DISCUSSION

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It is a pleasure to be a discussant for this session, as it gives me an opportunity to be involved again with SASS. I truly found these papers to very worthwhile. From the standpoint of what is interesting and useful to me, this is one of the very best sessions at the whole convention. I would particularly like to congratulate Steve Kaufman, who was remarkably a co-author of all four papers in the session.

Let me begin with a general comparison of the two estimation papers. Both papers deal with very difficult estimation problems, but take different philosophical approaches. The King paper takes the view that there is an operational problem for which the estimation method must be determined in time for the 93-94 SASS tabulations. In contrast, the Smith paper treats its estimation problem as a research issue - the problem is to be investigated and studied, with no rush to determine an immediate solution. Specific comments on these two papers, as well as the other two papers, follow.

I. Smith Paper on Intersurvey Inconsistency

This paper deals with a "simple" problem: controlling SASS figures to three sets of figures. The authors determined a generalized least squares (GLS) solution, which they could have just applied. However, they recognized that the "...real challenges...require statistical judgments". This is not an obvious conclusion that all investigators would have come to. I believe that many would have been satisfied with the initial GLS solution and would have applied it blindly without considering alternatives.

The authors began with a GLS method to minimize the sum of squares of the differences among the weights. I have observed instances where this was treated as the obvious and only possible quantity to minimize. I was very pleased to see that the authors of this paper did not do that and explored other minimizations as well. Personally, I find the motivation for this particular minimization weak.

I also commend the authors on working through the very simple example given in the paper. This was invaluable in assuring that the authors thoroughly understood what was going on, and also makes it very easy for a reader to understand.

I have one question. One of the alternatives considered was to reweight SASS to the Private School Survey by post-stratification, prior to applying the GLS procedure. I'm interested to know whether the post-stratification by itself gets SASS estimates close to Private School Survey estimates. If so, it might be feasible to only use post-stratification.

Finally, I wonder if there needs to be some movement towards the philosophy of the other estimation paper: If a decision is needed at some point, then the focus must be narrowed and a decision reached about which estimation methodology to use.

II. King Paper on Student Component Estimation

The student weighting in SASS is very difficult due to the complex survey methodology and the need to minimize the burden on schools. The weighting approximation that was derived appears to be a good choice to me, and I have no suggestions for improving it.

The original version of this paper stated that no further research was planned. I admired the honesty of this statement, as most papers talk about future research, even when there is little intent to conduct it. I was nonetheless pleased that the paper was revised to indicate that further research is planned. Since the need to estimate students is likely to be an issue for future years of SASS, it would be useful to evaluate how good the methodology here was. I suggest that an artificial data set be constructed, or/and that a full set of data be collected from a few schools. With such data sets, it will be possible to compare the "correct" estimates and the estimates using the methodology of the paper.

III. Kaufman Paper on Bootstrap Variance Estimator

Bootstrap variance estimation appears to a rather hot topic, in that there have been a number of papers at these meetings on the topic. In session #20, there were 3 papers on this topic:
Kovacevic, Yung and Pandher discuss the use of bootstrap variance estimation for quantile shares. Brodsky and Hughes provide a case study and a simulation. Robb also did a simulation study of bootstrap variance estimation.

Rao, in a different session, presented a review paper on re-sampling methods for variance estimation, including the bootstrap. Hinkins and Scheuren, in yet another session, included some rather disparaging remarks about bootstrap variance estimation in their wide-ranging paper.

This paper shows quite promising and encouraging results for bootstrap variance estimation, in that it does better than other methods. Robb, however, reported very much opposite results in his paper.

Perhaps Robb was not as clever as Kaufman in the application of the method.

Although I am not knowledgeable about bootstrap variance estimation, it appeared to me that determining j is rather cumbersome and difficult, and that this is an impediment to bootstrap variance estimation.

In general, this paper holds out the promise of making a substantial contribution towards the development of better variance estimates.

IV. Ghosh Paper on Optimal Periodicity

I found this an extremely interesting paper with a unique viewpoint. Agencies and policy makers may apply the objective approach presented in the paper to decide the periodicity of surveys, resulting in BIG efficiency gains. Of course, it is also possible that political considerations will preclude agencies from accomplishing any effective applications. I strongly encourage more research on the approach, with applications to additional surveys. I now make several specific comments and suggestions:

1. The paper assumes that survey estimates are unbiased. This is not realistic. I suggest that alternative assumptions are made, for example that there is a 5% relative bias. Such more realistic assumptions would lead towards relatively frequent periodicity as being optimal.
2. In Model 2, if the change is in the same direction as the periodicity bias, it is ignored. I do not see what the justification for this is, and suggest that the model be modified to not ignore the change in this case.
3. I recommend more study on SASS costs for the application of the methods. I realize that estimating cost components is quite difficult. Someone, perhaps Census Bureau staff, will need to spend a lot of time to produce good estimates of the cost components needed for the models.
4. Given the preliminary results of this work, I suggest that 1 year periodicity be evaluated as an alternative. Short periodicities of 1 or 2 years also have potential advantages of evening out survey costs among fiscal years.
5. I suggest the authors look at the work of Bob Fay on the Survey of Income and Education(SIE). Dr. Fay considered whether it was preferable to combine SIE and Current Population Survey for state estimates, or for SIE to stand alone. I believe his methods may also be useful for this work. I also suggest the authors look at the work currently being done by Chip Alexander and others at the Census Bureau on continuous measurement for the Census. Their methodology may have applications to this work.