

Three Views of Peer Review

The ways in which mathematicians communicate their work are evolving in response to technological advances. The *Notices* published an article about electronic journals in the October 2000 issue and an article about electronic preprint servers in the January 2002 issue.

A traditional part of the publication process is refereeing. What is peer review today? What should it be? How might it evolve? The *Notices* asked three individuals to write short pieces on this theme; these pieces appear below.

—Harold P. Boas

Steven G. Krantz

One of the gratifying things about being a mathematician is that one exercises a considerable amount of control over the world we live in. The typical mathematician has a lot to say about the curriculum in the mathematics department, about which students are admitted to the program, and particularly about what goes on in the classroom. Even better, most of us have a considerable slice of time to pursue whatever scholarly interests may capture our imaginations. It is really a pretty good life.

The one painful part (unless you are a department chairman; then there is constant pain) is getting one's work refereed. Let us concentrate on the peer review of papers. I am the managing editor of two journals and associate editor on two more. I like being an editor. The process allows me to help others to write better and to improve the quality of journals that I care about. What is more, I can (in subtle ways) shape the subject by what papers I publish and what papers I do not.

I could not possibly carry out this process without the aid of referees. I have neither the time nor the expertise to judge all the papers that come across my desk. I must have help. And there is really no alternative but peer review. The work of the automated theorem-checkers notwithstanding, it

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is still necessary in our world to have an expert with solid judgment review the work. There are those who will argue that we should just throw everything up on the Internet and let the world sort things out. Well, power to the people and all that. But such a system cannot work. There are already more than 50,000 papers published each year. In fact, that is merely the number that *Math Reviews* handles. The full number must be many more, and if there were no refereeing, the number of papers would be incalculable. If I lived in a world where there was no refereeing, I would probably just gravitate to the work of people my age whom I know and trust. I would have neither the leisure nor the patience to seek out the work of new, young mathematicians to see what they have to offer. And this would not be fair to the neophytes in the profession. The refereed journals help me to see what is worth considering and what is not.

If we had no refereed journals, how would deans judge tenure cases? This is a very serious problem. Deans are already quite skeptical of a journal that lives on a PC on someone's desk. A dean whose pedigree is in Celtic history is probably something of a Luddite and is (in my experience) likely to be most comfortable with traditional, hard-copy, refereed journals. Most deans are usually advised by a tenure and promotion committee of about a dozen faculty from all over the campus. These will be senior people, most of them about as conservative as the dean. If a chairman must sell the dean and his committee a whole new medium and set

of values before presenting a tenure case, that chairman will be strictly out of luck.

We use refereed journals as the yardstick by which we measure quality. There is no way in the academic world to throw the spaghetti against the wall and see whether it sticks. Somebody has to do the work and take the responsibility and make a judgment. It is sometimes agonizing and always a burden, but we must do it. That is the only way to maintain our sense of scholarly excellence and our communal sense of purpose.

Nothing in life is perfect, and the peer-review system has many flaws. First, everyone is being asked these days to perform more tasks with less time and fewer resources. When an editor asks someone to referee a paper, it gets thrown into the old work pile and immediately forgotten. The typical scenario is that once a month or so the mathematician picks the paper up for fifteen minutes, flips through it, and says, "Yeah, I've got to get around to this one of these days." Since the editor is 2,000 miles away and not in the putative referee's face, the task gets shuffled to the bottom of the "To Do" list. Second, referees (like all people) have their foibles, and these manifest themselves in ways that can be unfair. A paper by a famous person always gets special deference. A paper by a newcomer at a non-prestigious university may be judged particularly harshly. If a paper reflects on or scoops or does not adequately reference the referee's work, that referee is liable to react emotionally. A paper on a non-fashionable topic may have a tough time getting a fair hearing.

But that is why we have editors. A good editor manages the refereeing process. The editor can tell when things are not going well, when the referee is not getting around to doing the assigned task, when the author is not being treated fairly. The editor can then make adjustments. A good editor can tell when a referee's report is reliable and trustworthy; if there is doubt, then the editor will get other reports. In extreme cases the editor may actually read the paper. In most cases the referee makes a recommendation for acceptance or rejection of the work, but the editor puts that recommendation together with other information (including possibly other reports) and renders the final decision.

What is a referee supposed to do when asked to judge a paper? Of course, ideally, the referee reads the paper word for word and checks every detail. I can assure you that in most cases this does not happen, and it is not practical to expect it. If the author/paper is purporting to prove a famous conjecture, then *everyone* is extremely careful, because it would be mortifying for all concerned to publish an incorrect proof of the Riemann hypothesis or of the Poincaré conjecture. But for most papers what is reasonable is for the referee (presumably a recognized expert) to spend a few hours checking the key

ideas, the exposition, the quality of the proofs, and the overall credibility of the paper. In the long run, just as the populists claim, the entire world judges scientific work, and this takes place over a period of years. The referee cannot serve as a surrogate for the entire scientific community. Instead, the referee serves as a sieve, so that only worthwhile material is put before the readership of the journal in question.

To come to the point: The peer-review system is all we have, and it will work well if we all participate responsibly and manage it properly. It is not a system that works by itself. It requires knowledgeable and experienced people to run it and adjust it and perhaps to periodically reinvent it. During my career we have experimented with blind refereeing, double-blind refereeing, and many other variants of the tried-and-true formula. And we will continue to try new things. The peer-review process is alive and well, and it is our collective responsibility to nurture it and maintain it—for the sake of our subject and our profession.

A Proposal to Reform Peer Review in Mathematics

Greg Kuperberg

Over the past twenty years personal computers and the Internet have led to great changes in scholarly communication in mathematics. Email has replaced paper mail, TeX now dominates typesetting, *Math Reviews* and *Zentralblatt* have gone electronic, and many math research articles are available from the arXiv [4] and other online sources. These technological reforms have in turn affected the social and professional organization of the mathematical community. Yet in the midst of so much change, the procedures of formal peer review, as practiced by refereed journals, have changed very little; they are about the same as they were fifty or even one hundred years ago. Some people argue that peer review is so important to research in mathematics that it should not change, at least not any time soon. But I believe that peer review will inevitably change along with all other aspects of mathematical communication. Here I will describe my proposal to reform peer review, in particular to use the arXiv to leverage reform.

In the idealized journal system, the diligent referee first checks the main results of a submitted

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This piece is derived from "Scholarly mathematical communications at a crossroads," by Greg Kuperberg, Nieuw Arch. Wisk. (5) 3 (2002), 262–4.

paper. If they are correct, the referee and the editor then consider whether the results meet the journal's standard. In practice the system is far from ideal. Referees cannot be held accountable (although some do an admirable job anyway). Many papers are accepted on the basis of name recognition or out of guilt, just because the referees sat on them for too long. Authors need not take no for an answer, because they can scout for journals that will publish them. Journal editors should serve as a second line of defense, but in practice they are only slightly more accountable than referees. (Again, some do an admirable job anyway.) In the end, readers do not know who refereed any given paper or why it was accepted. Because papers can only be published once, the system reduces peer review to simple binary approval.

In my opinion, *Math Reviews* and *Zentralblatt* are inherently more useful forms of peer review, because the reviews are not anonymous. Their publishers do not agree with me; by their own rules, reviewers are not required to fully referee papers. But some do so anyway. Many mathematicians know of notoriously mistaken papers that were inexplicably published. The community is often angry with the referees of such papers, but anonymity protects them from the readers rather than the authors. Typically *Math Reviews* sets the record straight. My favorite example is the forthright review by Gábor Fejes Tóth of Wu-Yi Hsiang's inadequate proof of the Kepler sphere-packing conjecture [3]. Even in the usual case when both authors and referees do a good job, *Math Reviews* obviously informs readers more than any referee reports could, since the latter are confidential.

Enter the arXiv. The arXiv was created in 1991 by Paul Ginsparg as a service for high-energy physics. (Ginsparg was recently awarded a MacArthur Fellowship for his invention.) It has since developed into a large, permanent, freely available digital library with nearly 200,000 research articles in physics and 25,000 in mathematics. The arXiv has been described in detail in the *Notices* by Allyn Jackson [2] and elsewhere by others [1]. One feature of the arXiv which is relevant to peer review is that although most of its articles are contributed before they are refereed, it also supports several journals. A journal that pools its papers with the arXiv is called an arXiv overlay; one of the most successful overlay journals is *Geometry and Topology* [5].

In the presence of the arXiv it is relatively easy to reform journals so that they function much more like *Math Reviews*. If a journal is purely an arXiv overlay, then it need not take possession of its papers. So why should it wait for authors to submit to it? It could instead allow anyone to nominate ("submit") any arXiv article for review, whether or not it has been published elsewhere. For lack of a better name, I will call such a review service an

"open journal". If an open journal reduces selection to its utilitarian minimum, it should add some other value for readers to take it seriously. It is therefore natural for open journal referees to write public, nonanonymous reports like those in *Math Reviews*. (But reviewers who are not interested in a submission can reject it privately and anonymously.)

Open journals have been tried before, both in connection with the arXiv and elsewhere. But existing experiments lack a crucial feature: They are not designed to substitute for journal names in the author's list of publications. For this purpose an open journal should do three things. (1) It should retain the trappings of a traditional journal, such as an editorial board, an online masthead, and a bibliographic citation style. (2) It should keep the author informed of the status of papers under review. And (3) it should prod editors and reviewers to attend to submissions, as traditional journals do.

While many readers presume that referees check the results of papers, in practice editors would scare away their referees if they actually demanded this. This inconsistency is only tenable because refereeing is anonymous. My best idea to address the problem is to have the reviewers check one of three options:

1. I have checked the main results.
2. I do not doubt the main results.
3. I doubt the main results.

Some reviews might need to be cosigned to support option 1. Presumably option 3 would be rarely used.

It remains to be seen whether the open journal model will succeed. Or one might ask, Who will pay people to do all of the work? I see two reasons for guarded optimism. First, publishers pay the mathematicians who manage, edit, and referee traditional journals very little or not at all. Most of the paid work for a traditional journal is for typesetting and distribution. Most of the incentive for the mathematicians involved is in credit for professional service. In principle this incentive could also sustain open journals. Second, I think that once the arXiv is sufficiently established in mathematics, journals will naturally evolve toward the open model. But I hope that the mathematical community can plan this evolution instead of leaving it to fate.

References

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- [3] GÁBOR FEJES TÓTH, Review of Hsiang, "On the sphere packing problem and the proof of Kepler's conjecture", MR 95g:52032.
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- [5] *Geometry and Topology*, <http://www.maths.warwick.ac.uk/gt/>.

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Generally, a referee has to make one of the following recommendations:

1. Publish essentially as is: the only changes necessary are very simple typographical matters which can be changed by the editor.
2. Publish after author's minor revision: the referee suggests points which must be changed before the paper meets the standards for publication.
3. Publish only if the author makes major revisions. (Perhaps the paper is much too long or is badly written.) The revised paper will be refereed again.
4. Reject. There is nothing salvageable.

These "Hints for Referees" [2, p. 36] are all very well, but one might wonder just how to distinguish. So Don Knuth reminds referees that to be publishable:

1. The paper should contribute to the state of the art and/or should be a good expository paper. If it is purely expository, it should be clearly designated as such.
2. All technical material must be accurate. A referee should check this carefully.
3. The article must be understandable, readable, and written in good English style.
4. The bibliography should be adequate.

What Is Refereeing?

The unthinking view of the person in the street or, for that matter, of the mathematician in the corridor is that the purpose of a referee is only to certify the correctness and originality of an article's results.

However, no referee (unless, perhaps, assisted by able and energetic graduate students) can possibly guarantee that submissions are error-free or new. In practice, it is commonplace for old results to be rehashed—and a good thing too. Well known facts only become known well by repetition, and, in any case, occasionally a new proof of an old fact actually explains it properly. Republishing a result may add to "the state of the art".

Refereeing does provide an imprimatur alleging a paper has survived a certain rite of passage. More important, refereeing is a mechanism for selecting *preferred* papers from potentially *suitable* papers. Refereeing moderates the quality of papers accepted by a journal.

Correctness is primarily the province of authors, not of their referees. As referees we are authors' victims. We are not guilty of authors' crimes.

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An Editor's Duty

The notion that every submission must be formally refereed is absurd. There is no obligation on us as editor or referee to convince authors that they are surely in error. It's up to authors to compel us to the view that their claims are not necessarily wrong and, indeed, are likely to be correct.

Editors are responsible for more than just the final decision to accept a suitable paper. They must also recognize immediately that certain submissions plainly are unsuitable for their journal and dismiss such papers promptly.

Archiving Is Publication

One must of course make one's work available to the mathematical community so that the community may judge it and use it. It seems reasonable to believe that publication in a generally accessible, well-managed, and well-mirrored archive can do that adequately. The arXiv is a fine example of such an archive, inter alia because it is an exemplar of relevant current best practice [1]; see also remarks in [3].

Unconscionable Practices

Slow refereeing and severe backlogs in publication should not be accepted as normal. Many journals have editorial policies apparently formulated in 1953 and unchanged since.

By 1953 policies I mean a range of bad habits from such simple misdemeanors as failing to communicate by email and failing to invite response by email for preference, to absurdities such as paying for snail mail to return copies of a computer-generated manuscript to an unsuccessful author.

Let me contrast such nonsense with a recent sensible experience. An email invited me to referee a submission and suggested I might well already possess the preprint (I did not, but was able instantly to download it from the website of one of the authors). I was reminded of the very ferocious criteria that would have to be applied—occasioned by an atrocious backlog—and was asked merely to scan the paper to determine whether it might possibly meet those criteria. In practice, I read the submission carefully and provided a fairly detailed one-page report, including a certification that in my opinion the results alleged were unlikely to be false. In all, my effort took me an hour and a half (I can remember having taken longer to search my office for a mailed manuscript that I had immorally put aside until the next week), and the entire matter was resolved within the day. Of course, the suggestion that a "proper" refereeing job had not yet been done still grates, as does the fact that the sheer size of the rather good paper I had read meant it could not be recommended for publication. An electronic-only journal could have reacted differently.

Moderation

It is absurd to suggest that peer refereeing will or can disappear. Whenever one selects one manuscript but not another, for whatever purpose, a peer-review or peer-moderation process has intervened. Whenever one comments on a manuscript, no matter how incidentally, that remark is a peer's remark. It is reasonable for a nonexpert to ask that papers be, at least implicitly, differentiated by informed comment and other peer-moderation or selection processes.

Publish or Perish

It would be a good thing if the notion “peer refereeing” were to shed its ritual aspects, particularly if the only purpose of emphasizing those rituals is to humor tenure and promotion committees.

Nonetheless, it remains important to many of us to publish our work in refereed journals. In this context, a virtual *Journal of Archived Mathematics* à la [3], formally certifying certain archived papers by supplying those papers with referees' reports and, say, with a JAM format file for show-and-tell purposes, clearly performs a service.

Journals Are Here to Stay

Journals will remain with us, often probably as no more than virtual versions of their paper and bound antecedents. Selecting their contents brings up the traditional refereeing issues.

There is now and—all the more when almost all journals are electronic—will be renewed opportunities for traditional print books featuring “the best of. . .”

Formal peer refereeing will survive, but its strange rituals ought to become a diminishing element of informed comment on published work.

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