

The responsibilities of publication

One astute member of our community once stated that, "if meetings are the heart of our Society, the journals are its soul". By "Society" I will assume he meant our research community in its widest sense, because it is not a parochial sentiment. Publication is the life blood of all research. The hours spent in the laboratory, whether at the microscope, the computer terminal, in the wet lab, or any other place, amount to nothing if the work is not published. Publishing is how we communicate and how we think out loud. Publishing is our communal effort at understanding our environment.

But it is an imperfect process. Bogard summarized some of the tricks of the trade from the authors' perspective, and one might readily imagine a comparable list for reviewers and editors (see Shaw, 1988, page 2745). The humans involved in the process are no less free of frailties because they gained the Ph.D. degree, accepted external funds or agreed to review a paper. Like democracy, it is a poor system but it is the best we have and in the long term works fairly well. Of course, this does not free us of the responsibility of worrying over the imperfections.

It behoves us, I think, when acting as reviewers, to avoid being preoccupied with the empty half of the glass and ignoring the contents (*i.e.*, stressing the flaws of a paper and not acknowledging its strengths). For the author submitting his 70th paper, this is not an important point. However, a very large fraction of our papers are authored by young colleagues or even students to whom the reviewer assumes monumental authority. I sometimes observe that young colleagues drawn to research will grow noticeably in the light of a patient and constructive review of one of their earliest papers. The return from constructive reviewing exceeds that from the money we all spend encouraging fresh blood to the field. I also see more creativity when reviewers restrain the competitive spirit that works well in the sports arena and take a more positive approach to each others scientific efforts.

We must also actively search for fresh minds to bring into our deliberations over individual papers. As we get more and more specialized, and subject areas get larger and more interdisciplinary and diffuse, there is a remarkable shrinkage of the reviewer pool for a particular topic. There is then a real danger that issues do not get the full and open hearing they deserve. Editors must seek to include among reviewers non-specialists who, while still able to recognize poor work and comment on the value of the work to the larger community, are not subject to the pressures of being an activist in the topic. Authors especially should be encouraged to nominate reviewers the editors would not normally identify. Conflicts of interest are usually quite obvious, and authors' suggestions are nearly always worth following.

Then there is the need to foster a culture in which new ideas and different ways of doing research or thinking about problems are welcomed. Chapman (1992) touched on this when he insisted that not all original papers will follow the traditional pattern. It is true that the surest way to guarantee quick and painless publication is to perform measurements of a kind and in a way that have often been done before, write the paper according to well-prescribed patterns and not to challenge conventional interpretations. This is, of course, not necessarily an indictment against the work or an argument against publication, but echoing mainstream views without substantially adding to the hard evidence reinforces them without reason. We need to avoid intellectual inertia and bias against non-

conventional views. We must strive to keep issues open. After all, this is the most fundamental tenet of the scientific method. However, it is also when the review system is put to its toughest test. How can it weed out papers that are genuinely weak from those that the system would like to consider weak because they challenge that system? Maddox (1995a) is surely right when he claims that "Principia" would have been rejected were it submitted in today's climate.

Finally, there is the need to avoid "bad faith". Fortunately this is very rare, but it occurs occasionally. "Bad faith" is when reviewers attack a paper they have barely read or when authors revise a paper without thinking about the reviews. Of course, this is one of the editors' prime concerns, and it is a common cause of protracted reviewing. "Bad faith" is also displayed when we commit plagiarism or inappropriate authorship (Maddox, 1994, 1995b).

I did not want to commit the sin of negativism, and fortunately Shaw has given me, and all of us, cause for optimism. Shaw's hypothesis, which he calls "the non-interchangeability of hats," is that when we are called upon to be reviewers, authors or editors, we act as reviewers, authors, or editors, seldom confusing the roles even if it means contradicting ourselves overtly. Shaw's hypothesis is undoubtedly true much of the time. Chapman (1993) has also rightly pointed out that most reviewers perform a sterling duty. Some reviewers do such an excellent job that they deserve co-authorship. This has actually happened to one paper submitted to *Meteoritics*. Surely this is the peer-review system succeeding beyond all reasonable expectations and something of which we can feel very proud.

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