

## WHOSE PAPER IS IT ANYWAY?



Bruce Yardley

A while back I returned a manuscript to a journal after making the relatively minor revisions requested by the reviewers. The editor responded promptly but explained that he could not make a final decision without sending it back to the reviewers for further comment. Given that there is so much literature out there, should we be grateful that there are mechanisms in place to spare us from more than is really necessary, or does micromanagement of publication stifle independent thought and make unnecessary work for reviewers and editors?

Attitudes to reviewing evolved throughout the twentieth century and sometimes varied significantly between countries. But then, new technology and the dominance of just a few publishers have made everything a lot more homogeneous over the past decade. When I edited a volume of Russian science about 20 years ago, I found that many of the authors were not used to having reviewers read their papers in sufficient detail to actually point out when figures did not show what was claimed in the text or when data tables were incomplete. Without exception they really appreciated the rigorous scientific reviewing. In the early 20<sup>th</sup> century, reviewing seems to have been universally a lot more lax than today, although the classic techniques for using the review process to delay or prevent the appearance of your rival's work were of course perfected at a very early stage. At one time it seemed that North American journals took reviewing more seriously than many of their European counterparts, who were happier to let authors publish rubbish under their own names if they insisted on doing so. Even with modern technology and lengthy check lists for appraisal, some papers still appear with glaring errors of content or communication because reviewers have ticked the boxes without really checking things out.

At the other extreme, some reviewers and editors insist on detailed changes before they will accept a manuscript, even against the author's better judgment. It may be insistence on a particular grammatical usage, but it can also be more contentious: citation of a particular source for an idea for example. I can recall one particularly pernicky editor who was so exact in his demands that after the fifth revision I seriously thought of making him an author. He seemed to have rewritten most of the text and at least the journal would have had to pass the manuscript on to another editor at that point!

It might seem a good thing that reviewers and editors take such care over manuscripts, but it is possible that they may be cutting out independence and originality, and making us concentrate on presenting things in a way that will accord with mainstream views, even when it is

not really relevant or necessary to do so. If the reviewers have checked that the data are gathered appropriately and are well documented and that calculations have been done correctly, shouldn't that be enough for publication? What if the interpretation seems crazy? Should journals spare the scientific community the sight of your hard-won data because you chose to interpret them in a way that is unconventional, or should they encourage publication, if only so that right-thinking scientists can reinterpret your data and show how they actually fit better with their own ideas? As far as possible, I think it is best to let the author put his name on the paper and take the flak if it turns out to be wrong, although I would certainly insist on removing lengthy flights of fancy that are not supported by the data, irrespective of whether they are mainstream views or novel ones. I have heard journals rightly criticised for publishing papers with data and observations that are clearly flawed, but if it really mattered whether all the ideas in a paper also turn out to be correct in the long run, then *Nature* would hardly be treated with such reverence by the scientific community!

Classically, the job of an editor is to suppress flawed papers and encourage those that are right, and much of the time it comes down to rejecting papers that are probably not wrong but say nothing new and looking for papers with new data or new ideas that will be of

value to other scientists. Reviewers point out problems that are apparent through their specialist knowledge, but the editor has final responsibility to decide whether the flaws are fatal or simply an irritation to the cognoscenti. Increasingly, however, the major publishing houses seem to want to publish only those papers of which the reviewers actually approve, which is not at all the same thing as publishing those papers in which reviewers have not found an obvious flaw. Reviewers are now routinely asked if they are prepared to take a second look at a manuscript after it is returned in the light of the initial review. Why? There are times when this is appropriate, but not many. At this stage the editor should be able to make an informed call, based on personal experience and judgment. Are the authors at least as well informed about

the subject as the reviewers? Have the authors taken valid comments and suggestions seriously, or did they have no intention of making any substantive changes and just wanted to see the paper in print with minimal effort on their part? Peer review is rightly regarded as a cornerstone of how we advance our science, but it is review, not veto, and journals should ensure that good observations and data can still get published even if the interpretations and conclusions that go with them are not considered to be of the same standard. It is the authors who will be judged by a paper, very rarely the reviewers or editors.

Go back just 50 years and you will find that some of the most intense debates in the Earth sciences were on topics that look to us now as about as sensible as counting the number of angels that can stand on the head of a pin. Geosyncline theory springs to mind for example. My guess is that this observation will be just as true in 50 years time, despite the best endeavours of authors, reviewers, editors and publishers to ensure that now we only publish the truth. Fortunately, it matters not a jot: what matters is that we will have moved on.

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