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# On serving as an Associate Editor

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***Editor's Note:***

*Bill Harlan has been one of our most conscientious and loyal Associate Editors over a period of many years. This experience has provided Bill with a feeling for what's good and appealing about a successful scientific paper. Some months ago he committed these thoughts to paper for the benefit of his fellow Associate Editors. I believe, however, that what Bill has to say is at least of equal relevance to all those who toy with the idea of writing a scientific contribution. I hope our readers will enjoy Bill's essay as much as I have.*

*—Sven Treitel*
*Editor*

In my first year as an associate editor, I found that maybe one third of all manuscripts were taking two thirds of my time. A few years later, I have been able to distribute my time much more evenly with a few preventive measures. First, I make sure that reviewers will be interested in the paper and will not leave too many details to me. Second, I make sure authors understand what is expected of all requested revisions.

Currently, only two reviewers are required to examine a manuscript. Two reviews are often adequate, so this is a sensible minimum number. Nevertheless, I begin by asking four reviewers.

The most conscientious reviewers are authors who recently submitted a paper on a similar subject. Second best are those who have published recently on related topics. The SEG's Digital Cumulative Index is a great way to search title keywords. GEOROM would be even better. I check the authors' bibliography for related work, but remember that bibliographies may have important omissions. The SEG homepage finds member addresses and even e-mail addresses. If not, a recently published paper will have a mailing address. I avoid asking a given reviewer more than once a year.

Most importantly, I try to be patient as I look for candidates. I prefer to avoid the phone and ask by e-mail or ordinary mail. A formal letter makes the potential reviewer take the request more seriously. On the phone, the candidate might find it difficult to say no, when in fact that candidate will be too busy. Some may instinctively say no before they realize how interesting the paper will be to them personally.

When I receive a new manuscript, I have the abstract typed into a file. This abstract is incorporated into a form letter, which points out that the work is "closely related" to the potential reviewer's recent work. I add a few sentences describing how the work is related. I may emphasize that I want the reviewer to address one aspect of the work and other reviewers will address other issues. (Many papers combine diverse specialities.) The end of the letter says that the manuscript will arrive in ten days with at least three additional weeks for review. I ask the candidate to check the mailing address and to respond by e-mail, fax, or my answering machine, if possible. These days, most respond by e-mail. Reviewer addresses go into separate files that are incorporated in all the following letters, to minimize typos.

If I have chosen the candidates well, then two will allow me to forward their names to SEG headquarters immediately. Usually a third will agree later. I prefer to find a third reviewer unless I am very familiar with the subject. If all four say yes, then so much the better. Later, if a reviewer takes too long, I can rely on the two or three reviews that have arrived. If I end up with just two perfunctory reviews, then it is my punishment to go through every detail of the paper carefully and do the reviewers' work for them. Next time, I ensure that I get more reviews. Extra reviews also make it easier to arrive at a consensus.

For whatever reason, most papers submitted to Geophysics seem to require significant revision. Sometimes it seems we fill a publishing niche for those who have good ideas but are not sure that anyone is interested. Not to exempt myself, I have also submitted manuscripts that I knew needed to be rewritten, just to see if I was wasting my time. I think we help such authors find the essence of their papers so they can finish. Often the second draft of an inconclusive paper improves enormously and becomes an authoritative reference in the field. The author has finally found some interested readers and writes directly for those readers. (Do we receive so many unfinished papers because we are slow, or are we slow because we receive so many unfinished papers?)

My particular subject of inversion and tomography may not be typical, but less than a third of the manuscripts can

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be accepted or rejected outright—it feels like less than one-sixth either way. When a paper requires only minor revision, I simply express my admiration. When the reviewers and I agree completely, a paper may be rejected as inappropriate for GEOPHYSICS. Occasionally, I suggest that "we avoid the unnecessary delay of a full review because the paper could not possibly pass review in its current form." For example, a paper is entirely theoretical and lacks any clear geophysical application. I might recommend another journal like "Inverse Theory." Sometimes the format of the paper does not remotely resemble the description in the Instructions for Authors. I may then ask that the paper first be reformatted, or drastically shortened, before review begins. Reviewers will feel their time is wasted if they are forced to point out an obvious overwhelming defect.

Perhaps two thirds of papers require "significant revision." About half of these will be returned to one or two selected reviewers after revision to see if their "serious concerns" have been addressed. Most papers have at least one shortcoming that could cause many readers to dismiss it hastily.

All papers contain something of interest. It is not difficult to persuade authors to restructure their papers and emphasize different ideas, if they feel their ideas are genuinely appreciated. Reviews should stress positive contributions of the paper so far as possible and avoid dwelling on the negative. There is no reason ever to say that certain content is without value (it was of value to the author). Instead, we can stress that other parts of the paper deserve a greater proportion of space. Almost all criticism can be offered constructively, with a realistic suggestion for improvement. Never should a comment appear to criticize the author personally.

When the reviews arrive, I read them all first, then read through the paper, looking for problems not mentioned by the reviewers, particularly structural problems. I then collect my notes and write for the Editor a summary review that has evolved into a consistent format. First, I give the consensus recommendation of the reviewers and of myself for the level of revisions necessary.

Next, I explain what I really liked about the paper. For example, I liked the thoroughness of the analysis, the numerical methods, or the examples. The paper will be very influential, will inspire new directions in research, will have immediate practical application, etc. Authors discourage easily and may overestimate the difficulty of the revisions, so it is important to emphasize that you really do like their work. The last sentence of my letter is usually something like "I look forward to seeing the revised paper in print."

Next, my review for the Editor summarizes those revisions that must not be overlooked. To avoid unnecessary second revisions, it is important that the author understand what changes are expected, particularly major changes. These are issues that must be resolved. I recapitulate any of the reviewers' comments that cannot be ignored. The first few paragraphs summarize these changes and leave details for later. If the abstract is defective, I ask that standards for abstracts be forwarded.

After the summary, the remainder is marked as "additional comments for the authors." Here, I generally explain too much, rather than risk having to explain again after the first revision has returned. A little extra time here could save three or four months delay later. I start typing into this section immediately, so as not to forget anything. All suggestions are phrased as constructively as possible.

Any paper that carefully analyzes an actual recorded data set has my respect. Even if the processing methods are conventional, the application will demonstrate how well these methods performed in practice. A good case study will also reveal the most important issues to investigate next. The most common weakness of such papers is that they claim too much unequivocal success for their approach. Perhaps a literature search is in order to discover and compare different approaches to the same problem. The authors can be reassured that their approach need not be the final word on the subject. They need only identify the assumptions and circumstances that are appropriate for using this new approach. How should the method be modified in different circumstances? An original approach is always welcome because it gives readers an alternative they did not have before, even if they continue to favor other methods. Maybe readers will be inspired to modify or combine other approaches.

My least favorite papers are those with a totally artificial synthetic experiment, such as a survey with ten sources, ten receivers, and a simplistic velocity model. Such papers are now requested to include a "useful geophysical application to a realistic data set." The theory should not be disproportionately more complicated than the application.

Often authors emphasize the material that is newest to them and deëmphasize those ideas that they actually understand best. They need to be persuaded to rely on references for familiar material. Alternatively, if some unusual methods have not appeared before in GEOPHYSICS, then I may encourage the author to add short tutorial explanations, even if a full explanation can be found in nongeophysical journals. Readers must be convinced first that these ideas deserve further investigation.

Sometimes the most original ideas are buried late in the paper, perhaps out of fear that decisions were too unconventional. Such authors need to be reassured that they can emphasize their new approach in the Abstract, Introduction, and Conclusions, with more discussion and justification. Again it is important to say that the chosen method need not be the best of all possible methods: it is enough to provide a new alternative that works in situations described in the paper.

Sometimes reviewers are excessively critical. A weak or slightly oversimplified assumption is declared to undermine the entire work. In such cases, I explain that the author can more carefully explain the reasons for this particular assumption and give some alternatives, with their advantages and disadvantages. The author shows how the methods in the paper can be modified to allow different assumptions. Thus, a well-written paper is rarely invalidated by a single bad assumption.

I may simply contradict an unconstructive or excessively negative comment from a reviewer. Sometimes, reviewers point out a problem and then suggest a patently impractical revision, such as a total rewrite of all algorithms. (I prefer to emphasize stronger parts of the paper.) Less hardened authors might simply throw up their hands and submit to another journal. I do not usually withhold a review from the author, but I do say when I disagree with the review, or with the tone of the review.

I have taken to including a standard sentence: "Please respond to all suggestions, either by an appropriate revision of the text, or by a separate explanation of why the suggestion is inappropriate. If a reviewer has misunderstood the manuscript, then I encourage you to clarify the text: other readers may make similar mistakes." Another common sentence: "Algorithms should be described with enough detail and citations that a diligent reader could conceivably reproduce your results."

My standard review letter includes optional sentences for common corrections such as adding an informative abstract, labeling all figure axes, marking vectors and tensors, equation numbering, reference style, etc. An oversight here could delay the final revision.

If a revision is returned, and a major suggestion has been ignored completely, then I feel entitled to send it back and ask for an explanation. Most authors will conscientiously try to answer all questions. Many will even number the suggestions and highlight the corresponding changes in the text. A small number will try minimal changes, just in case we don't bother to check.

I do not ask a reviewer to look at more than one revision of a paper. After that it is my problem. Perhaps I did not state clearly enough what changes were wanted in the revision.

Very rarely, I believe a revision essential and the author disagrees. I make my case and let the Editor decide. Never, as I once feared, has there been an irresolvable disagreement about the *ideas* in the paper. If the author has given a full explanation of the reasoning behind the work, with adequate citations, and acknowledging alternative approaches, then I think it safe to let readers decide for themselves.