A Survey Practitioner's Viewpoint

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1. INTRODUCTION

A prime objective of statisticians at the Bureau of the Census is to provide accurate and timely data from censuses and surveys in a cost-efficient way. Since the majority of the surveys we conduct are periodic and recurring, it is also important to us that the data are comparable from one time period to the next. By being extremely careful about methodology we believe the public will find our data credible. Since so much public policy is determined and so many public funds are distributed on the basis of data from these recurring surveys, measures to insure public credibility are very important.

Survey design and methodological procedures to insure accurate, timely, comparable, and cost-effective data go far beyond the selection of a sampling design, a survey estimator, and a statistical model for the characteristics of interest. They must also include such issues as: selection of a frame, choice of sampling unit, periodicity for recurring surveys, questionnaire design, choice of lengths of recall, collection method, measurement error, nonresponse, treatment of outliers, and seasonal adjustment.

A survey strategy may have excellent features but the data produced may be completely incomparable or biased unless careful attention is paid to these other features. Though nonsampling errors in large national surveys are far more serious than sampling errors, recognition of the effects of nonsampling errors have, for the most part, been ignored in the discussions of the foundations of survey sampling.

Models play a large role in the design of surveys at the Bureau. They are used in different ways for the large, national, household surveys which have multiple uses and multiple users than in some of the smaller industrial surveys which tend to have a narrower focus. Thus, Bureau practice of a probability sampling design that ensures that confidence intervals can be computed which will be valid for large enough samples for a wide range of characteristics is important when we cannot specify ahead of time all the multiple parameters of interest.

It is also important for data that are to be used in the determination of public policy that the distribution-free aspects of the Bureau's survey data be maintained. Randomization and lack of dependence on assumed models for inference purposes give credibility to the data.

On the other hand, in many of the surveys of establishments, the distribution of establishments by characteristic is highly skewed. Since there is interest in many characteristics, and they are not all highly correlated, one cannot assume a single simple model would be appropriate or meaningful for all the characteristics of interest. But there are some surveys which do have a more narrow focus and for which the Bureau uses model-dependent procedures.

As stated by Smith (1976), there is not such a wide gulf between the advocates of the design-based and the model-based methods, and there is probably very little disagreement about the design of large multi-purpose surveys. There is little disagreement about the role of randomization for such surveys for protection against selection biases. There is no disagreement on the part of those who favor design based methods that models play a big part in survey design. However the design-based methodologists do believe that estimation to a finite population should not be dependent on assumed models. Though this is true for most of the Bureau surveys, there are some exceptions.

In the remainder of this paper, we will examine the uses of randomization, the ways in which survey practitioners at the Bureau use models and special procedures used to assure robustness. In the final section a real population of establishments is used to illustrate the differences among several different types of survey estimators.

2. ROLE OF RANDOMIZATION

As stated by Hansen, Madow, and Tepping (1978), the use of randomization and consistent estimators, along with a large enough sample size so that the Central Limit Theorem holds is a way of achieving robustness of results. There are also political reasons to favor the use of randomization. Randomization removes personal biases in the selection of a sample and thus increases public acceptance. It also permits other data users to reanalyze survey data.

However the use of randomization in sample selection is usually restricted. No surveys at the Bureau use simple random sampling over all members of the population. All of the surveys in which randomization is an essential element use stratification to reduce variability. Many of these surveys include a "certainty stratum" which contains very large sampling units which will be drawn into the sample with probability one. This is true in the household surveys as well as establishment surveys. Systematic sampling is often used, and in some surveys controlled selection. The net result is to restrict the set of samples that are given positive probability of selection.

3. ROLE OF MODELS

Most of the discussion on the role of models in sample surveys has focused on the use of prior or superpopulation models in inference. Since we prefer to enlarge the discussion to total survey design and not just sample design, we also want to enlarge the scope of models that we discuss. As stated by Hansen, Madow, and Tepping (1978), the proper use of models has much to contribute to survey design, ordinarily within the framework of probability sampling.

Models are used extensively at the Bureau in all phases of survey planning. We agree with

Sarndal's (1978) view that "Every survey sampling statistician would probably in the long run arrive at a philosophy where probability sampling elements and model-based elements are mixed, but where emphasis varies......." Though we use models extensively, for planning survey methodologies, we do not depend on them for purposes of making inferences about the superpopulation models to finite population. We also do not use them, except in rare cases, in the selection of sample units.

However, we believe strongly that the use of models improves the choice of survey designs, and estimators, and other survey methodologies. We shall describe briefly some use of models in survey design.

3.1 Sample Design

Given the goals and constraints of a survey, an essential task is to determine the sampling frame and the sample unit. Considerable care must be taken on this point especially if the survey is to be periodic. Considerations such as the handling of births and deaths in the population as well as mergers and divestitures in the case of business establishments must be allowed for in the selection of frame and sampling unit. Sometimes models are used in the determination of sample unit such as in modeling intraclass correlations with respect to cluster size in an area sample. At other times specific models are not used and the determination is based on knowledge obtained by practical experience.

In panel surveys (overlapping or not), models have been constructed to account for different proportions of overlap between successive samples as well as to account for the amount of periodicity in panel usage. When overlapping samples are used, models for improved estimator construction are developed for estimating change over successive occasions.

Superpopulation models are also used in the determination of sample design and estimation; however, in almost all cases estimators selected are consistent with respect to the sample design. To quote Hansen and Madow (1978), "The specification of the design utilizes substantive and statistical judgments and varying amounts of information available concerning the population and its characteristics, in such a way that good judgments, including good use of prior information, will reduce the mean square error per unit of cost, but poorer judgments, or larger errors in the prior information, may lead to larger mean square errors but the estimator will nonetheless be consistent." This statement embodies the view of Bureau practitioners of sample design when sample sizes are large.

3.2 Questionnaire Design

Considerable attention is paid to possible biases arising from the questionnaire. 'It is fairly well established that the context in which questions are placed, the length of the recall period, and the format of the questionnaire affects the replies. Models are used in the development of a survey questionnaire to reduce the amount of bias in the results.

3.3 Collection Methods

Survey practitioners are eager to reduce the cost of surveys. Using the telephone for data collection is one way to reduce costs. In fact many surveys in marketing and in academic settings are based on random-digit dialing methods in which the set of all assigned telephone numbers is the frame for the survey. The fraction of the population having telephones is about .93 in the U.S., but those people who do not have telephones are more likely to be unemployed, to have acute health conditions, to suffer criminal victimizations, and to exhibit several other characteristics of interest in setting public policy. Therefore, it is essential that those people be represented in our surveys. This implies a dual-frame design with the sample split between the two frames -- one a frame of telephone numbers and the second an area sample frame to pick up nontelephone households. A considerable amount of modeling work is currently underway to determine the appropriate sample allocation for the two frames.

3.4 Measurement Error

Measurement errors in sample surveys are caused by respondents, interviewers, coders, and the interaction among them. The Bureau developed a mean-square error model to show the combined effects of sampling and measurement error on the estimator of a population mean. The use of interpenetrated subsamples as suggested by Mahalanobis has been the basis of the estimation of the parameters of that model. The results are used to help determine if resources should be used to increase the sample size thus reducing sampling error or to reduce sample size in order to use resources to reduce interviewer or coder error.

Models have been used to help in interpreting other survey phenomena, one of them being inconsistencies in what is known as "gross flow" data from the Current Population Survey. This phenomenon is illustrated by large changes in labor force status of people in sample in consecutive months. Fuller and Chua (1982) have developed a model to describe the problem as have Zellner and Abowd (1982).

3.5 Nonresponse

No survey is fortunate enough that complete response is realized. There is always some residue of nonresponse from units that could not be contacted. Models are implicit in all of the procedures the Census Bureau uses for nonresponse adjustment. In recent years we have begun experimenting with explicit models for such adjustments.

3.6 Estimation

The choice of a survey estimator depends either implicitly or explicitly on the use of models. But such models are not limited to superpopulation models used by authors such as Royall, Godambe, and so forth. Rather, the models must take into account the total components of survey design such as data collection and processing as well as constraints such as cost. The existence of an auxiliary variable, x, correlated with a variable of interest y may dictate and indeed result in the choice of a ratio or regression estimator whether they are optimum with respect to a superpopulation model or not.

4. PROCEDURES TO ENSURE ROBUSTNESS

As we view it robustness in survey work pertains to the issue of satisfactory performance of survey designs and estimators when models, previously thought to be correct and used in the survey planning, are not valid at the time of the survey. A survey strategy is felt to be robust when despite model failure, the goals of the survey can still be achieved with minor loss in precision over that obtainable with no model failure.

As stated previously, the use of randomization and design-consistent estimators together with large sample sizes tends to provide robust survey designs. The effect of outliers on sample estimates can be treated, for example, in panel surveys, by estimation procedures that reduce sample weights of such outliers but requiring additional reporting in successive panels. We feel that stringent editing routines that check for logical relationships among data reported as well as analyst review reinforces the "robustness" property of our survey methods.

Much effort is expended in maintaining and updating survey coverage of the target population to eliminate biases in the survey estimators. Details such as removing deaths and adding births permit survey estimates to be comparable over time. Large per unit costs for some segments of the population are tolerated in attaining complete coverage. This occurs in populations of business establishments where the smaller ones tend to enter and leave frequently and survey estimates for the entire population are required monthly. Because coverage is complete it is felt that large changes in characteristics can be satisfactorily detected. For example, such a survey will be satisfactory and robust in a severe recession where many small establishments leave and few establishments enter in a disproportionate manner to the entire population.

5. A NUMERICAL EXAMPLE

In the following numerical work, the estimation of a total for several data items collected in a 1979 annual survey of confectionary establishments is considered. The characteristics to be examined are quantity and value of shipments for chocolate and non-chocolate confectioners and for their combined totals. The universe is highly skewed with 40 percent of the establishments accounting for 90 percent of the total. To simplify the comparison of methods, the original universe of 250 establishments was reduced to 203 by eliminating establishments that had gone out of business or had recently come into business. The data file used contained both 1978 and 1979 data for each characteristic of interest. A sample size of 50 was arbitrarily chosen for the analysis.

5.1 Cutoff Sample Versus Stratification

In this section we utilize the development related to the use of cutoff samples as outlined in Hansen, Hurwitz and Madow (1953) Vol. I, page 486 in which the class of ratio type estimators $r = [W_1r_1 + W_2r_2]X$ in a two stratum design with simple random sampling within strata is examined with respect to mean square error (MSE). Notationally, in the present context r_i, i=1, 2 is the ratio of sample sums of 1979 to 1978 data for stratum i based on a sample of size n_i, $n_1 + n_2 = 50$ and W_1 is a weight constrained to lie in the interval [0,1]. The X represents the known 1978 total for the characteristic of interest. The class of ratio type estimators contains the separate ratio estimator. The strata were formed by assigning all establishments with 1978 total value exceeding 12 million dollars in stratum 1. The results for each of the six characteristics examined (e.g., total quantity of chocolate, total value, etc., for 1979) were similar. In every case, the MSE(r) was minimized for $n_1 = 50$ and $W_1 = 1$ indicating the optimality of a cutoff sample with a ratio estimator. The mildly interesting observation with respect to the characteristics was that the establishments in the cutoff sample represented only 63 to 90 percent of the total. Since the data represented two successive years the overall rates of change differed little from the rates of change of the cutoff sample. However, characteristics where there were some differences in the rates of change and where the cutoff sample represented 63 percent of the total yielded MSE's that were similar in size to designs specifying samples from both strata. Should the differences in the rates of change increase over time, the stratified random sample design would yield the smaller MSE.

5.2 Traditional Estimators and a Stratified Design

The universe of confectionary establishments described above was used further in comparing some traditional estimators of total under a stratified design. The establishments were stratified by size of 1978 total value of shipments and sample sizes were allocated to strata using the variables 1978 and 1979 non-chocolate quantity of shipments under Neyman allocation. Four strata were designated including a certainty stratum of 17 units. Keeping with a total sample size of 50, the remaining 33 sample units were allocated to the other three strata. Five estimators of total including the usual stratified estimator denoted $\hat{Y}$ were examined. The other four were the combined ratio, $\hat{Y}_{CR}$, the separate ratio, $\hat{Y}_{SR}$, the combined regression, $\hat{Y}_{CG}$ and the separate regression, $\hat{Y}_{SG}$. The estimators can easily be found in Cochran (1963) so they are not detailed here. For $\hat{Y}$ the stratum sample sizes were 9, 14 and 10 and for the other four they were 10, 11 and 12. In estimating each 1979 characteristic, the same 1978 characteristric was used as an auxiliary variable. For every characteristic of interest, the coefficient of variation (C.V.) of $\hat{Y}$ was at least double that of any of the remaining estimators. It was also observed that there was almost no difference in C.V. among any of the four remaining estimators. Finally, note that the C.V.'s were computed using large sample approximations to the variance.

5.3 Monte Carlo Investigation

Given the stratified design described in the previous section with stratum sample sizes 10, 11 and 12 a limited Monte Carlo investigation was conducted. A total of 150 stratified samples was generated, each consisting of combinations of systematic samples selected from within each stratum. The establishments within each stratum were ordered by their 1978 total value of shipments prior to sampling. In addition to the five estimators of total mentioned in section 5.2 four other estimators were investigated. Two of the estimators were motivated by a prediction theory approach (Royall, 1981) and two from a Bayesian approach.

The study focused on a single characteristic, namely 1979 value of shipments (y) with 1978 total value of shipments (x) as an auxiliary variable. It was assumed that the superpopulation variance of y was proportional to x. The first prediction theory based estimator considered was $\hat{Y}_{GLS}$ where $\hat{Y}_{GLS} = \sum_{i\varepsilon s} y_i + \hat{\beta}_{GLS} \sum_{i\varepsilon s} x_i$ and $\hat{\beta}_{GLS} = (\sum_{i\varepsilon s} x_i)^{-1} \sum_{i\varepsilon s} y_i$. The second prediction theory based estimator was $\hat{Y}_G$ where $\hat{Y}_G = \sum_{i\varepsilon s} y_i + (N-n)\hat{\beta}_0 + \hat{\beta}_1 \sum_{i\not\varepsilon s} x_i$ where $\hat{\beta}_0$ and $\hat{\beta}_1$ are the usual weighted least squares estimators under the model $y_i = \beta_0 + \beta_1 x_i + e_i$ $i = 1, 2, \ldots, n$ and the e_i are distributed independently $(0, \sigma^2 x_i)$. The first estimator is described in Royall and Cumberland (1981). The second estimator is a direct application of weighted least squares estimation of the regression coefficients.

The remaining two estimators are derived via a Bayesian argument and use prior data relationships. That is, in addition to a linear model relating y and x, a relationship between x and z (where z represents data for a year prior to that for x) is also used to estimate the relationship between y and x. Assume that 1) $y_i = \beta_c x_i + \alpha_i$ where $\alpha_i \sim N(0, \sigma^2 x_i)$ and 2) $x_i = \beta_p z_i + \gamma_i$ where $\gamma_i \sim N(0, \sigma^2 z_i)$. Hence the data are assumed to obey a regression relationship but the regression parameters may change from year to year. The two point estimators of total presented below minimize the posterior expectation of the quadratic loss function under separate specifications of prior distributions of the regression coefficients. In the first case, $\beta_c = \beta_p = \beta$ and β is assumed to have a noninformative prior. The Bayes estimator of total in this case is $\hat{Y}_B = \sum_{i\varepsilon s} y_i + \hat{\beta}_B \sum_{i\not\varepsilon s} x_i$ where $\hat{\beta}_B = \lambda\hat{\beta}_c + (1-\lambda)\hat{\beta}_p$,

$$\hat{\beta}_c = [\sum_{i\varepsilon s} x_i]^{-1} \sum_{i\varepsilon s} y_i, \quad \hat{\beta}_p = [\sum_{i\varepsilon s} z_i]^{-1} \sum_{i\varepsilon s} x_i \text{ and}$$

$$\lambda = ([\sum_i^N z_i]^{-1} + [\sum_{i\varepsilon s} x_i]^{-1})^{-1} [\sum_i^N z_i]^{-1}.$$

The second estimator is based on the prior distribution specifying that β_c given β_p is normally distributed with mean β_p and variance δ^2 and that β_p has a noninformative prior. The estimator of total in this case is $\hat{Y}_C = \sum_{i\varepsilon s} y_i + \hat{\beta}_M \sum_{i\not\varepsilon s} x_i$ where $\hat{\beta}_M = \phi\hat{\beta}_c + (1-\phi)\hat{\beta}_p$, $\hat{\beta}_c$ and $\hat{\beta}_p$ are as previously defined,

$$\phi = [\delta^2 + (\hat{\sigma}^{-2} \sum_i^N z_i)^{-1} + (\hat{\sigma}^{-2} \sum_{i\varepsilon s} x_i)^{-1}]^{-1} \cdot [\delta^2 + \hat{\sigma}^{-2} \sum_i^N z_i] \text{ and } \hat{\sigma}^2 = (n-1)^{-1} \sum_{i\varepsilon s} (y_i - \hat{\beta}_c x_i)^2 x_i^{-1}.$$

A prior was not formulated for δ^2. Instead δ^2 was estimated in a rough manner by using published totals from previous years and computing the sampling variance of ratios of current to previous year data for eight successive years.

The nine estimators together with their variance estimators were applied to each of the 150 samples in the Monte Carlo investigation. In the case of the prediction based estimators, estimates of their error variances (see Royall and Cumberland (1981)) were computed. The variance estimators of the traditional estimators mentioned in section 5.2 can be found in Cochran (1963) and will not be repeated here. We let $v(\hat{Y}_{SG})$ denote the estimator of variance of the separate regression estimator and use similar notation for the other traditional estimators in the graphs below. Three estimators of the error variance of Y_{GLS} were considered. The first two estimators, $v_1(Y_{GLS})$ and $v_2(Y_{GLS})$ are denoted v_L and v_H respectively by Royall and Cumberland (1981). The third, $v_3(Y_{GLS})$, is similar to v_H when the lower order term is omitted. It is sometimes used as an estimator of variance of a ratio estimator of total under a simple random sampling design. The estimator of the error variance of Y_G is straightforward and is denoted $v(Y_G)$. Finally, the estimators for Y_B and Y_C are $v(Y_B) = (N-n)\sigma^2 + \sigma^2[\sum_i^N z_i + \sum_{i\varepsilon s} x_i]^{-1} [\sum_{i\not\varepsilon s} x_i]^2$ and $v(Y_C) = (N-n)\sigma^2 + [\sigma^{-2} \sum_{i\varepsilon s} x_i + (\delta^2 + \{\sigma^{-2} \sum_i^N z_i\}^{-1})^{-1}]^{-1} [\sum_{i\not\varepsilon s} x_i]^2$ where σ^2 has been previously defined.

5.4 Discussion

Figures A-D present results for the estimators Y_{SG}, Y_{GLS}, Y_G and Y_C. The others were eliminated because of space limitations but are included in the discussion. The results represent calculations based on a 21-term sliding window. They are similar to centered 21-term, moving averages, except some of the results are not averages. For a given estimator of total Y and a given estimator of its variance v(Y), the terms ERROR (Y), $(MSE(Y))^{1/2}$, and $(v(Y))^{1/2}$ denote ERROR (Y) $= 21^{-1} \Sigma (Y-Y)$, $(MSE(Y))^{1/2} = (21^{-1} \Sigma (Y-Y)^2)^{1/2}$, and $(v(Y))^{1/2} = (21^{-1} \Sigma v(Y))^{1/2}$, respectively, where the summations are over the 21 consecutive samples in a given window. These data are plotted versus $\bar{x}_s = 21^{-1} \Sigma \bar{x}$, where once again the summation is over the 21 samples in the window. The samples were ordered by value of x_s prior to these calculations. See Royall and Cumberland (1981) for a more complete description of this type of graphic.

As expected, the simple stratified estimate did not perform as well as the other four traditional estimators. Of the four estimators Y_{CR}, Y_{SR} and Y_{SG} behaved similarly with Y_{SG} tending to have marginally smaller error. The combined regression estimator, $\hat{Y}_{CG}$ and the first Bayes estimator $\hat{Y}_B$ were downward biased. This behavior deserves further study. The smallest errors occur for the prediction-theory estimators $\hat{Y}_{GLS}$ and $\hat{Y}_G$ and for the second Bayes estimator $\hat{Y}_C$. The empirical behavior of all three estimators is similar.

The estimators of standard error tend to track $(MSE)^{1/2}$ fairly well for the randomization theory estimators $\hat{Y}_{CR}$, $\hat{Y}_{CS}$, $\hat{Y}_{SG}$. Once again, however, there are some problems with the combined regression estimator, particularly for samples with small $\bar{x}_s$. For the prediction theory estimators $\hat{Y}_{GLS}$ and $\hat{Y}_G$ the variance estimators tend to be too large, but only marginally so. For the first Bayes estimator the variance estimator tends to be too large, but the estimated standard error for the second Bayes estimator tracks $(MSE)^{1/2}$ extremely well.

Obviously these results are favorable to the prediction-theory and Bayes-theory estimators. We are concerned, however, about whether the results are sustained when longer time lags occur between model specification and model estimation or when multiple characteristics are involved in the survey. We intend to look at some of these questions in future work.

We note that all of the 150 samples considered in this study might be viewed as both "realistic" from a randomization-theory point of view, and "balanced" from a prediction-theory point of view. That is, each sample consists of a certainty stratum and a stratified-systematic sample from the balance of the population. Our sample space does not contain any abberant samples as presented, e.g., by Royall and Cumberland (1981), and thus we did not observe a wide range of differences between the various estimators. Evidently well-chosen probability sampling designs impart a robustness quality to the survey estimators whether they be randomization-, prediction-, or Bayes-theory based.

Table 4 presents results for $\hat{Y}_{GLS}$ and $\hat{Y}_G$ for the cutoff sample (50 largest units) and for the best-fit sample (a centered systematic sample from the entire population). The error in the estimator of total is quite small for both samples, certainly competitive with the level of error in the estimators displayed in Figures B and C. It is interesting to note that the estimated standard errors for the best-fit sample are much larger than for the earlier 150 samples, while the estimated standard errors for the cutoff sample are much smaller than for the earlier samples.

Finally, Figure E displays results for the studentized statistic $t = [v(\hat{Y})^{-1/2}](\hat{Y}-Y)$ for the separate regression estimator and the two prediction-theory estimators. The figure plots the empirical distribution function of t for each of the three estimators, along with the distribution function of a standard normal variate. The t associated with the randomization-theory estimator $\hat{Y}_{SG}$ behaves nicely, being reasonably well approximated by the standard normal distribution. This is not the case, however, for the two prediction-theory estimators $\hat{Y}_{GLS}$ and $\hat{Y}_G$, where the corresponding t's show important departures from normality. These results are interesting because they run counter to the earlier results in Figure A to D, where the prediction-theory estimators performed better than the randomization-theory estimators. If the present results are sustained by additional empirical work, they surely suggest that inferences from the prediction-theory approach tend to be conservative. The t-distributions associated with $\hat{Y}_{GLS}$ and $\hat{Y}_G$ are evidently shorter tailed than the standard normal distribution, and if normal theory confidence intervals were constructed using such methods the actual rates of coverage of the true Y should exceed the nominal confidence levels.

6. SUMMARY

Obviously this paper is incomplete. When we began the empirical work we wanted to look at how well the models held up over time and how well the models constructed for one variable worked for other variables in a survey. Unfortunately, time ran out. We have learned something from this work, though, and will continue our research.

The Bureau uses a fairly large number of cut-off surveys in the industrial area. Part of the reason for doing that is that most of the monthly surveys are voluntary, not mandatory, and the small establishments have a poor record of responding. That is partly the Bureau's fault because we expend our greatest efforts in getting responses from the larger establishments, but even on studies where strenuous efforts have been made, it is difficult to get the small establishments to report. When the Bureau uses cut-off surveys, we publish no estimates of uncertainty with the estimates of totals and means. The work in Section 5 leads us to believe that there may be better ways of using model-based designs for these small universes that would also permit us to inform users about variances.

The work in Section 5 also suggests that additional research is required in the treatment of births and in the treatment of universes that require estimation of several characteristics and where model specifications do not hold everywhere.

In summary, the Bureau uses models in many phases of its survey work, but we believe that for our large, national, multi-purpose surveys the practice of probability-sampling designs is necessary.

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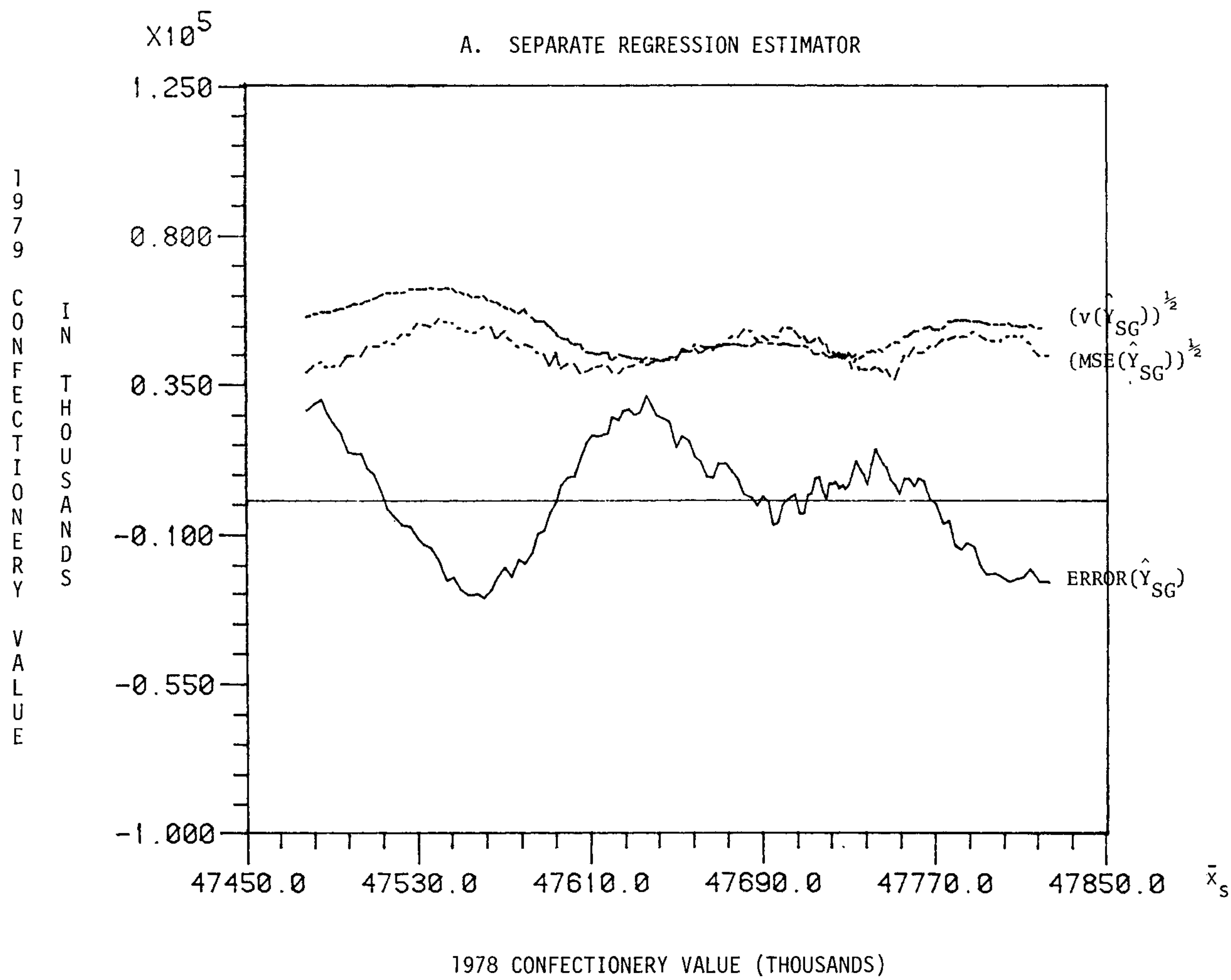
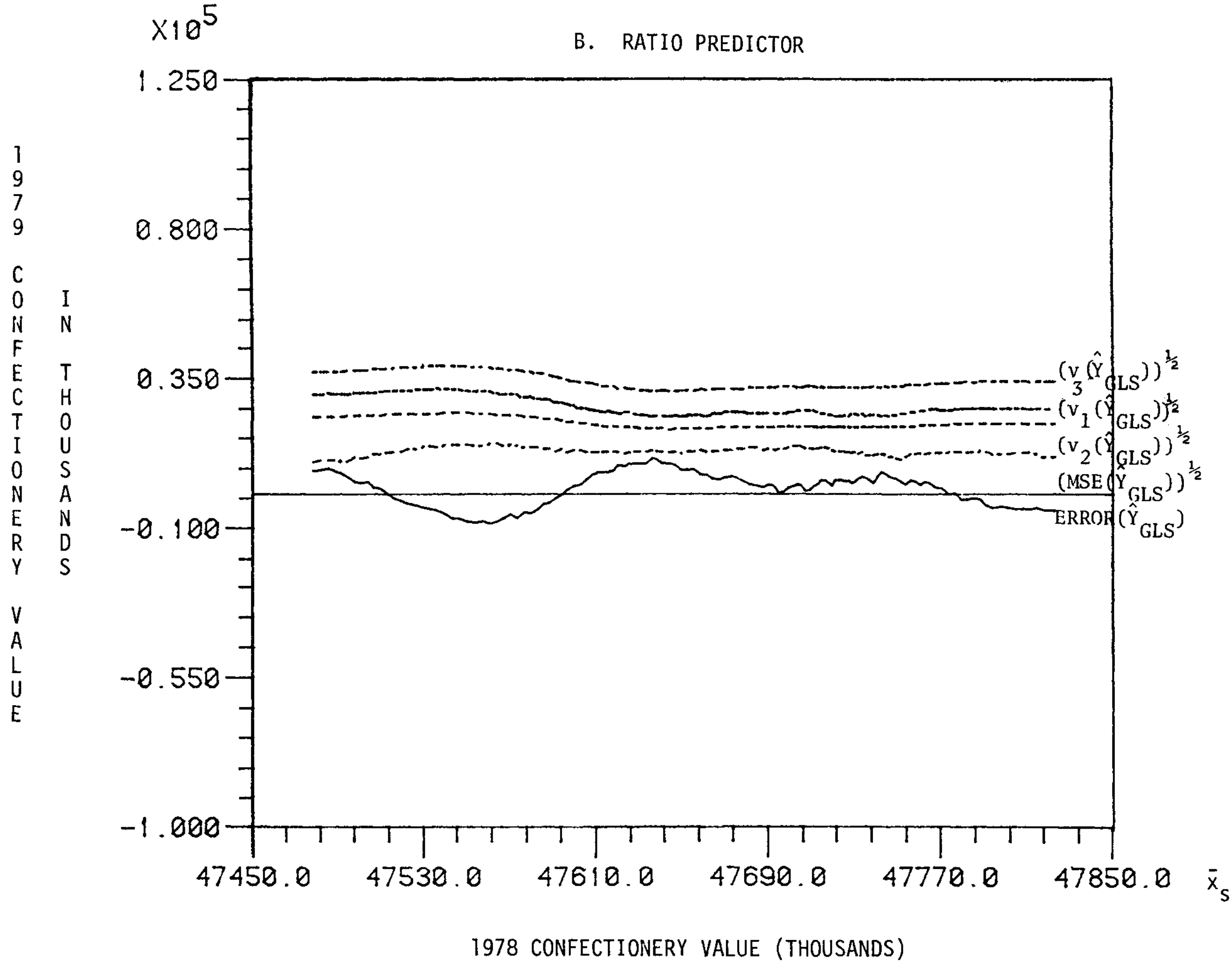
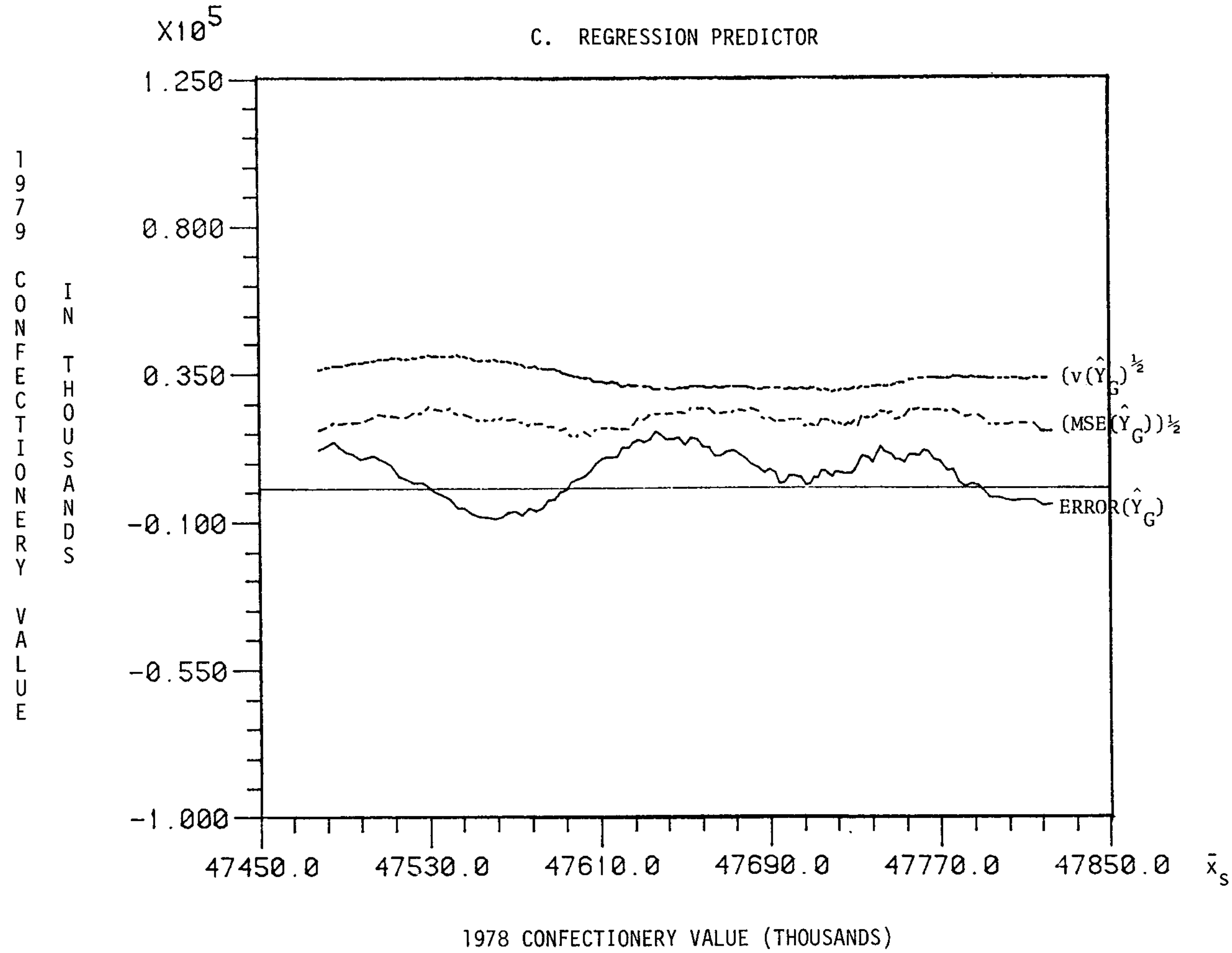
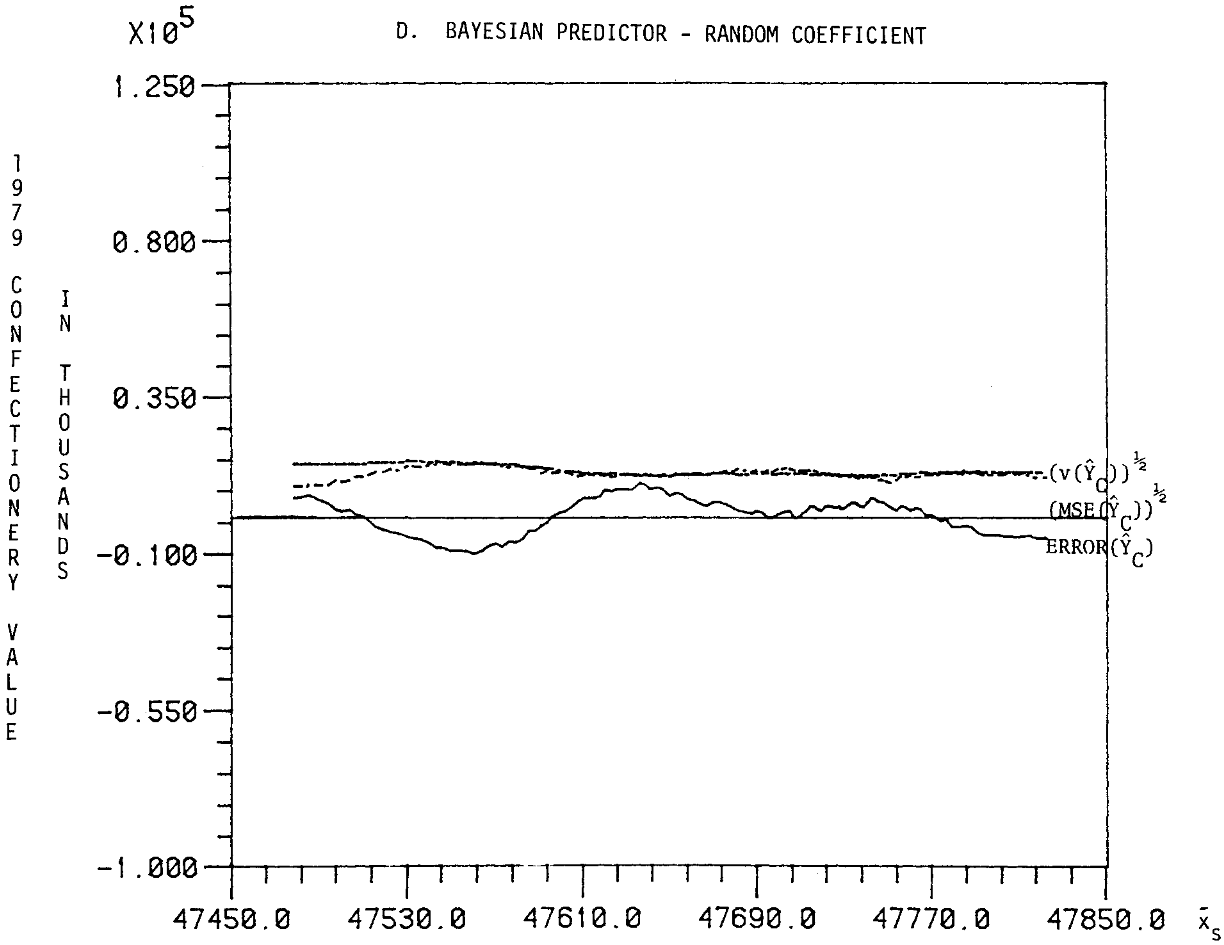



X10^5
B. RATIO PREDICTOR
1.250
0.800
0.350
-0.100
-0.550
-1.000
1979 CONFECTIONERY VALUE IN THOUSANDS
(v_3(Ŷ_GLS))^½
(v_1(Ŷ_GLS))^½
(v_2(Ŷ_GLS))^½
(MSE(Ŷ_GLS))^½
ERROR(Ŷ_GLS)
47450.0
47530.0
47610.0
47690.0
47770.0
47850.0
x̄_s
1978 CONFECTIONERY VALUE (THOUSANDS)



X10^5
C. REGRESSION PREDICTOR
1.250
0.800
0.350
-0.100
-0.550
-1.000
47450.0
47530.0
47610.0
47690.0
47770.0
47850.0
$\bar{x}_s$
1979 CONFECTIONERY VALUE IN THOUSANDS
$(v(\hat{Y}_G))^{1/2}$
$(MSE(\hat{Y}_G))^{1/2}$
$ERROR(\hat{Y}_G)$
1978 CONFECTIONERY VALUE (THOUSANDS)

D. BAYESIAN PREDICTOR - RANDOM COEFFICIENT



1978 CONFECTIONERY VALUE (THOUSANDS)

E. MONTE CARLO DISTRIBUTION OF STUDENTIZED STATISTICS AND N(0,1)

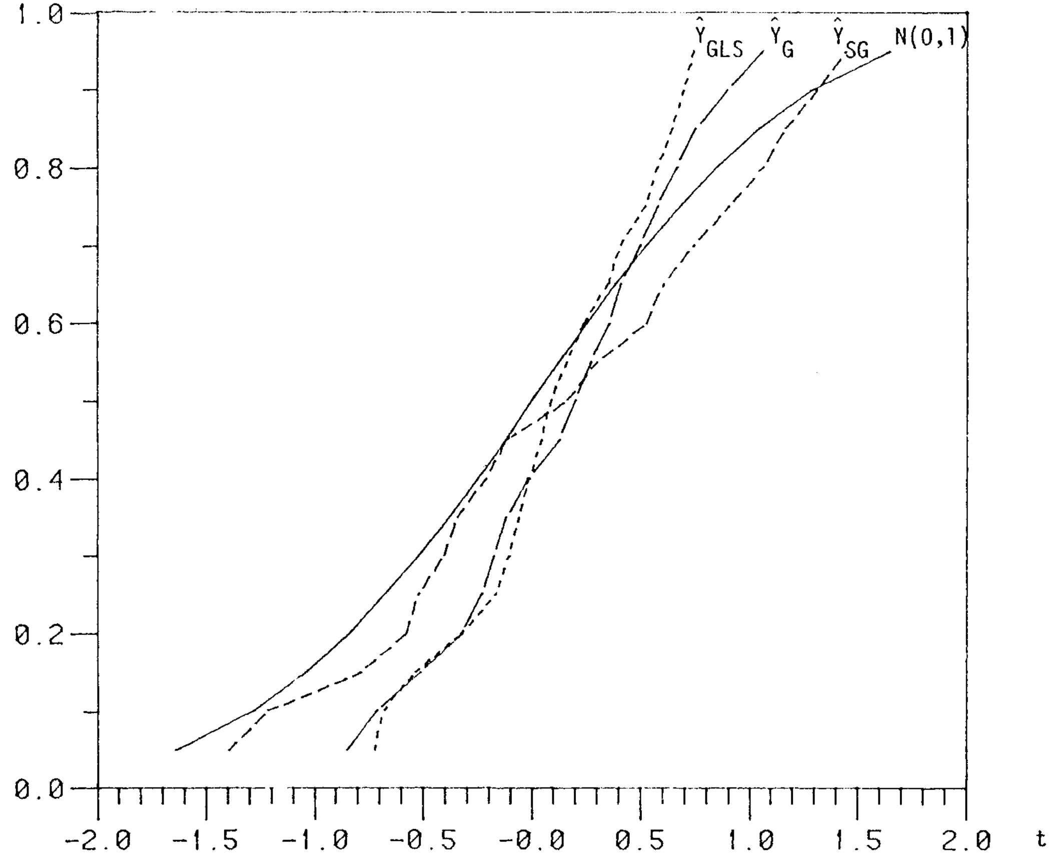


Table 4. Errors and Standard Errors in Cutoff and Best-Fit Samples of n = 50

Variables	$(MSE)^{1/2}$	Sample	$\bar{x}_s$	$(\hat{Y}_{GLS}-Y)$	$v_1(\hat{Y}_{GLS})^{1/2}$	$v_2(\hat{Y}_{GLS})^{1/2}$	$v_3(\hat{Y}_{GLS})^{1/2}$	$(\hat{Y}_G-Y)$	$v(\hat{Y}_G)^{1/2}$
Z2C	44.9+3	Cutoff	53.7+3	-4.1+3	22.2+3	17.9+3	35.7+3	-67.7+3	110.0+3
		Best-Fit	13.8+3	-0.6+3	115.7+3	92.6+3	84.1+3	- 4.7+3	116.2+3

NOTE: $(MSE)^{1/2}$ corresponds to the Monte Carlo results for the separate regression estimator.