

## DISCUSSION

Michael Lee Cohen, University of Maryland  
College Park, Maryland 20742

In beginning my discussion of these five papers, I would like to mention my support of the recent court settlement, with the hope that statisticians will now, to some extent, be determining the utility of adjustment and, given a decision to go ahead, can decide the best way to accomplish it. I will first discuss the five papers presented in turn, and then I will make some general comments about adjustment research.

Childers and Hogan present results from the 1988 Dress Rehearsal Post-Enumeration Survey (PES). To begin, I point out that Ivan Fellegi was right when five or six years ago he predicted that computer matching would at best be able to match 70-80% of the records in this context.

The Census Bureau has obviously come a long way with PES methodology. These improvements have occurred in the areas of field work and interviewing, computer matching, sample design, and the use of post-strata and other smoothing techniques such as empirical Bayes regression modelling.

The success in keeping the percentage of unresolved matches in the dress rehearsal PES to 2% is very encouraging. What is responsible for this change, given that the corresponding figure for 1980 was around 9%? What is expected in 1990 for a national administration of the PES? This is, of course, a crucial factor, and will be a key element of any standards that are developed to compare the adjusted counts to the census counts.

Another question that I have is why did the alternate address matching take so long? Could the time be shortened for 1990? Also, how will the major changes in census procedures envisioned for 1990, especially the integrating of computers into census-taking, affect PES-taking? Has this been examined? Finally, can't the computer be used to greater advantage in computer matching? If the computer was only used for one day, it was sitting idle for at least a month, if not longer. While it was sitting idle, couldn't a wider blocking strategy be examined to search for matches further away geographically or even soundex-wise. As long as the computer is there it should be used as much as possible.

West, Corby, and Van Nest described the automated quality control system used to monitor and document the performance of the special matching group in Kansas. This group was involved in a difficult endeavor, matching sometimes incomplete, sometimes conflicting information from the census and the PES. I personally observed their efforts and the level of care that they demonstrated was very impressive. Matching the difficult cases is very intellectually demanding, investigative work for which federal retirees would be well-suited. The system designed by Corby is an A+ system. It kept track of all of the matching decisions, was fun to use by the staff of the special matching group, and could be used for an analysis of the likely rate of error of the matching group. This should give some degree of comfort to those concerned with the quality of the matching operation.

A question that I have is why can't the computer match be accomplished household to household, in addition to person to person. This might help the computer in deciding some of the unresolved cases. Also, what did the false computer matches look like? Obviously, if they had some common features, the computer-matching algorithm could be improved by modifying it to take these features into account. This type of feedback mechanism is essential to improving the computer matching algorithm.

Anolik presented results from the 1987 rural PES which gave a very optimistic picture of the potential efficacy of computer matching in rural areas in 1990. The rate of unresolved matches was very low, and the level of completion of PES interviews was very high. These and other results demonstrated the very careful work by all concerned. The results instill hope to all that the computer matching - post-enumeration survey methodology can be utilized in 1990 with some degree of confidence on a national scale.

However, the results might be too good. I got the impression that the methods used were more resource-intensive than would be anticipated in a national PES, and that the measures of the quality of the information and the matches might be overly optimistic. That is, were the resources devoted to the 1987 rural PES roughly the same as would be used in 1990?

The opportunity existed in the 1987 rural PES to measure the error of non-match due to the Soundex blocking. This should be done, since it might result in some ideas as to how the blocking by surname could be improved.

Mulry, Dajani, and Biemer's paper is a good start on estimating the matching error rate. The stratification using block matching error rate that they made use of should be evaluated by examining the correlation between the matching error rate and the block matching error rate. It is possible that other variables along with block matching error rate would provide an improved stratification? This is an extremely important area since the matching error rate is a key component of the assessment of the quality of adjusted counts. Stokes and Jones propose two variables to identify curbstoned interviews, year of birth and telephone number. These variables have the additional advantage that they are so innocuous that a curbstoner probably would not think hard about how often to make up responses to them. One question I have is whether one variable should be used initially with one kept in reserve with greater security, so that if the first variable becomes known, the second can catch curbstoners that pass the first test. In addition, I believe that the Census Bureau should plan to verify use of the non-match rate as a surrogate of curb-stoning. If this surrogacy is not validated, this research becomes less valuable.

### General Comments

Kirk Wolter has expressed two key concerns with respect to the use of PES methodology to adjust the 1990 census. First, the initial administration of a large sample is difficult to do without encountering some unforeseen snafus. Second, the difficulty posed by movers has not been adequately addressed to date. The problem can and is being approached on many fronts, including getting the PES interviewers into the field as early as possible, and learning more about the imputation models used to treat cases of nonresponse due to people moving during the census process. I expect little progress on the first problem, but there is much progress on the second.

Robert Fay has stressed the importance of integrating the evaluation of the post-enumeration survey and the associated matching operation into the program itself, rather than viewing the evaluation of the evaluation as something that is done after the fact, when records may have been lost and the opportunity to collect new information is gone. I agree that this integration process must be more fully accomplished.

One persistent problem with adjustment research is the limited number of people involved. This is for two reasons. First, the adjustment issue requires learning a great deal about the census process as well as the specific mathematical and statistical models that are used. Second, the confidentiality of the data restricts those that have access to these interesting and important data sets.

The second point is one I would like to address. Could the confidentiality be relaxed in this instance to allow a public-use file on the 1988 PES or even the 1990 PES to be released so that a wide variety of researchers could try out various adjustment-related techniques? This would not only benefit the development of models for adjustment, but the data sets are simply rich data sets that many statisticians could use for examples in Ph.D. theses, for interesting papers, etc., that would benefit a wider statistical community.

Of late, there have been some efforts to construct total error models of the error present in the PES. The most notable published efforts are those of Wolter and Hogan and Mulry and Spencer. These two papers represent a great deal of progress, helping us more fully understand the error present in the adjusted counts. The main difference between these two efforts is that Wolter and Hogan are primarily interested in understanding the mean of the error, whereas Mulry and Spencer attempt to estimate the variability of the error. My own feeling is that the quality of the information now available gives a slight advantage to the former type of analysis, but that eventually, gaining information about the covariance matrix of variance components of PES error will give more precision to the research, as evidenced in Mulry and Spencer. This is the direction in which we must go.

## Other Areas of Interesting PES Research

One problem that needs to be addressed is the resolution of the proper model to use for adjustment. A major component of this is how to integrate demographic analysis and systematic observation into the PES. Wolter has developed a model for combining demographic analysis with the PES information. More of this type of research clearly needs to be done. Otherwise a great deal of information will be lost. Also, the smoothing and variance reduction techniques used have to be more fully examined. Will effective post-stratification eliminate the need for the empirical Bayes regression modelling? In the regression model itself, what weighting should be used, and how should we select the independent variables? Also, what is the correct form of the dependent variable to model? Possibly the dependent variable, percent undercount, should be broken up into the following components: non-match rate, rate of erroneous enumeration, and rate of substitution. We might find that these components are more amenable to modelling than the rate of undercoverage itself. Also, we probably should model gross undercoverage and gross overcoverage separately. These all need to be more fully examined, using data from the dress rehearsal and possible improvements to the artificial populations.

I expect there to be further improvements in field work, especially more fully integrating the PES interviewing within the census process, which should reduce the problem posed by movers. Finally, I eagerly anticipate the development of standards for census adjustment, a logical outgrowth of the total error models discussed above.

The opportunity is here to make use of the existing research, and the interest of many talented statisticians to solve a hard problem with important public policy implications. The work reported here today is bringing us closer to solution.