



Editing an Electronic Journal

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In 1998 Neil Sloane—sometimes known as the Father of Integer Sequences [1]—started a free electronic journal which he called the *Journal of Integer Sequences*. In the first four years of the journal's existence, fifty papers were published.

In 2002 Sloane decided to seek another editor, and I (influenced by Leonard Eugene Dickson's dictum [2])¹ volunteered to take over. I have edited the journal ever since, assisted by an editorial board of eight colleagues. Our journal is entirely free for both authors and readers and is hosted on computers at the University of Waterloo [3]. We get about 100–150 submissions per year, of which about 50–75 are published.

Editing the journal is rather time-consuming, and I estimate that I spend roughly one day a week on it. Here I reflect on some of the things I've learned while editing the journal for twelve years.

Referees

At the *Journal of Integer Sequences*, to avoid burdening referees unduly, we try not to ask the same referee twice within a year. Unless the paper is exceptionally long or there are other extenuating circumstances, we ask for referee reports to be

completed within two months. After two months we send a reminder and, if necessary, another reminder after three months. In the rare case when no report is produced after four months, we give the referee a one-week ultimatum. If there is still no report, we look for a different referee. The result is that our mean time from submission to a decision is under one hundred days.

The delay between submission and decision is mostly due to waiting for referee reports. Good referees tell you right away if they are able to write a report and deliver their report on time. Bad referees don't answer your initial request quickly, don't deliver a report on time, and don't respond to repeated requests for the report. The worst referees of all, however, are those who agree to write the report and then string you along with an endless series of "I'm almost done with the report, and I should have it to you next week" messages. I once had a referee do this to me for almost a year until I finally gave up. This particular referee was very convincing, I have to say.

Potential referees offer many reasons for not agreeing to read a paper. One explained that he no longer refereed any papers at all because he objected, on philosophical grounds, to the imposition of any time limit on the production of the report. I notice that he continues to publish and presumably get his own papers refereed by others.

Finally, one referee who we've asked at least four times kept refusing because the papers we sent him were "not in his area of competence," even though they evidently were. Pretending incompetence certainly succeeds as a strategy to avoid more work, although I imagine it doesn't improve one's reputation.

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¹"...every person should aim to perform at some time in his life some serious useful work for which it is highly improbable that there will be any reward whatever other than his satisfaction therefrom."

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Two Paradoxes of Refereeing

The first paradox of refereeing is that good referees are not compensated for doing a good job on time. Instead, they are effectively penalized by additional requests for refereeing, while bad referees (or people who don't agree to referee) are rewarded by having less work to do. I have no easy solution to this paradox, although when good referees submit papers to our journal, we do try to match them with other good referees.

Matching referees with a paper leads to the second paradox of refereeing: very good papers are typically sent to very good mathematicians as referees. Not-so-good papers don't usually get sent to the mathematicians of the highest reputation for two reasons:

1. There are many more mediocre papers than truly excellent mathematicians.
2. You want to conserve the resources of great mathematicians by not asking them very often, and by sending them papers that are deserving of their time.

The result is that very good papers, which get sent to good mathematicians with high standards, might very well have a lower acceptance rate than much weaker papers. I suppose it is the role of the editor to try to adjust for this paradox.

Referee Reports

In twelve years as editor-in-chief, I've seen more than a thousand referee reports, both good ones and bad ones. To paraphrase Tolstoy, good referee reports are all alike: they evaluate the quality and correctness of the paper; give specific, detailed suggestions about how it could be improved; mention relevant papers missing from the bibliography; and end with an explicit recommendation to accept, revise, or reject.

Bad referee reports, however, are each bad in their own way. For example, here is one report I got: "I had a brief look at the paper, not worth my or anyone else's time." I'm not sure what I am supposed to do with this, even if it's true. I can hardly send it verbatim to the author.

One risk of electronic referee reports that does not seem very widely known is the risk of revealing your identity (name, institution, and so forth) in the "metadata" that accompanies your report. In my experience, this is particularly true of reports that are prepared using Microsoft Word (and one reason that we ask referees not to prepare their reports in this fashion).

Authors

Although most authors appreciate the work done by referees, a few do not. One author, a rather well-known mathematician, fell in this latter class. After

I forwarded a report with some reasonable suggestions for improvements, he sent a reply as follows:

I feel bad for you and JIS for messing up so badly. You are hereby granted twenty-four hours to accept our paper subject to the few minor revisions that we agree with, without going back to that stupid referee.

Needless to say, we did not give in to this kind of threat, and the paper was not published.

Another author, upon receiving a negative report from the world's expert on the subject, complained that our journal is "a forum taken to belong to an elite that think to hold [*sic*] the absolute truth delivering a decision based on an incredibly deprecatory pseudo review" and attributed the rejection to his belonging to the group of "people that are obviously not in the social network of the journal." Luckily, responses like this are the exception, not the rule.

Perhaps because of the content of our journal, we also get a nontrivial number of submissions from amateurs claiming to solve major open problems, such as Goldbach's conjecture. Lately we have resorted to requiring that these kinds of papers be accompanied by a physical (nonelectronic) signed letter from a PhD mathematician at a university, stating that he or she has read the submission and agrees it is correct. This requirement has cut down on our time commitment in handling low-quality submissions.

Plagiarism

Another problem that we've had to deal with is plagiarism. I once had a paper submitted that plagiarized almost word-for-word parts of a paper listed in the submission's bibliography. I learned this from the referee report, written by...the author of that paper in the bibliography! Needless to say, this was not exactly the smartest move on the part of the author.

Most cases of plagiarism, however, have not concerned mathematical content but arise because the authors of the papers feel their English is not up to the standards of a scientific paper. They "borrow" sections of the introduction of other papers. These authors often don't seem to understand that what they have done is not legitimate.

Preparing Papers

At our journal, we have no editorial staff to prepare papers, so the job falls on the shoulders of the authors and the editor-in-chief. It's hard to teach people how to write good papers, but Steven Krantz's book [4] is a good start.

Our journal relies on L^AT_EX as the source code for manuscripts. We have a certain journal style, which we distribute to authors [5] and ask them to follow. Most of the recommendations there are

along the lines of “brush your teeth,” but it’s startling how few authors follow them.

For example, despite the fact that mathematicians are intimately familiar with the notion of using a variable name to denote a numerical quantity, for some inexplicable reason this familiarity does not extend to giving names to theorems, lemmas, and so forth using labels in L^AT_EX. Many authors insist on “hard-coding” these references. This choice makes it very hard to revise the paper, since inserting a new theorem requires renumbering throughout.

There is a relatively short list of latex errors that are made again and again by authors [6]. And the American Mathematical Society has a set of packages (amsmath, amscd, amsthm, amsfonts, amssymb) that make life much easier for a mathematician. Use them!

Conclusions

Running an electronic journal has been a great experience for me. I’ve learned a lot about manuscript preparation. I get to see interesting papers before they appear and contribute to improving their presentation. Sometimes I even get inspired to write my own papers following up on submissions. I think I underestimated, however, how much time editorial work would take and the kinds of challenges I would face. Perhaps this brief note lets you know what you’re in for if you decide to do the same.

References

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- [3] cs.uwaterloo.ca/journals/JIS/
- [4] STEVEN G. KRANTZ, *A Primer of Mathematical Writing*, American Mathematical Society, 1996.
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- [6] recursed.blogspot.ca/2013/05/ten-common-latex-errors.html

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