

Special Issue: On Good Refereeing

The peer-review process rests upon a *sine qua non*: referees. Without them, there would be no journals worth reading. But what makes a good referee? If you're sitting down to review your first ever paper, where do you start? Just how many ways of saying politely 'not like this' are there? These and other questions are addressed by this issue's panel of peers. **John Marden** explains how he has matured in his style. **Michael Stein** reflects on his experience and offers some advice on how to proceed if you are invited to review a paper. Over the page, **Xiao-Li Meng** has an idea for making reviews faster, and **Rick Durrett's** "Rambling" is on how long we should be prepared to wait. We are interested in hearing from more IMS members, so if you have something to say, why not write us a letter? Email it to bulletin@imstat.org.

Maturity in Refereeing

John I. Marden, University of Illinois at Urbana-Champaign, writes:

When I first started refereeing papers, I thought it was an adversarial activity: Once I decide whether the paper should be published, I write my report as a legal brief, presenting only those arguments that support my position. When in prosecutor mode, I was especially relentless in my argumentation. At least until Jack Kiefer sent me a nice note suggesting that a particular paper in fact did not merit the death penalty, even though it was not *Annals*-level.

Eventually I matured enough to realize that the refereeing process is an important part of the research life of the discipline, not a manning of the barricades.

What is your job as a referee? Help the author with research, help the editor with decisions.

To the author, write with the idea that you are trying to help improve the paper. Any advice is fine, from picky details to overall philosophy, including suggestions for additional references and directions. But rein in the desire to rewrite the paper, or propose changes that are fundamentally a matter of taste.

Go with your strengths. There has been a trend over the years away from checking every single mathematical detail, towards judging just whether the mathematics seem plausible. It is a good trend, but don't shy away from checking details if you feel you can contribute. Editors assign referees to obtain a spectrum of reactions, e.g., more

experienced researchers are less likely to be willing to look at details than newer researchers, but may be able to provide more historical perspective.

If the paper is wrong about something, point it out dispassionately. Referee the paper, not the author. Say, "One cannot assume the variance is finite in this case," not, "Any first-year graduate student knows to check whether the variance exists!"

It is good to be accurate. It might be obvious that the paper is not up to par, so you do not bother to read the whole manuscript carefully.

You might comment negatively on the simulations, when in fact those calculations were exact numerical ones. Though an understandable mistake, it could set off the author, who'll claim (in capital letters, if by email) that the referee didn't read the paper carefully, so their opinions are worthless!

If you suggest the paper is too long, make some specific suggestions. "The paper is excellent, cut it in half!" is encouraging, perhaps, but not very helpful.

Timeliness is important. Most journals will suggest a deadline for the report. Take this as an opportunity—if you know you have only six weeks, you know you are not expected to research the complete background of the differential equations leading to the confluent hypergeometric distribution. If you do not feel that you can do an adequate job in the time allotted, feel free to decline the request to referee.

I find getting started is the hard part, so it is best not to put it aside for a while. As I

age, "whiles" keep lengthening.

(The reverse occasionally occurs: As soon as I receive the paper, I will read it and immediately realize it is not acceptable. Don't tell anyone, but when that happens, I still wait a couple of weeks. I don't want editors to think I am too efficient.)

To the (associate) editor, write with the idea that you are trying to make the editorial job easier. The most helpful letters briefly describe the paper's main results, and its strengths and weaknesses, then make an up-or-down recommendation. Shadings within those categories are perfectly fine, but try to make an actual recommendation.

I find a couple of intense sessions of some hours work best for me. In the first session, I read through the paper, see if the methods look plausible, look at the conclusions, verify a few claims, and think hard about whether it is acceptable for the particular journal. Then I put it away for a week or so, letting my mind ruminate on its own. Meanwhile, there may be some references to check on, or maybe I will try some simulations or calculations. I often find when I go back to it that my original opinion was too harsh or too lenient, that I missed a key point or that I missed a key error. Once I am satisfied that I have a well-justified opinion, I will write up my report to the author and letter to the editor.

After you have sent off your report, bask in the warm glow of your effort. You have helped move research forward, and the discipline is very, very, very thankful! ■

How — and Why — to Be a Good Referee

Michael Stein, University of Chicago, writes:

Many years ago I co-organized a session at a JSM on speeding up the refereeing process in statistics, together with Xiao-Li Meng (whose article follows—then my colleague at Chicago). At the time (1995, according to Xiao-Li's CV), we were both young and energetic and made statements about how we were going to follow up on the ideas discussed at the session to change the culture of refereeing in the statistics profession. In fact, I don't remember us doing much of any follow-up at the time, which could be blamed on each of us becoming parents that year. Now we are older, less energetic (at least in my case), but in better positions to do something about the problem. Xiao-Li is Co-Editor for *Statistica Sinica* and I am an editor at the new IMS journal, *The Annals of Applied Statistics*. Both journals have aggressive policies for fast (by statistical standards) reviewing, with at least some success. Furthermore, other statistical journals have gotten onto the faster reviewing bandwagon, so that culture change may actually be happening now.

So obviously I am a strong advocate for rapid reviewing, but what else do I expect from a referee beyond a prompt reply? The first thing you need to do when invited to review a paper is to accept or decline the invitation. In making this decision, you should judge how much of an effort will be required on your part to prepare your review. You should not generally be expected to spend more than a full working day in total reading the paper and writing your review and, in many cases, half a day or less should suffice. If you do not think you will be able to find this time within the time frame provided by the journal but you still would like to review the paper, you should suggest a date by which you can provide a review and ask if that is accept-

able. If you cannot provide a review by a time acceptable to the journal or you just don't want to review the paper, then immediately decline the invitation to serve as a referee. Be honest with yourself in assessing your ability to complete a review on time; if there are already several overdue reviews lying on your desk (or, more likely these days, your disk), just say no.

If you accept an invitation to review a paper, what should you provide in your review? In my opinion, it is not a primary responsibility of referees to decide if the subject matter of a paper is appropriate for the journal to which it has been submitted. Editors and associate editors should be screening out papers that are clearly inappropriate, so the fact that you have been sent the paper indicates that the editorial board finds the general topic of the paper within the journal's realm. The most important piece of information you can provide the editor is your opinion as to how good the paper is and the reasons behind this judgment. Of course, "goodness" can be measured in many dimensions (originality, breadth of applicability, quality of presentation, etc.) and your overall assessment will be some combination of these. Although neither journals nor authors want to publish errors, it is not the referee's job to verify that the paper is free of mathematical or numerical mistakes. Nevertheless, a good referee will often uncover some such errors, not because they check proofs line by line or repeat data analyses, but because they find an ambiguity in the statement of a theorem, or inconsistent or implausible results in a data analysis.

If you recommend that a paper be rejected (and this recommendation should normally be placed in a cover letter to the editor rather than in the review), give a few of what you consider the most salient reasons. You do not need to give an exhaus-

tive list of everything you find wrong with the paper. If you recommend that a paper be accepted, you will most often make some recommendations for changes before final acceptance. In this case, you should list every critical change in substance and presentation that you believe is needed. I find that both authors and reviewers often pay insufficient attention to the quality of tables and figures and welcome comments on how these could be improved. If you are asked to review a paper a second time that had been tentatively accepted, you should not normally raise new issues at this time, but just (quickly) verify that the authors have adequately responded to the comments in your initial review.

One reason it can be difficult to get prompt and helpful reviews of papers is that it is sometimes hard to see the benefit of being a good reviewer, and the cost of being a bad one. Indeed, an inevitable consequence of being a good reviewer is that you will be asked to review yet more papers. But there are a number of benefits beyond the satisfaction of a job well done. There is the opportunity to learn about new work in your area of interest. The critical skills you learn by assessing the work of others may be effectively applied to improving your own research. The opinions of people in your field may matter to your professional advancement: writing an insightful referee report is a great way to impress an associate editor or editor that could lead to all kinds of unanticipated benefits down the road. These benefits are admittedly rather intangible, so the **IMS is thinking about establishing an award for excellence in refereeing** that would provide public recognition for at least some of you who toil away in anonymity. If you have any thoughts or ideas about the nature of such an award, please send them to the Bulletin editor, at bulletin@imstat.org. ■

Changing our Review Culture: Younger and Faster

Xiao-Li Meng, Harvard University, writes: “Double Effort, Not Double Blind!” (hereafter DOUBLE, available at <http://galton.uchicago.edu> as Technical Report 382), an article I wrote in early 1994, contains my reaction to the then debate on double-blind refereeing for statistical journals. It also collected thoughts and experiences I had during 1991–93 as a junior author, referee, and associate editor. My view then was that the primary problem in our review process was not the lack of double-blind refereeing, but rather the excessive length of the review process. Tremendous progress has since been made, but stories about papers being reviewed for excessive periods are still being told too often—I was just waiting for one for over a year.

As authors, we agree that a change in our review culture is needed; yet most of us are guilty as reviewers, despite the fact that we are acutely aware of the importance of timely review. The reason for this is obvious, as I wrote in DOUBLE:

“When we are asked to referee or handle a paper, I believe almost every one of us has the intention to finish it as soon as possible. There is simply no (ethical) incentive to delay such a process. What happens next, however, often departs substantially from what we initially hoped. We constantly find ourselves replacing old deadlines with new ones and watching our file piles growing in an (dis)orderly fashion. An apparent reason for such an unfortunate situation is that we always find that other demands, professional and personal, request higher priority than those silent manuscripts. Sometimes, manuscripts are simply forgotten for a time when our minds are being occupied by so many other demands. I do not want get into the issue of how we should assign our priorities, as such a

complex issue often results in fruitless debate; Gleser (1986, Amer. Statist., 310–312) sheds some light on this issue. What I do want to discuss is how we can find more fellow colleagues to share the editorial work, so each of us can have more flexibility in allocating our time and energy, thus eventually helping to reduce the length of the review process.”

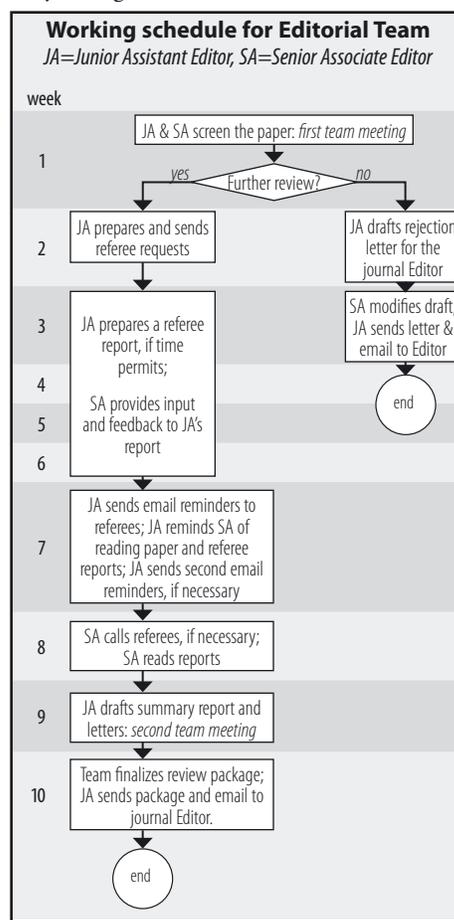
In DOUBLE, I then proposed a “team system”, aiming to combine junior researchers’ time/energy with senior (associate) editors’ wisdom/experience. The proposal included a “censoring” mechanism for referees’ delays to ensure a 3–4 month turnaround time, and a back-up system for editors to guarantee the journal’s responses on submissions within 6 months under the worst circumstances.

My current experience as Co-Editor of *Statistica Sinica* reinforces my belief that formally including more junior researchers in our editorial system is tremendously beneficial. The editorial board we appointed is twice the size of the previous board, and includes many young researchers, including assistant professors. With this number of energetic and viable associate editors, *Statistica Sinica* has been able largely to reach the goal I hoped for in DOUBLE. Currently, about 90% of submissions to *Statistica Sinica* have their first turnaround time within the four-month targeted deadline, with the longest being 203 days (excluding submissions to a theme issue on Brain Sciences, requiring a special board because of overwhelming submissions to a very young and diversified interdisciplinary field).

As a more “burned out senior” looking back at what I proposed then, it is inevitable that if I were to edit the proposal now I would likely modify the timeline somewhat, to better fit my current “senior

schedule”. But this is exactly the problem my proposal tried to address! Senior researchers are overwhelmed by many duties, professional and personal, and we need help from those who have more time, energy, willingness—that is, junior researchers!

I therefore choose to retain the “freshness” and “naïvety” of my original proposal, quoted and sketched below, for the purpose of “casting a stone to attract jade”, in other words, to encourage brainstorming of better and newer ideas on how to involve many more young researchers, and thereby invigorate our efforts to fundamentally change our slow review culture. ■



Proposal: After appointing associate editors, allow (but not require) each of them to appoint one assistant editor. Each assistant editor will work with the associate editor as a team. Assistant editors are formal members of the editorial board, and should be listed in the journals along with the associate editors and the editor(s).

Rick's Ramblings: *How long is too long?*

◀ How long should a referee's report take? ▶

◀ a: 7–10 days ▶

◀ b: 2 months ▶

◀ c: 6 months ▶

◀ d: 1–2 years ▶

To steal a line from a local TV commercial for an insurance agency: *all the answers are TRUE.*

- a:** *Nature* asks for seven days, and sends you an email after three or four to ask you how it is going. The Public Library of Science journals ask for ten days. When I reviewed a paper for PLoS *Computational Biology* and the tenth day was to come on the Thursday of the Brazilian Summer School where I gave six lectures, they generously allowed me to submit my review on Monday. Some of you may not like me making fun of the PLoS journals who are heroes of the open access movement. However, before you start waving your “Death to Elsevier” flags, you should realize that the access to their papers is open because authors pay \$1500 in page charges to put their PDF on-line, and they don't waive these charges.
- b:** As an Associate Editor, I ask for a report to be sent within two months. Of the 30 papers I have handled in the last two years for *Annals of Applied Probability*, one took five months and two took three months, but all of the others were handled within this time window, and quite a few rejections took less than a week. (Please hold your applause until the end of the article.)
- c:** When I was editor of *Annals of Applied Probability*, 1997–1999, six months was the time at which I started bugging Associate Editors to get after their referees. It seems that many people think that six months is a reasonable amount of time for a referee's report. When I wrote to one referee who had gone well beyond her two-month deadline, she told me: “What are you complaining about? It has only been four months.” One month later I had my report.
- d:** I think we have all experienced delays of more than one year on the refereeing of our papers. My personal maximum is 22 months for my paper with Jason Schweinsberg on approximating selective sweeps. I can sympathize somewhat with the referee because when I read a draft it would take me 3 days to wade through the 57 pages of calculations, but the long wait was due not to slow and steady effort, but to prolonged procrastination.

There is some evidence that people think that this time frame is not too long. In ancient times when people put four copies of their papers in a big envelope and mailed them, I had a paper with Claudia Neuhauser that had taken nine months. I mailed a letter to the editor of *Annals of Applied Probability* asking when I might get a report, and he wrote me back: “What do you expect? It is a long and complicated paper.”

What can we do about this situation? Well, biology tells us the answer: *nothing!* Refereeing is altruistic behavior. That is, something that you do, at a cost to you, for the benefit of the group. Thus, from a game theory point of view, it is advantageous to be a cheater, who enjoys the benefits of other people's efforts, but does not provide the service. In some social systems, punishment is sometimes used to enforce norms of behavior.

Join Contributing Editor Rick Durrett for an opinion piece on the Special Issue subject of Refereeing



However, even though it sounds like fun to have a public flogging of the most delinquent referees at the banquet at the IMS annual meeting, it is not really a practical possibility.

Despite this negative theoretical result, there are some things that we can do.

1. When you, as Associate Editor, send out a paper to be reviewed, ask for, say, a two-month deadline, and write the date into the email. When the date has passed, write to the referee to politely remind them the deadline has passed and ask when you might get their report.
2. When you, as referee, receive a paper to review, take the deadline seriously. The task will take the same amount of time whether you wait two months or four months to do it.
3. As a referee, remember that it is your job to check to see if the paper is worthwhile and correct. There is no need to micromanage the author's writing by making dozens of little suggestions about the English. I would go further and say that you don't need to check every detail of the paper if you think the approach is solid. The authors' name will be on the paper, not yours.

So, now that you have read my article, put down this magazine and go do that referee's report you have been putting off. When you are done you can reward yourself by doing the Kakuro puzzle on the back page.