

more optimistic than Lyberg and Lundstrom that further investment in methodology will pay dividends. One example of methodology that we cited repeatedly in our paper is Zaslavsky (1993a); this work not only advances the state of the art for census undercount estimation, but it serves as a useful case study that could be adapted to other statistical arenas as well. Overall, however, we appreciate their endorsement of our general perspective on the adjustment controversy.

Ericksen, Fienberg and Kadane make few comments directly about our paper. We would simply point out that some of their recent references (Kadane, Meyer and Tukey, 1992; Darroch, Fienberg, Glonek and Junker, 1993) also serve to illustrate that progress is still being made on undercount-related issues, yielding both new theory and new methods.

We understand that in their rejoinder, FW cite a personal communication from us. We offer the following comment in the spirit of "setting the record straight."

In the initial version of his paper, Breiman made a stronger claim about the increasing proportion of unresolved cases in the Evaluation Followup Survey (EFU) when one reads across his Table 12, which we saw as the kind of nitpicking criticism that deserves to be pushed to the margin. Originally, after we

pointed to Breiman's curious claim that one might do just as well in imputing for unresolved cases by flipping a coin with probability 15% of heads, we had written, "The higher proportion of remaining unresolved cases in the higher imputed probability categories is explained in large part by the fact that names were not recorded for many PES individuals." However, it turned out that our explanation was inaccurate; although cases without names constituted approximately 70% of the P-sample cases receiving probabilities of having been enumerated of 75–100%, these cases without names were largely excluded from the EFU and so were not reflected in Breiman's Table 12.

We acknowledge that there were a substantial number of unresolved cases in the EFU and that there is remaining uncertainty about the accuracy of the imputation methods. Our essential point is that there is not much to criticize based on available data, which agree with predicted values extremely well (Belin et al., 1993). To attribute our earlier statement to us as if it is our current view is a misrepresentation.

Overall, although we anticipate that our Berkeley colleagues will continue to support one another, we are pleased at the signs of consensus in this exchange.

Rejoinder

Leo Breiman

I thank the discussants. The descriptions of the methods used in Australia, Great Britain and Sweden were interesting and form a compact introduction to the diversity of methods for estimating population counts. They also underline the difficulty of the census undertaking in the United States. The discussion by Ericksen, Fienberg and Kadane and the Belin–Rolph article contain most of the direct comments about my paper.

BACKGROUND

The effort to adjust the census counts was a complex process. After the initial error evaluation, additional errors were discovered, some of which are discussed in my article. Because the original error analysis has not been updated to take these additional errors into account, the widespread impression remains that the adjustment process was proven to produce more accurate counts than the census.

The validity of any such proof is currently in serious doubt. For one thing, errors of various types are now acknowledged to account for the major part of the original national undercount estimate of 2.1%. The initial loss function analysis used earlier estimates of the bias that, on the national level, were too small by at least a factor of 2. The analysis was also flawed by a significant underestimation of sampling variances (Fay and Thompson, 1993; Freedman, Wachter, Cutler and Klein, 1994). There are also questions about the additional local bias due to heterogeneity (Freedman and Wachter, 1994), the errors resulting from smoothing the adjustment factors (Freedman et al., 1993) and many of the assumptions going into the loss function analysis (Freedman, Wachter, Cutler and Klein, 1994).

This careful scrutiny was possible, in part, due to the availability of three sets of numbers: the census counts, the adjustments and the extensive evaluation data. We view the controversy over