An abundance of challenges in journal editing

Some scientific journals in the past few years have increased page counts so much that a single editor can no longer handle all of the incoming manuscripts. As a result of that growth, many new and inexperienced associate editors are employed. They may not be familiar with all the problems encountered in scientific editing nor with the best ways to handle them.

Almost any manuscript that includes breakthrough or radical new ideas will encounter extensive discussion, partly because our concise contemporary writing style and constraints do not allow for lengthy explanations. Several journals have adopted a practice of automatically rejecting any manuscript that has received two critical reports. Unfortunately, such a policy virtually ensures that important new ideas are rejected, whereas innovative papers are just the sort that we should most want to publish. If exchanges between authors and referees lead to improvements to a paper and a better understanding of the science involved, an editor's arbitrary rules can be a detriment to the process.

It is also unfair when referees present new objections to a paper in each successive report; that is, a statement that has passed first review without comment should not be questioned in a subsequent review. I and others have had papers rejected when that practice is combined with a limit on the number of critical reports.

Not all referees are unbiased or fair. A well-known example is that of Cecilia Payne's thesis, which showed that the difference between the spectra of giant and dwarf stars is due to different electron pressures, not differences in abundances. The referee said that her work went against current thinking, so her thesis paper was never published. Because reviewers are not infallible, authors of papers should be allowed arbitration.

The late Allan Sandage told me that he saw in the *Astrophysical Journal* (not during my 29 years as its editor) a paper that criticized his published work. He had not previously been warned of its publication or given an opportunity to comment on it. Of course, he had to write and submit another manuscript clarifying his published paper and explaining why the criticism was incorrect.

One way to avoid publishing a series of critical papers followed by clarifying ones is as follows. The new, critical manuscript should be sent first to the author criticized, so that he or she can add signed comments, but not act as a referee. Then those comments should be sent to the new authors for reply. That reply should be sent back to the criticized author, and the manuscript and two sets of signed comments should be sent to a neutral referee for review. The original author should be kept informed of each step in the reviewing and revision process but is generally not asked for additional comments. This process takes longer, but it avoids publishing a series of critical papers and authors' defenses.

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Clarifying the credit for KamLAND

Our colleague Giorgio Gratta of Stanford University pointed out to us that our obituary for Stuart Freedman (PHYSICS TODAY, March 2013, page 72) ought to have credited Atsuto Suzuki alone with the realization that power reactors in Japan were fortuitously situated to allow a long-baseline reactor antineutrino experiment, KamLAND.

US involvement in KamLAND began in 1997, under the leadership of Gratta, with groups from the University of Alabama, Caltech, Duke University, the University of New Mexico, North Carolina State University, Oak Ridge National Laboratory, Stanford University, the University of Tennessee, and the College of Chemistry at the University of California, Berkeley. The basic configuration of KamLAND had been defined by 1998 when the Oak Ridge group withdrew and Freedman's Berkeley group joined. The Berkeley physicists designed the readout electronics and were instrumental in calibration. During the construction, commissioning, and data taking, Freedman and Gratta were US cospokesmen for KamLAND.

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