

throughout India to obtain a better estimation of PGA value and attenuation relation; preparing a homogeneous and consistent catalogue of Indian earthquakes; and preparing seismic hazard maps incorporating soil characteristics, liquefaction potential, site response, and better attenuation relationships for corresponding regions.

A panel discussion on the future directions of geology, tectonics, and seismology in India was focused on monitoring seismicity and understanding the geologic context and tectonic history of major earthquakes in that country. Recommendations were made to stimulate comprehensive tectonic and structural model building and testing; initiate workshops on themes related to understanding intra-continental seismicity; establish a working group on intra-plate seismicity to assess the state of knowledge of the Indian shield and its intra-continental tectonic features in associated seismicity; and to conduct a comprehensive study of "associated events" during the earthquakes on the basis of thermal

anomalies associated with the Gujarat earthquake detected by infrared satellite data.

A panel discussion on natural and man-made hazards using space technology and international cooperation formulated different problems and themes related to natural hazards, and focused on India's cooperation with the international community toward disaster management. The panelists represented international agencies, and the following recommendations were made:

- Studies on earthquake "associated phenomena" and climate change need to be initiated at the international level.
- A more transparent system for exchange of satellite data needs to be created.
- Geomagnetic field satellites should be used to detect ionospheric current system fluctuations whose origin is not yet fully understood.
- Weather fluctuations associated with natural hazards need to be investigated.
- A joint Indo-U.S. satellite mission needs to be designed by the international community

in coordination with the current geomagnetic field satellite and those in the planning stages.

The workshop was supported by the ICSU Committee of Space Research and various agencies of the Indian government. Abstracts of the workshop are available online at <http://home.iitk.ac.in/~ramesh/>. Proceedings materials, which are currently being prepared, will include major publications. To reserve a copy, please send an e-mail message to Ramesh Singh ([ramesh@iitk.ac.in](mailto:ramesh@iitk.ac.in)).

The International Workshop on Earth System Processes Related to Gujarat Earthquake Using Space Technology was held 27–29 January 2003, at the Indian Institute of Technology in Kanpur.

—RAMESH P. SINGH, Indian Institute of Technology, Kanpur; and DIMITAR OUZOUNOV, Science Systems and Applications, Inc., Goddard Space Flight Center, Greenbelt, Md.

## FORUM

### Anonymous Reviews: Self-serving, Counterproductive, and Unacceptable

PAGE 249

Recently, I became involved in an incident in which a manuscript was rejected on the advice of two anonymous reviewers and an anonymous associate editor. This re-activated my long-standing disgust at the entire system of anonymous reviews and pushed me—finally—into doing something about it. A few weeks or months ago, I read a similar protest, somewhere—one much more persuasive than I am likely to write—but my high-mileage brain has misplaced its provenance. Consider this a "high-five" to that misplaced author.

The system of reviewing is supposed to filter out junk science and provide useful feedback to authors of non-junk science who have submitted work that can be improved. These are honest, commendable endeavors that can be accomplished quite comfortably out in the open. Concealment, on the other hand, permits

and invites all manner of dishonorable motives—not least of which is laziness—to creep in. Off-hand I can think of four reasons for remaining anonymous in a review, none valid.

1. You need to say something negative about a manuscript written by somebody in power over you. If you can't do so openly, you should of course refuse to do the review. Perhaps you should also look for another job. Early in my career I remained anonymous (the only time, I think) for this reason, and I have been ashamed of myself ever since.

2. You have just deep-sixed a paper by a bitter scientific rival and hope to get away with it. Again, obviously, you should have declined the review. A more courageous and useful alternative would be to do the review, suppressing your dislike of the conclusions, then argue politely for a different interpretation. But this is hard, and would take more time.

3. You have agreed to do a review, then find you have no time, or are uninterested in the topic. One way out is to do a slap-dash, superficial, inadequate job, then remain anonymous to protect your reputation. Far better to apologize to the editor and send it back.

4. A friend and/or respected colleague has submitted a paper that, in your judgment, is wrong and should be rejected. Clearly, if your

relationship with the author is such that a friendship would be ended by a negative review, you shouldn't be doing the review. However, having agreed to do it, the easy way out is to lambast the paper, then duck from sight. A far better course of action would be to return the paper to the editor unreviewed, then contact the author and explain your problems with his/her ideas.

The bottom line on reviewing in general is that we should all do it in such a way as to eliminate any incentive to stay anonymous. A good review is hard to do, and takes time. In particular, it takes great effort and ingenuity to write a negative review in such a way that the author is glad to get it. The problem with anonymous reviews is that they afford the opportunity to be rude, vindictive, and/or lazy—especially the latter. In my 40-odd years of writing papers for publication, I have received plenty of negative reviews, most of them ultimately useful. I don't like receiving negative reviews—who does?—but I can swallow my pride and work through a thoughtful negative review to the betterment of the paper. The only reviews I remember that left me permanently angry were anonymous.

There is far too much unpleasantness in the world already to needlessly introduce even a little bit more. Anonymous reviews are unnecessary, unacceptable, and should not be permitted.

—MYRL E. BECK, JR., Professor of Geology, Emeritus, Western Washington University, Bellingham

3. Specific suggestions were made for new and additional technology that should be evaluated and considered for inclusion in NCALM: ALSM wave-form digitization, single-photon signal level ALSM, color digital photography, hyper-spectral sensors, and shallow water bathymetry (integrated with the terrestrial ALSM unit).

4. A recommendation that funds be set aside for seed projects, rapid response, and other opportunities that would not need to pass through the NSF peer-review process.

5. A recommendation for a strong commitment to the development of open-source, public domain software, and to strong, Web-based access and analysis.

The proposed structure of NCALM, as presented in the UF-UCB proposal submitted to NSF is intentionally simple, modest, and user-oriented. The existing UF ALSM unit and supporting equipment and facilities will be used to collect data in areas selected through the competitive NSF grant review process. These data will be analyzed both at UF and UCB, and made available to the principal investigators (PIs) through an archiving and distribution center at UCB (building on the Berkeley Seismological Laboratory-Northern California Earthquake Data Center system). Both the UF and UCB groups will contribute to software development that will increase the processing speed and data accuracy. PIs will contact the UF group during proposal preparation to obtain guidance on cost estimates, scheduling, and related issues.

Once funded, PIs and their students will be able to participate in all phases of the work, and there is support for student involvement at both Florida and Berkeley. One of the primary goals of NCALM will be to assist graduate students in applying ALSM in their research, and the graduation of these students will contribute to the pool of personnel qualified to staff academic institutions, government agencies, and private companies.

NCALM will establish a strong, Web-based distribution system of data and software, and data will be made available to the general research community after an agreed-upon amount of time (most likely 2 years). The Web material will be an important part of the NCALM education and outreach component.

The nine-member steering committee will be used to prioritize schedules, review the center's financial management, form contacts with other programs, and review the potential for technological upgrade. The steering committee will meet twice a year. The primary contact person at the NCALM for NSF will be the elected steering committee chair.

The workshop community agreed that the duties of the initial steering committee should include writing detailed governance guidelines for NCALM, with specific procedures for electing members to the board. The following people agreed to serve on the initial steering committee: Collin Stark, Lamont-Doherty Earth Observatory of Columbia University; Bea Csatho, Ohio State University; David Tarbonton, Utah State University;

Mike Ellis, University of Memphis; Rudy Slingerland, Pennsylvania State University; Ramon Arrowsmith, Arizona State University; Don Carswell, Optech Inc.; William Dietrich, UCB; and Ramesh Shrestha, UF

The workshop participants recommended that some portion of the annual budget of the NCALM be set aside for "seed projects" that will enable PIs to get critical data to test ideas and develop major research proposals. They also emphasized that the NCALM should be able to provide rapid response to document the topographic effects of such occurrences as major earthquakes, large-scale flooding, or extensive landslides. In general, such opportunities may come along in a variety of ways, including partnering with groups in some projects that could provide valuable topographic data to the broad research community. It was suggested that something like 20% of the NCALM budget should be reserved for this purpose.

The Workshop to Discuss the National Center for Airborne Laser Mapping (NCALM) was held 24–26 April 2003, in Gainesville, Florida.

#### Acknowledgment

This workshop was made possible by a grant from the U.S. National Science Foundation, Division of Earth Sciences, Instrumentation and Facilities, Contract #EAR-0314680.

—BILL CARTER and RAMESH L. SHRESTHA, University of Florida, Gainesville; and BILL DIETRICH, University of California, Berkeley

## FORUM

### Comments on "Anonymous Reviewers"

#### From C.J. Robinove

PAGE 282

Myrl Beck's Forum article denouncing anonymous reviews (*Eos*, 1 July 2003) is right on the money. Perhaps he read my letter in *Applied Physics* in 1990 also denouncing anonymous reviews.

Some years ago, I received an anonymous review of a paper I had submitted for journal publication. The reviewer raised such interesting questions that I wanted to discuss them with him. I phoned the editor of the journal and asked if he would tell me the name of the reviewer. He politely declined, but when I told him I thought I recognized the handwriting of the reviewer and named him, he relented and said I was correct! I called the reviewer and he was generous enough to spend a wonderful hour on the phone with me discussing the paper. The paper was published with great consideration given to his ideas, much to its

betterment. Now *that's* a reviewer whose interest is in improving the paper and helping the author, not just showing how smart he is or slapping down a junior colleague. The AGU motto, "unselfish cooperation in research," can be well exemplified by those who wish to help rather than to tear down.

When I review a paper for a journal that insists on anonymous reviews, I always state in the review that I want the author to know I reviewed it. I am not ashamed of my knowledge or my lack of knowledge in a particular field, and I will comment only on the basis of my knowledge. I may question a part of a paper, but that is because I don't know the answer and believe that other readers may also be ignorant of that point. Reviewers have a duty to help the authors, help the advancement of the science, and help the journals publish useful papers. When you criticize the work of one of your students or of a scientist who works under your direction, you are not anonymous. Why should you be with others?

Unethical reviews should also be punished, if they can be identified. I wrote a paper that was reviewed in-house by one of my USGS colleagues. I made revisions based on his

helpful suggestions. The journal to which I submitted the paper asked him to review it. Instead of telling the editor that he had already done so, he did a new review with new ideas (which were not at all helpful). He should have declined to review the paper for the journal; and I, in turn, should have told the editor about that double-dealing.

Let's do away with anonymous reviews and take both the credit and the blame for our ideas.

—CHARLES J. ROBINOVE, U.S. Geological Survey (retired), Monument, Colo.

#### From R.E. Criss and A.M. Hofmeister

PAGE 282

We share many of the experiences and most of the sentiments relayed by Myrl Beck in his 1 July contribution to the *Eos* Forum, as well as those of a similar nature expressed by Alexander McBirney in his March 2003 commentary in *GSA Today*. We are in fact delighted that senior scientists are speaking up about the unsatisfactory nature of anonymous reviews. However, we believe they understate the problems, partly because the situation is worsening with time. Moreover, the brunt of

such problems is disproportionately felt not by emeritus professors but by young scientists, women, and minorities, and this is the crux of the issue.

This year, we have, like Beck, received rejections based on comments by two anonymous reviewers and an anonymous associate editor. We have also received rejections from anonymous associate editors based on a single constructive review requesting minor revision, along with an anonymous hostile "review" that could have been written about any manuscript on any topic by any author. More common than these are rejections from identified associate editors based on one or two anonymous reviews, which more often than not err on most of the points made, and in two cases dispute work that resulted in Nobel Prizes. We have examples this year of each type where the senior authors are recent Ph.D.s, whose vulnerabilities underscore the reprehensible nature of this "process."

The unsatisfactory nature of anonymous reviews looms even larger in the proposal evaluation process. Here, the applicant must run a gauntlet of as many as eight anonymous reviews, in a situation where he will generally receive a declination given a single, baseless

"good" review in the company of otherwise uniformly "excellent" and "very good" reviews. The only way to succeed in such a system is to supply large lists of potential "reviewers" (i.e., friends) and equally massive lists of unacceptable "reviewers" (i.e., real or imagined enemies), and hope that the program director will follow your suggestions. In other words, the process has become political rather than scientific. A major problem is that neither popular, majority, nor biased opinion is the stuff of scientific discovery.

We sign our reviews whenever permitted; but of recent date, no more than 20% of the reviews we receive are signed. We are thus certain that signing our reviews places us at a disadvantage in a competitive, increasingly secretive environment. No one is safe though, as retaliation can be directed toward anyone who is imagined to have authored a hostile anonymous review in an earlier round. We believe that trust in the system will continue to erode, as senior scientists who experienced fairness for much of their careers are replaced by younger scientists who at least benefitted from a constructive review process in their early careers, to new Ph.D.s who experience for their first, treasured papers an unfair

process where vituperative, baseless, anonymous reviews constitute blocks of obvious effectiveness. The problem may be equally serious for females and minorities. For example, in our small sampling, the frequency of signed reviews received by Anne is about half that for Bob, despite our similar ages, education, and stature in our respective specialties.

The remedy has long been evident—reviews must be either signed or discounted. No honest, well-intentioned person requires a cloak of secrecy—the costume of crooks. Honest people, young people, women, minorities, original thinkers, and true professionals are all hugely disadvantaged by a process where anonymity is advantageous. So why do we have one?

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—ROBERT E. CRISS and ANNE M. HOFMEISTER, Washington University, St. Louis, Mo.

## BOOK REVIEW

### Uncertain Science... Uncertain World



**HENRY N. POLLACK**

Cambridge University Press, U.K.; ISBN: 0521781884; 256 pp.; 2003; \$28.

PAGES 285–286

Why doesn't society act in the face of overwhelming evidence that human influences are changing the Earth's climate? Henry Pollack devotes his book, *Uncertain Science... Uncertain World*, to addressing one important reason. Much about climate science is highly uncertain, he argues; and as a general rule, most well-educated citizens don't understand the nature of scientific uncertainty. Thus, they have difficulty understanding competing claims made by advocates in the policy debate and in internalizing the extent of the problem.

A distinguished professor of geophysics at the University of Michigan, as well as a participant in many public debates involving science, Pollack aims to help lay readers understand the role of uncertainty in science. "In making comparisons and analogies with uncertainties that exist in science and everyday life," he writes, "my goal is to help readers to understand and accommodate scientific uncertainties in much the same way that they deal with other uncertainties in life."

Pollack does an admirable job of achieving his goal. He explains technical topics clearly

and purposefully, often relating them to familiar examples. How do scientists really know the length of an object? The book describes the history of standard lengths for the meter, first defined as the length of the king's body parts, to today's definition based on the speed of light. But even counting is not always straightforward, as Pollack exemplifies with the challenges of providing an accurate census of the U.S. population, or determining the winning margin in a very close election. He provides a masterful description of how scientists use probability to describe the accuracy of their data and measurements.

People often do not understand how opinion polls that sample only a few thousand voters can accurately represent the views of an electorate of tens of millions. Pollack explains how. He builds on these examples to provide a lucid description of the Bayesian inference methods used in his own research on the heat flows through the crust of the Earth, culminating in an elegant summary of the evidence that human actions are changing the climate, and of what scientists know and do not know about the potential impacts.

Throughout the book, Pollack emphasizes that examples of uncertainty in science are not anomalies or imperfections. Rather, uncertainty is a ubiquitous strength inherent in the endeavor. Only ideology holds definite truths. In contrast, science aims to separate the probably true from the definitely false.

Uncertainty helps drive the process forward. Science suffers when practitioners too certain

of their truth fail to question and test their conceptual models. Pollack provides a sensible overview of how people should and often do make decisions in the face of uncertainty. Predictions are often wrong, so people should develop long-term plans but prepare for many mid-course corrections along the way. To premise action on certainty is an implicit argument for inaction, since science—and little else we know about the world—is ever certain.

In contrast to his masterful summaries of scientific literature, Pollack uses vignettes to buttress his discussions of how individuals, businesses, and governments routinely make successful decisions in the face of uncertainty. But the vignettes are often wonderful. For instance, he reports on a study of the characteristics of successful—that is, surviving—World War II submarine captains. Those trained in geology and economics (then a much less theoretical discipline than today) performed better than those trained in mathematics and physics. The reason? Those with a low tolerance for ambiguity failed to act before the depth charges began falling.

A misunderstanding of scientific uncertainty is, however, only one reason society's actions against climate change often fall short. In many areas of human endeavor, decision-makers understand the uncertainties, but nonetheless lack the capability to integrate them fully into their decisions. For instance, national leaders generally understand the huge uncertainties inherent in intelligence estimates related to the war on terrorism. Legislators know from frequent experience that forecasts of future spending and tax revenues are rarely correct. Nonetheless, policy-makers often feel compelled to argue strongly and act more certain than they know they are to advance their policy

km requires its base to rotate with a linear velocity greater than 30,000 km/hr! We see footprints of fast-moving dinosaurs, but where are the footprints of these supersonic whirlpools?

*Grand Canyon: A Different View* is an example of a new, slick strategy by Biblical literalists to proselytize using a beautifully illustrated, multi-authored book about a world-famous, spectacular locality. Allowing the sale of this book within the national park was an unfortunate decision. In the minds of some buyers, this could imply National Park Service approval of young Earth creationists and their religious proselytizing. I believe that the continued sale of this book within the National Park would

undermine the work of the National Park Service interpreters who work so hard to educate the public.

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—WILFRED A. ELDERS, University of California, Riverside

## Comments on “Anonymous Reviews”

PAGE 384

From Robert J. Geller

### No Anonymity for Associate Editors

In an ideal world, I would agree with the recent letters to *Eos* by M. Beck (1 July), C. J. Robinove (29 July), and R. E. Criss, and A. M. Hofmeister (29 July) calling for all reviews by referees and associate editors (AEs) to be signed. But in an ideal world, they, as well as the rest of us, would

invariably be fined for petty infractions such as exceeding the speed limit by even 1 km/hr, so it might not be much fun to live there. Human nature being what it is, offering anonymity to referees is probably necessary to ensure the smooth operation of journals.

The above correspondents did not distinguish between the problems created by anonymous referees and those created by anonymous AEs, but I believe this is an important distinction. The AGU publication guidelines state that the editor is the sole judge of what is accepted

and what is rejected, while all other opinions are advisory. This is fine in theory, but may not always reflect reality. When there is, say, only one editor assisted by a team of 20 to 30 AEs, as a practical matter, the AEs are the real decision-makers and the editor cannot possibly find the time to study each manuscript carefully. This reality should be reflected by having the AEs sign their reviews without exception, so that the identity of the actual decision-maker is made clear in every case.

—ROBERT J. GELLER, Tokyo University, Japan (Editor, GRL, 1993–1997)

From John A. Goff

### An Editor's View of Anonymous Reviews

PAGE 384

I have read with great interest the recent Forum commentaries in *Eos* by Myrl Beck, Charles Robinove, Robert Criss, and Anne Hofmeister regarding anonymous reviews. I heartily support their position that anonymous reviews should be avoided. I have not written an anonymous review in ages (and regret the few that I did), and have always appreciated and respected greatly anyone who signs a critical review of one of my own papers. However, I would like to add some perspective from the editorial standpoint. I have served as JGR associate editor for 3 years (never anonymously!), and as *Eos* editor for seismology and tectonophysics for 4.

Over the years, I have rejected a fair number of papers, most of those based on anonymous reviews (fortunately, none of the above commentators was one of them). The vast majority of anonymous reviews I received were well

considered. While I would wish that all reviews were signed, I don't think we can summarily dismiss the fear that many would have of enmity and reprisal over a critical review. Some of these fears are likely justified. On more than one occasion, have I witnessed overly aggressive responses on the part of authors to anonymous reviews that I considered to be entirely fair and constructive in their criticisms. I do think we need to do all we can to discourage anonymous reviews, but it will be difficult to completely remove that choice from the process.

I put the blame squarely on the editor when anonymous reviews go bad. “Cheap shot” reviews can be spotted a mile away, and any editor worth their position will discount such reviews in making their decision. An editor who rejects a paper must be fully accountable for that decision—they must be able to defend their position to the authors and be willing to fairly consider any rebuttal that the author may offer in their defense. For this reason, I consider JGR's practice of insulating associate editors from authors to be unsupportable. Officially, it is the head editor of JGR that makes the decisions, but this is creative fiction. In

truth, the associate editor picks the reviewers and makes the recommendation that is eventually accepted by the head editor. The associate editor should be identified to the authors and be available to hear their concerns.

I concur with the other commentators that anonymity in the review process is, for the most part, counterproductive. Our cultural inertia has gotten the best of us and it's time to change course. I would recommend two approaches to get us started. First, AGU journals should begin actively encouraging the signing of reviews. I think many folks simply remain anonymous out of habit, and a simple nudge will get them to change it. Then, as each of us receives more signed reviews, the more likely we will be to sign our own reviews. Second, I think that a somewhat experimental journal like G3 might experiment with requiring reviewers to identify themselves if they agree to do the review. Such an author-friendly policy would likely bring in extra submissions, but would it hamper the review process? I don't know, but we ought to find out.

—JOHN A. GOFF, Institute for Geophysics, University of Texas, Austin

From Joseph S. Walder

### Anecdotal Information is Insufficient to Claim...

PAGE 384

I have read the recent Forum commentaries describing disgruntlement with particular anonymous reviews (1 July 2003 and 29 July 2003 issues), and nodded sympathetically. After all, who among us has not felt, at one

time or another, that a reviewer badly misunderstood our contribution to the scientific literature or our grant proposal? But each of these recent Forum contributors in fact committed the elementary logical error of invoking anecdotal evidence to prove a point. I doubt that any of these correspondents would accept *scientific* claims based on anecdotes. Yet somehow they believe that anecdotes form a sufficient basis for claiming that anonymous reviewing is inherently a nasty business. These correspondents further employed the dubious

rhetorical device of contrasting their own self-defined, high ethical standards with those of their adversaries, whom they variously described as rude, hostile, vindictive, lazy, cowardly, selfish, bigoted against women and minorities, and attired in “the costume of crooks.” The parallel with the discourse of political argument is hard to miss.

Reviews should obviously be fair-minded and professional, and decisions by editors and funding agencies should be as transparent as possible: on these points, I most certainly

agree with the Forum correspondents. By all means, let there be a debate on the proposition that anonymous reviewing and editing should be abandoned. As a starting point, I urge AGU journals to reverse their practice of letting associate editors remain anonymous. Anyone who chooses to serve as an associate editor needs to recognize that he or she has effectively become the gateway through which

every author must pass en route to publication. This privilege carries with it responsibilities. One is to read every manuscript carefully; a second is to scrutinize all reviews, discard those that are unfair or inept, and solicit additional reviews as needed; a third should be a willingness to directly engage authors.

I also suggest that AGU journals take the obvious step of asking reviewers why they

choose, in some cases, to remain anonymous. This would provide us with data: fuzzy data, perhaps, but data nonetheless. And finally, let's have a debate that does not rely on logical fallacies and rhetorical tricks.

—JOSEPH S. WALDER, Cascades Volcano Observatory, U.S. Geological Survey, Vancouver, Wash.

# THE BOOKSHELF

## Earth Science in the City: A Reader

PAGE 378

*More than half of Earth's population lives in cities today, a figure that is projected to grow to nearly 5 billion in the coming quarter-century. A new AGU book edited by Grant Heiken, Robert Fakundiny, and John Sutter, Earth Science in the City: A Reader, suggests that cities are becoming increasingly coupled with and vulnerable to their environment. The book explores the interrelationship between natural processes and the man-made urban environment, and reports on research examining the effects on urban residents and their surroundings. Included are papers looking at water use, environmental sustainability, hazard mitigation, and atmospheric sciences that propose integrated solutions to city planning, population growth, and policy decisions.*

*In this issue, Eos talks with lead editor Grant Heiken. Heiken recently retired from the Earth and Environmental Science divisions at Los Alamos National Laboratory in New Mexico.*

**Eos:** How would a city that is run with a scientific outlook differ from cities today?

**Heiken:** For the purposes of planning, day-to-day management, and emergency response, a comprehensive, dynamic view of a city should be required. Some of the book's recommendations include creating Geographic Information System (GIS) databases to establish a framework for numerical models of all of the natural and infrastructure systems that comprise a city. Some of the areas that could use scientific analyses are water and air quality, energy resources, building materials, the fate and transport of chemicals from pollution or chemical/biological weapons attack, and understanding urban micro-environments as incubators of disease.

In addition, it is important that the traditional walls between disciplines be broken down to understand the interconnectivities between this complex "system of systems" that is a city. Many of the components include prediction, atmospheric sciences, Earth sciences, and even space weather. As things are now, nearly every city department across the nation is

balkanized and doesn't realize the interconnectivities until there is a disaster. At the city level there should, for example, be a team of scientists, engineers, economists, and sociologists, or at least liaisons between city departments.

**Eos:** What changes in academia, government, and professional settings would be needed to address the urban area of the 21st century?

**Heiken:** I think of this type of scientific study as a new field that I refer to as urban science. Urban science must become an important part of the university culture; more than three-quarters of the U.S. population lives in towns or cities. Universities must also do more to create collaborations between departments and offer multidisciplinary courses on various aspects of urban sciences to train pioneers that have new ways of managing a city. Also, we need more sessions on urban issues at professional meetings among academics and geoscience organizations.

We are seeing some efforts, like the Central Arizona-Phoenix Long-Term Ecological Research project at Arizona State University (part of a National Science Foundation project to analyze geoscience in metropolitan areas), but we need more cross-departmental research on the urban environment. We need scientists interested in urban issues to present papers at meetings of organizations like the National League of Cities. The scientific community needs to further embrace urban systems as an important and credible field of research, in part by promoting study to train a new generation of science-based urban planners. None of these changes will be easy, but we need to recognize that applying science and technology to the urban condition is mandatory thinking as we move into the future.

**Eos:** The book suggests that most cities are not sustainable under their current design. How might urban science help remedy the future consequences of current plans?

**Heiken:** To allow sustainable growth in the world's cities, city managers need to integrate Earth sciences into their thinking. In many cities, for example, there is no holistic understanding of even the most basic aspects of their water systems. Long-term research on natural hazard risk and mitigation, if implemented, can benefit a

city. For example, Robert Leggett, the foremost expert on urban geology in the 20th century, emphasized that the natural setting of a city is its foundation. In the past, most urban planning decisions were made with little or no regard for the role of the natural setting in the city's long-term health and stability. Geology has been shown to have a huge impact on urban management, ranging from new construction to infrastructure development. GIS has revolutionized how cities are viewed and managed, including new remote-sensing techniques that have made city planning much easier.

From my perspective, cities with similar environments could benefit from a regular exchange of ideas and management policies, including cities on floodplains or those in arid regions. The volcanological community, for example, has encouraged this sort of exchange with regular interdisciplinary meetings that involve many fields and include city managers and politicians.

**Eos:** Do you think that city bureaucracies and politicians would willingly share power and responsibility for urban planning with scientists?

**Heiken:** If a strong case can be made for the value of science to a city administration, a few cities will give it a try. If, after a decade or so, these cities demonstrate success in creating a more sustainable environment for their residents and an improved economy, then more cities will follow their example. The biggest difficulties facing integrated urban science are "turf battles" between city departments and worries about the legal aspects of planning and prediction.

This book is for those people who can have a direct impact on decisions influenced by urban science: scientists, engineers, students, civil defense workers, and the professionals who manage cities and their infrastructures. Very few geoscience monographs and texts have focused on the application of Earth sciences to urban problems. This book is intended to be both an introduction to this emerging area of scientific study and a response to a growing concern, expressed at national and international levels, about the lack of geoscientific analysis in urban development.

**AGU Special Publications Series, Volume 56; 444 pp.; softcover ISBN: 0-87590-299-5; AGU member price: \$39.90; non-member price: \$57.**

—JONATHAN LIFLAND, AGU Science Writer

## Bill Would Allow Platforms as Artificial Reefs

PAGE 395

A 17 September U.S. congressional hearing on environmental aspects of oil and gas development focused primarily on potentially beneficial, alternative usages of thousands of decommissioned offshore oil and gas platforms in the Gulf of Mexico and elsewhere.

These platforms and associated rigs would be permitted to be used as artificial reefs for corals and fish populations, mariculture sites, and scientific research stations, under the Rigs to Reef Act, House Resolution 2654. The bill, introduced by Rep. David Vitter (R-Louisiana), is in referral in the Resources and in the Ways and Means committees of the House of Representatives.

About 4000 platforms currently speckle the Gulf. Just 8% of 2000 already-decommissioned platforms have been re-used as artificial reefs, with the others having been removed. Over the next 40 years, an estimated 120 platforms should be decommissioned each year. Options for old platforms include toppling them in deep water, cutting off the top of a platform to form two structures that rise to different water depths, leaving them in place, and removal.

H.R. 2654 would allow suspension of current federal requirements to remove platforms just one year after they are decommissioned, and would address some liability issues, Vitter told the House Resources Subcommittee on Energy and Mineral Resources. He said the bill also calls for a study to quantify benefits that these platforms provide to the offshore underwater environment, in addition to fishing and employment benefits.

At the hearing, Vitter claimed, "Offshore oil and gas platforms are home to some of the most prolific ecosystems on our planet. These structures attract new coral populations that attach quickly after the platform is placed and then continue to flourish for the entire life of the platform. With the corals come fish species, many of which are protected or endangered."

He also said that removing these platforms means that "thriving ecosystems are ripped from the water, fish habitats are disrupted, and many rare species of sea life are even destroyed."

Lisa Speer, senior policy analyst with the Natural Resources Defense Council, a nonprofit environmental group, questioned whether the platforms are simply attracting fish from other habitats and breeding areas and concentrating the fish for easier harvesting.

"From an ecological standpoint, the question is not so much whether there is increased life at the platform itself, but whether that benefits the ecosystem as a whole," she said.

Speer cited an October 2000 report to the University of California Marine Council on ecological issues related to decommissioning of California's offshore production platforms. That report states, "The fact that an artificial structure has lots of organisms on it does not necessarily imply its presence has enhanced regional stocks. The artificial structure may have merely attracted individuals from more suitable habitats, via larval settlement or movement of adults."

Steve Kolian, an environmental scientist with the Louisiana Department of Environmental Quality, said the platforms "clearly produce fish rather than merely attract fish" from other marine habitats. He said that more than 50 species of federally managed fish, crustaceans, and live rock organisms settle and forage around the platforms, but that these new ecosystems are not designated as protected habitat under current Gulf Fisheries Management Plans.

Kolian added that the platforms, which extend through different underwater trophic zones, "are the only hard substrate that rises through the anoxic layer" affecting Louisiana's continental shelf during part of the year.

RANDY SHOWSTACK, Staff Writer

## Montreal Protocol Benefits Cited

PAGE 395

The Montreal Protocol on Substances that Deplete the Ozone Layer has succeeded in eliminating the emission of millions of tons of ozone-depleting chemicals per year, according to a report issued by the World Bank on 16 September.

The "Montreal Protocol Status Report" notes that annual global consumption of chlorofluorocarbons (CFCs) plummeted from 1.1 million to 150,000 tons between 1986 and 1999. Without the protocol, CFC consumption would have reached 3 million tons in 2010 and 8 million tons in 2060, according to the report.

Robert Watson, the World Bank's chief scientist and co-chair of the International Ozone Assess-

ment Science Panel, said that concentrations of many of the problem chemicals in the atmosphere either have peaked or already are in decline.

Although 180 countries have ratified the protocol, which was adopted in 1987 and entered into force in 1990, several challenges remain. These include curbing illegal trade in CFCs and ensuring that countries adhere to their schedules for the full phase-out of ozone-depleting substances.

The 2003 ozone "hole" above the Antarctic peaked at about 28 million square km, according to measurements by New Zealand's National Institute of Water and Atmospheric Research (NIWA). That is larger than in 2002 and slightly smaller than the record measurement in mid-September 2000. Many scientists

attribute the increase in 2003 to colder-than-usual atmospheric temperatures above the Antarctic.

However, Stephen Wood, a NIWA research scientist, is cautious about declaring victory. He said that "although man-made chemicals that contribute to the ozone depletion are already starting to decline in the atmosphere, we haven't yet seen a sustained reduction in the severity of the Antarctic ozone hole. There will always be variations from one year to the next, so before we can confirm the expected recovery, we would need to see smaller or less severe ozone holes over a number of years. Realistically, it might take another 10 years before we can be sure."

—RANDY SHOWSTACK, Staff Writer

# FORUM

## Comments on "Anonymous Reviews"

### From D. Fisher

PAGE 395

I'd like to suggest that the recent letters complaining about reviewers' anonymity are on the wrong track. What we need is more anonymity, not less: we need double-blind mandatory anonymity.

The main argument proposed so far is the unfairness of not being able to confront the reviewers' criticisms. But you don't need to know who someone is to be able to argue

against their ideas. Reviewers' reports are spelled out clearly and can be rebutted without getting into personalities.

The more serious problem is the opposite. As an associate editor of *Geochim/Cosmochim* for many years, I found that young people were often reluctant to review an influential scientist's work for fear of reprisal, for we all know of people whose egos are such that they truly feel anyone who criticizes their work is not worthy of respect, tenure, funded grants, etc. These potential and necessary reviewers

are not stupid people, and they realize that an Old Boys' network can supercede promises of anonymity. On the other hand, young workers may also be overly influenced by a senior scientist's reputation. To get really honest reviews, the reviewer should not know whose work he's inspecting and should be guaranteed the same protection. In fact, the anonymity should be mandatory, to eliminate the temptation to curry favor by providing a good review.

This idea is, of course, an unattainable ideal. One frequently can identify both author and reviewer, either from the content or the papers in their list of references (i.e., their own); still, we don't give up on democracy simply because it doesn't always work. We stagger on, but let's try to get onto the right staggering track.

—DAVID FISHER, University of Miami, Coral Gables, Fla.

## From D. Forel

PAGE 395

I recently read four letters in *Eos* against anonymous reviews and zero letters for. I feel the need to add one to zero. When I started reviewing manuscripts, I had the ethical choice of whether or not to sign my name. After some thought, I decided I would not. Today, I feel the same for the same reason: I do not want people to think about who I am; I want them to think about what I write. R.E. Criss and A.M. Hofmeister would have me throw off my "cloak of secrecy—the costume of crooks." Would seeing my face make my argument clearer or is it an excuse to judge the messenger?

A while back, I spent 2 years as an associate editor. During that time, I signed my name because I felt people had the right to know who was passing judgment. In this, I agree with A. McBirney: "A fundamental rule of our justice system holds that one who is being judged has the right to confront his accusers." As a lowly reviewer, I did not feel I passed judgment; I felt I was contributing to the discussion.

Beyond my reviews were two higher authorities: the associate editor and the author. As associate editor, I would write cover letters to authors in which I went over points raised by the reviewers. In the way I summarized the reviews, I tried to let authors know which points I felt were critical to getting the manuscript to publication. In the way I ignored a reviewer's points, I implicitly let authors know which points I felt were not critical.

As associate editor, when the revised manuscript appeared, I compared the first draft to the second. I looked for what authors considered worthy of revision and what authors considered beneath notice. Frankly, authors rarely disappointed me. Remembering my time as associate editor, I want to give a load of thanks to those I rounded up to be my reviewers. They let me drag them from their usual duties and hound them into crawling into the minds of the authors: no small task! A. McBirney also writes, "...a signed review demands much more of time and effort..." My experience as associate editor left me with no feeling that signed versus anonymous reviews were imbued with different levels of dedication.

So, I advocate anonymous reviews. On the other hand, I am against anonymous associate editors. In fact, I believe some problems (lack of objectivity, lack of supporting evidence, lack of civil tone, etc.) the other writers cite can be solved by more responsible associate editors and editors. Just as manuscripts are sent back to authors for revision, an ill-toned review can be returned.

In closing, I want to explain, half in jest, why I am against signed reviews. I see many papers close with the acknowledgment, "The authors thank A. Smith, B. Ramirez, and C. Li for their constructive comments that greatly improved this manuscript." It seems to me authors are expressing gratitude to the people who put them through great stress in the hope that these reviewers will be gentler next time. At the same time, when reviewers get acknowledged in print, their reputations as "experts" are enhanced. Without publishing, but by critiquing, reviewers become "names."

—DAVID FOREL, Michigan Technological University, Houghton

## From E. Okal

PAGE 396

I would like to add the triple perspective of a now-retired editor (GRL, 1993–1997), a reviewer, and an author to the ongoing debate in *Eos* about anonymous versus signed reviews.

As an editor, I did not keep precise statistics, but my recollection would be that a little under (perhaps 40%) of the more than 3000 reviews I handled were signed. While some sort of "trend" expectedly existed between glowing reviews and signed ones, the correlation would probably not have passed a statistical test. By and large, my reviewers, whether or not they waived anonymity, were a professional and responsible pool, and the kind of personal and potentially unethical antagonisms described by Myrl Beck was the rare exception, rather than the rule, among anonymous reviews. The careful editor should be able to recognize this attitude in the tone and style of the review, and

through comparison with other reviews of the same paper. In a handful of cases, I exercised editorial privilege by simply ignoring the bellicose review, and not transmitting it to the author. Incidentally, note that personal animosity usually expresses itself most forcefully in the format of "Comments" and "Replies" (an editor's nightmare), where anonymity is waived de facto.

As a reviewer of ca. 10 manuscripts a year, I sign about two-thirds of my reviews; my decision has more to do with whether or not the work is directly related to mine than with its quality. In the former case, it is next to impossible to remain anonymous.

Finally, as an author, I get about half of the reviews returned to me signed. I find no obvious correlation between anonymity and pugnacity.

The above numbers—40%; two-thirds; one-half—suggest that the community is divided, with no overwhelming majority in its attitude toward anonymous versus signed reviews. This diversity may indeed be precious and should be respected. Why not keep the system

as it is now, leaving it to the individual reviewer to exercise a free decision regarding waiving anonymity? At any rate, it is probable that imposing signed reviews would make the editor's job of finding reviewers much more difficult.

Let us also remember that manuscript reviewing is only one form of peer review. There is generally no provision for signing reviews of proposals to funding agencies; and letters of recommendation are and should remain not only anonymous, but strictly confidential. The debate on peer review has been going on for decades. It may be the worst possible system, but by and large it works. And to paraphrase Sir Winston Churchill, wait until you consider all the other ones...unreviewed pseudo-publishing on the Internet gives us plenty of examples in this respect.

—EMILE A. OKAL, Northwestern University, Chicago, Ill.

## From E.P. Savov

PAGE 396

I came upon the observations and experiences of Myrl Beck, Charles Robinove, Robert Criss and Anne Hofmeister in the July issues of the *Eos* Forum.

I can say that their experiences are similar to those of my colleagues and friends, some working in different scientific fields. A colleague of mine has shown me an article that has generated mutually excluding comments by its reviewers. There also reviews 90% of whose content is dedicated to the qualifications of the reviewer; and for the rest, one or two sentences simply reject the paper, completely

unaware of the presented findings. The worst-case scenario was mentioned in the Forum "Comment" of Robert Criss and Anne Hofmeister in the 29 July issue of *Eos*. They described it as a "hostile 'review' that could have been written about any manuscript on any topic by any author."

I would call it "copy and paste review." This kind of "reviewing" does no good for the journals and anybody associated it, although on a short-term scale, it may look satisfactory to some short-sighted people. The improper reviews essentially degrade the purpose of scientific research. So I would not be surprised if the cutting edge of science shifts from some journals to others that take more care in their review policies.

Science by its nature has to be discussed in the open air. Probably a way out of the incorrect review situation is publication of the paper, together with its reviews and the names of the reviewers. In this way, all parties will fairly take credit and "discredits" for their work. If the reviewer knows that his/her name and comments will appear together with the considered research article, then there will be no "copy and paste" reviews and other examples of anonymous scientific misconduct.

—EUGENE P. SAVOV, Solar-Terrestrial Influences Laboratory, Bulgarian Academy of Sciences, Sofia

Reunion, and Kerguelen—the big hot spots—as well as the lesser hot spots, all manifest shallow reference frames (mesoplates).

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### Author Information

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### Laramide Subduction:

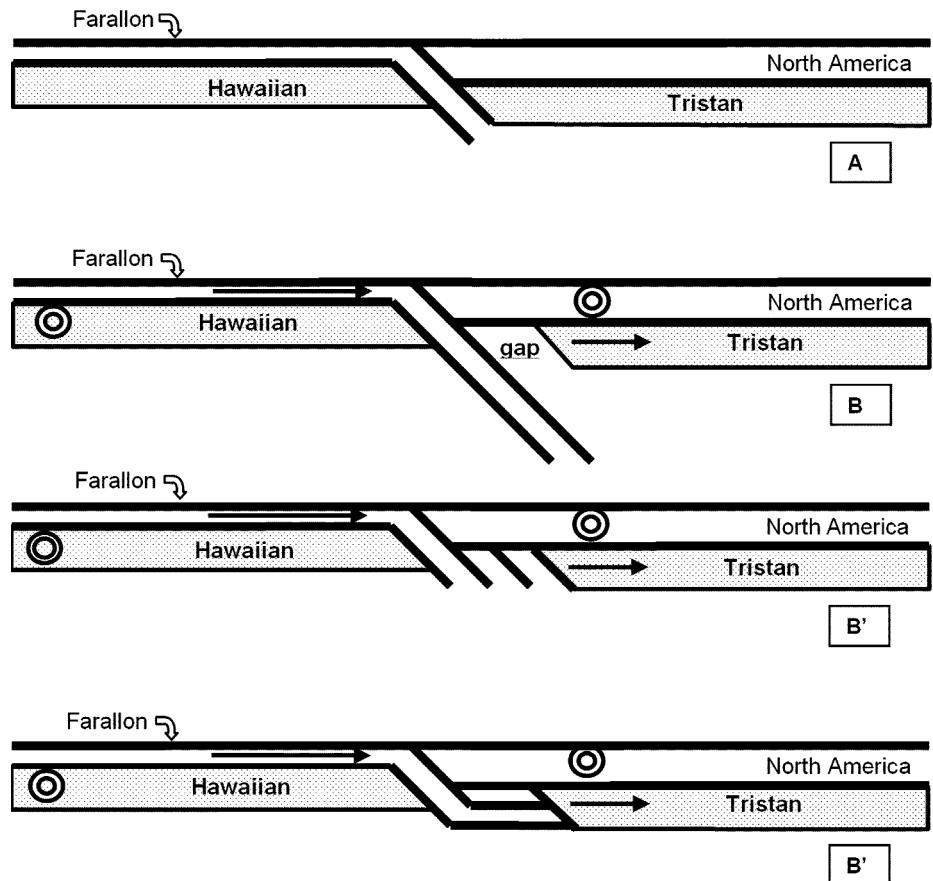


Fig. 3. Cartoons of Laramide evolution of western North America are shown in the context of the mesoplate hypothesis (asthenosphere, separating lithoplates and mesoplates, is not shown). (A) The Laramide event begins ca. 80 Ma. (B) North America moves westerly relative to the Tristan mesoplate. The Hawaiian mesoplate is nearly fixed relative to the North American lithoplate. The Farallon (or Kula) lithoplate continues to be subducted beneath North America with a gap between the subduction zone and the Tristan mesoplate. (B') Same as B, except that the Farallon plate imbricates, filling the gap. (B'') Same as B, except that the Farallon plate subducts at very low angle [Cross and Pilger, 1978], filling the gap.

## FORUM

### On the Review Process: Editors Speak

PAGE 575

We have read with interest the letters to *Eos* that have discussed the issue of peer review. Although this letter is written largely with the members of the Atmospheric Sciences Section in mind, it may also add some perspective to the general discussion of anonymous reviews.

Due to the volume of papers submitted to the *Journal of Geophysical Research-Atmospheres* (~1200 every year), we have appointed a number of associate editors (currently about 40). These AEs serve in a variety of ways, including recommending reviewers, consulting on papers in their areas of expertise, and assisting when there are potential conflicts of interest. The ultimate responsibility for decisions on all papers,

however, rests with the editors alone. Apparently, this procedure is different from that taken by other journals, as noted by Robert J. Geller and John A. Goff in their letters printed in the 23 September 2003 issue of *Eos*.

It is natural to question how we can handle such a volume of papers without delegating authority for final decisions to AEs. We note that the vast majority of papers are reviewed fairly, yet critically, by our colleagues and do not need extensive editorial attention. In most cases, authors respond positively to the recommendations of the reviewers or respectfully point out where there are genuine differences of opinion. In the case of the latter, we may consult with our AEs who are more knowledgeable in the subject matter of the paper (and whose names are listed as a group on the inside front cover of the journal). However, the final decisions are ultimately ours. Any author

who has concerns about how a paper is handled should raise those with the editor, and not criticize anonymous AEs who are providing an important service to the journal.

Unlike some other journals that only publish a small number of most newsworthy submissions, we do not feel that it is our responsibility to reduce our journal to an arbitrary “shelf-friendly size” by rejecting a majority of the submissions because they may not be of interest to all readers.

Rather, it is our goal to publish excellent science, and to do that it is important that the reviewers and authors enter into a constructive dialogue that will help to reveal and minimize the most important barriers to effective communication of the key results. Sometimes, potentially exciting new ideas are rejected because they are poorly communicated. Sometimes, less exciting results are published because they are well written and accurate. Nevertheless, it is our hope that in all cases, the review process helps to identify errors and to improve the clarity of the writing and figures, so that readers can understand the work and trust the accuracy of the results and the strength of the conclusions.



We feel some need to inform the members of our section that we do not see any serious problem with the current procedure that allows reviewers of *JGR-Atmospheres* to voluntarily sign their reviews. Ultimately, it is our responsibility as editors to recognize when the dialogue is not constructive and to focus it back onto the relevant issues. While a fair number of reviews are signed, the vast majority are not. We do not believe that the reasons for maintaining one's anonymity are only a few. To base radical change on anecdotes, such as those raised in previous letters to *Eos*, would be unwise. However, we do note that hostile responses from authors after a decision to

reject a paper are much more common than hostile reviews.

It is our recommendation that, after receiving a difficult review and unexpected decision, authors take time to adequately digest the contents of the reviews and decision before responding. The strong comments that are expressed in frustration immediately after a rejection only reinforce the views of some reviewers of the need to remain anonymous. Reviewers are performing an important service when they provide thorough evaluations of papers. If they feel that anonymity allows them to offer critical (yet fair) reviews, then we should not attack that value on the basis of a few cases where it is misused. Ultimately,

it is the responsibility of the editor to keep the dialogue constructive. A good editor will quickly learn which reviewers provide well-balanced, fair, and constructive reviews and, likewise, which reviewers to avoid because they do not.

In closing, we would like to take this opportunity to thank our reviewers and associate editors for their continued fine service to the journal. We feel that they are ultimately the force behind our high ranking amongst the geosciences journals.

—COLIN O'DOWD, STEVEN PAWSON, ALAN ROBOCK, AND DARIN TOOHEY, Editors, *JGR - Atmospheres*

## ABOUT AGU

### Climate Change Statement Highlights Human Influence

PAGE 574

A new AGU position statement on human impacts on climate states that "human activities are increasingly altering the Earth's climate." Natural influences alone do not explain the increase in global near-surface temperatures in the latter half of the 20th century, the statement explains.

Announced at a 16 December press conference in Washington, D.C., the statement notes that human impacts include atmospheric greenhouse gases, as well as air pollution, airborne particles, and land alteration.

The statement stresses, "A particular concern is that atmospheric levels of carbon dioxide may be rising faster than at any time in Earth's history, except possibly following rare events like impacts from large extraterrestrial objects....The unprecedented increases in greenhouse gas concentrations, together with other human influences on climate over the past century and those anticipated for the future, constitute a real basis for concern."

While noting the difficulty in predicting some aspects of human-induced climate change, the statement indicates that scientists are confident in predictions concerning the melting of some polar and glacial ice cover, ocean warming, and changes and intensification of the hydrological cycle.

The statement calls for enhanced research, observations, modeling, computational capability, and educational outreach to support climate-related policy decisions. "AGU also urges that the scientific basis for policy discussions and decision-making be based upon objective assessment of peer-reviewed research results," it says.

The statement was adopted unanimously by the AGU Council at a 12 December meeting in San Francisco, and replaces an earlier 1998 statement that had been reaffirmed in 2002.

Marvin Geller, chair of the AGU panel that drafted the new statement, said it is consistent with statements and assessments by other scientific bodies including the Intergovernmental Panel on Climate Change and the U.S. National Research Council. Geller is with the Marine Science Research Center, SUNY-Stony Brook.

Geller, who is also past president of AGU's Atmospheric Sciences Section, added, "We are not reporting on startling new science here, but rather, the statement is based on the peer-reviewed literature, and much of this has appeared since the last statement was adopted." He noted that while the earlier statement dealt with the issue of greenhouse gases and climate, the new statement also deals with many more human influences.

AGU President Robert Dickinson noted that climate change is an issue advancing relatively rapidly, and some people at the AGU Council meeting wondered "whether this statement is already obsolete, even as it hits the streets." He said, though, that it is unlikely there will be another review of the climate statement for another four years.

Responding to a question about whether all peer-reviewed papers agree with the position statement, Dickinson said, "We are not saying you can't come up with other conclusions by finding one or two papers somewhere. We are saying, if you look at [the peer-reviewed literature] overall and you synthesize the evidence, the statement we are putting [out] here is the consensus view of where we are now."

John Christy, director of the Earth Systems Science Center at the University of Alabama at Huntsville, and a member of the panel that drafted the statement, said, "It is inconceivable that after changing forests into cities or putting dust and soot into the atmosphere, or putting millions of acres of desert into irrigated agriculture and putting greenhouse gases into the atmosphere, that in some way the natural

course of the climate system has not been changed. As a climate scientist, you do come to the conclusion that physically, the system is changing due to the things that humans have done."

Christy said the statement also does not highlight the uncertainty of smaller-scale, regional features in the climate system, such as the decrease in temperature in the southeastern United States over the past 100 years. But he emphasized, "I want to support this [AGU] statement and come out strongly and say, 'it had better be on the radar screen of any administration or political body.'"

The statement does not mention specific temperature projections or focus on some other areas. Geller said that while there has been much attention to the possibility of abrupt climate change, the panel did not address it. "We did not think we knew enough about it at this point to anticipate exactly when that might occur," he said.

Panel member Ellen Druffel, a professor of Earth system science at the University of California, Irvine, said of the overall accomplishment of the statement, "Scientists are in general a conservative bunch. To get the AGU Council and this panel to agree unanimously that humans are changing climate; that in itself is significant."

—RANDY SHOWSTACK, Staff Writer

*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*

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—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.

# FORUM

## More on Anonymous Reviews

PAGE 160

Reading the ongoing correspondence in *Eos*, I would propose that the difficulty lies in the dual nature of the reviewing process. The first stage asks, is the work worth publishing? The second asks, is the paper as submitted worthy of the work done? The dilemma is that the requirements for anonymity are different for the two functions.

Like most of the correspondents in *Eos*, I feel that the evaluation of the merit of the work must remain anonymous. Personally, I prefer it to be "double blind," in which the author of the paper is not revealed, although it is often not hard to guess. That way I can give the material the fairest possible treatment.

However, the detailed review of the presentation and the material could be a more two-way process, between author and reviewer, with the editor acting as judge. Here, the external reviewer can make a real contribution that should

then be acknowledged at the publication stage. In some instances, the process becomes so interactive that the reviewer becomes an additional author of the paper. The danger of this stage is, of course, that authors can become sloppy, leaving work to the external reviewer. It must remain the privilege of the reviewer to decline to work on a paper that, no matter how good the work, is just plain poorly presented.

It would be possible, but cumbersome, for this process to be carried out anonymously through the editor, with the names revealed when the review process is over. The authors should then acknowledge the contribution of the reviewers and editors. It would make much of the process more transparent, and help the development of the science, if the published document were to routinely name the reviewers.

The two phases of reviewing, the initial evaluation and the detailed discussion of content and presentation, thus have different require-

ments for anonymity, and the root of the current debate is the confusion of the two roles in the current system of single-stage reviewing. The initial evaluation is particularly important, but should not be enormously onerous, and it should remain within the current anonymity conventions. The second stage should be much more of a dialogue than a confrontation, and requires a lot of effort on behalf of both reviewer and the editor. Attributing this process to those who put in the effort would do much to make the effort visible and, in these bean-counting days, carry some element of reward for those who put in the time.

Lastly, the overall guiding principle must be that of personal integrity. The duty of all authors, editors, and reviewers is to advance their science; this requires constant vigilance, hard work, and the highest personal standards of integrity. Those who have done so in the past deserve our thanks and perhaps more credit than they have received in the past. Those who continue to do so in the future should receive more immediate personal credit, which can be given only if the cloak of anonymity is lifted.

—ADRIAN ARMSTRONG, Entec U.K., Bristol, U.K.

## A Code of Ethics for Referees?

PAGE 160

I have read with interest the many letters commenting on the pros and cons of anonymity for referees. While I sympathize with writers who have suffered from referees who are incompetent or uncivil, I also sympathize with those who argue that one would simply exchange one set of problems for another if

journals were to require that all referees waive anonymity.

Perhaps there is a more direct way to address the issue. It may help if guidelines for referees were to include a code of ethics. Personally, I would like to see each referee subscribe to the following:

- I will treat each article with the same care and respect that I would wish to have accorded to my own articles.
- I will withdraw from reviewing an article if I find that I do not have the necessary background and interest.

• I will identify what there is in each article that would be interesting and useful to readers, and then—if necessary—try to help the author present that material more effectively.

• If I have valid criticisms to make, I will be specific, clear, and polite.

• If I believe that some result has already been published, I will give at least one relevant citation.

*Editor's Note: see AGU's Guidelines to Publication of Geophysical Research: [www.agu.org/pubs/pubs\\_guidelines.html](http://www.agu.org/pubs/pubs_guidelines.html).*

—PETER A. STURROCK, Stanford University, Calif.

## Young Solid Earth Researchers of the World Unite!

PAGES 160–161

In early January 2004, one of us attended a workshop on "science priorities and educational opportunities that can be addressed using ocean observatories." The attendees constituted a broad group—men and women, scientists, engineers, educators, representatives from the private and public sector—but lacked diversity in at least one important aspect: age.

A well-known marine geophysicist (with a published record stretching over 30 years) came to me at the ice-breaker party and said (and I paraphrase): "I'm glad you're here: you're young, you might actually see this project flourish before you retire. There're not enough young people here." At some point or another, every young scientist may have a similar experience.

However many hours one spends in solitary confinement in the lab or behind a desk,

science is fundamentally a social activity. Community-building needs to happen early on in the career of a young researcher. Meetings like the popular AGU Fall Meeting are often too massive to get to know many new colleagues. More focused meetings like the Gordon Conferences tend to attract senior scientists first, not only in attendance, but in meeting-room dominance as well. Young oceanographers and atmospheric chemists are the lucky ones; with the Physical Oceanography Dissertation Symposium (PODS) and Atmospheric Chemistry Colloquium for Emerging Senior Scientists (ACCESS), they have a forum focused on recent Ph.D.s.

However, the Meeting of Young Researchers in the Earth Sciences initiative (MYRES) attempts to provide a similar framework in the solid Earth sections of AGU.

Here's how the recently funded proposed activity works, and how you—young solid

Earth geoscientist—can benefit from it. The MYRES "manifesto" lists its aim as "to further science by accelerating the growth of an interdisciplinary, international, open, and unbiased community of colleagues who interact regularly to informally exchange ideas, data, and tools, and formulate new collaborative research projects." A biennial conference series for junior scientists in geochemistry, geodynamics, mineral physics, seismology, and related solid Earth fields is the first step. The first MYRES conference will be held 12–15 August 2004 in La Jolla, Calif., and will focus on the topic, "Heat, Helium and Whole Mantle Convection." The meeting will be small, with fewer than 100 attendees selected on the basis of a brief statement. Almost all travel and lodging costs will be provided by the U.S. National Science Foundation.

At a MYRES meeting, young specialists will educate each other about the issues each of their disciplines can address in the format of a summer school. What you should hope to gain from this is a broader understanding,