

Do we need editors?

I submitted this editorial to *Meteoritics & Planetary Science*, not only because it is a journal I know and love (having been editor for 11 years), and I trust my opinions to its readers (having written editorials for 11 years), but because—as far as I am aware—it is one journal not guilty of the problem I want to highlight. Hence, my comments are directed to the wider community.

I believe the planetary science community, in particular, is suffering from an outbreak of “over reviewing,” or should I say, “under editing.” Don’t get me wrong. I am an ardent believer in peer review, recognizing as we all do that—like democracy—it is a lousy system, but the best we have. Peer review serves to catch major mistakes and ensure that authors have considered all options. Editors read the paper and the reviews and ensure that this process is being followed, making the ultimate decision on acceptance. The guiding principle to good editing is, in my opinion, ensuring that the well-chosen peer reviewers critique the paper, and that authors take full advantage of the critiques to produce their best science.

But my experience is that for the last decade or so, there has been a rising tide of disengaged editors. There is an apparently new trend for editors to return papers to authors and reviewers repeatedly until either the reviewers say publish or the authors go elsewhere. The authors and reviewers are left to fight it out, to review, revise, review, revise, review, revise, and so on, while editors look on without intervening. I have heard of as many as six or seven rounds of review and revision for a single paper, until reviewers become word police and changes simply flip-flop. Papers become jaded and lose their flush of new discovery. Enormous amounts of precious time and energy are wasted. There is, in fact, a war of attrition, the outcome determined by the last man or woman standing. If the author wins, the paper is published, if the reviewer wins, it is not. Of course the disengaged editors’ assumption is that we live in an ideal world and that the reviewers and the authors, being perfect scholars under no pressure other than a zealous search for truth, will come to some blissful agreement as science slides gracefully towards the truth. Yeah, sure.

This is not the scientific method. Science is not well done by committees, or consensus, or compromise, or

pragmatism, or obstinance. We do not make progress when an author and a reviewer are forced to agree. Cliques and bandwagons are the enemy of science progress. Publication is an author placing ideas on the table. Science progress is letting them stand the test of time, being absorbed into communal thinking, embellished, and accepted, or being proved wrong, either through overt rebuttal in the open literature or by being allowed to wither on the vine. Sometimes it takes weeks, months, or years for an idea to be appreciated. Sometimes it takes decades. Sometimes withered remnants become exciting new directions. Need I say “plate tectonics”?

One round of reviews by peer experts should normally be sufficient. If the issues are sufficiently complex, and it is doubtful that authors could realistically be expected to take care of everything at one shot, then second reviews might be worthwhile. More than two reviews, in my opinion, harm the paper, because the author is now being forced to either compromise his or her views in order to please the reviewers who, after all, are not the authors, or enter a war of attrition. In fact, both can happen.

I have tried to understand this phenomenon, which gives to reviewers a kind of power to accept or reject papers; a power that rightly belongs to the editors. Does it come from the Arizona Space Science series, where researchers with known differences are deliberately asked to coauthor in order to present a “balanced” view? This is a sound enough philosophy for a textbook. The Arizona book series has been very successful. But forced co-authorship is not what we are asking of reviewers. Perhaps it is that so many of us are involved in mission teams, where decisions must be made and a consensus is forced? But these are expensive short-term projects that must happen on a schedule. This is not the way Science works. The scientific method thrives on unimpeded exposure of diverse ideas and opinions. Perhaps it is the result of overworked editors, who take on the job out of a sense of good citizenship, or the need to strengthen their resume, and do not really have the time or interest.

Whatever the causes of unrestrained cycles of revision and reviewing, it is certainly catalyzed by on-line manuscript handling. It is all too easy to look at the incoming files, especially the final recommendation

in the case of reviews, and hit “forward” and within microseconds a form letter goes to the author or the reviewer with very little going through the mind of the editor. And so the cycle repeats. On second thoughts, maybe this is the main culprit.

I do not really need to list our weaknesses, and why there is rarely a delightful empathy between reviewers and authors. We are all aware of the competitiveness in our field, our need to protect our ideas and our position, turf guarding, and the ego that everyone but us seems unable to control. We all know about the professional negativists who seek to improve a paper by attacking it, and the more it is attacked the better it will be.

Denis Shaw summarized the strength of properly implemented peer review in his paper on the “non-interchangeability of hats,” the “hats” being the separate roles of author, reviewer, and editor (Shaw

1988). These are roles we all play when we wear the appropriate hat. When we wear the reviewer’s hat, we should not expect to be given the rights of the editors, as the editors allow us to trample on the role of the authors. We as a community really must reverse this slide into a publication environment where editors are not necessary.

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REFERENCE

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