better understand what is actually being determined by the different methods and how these results relate to one another. Researchers

of all disciplines are being actively solicited for this intercomparison. We are particularly interested in connecting with researchers who measure BC in the atmosphere with those who measure it in soils and sediments.

Participating research groups will be expected to make available all results and details of their methodology for eventual group publication. Distribution of standards will commence immediately, and all samples must be requested by January 2004. Data will be posted on a secure Web site by December 2004 for general discussion.

Further details can be found at the Web site: <http://www.geo.unizh.ch/phys/bc>.

The symposium, "New Approaches in Marine Organic Biogeochemistry," and the associated workshop were held 28–30 August 2003.

Reference

Schmidt, M. W. I., J. O. Skjemstad, C. I. Czimczik, B. Glaser, K. M. Prentice, Y. Gelinias, and T. A. J. Kuhlbusch, Comparative analysis of black carbon in soils, *Global Biogeochemical Cycles*, 15, 163–167, 2001.

—MICHAEL W. I. SCHMIDT, University of Zurich, Switzerland; CAROLINE A. MASIELLO, University of California-Santa Barbara and California Institute of Technology, Pasadena; and JAN O. SKJEMSTAD, CSIRO Land and Water, Adelaide, Australia

FORUM

From A. D. Kirwan, Jr.: More on Anonymous Reviews

PAGE 583

Judging by the considerable and varied response it generated, the letter by Myrl Beck in the 1 July 2003 issue calling for signed reviews touched a sensitive point with many AGU members. As succinctly noted by Joseph Walder (*Eos*, 23 September 2003), all of the evidence cited for abuse of the review process is anecdotal. Yet, the sheer volume and variety of the responses was surprising, at least to me. This suggests there are some broad editorial issues that appropriate AGU oversight committees might address. To further this, I offer my perspective as a former editor of the *Journal of Geophysical Research* (Oceans and Atmospheres) and *Nonlinear Processes in Geophysics*, and as current editor of the *International Journal of Engineering Science*.

We, as scientists, are distinguished from the chattering professions by use of the scientific method and by peer review of our research to ensure compliance to this principle. Peer-reviewed literature is the primary vehicle for the transition of basic research to users. Because of this pivotal role, peer review must meet the Caesar's

wife criterion: it not only must be proper, it must appear proper. Signed reviews do not meet this standard.

Despite the honorable intentions of John Goff (*Eos*, 23 September 2003) and others, this practice is easily corrupted by the unscrupulous and self-serving. Moreover, signing reviews is hard to defend to some in the chattering professions who claim that peer review in science is just another scheme for cronies to protect their turf, propagate "the same old same old," and that every good review should not go unrewarded. There are ample avenues for reviewers and authors to communicate informally. AGU and other scientific society meetings are one example. I do not understand why an editor, journal, or AGU needs to officially sanction this through the review process. Moreover, it is not clear to me how signed reviews address the fundamental complaint that some reviews communicated to contributors are personally abusive and otherwise not constructive. Isn't it the editor's responsibility to review the reviews and communicate those parts that need to be addressed for further

consideration of any contribution? Perhaps the real message in the *Eos* dialogue is that some editors are not doing this and that the screening process for editors needs to be reviewed.

David Fisher's suggestion (*Eos*, 30 September 2003) that the review process should be double-blind was made to me by Bob Garrels many decades ago when I was whining about the trials and tribulations of being a *Journal of Geophysical Research* editor. This is worth considering, however. Fisher's last paragraph suggests that with a double-blind system that reviewers might be tempted to pierce the cloak of anonymity by speculating about author identities. Under the present system some authors speculate about reviewers identities, especially when they feel the reviews are abusive and unconstructive. Such speculation is counterproductive, as my anecdotal experience illustrates. I have been confronted and severely rebuked four times by senior investigators (two are members of the U.S. National Academy) for hatchet reviews of their submissions. In fact, the confrontations were the first I learned of each of these papers.

In these cases, the potential damage to the review process was limited. But if reviewers elect to speculate about author identities, similar mistakes might affect objectivity of their reviews and the review process would suffer.

—A. D. KIRWAN, JR., University of Delaware, Newark

From D. J. Wesolowski: Preserving Anonymity in the Review Process

PAGE 583

To those scientists who haven't served as editors or associate editors of scientific journals, I can assure you that it's already too hard to find enough qualified reviewers willing to do the job without threatening them with exposure as well! So, if you want your papers to be published within a reasonable timeframe, you'll pretty much have to put up with anonymous reviews.

I've been an associate editor of *Geochimica et Cosmochimica Acta* for 11 years and an Editorial Board member of *Chemical Geology* for 4.

I think anonymous reviews are perfectly acceptable, but that the editor evaluating the reviews should always be identified, both to the authors and in the published manuscript. Reviewers should be permitted to request anonymity, but authors should always be instructed to specifically acknowledge in their manuscripts the contributions of those reviewers who do not request anonymity. This, in fact, might encourage more reviewers to identify themselves. I don't know about you, but it tickles me pink when I see my name in print!

The review process is intended to ensure that: the material is new or a useful summary

of previous work, the data and conclusions are correct or at least believable, proper credit is given to previous researchers, the subject matter and impact are appropriate for the target journal, and the presentation is readable and civil. How best to ensure these should be the only consideration in soliciting and evaluating reviews. Requiring reviewers to identify themselves to the authors is likely to force a more favorable review than is warranted or more likely result in the most suitable reviewers declining to comment. Anonymity certainly encourages vindictive or superficial reviews to be submitted, but it is the associate editor's job to weed these out. Furthermore, authors normally have the option of appealing a rejection to the editor-in-chief directly or options should always be made available.