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EXCLUDING SAMPLE THAT MISSES SOME INTERVIEWS FROM SIPP LONGITUDINAL ESTIMATES

by

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1. INTRODUCTION

The Survey of Income and Program Participation is a major household survey conducted by the Census Bureau, which is intended to be an important source of information on the economic situation of persons and households in the United States. In the current design a new sample panel is selected each year. A person in a SIPP panel is generally interviewed eight times over a period of 2 2/3 years, with each round or wave of interviewing collecting information for each month of a four month reference period. Although the survey has cross-sectional uses, a major interest is in longitudinal estimates. Under current procedures, a sample person who misses any interviews may be excluded from the longitudinal estimates, sometimes, as explained later in the paper, even for estimates for time intervals which do not overlap any missed interviews. Concern has been expressed by some data users, particularly the Food and Nutrition Service (FNS), over the detrimental effects on both variances and biases of the exclusion of these sample cases, particularly the cases that miss some interviews but later return to sample. The purpose of this paper is to investigate the implications of this exclusion and to consider alternative approaches.

The structure of this paper is as follows. In order to fully understand the problem of the sample omitted from the longitudinal estimates, some knowledge is required of the SIPP design, the noninterview problem for this survey and the weighting procedures used. These areas are briefly reviewed in Section 2. In Section 3, using data from the 1984 SIPP panel, estimates are compared from four groups which partition the set of people for whom at least one interview was obtained as part of this panel. These four groups, which are defined more precisely in Section 3, are roughly the portion of the sample interviewed for all waves (group 1), the portion interviewed the first wave, but who eventually leave permanently (group 2), the portion interviewed the first and last waves but who miss some interviews (group 3), and the portion first interviewed subsequent to the

first wave (group 4). The comparisons are for certain demographic and economic characteristics at the time of each sample person's first interview, and also for gross change estimates for these characteristics. The cross-sectional weighting system which, unlike the longitudinal system, assigns positive weights to all sample cases for any month that an interview is obtained, is used in these comparisons. Also in Section 3, first wave estimates for these characteristics obtained from the cross-sectional weighting system, which, as just noted, includes the sample cases that miss later interviews, are compared to the same estimates obtained from the longitudinal weighting system which, in the file used, excludes these cases. This comparison provides some insight on the effects of the longitudinal weighting adjustments in compensating for the exclusion of the sample that miss interviews.

Finally, in Section 4, the following possible modifications of the current longitudinal weighting procedures are discussed: inclusion of group 3 cases into the estimates after imputation for missing waves, which might also be combined with a weighting adjustment of the group 3 cases to compensate for exclusion of group 2 cases; inclusion of group 2 cases; inclusion of group 4 cases; and use of multiple sets of weights to cover different time intervals.

The authors would like to thank Pat Doyle of Mathematica Policy Research for suggesting many of the comparisons in Section 3. We would also like to thank Fred Cavanaugh and Edith McArthur of the Census Bureau for answering many of our questions and, in Fred's case, for programming assistance. Finally, we would like to thank Robert Fay and Lynn Weidman of the Census Bureau for their suggestions on significance testing for the data presented in the paper.

2. BACKGROUND ON SIPP

In this section we briefly review the definitions of SIPP universes, key survey procedures, the noninterview problem and aspects of the weighting procedures relevant to this paper.

There are both cross-sectional and longitudinal universes for SIPP. Cross-sectionally the universe is, for each month, all residents of the United States that month, at least 15 years old, not living in an institution or military barracks. Longitudinally, the universe can be taken to be the cohort of all people in the cross-sectional universe at the time of the first interview for that panel or, alternatively, at the beginning of an interval for which estimates are being made.

A SIPP sample panel is selected from a multistage stratified design of housing units and other living quarters. All individuals at least 15 years old in the selected housing units at the time of the first interview who are actually interviewed become part of the panel, and are referred to in this paper as original sample people. In order to meet the longitudinal needs of the survey, all original sample people are to be interviewed as long as they remain in the universe, unless they move more than 100 miles from the nearest primary sampling unit. Anyone at least 15 years old who is not an original sample person, but who sometime during the life of the panel is living with an original sample person, is also to be interviewed, but only for the months that they are living with an original sample person. These people are referred to in this paper as associated sample persons. This brief discussion actually oversimplifies the SIPP procedures, but is sufficient for purposes of this paper. Further information on these procedures is presented in Nelson, McMillen and Kasprzyk (1985) and King, Petroni and Singh (1987).

The problem of concern to FNS arises because SIPP, like most surveys, particularly longitudinal surveys, has a nontrivial

noninterview rate. In SIPP, if no interview is obtained from a selected housing unit in the first wave, no attempt is made to obtain an interview in subsequent waves. (This is not true for the 1985 SIPP panel, which was not used in our work.) However, if an original sample person misses an interview in a later wave, the operational procedures allow for the possibility of obtaining interviews in subsequent waves.

There are currently two separate SIPP weighting procedures, one for cross-sectional estimates of persons, families and households, and the other for longitudinal person estimates, both of which were used in our work.

The cross-sectional weighting procedure assigns a different set of weights to each SIPP panel for each month of the panel. For each month, positive weights are assigned to each original sample person and each associated sample person from whom data is obtained for that month. The weighting adjustment procedures include noninterview adjustments and also a control to independent estimates that partially serves to compensate for noninterviews.

For longitudinal estimates there currently exists a file covering the period of the first three interviews for the 1984 panel. All persons from whom at least one interview is obtained are on this file. However, in contrast to the cross-sectional file, there is only one set of weights on this file, which assign positive weights only to original sample people who respond to all three interviews or who leave the cross-sectional universe, which is the cause of FNS's concern. This is the only longitudinal file used as a data source in this paper, although other files with only cross-sectional weights were used.

As of this writing, there is about to be released a full 1984 panel file. It would have been preferable to use this file instead of the three-interview file in our work, but time did not

permit, since the full-panel file was just recently released. This file has three sets of longitudinal weights. One set, the panel weights, assigns a positive weight only to original sample people from whom interviews were obtained for each of the reference months of the eight interview periods that they were in the cross-sectional universe. Although this set of weights is usable for all time intervals during the life of the panel, it is particularly intended for use for estimates for time intervals for which it is the only usable set, that is intervals that do not fit within either calendar year 1984 or 1985. The second set and third set assign positive weights to all original and associated sample persons who were interviewed for each month of 1984 and 1985 respectively that they were in the universe. These weights are intended to be used for 1984 and 1985 calendar year estimates or for estimates covering time intervals contained within these calendar years. For such estimates, these calendar year weights have the advantage over the panel weights of assigning positive weights to a larger set of people, which should produce smaller variances.

The weighting procedures used to obtain the final sets of weights for each of these sets of longitudinal weights incorporate at least two noninterview adjustments to compensate for people excluded due to noninterviews. The purpose of the first noninterview adjustment is to compensate for people who were first wave noninterviews. The same noninterview adjustment is also used in the cross-sectional weighting procedure and is described in Jones (1983).

The purpose of the second noninterview adjustment is to compensate for people excluded because of later noninterviews who were interviewed at the time for which the cohorts are defined, that is the time of the first interview for the three-interview file and also for the panel weights for the full-panel file, and January 1984 and January 1985 for the two calendar year weights. This noninterview adjustment will be referred to as the

longitudinal noninterview adjustment since it is not used in the cross-sectional weighting procedure. The noninterview adjustment cells for this adjustment are defined using a subset of the abundant information available for this set of excluded people from their interview for the wave which includes the time that the cohorts are defined. This longitudinal noninterview adjustment is described in Jones (1986).

For the calendar year weights, a third noninterview adjustment is also used which, in the overall sequence of adjustments, fits between the other two adjustments. Its purpose is to compensate for all persons who were not assigned positive calendar year weights due to noninterviews at the beginning of the year, other than those people who were wave 1 noninterviews. The identical adjustment is also used in cross-sectional weighting and is described in Jones (1984).

In addition to these noninterview adjustments, the final stage of the longitudinal weighting procedure controls the estimates for key demographic characteristics to independent estimates at the time for which the cohorts are defined. This adjustment, in addition to generally reducing variances and compensating for undercoverage, also may reduce the detrimental effects of noninterviews. This adjustment is also described in Jones (1986).

The effectiveness of the longitudinal noninterview adjustment and the control to independent estimates in compensating for original sample people excluded from estimates from the three-interview file due to noninterviews for the second and/or third interview is one of the areas to be examined in the next section.

3. EMPIRICAL INVESTIGATION OF EFFECTS OF EXCLUDING NONINTERVIEW CASES

In the first part of this section, using data from the 1984 SIPP

panel, characteristics are compared among the following four groups which partition the set of people for whom at least one interview was obtained as part of this panel. Then the effectiveness of the longitudinal adjustment procedures in compensating for the excluded groups is studied.

Group 1. Original sample people who were interviewed for each of the 32 reference months during the 8 interview periods or for all reference months until leaving the SIPP cross-sectional universe. This is the group that is assigned positive panel weights on the full-panel longitudinal file.

Group 2. Original sample people who missed the eighth interview and were in the SIPP universe at the time of their first missed interview. In addition, original sample people who missed any three consecutive interviews are included in the group.

Group 3. Original sample people who were interviewed for the first and eighth interview, missed at least one interview, but did not miss any three consecutive interviews.

Group 4. All associated sample people.

The method of division of the set of original sample people who missed at least one interview into the groups 2 and 3 evolved from a suggestion by Pat Doyle. The rationale is that since group 3 people never missed more than two consecutive interviews, they may be good candidates for inclusion into longitudinal estimates with imputation for missed interviews. It is possible that other divisions might be more appropriate for this purpose. For example, limiting group 3 to cases that never missed more than one consecutive interview would not decrease the size of this group very much, but may allow for better imputation.

Note that these groups have been defined in a manner more appropriate for the full-panel file than for the three-interview file, even though it is the latter longitudinal file that is used in this work. There are two reasons for this. First, a full-panel file should be much more important to data analysts than a three-interview file. Also, much of the work described in this section involves comparisons among the groups. It is necessary to use files with cross-sectional weights for this purpose, because either some or all of the group 2, 3, or 4 cases receive zero weights for a set of longitudinal weights. In fact the three-interview longitudinal file is only used in producing the last table described in this section.

The comparisons are with respect to the demographic and economic characteristics that are listed in Table 1. The file used in producing this table is one, not previously mentioned, which covers data from all eight interview periods. On this file are cross-sectional weights for each reference month but no longitudinal weights. This file excludes all sample cases that were part of a sample cut that took place during the fifth interview period. The main purpose of the comparisons is to obtain some indication of the effects of excluding group 2 and group 3 cases from the longitudinal estimates. As for group 4, with the cohort definition these sample cases are not needed. Also, they are not used in the estimates from the three-interview file in order to obtain theoretically unbiased estimates in the absence of any nonsampling errors, since unbiased estimates for the cohort population can be obtained from the set of all original people under these assumptions. However, for the 1984 and 1985 calendar year weights on the full-panel file, group 4 cases have been assigned positive calendar year weights if they were interviewed for each month of the specific year. This is necessary for a cohort universe defined at the beginning of a calendar year, since only group 4 cases can account for additions to the cross-sectional universe after the first wave. (Actually, those additions to the universe that are not living with a member

of the wave 1 cross-sectional universe at the beginning of the calendar year cannot be represented by even group 4 cases.) Even where group 4 cases are not needed for this purpose, the inclusion of these cases can reduce variances and, if their characteristics are similar enough to group 2 and group 3 cases, biases result from the exclusion of these two groups, which is why group 4 has been included in Table 1. This will be discussed further in Section 4. A more comprehensive treatment of issues relating to inclusion of group 4 cases is to be found in Judkins et al. (1984).

In Table 1, for each of the four groups, the distributions of the indicated characteristics are presented. For all four groups, the values of the characteristics are for the first reference month that the person was in sample, and the weights are the final cross-sectional weights at that time, with an adjustment to compensate for the cases excluded due to the sample cut. With the exception of group 4, the month in question is the first reference month of wave 1. Group 4 characteristics are not strictly comparable to the other three groups, since by necessity they are from different points in time. A column for groups 1-3 combined is also presented, which represents the overall distributions of the cross-sectional estimates as of the first reference month of wave 1.

To test which pairs of groups have significantly different distributions for these characteristics, chi-square statistics were computed with adjustments for the complex SIPP design. The results of the tests are presented in Table 2 with starred entries indicating a significant difference at the five percent level. Interestingly, significant differences only occurred for demographic variables, not economic variables. (Relatively large design effects were assumed in the computations. With smaller design effects, some significant difference also would have occurred for economic variables.) There are no significant differences at all between groups 2 and 3, and for most of the

characteristics the chi-square values were very close, an indication that these two groups may be similar in some respects.

The second set of comparisons, presented in Table 3, are with respect to gross change tables for the characteristics in Table 1, with the exception of age and sex which are omitted for obvious reasons. To simplify Table 3, the characteristics with more than two values in Table 1, have been collapsed into two categories. For groups 2 (Table 3a) and 4 (Table 3d), the pair of months for which the estimates were computed for each sample person are the first and last reference months for which an interview was obtained. For group 3, two gross change estimates were computed corresponding to two time periods for each sample case. For one set of estimates, it is the change from the first reference month of the first interview to the last reference month of the eighth interview (Table 3b). For the other set of estimates, the pair of months are the last reference month before the first missed interview and the first reference month for the next completed interview (Table 3c). Gross change estimates for groups 2, 3, and 4 cannot be directly compared because of the different distributions of pairs of months in the table. However, estimates from each of the groups can be compared to the estimates in the same row of Table 3 computed from group 1 cases, as follows. Corresponding to the estimates computed from the group 2, 3, or 4 cases in each of the four subtables a-d, is a distribution of pairs of months used in the estimates. For each such distribution, a comparable group 1 estimate is obtained by first computing gross change estimates for each pair of months for all group 1 cases, except those cases that are excluded from this computation because they left the universe before the end of the panel. Then this group 1 estimate for each pair of months is multiplied by the proportion of the distribution corresponding to that pair of months, and the result is summed over all pairs of months. By computing group 1 estimates in this manner, the

relative contribution to the overall group 1 estimates for each pair of months for the group 1 estimates in each subtable is the same as for the group 2, 3, or 4 estimates in the subtable.

For each row in Table 3 the proportion remaining in category for group 2, 3, or 4 was tested for a significant difference at the five percent level against the same proportion for group 1, with the results indicated in the last column. In this table and in Table 4, Wald statistics (Kendall and Stuart 1979) were used instead of adjusted chi-square statistics to test for significant differences, since even approximate design effects are unknown for the estimates being compared. The variance-covariance matrix used in computing this statistic was obtained by means of a replicate estimator. With few exceptions the proportion changed in each row in the table is lower for group 1 than for groups 2, 3 or 4, and in many cases the differences were significant. These results are not inconsistent with the hypothesis that people who do not miss any interviews tend to have more stable characteristics than these who do.

From the results in Tables 1-3 it appears that group 1 is significantly different than groups 2 and 3 for some key demographic characteristics. Indeed, the comparison of the group 1 column to the combined groups 1-3 column in Table 1 indicates how the estimates would change for the listed characteristics from the exclusion of groups 2 and 3 if the group 1 cases were used together with the cross-sectional weights.

However, as noted in Section 2, longitudinal estimates are actually computed with a different set of weights which include adjustments that attempt to compensate for cases excluded due to noninterviews. To provide some insight into the success of these adjustments in attaining this goal, estimates for the characteristics were recomputed using the longitudinal weights from the three-interview longitudinal file. These estimates are presented in the first numerical column in Table 4. For

comparative purposes, two other sets of estimates for the same characteristics are also presented in this table which are estimates obtained from all original sample people using first wave cross-sectional weights; and also estimates obtained using these weights but only including the people receiving positive longitudinal weights on the three-interview longitudinal file. Since this last set of people includes not only all group 1 people, but also those group 2 and group 3 people who did not miss interviews until after the third interview, it would be reasonable to expect that estimates in both columns 1 and 3 would be closer to the estimates in column 2 than if the full 1984 SIPP panel weights were used in column 1, and only group 1 cases in columns 1 and 3.

There were two changes in the manner in which estimates were computed in Table 4 compared to Table 1, which resulted from the desire to have the estimates in Table 4 computed using longitudinal weights as comparable as possible to those using cross-sectional weights. First, the three-interview longitudinal file included cases dropped in the sample cut, since the sample cut took place after the third interview. Consequently, the estimates presented in Table 4 using cross-sectional weights were computed from a file which, unlike the file used to obtain Table 1, also included these cases. The second change was made because the estimates on the three-interview longitudinal file are controlled to independent demographic estimates as of December 1983. This led us to use the characteristic values and cross-sectional weights from the fourth reference month of wave 1 instead of the first in computing the estimates in Table 4. This is because the fourth reference month for wave 1 for the four subpanels or rotation groups that the 1984 panel was divided into are September, October, November and December 1983, which results in cross-sectional weights with average controls as close as possible to December 1983 as can be obtained from wave 1 weights on the file being used.

For the demographic characteristics, the values of the Wald statistic corresponding to columns 1 and 2 of Table 4 are smaller than the corresponding values for columns 2 and 3, an indication of the effectiveness of the longitudinal weighting adjustments in compensating for the excluded cases. Indeed, for some of the characteristics the distributions in columns 1 and 2 are almost identical. For age and sex this can be completely explained by the fact that the controls to independent demographic estimates used in both the longitudinal and cross-sectional weighting procedures essentially force agreement on these two variables. Marital status and relationship to reference person are also used in these controls, but in a much more complex manner, which should result in very close but not necessary identical proportions for marital status, and the householder and spouse of householder categories for relationship to reference person, but not for the other relationship categories separately.

Even for those characteristics for which the distributions in columns 1 and 2 are nearly identical due to the controls to independent estimates, the computed Wald statistic is not negligible, except for sex. This may be explained by the fact that the variance-covariance matrix used in computing this statistic was estimated by a replicate estimator which did not reweight the replicates in order to simplify the programming. If the replicates had been reweighted, as properly should have been done, the computed values for the Wald statistic for these characteristics may have been smaller.

Income, means-tested cash benefits, and food stamps are not used in the controls to independent estimates. However, the first two of these characteristics are used in the longitudinal noninterview adjustment procedure to compensate for cases used in the column 2 estimates but not the column 1 estimates. Income is used in this adjustment with a different distributions breakdown than in Table 4. There is no characteristic titled means-tested cash benefits in this adjustment, but there is a receipt or

nonreceipt of selected welfare benefits characteristic which includes all of the means-tested transfers income categories plus some additional income sources. Receipt of food stamps is not used directly in the noninterview adjustments, but is correlated with some of the variables used in the noninterview adjustment such as income. Surprisingly, the Wald statistic corresponding to columns 1 and 2 is larger than the statistic corresponding to columns 2 and 3 for all three of these characteristics.

In order to test the effectiveness of the adjustments on a broader range of characteristics, three additional variables, educational attainment, residential tenure and ownership of a savings account, are included in Table 4 which are not included in the previous tables. Of these characteristics, educational attainment is the only one directly used in the longitudinal noninterview adjustment, while none are used in the control to independent demographic estimates. The Wald statistic corresponding to columns 1 and 2 is smaller for each of these characteristics than the statistic corresponding to columns 2 and 3.

To summarize the analyses in this section, from the data in Tables 1 and 3 there is some evidence that, at least for certain characteristics, groups and 2 and 3 are significantly different than group 1, while Table 4 provides an indication that longitudinal weighting partially, but not completely, compensates for this problem.

4. POSSIBLE MODIFICATIONS TO THE LONGITUDINAL WEIGHTING SYSTEM

In this section, possible modifications of the longitudinal weighting system are discussed which alter the set of cases assigned positive weights and the method of adjusting these weights, with the goal of reducing detrimental effects on mean square error resulting from the current exclusion of group 2 and 3 cases.

First is the possibility of assigning group 3 cases positive longitudinal weights, even for time intervals which include missed interviews, with imputation for the missed interviews. If the imputation could be done without error, then this modification should generally result in smaller variances, since a larger proportion of the sample would be included in the estimates. Generally smaller biases should also result for this reason and also because some of the characteristics of groups 2 and 3 appear similar, as noted in the previous section. (As an indication of how this would affect sample size, the 1984 SIPP panel contained 24,536 group 1, 6,641 group 2, 2,179 group 3 and 4,394 group 4 cases.)

However, we believe there is at least one major difficulty to overcome before this modification should be considered, that is the development of a sound procedure for imputing for the missed interviews. If imputation were to be performed by simply substituting data from the previous or subsequent completed interviews, a downward bias in gross change estimates may result, while other imputation procedures may have the opposite effect. Some imputation methods could easily get very complex, and possibly result in some records with impossible data sets. Furthermore, even if a good imputation procedure were developed, there would still be some imputation variance and imputation bias associated with this procedure.

Even if imputation error is ignored, any reductions in biases from including the group 3 cases would generally be less than otherwise might be expected due to the reduction in biases that already arise from the weighting adjustments. The same is true for variances. In fact, for those characteristics controlled to independent demographic estimates, there is, of course, no variability arising from the SIPP sample at all if the same categories are used in the estimates and the controls. Also, the exclusion of group 2 and group 3 cases has no effect on the estimates for those characteristics which are included in the

longitudinal noninterview adjustment, again assuming the same categories, and hence, no effect on variance estimates. For a characteristic not directly included in these adjustments, but highly correlated with some characteristics that are included, there generally is an increase in variance from exclusion of the group 2 and 3 cases, but not as large an increase as there would be without these adjustments. Another reason for smaller reductions in variances from inclusion of group 3 cases is that there are both between primary sampling unit (PSU) and within PSU components of variance arising from the SIPP design and it is only the latter component that would be reduced by including group 3.

The inclusion of group 3 in the longitudinal estimates could not, by itself, compensate for the biases remaining due to the continued exclusion of the much larger group 2. However, if it is assumed that the characteristics of group 3 are identical to group 2 or, more realistically, that the distributions for group 3 are generally closer than the distributions for group 1 to the distributions for group 2, then it would be possible to use an alternative noninterview procedure which only would adjust the weights of the group 3 cases to compensate for exclusion of group 2 cases. Although such an adjustment might yield smaller biases, this would generally be offset by potentially large increases in variances. For example, consider the case of simple random sampling with replacement with a single cell noninterview adjustment cell. Assume that each unit in the population if selected in the initial sample would always be a member of the same group and that the population variance for a particular characteristic for both groups 1 and 3 is σ^2. Let n_1, n_2, n_3, denote the number of sample cases in groups 1, 2 and 3 respectively, which are random variables. Then if a uniform factor is applied to all group 1 and 3 cases to compensate for the exclusion of group 2 cases, the variance of the sample mean for the characteristic would not be affected by this adjustment and would be, conditioned on n_1, n_3,

$$\frac{\sigma^2}{n_1 + n_3} \quad . \tag{4.1}$$

Alternatively, if the adjustment factor is applied only to group 3 cases, then the factor would be $(n_3 + n_2)/n_3$ and the resulting variance conditioned on n_1, n_2, n_3 would be

$$\frac{n_1 + (n_2 + n_3)^2/n_3}{(n_1 + n_2 + n_3)^2}\,\sigma^2 \quad . \tag{4.2}$$

The ratio of (4.1) to (4.2) which is the relative increase in variance, conditioned on n_1, n_2, n_3, from applying a noninterview adjustment factor to group 3 cases only is

$$\frac{(n_1 + n_3)\,[n_1 + (n_2 + n_3)^2/n_3]}{(n_1 + n_2 + n_3)^2} \quad . \tag{4.3}$$

The values of the n_i's for the 1984 SIPP panel, that is the number of unweighted cases used in the computations in Table 1 are n_1 = 24,536, n_2 = 6,641, n_3 = 2,179. With these n_i's, the value of (4.3) is 1.45. Although (4.3) is conditioned on the n_i's for a specific sample, (4.3) should be a good estimate of the ratio of the expected value of (4.1) over all samples to the expected value of (4.2) since all the n_i's are relatively large for a SIPP panel. The large increase in variance resulting from applying a noninterview adjustment factor to only group 3 cases in this example may indicate that there would be little interest in using such an approach to compensate for exclusion of group 2 cases.

Group 2 cases could also possibly be used directly in the longitudinal estimates by imputing for missed interviews. However, we believe the development of a good imputation procedure for missed interviews would be even more difficult for

group 2 cases than for group 3 cases, particularly for usage in gross change estimates. This is principally because for most group 2 cases there are missed interviews without any subsequent interview to provide information that could be used in imputation.

Increasing the number of sets of longitudinal weights, corresponding to a larger set of time intervals, is an alternative method of modifying the longitudinal weighting system that would allow for greater usage of group 2 and 3 cases without requiring imputation for missed interviews. To understand this, observe that without imputation, any sample person not interviewed for at least part of a time interval over which estimates are being made would be excluded from these estimates. Furthermore, a sample person may also be excluded from estimates over a time internal for which no interviews are missed. For example, for the 1984 full-panel file, a sample person whose first reference month is December 1984 would be excluded from estimates over the first half of 1984, since such a case would be zero-weighted, whether the full panel weights or the 1984 calendar year weights were used. However, the same sample person would be included in these estimates if there was a set of weights on the file which covered a smaller time interval that included the first half of 1984, but not December 1984.

Two types of time intervals can be used in constructing additional sets of longitudinal weights. Consider first intervals beginning with the first wave, but with varying possible ending dates, and assume there will be no imputation for missing interviews. The advantage of constructing sets of weights for several intervals of this type is that any group 2 or group 3 case could then be given a positive weight for any such time interval that ends before the first missed interview. At one extreme if there exists only one set of weights for the full length of the panel, no group 2 or 3 cases could be used at all. At the other extreme if there exists a set of weights

corresponding to each month during the life of the panel, every group 2 and group 3 case could be included for estimates covering some time interval.

The idea of having sets of weights corresponding to more than one time interval beginning in wave 1 is not at all a new one. It was first discussed by Census Bureau staff when the three-interval file was created, although it was eventually decided to use only one set of weights in this initial research file for reasons of simplicity, which included concern over multiple sets of weights creating confusion among data users.

Weights can also be constructed to correspond to time intervals that begin after the first wave. In fact, as previously noted, weights for 1984 and 1985 calendar year estimates will appear on the full 1984 SIPP panel file. For such time intervals, not only can all group 2 and 3 cases for which there are interviews throughout the interval be used in the estimates, but also group 4 cases which satisfy this condition can be included. The inclusion of group 4 cases requires a modification of the method needed to obtain unbiased weights, which is described in Judkins et al. (1984). Furthermore, depending on how the longitudinal universe is defined, some group 4 cases should be excluded even if interviews were obtained throughout the interval for which the weights are being constructed. For example, with a cohort universe defined as of wave 1, all group 4 cases that joined the universe after wave 1 should be excluded while, for a cohort universe defined at the beginning of the time interval for which estimates are being made, there is no need to exclude any group 4 cases for this reason.

Another advantage of the inclusion of group 4 cases is that these cases should partially offset the detrimental effects on variances from the exclusion of group 2 and 3 cases. In addition, if the characteristics of the group 4 people are closer to those of groups 2 and 3 than to those of group 1, then the

inclusion of these cases may also partially offset any biases resulting from the exclusion of groups 2 and 3 cases.

In practice though, the advantages just cited for the inclusion of group 4 cases are greatly diminished since this group also contains people with missed interviews. For example, it might be expected that the inclusion of group 4 cases would be most helpful for an interval covering the latter part of the life of a SIPP panel, since such intervals would exclude most of group 2 cases, and most of group 4 cases would have entered sample by the beginning of such an interval. Indeed for 1985 calendar year estimates all but 207 of the 6,641 group 2 cases and 923 of the 2,179 group 3 cases would be excluded due to missed interviews for at least one month in 1985. However, only 1008 of the 4,394 group 4 would be included. Part of the reason for the low number of group 4 cases that can be included is that for associated sample people, missed interviews can occur not only for all the reasons that they can occur for original sample people, such as refusal or temporary absence, but also for the additional reason that, under current SIPP procedures, associated sample should not be interviewed for any time periods for which they are not living with an original sample person. In fact, of the 3,386 group 4 cases that would be excluded from 1985 calendar year estimates, there are 1026 cases that first entered sample early enough to be included in 1985 estimates, but whose first missed interview was due to not being followed because they were no longer living with an original sample person, although some of these cases might have missed interviews for other reasons even if they were followed. We believe that consideration should be given to following associated sample people throughout the life of the panel if they are going to be used in longitudinal estimates.

In summary, it is our belief that an overall beneficial effect from the inclusion of the relatively small number of group 3 cases with missed interviews within a time interval for which longitudinal estimates are being made, is dependent on success at

the very difficult task of developing a sound longitudinal imputation procedure for the missed interviews. With less effort, more use can be made of some groups 2, 3 and 4 cases for intervals for which these cases missed no interviews, by the creation of additional sets of weights. There should be some optimal number of such sets of weights beyond which any further gains would be slight because few more cases would be included and the intervals that correspond to the additional set of weights might not be of much interest to data users. These slight gains if too many weights were used would be more than offset by the additional expense required in producing the extra sets of weights and the resulting larger and more complex file. Whether this optimal number has already been reached with the three sets of weights on the 1984 full-panel file requires, we believe, further investigation.

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Table 1. Distributions of Characteristics of 1984 SIPP Panel at Initial Reference Month by Group

Characteristic	Group 1	2	3	4	1+2+3
AGE					
15-17	5.8	6.6	6.9	5.5	6.0
18-24	14.5	20.7	21.4	39.2	16.2
25-34	21.8	23.1	23.6	29.0	22.2
35-44	16.5	16.2	16.1	9.6	16.4
45-59	19.0	18.1	17.3	9.7	18.7
> 60	22.4	15.3	14.7	7.1	20.5
SEX					
Male	46.8	50.1	49.8	54.7	47.7
Female	53.2	49.9	50.2	45.3	52.3
MARITAL STATUS					
Married, spouse present	59.7	50.4	50.6	31.3	57.2
Otherwise	40.3	49.6	49.4	68.7	42.8
RELATIONSHIP TO REFERENCE PERSON					
Reference person	48.5	43.6	44.1	18.1	47.2
Spouse of reference person	29.4	24.2	24.7	17.1	28.0
Child/relative of reference person	19.6	26.8	25.9	35.2	21.5
Non-relative of reference person with household relatives	0.3	0.9	0.8	2.7	0.5
Non-relative of reference person without household relatives	2.2	4.5	4.6	26.8	2.8
INCOME AS PERCENTAGE OF FOOD STAMP CUTOFF					
< 100%	11.8	13.8	12.9	10.9	12.3
101 - 130%	5.6	6.1	6.1	6.0	5.7
131 - 185%	10.5	10.1	9.7	11.0	10.2
> 185%	72.1	70.0	71.3	72.1	71.6
HOUSEHOLD RECEIVES FOOD STAMPS					
Yes	6.5	6.6	9.6	10.2	6.7
No	93.5	93.4	90.4	89.8	93.3
HOUSEHOLD RECEIVES MEANS-TESTED CASH BENEFITS					
Yes	8.2	7.8	8.1	11.9	8.1
No	91.8	92.2	91.9	88.1	91.9

Table 2. Chi-Square for Pairs of Columns in Table 1

Characteristic	Degrees of Freedom	Pairs 1,2	1,3	1,4	2,3	2,4	3,4	1,1+2+3
Age	5	38.7*	18.8*	213.0*	0.3	88.6*	57.9*	50.6*
Sex	1	3.2	1.0	9.1*	0.0	2.7	2.2	3.8
Marital status	1	26.8*	10.0*	122.2*	0.0	47.4*	36.2*	32.7*
Relationship to reference person	4	52.0*	17.9*	660.1*	0.2	226.2*	139.0*	59.9*
Income as percentage of food stamp cutoff	3	1.5	0.3	0.2	0.1	1.8	0.5	1.6
Household receives food stamps	1	0.0	2.0	3.6	1.5	2.8	0.0	0.4
Household receives means-tested cash benefits	1	0.1	0.0	2.9	0.0	3.0	1.7	0.0

Table 3a. Distributions for Gross Change Estimates for 1984 SIPP Panel for Group 2 and Comparable Group 1 Estimates

Characteristic	Group 2 Percentage Unchanged	Group 2 Percentage Changed	Group 1 Percentage Unchanged	Group 1 Percentage Changed	Wald Statistic for row
MARITAL STATUS					
Married, spouse present	95.8	4.2	97.4	2.6	11.3 *
Otherwise	94.5	5.5	95.3	4.7	1.5 *
HOUSEHOLD RECEIVES FOOD STAMPS					
Yes	63.2	36.8	72.2	27.8	6.8 *
No	97.7	2.3	98.2	1.8	4.4 *
HOUSEHOLD RECEIVES MEANS-TESTED CASH BENEFITS					
Yes	71.1	28.9	75.9	24.1	1.3
No	96.4	3.6	97.6	2.4	9.6 *
INCOME AS PERCENTAGE OF FOOD STAMP CUTOFF					
< 100%	45.8	54.2	54.1	45.9	9.5 *
> 100%	92.3	7.7	95.7	4.3	45.7 *
RELATIONSHIP TO REFERENCE PERSON					
Self or spouse	97.1	2.9	99.1	0.9	23.1 *
Other	85.1	14.9	87.1	12.9	3.6

Table 3b. Distributions for Gross Change Estimates for 1984 SIPP Panel for Group 3 (from First to Last of 32 Reference Months) and Comparable Group 1 Estimates

Characteristic	Group 3 Percentage Unchanged	Group 3 Percentage Changed	Group 1 Percentage Unchanged	Group 1 Percentage Changed	Wald Statistic for row
MARITAL STATUS					
Married, spouse present	89.8	10.2	94.7	5.3	6.5*
Otherwise	86.4	13.6	90.0	10.0	1.9
HOUSEHOLD RECEIVES FOOD STAMPS					
Yes	57.9	42.1	61.7	38.3	0.1
No	96.5	3.5	98.1	1.9	1.7
HOUSEHOLD RECEIVES MEANS-TESTED CASH BENEFITS					
Yes	59.4	40.6	70.3	29.7	0.6
No	95.7	4.3	97.2	2.8	1.1
INCOME AS PERCENTAGE OF FOOD STAMP CUTOFF					
< 100%	46.2	53.8	47.5	52.5	0.0
> 100%	92.9	7.1	95.1	4.9	1.2
RELATIONSHIP TO REFERENCE PERSON					
Self or spouse	95.6	4.4	98.5	1.5	9.5*
Other	61.9	38.1	76.0	24.0	8.6*

Table 3c. Distributions for Gross Change Estimates for 1984 SIPP Panel for Group 3 (from Last Month Preceeding to First Month Following First Period of Missed Reference Months) and Comparable Group 1 Estimates

Characteristic	Group 3 Percentage Unchanged	Group 3 Percentage Changed	Group 1 Percentage Unchanged	Group 1 Percentage Changed	Wald Statistic for row
MARITAL STATUS					
Married, spouse present	94.3	5.7	98.5	1.5	24.9 *
Otherwise	96.0	4.0	97.1	2.9	2.8
HOUSEHOLD RECEIVES FOOD STAMPS					
Yes	74.4	25.6	77.2	22.8	0.3
No	98.2	1.8	98.9	1.1	3.6
HOUSEHOLD RECEIVES MEANS-TESTED CASH BENEFITS					
Yes	76.5	23.5	81.5	18.5	1.4
No	96.9	3.1	98.3	1.7	7.0 *
INCOME AS PERCENTAGE OF FOOD STAMP CUTOFF					
≤ 100%	51.5	48.5	62.5	37.5	7.5 *
> 100%	92.3	7.7	96.2	3.8	17.3 *
RELATIONSHIP TO REFERENCE PERSON					
Self or spouse	95.9	4.1	99.2	0.8	24.7 *
Other	80.9	19.1	92.1	7.9	38.9 *

Table 3d. Distributions for Gross Change Estimates for 1984 SIPP Panel for Group 4 and Comparable Group 1 Estimates

Characteristic	Group 4 Percentage Unchanged	Group 4 Percentage Changed	Group 1 Percentage Unchanged	Group 1 Percentage Changed	Wald Statistic for row
MARITAL STATUS					
Married, spouse present	98.4	1.6	97.4	2.6	6.1 *
Otherwise	92.7	7.3	95.1	4.9	9.5 *
HOUSEHOLD RECEIVES FOOD STAMPS					
Yes	62.7	37.3	74.5	25.5	12.8 *
No	97.5	2.5	98.7	1.3	7.0 *
HOUSEHOLD RECEIVES MEANS-TESTED CASH BENEFITS					
Yes	77.3	22.7	79.5	20.5	1.2
No	97.4	2.6	98.2	1.8	6.6 *
INCOME AS PERCENTAGE OF FOOD STAMP CUTOFF					
< 100%	54.5	45.5	61.7	38.3	2.6
> 100%	95.1	4.9	95.9	4.1	2.3
RELATIONSHIP TO REFERENCE PERSON					
Self or spouse	98.6	1.4	98.9	1.1	0.4
Other	92.1	7.9	87.5	12.5	23.2 *

Table 4. Distribution of Characteristics for 1984 SIPP Panel at WAVE 1, Month 4 by Weighting Procedure

Characteristic	Longitudinal weights	Cross-sectional weights all original sample	Cross-sectional weights positive longitudinal weight
AGE			
15-17	6.1	6.0	6.1
18-24	16.0	16.1	15.5
25-34	22.2	22.2	22.2
34-44	16.6	16.5	16.5
45-59	18.6	18.7	18.8
< 60	20.5	20.5	20.9
Wald statistic for columns 1 and 2: 7.7; columns 2 and 3: 5.5			
SEX			
Male	47.7	47.7	47.3
Female	52.3	52.3	52.7
Wald statistic for columns 1 and 2: 0.1; columns 2 and 3: 29.1*			
MARITAL STATUS			
Married, spouse present	57.8	57.2	58.1
Otherwise	42.2	42.8	41.9
Wald statistic for columns 1 and 2: 24.3*; columns 2 and 3: 59.1*			
RELATIONSHIP TO REFERENCE PERSON			
Reference person	47.5	47.5	47.9
Spouse of reference person	28.5	28.1	28.6
Child/relative of reference person	20.8	21.1	20.5
Non-relative related to others	0.4	0.4	0.4
Non-relative unrelated to others	2.8	2.9	2.6
Wald statistic for columns 1 and 2: 48.4*; columns 2 and 3: 106.7*			
INCOME AS PERCENTAGE OF FOOD STAMP CUTOFF			
< 100%	12.2	12.0	12.0
101 - 130%	5.3	5.4	5.3
131 - 185%	10.8	10.7	10.7
> 185%	71.7	71.9	72.1
Wald statistic for columns 1 and 2: 7.7; columns 2 and 3: 5.5			
HOUSEHOLD RECEIVES FOOD STAMPS			
Yes	7.5	7.2	7.3
No	92.5	92.8	92.7
Wald statistic for columns 1 and 2: 9.6*; columns 2 and 3: 0.7			
HOUSEHOLD RECEIVES MEANS-TESTED CASH BENEFITS			
Yes	8.6	8.5	8.5
No	91.4	91.5	91.5
Wald statistic for columns 1 and 2: 2.6; columns 2 and 3: 0.1			
HIGHEST GRADE COMPLETED			
< 9	13.2	12.8	13.2
9-11	17.9	18.0	17.8
12	32.9	32.9	32.8
> 12	36.0	36.3	36.2
Wald statistic for columns 1 and 2: 12.8*; columns 2 and 3: 14.3*			
TENURE IN LIVING QUARTERS			
Owned or being bought	69.8	69.5	70.3
Otherwise	30.2	30.5	29.7
Wald statistic for columns 1 and 2: 4.3*; columns 2 and 3: 34.2*			
HAVE SAVINGS ACCOUNT			
Yes	56.5	56.3	57.0
No	43.5	93.7	43.0
Wald statistic for columns 1 and 2: 3.6; columns 2 and 3: 34.9*			