# Peer review: can it really work?

Daniel Liberzon

We all want our research papers to be published. We all from time to time have trouble publishing our papers. Since we rarely think that our paper was not good enough to be published, we usually attribute the trouble to imperfections of the peer review process. Thus the topic of peer review is very dear to our hearts.

It is certainly true that the peer review process is imperfect. Sometimes it works smoothly, and sometimes it doesn't. Over the past few years, I've had numerous conversations with colleagues on this topic. Most of these conversations are concluded by stating that nothing can really be done about this. But is this really true? Perhaps something *can* be done to improve the system?

This note offers some observations and suggestions regarding peer review. Some of them are intended to be constructive and serious, while others are simply meant to provoke further thought and debate. Most of the ideas discussed below are not my own but came up in conversations with other people. However, since I cannot hope to recall all these people accurately and cannot be sure that they would like to be quoted here, I will present everything on my behalf and take all the blame. Actually, some of the procedures suggested below are already being implemented here and there, but not as widely as they could be.

In the game of peer review, there are three kinds of players: the author, the (associate) editor, and the reviewer. I invite you to take turns stepping into the shoes of each, so that we can discuss various issues from all relevant perspectives.

## The author's rights

The author spends more time and effort writing a paper than the editor and the reviewer spend handling it (in most cases anyway). Also, without the authors' contributions there would be no journals and conferences and we would have nothing to discuss. So it seems natural to take the author's point of view first and think about what the author can expect from the peer review procedure.

Clearly, the author must have certain rights. As the name "peer review" indicates, the author's work should be evaluated by reviewers whose research credentials and interests are comparable with those of the author (and, you might add, compatible with the level and scope of the journal). In other words, the reviewers should be sufficiently qualified to understand the paper and comment on its novelty, correctness, significance, and suitability for the journal. Is this always the case?

We might note that there are other contexts in which the term "peer review" is used and where it is arguably better formalized. For example, when a faculty member at University X is being considered for promotion, recommendation letters are solicited from "peer institutions". In most cases, there is actually a list of institutions that are considered to be peer by University X. Also, a letter from a more reputable institution might be given a higher weight in the decision-making process.

If we go back to the peer review in journals and conferences, things get more fuzzy. It is expected that the editor will find reviewers with suitable qualifications. Exactly what this means is not always clear. Also, the editor might not succeed in this task (for reasons to be further discussed below). In fact, editors

are often put in a desperate situation where they are happy to just get comments from anyone. As a result, the quality of reviews that the editor is able to collect can vary quite dramatically. Since the reviews are anonymous and the comments sent to the author are often brief (or absent altogether), there is no way for the author to be sure that they came from sufficiently qualified individuals.

The above analogy between paper review and promotion review is not to be taken too literally. For example, a knowledgeable and diligent graduate student can provide an excellent review for a paper written by a senior faculty member, and we cannot always expect a good outcome in the reversed situation. Nevertheless, it must be recognized that not every reviewer is capable of correctly evaluating the contribution of a paper. It is *not* sufficient that the reviewer work in the same research field as the author. If a reviewer from the same field is unable to understand the paper, it is possible that the paper is not very well written—but it is also possible that the reviewer is not sufficiently qualified (or didn't try hard enough).

So, suppose that the editor collects reviews showing lack of understanding and (hopefully) recognizes the problem. The question remains, what to do? Obviously, collecting more reviews might help. There is one more course of action which is not taken often enough by the editors: contact the author and give him/her a chance to respond *before* making the decision. I do not see why the author should not be able to provide input between the time the paper is submitted and the time a publication decision is made.

Let's say that you are the author and you have just received reviews of your paper. You read the comments and, in your calm professional judgement, they do not do justice to your work. That is, there are important technical points which you can precisely identify and which the reviewers got wrong. Should you contest the decision? Think of the following analogy: when a cop pulls you over for a traffic violation and hands you a ticket, it says right on the ticket that you have the right to contest the charge (within a specified time period). I think the letter from the editor should contain similar information. (After all, driving is not our primary job, and let's face it: the cop is usually right. Not so clear with peer review.)

Of course, authors also have responsibilities in the peer review process. They can avert many problems by writing papers carefully in the first place. But that is a topic for another article.

## The editor's tools

It is clear from the above discussion that the editor plays a crucial role in enforcing the author's rights in the peer review process. Editors are qualified and committed individuals who take considerable time to do this difficult job. When the procedure fails, it is certainly not due to the lack of good intentions on the part of the editor. What should be examined, therefore, is whether the editors have sufficient tools available to them. While some tools do exist, they could probably use considerable improvement.

Many journals and conferences maintain profiles of everyone who wrote or reviewed a paper for them. Such a profile contains keywords describing the person's research interests, along with other relevant information. These profiles can be accessed by editors to help them find suitable reviewers.

One feature of a typical reviewer profile is that it has no "memory". That is, it contains fixed data which only gets updated when, for example, the person's address changes. A way to build memory into the system is to update the profile every time the person writes a paper or a review.

Many of you have heard the following argument. Assume that an average paper requires three reviewers. Then, to keep things in balance, each one of us has to do three reviews for every paper that we write (this number can be smaller for papers with several coauthors). Now, imagine that a journal keeps a score in your profile by adding 1 for every paper you review for this journal and subtracting 3 (divided by the number of authors) for every paper you submit to the same journal. To be allowed to publish your work in the journal, you must make sure that your score does not stay too negative for too long.

The above scheme is very basic, and more features can be added to it. Instead of using a simple counter, the editor might assign a grade to your review. Then a competent and helpful review would improve your standing, while a superficial and biased review would rightfully hurt you. Such databases can be shared among different publications to balance reviewers' loads, although each journal might give priority to reviewers who have published in that journal (preferably in the same paper category). Many other features can be envisioned, and we could spend a long time debating whether they might be helpful or realistic. The point here is to try to motivate potential reviewers to be more responsive, thus enabling editors to better enforce the author's rights.

There is one route taken by some authors which places an extra burden on the review system. It consists in submitting to a journal a paper rejected by another journal, without revising the paper and as if it were an original submission. Resubmission "as is" is appropriate only if the sole reason for rejection was that the paper did not fit the scope of the journal. Otherwise, the comments of the previous reviewers should be taken into account by the authors before resubmission. Or, if the authors feel that the reviews were unfair, perhaps a better course of action for them is to contest the decision taken on the original submission. When resubmitting a paper previously rejected somewhere else, the authors should be required to furnish this information to the editor. The editor should be able to access the reviews collected by the other journal and then decide how to proceed.

The author can greatly help the editor choose appropriate reviewers by suggesting a few names. Authors sometimes do it, but this possibility is rarely advertised to them and remains underappreciated. Many authors are unaware of this option and of the fact that editors would like to receive such suggestions from them. In fact, editors often look for clues in submitted papers—the list of references being the most notable example—that would help them identify suitable reviewers. It would be helpful if authors were *required* to provide names of potential reviewers in the cover letter accompanying the paper submission. Of course, it is up to the editor to accept or ignore the author's suggestions.

## The reviewer's responsibilities

Whatever we say or do, the outcome of the peer review process still depends heavily on the good will of reviewers. These are volunteers who are busy with research, teaching, administrative duties, and other things. Thus it is important to acknowledge that potential reviewers have the right to protect their time and cannot always be pressed. However, if approached with the right attitude, the task of reviewing papers is not a distraction but rather an integral part of the research process.

To stay informed about progress in our fields, we all read papers by other researchers. It is reasonable to suppose that you read several papers for every paper that you write. When you review papers, you do the same thing—only earlier, before these papers appear in print. True, due to your efforts some of these papers will never appear. This means that you're doing service to your colleagues and yourself by maintaining the prestige of the journals in which you publish.

Contrary to what some people believe, you cannot be expected to review a paper just because your name appears in the references. You can (and probably should) decline a review request if you don't feel completely comfortable with the material of the paper. It is okay to say "no" if it's one of those times of the year when the pile of papers on your desk is higher than usual. But it's still hard to believe that for every paper you write, there aren't two or three new submissions that will be *interesting* and useful for you to review (of course, it is the editor's job to get them to you). And if you're thinking why you should waste an afternoon reviewing someone else's paper when you can prove a theorem instead, think about the time you will waste when you submit your paper and receive incompetent reviews.

Now let us say that you agreed to do a review. You are supposed to evaluate the paper's correctness,

novelty, and quality of presentation and organization. However, many reviewers find this boring and cannot resist the temptation to pass their subjective judgment on the paper. This is potentially a very dangerous thing. A paper that some people will find uninteresting might be fascinating to others. It is much safer to let readers decide for themselves, rather than attempt to predict everyone's opinion.

It is clear that if the paper has substantial technical flaws, or its findings are already known, or it is badly organized and not understandable, then it should not be published. Everything else is a gray area. If the main result is a straightforward modification of a known one in a slightly different setting, with no new tools or ideas introduced in arriving at it, then the contribution is probably not significant enough for publication. However, the proof doesn't necessarily have to be long and complicated for the result to be substantial. Sometimes a paper that solves a problem by borrowing a technique from another field (or a combination of techniques from several fields) can have great value, especially if the problem is well motivated. I think many people will be happy if journals publish only technically correct, well-written papers containing new results which may or may not be interesting.

There are quite a few papers and even books available which discuss recommended standards and procedures for reviewers and identify various logistic and ethical issues associated with the reviewing process. To get started, the reader can check the articles [1], [2] and the references cited therein. (I thank Dennis Bernstein for calling my attention to these references.) It is somewhat unfortunate that many of us review papers on a regular basis but have not had a chance to study this body of literature. This is a part of a bigger problem with the traditional engineering education, in which courses on topics such as engineering ethics are only now starting to find their place.

A pessimistic point of view is that the above discussion is useless and volunteer reviewing will never be reliable. I hope this is not true. But if it is true, maybe reviewers don't have to be volunteers? Maybe journals could consider appointing large editorial boards which would conduct the review process from the beginning to the end? Something like this is done by program committees of smaller conferences and workshops, and it seems to work quite well. This might have the danger of making things too political, though, and so I will shy away from this topic.

## Conclusions

I have attempted to examine some aspects of the peer review process from the author's, the editor's, and the reviewer's points of view. The division of issues into three categories is obviously quite artificial. After all, the three people are really the same person on different days (or even different parts of the same day). So the system should really be discussed as a whole. I hope that this note will prompt some further discussion and increase our collective awareness of the issues. While the system cannot be changed overnight, some incremental steps toward improving it can be considered.

It is often argued that procedures such as the ones suggested above are not feasible because they would increase the already long turnaround time. In this regard, it is important to make a distinction between different publication media. Conference proceedings are intended for fast dissemination of current, often preliminary results. The internet offers us many ways of getting our results out even faster. Journals, on the other hand, primarily serve a long-term archival purpose. In this case, the quality of the review procedure is probably more important than the quantity of time it takes.

If nothing else, this note is another experiment with the peer review procedure. As I am writing it, I don't know if it will ever be published.[1] But that makes things more interesting, doesn't it?

[1] A note added later: It never was.

**Acknowledgment.** The author is grateful to Tamer Basar and Dennis Bernstein for stimulating discussions.

## References

[1] D. S. Bernstein. Peer review. *IEEE Control Systems Magazine*, 20:8–11, June 2000.

[2] A. J. Smith. The task of the referee. *Computer*, 23:65–71, April 1990.

The author can be reached at `liberzon@uiuc.edu`.