COMMENTARY

Coping with the journal review process

Tapan K. Sengupta

Publishing in reputed journals is the ultimate aim of any researcher guided by an inner urge to express intellectual creativity and be acknowledged for that. In the classical fields, there is enormous pressure to publish (the famous last word!) in highly ranked journals to secure or protect few permanent jobs available to the growing number of researchers in a field. In emerging areas, the pressure to publish (the first word) has in some recent cases led to fraudulent practices. All these diverse pulls and pushes are supposed to be moderated by the prevalent practices of journal review process, guided by an editor. Interestingly enough, some fraudulent practices escape the journal review process. Today, actual publication is run professionally by publishers (as a commercial venture; by professional bodies or by a university press), while an informal chain of commands is created via the choice of editors who decide what eventually gets into the print, via the peer-review process.

The present blind, peer-review process is flawed. Think of a jury system, where the jury stands to gain by the eventual verdict! In most cases, reviewers represent competing schools of thought – as they are active researchers in the same field. It is naïve to expect objective views from reviewers who have vested interest. It gets worse, when a manuscript spots errors in existing theories/practices. A reviewer once wrote to this author that the work reported in a manuscript is correct in identifying a long-standing conceptual error, but it is highly unlikely that it should be via that manuscript – an open challenge of suppression! Of course, the same paper appeared in print in another journal later and was acknowledged by others!

Some editors/journals try to avoid this problem by offering authors cherry-pick their reviewers – ostensibly from the same school of thought. These reviewers may take a softer view on the shortcomings because of familiarity with the authors'/ their own work. The authors may also cite generously the reviewers' work on the topic. However, the authors cannot complain, if their manuscripts are turned down!

For blind-review process, usually an editor employs more than one reviewer and when conflicting reports are received, it is the editor's responsibility to take an objective view. According to an editorial in *Nature*[1], they do it often and in one instance, an editor championed a paper in the teeth of referees' resistance – that was acknowledged in the Nobel lecture by the author, Thomas Cech. This editorial records accounts of rejected papers (by *Nature*) which went on to win the Nobel Prize. This may soothe many ruffled feathers among readers, who have been spurned for less ambitious ventures, other than winning a Nobel!

It is not always that the editors muster sufficient courage and are more inclined to err in favour of reviewers. Recently, we received three reports from a highly ranked computational physics journal. In a similar experience as narrated by Prathap[2], one of the reviewers stated that the conclusions reached are either taught at graduate courses or they are already well-known and so the manuscript must be rejected. We would not have spent more than a year investigating issues, if this was true. The second reviewer praised the study as critically executed in terms of its main objective, but made some constructive suggestions to improve the work further, that should eventually benefit the research community in that area. But the most damning review was posted by the third reviewer who accused us of scratching superficially; not even aware of experimental studies listed in three references that settled the problem long ago. The reviewer added that we did not cite a method given in a reference with 250 citations! Actually in that reference, no such method is given! The editor rejected our paper. While we have been looking at this problem for decades, we were seemingly not aware of these experimental papers – an unpardonable situation! We obtained the papers to find that these were theoretical papers (not experimental!), using the same models that are contested in our manuscript. When we brought this to the notice of the editor, he provided the usual readymade answer: 'The three reviewers that looked at this paper are experts in the field. If they find the purpose of the paper unclear or miss essential points or do not recognize its true merits, one must conclude that the paper is not well written'. One would have appreciated courage from the editor by at least acknowledging the dishonesty of the third reviewer! Note also that according to the editor, the paper is rejected for not being well written – not for the reasons given in the negative reviews!

Or consider the following comments of the reviewer that we received from the same highest ranked journal earlier. 'Reviewer #1: The authors have addressed my questions reasonably well. Therefore, I have no concrete reason to hold up publication of this manuscript. Nevertheless, I must confess that I am not particularly enamored of the analysis and interpretation in this paper, nor am I 100% convinced of its correctness. However, I do not have time to check the analysis more thoroughly'. Based on these comments, the handling editor turned the paper down, simply because the reviewer was very 'busy' and could not check the analysis!

The above examples relate to situations when editors refused to act. The next example shows total inadequacy or competence of the reviewer and desperate attempts by the editor-in-chief to support the review. This happened when the handling editor-in-chief (a renowned professor of applied mathematics) sent our manuscript for a single review. The paper pertains to property of a numerical method with respect to the simple wave equation:

$$\frac{\partial u}{\partial t} + c\frac{\partial u}{\partial x} = 0.$$

This equation, having exact solution, helps in the study of dispersion and dissipation using numerical methods. We were looking at the well-known leaf- or leap-frog (LF) method based on a method of analysis that highlighted some special properties of the scheme, contrary to accepted idea in the literature.

The reviewer noted that the LF scheme 'has been thoroughly analysed and well explained over the years. I cannot find anything that is especially noteworthy with regard either to theoretical understanding or practical computing aspects. At the same time, the reviewer noted that

in several places, it seems to be brought into question whether the scheme really is unstable at the CFL limit. It is elementary to show that this is the case; simply use for the two initial time levels the values

…1 1 –1 –1 1 1 –1 –1 1 1…
…0 0 0 0 0 0 0 0 0 0…

and the solution will then grow exactly as the number of time steps taken, i.e. at an unbounded rate if the steps in space and time are reduced'.

In fact, the main aim of the work was to show that the method is stable and produces most accurate solution for that limit. When we wrote back for reconsideration, the editor-in-chief responded: 'Both the editor in charge of the review and the referee are senior mathematicians who are experts in numerical analysis. It is their opinion, which I agree, that your result about stability of the LF scheme at CFL = 1 is wrong. The referee gave an example of first and second level grid values which will grow without bound. Being a linear scheme, if the random perturbation is epsilon times these grid values, then the solution will also grow without bound. For a scheme to be stable, the solution must be bounded for ANY random perturbation'.

It is interesting that neither our analysis nor the numerical solution became unbounded as claimed by the reviewer and supported by the editor-in-chief! Subsequent rejoinder to the editor-in-chief from us noted politely that for the governing first-order equation, it is theoretically not tenable to prescribe conditions at two time levels! We also noted that the number strings produced by the reviewer are anything but random numbers! Hence we enquired if the editor would obtain a second opinion. The editor-in-chief went completely silent about these errors of judgement and started talking about probable reasons why our computational results did not show instability due to random errors, as we computed only for 10^5 time-steps and if we would have computed up to 10^{14} time-steps (perhaps taking few years of computing!), then we would have at least seen algebraic growth, if not instability. Here is a case that can be summed up as condemnant quod non-intellegunt (they condemn what they do not understand).

Above are mere illustrations that call for an urgent need to reform the publication process with many ideas in circulation, worthy of consideration. For the blind-review process:

1. Let the reviews be cross-reviewed by all the reviewers. This will increase the workload, but it will certainly make reviewers circumspect in their pronouncements and enhance the quality of journal publications.

2. Existing journals can create generously open forums for comments on already published papers. Once when this author pointed out mistakes in a published paper in a prestigious journal, the editor-in-chief wrote back saying that the paper should not be rebutted and let that remain in its obscurity; any further comments only glorify it! How wrong it was – as that paper was instrumental for its author to get a national award subsequently for the same work!

3. Remove the authors names before sending it for review. This will remove bias against groups or individuals and probably reduce self-citation. Adding 'star' authors to dazzle reviewers for enhanced acceptance will also be out of question.

4. Mandatory declaration of contributions by each author. Again this will remove 'star' authors and potential for fraud.

5. Instead of reviews performed singly, consider group-reviews, specifically for papers in multidisciplinary areas.

A need for the following open-review process has also been articulated[2,3]. Advantages of open peer-review process are:

1. The reviewers will be constructive and not sarcastic. This will eliminate obtaining reviews from graduate students.

2. Reviewers with mal-intent in suppressing contrary ideas will have to reveal their identity and remove scientific tyranny[2].

3. The efforts of reviewers will be acknowledged by printing their name in the published paper.

4. The editors will be more proactive while mediating contentious issues and eventually the final journal paper will be a combined effort of the authors, reviewers and the editor.

In addition, some new publishing ideas are already being practised:

1. Adopt a 'publish first – judge later' model followed in the on-line journal, *PLoSONE*, from Public Library of Science, that allows the users to annotate a paper with comments. This also allows the authors to improve or correct their paper.

2. Follow the trackback system, as used in ArXiv (on-line physics preprints hosted by Cornell University) that allows on-line discussion on pre-prints.

Finally, one must consider that needlessly rough reviews can turn-off the young generation from research altogether. See, for example, the letter to editor of *Physics Today* at http://www.aip.org/pt/vol-56/iss-1/p71b.html, that narrates a specific case. The present exercise is specifically addressed to young researchers to inform them that actual research is an enjoyable pursuit, though publishing its results may not always be that simple! Specifically if you have some fundamental ideas to communicate from a non-glamorous geographical location. To alleviate, there is a need to have more good journals with committed reviewers, whose efforts will be acknowledged in print and recognized by their employers.

1. Editorial, *Nature*, 2003, **425**, 645.
2. Prathap, G., *Curr. Sci.*, 1989, **58**, 1114.
3. DeCoursey, T., *Nature*, 2006; http://www.nature.com/nature/peerreview/debate/nature04991.html

Tapan K. Sengupta is in the Department of Aerospace Engineering, Indian Institute of Technology Kanpur, Kanpur 208 106, India. e-mail: tksen@iitk.ac.in