In Response

The Glass Ceiling Is Not Fragile: A Response to Odum (2000)

Frances K. McSweeney and Samantha Swindell
Washington State University

Odum (2000) criticized our recent conclusions about the participation of women in the experimental analysis of behavior (McSweeney & Swindell, 1998). We address her criticisms here. We argue against the need for statistical tests. We show that our conclusions still apply to all journals except the *Journal of the Experimental Analysis of Behavior* even when we include the senior editorial staff along with members of the editorial board. We argue that the data that Odum provides to show gender equity are limited, inconsistent with past findings, and hard to interpret in the absence of other data. Finally, we argue that Odum failed to address our most convincing argument for gender inequity and misinterpreted our suggestions for improvements.

Key words: participation of women, gender equity, experimental analysis of behavior, applied behavior analysis

Odum (2000) responded to our recent paper on the participation of women in the experimental analysis of behavior (McSweeney & Swindell, 1998). We hoped that this paper and our paper about the participation of women in applied behavior analysis (McSweeney, Donahoe, & Swindell, 2000) would generate a discussion of the complex and important issue of gender equity. As with all data, our data are subject to interpretation, and reasonable people can disagree with our interpretation. Therefore, we appreciate Odum's response. Nevertheless, we believe that our data counter her criticisms, and we will briefly comment on each.

STATISTICS

Odum (2000) believes that our data are difficult to interpret because we failed to present measures of variability and to conduct inferential statistics to support our visual summary of the data. We do not believe that such sta-

tistics are necessary when data are as orderly as those that we presented. For example, McSweeney and Swindell (1998) argued that women's participation as authors in the *Journal of the Experimental Analysis of Behavior (JEAB)* was lower than their participation in other, comparable, journals. Three figures supported this conclusion. Figure 2 showed that participation by women in *JEAB* was lower than their participation in each of the other three comparison journals for 10 of the presented 12 points. Figures 1 and 3 showed that participation by women in *JEAB* trailed their participation in the other journals for 12 of 12 points. That is, participation by women in *JEAB* trailed their participation in other journals in 34 of 36 comparisons. The difference in participation rate was less than 1% in the two cases in which participation in *JEAB* exceeded that in another journal. We do not believe that such orderly data require support from inferential statistics.

Odum's (2000) call for statistical tests is based on a misunderstanding of the data. She reasoned that the differences in means that we report may not be statistically significant because the year-to-year variance in our data

Comments on this paper should be addressed to Frances K. McSweeney, Department of Psychology, Washington State University, Pullman, Washington 99164-4820 (E-mail: fkmcs@mail.wsu.edu).

Figure 1. Percentage of women occupying each of three increasingly selective positions (author, all; first author, first; editorial board member, editors) over the 5-year intervals from 1978 to 1998 for the *Journal of the Experimental Analysis of Behavior (JEAB)*. All results are means. The error bars represent one standard deviation.

should be large. She writes, “the absolute numbers are not large, and an absolute difference of even one author or editor could make a difference of several percentage points” (p. 280). We disagree. The absolute numbers on which our statistics were based were usually large. We recorded the number of authors, number of articles, number of female authors, number of female first authors, number of articles with a female author, number of editorial board members, and number of female editorial board members. The average per year for *JEAB* over the 20 years of our analysis was 145.0, 70.4, 25.2, 9.1, 19.6, 34.0, and 4.8 for each of these categories, respectively. The only small numbers occurred for the number of female first authors (9.1) and for the number of female editorial board members (4.1). However, these numbers should show little variability from year

to year for other reasons. For example, people are usually appointed to the editorial board for multiple-year terms. As a result, there is no reason to expect the large year-to-year variability predicted by Odum.

Figure 1 illustrates the relatively small variance that we observed. It replots Figure 5 from McSweeney and Swindell (1998) for *JEAB* with the addition of error bars that represent one standard deviation. We presented standard deviations to be consistent with Odum (see her Figure 1). We applied a parametric statistical test to be consistent with this measure of variance. When a two-way (Selectivity Category $\times$ Interval) repeated measures analysis of variance was applied to the percentage of women in each category in each interval, the interaction term was significant, $F(6, 24) = 2.616, p < .043$. This confirms McSweeney and Swin-

dell's (1998) conclusion that the changes in participation by women in *JEAB* differed over time for the different selectivity categories.

We prefer to present our data without statistical test. We included this test only to show that the variance in our measures is small enough so that it does not confound our conclusions. We used a parametric test to be consistent with Odum (2000). The choice of any particular test can be questioned, and this test may be particularly questionable because percentages are bounded and cannot be assumed to be normally distributed. Our point is only that the variance in our data is sufficiently small so that our conclusions can stand up to statistical test.

INCLUSION OF ASSOCIATE EDITORS

Odum (2000) criticized us for ignoring the percentage of associate editors who are women. She argued that, for 1993 to 1997, the percentage of female associate editors of *JEAB* was 30%. This number is greater than the 12.7% of members of the editorial board who were women over the same period. On the reasonable assumption that the position of associate editor is more highly selective than membership on the editorial board, these data contradict our conclusion that participation by women decreases with increases in the selectivity of the position.

We still believe that we acted correctly in excluding associate editors. To begin with, Odum (2000) reported a standard deviation for the percentage of female associate editors that is almost as large as the mean. We do not think that a statistic with that much variance can be considered to be reliable enough to warrant reporting. Further evidence of the unreliability of this measure can be found by examining *JEAB* today. Currently, 0% of the associate editors are women.

McSweeney and Swindell (1998) also compared data from several journals. To do this fairly, data had to be

analyzed in the same way for all journals. Unfortunately, some of the journals did not have associate editors (e.g., *Journal of Experimental Psychology: Animal Behavior Processes*, *JEP:ABP*) and one of the journals had associate editors that rarely changed (*Learning and Motivation*, *L&M*). Therefore, our data would have been biased against finding temporal increases in participation by women for these journals if we had included associate editors in our counts.

Most important, however, the overall conclusions of our work would be unchanged even if we had extended our analysis to include the senior editorial staff. Figure 2 contains the mean percentage of members of the editorial board who were women and the mean percentage of the senior editorial staff who were women, plotted as a function of the four 5-year intervals from 1978 to 1997. Each graph contains information about one of the six journals examined by McSweeney and Swindell (1998) or by McSweeney et al. (2000) that had an editorial board. The senior editorial staff includes anyone who was responsible for the scientific oversight of the journal. This includes the editor, associate editors, and editors for special topics (e.g., reviews, behavioral pharmacology). We did not include people who were primarily concerned with the business of running the journal (e.g., the business manager). The cases in which no data are presented for the senior editorial staff are not missing data. Instead, the missing bars indicate that 0% of the senior editorial staff were female for that journal at that time. These data points should be interpreted carefully, however, because the total number of senior editorial staff is small for some journals. The missing bars for *JEP:ABP*, for example, mean only that no woman served as the senior editor over the 20 years that we examined.

The analysis offered in Figure 2 differs somewhat from that of Odum (2000). Odum compared participation by women on the editorial board to

participation by women as associate editors only. Figure 2 compares participation by women on the editorial board to participation by women on the entire senior editorial staff, including editors, associate editors, and special section editors. We think the percentage of senior editorial staff is a better index of women's participation in the upper levels of the profession than the percentage of female associate editors only.

Consistent with Odum's (2000) conclusion, Figure 2 shows that in two cases, the participation of women on the senior editorial staff of *JEAB* exceeded their participation on the editorial board. However, it also shows that these two points are unrepresentative. In 20 of 24 comparisons across journals, the percentage of women serving on the editorial board was greater than the percentage of women serving on the senior editorial staff, usually by a large amount. That is, although Figure 2 supports Odum's conclusion about *JEAB*, it also supports, rather than contradicts, our more general argument. It extends our conclusion that the participation by women usually decreases with increases in the selectivity of the position to data that include the senior editorial staff.

ADDITIONAL DATA

Odum (2000) offered some additional data to contradict our conclusion that women may have more difficulty than men in reaching selective positions in the profession. We applaud the presentation of additional data. The complex issues discussed here will be understood only if many data are pre-

sented. However, we also believe that each of her statistics suffers from problems that weaken its interpretation.

Male Versus Female Editorial Board Members

Odum (2000) argued that men and women do not face different barriers in reaching the editorial board because the number of papers published in *JEAB* was similar for women and men who were members of the editorial board or associate editors of *JEAB* in January 1999. Her argument rests on the assumption that the number of papers authored in *JEAB* is a good measure of equal merit, and therefore, is an appropriate baseline from which to judge the difficulty faced in reaching the editorial board. Several arguments contradict this conclusion. As Odum notes, many factors in addition to scientific reputation influence selection for the editorial board (e.g., expertise in a particular area). Because any measure of scientific contribution ignores these factors, it may be a poor predictor of selection for the editorial board. But, even if we assume that scientific merit alone determines membership on the board, number of publications in *JEAB* may still be a poor measure of scientific merit. Among other things, it ignores the quality of the publications as well as the influence of publications in other journals. Until evidence to the contrary is provided, we conclude that the number of publications in *JEAB* may not be a good measure of equal merit for the men and women who are admitted to the editorial board.

←
Figure 2. Mean percentage of editorial board members who were women (board), as well as the mean percentage of the senior editorial staff who were women (editors), plotted as a function of the four 5-year intervals from 1978 to 1997. Each graph contains information about one of the six journals examined in McSweeney and Swindell (1998) or McSweeney et al. (2000) that had an editorial board (*JEAB* = *Journal of the Experimental Analysis of Behavior*, *JEP:ABP* = *Journal of Experimental Psychology: Animal Behavior Processes*, *JABA* = *Journal of Applied Behavior Analysis*, *BT* = *Behavior Therapy*, *BM* = *Behavior Modification*, *BR&T* = *Behaviour Research & Therapy*).

Rejected Manuscripts

Odum (2000) argued that men and women do not face different barriers when publishing in *JEAB* because the percentage of rejected manuscripts is similar for men (55%) and women (60%). These statistics are also flawed. To begin with, they are based on submissions to only one journal calculated over only 3 years (1996 to 1998). Finding equal rejection rates also supports a conclusion of equal treatment only if the submitted manuscripts are equally meritorious. On the surface, such an assumption seems reasonable. Nevertheless, if this assumption is accepted, then Odum's data differ from past reports that work by men and women is evaluated differently (e.g., Graham, O'Reilly, & Rawlings, 1985; Lott, 1985; Wenneras & Wold, 1997). Some of these data are compelling because detailed measures of equal merit were constructed or because equal merit was ensured by having the same work evaluated under stereotypically male and female names. We believe that Odum's data need to be reconciled with these past data before strong conclusions are drawn from them.

Few Submissions

Finally, Odum (2000) argued that the reason that women do not appear more often as authors in the pages of *JEAB* is because women submit only approximately 20.5% of all manuscripts submitted to *JEAB*. The failure to submit manuscripts, rather than inequitable treatment of those manuscripts, produces the low participation of women. However, this statistic is difficult to interpret without further information. We need to know how frequently women submit manuscripts to *JEAB*'s comparison journals (*JEP: ABP*, *L&M*, *Animal Learning & Behavior*). If the submission rate by women is lower for *JEAB* than for other journals, then women's past experience with *JEAB* may have discouraged submissions. That is, without further information, we cannot conclude that

the journal has not contributed to the low submission rate by women.

Odum's (2000) argument that the low submission rate may be the result of a "long-standing disproportionate participation by women in science" (p. 283) cannot be the sole explanation. If it were, then women should publish at similar rates in *JEAB* and its comparator journals because authors in all of these journals are drawn from the pool of available scientists. However, the publication rate by women in *JEAB* is below that of its comparators (McSweeney & Swindell, 1998).

MISINTERPRETATION OF SPECIAL STEPS

Odum (2000) is concerned that our suggestion that "the work of women in the experimental analysis of behavior be evaluated by 'special steps' (p. 201)" (p. 283) might have a chilling effect on the field. People might attribute women's success to special treatment rather than to merit if taking special steps promoted the use of different standards for women than for men. We agree. We did not mean to suggest that work by men and women should be treated differently. In fact, our reference to "special steps" actually reads, "Editors could also take special steps to ensure that manuscripts by men and women are treated similarly in the review process" (p. 201). We assumed that our suggestions, such as the use of blind reviews and reviews by both female and male reviewers, would apply to work by men as well as to work by women. We apologize if that was not clear.

GENERALITY

Our strongest objection to Odum (2000) is that her reasoning leaves the largest and most striking trends in our data unexplained. Odum concludes that there is no evidence for gender inequity (p. 282), the opposite of our conclusion. Because the data that she presents apply only to *JEAB* in recent years, she reasonably restricts her con-

clusion to *JEAB* in recent years. Although she had access to McSweeney et al. (2000), she argues that the data presented in that paper did not change her conclusion (Footnote 1, p. 279). On the surface, this seems reasonable because the data presented by McSweeney et al. were collected for other journals, not for *JEAB*.

However, the data presented by McSweeney et al. (2000) do bear on the interpretation of data for *JEAB*. Drawing a conclusion that applies only to a single journal for a limited number of years is reasonable if the factors that produced that conclusion are local ones that operate over limited periods of time. For example, if gender inequity was produced by the discriminatory actions of a few men, then it would be reasonable to ask whether such discrimination was present at *JEAB* in recent years. In contrast, the data presented by McSweeney et al. (2000) suggest that the factors that produce gender inequity are widespread and long lasting, not local. They reported that participation by women decreased with increases in the selectivity of the position (a glass ceiling) and that the size of this glass ceiling increased in recent years. Because similar trends appeared for *JEAB* as for these other journals, common factors probably produced the trends in all cases.

An adequate theory must account for all of the data. Therefore, it must explain the development of the glass ceiling. McSweeney et al. (2000) argued that the most likely explanation for the glass ceiling is gender inequity. Odum (2000) did not offer an alternative. As a result, we believe that the most likely explanation for all of the data is that gender inequity occurs and that Odum's data are flawed, as we

have argued. A potentially viable alternative is that gender inequity usually occurs, but that Odum's data show that *JEAB* was an exception to this rule in recent years. Odum's data cannot be used to justify the more general conclusion that gender inequity does not occur. If she has made a case for gender equity at all, her case is one that applies only in limited circumstances.

CONCLUSION

In conclusion, we do not wish to argue that gender inequity provides the only possible explanation for the data offered by McSweeney and Swindell (1998) and McSweeney et al. (2000). Such a conclusion cannot be drawn because it is impossible to test and reject all other possible explanations for the data. However, we do believe that the development of the glass ceiling over time is surprising and that these data are difficult to explain without postulating gender inequity (see McSweeney et al., 2000). As a result, the data deserve discussion. We applaud Odum for starting this discussion, but we question her arguments.

REFERENCES

- Graham, D. L. R., O'Reilly, P., & Rawlings, E. I. (1985). Costs and benefits of advocacy for faculty women: A case study. *Journal of Social Issues, 41*, 85-98.
- Lott, B. (1985). The devaluation of women's competence. *Journal of Social Issues, 41*, 43-60.
- McSweeney, F. K., Donahoe, P., & Swindell, S. (2000). Women in applied behavior analysis. *The Behavior Analyst, 23*, 267-277.
- McSweeney, F. K., & Swindell, S. (1998). Women in the experimental analysis of behavior. *The Behavior Analyst, 21*, 193-202.
- Odum, A. (2000). Reflections on the glass ceiling: Women in the experimental analysis of behavior. *The Behavior Analyst, 23*, 279-283.
- Wenneras, C., & Wold, A. (1997). Nepotism and sexism in peer-review. *Nature, 387*, 341-343.