The Behavior Analyst 1984, 7, 77–78 No. 1 (Spring)

In Response

Participation by Women in Behavior Analysis: Some Recent Data on Authorship of Manuscripts Submitted to the *Journal of Applied Behavior Analysis*

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Poling, Grossett, Fulton, Roy, Beechler, and Wittkopp (1983) recently presented statistics from a variety of sources documenting the extent of women's participation in our field. Their data, especially those related to journal publications and conference presentations, supported a general conclusion that there is noticeable under-representation on the part of women. Poling et al. also made several recommendations that, if followed, might lead to an improvement in the current state of affairs.

There can be little disagreement with the basic message conveyed by Poling et al. In fact, the overall intent of their article is most commendable, and the data they presented are quite convincing. However, two points deserve further clarification.

First, more recent data suggest that women's contribution in at least one area—articles published in *JABA*—is greater than that estimated by Poling et al. They calculated two sets of statistics on authorship ratio for articles published in *JABA*: the proportion of female authors among first authors only and the proportion of female authors among all authors. From these statistics they concluded that: (1) women appear as authors of *JABA* articles far less often than do men, (2) the proportion of women appearing as first authors was less than the proportion of female authors when all authors are considered, (3) the proportion of female first authors seemed to be increasing over time, and (4) the proportion of female authors overall showed no similar increasing trend.

From the data portrayed in their figure, we estimated that the mean proportion of female first authors for the last five years of their survey (1977–1981) was 25% and that the mean proportion of female authors overall was 24%. We then extended their analysis by calculating the same statistics for authorship of articles published in *JABA* during 1982 and 1983, which exceeded 35%. These more recent statistics are noticeably higher than those presented by Poling et al., and, we believe, suggest a more favorable outlook on the status of women as *JABA* authors.

The second point of clarification involves one of the recommendations made by Poling et al. In attempting to interpret their data on authorship ratio, Poling et al. suggested that bias against women during the review process may be a causal factor; in order to guard against such bias, they further recommended that all manuscripts submitted to *JABA* undergo blind editorial review. It must be emphasized that a low authorship ratio for a given group, such as women, may result from either: (1) a low submission ratio for that group, or (2) a low acceptance ratio for that group. To determine the degree to which *JABA*'s female authorship ratio is currently affected by acceptance ratio, we calculated the female and male acceptance ratios for manuscripts submitted to *JABA* during 1982 (a num-

We thank Joan Driessen and Gary Pace for their helpful comments on a previous draft of this manuscript.

ber of revisions pending at the time of this writing precluded a similar calculation for 1983). Considering first authors only, the acceptance ratios for women and men were virtually identical: 18.2% and 20%, respectively. Had the numbers been different by a single manuscript, the ratios would have been equal. These data indicate that the lower authorship ratio for women in *JABA*—37.5% for first authors in 1982—was almost solely a function of a lower submission ratio and that, from a statistical standpoint, there is no evidence of sex bias in the *JABA* review process. Moreover, these data are consistent with results from a large-scale analysis of editorial decisions reported by Zuckerman and Merten (1971). Based on an examination of over 14,000 manuscripts submitted to *Physics Review* over a nine-year period, they found no relationship between author rank and acceptance ratio (i.e., seniority bias).

In the present case, however, it is not clear that data constitute an effective argument against the practice of blind reviewing. Poling et al. admitted that ". . . neither data nor personal experience suggest that referees for either journal (*JEAB* and *JABA*) are biased against articles written by females . . . ;" still, they suggested that ". . . no harm could come from the employment of blind reviews."

A full discussion of the pros and cons of blind reviewing would require consideration of many factors that are unrelated to the question of bias. The following examples are offered to illustrate the complexity of the issue. First, it has been observed that two infrequent but serious problems—data fragmentation and manuscript duplication—are difficult to detect (Abelson, 1982; Broad, 1981; Peters & Ceci, 1982). Blind reviewing may make detection even more difficult, especially for journals that accept manuscripts on a wide range of topics. Second, it is possible that blind reviewing might jeopardize the educational function of the review process that, for *JABA*, is as important as the editorial decision itself. Inability to identify an author may dissuade reviewers from providing extensive and detailed suggestions that inexperienced researchers would find highly valuable and for which they would be most appreciative because lengthy commentary requires extra reviewer effort that is not always appreciated by more senior researchers. It would be unfortunate and ironic if this practice had the unintended effect of curtailing the editorial shaping process for young and otherwise less-published authors, for the data indicate that this group includes a significant number of women. A third way of addressing the matter of blind reviewing might take the form of the following question. Because the sole purpose of blind reviewing is to minimize reviewer bias, would such an editorial practice be effective, assuming that bias did exist? While no data are available to answer this question, it has been suggested that the most knowledgeable and, therefore, perhaps most influential reviewers often can identify the manuscripts written by authors working in the same area (O'Connor, 1979). It is unlikely that the behavior of these reviewers, if biased, will be affected merely by removing the author's name from a manuscript.

Every attempt should be made to insure that editorial recommendations are not based on factors other than scientific merit. There is currently little evidence, however, indicating either the need for or the advantages of blind reviewing toward that end.

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