

Comment

R. J. Carroll

While the Editor of the *JASA Theory and Methods* section (1988–90), I was particularly interested in the issues of bias as they related to new researchers and senior researchers of outstanding reputation. Lacking hard numbers, I can only fall back on my remembered impressions. I have, however, discussed this with a number of current *JASA* Associate Editors, who have all assured me that I am not too badly miscalibrated.

The issue of double-blind reviewing has some similarities to the bias-variance tradeoff in nonparametric curve estimation. The Reid Report and the proposed pilot study make clear that adding variance to an already noisy process is a concern if a double-blind review system were to be implemented. If increased variance is observed, and I have no idea whether this will be the case, the question is whether the current biases can and should be removed.

Bias means that there are changes in response probabilities on the basis of factors external to the science (age, gender, institution, etc.). As an Editor, I observed a strong positive bias in favor of new researchers, both by Associate Editors and by referees. The New Researchers' report feared that the bias was to accept weak papers simply on the basis of new researcher status, but I found that this was not the case. Overwhelmingly, the form of the bias was in helping the new researcher turn a publishable idea into a publishable paper. Most often, this involved cutting the length of the paper and focusing on main points.

In effect, in my experience, new researchers had a higher probability of getting a helpful report and a

friendlier reception for their work. There were many cases of papers which in their published form are quite good, but whose original version would have received a quick and curt rejection except for the fact that the author was a new researcher. The pilot study, if done, should modify its stature variable to account for new researcher status, the categories "unknown or not well regarded" not being necessarily the same!

Bias in reviewing papers by senior authors is more complex than one might think. I followed a policy of having only senior Associate Editors handle such papers, to lessen the potential for intimidation to drive the system. There were three striking characteristics of reviews in this context:

- New researchers tended to give honest and fair reviews of papers, generally higher quality reviews than one would expect for papers from the average author. Interestingly, the recommendations tended to be more positive than the reviews. Perhaps simplistically, I attributed this to a reluctance to criticize superstars (halo effect).
- Senior referees and Associate Editors not personally connected to the author were often extremely, even shockingly harsh. There was a tendency for the peers of senior authors to hold the authors to a higher standard. If I had \$1,000 for every report that said something like "the author is capable of much better work," I would have a down payment for 100 acres bordering a trout stream in Montana.
- Senior authors had a lower probability of being hassled about the correctness of technical details.

My no-data impression is that a move to an implementable double-blind review system would have minor effects on senior authors and somewhat negative effects on new researchers not readily identifiable as such.

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Sometimes described as the gatekeeper of science, the peer review system provides credibility to the mod-

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ern academic publishing industry. Its chief function is to ensure that minimum standards of objectivity and rigor are met in communicating the results of a scientific investigation. Quality, originality and pertinence of the material are some of the other issues that may be dealt with through refereeing, notably in highly