DISCUSSION

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I will first discuss "On Link Relative Estimators" by Lillian H. Madow and William G. Madow.

The authors present an interesting problem that has arisen in the Bureua of Labor Statistics current employment statistics program. It is concerned with providing monthly estimates of employment, hours and earnings of workers on nonagricultural establishment payrolls. Benchmark employment is obtained every year from unemployment insurance administrative records. Monthly estimates of change between benchmarks are obtained from a monthly mail survey known as the 790 Survey.

As the authors have stated, the main difficulty afflicting this survey is the fact that the population of establishments may change from month to month. The authors therefore face one of the most difficult problems in survey methodology, namely, the problem of a changing population of establishments involving births, deaths, mergers, splits, and drop outs. It would probably carry "coal to Newcastle" (to use an English expression) if I were to point out to the authors (so expert in official statistics) that at least one similar situation arises in the so-called Hospital Discharge Survey (HDS) conducted by the National Center for Health Statistics. Here "the establishments" are short stay hospitals, the benchmark frame is the so-called Master Facility Inventory for hospitals. However, there are two notable differences, namely, a benchmark survey is only carried out for basic items which are used for the stratification of the frame and accordingly the HDS is not employing any "link relative estimators" since they have nothing to "hook on" in the benchmark year. Incidentally, this is an annual survey and not a monthly survey. However, this survey has to cope with the difficulty of a changing population and undoubtedly the authors will consult sources like these when they deal with this difficult problem which they say will be discussed in their published paper but has not been discussed by them today.

Turning now to their link relative survey design and associated estimators, the sample consists of samples of establishments, s_g, for which content items are recorded for both month g and preceding month g - 1. The probability selection of the s_g is left completely open but the link relative estimators of the content item for month g are defined by

$$Y'_g = Y_0 R'_1 \dots R'_g = Y'_{g-1} R'_g \qquad (1)$$

where $R'_g = \frac{Y'_{gg}}{Y'_{g-1\,g}}$ and $Y'_{gg} = \sum_{i \varepsilon s_g} Y_{gi}$ and $Y'_{g-1\,g} = \sum_{i \varepsilon s_g} Y_{g-1\,i}$.

The authors' paper consists of a very careful study of these estimators both for a finite population probability background as well as for a super-population background. Specificially, this study is concerned with the possibility of biases arising from the fact that through the changing population, the samples designated to be drawn on the basis of the benchmark frame may obviously no longer lead to unbiased estimates of the change ratios for the new total population of establishments, certainly not for the probability sampling design.

Rather than commenting on the interesting model study, I prefer to raise some questions about both the estimators and the design used. With regard to the estimators, it is obvious that an estimate of the content item at month g could be based on a combination of the link relative estimator given by equation (1) with the content items for month g from both samples s_g ans s_{g+1}. The combined use of the link relative estimator and the "current estimator" provided by the above two samples would correspond conceptually to the so-called composite estimators used for example in the Current Population Survey and well known to the authors.

Finally, I would like to turn to what would appear to be an obvious improvement in the design which I would like to refer to as a "longitudinal survey." With this design, a sample of establishments would be drawn from the benchmark frame and this would be followed up right through the twelve months or until the death or drop out of each individual establishment. For the subsample of establishments 'surviving' at least until month g, an estimate of the content item would now simply be given by a single link ratio applied to the benchmark value, i.e., by $\dot{Y}_g = Y_0 \dot{R}_g$. Moreover, for an approximately equal work load of establishments sampled, the sample sizes for the longitudinal surveys could be approximately double the sample sizes of those for the link relative estimators. If this is so, it is easy to show (by switching to the logarithms of the content items) that the variance of the longitudinal estimator is considerably smaller than that of the link relative estimators. My algebra assumed an exponentially decaying log correlogram. Moreover, it can under very general conditions be regarded as an unbiased estimator for the subpopulation of the benchmark frame that will 'survive' to at least month g. The longitudinal estimators could also be combined as weighted averages with the current estimators available at month g. Again, these weighted averages appear to be more precise than the link relative estimators combined with their current estimators.

Since I believe that the discussion of the changing population by the authors will eventually lead them to a redesign of Survey 790, I would like to draw their attention to the above estimators for it appears to me that they offer the possibility of dealing with both deaths and births (though not with drop outs) if provision is made for supplementing the sample every month by sampling newly born establishments.

I will not comment here on the treatment of mergers and splits. This can be safely left in the capable hands of the authors.

Next I will discuss "Mileage and Other Complex Estimates, and Some Bias Research on the National Travel Survey and the National Personal Transportation Survey" by Joan Kahn, Dennis Schwanz, and Larry Carstensen.

This paper represents a report on an interesting study based upon the National Transportation Survey (NTS) and the National Personal Transportation Survey (NPTS). Since the surveys have been admirably described in the paper, we shall not repeat such a description.

Under normal circumstances, a survey concerned with travel characteristics would have to face a most difficult screening problem since there are many households which would not contribute to many of the content items. However, the Bureau of the Census is apparently in the fortunate position that two already existing household surveys, namely, the Current Population Survey (CPS) and the Quarterly Housing Survey (QHS) can be used as vehicles to carry the questionnaires required for the present studies. Moreover, the content items cover a great variety of information, for example, they also cover the use of public transportation so that households not contributing to travel by a privately owned vehicle may well contribute to the public transportation content items. Nevertheless, a "supplemental sample of 4,000 housing units" from the CPS PSUs had to be drawn in certain states to get sufficiently reliable estimates for selected items in the NTS. Since the survey is already multiple frame (the CPS frame and the QHS frame), I would like to ask the question whether this supplemental sample could have been deliberately drawn from an "informative" subpopulation of households, namely, households with at least two registered vehicles. It is conceivable that this would increase the reliability of estimates concerned with "trips". However, there may well be reasons against such a procedure.

A second minor question is whether the sample sizes of 18,000 housing units from the CPS and 7,000 housing units for the QHS in the case of the NPTS survey, were computed from optimization formulas allowing for the differential costs and differences in the design of CPS and QHS using two frame optimization techniques. A similar question arises in conjunction with the NTS survey.

The combination of the separate estimates that could be computed from the CPS and QHS sections of the sample is achieved by applying universal weights of W = .775 and 1 - W = .225 to these two survey estimates or directly to the content items recorded in the two separate surveys. While this procedure is certainly very simple, I would like to raise the question whether such weights could not have been differentially chosen for different content items, again using two frame optimization techniques. Again, I regard this as a minor comment.

I have no criticism of the biases study and on the whole, the authors have to be congratulated for their interesting presentation.

Finally, I will discuss "Ratio and Regression Type Estimators Employing a Priori Knowledge" by Paul H. Tomlin.

I cannot really claim to have followed the involved concepts of this paper since many definitions are somewhat ambiguous.

Apparently, the y-values associated with a finite population of size N are assumed to follow a prior distribution with means M_k (for unit k) and variance covariance matrix V. The criterion for the merit of a design D apparently is the so-called "conditional a priori mean square error Cap MSE" which is apparently defined by

$$\text{Cap MSE} = E[(r - R_0)^2 | \kappa, \underset{\sim}{u}] \qquad (2)$$

statistic — a priori model

prior mean — sampled units

If the sampled units $\underset{\sim}{u}$ are given, then r is a variable only because of the prior distribution of y. It appears to me that this quantity is not relevant for inferences usually required in finite population sampling. Surely, we would be more concerned with a quantity like

$$E[(r - R)^2 | \kappa, D] \qquad (3)$$

statistic computed from sample — statistic computed from finite population — a priori model — survey design

Even with this more appropriate objective function, I would hesitate to optimize it (minimize it) given κ, D. For it depends on an assumed super-population model κ and it is a golden rule that we need robustness against a model breakdown, rather than variance minimization.