EXCLUDING SAMPLE THAT MISSES SOME INTERVIEWS FROM SIPP LONGITUDINAL ESTIMATES

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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 variances and biases of the exclusion of these sample cases, particularly those 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 understand the problem of sample omitted from the longitudinal estimates, some knowledge is required of the design, the noninterview problem, and the weighting procedures used in SIPP. 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. The comparisons are for certain demographic and economic characteristics at the time of each sample person's first interview and 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 weights are compared to the same estimates obtained from the longitudinal weights. 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, possible modifications of the current longitudinal weighting procedures are discussed which would include some presently excluded cases or use multiple sets of weights to cover different time intervals.

Due to space limitations the full paper could not be presented here. In particular, nearly all references to group 4 cases (as defined in Section 3) have been omitted. The complete paper is available from the authors.

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. 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 has a nontrivial noninterview rate. In the 1984 SIPP panel 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. 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 and the other for longitudinal person estimates. The cross-sectional weighting procedure assigns a different set of weights to each month of the panel. For each month, positive weights are given to each original and 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 responded

to all three interviews or who left 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, a full 1984 panel file, has just been released, but too late to use in our work. 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. This set is particularly intended for use for estimates for time 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.

The weighting procedures used to obtain the final weights for each of these sets of longitudinal weights incorporate two adjustments 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 for the panel weights for the full-panel file, and January 1984 and January 1985 for the two calendar year weights. The first such adjustment, referred to as the longitudinal noninterview adjustment, is used exclusively for this purpose. The other adjustment is the final stage of the longitudinal weighting procedure, which 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. Both adjustments are described in Jones (1986). The effectiveness of these two adjustments 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. 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.

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 rationale for the partition of the set of original sample people who missed at least one interview into the groups 2 and 3 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.

The comparisons are with respect to the demographic and economic characteristics that are listed in Table 1. The file used in producing this table covers data from all eight interview periods and contains cross-sectional weights for each reference month but no longitudinal weights. The main purpose of the comparisons is to help determine the effects of excluding group 2 and group 3 cases from the longitudinal estimates.

In Table 1, the distributions of the indicated characteristics are presented for groups 1-3. 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.

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 in Table 2, which is only presented in the full paper. To summarize the results from that table, there are no significant differences at all between groups 2 and 3 at the five present level, and for most of the characteristics the chi-square values are very close, an indication that these two groups may be similar in some respects. The differences between groups 1 and 2, and 1 and 3 are significant for the first four characteristics in Table 1, which are the demographic variables, but there are no significant differences for the 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.)

The second set of comparisons, presented in Table 3, are gross change tables for the characteristics in Table 1, except age and sex which are omitted for obvious reasons. To simplify Table 3, the characteristics with more than two values in Table 1 were collapsed into two categories. For group 2 (Table 3a) 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 and 3 cannot be directly compared because of the different distributions of pairs of months in the table. However, estimates from both of these groups can be compared to estimates from group 1 cases, as follows. Corresponding to the estimates computed from the

group 2 or 3 cases in each of the three subtables a–c, 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 from each pair of months in each subtable is the same as for the group 2 or 3 estimates in the subtable.

For each row in each subtable the proportion remaining in category for group 2 or 3 was tested for a significant difference at the five percent level against the same proportion for group 1, with a significant difference indicated by a asterisk to the right of the last column in the subtable. (The actual values of the test statistic are presented in the full paper.) In Tables 3 and 4 a Wald statistic (Kendall and Stuart 1979) was used instead of an adjusted chi-square statistic as a test statistic, 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 or 3, and in many cases the differences are significant. These results provide some support to the hypothesis that people who do not miss any interviews tend to have more stable characteristics than those 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. However, as previously noted, longitudinal estimates are computed with a 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, 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 are presented in this table: one set obtained from all original sample people using first wave cross-sectional weights, and the other set obtained using these weights but only including the people receiving positive longitudinal weights on the three-interview longitudinal file.

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, 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 different intervals than in Table 4. There is no characteristic titled means-tested cash benefits in this adjustment, but there is a characteristic for receipt of selected welfare benefits 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 adjustment, 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.

To summarize the analyses in this section, the data in Tables 1-3 show there is some evidence that, for certain characteristics, groups 2 and 3 are significantly different than group 1, while Table 4 indicates 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 result in smaller variances, since a larger proportion of the sample would be included in the estimates. Smaller biases should also result for this reason and because some of the characteristics of groups 2 and 3 appear similar, as noted in the previous section.

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 bias associated with it.

Even if imputation error is ignored, any reductions in biases from including the group 3 cases would generally be less than otherwise 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 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 within PSU 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 larger group 2. However, if it is assumed that the distributions for group 3 are closer than the distributions for group 1 to the distributions for group 2, then it would be possible to use a 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 be offset by potentially large increases in variances. For example, consider the case of simple random sampling with replacement with a single 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,

$$\frac{\sigma^2}{n_1 + n_3} . \qquad (4.1)$$

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

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

The ratio of (4.2) to (4.1) 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

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

For the 1984 SIPP panel, 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 (4.2) over all samples to the expected value of (4.1), 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 interviews 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 missed 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 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 3 case could be included for estimates covering some time interval.

Weights can also be constructed to correspond to time intervals that begin after the first wave. For example, weights for 1984 and 1985 calendar year estimates appear on the full 1984 SIPP panel file. For such time intervals, not only can all group 2 and 3

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cases for which there are interviews throughout the interval be used in the estimates, but group 4 cases which satisfy this condition can be included, too. The advantages and disadvantages of the inclusion of group 4 cases are discussed in the complete paper.

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 and 3 cases for intervals for which these cases missed no interviews, by the creation of additional sets of weights. There should be an 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. Also, these slight gains would be more than offset by the additional expense required in producing the extra sets of weights and the resulting larger 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.

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

Characteristic	Group 1	Group 2	Group 3
AGE
15-17	5.8	6.6	6.9
18-24	14.5	20.7	21.4
25-34	21.8	23.1	23.6
35-44	16.5	16.2	16.1
45-59	19.0	18.1	17.3
> 60	22.4	15.3	14.7
SEX
Male	46.8	50.1	49.8
Female	53.2	49.9	50.2
MARITAL STATUS
Married, spouse present	59.7	50.4	50.6
Otherwise	40.3	49.6	49.4
RELATIONSHIP TO REFERENCE PERSON
Reference person	48.5	43.6	44.1
Spouse of reference person	29.4	24.2	24.7
Child/relative of reference person	19.6	26.8	25.9
Non-relative of reference person with household relatives	0.3	0.9	0.8
Non-relative of reference person without household relatives	2.2	4.5	4.6
INCOME AS PERCENTAGE OF FOOD STAMP CUTOFF
< 100%	11.8	13.8	12.9
101 - 130%	5.6	6.1	6.1
131 - 185%	10.5	10.1	9.7
> 185%	72.1	70.0	71.3
HOUSEHOLD RECEIVES FOOD STAMPS
Yes	6.5	6.6	9.6
No	93.5	93.4	90.4
HOUSEHOLD RECEIVES MEANS-TESTED CASH BENEFITS
Yes	8.2	7.8	8.1
No	91.8	92.2	91.9

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

a. Group 2 vs. Group 1

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

b. Group 3 (from First to Last of 32 Reference Months) vs. Group 1

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

c. Group 3 (from Last Month Preceeding to First Month Following First Period of Missed Reference Months) vs. Group 1

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

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

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