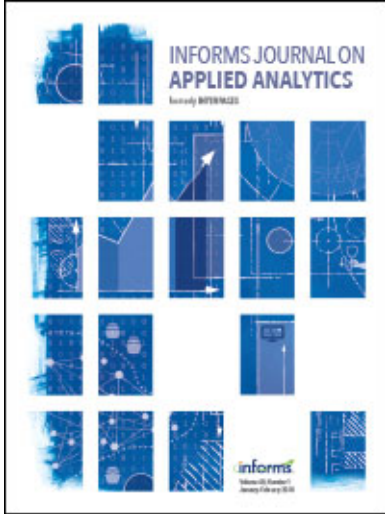




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Editorial: Refereeing—How Blind Should We Be?

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Many scholarly journals use some form of double-blind refereeing. Many do not. It varies considerably by field, with double-blind refereeing being the norm in some disciplines, such as political science and so-

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ciology, and being the exception in others, such as psychology and physics. Economics is almost evenly split. Most TIMS and ORSA journals do not use double-blind refereeing, but two, *Organization Science* and *Information Systems Research*, use it faithfully. For those wanting to know much more than can be reasonably crammed into this editorial, Blank [1991] and Cox et al. [1993] are good references to start with.

In TIMS and ORSA, the issue of double-blind refereeing arose when several members from traditionally black colleges complained about what seemed to them to be prejudicial treatment and asked us to use double-blind refereeing. The OR/MS board appointed a special committee composed of joint publications committee members and headed by Earl Barnes to look into the matter and report back. The committee found, somewhat to its surprise I believe, that many TIMS and ORSA members believed that they had experienced discrimination—that is, criticism, delay, or rejection for reasons other than the content of their papers—and had strong feelings about the matter. For example, a now well-established and respected member of TIMS and ORSA vividly recalled using her initials instead of her first name on her first paper to cope with her concern about sex discrimination only to receive a referee's report that referred to "the author-ess." The committee and a unanimous joint publications committee recommended double-blind reviewing to the OR/MS board. Early in the ensuing debate, the publications committee accepted an amendment that defined double-blind reviewing as merely removing the authors'

names and affiliations from the cover page. During the extensive debate, at two different board meetings as well as separate council meetings, there was discussion of the statistical evidence on the effects of double-blind reviewing, the lack of statistical evidence on the extent of discrimination, and the desirability of the board forcing editors to use double-blind reviewing if they didn't want to. The outcome of the debate was that the OR/MS board adopted double-blind reviewing as its goal and directed the joint publications committee to work towards its adoption in joint journals. ORSA and TIMS councils took similar positions with respect to their separate journals. The votes were far from unanimous, and the editors of some journals are strongly opposed. There has been very little pressure on editors on this issue from the publications committee. It is my impression that among both the supporters of double-blind reviewing and those opposed there are those for whom the issue arouses considerable emotion. As a member of the TIMS council and the OR/MS board, I voted for double-blind refereeing.

Why Double-Blind Refereeing?

I take the following to be relevant and well-established facts:

- (1) A substantial number of members feel strongly that there is some discrimination and that double-blind refereeing should be undertaken to help combat it.
- (2) Very little objective evidence exists of the extent of actual discrimination in TIMS and ORSA journals.
- (3) The "costs" of double-blind reviewing depend significantly upon how it is done.
- (4) Some objective evidence exists on the effects of double-blind reviewing, at least

at the *American Economic Review*, and this evidence does not show it to be either silly or wildly effective.

The evidence on the effects of double-blind reviewing at the *American Economic Review* comes from a controlled experiment reported by Blank [1991]. In the experiment, half the papers, selected at random, were reviewed double blind and half were reviewed single blind. The effects were not dramatic. Papers by women, papers by scholars at the most prestigious universities, and papers by scholars at the least prestigious schools had acceptance rates that did not differ significantly statistically. Papers by scholars at middle prestige universities had somewhat lower acceptance rates with double-blind reviewing. Also, based upon questionnaires to the referees doing double-blind reviewing, about half of the reviewers thought they knew the identity of at least one author of the paper they reviewed, and about 90 percent of that half were right.

It is important to note that this study does not show that there was not a significant number of prejudicial rejections of papers by women or those at low prestige schools with single-blind reviewing. It only shows that the net effect of discriminatory rejections and affirmative-action acceptances was insignificant. (I believe, and I think that most would agree, that a journal that has a discriminatory rejection and an affirmative-action acceptance is much worse off than one that has two decisions on the merits.) It is also worth noting that the *American Economic Review*, which through much of its history had used single-blind reviewing, went to double-blind reviewing after the experiment.

Now, some subjective judgments. First of all, I believe that most journals make many mistakes in accepting or rejecting papers. (I offer three pieces of objective evidence. First, the correlation between the numerical ratings provided by the referees on those papers having exactly two referees' reports in the *American Economic Review* study was only 0.24. Second, the

Many TIMS and ORSA members believed they had experienced discrimination.

Journal of Economic Perspectives published an entertaining paper based on interviews with Nobel Prize and Clark medal winners about their classic papers that were rejected before being published [Gans and Shepherd 1994]. Finally and closer to home, ORSA's Lanchester Prize was once given to a paper that was rejected by *Management Science* before it was published in *Operations Research*.) The vast bulk of these mistakes are for reasons that are nondiscriminatory, but I suspect that it often doesn't feel that way to the female and minority authors. Indeed, the justified rejections they receive may well produce feelings of prejudice too. On the other hand, TIMS and ORSA members are human beings, and in my experience, human beings sometimes have prejudices and sometimes are unaware of them. Thus, I find it hard to believe that, policy notwithstanding, no TIMS or ORSA journal ever made a discriminatory rejection or ever will in the future.

I find a couple of arguments used against double-blind refereeing not to be

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persuasive. It is sometimes argued that journals should take into account the prestige of the author or the author's institution in judging a paper. I doubt that we should print a bad paper by a member of the National Academy or reject a good one by an unknown, underemployed author. And I think that it is our job to do our best to tell the difference between a good paper and a bad one. But even if an editor wants to take the author's prestige and affiliation into account, that information is still avail-

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able to him or her. The editor should certainly not want the referees to depend upon it in assessing the paper itself.

I am also not persuaded by the argument that referees want to know the identity of the author of the papers they review so that they can contact them or be knowledgeable about who is doing what. First of all, I don't believe that such desires by potential referees are strong enough to erect any practical barrier to getting reviews. Second, even if this is an important motivation for a referee, I don't see any reason why the journal can't obtain the referee's report before revealing the author's identity. Furthermore, statistical evidence from a study of medical journal papers shows that the average quality of reviews done double blind is better from the point of view of editors and no worse from the point of view of authors [McNutt, Evans, and Fletcher 1990].

In my view, the best argument against

low-effort double-blind reviewing policies is that they may eliminate the extra effort that some referees are willing to extend to help new or minority authors or those with low prestige affiliations to improve a poorly presented article with a good idea. I'm sure that such special efforts are sometimes made. Recall, however, that the only statistical evidence is that the average quality of reviewing is at least as good with double-blind policies. In any case, editors can and often do still provide such help to any author that needs it. At *Interfaces*, editors, especially Managing Editor Mary Haight, provide extensive and valuable rewrite assistance even to skilled and experienced authors. Furthermore, it would be presumptuous to tell those asking for the protection of double-blind refereeing that they are acting against their own interests.

Overall, it seems to me that in the absence of proof that there is no discrimination and in the presence of a real concern from a significant number of our members, it is neither good policy nor justifiable to refuse to do things that are free or nearly so that might help eliminate some prejudice. On the other hand, in the absence of strong evidence that there is a significant amount of discrimination, it is not appropriate to use large amounts of scarce resources on means of dubious benefit. Thus, I am quite happy with the low-effort double-blind policy adopted by the OR/MS Board, and I am happy to implement it at *Interfaces*. While we don't plan a controlled experiment, we will keep an eye on how our policy is working. And we will continue to strive to get into *Interfaces* the best practice papers we can find.

ROTHKOPF

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