


Lott raises several objections to the methods used, and the conclusions drawn by Ferber and Teiman (1980), F & T henceforth.

He also suggests alternative approaches and different interpretations. We shall briefly explain why we do not find either his methods or his conclusions acceptable.

Lott chooses a logit model to test our data. It is customary to use logit when a "yes" or "no" dichotomy is involved. Such an approach would be appropriate if he used the decisions for individual articles, rather than percentages. In that case, however, the N would be far larger, and his conclusions might well be different. He further uses ln's of the percentages, without even making clear why he considers this preferable, or even acceptable.

The reason for using logit is that Lott believes a series of tests of differences for the data in Table 2, F and T is not appropriate, because the tests are not independent. He is right, of course, that the categories of "at least one woman author" and "mixed authors" are not independent, since the former category includes the latter, but this is not true of the categories "male only," "female only," and "mixed authors." Our findings that the acceptance rate for manuscripts written by only women was higher for journals with double-blind refereeing, just as was true for "mixed authors" stands.

Furthermore, while Lott's claim that the use of a logit model permits him to test for discrimination in acceptance rates across both types of refereeing simultaneously, it is precisely for this reason that the test is not useful. When women's acceptance rates are lower in one data set, and higher in the second, putting them into one regression is not a way of testing for the difference, but of obscuring it. The same problem arises with the second proposed logit model. Furthermore one may wonder how serious Lott is about this one, since he himself says that it is difficult to argue the existence of a regression for it.

Let us next turn to Lott's claim that odds of being published between strictly male authored articles and strictly female authored articles are not significantly different. The variable in his regression, on which this conclusion is supposedly based, is defined \( F_i = 0 \) if authorship of \( i \)th group was not wholly female; \( F_i = 1 \) if authorship of \( i \)th group wholly female. He is actually comparing strictly female authored articles with those that are strictly male authored, plus those that have mixed authorship. Thus Lott tests the difference between two groups never actually compared in F & T.

Last we turn to the suggested interpretation for the agreed-upon fact that "mixed" authors have a particularly high acceptance rate. It is very puzzling that Lott, having just established to his satisfaction that there is no discrimination against female authors, would now argue that the better and more prolific ones perceive discrimination in refereeing and hence endeavor to avoid it, and that sex of author(s) is therefore not independent of refereeing. Given the tenacious nature of this reasoning, it is not surprising to find little empirical support for this hypothesis. Of the three groupings considered only one shows a significant difference, and that only at the 10% level, between journals with and without double-blind refereeing. Not it is easy to see why "mixed authors" but not women should...
be particularly eager to avoid (the presumably nonexistent) discrimination.

Lott does suggest a plausible hypothesis for the higher acceptance rate for "mixed author" manuscripts as opposed to others. It is possible that multiple authorship is associated with higher quality because of specialization and division of labor. (As a personal aside we would like to express our appreciation to the author's good taste in "for the moment ruling out any argument along the line of chemistry between the sexes." We would have been even more favorably impressed if he had not raised the subject at all in a discussion of scholarly work.) This is, in principle, a testable proposition, but unfortunately the data on which F and T's analysis is based are not useful for this purpose.


In 1970, The Brookings Institution began to publish its annual reviews of the Federal budget, *Setting National Priorities*. The project had been organized by Charles L. Schultze, who had just recently emerged from a period of service as the director of the Office of Management and Budget. The first volume and its 12 successors analyzed the general strategies and magnitudes of each budget, and focused on the principal expenditure program and tax issues.

While there had been earlier sporadic attempts to provide private, independent assessments of the Federal budget by such groups as the National Planning Association and the Committee for Economic Development, the Brookings project was the first systematic effort to provide some check and balance on the government's own work. Since governments always have an incentive to make the budget look better than it really is and to present the program issues in somewhat propagandistic form, an independent check is an invaluable service, not only in guiding the public debate on the budget, but in providing help to Congress which has to act on it.

The initial reports showed the usefulness of this kind of independent analysis. Partly following the Brookings example, Congress organized the Congressional Budget Office, (CBO) drawing Alice Rivlin from the Brookings group to head the new organization. In the subsequent years, the CBO has grown to a staff of over 200 professionals, who are able to analyze every aspect of the budget and who provide the Congress with objective reviews of administration proposals. The American Enterprise Institute organized its own budget review project under the leadership of Rudolf Penner, and served as a moderately conservative counterweight to the mild liberalism of the Brookings group.

With the CBO now possessing a very large staff and turning out reports of near-Brookings-like quality in profusion, one has to ask whether the annual Brookings project still needs doing. Is there anything here which cannot be found in the CBO reports? Can eight research economists, devoting only part of their time to this effort, hope to add much to the CBO's enormous capability? The analysis of the 1983 budget provides a reassuring answer that the Brookings analysis is still needed. The volume is not only the handiest summary of both broad and narrow budget issues, making the more arcane matters of budget policy accessible to both the policy and the research communities, but the judgments which are reached embody a common sense that is badly needed in an era when the budget, in truth, has gone slightly mad.

The opening chapters, by Joseph A. Pechman and Barry P. Bosworth, paint a stark picture of a budget that has lapsed into hundred plus billion dollar deficits. In a somewhat misguided effort at objectivity, the authors accept the CBO and OMB projections, weighting the former by two-thirds; in actuality, things are worse than either projections, and Brookings might well be advised to go back to doing its own work on the overall budget magnitudes. The commentary covers what is by now very well-known ground: despite enormous cutbacks in discretionary civilian programs, the enormity of the 1981 tax reductions and defense increases creates a budget crisis which is particularly difficult to accommodate in an era when monetary