

Peer Refereeing . . . Will it be Missed?

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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' [1, p.36] are all very well, but one might wonder just how to distinguish. So Don Knuth reminds referees that to be publishable:

- a. 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.
- b. All technical material must be accurate. A referee should check this carefully.
- c. The article must be understandable, readable, and written in good English style.
- d. The bibliography should be adequate.

1 An Opening Discussion

The issue is that we have somehow to cope with a world in which it is easy and cheap to produce and distribute one's manuscripts. Thus 'publication' is yet more uncontrolled than it was traditionally. In this context, it seems absurd to insist that a paper has not been *published* unless it has suffered the ministrations of a peer referee; and this no matter how readily the paper is available nor how elegantly it has been formatted.

Most of us have been brought up to be polite at all times. That upbringing forces us to be yes persons, always acknowledging that our interlocutors might well be in the right. Below I introduce a no person, wantonly disagreeing with common understandings.

Y: Without peer refereeing papers would have errors.

N: Many published papers have errors. Mind you, I don't blame the referees. The author is primarily guilty of those mistakes. In any case, most errors are

only relatively minor sloppiness (though occasionally a trivial gap in a proof turns out to be a bottomless chasm).

Y: But if serious papers might have errors how can one rely on the literature?

N: If one uses a published result, let's call that hypothetical result 'ERH', without having the vaguest notion of the principles underlying its proof, one is in effect writing: "*Given* ERH, my claim follows by the following argument." It would make no practical difference to you, as author, if ERH were no more than a conjecture. Besides, the more important a result the more likely the paper has actually been read carefully, for example by an assiduous graduate student or post-doc.

Y: Without peer refereeing many old results would be republished.

N: Many old results *are* republished (and not infrequently the 'new' proof is less elegant than the old ones. Happily, an old result is sometimes published with a new argument that actually explains the result, rather than only proving it).

Y: OK. So refereeing as we know it isn't perfect. That barely matters. Referees mostly work for free. Refereeing doesn't cost anything and sometimes does some good.

N: No! The refereeing process is a *major cost* in the traditional publication process. The process is hugely time consuming both for editors, in finding and corresponding with suitable referees, and in the delays traditional in mathematics in obtaining appropriate reports.

Moreover, this cost — viewed as a cost per published article — is compounded by not only published articles incurring a refereeing cost but also those eventually rejected.

Sure, the traditional process appears to assume that the efforts of referees are of no cost to the mathematical community; that assumption presumes our time has no value (whether for ourselves or to our employers).

Y: But refereeing is vital! Publication in refereed publications is essential for career advancement and recognition.

N: One wonders whether the time might be ripe for a new approach in which career advancement relies on the quality of one's work and on the judgment of peers who have actually read and used it.

Y: But, but, You can't just let everything be published!

N: You cannot *stop* anything from being published. Have a look at the web. Besides, the problem (if it is indeed a problem) is much older than that. The photocopier thirty years ago made it easy to promulgate one's preprints.

Once people learned to speak they could announce their ideas, with no editorial control. Worse, once people learned to write they could spread their ideas even further.

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

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

2 The Exchange Continued

Y: It seems that a referee can only be asked to reject blatant nonsense or material that plainly seems unlikely to be true.

N: Certainly not. Normally, referees should *never* be asked to deal with ‘blatant nonsense’ or material that is fairly plainly inadequate. That's the editor's task.

Y: But it's the right of an author to have his submitted article refereed formally. It would be improper for an editor to make a unilateral decision.

N: Nonsense. An editor should deal politely with authors (“I regret to have to advise you that your submitted article is not suitable for publication in *My Journal*.”); but that's only so as to follow Aristotle's advice to be courteous to the weak. Beyond offering courtesy, anything else done for an author making a plainly inappropriate submission is purely gratuitous.

On the contrary, moreover, I insist that it is improper on an editor's part, and is a gross discourtesy to his referees, to ask them to waste their time formally advising rejection of material that was never, ever, going to be accepted. It's an obligation on an editor to have made such decisions expeditiously.

Y: So referees have no proper task at all. Why then have them?

N: Why, indeed? I'm tempted by your momentary conclusion, but the fact is that referees do play a critical role in the ritual of transforming a manuscript into a refereed publication, namely by making it possible to say of them that the paper has been refereed. Providing an imprimatur (‘blessing’ the paper) is an important task.

Y: But that's absurd!

N: The value and importance of tradition and ritual should not be underrated.

Y: But surely referees have more than ritual purpose.

N: Of course. Referees perform the invaluable task of advising on whether a conceivably appropriate article is in fact suitable for publication in the journal to which it is submitted.

In the nature of things, a positive recommendation includes the referee implicitly certifying that she has no reason to believe that the major allegations of the paper are unlikely to be true and more particularly that she has not noticed any (significant) errors.

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

Refereeing does provide an *imprimatur*. More important, though, refereeing is a mechanism for selecting *preferred* papers from potentially suitable papers. Refereeing moderates the *quality* of papers accepted by a given journal.

3 A Digression: On Recognising Blatant Nonsense

When I get a letter purporting to make some great contribution to mathematics I first test its claims by applying several principles.

The first is *the principle of the meatgrinder*. In brief, on my complaining to Kurt Mahler that hard work and apparent ingenuity was making no impact on a problem he had set me, Mahler responded: “Ach, Alf. If you want to get gehaktes Rindfleisch out of a meatgrinder then you must put some *steak* into the meatgrinder.”

The principle of the meatgrinder points out that there’s no gain from busily turning the handle, no matter how energetically or painfully. To get chopped steak out of a meatgrinder you have to put some quality meat into it.

There’s a second important principle for which I don’t as yet have a succinct catchy title. But it boils down to this:

Mathematics ain’t about getting it right; it’s about not getting it wrong.

My thinking is this: There’s no particular merit in getting an answer to a problem, even a correct answer. After all, applying some algorithm is better done by a machine, and in any case is no more than an organised technique for guessing an answer. An easier way to guess — and therefore a better way — is to look at the answers in the back of the book, or to lean across and look at what the person next to you has written (on the presumption she is either smarter or more energetic or both than you), or to phone a friend, or Contrary to what we tend to teach, finding an answer isn’t mathematics at all; it’s just guesswork, intelligent guesswork, maybe.

Mathematics begins when one checks and verifies that one’s guess is indeed not necessarily wrong. So it’s a very good idea to illustrate that one’s argument fails when it should, that is, when it has no business in working. And one is doing meaningfully higher mathematics when one dissects one’s argument — not the turgid computations and details, but the underlying logic — into immediately digestible pieces.

What’s the point of this outburst? It’s often quite difficult to prove that an argument is correct. Thus the principal obligation on a mathematician is to struggle to prove that her argument is false. If, and only if, that struggle is properly pursued diligently, but fails, does she have any business purporting that her argument might possibly be right.

Summary There is no obligation on us as referee or teacher to convince an author (or student) that he is surely in error. It's up to the author to compel us to the view that her claims are not necessarily wrong and may therefore well be correct.

4 Publish or Perish

Y: If a result is new and correct then it warrants publication.

N: Rubbish! Many a new result fills a much-needed gap.

Y: Ho, ho. But smart-alec retorts aside, you'll agree that it would be a loss to mathematics if new results had no avenue of publication.

N: I don't agree. A majority of 'new' results are no more than exercises capable of being carried out by anyone mildly familiar with the results being employed. Not infrequently, they are only cheap corollaries of a ground breaking result. Sometimes they involve considerable and, often, wasted effort in tracking the implications of someone else's slight improvement in understanding; they do not add to our understanding. Worse, many new results are part of a 'cottage industry': a sequence of published minor improvements in understanding forced upon us by a group of authors insufficiently disciplined to begin to digest their ideas before regurgitating them onto the public.

It's not a bad idea, mind you, to write up one's intermediate results and ideas; at the least for one's own use. But that's no excuse for paining an innocent editor or for victimising some referee.

But it's beside the point whether the response above is excessively cynical. It's just not true that a result must be published in a refereed journal lest it be lost forever. Nowadays, for example, one might deposit one's draft in the arXiv.

Y: What's the arXiv?

N: The help files at <http://arxiv.org/help/> answer your first question and the address <http://front.math.ucdavis.edu/> answers the follow-up question you should have asked; namely, "How do I use the arXiv?"

Y: But if people don't publish they will perish!

N: Quite, too true. It's high time that there was some uncoupling of research recognition from formal refereed publication.

Most of us dismiss hope of substantial change in our environment, mumbling sadly to ourselves that "they'll never do it". It might help us to remember that they are us.

Decoupling It's not obvious just what it might entail to *decouple* recognition and refereed publication.

An extreme interpretation might require one to present one's opus all in preprint or arXiv format, with no explicit hint of where or whether its parts have been formally published. [Sure, that's absurd. But is it any the more silly

than the fad of pretending that omitting authors' names from papers somehow leads to more accurate refereeing?]

In contrast to decoupling, imagine a national university funding system[†] which rewards universities for research outputs according to papers published in recognised peer reviewed journals, or in refereed conference proceedings. In such an environment, cottage industries flourish and the notion that refereed publication is at a premium acquires a new meaning.

However, the matter at issue is the coupling of refereed journal publication with promotion and related personal recognition. At the least, decoupling entails giving serious recognition to all work of the candidate rather than confining attention to journal articles. In brief, one argues that the purpose of scholarly publication is scholarly communication. It need not matter whether the item has been refereed. The questions should be whether the item is indeed accessible, and whether peers have chosen to use it. Next one might attempt to judge its quality. The hurdle presented by the refereeing criterion may well be lower. It purports little more than that some soul has looked at the paper and has not found it noticeably weaker than other articles appearing in the journal.

One real issue is the exaggerated and cynical belief that recognition is not at all a function of the quality of one's articles but depends only on their number, or their height and weight. Decoupling will already have occurred if the assessor actually is required to have looked at the candidate's opus.

Summary One must of course make one's work available to the mathematical community, so that the community may judge it and then use it. It seems reasonable to believe that publication in a generally accessible well-mirrored archive can do that adequately.

5 Some Scandals of our Present System

Y: Luckily, it's plain that mathematics referees care. They put months of effort into their reports.

N: Goodness me. You did attend on the day the teacher explained irony!

Y: Huh?

I opened with some remarks from [1]. Let me add another, with which I largely agree.

It is tempting to postpone refereeing tasks by putting the paper aside for a few days. But it takes no longer to do it today than it will in a weeks time.

[†] Amazingly, such a system exists in real life; for details and definitions see <http://www.ro.mq.edu.au/OPUS/guidelines/2.htm>.

It is rarely true that putting a paper aside to allow it to age gracefully improves the quality of one's referee's report.

Of course, I don't do my refereeing within a few days. I find it hard to shake off the ugly tradition in which I was brought up.

Let me mention some other ugly traditions. It is an absurd scandal that respectable mathematics journals can stomach as standard practice a delay time of two years or more between receipt of a manuscript and its publication. One reason for those delays is extensive backlog — over a period the journal has accepted more papers than it is able to publish. But that's a consequence of doubtful refereeing and of editors failing to have the wit and courage to refine their acceptance criteria in the light of the volume and quality of manuscript submitted.

Y: Happily, electronic publication allows journals to catch up on their backlog. With print and postage costs eliminated there need be no restriction on the amount a journal publishes annually. The journal can set its standards and can freely publish all submissions that attain that those standards.

N: Unhappily, not quite. Print and postage expenses are different from other costs only in that they vary according to the number of subscribers and the weight of the publication. Sadly, other costs depend variously on the number of submissions, or the number of papers accepted, or the number of pages that must be edited. Most mathematics publications have so few subscribers that these 'administrative costs' predominate whether or not there is print publication.

Some think that these are 'costs' that need not cost. Administrative tasks can be performed by volunteers. Say to them: "There ain't nothing that comes for free, except perhaps sloppy thinking." Point out that you value your time and that when you volunteer your efforts there is a real opportunity cost, even if it is only the opportunity to give your full attention to the test match rather than just to watch it out of the corner of your eye. In any case, even volunteered time has an upper bound; it is not arbitrarily extensible.

However, now that there is the opportunity to publish without blatantly visible costs it is possible to initiate new 'free' journals, at first totally supported by the enthusiasm of volunteers and employers prepared to support the project. Once existing, such ventures may be maintained by that and the sort of grant support that established projects can attract.

Y: Well, at least electronic communication now speeds up the refereeing process; and that applies also to the traditional journals.

N: Not always 'also'; indeed, in my experience, rarely. One difficulty is that many traditional journals do not conduct their correspondence by e-mail; nor do they ask for electronic submission typed in a suitable flavour of T_EX.

You'll hear bleating that so to insist would be unfair to mathematicians with no T_EXpertise. And what about mathematicians without access to a computer? Tell those complainants that it's no more unfair than the demands we faced until twenty years ago requiring us to type our manuscripts and then to buy a green pencil with which to insert $\mathfrak{}$ symbols.

In any case, the point is the absence of sensible *default* policies. Just as rules are there to be broken, policies exist so they may be adjusted when appropriate. A policy should not deal with out of the ordinary circumstances, other than to have a rider agreeing to deal with special cases in out of the ordinary ways.

It's ludicrous for an editor not to ask, for preference, that referees respond by e-mail. It's scandalous to be satisfied that a referee agree within a month to post a card promising, eventually, to referee a paper. It's been silly, ever since the universal existence of the photocopier, let alone in the case of manuscripts plainly produced by a computer, to post back copies of a rejected manuscript. It is poor policy or sheer rudeness not to advise a referee of the decision eventually taken by the editor, for preference by sending the referee a copy of the advisory e-mail sent to the author.

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

6 Alternative Models for Refereeing

Y: How about making better use of *Mathematical Reviews* and the *Zentralblatt*? If people 'publish' on their personal web page or on the arXiv, then the reviewers can take the place of our present referees. All that needs is for their reviews also to include a critical component.

N: Manuscript archives, of which the arXiv is mentioned as example, are more than just an efficient way of making one's preprints available. The arXiv fixes versions of papers and provides a well-mirrored searchable source for preprints. Placing a manuscript on one's web page is too impermanent and ill-defined an act to warrant being called 'publication'; so let's deal just with papers placed on manuscript archives of the quality of the arXiv.

First, it is a noticeably greater demand on reviewers to ask that they do more than provide a helpful abstract. Finding reviewers cannot be all that easy. *Math. Reviews* has found it useful (and may have found it necessary) to pay per review by giving its reviewers a discount certificate on AMS books. *Zentralblatt* offers reviewers the author discount on Springer books (and rewarded on a per review basis when it sent an international postage coupon with each review). With more demand made on them, it might well become too hard to attract suitable reviewers/referees.

A second problem is more subtle. It's not so much that the review journals review all research mathematics. Rather, in choosing to review an article the review journals *define* it to be mathematics. Naturally, publication in a well recognised peer refereed mathematics research journal is sufficient to qualify for review. However, a substantial part of reviewed research mathematics appears in learned journals that are not primarily mathematical. It's a nontrivial task to identify those papers, just as it may be difficult to decide whether, say, a

paper or book has a meaningful research expository component or is primarily a contribution to mathematical education. Then there's the question of vanity publication

Not surprisingly, those involved with the review journals are not all that mad keen to begin to think about reviewing mathematics that has not been published nor moderated in the traditional way.

Nonetheless, it does seem an attractive proposition to make broader use of the review journals by way of enhanced MR/Zentralblatt review. Mind you, critical reviewing already occurs implicitly. Compare such positive phrases as: 'This fine paper . . . ' 'These instructive remarks . . . ' with a remark such as: 'The author gives yet another . . . '; or with a review simply consisting of a few words from the abstract. Of course, the quality of reviews varies more with the quality of the reviewer, or the match of reviewer and paper, than according to the quality of the paper reviewed.

Y: Second, there's *refereeing by added comment*. For example, Amazon.com invites customers to critique books and to give them a rating of zero to five stars.

N: But don't average the number of stars awarded! A paper with one serious five star review and one zero star review plainly written by an idiot should not be thought of as a two and a half star paper.

It will rarely be a problem to decide which of the two reviews should be dismissed. In the crunch one might have to appeal to the Botvinnik principle[‡].

It does seem feasible to ask readers of papers on the arXiv to submit reports. One trouble is that there is little tradition of *open* refereeing in mathematics. That need not stop readers from pointing to rare actual errors, but it may inhibit critical comment on whether the paper is 'good' or 'worthwhile', let alone whether it's well written.

Curiously, perhaps, there *is* a long standing tradition of open critical book reviews. Short reviews may be little more than abstracts, but extended reviews sometimes are very instructive. Traditionally, such book reviews first survey the subject and then identify the extent to which the book fits into that survey. Happily, here there is no tradition that it is wrong to be scathing when that is appropriate. Some such reviews are wonderful[§]; ask your friends for their

[‡] On a train journey, grandmaster Botvinnik sees the other passenger in his carriage studying a chess board. The stranger notices Botvinnik's glance of interest and asks him whether he would like a game. Botvinnik begins to say no then shrugs his shoulders and answers that, sure, he'll play; but let's stake a good amount on it, say a hundred roubles.

"A *hundred!*" the passenger cries in horror and astonishment. "But how can you risk such a sum? You don't even know who I am!"

"Exactly", answers Botvinnik.

[§] One of my old favourites includes the line: [The author] seems to use a method of infinite ascent in expounding his proofs, that is, simple ideas are often developed by using more complicated ones.

favourite polemics. Book reviews differ from other refereeing and reviewing tasks also in that the reviewer gets a copy of the book as reward.

Generally, referees are not suitably acknowledged or rewarded. Some change to that is easy. For example, it surely is a simple matter for a journal annually to put on its web site a list of persons who have provided timely referees reports.

The primary purpose of refereeing is to *select* suitable or preferred papers. Nonetheless referees should accept the obligation to assist authors not only in correcting their paper but also in *improving* its organisation, exposition, and results. In extreme cases (*Scientific American?*) staff authors may effectively write the paper. It might be a good thing for editors to notice those cases where a referee has effectively become a co-author and to invite author and referee formally to collaborate.

Summary

Whenever one selects one manuscript but not another, for whatever purpose, a review or moderation process has intervened. Whenever one comments on a manuscript, no matter how incidentally, that remark is a review and it may aid a selection. The notion ‘peer refereeing’ does need to shed its ritual aspects, particularly if the only purpose of those rituals is to humour tenure and promotion committees.

7 The Future

Y: What do you think the future will bring?

N: It’s hard to predict, especially the future.

One guesses that the breakpoint is the ability of libraries to pay for their subscriptions. That ability has been resuscitated by publishers repackaging their offerings and adjusting their prices ingeniously, making different offers according to their view of a library’s apparent ability to pay.

But, recall that the mathematical sciences provide only a very small part of the learned publications at issue. Several of my remarks above may not apply beyond mathematics.

While libraries are prepared to pay for it, and while we continue to support it by our preparedness to submit our manuscripts, and continue to offer our contributions as referees and editors, we will continue to have learned journal publication as we know and knew it.

Y: But why do journals still exist? It doesn’t make sense for papers first to be published on preprint archives, and then to be republished.

N: It’s not that simple.

First, there *is* value added by being allowed to label an article as published in one of the better journals. Second, the editor and referees may have induced actual improvement of the article. Moreover, the journal will likely have warmed up the format and presentation of the manuscript.

Third, learned journals did not all come into existence because publishers and learned societies hoped to make a dollar or two at the expense of university libraries. Many journals are, at any rate in the first instance, a record of research of some society or group, or the minutes of meetings. Consider departmental preprint and technical report series; conference proceedings are a clear case of ‘minutes’; note titles such as Séminaire ... , Comptes Rendus de ... , Acta Whateveriensis.

Even though there are now many other opportunities to publish one’s work, the motives that once led some groups of mathematicians to display their work are active today and will remain valid tomorrow.

In all this, it’s worth noticing that nowadays a ‘journal’ can be almost virtual, consisting of little more than a .cls file. ‘Publishing’ it need only entail selecting and validating suitable papers, then creating a page of links on the web. Mind you, a ‘journal’ can be yet more virtual; see Greg Kuperberg’s *Open Journal of Mathematics* [2].

The preprint archives also present an extra source for new but traditional publishing opportunities. Could there be a market for honest to goodness books featuring, say, the ‘best’ papers archived or published over a recent period? In this context also see Jim Pitman’s proposal at <http://mathsurvey.org>.

It’s well known that lectures by keynote speakers are often published automatically in conference proceedings; now and then, a journal may ask an author to submit a survey article. So, for a book, or a journal, to find its content in the preprint archives is not really a new idea. Mind you, from the quality control point of view it might be an improvement to select articles rather than authors.

Indeed, if journals mined preprint archives an author might get to decide which of several competing journals should get the rights to publish. That’d be a pleasantly positive version of our present quandary as to where to submit our masterwork.

Particularly in the humanities, it is commonplace to find books of readings (consisting of previously published articles linked by wise editorial comment); this is not quite unknown in mathematics but is unusual.

Y: I still miss getting real preprints with nice covers.

N: That’s the trouble with home publishing. The output is on A4 paper (or, worse, on quarto if you’re a North American) and, no matter how good the L^AT_EX, or how expensive the fonts, it doesn’t look published at all.

Just so, it is a common complaint that it’s not easy to browse journals on the web. Searching is much easier, of course, but complainants purport to miss serendipitously noticing the previous article, and the next one.

One expects that all journals will eventually be available electronically, most electronically only (perhaps with printed offprints and an expensive option for print copies for very rich libraries). Publishers might make extra friends if, at least until our generation dies off, they maintained the illusion that their journal is more than just the sum of its articles by including, with each full article, the abstract of the previous and of the next article.

Summary Journals will remain with us, probably often as no more than virtual versions of their paper and bound originals. Selecting their contents brings up the traditional refereeing issues. There is now, and all the more when almost all journals are electronic there will be renewed opportunities for traditional print books featuring the best of

Formal peer refereeing differs from other forms of review only in being part of an accepted ritual. It will survive, but only as a diminishing element of informed comment on published work.

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