

Should Journals Compensate Referees?

Publications are central to advancement in science, and all research communities rely heavily on refereeing to insure the integrity of the literature. With growing community pressures and expansion of the mathematical enterprise, we might ask what refereeing does for mathematics, how well it goes and why, and what are its special needs and difficulties? That's a lot, so I limit myself to these points.

- Quality referees have valuable skills, neither adequately acknowledged nor appreciated.
- Compensating referees might improve their standards and timeliness.

For example, here's a typical request to me from a journal (with a large, no, *huge* editorial staff). Following that is my response (with a P.S. for the reader). I asked five colleagues—experienced in these tasks—to comment on my interchange. I then compare my comments with theirs. A two-question postscript seeks answers on who contributes most significantly to editing and refereeing.

Journal Message: [Salutation from editorial staff—not an editor] “We would greatly appreciate your serving as a reviewer for the enclosed paper (pdf-file-title): [Article Title] [Author A] which has been submitted to [Journal B].

“We have asked several referees but unluckily they all declined to do the job. We really hope you could take a look at this paper. Please complete your review and send it to us by May 31, 2006 [gives one month for an extremely technical 23-page paper]. Send your review to [e-mail]. If you are not able to referee the paper, please, if possible, suggest an alternate referee.” [Request I acknowledge receipt]

My response to Journal B: “Author A writes technical-papers requiring savvy refereeing. Even expert referees will tire of the same author's papers. This gives quality mathematicians like Author A, with little political base, serious problems getting into print.

“I'm asked to referee roughly one paper a month. (One year I had 47 refereeing requests, from many different journals. That dropped when I slowed my response rate.) These requests are often far from topics in which I publish papers. Too, it is my experience that over 50% of papers (yes, tough topics, but ...) have very serious (not typo or oversight) errors. This reflects poorly on referees.

“Why referees, rather than authors? Others don't report such an error rate. Yet, about 1/4 of these papers have another referee: I see unreported errors directly.

“Journals should recognize there are situations, including this, that require top-notch refereeing, and they should pay for it. I suspect slightly-above-token pay might go far. Maybe even develop a cadre of referees who establish expertise on handling difficult cases. Compensation could help the whole community: Journals would not over-rely on referees biased toward papers of their acolytes.

“Referees' reports, maybe with a slim list of typos, and what they think is the author's significance to the

community, often lack serious analysis. I know some (editors and referees) don't expect this of a referee. Yet, without such, refereeing is rife for abuse.

“Except for what the referee learns, and that would be precious little from many referee reports, he or she has a thankless task for the moment.”

P.S.: Journal B never responded to my discussion of compensation!

Comments of five respondents: All my correspondents agree refereeing is a service relevant for those publishing papers. Yet, so is administration. Still, not all are appropriate for it. Quality refereeing is a skill implying energy for considered analysis, and ability to surmise topics outside one's own published work. That skill is scarce; its upkeep requires practice.

#1 suggested a need for differentiating editor and referee tasks. “There are referees, editors, publishers, authors, and except maybe for the last, their roles have changed and may no longer be well-defined.” Most respondents mentioned many mathematicians play several roles in turn.

Certainly, I agree!: Those huge editorial boards ought to edit! Editors should initiate the process with partial refereeing. The anonymous referee should be for nontrivial tasks that represent refined expertise. #1 also had a peeve: “It is annoying ...to read that I should do the job in so many weeks.”

#2 and #5 add that refereeing tasks are onerous if done seriously, and those tasks don't fall equally on all.

All agree on calling some publishers to task. #2 example: “Many journals are ...expensive, though both their content and ...aspects of processing it, are typically done for free. ...as T_EX has virtually ended the role publishers [had once] in enhancing ...papers. So, what do publishers give for what they get?”

#3 and #4 see editorial compensation as an equally important topic: “For most journals, there is little compensation. [They included other comments on what specific journals provide to editors, though one wouldn't amaze one's university with such remuneration.]”

#5 was most emphatic that referees should do something about lack of compensation. “Why do mathematicians ...referee without compensation? ...because they find the paper at hand interesting and want to study it? Don't most have more important papers they should read first? Are they flattered to be asked to judge? Is it a fair trade for having their papers refereed?”

“[Many—he lists some] journals make money and can afford to pay their referees. But, why should they do [that] if mathematicians ...referee for free?”

Final Questions: Which mathematicians do editors see as the major contributors to refereeing, and by what criteria? How many mathematicians regularly do serious refereeing and how do they feel about it?

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Mathematical Gaps

In a letter on page 6 of the January 2007 issue, Joan Birman castigated the mathematics community for “not policing [itself] very well” in the *Perelman affair*. I will limit myself to addressing only one of the many points Birman tried to make. Birman took issue with the editors-in-chief of the *Asian Journal of Mathematics* for allowing Cao and Zhu to assert in their paper that there were “gaps in Perelman’s proof”.

A gap, by general consensus, means non-trivial but missing logical steps leading from one claim to the next. By this definition, Perelman’s papers are full of gaps because, for more than two years after their appearances, the whole world waited with bated breath for the definitive judgment of experts on whether the missing steps could indeed be filled in. The existence of gaps in Perelman’s paper was thus obvious to one and all.

I do not wish to give the impression that I am questioning the mathematical merits of Perelman’s contribution to the solution of the Poincaré conjecture. Yes, he fully deserves the Fields Medal. However, I do not believe that the way he presented his work to the world is a model that others should emulate. Why did Birman choose to ignore this aspect of Perelman’s work?

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No Assertion of Gaps

In the January 2007 issue of the *Notices*, Joan Birman wrote in her letter that our joint paper, which appeared in the June 2006 issue of *Asian Journal of Mathematics*, “asserted that there were gaps in Perelman’s proof.” We do not know how Professor Birman drew her conclusion, as nowhere in our paper did we make such an assertion. The phrase “complete” in the title of our paper refers to the facts that Perelman’s article contains the phrase “sketch” in several places and we completed these sketches, and also that we

included much other published material on the Ricci flow to make a complete exposition of the proof. None of this amounts to asserting that there is a gap. In addition, we stated that we had to substitute several arguments by our own “because we were unable to comprehend these original arguments of Perelman.” In other words, since we were not sure whether our arguments were the same as what Perelman had in his mind, we were responsible for what we wrote, and as such we made that statement. It does not imply in any way that we have doubt whether Perelman knew how to prove them. We are sure that Perelman has better arguments in these places, as the results he stated are stronger than what we proved.

We hope this message will clarify the misunderstanding of Professor Birman.

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Event Should Not Have Been Canceled

Shame on the AMS for canceling the Special Event on the Poincaré Conjecture and Geometrization Theorem at the 2007 Annual Meeting in New Orleans. The reason given is, “It became apparent that the continuing controversy was undermining this special event.” But that controversy is precisely why the event should *not* have been canceled!

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End Discrimination First

The interview in the January 2007 issue of the *Notices* with Joan Birman was very interesting. I would like, however, to take issue with Birman’s

remark on the comment by Harvard president Larry Summers calling for research on the innate differences between men and women to see if it might explain the difference between the achievements of men and women in the sciences.

Perhaps Birman and Summers do not know that a great deal of research has been done that shows the extent and nature of unconscious bias against women which results in discriminatory treatment. For an excellent exposition of this research I recommend *Why So Slow*, by Virginia Valian, published by the MIT Press.

At this time we need action to reverse discriminatory treatment. When that has been accomplished, if inequality of achievement still exists we might then appropriately call for further research on the differences between the minds of men and women. For now, the issue is to acknowledge discrimination and work to end it.

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Corrections to George Dantzig memorial article

The photo caption on page 359 in the March 2007 issue of the *Notices* incorrectly identified the pictured event as the “National Medal of Honor ceremony, 1971”. Instead, the caption should have read “National Medal of Science ceremony, 1975”.

The photograph on page 345 should have been credited to Ed Souza, Stanford News Service.

—Sandy Frost