

Review-by-Few or Review-by-Many?

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ABSTRACT

It's clear that traditional forms of academic publication are rapidly becoming obsolete. It's also clear that peer review is a seriously flawed process that could be much improved upon by modern methods of communication. The primary flaw in the peer review process is that it gathers the opinions of a small group (two-to-four) of individuals, and attempts to make a decision based on that sample. It is my contention, that, except in rare cases, this decision is based on too small a sample to be valid. In this opinion piece, I suggest a method for gathering reviews from a wider group of people with the aim of improving the decision-making process.

INTRODUCTION

As academics, we find ourselves at a strange point in history. We have dedicated ourselves to learning, teaching, and to accumulating and disseminating knowledge. Yet, it seems that the very tools that we have used, seemingly for ages, are disappearing before our eyes. In many fields conference publications have supplanted journal publications as the primary means of professional communications. The best conference publications sometimes are published in archival journals, but more often, the conference paper is the final form of the paper. Conference proceedings that used to be published as semi-archival volumes are now published on CD ROM or electronically. Indeed, in many fields, electronic preprints have supplanted conference publications as the primary means of professional communication.

And what of the book? Books were once the mainstay of literacy, the cornerstone of modern civilization. When was the last time you read a book for information? Who needs an encyclopedia when you have Wikipedia? Who needs a cook book when you've got recipies.com? Who needs a math reference when you've got MathWorld or integral-table.com? Virtually anything you want to know is available on the World Wide Web, and you can find what you want in mere seconds without searching through stacks of journals, books, and other printed media. Of course, some of the information you obtain from the web is not particularly reliable, but if you're a sensible person you can usually filter out the fact from the fiction.

To be sure, this is no tragedy. Fast, reliable communications, vast repositories of information and instantaneous searches are a researcher's dream come true. Nevertheless, the one thing that gets lost in all of this is peer review. As academics, we have a need to prove that our work is worthwhile. At the very least, we need to get tenure and we need to get promoted. Our annual reviews are based on the number and quality of our peer-reviewed publications. Peer review is (or is supposed to be) the stamp-of-approval that certifies that a work is original and not a copy or trivial extension of some other work, that it is correct – that we didn't add two plus two and get seven, and that it is somehow a worthwhile addition to the body of human knowledge.

Unfortunately, the ideal of peer review is far from the reality. Rather than being the last bastion of truth that separates good science and nonsense, it is a veritable lunatic asylum and the lunatics are definitely in charge. Let me cite a few examples. I have published many peer reviewed papers, and I have served as both a reviewer and as an editor so I am intimately familiar with the peer review process. To be sure, there are instances where it works well, but these tend to be the exception rather than the rule. Here are a few of the types of individuals I have encountered both as an author and as an editor. These are examples taken from real life with the names changed to protect the guilty. Despite the ironic way I tell these stories, each one is taken from a *real review*.

A FEW BAD REVIEWS (AND REVIEWERS)

The Rubber Stamp.

The Rubber Stamp is an agreeable person who responds positively to every request for a review. Unfortunately, he really doesn't like reading papers, so he puts it off as long as possible. When the review is finally due he either scans the paper lightly, reads only the abstract, or doesn't bother to read the paper at all. Of course he's an agreeable person who would never consider recommending rejection. He writes a bland review containing generic statements like "Excellent paper, I really enjoyed it," and recommends acceptance. The paper, unfortunately, was written by a lunatic who believes that all Science Fiction is real. The references include citations from several Star Trek episodes, a couple passages from the Encyclopedia Galactica, and a few papers that exist only in the author's imagination.

The other reviewers don't bother to send in their reviews, so the editor must make a decision based on a single review. Of course the editor is much too busy to actually look at the paper so he passes it along to the EIC with a recommendation for acceptance. The EIC is also too busy to look at the paper, so it appears in print with everyone looking like an idiot.

The Detective

The detective believes that all authors have a hidden agenda and that it is his sworn duty to expose the author's chicanery. Of course, he is much cleverer than any mere author, so he quickly spots the odd turn of phrase and the hidden misdirection that conceal the author's true intent. He triumphantly recommends rejection of the first real solution to the P=NP problem.

King for a Day

If you knew this guy you'd wonder why anyone would ask his opinion about anything. The chance of him ever writing anything for publication is nil. He knows that nobody respects him, but somehow he was asked to review a paper. Now is his chance to show the world that he's smarter than those bozos who write papers. He points out the mathematical error on page 5 ($2+2=4$? I don't think so!) He notes that only an idiot would use a variable name like X when Z is so much more appropriate. He gleefully recommends rejection of the first valid unified field theory paper.

The Curmudgeon

The Curmudgeon has written only one good paper in his life. It really is a good paper. In fact it's considered to be one of the key papers in the field and any new paper in the field really ought to reference it. The editor, seeing the reference, sends the new paper to the Curmudgeon who never turns down a review. His reviews are all the same, "Why would anyone be so stupid as to try to publish *this*? 'The Curmudgeon' (using his own name, but speaking in the third person) solved this problem years ago." He viciously recommends rejection.

All progress in the Curmudgeon's field has come to a halt, even though there are many interesting and useful problems to be solved.

The Turf Warrior

The Turf Warrior is a cousin to the Curmudgeon. He doesn't like people horning in on his field, so he routinely recommends rejection of any paper that is even close to his field of expertise. He's not a bad man, he just believes, in his heart of hearts, that nobody can do the job as well as he can. Progress in his field is slow or non-existent.

The Club President

The Club President works in a research area that is "out of the main stream." When he receives a paper for review, he first looks at the author's name and checks to see if it is on his personal list of "acceptable authors." If not, he writes his standard rejection, which reads in part, "While we truly appreciate Dr. Bullfrog letting us know what he's been up to, ..."

Because of The Club President's attitude, nobody knows, *or cares* what's going on in his field.

The Clairvoyant

The Clairvoyant is well-meaning individual, but he truly believes that the Berzwack algorithm is optimal for problem Q. He sees no point in exploring other approaches. If an author suggests a new solution to problem Q using anything other than the Berzwack algorithm he will recommend rejection of the paper. The Clairvoyant does not believe in looking at experimental results.

Because of him, no new solution to problem Q has appeared in decades, even though all known solutions are far from optimal.

The Drowning Victim

Professor Jaundice has just received a paper to review. The paper is quite challenging and would be a difficult read even for the top experts in the field. Professor Jaundice, however, does not like reviewing papers. Nevertheless, he believes he *should* review papers, so, he accepts the review, and passes it along to one of his graduate students. The student (i.e. the Drowning Victim) has just obtained his BS degree and cannot make head or tail of the paper. Even though he is a well-meaning individual, he is also human. It is human nature to say, "If I can't understand this, it must be nonsense." Most people never question their ability to understand *anything*. He recommends rejection of the paper. Professor Jaundice passes the review along to the editor without further comment.

The Reference Collector

At many of the more prestigious institutions, mere publication is not enough for promotion and tenure. It's also necessary that your publications be referenced. The more references the better. When the Reference Collector gets a paper for review, he immediately turns to the reference section looking for his name. If he sees it, he recommends acceptance. If he doesn't see his name, he adds one of his papers as a mandatory reference, and recommends acceptance with mandatory changes. He doesn't bother to read the paper.

Mr. Cut-Off-Your-Nose-To-Spite-Your-Face

The Reference Collector has a cousin called Mr. Cut-Off-Your-Nose-To-Spite-Your-Face. He also consults only the reference section. If he doesn't see his name, he feels that he's been slighted and angrily recommends rejection. It never occurs to him to recommend adding his paper as a reference, even when such a recommendation is legitimate.

The Savant

The Savant believes that all new knowledge comes to us from Bunwucky State University, where he is employed. He has received two papers for review one from Bunwucky State entitled "Exciting New Variable Names for the Quicksort Algorithm" and one from some other place containing a legitimate three-line proof of Fermat's Last Theorem. He enthusiastically recommends acceptance of the Bunwucky State paper, and recommends rejection of the other paper with a review that reads "Fermat's Last Theorem is a difficult problem that is best left to the experts. We recommend that you redirect your efforts toward problems more suited to your level of intelligence."

Etc.

I could go on, (and on and on) but you get my point.

REVIEW-BY-FEW OR REVIEW-BY-MANY?

Speaking as a former editor, I can say that obtaining good reviews is an excruciatingly difficult problem. The top people in most fields routinely reject all review requests saying that they are too busy. Those who do accept the reviews often pass them along to unqualified students. Few reviews are done by the author's peers. They are done either by unqualified students or by people whose main qualification is that they agreed to do the review after the first twelve people refused. Even when the reviews are good, the process is still chancy. In one case I received two excellent reviews for a paper and was about to recommend acceptance to the EIC when the third review arrived. The third review consisted of a single reference to an identical paper that the authors had published elsewhere. If I hadn't been lucky enough to select a reviewer who had actually read the other paper the authors would have gotten away with a duplicate publication.

In too many cases, individuals who promise to do reviews don't do them. I call this type of reviewer "The Black Hole." Once the paper has been sent to him, no further communication is ever received. He not only won't send you the review, he will also refuse to communicate with you in any way shape or form. When this happens, your decision on the paper is bound to be based on insufficient information.

Finding reviewers in new fields is agonizing. Very often there are no peers, so you try and find people in related

fields. All too often the reviews you get are "Drowning Victim" type reviews which are of no help.

One of the primary problems with peer review is that papers are reviewed by a select group of people chosen by an editor. There is no guarantee that this select group of individuals will give you a reasonable set of opinions on the paper. Although it seems good in theory, it doesn't work in practice.

But the biggest problem with the peer-review system is that there almost always no appeal. I have editors explain to me, very kindly and carefully, that even though I raised objections to every one of the reviewers' negative comments, it couldn't possibly change the fact that my paper was rejected. Even if you are lucky enough to have an understanding editor, the best he can do is send the paper out for another set of reviews. He's probably pretty far down the list of acceptable reviewers already, so the new reviews will usually be even worse than the first ones.

So how do we fix the system? We don't. We get rid of it. I believe that all papers should be published electronically with *no initial review*. Once the paper is published we should then collect comments from anyone who wants to submit them. The author should be able to respond to negative comments and should be able to revise the paper to correct deficiencies. Reviewers should be able to revise or withdraw their comments. And most importantly, reviewers should be able to comment on each other's reviews. I once reviewed an excellent paper for a well-known journal and enthusiastically recommended acceptance. I was stunned when the editor rejected the paper based on another review. After reading the other review, I realized it was seriously flawed and wrote a letter to the editor detailing the errors. The editor's response was: "Dear Dr. Maurer. I understand that you are upset that your paper was rejected, but I assure you it was reviewed with the utmost scientific accuracy" Meanwhile, back on *this planet* ...

I don't believe in anonymous reviews. People ought to stand behind their remarks take responsibility for what they say. All too often, reviewers treat the cloak of anonymity as a license to be nasty. Nevertheless I acknowledge that some form of anonymity might be needed to protect reviewers from behind-the-scenes personal attacks. The simplest solution to this is to require all reviewers to have a single on-line "handle." This would make it possible for everyone to identify individuals who continually make inaccurate and unfair comments, and distinguish them from people who are generally fair.

Of course, reviews written by anyone and everyone could be fragmentary, and potentially wildly inaccurate. Despite this, I believe that it would be relatively easy to distinguish the good reviews from the bad and assign a

rating to the paper based on them. The reviews could be as long or as short as the reviewer wished them to be, with no deadlines and no pressure to complete them. This is a *review-by-many* paradigm which I believe cannot help but be a major improvement over the current *review-by-few* model.

WHY PUBLISH AT ALL?

But perhaps the very idea of publishing is an anachronism. Perhaps a better paradigm would be for scientists and other researchers to communicate by E-Mail and abandon formal communications entirely.

I don't believe that this is a good idea. Preparing documents for formal release demands a level of care that is absent in informal communications. You can't just say that you believe something is true, you must formulate your thoughts as a series of lemmas and theorems and provide proofs for each one. Going through this process forces you to work out the bugs and clear up any errors or glaring omissions.

By the same token, preparing experimental data to prove your hypotheses, or to demonstrate that your algorithms are better than others, forces you to demonstrate the correctness of your ideas. Without the concept of formal publication there would be little incentive to prove anything. Errors would accumulate and scientific progress would be severely impeded.

But is there any reason to publish anything anywhere than on my own website? I think the answer to this is also, "yes." I need to know that my work is of value, and some kind of formal feedback is the only way I'm going to be certain that this is true. Furthermore, I want electronic search engines to put my papers on page one, or at least somewhere the first three pages. If the paper is on my own website, the chance of this happening is less than if the paper resided in some formal archive. Furthermore, a publication in a prestigious electronic archive is more likely to impress my dean when he is thinking about tenure, promotion or annual raise.

ELECTRONIC ARCHIVES

I believe that there should be electronic archives that essentially replace journals, conference proceedings and other forms of formal publication. Furthermore, I believe that these electronic archives should accept *everything* that is submitted to them, as long as the subject of the paper is appropriate to the mission of the archive. The mission of these archives should be much broader than that of a typical scientific journal. I would imagine an electronic archive that would comprise all of the publications of a professional society rather than a

collection of papers on a narrow topic. The archive would, of course, be subdivided into subsections of papers on specific topics. There is no reason why the subdivisions should be at the level of current journals, but this would probably be a good starting place. It should also be possible for authors to create new subdivisions (subject to editorial review) giving an instant venue for publication of results in new fields.

Papers should be given a quality score based on the number of times they have been accessed, the number of times they have been downloaded, a voluntary score (+3 to -3) assigned by readers, and voluntary reviews done by individuals with author responses. Authors could continually revise their papers to respond to reviewer comments and improve their scores. Inappropriate papers could be voted out of the archive by a sufficient number of votes, but perhaps kept in a "slush" archive for a time just in case the vote was incorrect.

This model would allow papers to be reviewed by many individuals, and confine reviews to those people who genuinely wish to write them. Papers would move up in the archive based on the quality of their reviews, and the responsiveness of the author to suggestions for improvement.

CONCLUSION

It's clear that traditional forms of academic publication are rapidly becoming obsolete. It's also clear that peer review is a seriously flawed process that could be much improved upon by modern methods of communication. As time progresses, traditional journals are becoming less and less a means of professional communication and for archiving significant scientific results, and more a mechanism for obtaining promotion and tenure by academic researchers. I don't believe that tenure and promotion is a sufficient *raison d'être* for the continued existence of journals. I believe that there are better mechanisms for distinguishing good papers from bad, and that these mechanisms can serve as a filter for electronic archives and as a substitute for peer review in promotion and tenure decisions.

The ideas presented here are the product of long discussions with many other people over many years. I thank all of these individuals for sharing their ideas with me. However, the views expressed here are purely my opinions. Because I am an academic, I am aching to provide a few references to "prove" that my opinions are shared by many others. But you already know this is the case, or you wouldn't be reading this. The blame for any exaggerations, inaccuracies or downright errors lies entirely with me. I invite all comments (Peter.Maurer@Baylor.edu).