DISCUSSION

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#1. Kalsbeek, Mendoza, and Budescu: New Model

The proposed new model is an expression that gives travel expenses as a function of size of interviewer assignment area, No. of PSU's in area, No. of PSU's to be visited in one trip and No. of callbacks to be made. It extends work of HH&M and is most welcome. The derivation seems eminently reasonable and actual survey expenditures should be found to follow the functional form, although further experiences or a review of existing ones would be required to establish the conformity between actual expenditures and the expression.

The authors compare recommended optimum PSU sizes based on three expressions for travel expenses: the simple one, the HHM and the new one. It seems that the recommendation based on ignoring travel expenses, the simple one, calls for too small PSU's, although there is no very serious loss of precision until the survey is taken to cover all of the US. In practice one may prefer to use the optimizing formula based on the simple model but also use some judgement in changing $C_{1\sim}^{(S)}$ and $C_{2\sim}^{(S)}$ so as to take account of travel expenses. This judgement could be sharpened by applying the authors' vision of how interviewers travel about.

#2. L. R. Ernst: Controlled Selection

The paper furnishes a way of tightening the control of the two-way stratification method given by Bryant, Hartley and Jessen (1960). I have been calling their method "merging random permutations" because of the way I carry it out. That is, a two-way stratification design selection can be exhibited as two columns of strata identifiers one for each "way." The two identifiers in each row point to a cell where a selection is to be made. By permuting the second column the cell selections are changed but the marginal selection numbers are "controlled." If there are, for example, two or more identifiers for strata in both ways then cells may be hit none, one, or two times and this may constitute too much loss of "control."

The author's method, if a solution exists, allows cells to be hit zero or one times, or one or two times, or two or three times, etc., but with no more flexibility relative to cell quotas. This method may be called "deep control," in parallel to the terminology "deep stratification" that describes multi-way subdivision of the population. I wonder if my merging random permutations approach could not be used after satisfying cell quotas up to none or one additional selection.

#3. Drummond: Workload Bias

The paper describes a variety of options for scheduling field work with a sympathetic appreciation for the realities of enumerating. The title of the paper suggests that imposed randomization might combat bias. Although I found expressions for inclusion probabilities, I don't believe there was even an expression for the estimator, much less its bias or variance. Since there is some cost to randomize, if only the looking at a random number, there ought to be some reduction in bias, if only a half of one percent. Some judgement of the probable amount of improvement would help in deciding whether to advocate the method.

In my own sample design practice I try to create subsamples (A series, B series, etc.) both replicated in Deming's sense and interpenetrating in Mahalanobis'. One instruction to the field workers would be to finish the A series before doing the B's, etc. A question may be posed as to the optimum number of subsamples to form with a sample of size n. There may be one, of size n, or two of size n/2, or three of size n/3 up to n of size n/n = 1. With r subsamples each of size n/r the instruction would be to assign the labels A, B, etc. randomly and then enumerate A series, B series, etc. One stops when money or time runs out and throws away the data on the incompleted subsample. Bias is always zero. The waste would be least for r = n but travel costs would also be maximized. What value of r is best?

#5. Charles R. Perry: Information

The author deals with recovery of ground-based data from photo interpretation of a satellite image. The data he uses as illustrative are qualitative, crop types, and he shows how Fisher's measure of information can be applied to characterize the quality of the photo interpretation. I confess I had not known how Fisher had introduced his notion of information and I enjoyed the author's presentation of Fisher's viewpoint. There have been questions raised as to the relative appropriateness of Fisher information as compared to the "n ℓog n" or communication theory type of information measure as used in Information Theory and Statistics by Kullback (1959). For example, Fisher's information rather unfortunately goes infinite as p goes to zero or one, while the communication theory type of quantity rises to zero as its maximum.

We could continue discussing "appropriateness" without settling much. What is needed is a clear statement of the problem and then we would be led to calculate some "best" estimate which might lead us to one or the other measure of information. When I described the problem to myself as one of having many, many photo interpreted pixels along with a few ground-based measurements and wishing to estimate the ground-based measurements over all many, many pixels, the sampling design was then recognized as the two-phase one, also called double sampling. Having named it, I looked into the JASA index and sure enough the problem had been answered for binary data by Aaron Tenebein (1970) 65: 1350.

Tenebein suggested a quantity K, the square of the correlation coefficient between the ground-based and the photo interpreted zero-one data, as a measure of quality of the photo interpretation. The variance formulas and optimum allocation of effort between phases become very simple expressions in terms of K. I suspect that there are still fertile fields of statistical investigation open to extend this model to polytomous (not just binary) data and also to three-phase (aerial photo too) sampling. For the present application, a particularly important extension would be the case in which estimates are needed for a number of strata although ground-based measurements are available from some but not all strata. These extensions also direct our attention more to the proportions of various kinds of misclassifications as well as to a summary measure of agreement.

#6. Lautenschlager and Perry: Comparison of Vegetation Indices

The paper furnishes background information on remote sensing using the Landsat bands that I found most fascinating. The listing of indices was less gripping, but their clustering was of some reasonableness. Then the authors introduce the concepts of decision rules and equivalence classes that seem very close to the notions of test in statistical inference. I began to look for a comparison of indices in terms of, say, their asymptotic relative efficiencies, but couldn't find it. Actually the paper seemed to stop in mid-argument. It was marked "Working Draft" and perhaps the final version will arrive at some comparison of power or of efficiency.