THE ADVANTAGES AND DISADVANTAGES OF ROTATING THE ANNUAL HOUSING SURVEY, NATIONAL SAMPLE FROM A NONRESPONSE POINT OF VIEW

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I. INTRODUCTION

Rotation of the sample in a repetitive survey is the systematic replacement, either complete or partial, of the survey sample units at different interview periods. Many continuing surveys conducted by the Bureau of the Census such as the Current Population Survey (CPS) include some kind of rotation scheme, as opposed to keeping units in sample definitely, in order to guard against changes in response patterns caused by repeated interviewing, to spread the respondent burden over a larger portion of the population, and to preclude increasing refusal rates caused by an undue reporting burden placed on the survey respondents.

One of the principal objectives of the Annual Housing Survey (AHS) is to detect year-to-year changes for characteristics and for different components of the housing inventory. Since it was felt that some of these changes would be quite small, a high overlap of the sample from year to year is desirable. It was therefore decided, except for relatively small changes in the sampling frame such as conversions from nonresidential to residential or vice versa, that the designated units would stay the same until a major survey redesign anticipated to occur about 10 years after the start of the survey. 1/

However, the question has been raised as to whether this "fixed panel" design (as opposed to a "rotating panel" design) is the better approach. Rising refusal rates 2/ in AHS have occurred and in addition, there is concern about possible changes in the response patterns due to repeated interviewing. Much work has recently been done in the area of AHS nonresponse which is documented by this paper. The other considerations for rotating the sample will not be discussed, but instead are left to future research.

II. BACKGROUND

A. Sample Design

The sample design for the Annual Housing Survey utilizes the basic CPS design. That is, the sample is a multi-stage cluster sample spread over 461 Primary Sampling Units (PSU's) comprising 923 counties and independent cities with coverage in each of the 50 States and the District of Columbia. Virtually, the same PSU's and enumeration districts are used for the Annual Housing Survey except that different housing units are reserved for Annual Housing Survey enumeration.

However, there is one major methodological difference between the two surveys. AHS is a "fixed panel" design while CPS is a "rotating panel" design.

To understand this, an explanation of the CPS rotating panels is in order. First, by design, the CPS sample is divided into eight subgroups referred to as panels. Each of these subgroups is scheduled to be interviewed for four successive months, not interviewed for the next eight successive months, and then interviewed for the following four successive months. After the eighth enumeration period the subgroup is retired, thus relieving these respondents of the burden of future enumeration in the CPS.

Second, the time in sample of the panels is staggered. That is for any month of CPS, one panel is in sample for the first time, one panel is in sample for the second time, etc. Thus during any calendar month the proportion of the CPS households which are being interviewed for the i^{th} time, i=1,2,..., 8 is approximately the same. In contrast, the cumulative respondent burden increases each year in AHS because most sample units remain the same.

Third, AHS maximizes the correlation between different years in interview by maximizing the overlap in sample. On the other hand, CPS has a 75 percent overlap of panels between succeeding months, a 50 percent overlap of panels between succeeding years, and no overlap of panels after two years. Thus, in CPS, the correlation between samples close in time is relatively high for some characteristics while the correlation is low between samples distant in time.

For the reasons given in section I above, this type of design was not determined to be optimal for AHS. Therefore, even though AHS was structured with 6 panels, these panels were fixed by design and were intended to remain so until the redesign subsequent to the 1980 census. This fixed panel design is hypothesized to be the cause of biases in the data.

B. Estimation

The Annual Housing Survey inventory estimates are derived basically by means of a three-stage ratio estimation procedure. However, prior to the ratio estimation, each sample unit is assigned a basic weight 3/ and a duplication control factor. 4/ Next, the interviewed sample units are assigned a noninterview adjustment factor to account for type A noninterviews, i.e., housing units eligible for interview but for which interviews were not obtained because the occupants refused to be interviewed, were temporarily absent, or were otherwise not available for interview. This noninterview adjustment factor has the effect of inflating the estimates from respondents to account for all sample units. The three stages of ratio estimation are only of minimal concern in this paper. The first stage adjusts the contribution to the variance arising from the sampling of non-self-representing PSU's. The second stage adjusts the estimates of new construction units for selected categories to independently derived estimates. 5/ And the third stage adjusts the total estimate to independently derived counts for four types of vacant housing units and 24 types of occupied housing units. 6/

III. A CLOSER LOOK AT REFUSALS AND NONRESPONSE

A. Increasing Rates of Nonresponse

It is speculated that repeated interviewing at the same units may be the most significant factor in the increasing nonresponse. In AHS, refusal and total type A 7/ rates have increased by 150 and 110 percent respectively between 1973 and 1976. On the other hand, other type A rates have stayed between 0.9 and 1.2 percent inclusive. Table A below shows these rates as well as rates from CPS. It should be noted that the refusal

rate for AHS was approximately equal to the refusal rate for CPS in 1973 and approximately 90 percent higher in 1976. Also, the type A rate for AHS was approximately 35 percent lower than the type A rate for CPS in 1973 and approximately 40 percent higher in 1976.

Table A. Refusal and Type A Rates* for the AHS-National and the CPS

	AHS			CPS		
Year	Refusal Rate	Other Type A Rate	Total Type A Rate	Refusal Rate	Other Type A Rate	Total Type A Rate
1973	1.9	0.9	2.8	1.9	2.4	4.3
1974	2.5	0.9	3.4	2.0	2.1	4.1
1975	3.8	1.2	5.0	2.2	2.0	4.2
1976	4.8	1.1	5.9	2.6	1.8	4.4
1977	NA	NA	NA	2.5	1.6	4.1

* Refusal Rate (Type A Rate) = $\frac{\text{Refusals (Type A's)}}{\text{Interviews + Type A's}}$

Thus, due to the increasing refusal rates in AHS-National, the following three research projects were undertaken within the Census Bureau:

1. High Refusal Rate Study. Determining the types of housing units which have high refusal rates.
2. Noninterview Bias Study. Obtaining estimates of the bias caused by the noninterview adjustment.
3. New/Old Refusal Study. Determining the number of new and old refusals; i.e., potential respondents who not only refused in the latest AHS but who refused in the previous year or years.

These projects as well as the implications of their results are discussed in detail in the following sections:

B. High Refusal Rate Study

1. Classifications of housing — It is speculated that the refusal rate (and therefore the type A rate) may be approaching a level that could seriously bias the survey. Related to this question is the concern that the refusal rate is high among all classifications of housing units, or is it high only for certain classifications of housing units. If the latter is true, it may only be necessary to rotate part of the AHS-National sample, the part where the percent of type A noninterviews is very high. This would effectively lower the type A rate at reduced cost.

Table B: Refusal and Type A Rates for Selected Characteristics from the Basic Sample

Characteristic \ Year	Refusal Rate 1973	Refusal Rate 1976	Type A Rate 1973	Type A Rate 1976
Total	1.9	5.4	2.8	6.5
Urban-Rural Status				
Urban	2.2	6.1	3.3	7.2
Rural	1.0	3.7	1.6	4.8
Metro Status				
SMSA	2.3	6.2	3.4	7.4
Central City	2.5	6.1	3.8	7.5
Outside Central City	2.2	6.3	3.1	7.2
Outside SMSA	1.0	3.6	1.6	4.6
Region				
Northeast	2.6	7.3	3.8	8.6
North Central	1.8	4.8	2.5	5.5
South	1.5	4.5	2.5	5.7
West	1.7	5.4	2.4	6.6
Segment Type				
Address	2.3	6.5	3.4	7.5
Area	1.0	3.6	1.5	4.7
Permit	1.4	4.6	2.5	5.6
Type of Quarters				
House, Apartment, Flat	1.9	5.6	2.8	6.6
Mobile Home	0.6	2.6	1.5	3.9

Even though no interview takes places in type A households, some information is available. Table B above shows some of the possible classifications for units from the basic sample and the respective noninterview rates in each classification category for the first four years of the AHS-National survey.

2. Differences — Although for each year table B shows many differences among the subgroups, these differences are subject to sampling error. Statistically, two subpopulation refusal rates may be considered different if the design effect, F, needed for the estimated difference to be equal to twice the estimated standard error is relatively large.

That is two rates may be considered different if the design effect, needed so that the following is true, is relatively large to what would normally be obtained from the AHS sample. 8/

$$\Delta = 2\sigma_\Delta \Rightarrow$$

$$\Delta^2 = 4\sigma_\Delta^2 \Rightarrow$$

$$\frac{\Delta^2}{4} = \frac{P_1Q_1}{n_1} F + \frac{P_2Q_2}{n_2} F \Rightarrow$$

$$F = \frac{\Delta^2}{4\left(\frac{P_1Q_1}{n} + \frac{P_2Q_2}{n}\right)}$$

where P_i is the estimated rate (percentage) for the i^{th} characteristic

$Q_i = 100 - P_i$

n_i is the sample size (unweighted count) for the i^{th} characteristic

i is an integer such that i = 1,2,

$\Delta = P_1 - P_2$, and

σ_Δ is the standard error of the difference (Δ)

For example, the 1976 type A rates for urban units and rural units in the basic sample are 7.2 percent and 4.8 percent, respectively. The design effect which is needed, to have twice the estimated standard error equal to the estimated difference, is 33.92.

$$33.92 = \frac{(2.4)^2}{(4)\left(\frac{(7.2)(92.8)}{40,900} + \frac{(4.8)(95.2)}{17,500}\right)}$$

This is a relatively large design effect. A rule of thumb for the unweighted data presented in this study is; if the computed design effect is greater than 10, a statistical difference exists; if the design effect is between 4 and 10, no conclusion can be drawn; and if the design effect is less than 4, no statistical difference exists. 9/

The table below shows the estimated design effect needed for some logical comparisons using 1976 data from the basic sample to have twice the estimated standard error equal to the estimated difference. Type A's are shown since the nonin-

terview adjustment is based on all type A's, and the basic sample is used because it is an equal probability sample. 10/

Table C: Design Effect Needed So That Twice the Estimated Standard Error is Equal to the Estimated Difference for Some Logical Comparisons Using 1976 Data from the Basic Sample

Characteristic	Type A Rate	Size	Characteristic	Type A Rate	Sample Size	Difference	Needed Design Effect
Urban	7.2	40,900	Rural	4.8	17,500	2.4	33.92
SMSA	7.4	39,400	Outside SMSA	4.6	19,000	2.8	48.41
Central City	7.5	17,700	Outside Central City	7.2	21,700	0.3	0.32
Northeast	8.6	13,200	North Central	5.5	15,400	3.1	25.75
North Central	5.5	15,400	South	5.7	19,000	0.2	0.16
Address Segment	7.5	34,000	Area Segment	4.7	16,200	2.8	40.79
Area Segment	4.7	16,200	Permit Segment	5.6	5,900	0.9	1.73
House, Apt. Flat	6.6	54,700	Mobile Home	3.9	3,200	2.7	14.19

These data suggest that differences exist between the type A rates for some different types of housing units. Therefore, a partial rotating sample may be a reasonable solution to the relatively high type A rate. 11/

3. Problems with a partial rotating sample — There are four major problems with a partial rotating sample. The first concerns the rising refusal rates. Even though certain types of housing units may have substantially lower refusal rates, these rates increased significantly between 1973 and 1976 for almost all categories. If this trend continues, the refusal rates for almost all categories may eventually become intolerable and the entire sample will need to be rotated anyway. Secondly, the problem categories may constitute most of the sample. Third, it has been pointed out that besides increasing the complexity of the survey and making the sample more difficult to control, rotating one type of housing unit may have an adverse effect on the quality of the data when comparisons between subpopulations are made. Assuming that changes in the response patterns do appear and that they are uniformly distributed throughout the sample, rotating one part of the sample may upset this balance. Hence, it may be possible that spurious differences between characteristics in the data may appear, causing wrong conclusions to be made. The fourth problem with a completely rotating sample concerns change. The reliability of some longitudinal characteristics will decrease since they require that the same units be visited every year.

4. Summary — At first glance it may seem that a partial rotation is a reasonable solution to the rising noninterview rates. But examination of the problems indicate that this solution has the potential to distort the data immediately as well as in the future.

C. Noninterview Bias Study

1. Background — Perhaps the most important question to be answered relates to the level of nonresponse for eligible sample units that can be tolerated. As mentioned before, a noninterview adjustment which attempts to eliminate the effect of nonresponse for eligible sample units is applied to the AHS data. For example, in urban areas in the Northeast that are inside SMSA's but not in the central city, the type A rate in 1976 for all construction, except for mobile homes, in area segments was 11.5 percent. 12/ Now suppose this cell could have been divided into housing units with and without plumbing, and the noninterviews were distributed proportionately across these two types of housing units. Then the noninterview adjustment would be effective (assuming that the basic assumptions for the technique are good) with respect to this housing characteristic. But, alternatively, suppose that the noninterviews were not distributed proportionately across housing units with and without plumbing. Then the noninterview adjustment would have the effect of causing the survey to overcount one of the characteristics and undercount the other.

2. Basic Results — The assumption is being made that certain characteristics remain relatively static over time. 13/ The information for these characteristics from a previous year can be used to estimate the bias caused by the noninterview adjustment. That is, proportions based on interviews can be compared to proportions based on interviews and type A's ("real" proportion) from previous years to produce an estimate of the desired bias. The bias will be estimated by the difference between the "real" proportion and the interviewed proportion.

3. Geographic characteristics and the noninterview adjustment — When first looking at the data, substantial differences seem to exist between interviews and noninterviews for geographic characteristics; e.g., from the basic sample 29.7 percent of the 1976 interviews are rural compared to 20.9 percent of the 1976 noninterviews. Fortunately, the noninterview adjustment is done for each region by 30 segment type-metropolitan-status-urban-rural status cells. For totals based on these categories, this has the effect of minimizing any noninterview bias by geography. Thus, geographic characteristics will not be considered in the following analysis.

4. Flaws in the Noninterview Bias Study — There are four flaws in the Noninterview Bias Study. First, not all the current type A's were interviews in the past. Second, certain characteristics that are assumed to remain static may actually change. Third, reporting, coding, and editing errors may also cause slight differences to appear. And finally, the data is limited to a small group of characteristics because data for other characteristics was too difficult to obtain.

The first of the four flaws may be substantial. For example, in the basic sample, of the 3,775 type A's in 1976, 1,006 were not an interview in a previous year. Hence the "complete picture" of noninterview bias was not obtainable, but it was felt that the data would still give some indication of nonresponse and its effects.

5. Estimation of the bias and its variance — To estimate the bias and the variance of the estimated bias caused by the noninterview adjustment, the following definitions and theorems are needed:

Let I_1 = total weighted interviews prior to the noninterview adjustment

I_2 = total weighted interviews in category prior to the noninterview adjustment

T_1 = total weighted type A's prior to the noninterview adjustment which were matched to an interview in a previous year [14]

T_2 = total weighted type A's in category prior to the noninterview adjustment which were matched to an interview in a previous year [15]

N = average noninterview adjustment [16]

$P_i = I_2/I_1$ = estimated proportion in category

$P_a = T_2/T_1$ = proportion type A in category

$Y_1 = I_1 N$ = final weighted count of interviews (total inventory)

$Y_2 = I_2 N$ = final weighted count of interviews in category

f = adjustment factor to account for type A's that were not interviewed in a previous year [17]

$X_1 = T_1 N$ = final weighted count of type A's which were matched to an interview in a previous year [18]

$X_2 = T_2 N$ = final weighted count of type A's in category which were matched to an interview in a previous year [18]

P_r = estimate of the "real" proportion based on weighted interviews and noninterviews,

$$\approx P_i\left(\frac{I_1}{I_1+fT_1}\right) + P_a\left(\frac{fT_1}{I_1+fT_1}\right), \text{ and}$$

b = estimated bias

$= P_r - P_i$

Unfortunately, weighted counts for I_1, I_2, T_1, and T_2 were not obtainable. Consequently, P_i, P_a, and P_r were approximated as follows:

$$P_i \approx \frac{Y_2}{Y_1}, \quad P_a \approx \frac{X_2}{X_1}, \text{ and}$$

$$P_r \approx P_i\left(\frac{Y_1}{Y_1 + fX_1}\right) + P_a\left(\frac{fX_1}{Y_1 + fX_1}\right)$$

Theorem 1: $b = (P_i - P_a)W$ where $W = \left(\frac{fX_1}{Y_1 + fX_1}\right)$

The estimated bias can be approximated by using the final weighted counts of interviews and noninterviews.

$$b = (P_i - P_a)W$$

Theorem 2:

$$\mathrm{Var}(b) \approx \frac{f^2}{Y_1^2}\mathrm{Var}(Y_2) + \frac{f^2}{Y_1^4}\left(\mathrm{Var}(X_1)\mathrm{Var}(X_2) + E^2(X_1)\mathrm{Var}(Y_2) + E^2(Y_2)\mathrm{Var}(X_1)\right) - \frac{2f^2}{Y_1^3}\left(\frac{E(X_2)E(Y_2)\mathrm{Var}(X_1)}{E(X_1)}\right)$$

Theorem 3: $\mathrm{Var}(b) \hat{=}$ (is estimated by)

$$\left(\hat{\sigma}_{X_2}^2 + \frac{Y_2^2\hat{\sigma}_{X_1}^2}{Y_1^2} - \frac{2X_2Y_2\hat{\sigma}_{X_1}^2}{X_1Y_1} + \frac{X_1^2\hat{\sigma}_{Y_2}^2}{Y_1^2}\right)\frac{f^2}{Y_1^2}$$

where $\hat{\sigma}^2$ denotes the estimate of the variance and X and Y are the estimates for the E(X) and E(Y).

6. Results — The following table shows for selected housing characteristics from the basic sample the estimated noninterview bias and the 95-percent confidence interval for the estimated noninterview bias. If the interval contains zero, it cannot be concluded that a noninterview bias exists. However, if the interval does not contain zero, then it can be concluded that the estimated bias is statistically significant.

95 Percent Confidence Interval on the Estimated Bias for Selected Housing Characteristics from the Basic Sample

Characteristic	P_i	b	Interval (In Percent)
Units in Structure			
1	67.0	-0.18	-0.32 to -0.04
2 or more	27.4	-0.02	-0.16 to 0.12
Mobile Home or Trailer	5.6	0.20	0.14 to 0.26
Number of Rooms			
4 or less	35.8	0.13	-0.01 to 0.27
5	24.2	0.01	-0.09 to 0.11
6 or more	40.0	-0.14	0.28 to 0.00
Heating Equipment			
Warm Air Furnace	51.4	-	-0.14 to 0.14
Steam or Hot Water	18.1	-0.50	-0.60 to -0.40
Other or None	30.5	0.51	0.37 to 0.65
Heating Fuel			
Utility Gas	55.3	-0.14	-0.30 to 0.02
Fuel Oil, Kerosene	22.4	-0.31	-0.45 to -0.17
Electricity, Coke	14.3	0.19	0.09 to 0.29
Other or None	8.0	0.26	0.20 to 0.32
Units in Structure-SMSA			
1	61.6	-0.31	-0.51 to -0.11
2 or more	34.9	0.17	-0.03 to 0.37
Mobile Home or Trailer	3.5	0.14	0.08 to 0.20
Number of Rooms-Rural			
4 or less	30.4	0.18	-0.06 to 0.42
5	26.3	0.05	-0.19 to 0.29
6 or more	43.3	-0.22	-0.52 to 0.08

7. Statistical bias versus reality.—The previous section shows that many of the characteristics for which data were available are represented by estimates which are biased by the noninterview adjustment. However, how important is this bias? From a statistical point of view it is there, but from a practical point of view it may have a trivial effect for many items. For instance, 67.0 percent of the total inventory is the usual estimate for single unit structures, the "improved" estimate should be 67.2 percent. To the user of the AHS-National data this typical difference probably does not matter.

8. Summary.—As stated before, in each year of the Annual Housing Survey there has been a higher refusal rate, so much so that the type A rate was also increased. If this trend continues, it has been speculated that the published data would become more biased. In fact, it has been questioned whether the data with the current levels of noninterview are already seriously biased. To address this question, the noninterview bias study was conducted and to our surprise (and relief) the results suggest that the bias caused by the noninterview adjustment seems to be so small that its effect must be negligible.

Therefore, when considering nonresponse, it seems that the fixed panel design was correct, and unless future trends are dramatically different there would be no advantage to rotating the Annual Housing Survey given the current noninterview levels.

D. Old/New Refusal Study

1. Background.—If a household refuses, an interview is attempted the following year, but if the household refuses for two consecutive years, future interviews are not attempted unless the household changes.

However, there is another side to refusals. Due to the mobility of the population in general and changes in the sampling frame (e.g., losses), some refusals may actually be eliminated from the survey. Thus, it has been speculated that an upper bound may exist for the refusal rate. In fact, this would be possible if the following were true:

a. The number of old (consecutive) refusals would decrease from year to year, e.g., the number of units that had refused in Years I, II, and III would be less than the number that had refused in both Years I and II, and eventually go to zero.

b. The number of new refusals each year would remain constant; or

a. The number of old refusals would decrease from year to year eventually reaching a lower bound.

b. After the first few years the number of new refusals would be less than the preceding year.

2. Analysis.—Table E below (unweighted data from the basic sample) shows that the number of old refusals is decreasing, thus implying that a lower bound may exist.

Table E. Old and New Refusals from the Basic Sample

Year	Old and New Refusals	Refusal Rate
1973	1,010	1.9
1974	1,579	2.9
First Year	1,086	2.0
Second Consecutive Year	493	0.9
1975	2,429	4.3
First Year	1,449	2.6
Second Consecutive Year	585	1.0
Third Consecutive Year	395	0.7
1976	3,143	5.4
First Year	1,437	2.5
Second Consecutive Year	908	1.6
Third Consecutive Year	461	0.8
Fourth Conseutive Year	337	0.6

However, the number of new refusals does not seem to exhibit any trend and thus a cap for the refusal rate may not be in sight. But it has been suggested that after four years of interviewing most potential refusals would have quit participating in the survey. This supposition along with the decreasing number of old refusals may indicate that the second model is not too unrealistic. Therefore, it may be possible for the refusal rate to reach an upper bound after all. 19/

E. Summary

1. Conclusions.—The three studies have shown the following:

a. Even though differences exist between type A rates for different types of housing units, a partial rotation of the sample is not a practical alternative.

b. The bias caused by the noninterview adjustment may only have a trivial effect on the data.

c. It may be possible for the refusal rate (and therefore the type A rate) to reach an upper bound.

2. Final word of caution.—The results presented in this section show the survey in a very favorable light. However, the potential for problems is there and because of this the Field staff of the Bureau of the Census will continue to make every effort to keep nonresponse at a minimum.

Also it would probably not be wise to extend these results to other surveys. Every survey is unique in terms of either design and/or data collected. In addition, even though AHS-National now has a higher rate of nonresponse than the other current surveys conducted by the Bureau of the Census, it has a relatively low rate when compared to nonrecurring surveys and other surveys not conducted by the Census Bureau. Consequently, the conclusions made in this paper should apply only to the AHS-National.

IV. FINAL SUMMARY

As a recap, the problem of nonresponse seems to be somewhat resolved. That is the three studies showed the following:

1. A partial or full rotation of the sample may not be advantageous,

2. Statistical differences resulting from noninterviews may not be important, 20/ and

3. It may be possible for the refusal rate to reach a reasonable upper bound.

Thus, it seems that the survey was correctly designed from the nonresponse point of view.

However, additional research needs to be done to examine the other reasons for rotating the survey, i.e., whether the respondent burden needs to be spread among a larger portion of the population and to what degree (if any) the response patterns have changed between interviews.

FOOTNOTES

1/ The major exceptions to this were the 1974 rural supplement, the 1976 coverage improvements, and the yearly adjustments caused by permanent losses and new construction.

2/ In 1976, over 80 percent of all nonresponses were refusals.

3/ The basic weight is equal to the inverse of the probability of selection. This was the same for all units in Year I (1366.10310). However, for future years the basic weight for rural units was halved due to the addition of the rural supplement.

4/ The duplication control factor (d.c.f.) is the inverse of special subsampling rates utilized in the field. For most units the d.c.f. is equal to 1.0000 since these units do not take part in any special subsampling.

5/ Due to the success of the Year IV coverage improvements, the sample estimate (i.e., the estimate after the first stage) was used as the control for most categories of new construction.

6/ For greater detail see the "appendix B" in the 1976 AHS-National reports, series H-150-76.

FOOTNOTES (Continued)

7/ A type A is an eligible sample unit for which an interview is not obtained because the occupants refuse to be interviewed, are temporarily absent, or are otherwise not available for interview.

8/ The assumption is being made that the two subpopulations from which the rates are derived are sampled independently. For regions this is correct. For the geographic characteristics and segment type this is approximately correct. For housing unit type this assumption is not correct and the above method should be used with extreme caution. For further information, see Technical Paper 40 entitled The Current Population Survey: Design and Methodology.

9/ This rule is based on experience with pre-ratio estimated weighted AHS data.

This method is used since standard errors and/or design effects are not known for unweighted data. Also two other approaches are available. The first method would be to let the design effect equal the following: $F = 1+\delta(\bar{n}-1)$ where δ is the interclass correlation, and $\bar{n}$ is the average cluster size (2 for urban, and 4 for rural)

However, other components of the variance such as subsampling of PSU's, special subsampling in the field, occasional irregular cluster sizes, etc., are not represented. Therefore, this method would produce an underestimate of the design effect.

The second method would be to guess the design effect and then compute the number of standard errors the estimates are apart. However, experience with pre-ratio estimated weighted data shows that the design effect can vary among characteristics. Although the refusal and type A rates are based on unweighted data, their design effects should be similar to the pre-ratio estimated weighted data. Therefore, to prevent the reader from assuming that more is known than really is, this method was ruled out.

10/ The supplemental (rural) sample is also an equal probability sample. However, when the two samples are combined (all housing units), the total sample is not an equal probability sample and the above formula for the design effect will be less accurate.

11/ That is the overall type A rate from AHS is relatively high when compared to the Current Population Survey.

12/ This noninterview adjustment category had the largest type A rate.

13/ See Census report HC(4)-1, Components of Inventory Chance, United States and Regions.

14/ The weight used is the weight from the year from which the "interview" information is obtained.

15/ The weight used is the weight from the year from which the "interview" information is obtained.

16/ It is being assumed that AHS is self-weighting and that the average noninterview adjustment is the same for interviews and type A's which were interviews in a previous year.

17/ Since 27 percent of the type A's could not be matched to an interview in a previous year, it is necessary to increase the weighted count of type A's which were matched. Also it is assumed that this factor is a constant.

18/ At first glance it seems incorrect to have as part of the final weighted counts of type A's the average noninterview adjustment N. However, since type A's are dropped from the weighting file (i.e., given a weight of zero), it was necessary to use the weight for the year from which the "interview" information is obtained. Since the unit was an interview during that year, its final weight includes a noninterview adjustment. For example, suppose a particular unit is a type A in year IV but an interview in year III. Then the year III characteristics were used as well as the year III final weight which includes the year III noninterview adjustment.

19/ The overall refusal rate for the basic and supplemental samples combined for 1977 was 5.2 percent. This was the smallest increase between years since the survey began. Unfortunately, no other 1977 data is yet available.

20/ Again it should be noted that the noninterview bias study was based on results that may be considered weak since data was not available for a large portion of the type A's and that many assumptions have been made.

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