

# Survey of Income and Program Participation

QUALITY OF SIPP ESTIMATES

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I. INTRODUCTION

The Bureau of the Census has been conducting interviews for the Survey of Income and Program Participation (SIPP) since October 1983. The SIPP is a national survey and is designed to provide improved information on income and participation in government programs for the noninstitutionalized United States population. Person and household characteristics that may influence income and program participation are also available from the SIPP. This information is vital for improving the capability of federal agencies to formulate and evaluate their policies and programs in the areas of income and social welfare.

A. Background

The estimates produced from the survey can be divided into two groups. The first group includes cross-sectional and cross-sectional type estimates. These estimates are obtained from the wave data files and the longitudinal data files. Examples of such estimates from wave files include the unemployment rate in March 1987, net change in unemployment rate between March 1986 and March 1987, number of persons participating in the Food Stamp Program in February 1987, and the number of females who completed high school in 1986. Annual estimates of income and estimates of change of certain characteristics are examples from longitudinal files. For our discussion, these estimates will be called cross-sectional estimates. The method developed for producing wave file estimates is described in King (1985), and King and Kim (1986). The estimation method developed for the first SIPP longitudinal file covering the first three interviews of the 1984 panel is presented in Kobilarcik and Singh (1986). The methods for the longitudinal 1984 panel file are presented in Hock (1988).

The second group includes the estimates of gross flows (transition from one state of economic or labor condition to another state) and distributions of the length of spells. The transition from any state, say 'A', to another state, say 'B' triggers an end to spell of state 'A' and the beginning of a spell for state 'B'. Thus, an estimate of gross flows has a direct effect on spell estimates. These estimates are important because they could serve as a very powerful instrument in explaining socio-economic processes. For example, what happens to the health insurance coverage of a person who no longer receives welfare benefits?

In this paper, we discuss quality issues for both cross-sectional and gross flow/spell estimates. We discuss what we know about the quality of the SIPP data, the different types

of error that can occur, and ideas for research to better understand and reduce error. A major purpose of the paper is to strongly encourage people outside the Census Bureau to research ideas discussed here and on other ideas that will improve our understanding of the quality of the estimates and help improve it.

We will first give a summary of the major points in the paper. We begin with a brief description of the SIPP sample design. Section II discusses in detail what we know about the quality of SIPP estimates. For cross-sectional core data, the SIPP estimates of the number of recipients for government programs and amounts of income received are generally lower than available independent estimates from administrative sources. However, SIPP estimates related to programs are generally closer to the independent estimates than are Current Population Survey (CPS) estimates. In particular, based on initial evaluation the SIPP estimates of persons below the poverty level may be superior to the CPS estimates.

Little information is available about estimates of change. There has been some evaluation of topical module data. A couple of apparent problems with this data have been uncovered. The apparent problems are 1) The educational financing data seems to be of generally poor quality; and 2) The characteristics of tax filers in SIPP are different from IRS data.

For gross flow and spell estimates from the core data there is one particular problem. Many more changes in reciprocity status and amounts occur between a pair of two consecutive months in a different interview than between two months within the same interview. We have examined three income sources to see if the start-up and exit rates are biased by this problem. For food stamps, there is no evidence of bias. For aid to families with dependent children (AFDC), sampling errors are too large to be able to draw reliable conclusions. For supplemental security income (SSI), start-up and exit rates do appear to be significantly biased. Thus, the quality of these rates appear to vary by income source. For some purposes, eg. multivariate analysis at the micro level of gross flow and spell estimates, the affect of this inconsistency problem is unknown. More evaluation of micro level relationship among variables is needed to judge the quality of SIPP data for its uses in multivariate analysis.

Section III briefly discusses a number of different error sources. Some appear to have minor effects on estimates and some have at least the potential to cause major effects on some estimates. The sources of minor effect are interviewer coding, data coding, and use of proxy respondents. The potentially major effect sources are changes in interviewer, nonresponse, undercoverage, imputation, questionnaire wording and design, length of recall, and learning effects of respondents.

We continue section III by discussing three studies that have examined some of the sources of error. In a recall effect study (Petroni, 1986), we concluded that for many questions respondents tend to give the same response for all four months covered by a single interview. In a transition pattern study (Weidman, 1986 and 1987), we concluded that transitions did not seem to differ much among demographic groups and by self vs. proxy respondent. However, transitions are greater when some of the data has to be imputed. For the third study (McArthur and Short, 1986), we looked at the characteristics of people who remained as respondents and those who became noninterviews after responding in earlier interviews.

Section IV of the paper discusses a number of ideas for research. There are 12 research proposals aimed at improving our understanding of quality and 14 proposals for improving estimates themselves. Some examples of areas for research to improve understanding are: time-in-sample bias, expanding reinterviews, and coverage research. Some examples for improving estimates are: reducing complexity, reducing nonresponse, changing the reference period, increasing respondent effort, and improving interest and dividend incomes. Section V presents a brief summary of the paper.

Section II of the paper makes it clear that there are major gaps in our knowledge about the quality of the SIPP estimates. Even if we were to do all the research discussed in Section IV, we would only close some of the gaps. With the amount of data that can be provided from the SIPP and the disparity in the uses that can be made of it, it would be impossible to make a simple overall statement of the quality and adequacy of the estimates even if we knew everything possible about quality. It is also obvious that only a few of the research areas of section IV can be substantially addressed by Census Bureau staff in the short term. Although we hope that people outside the Bureau will address a few areas as well, this will still leave a lot of important research undone.

#### B. Sample Design

The SIPP is a multistage stratified systematic sample of the noninstitutionalized resident population of the United States. This population includes persons living in group quarters, such as dormitories, rooming houses, and religious group dwellings. Noncitizens of the United States who work or attend school in this country and their families are eligible. This country and their families are eligible. Crew members of merchant vessels, Armed Forces personnel living in military barracks, and institutionalized persons, such as correctional facility inmates and nursing home residents are ineligible. In addition to these general restrictions, only persons who were residing in the United States at the time of the first interview were eligible for SIPP. Also, only per-

sons who were at least 15 years of age were eligible for interview, although limited data on children were also collected by proxy interviews.

Initially, a sample of living quarters in selected Primary Sampling Units (PSUs) is taken. Living quarters are considered separate if the occupants do not live and eat with any other person in the structure and have either direct access from the outside of the building or through a common hall, or complete kitchen facilities for that unit only.

The SIPP sample is divided into four groups of equal size called rotation groups. One rotation group is interviewed each month. In general, one cycle of four rotation groups is called a wave. This design provides a smooth and steady work load for data collection and processing. Persons in the sample are interviewed once every four months for approximately two and one-half years. The reference period for the interview is the four months preceding the interview month. For example, for the first SIPP sample, the reference period for the November 1983 interview month (rotation group 2) was July through October 1983. These sample persons were interviewed again in March 1984 for the November 1983 through February 1984 period.

Persons 15 years old and over present as household members at the time of first interview are to be part of the survey for the entire two and one-half year period. With certain restrictions, these sample persons are followed if they move to a new address. "New" persons living with sample persons are considered to be part of the sample only while residing with these sample persons. More details on the SIPP design are given in Nelson, McMillen, and Kasprzyk (1985).

The SIPP questionnaire is long and complex. Questions are asked by specific type of cash and non-cash income on months received and amounts per month. For many types of income, additional questions are asked of recipients. For example, in households with children covered by medicaid, up to 8 questions about health insurance are asked. Questions are also asked about assets and labor force status. Topical modules on various subjects are also included in most interviews.

## II. QUALITY OF ESTIMATES

The quality of the SIPP estimates is judged by comparing them with estimates from independent sources primarily to evaluate bias. These independent sources include administrative records maintained by various government agencies and household surveys conducted by government agencies and other survey organizations. The magnitude of nonsampling errors varies from source to source and makes it difficult to compare estimates. Furthermore, the estimates for the SIPP are produced only for the 1984 panel,

which may be different because it's the first one. Therefore, the results presented here should be considered preliminary and caution should be exercised in drawing conclusions about the quality of the SIPP estimates.

#### A. Quality of Core Items

Data on a large number of items are collected in each SIPP interview. These items are called core items and two different types of estimates - rates (or percents) and totals - are produced from them. Estimates of change are also produced for each of these. The quality of these estimates is discussed below.

##### 1. Estimates of Rates and Levels

The quality of selected cross-sectional estimates based on the core part of the questionnaire is discussed in this section. The selected estimates primarily represent income and program participation items and include income from wage and salary, food stamps, social security, etc. Table 1 presents quarterly SIPP and 1983 CPS estimates as a percent of independently derived estimates. The table shows that, except for wage and salary income, estimates derived from SIPP are higher than the corresponding 1983 CPS estimates and are better than the CPS assuming that the independent estimates are accurate. However, these estimates are lower than those for the corresponding independent source, except for social security income.

A careful examination of these estimates also suggests that SIPP provides better estimates of number of program participants than it does of aggregate income amounts for 1) veteran's compensation or pension and 2) food stamps. These results suggest that either the income amounts for these two programs tend to be underreported by beneficiaries or the beneficiaries with larger amounts are disproportionately underrepresented in the SIPP. The administrative record check study currently underway at the Census Bureau may shed light on this issue (Moore 1986). Furthermore, the quality of estimates other than unemployment compensation appears to be quite stable over time (see tables 1 and 2). Coder (1987b) monitored estimates of state unemployment compensation for all quarters through unemployment compensation for all quarters through 1985 from the SIPP 84 panel and found that their quality appears to be declining. These quarterly estimates are presented in table 2.

Carlson and Dalrymple (1986) compared selected income characteristics of food stamp recipients from two data sources: Wave 1 of the 1984 SIPP Panel and the Food and Nutrition Survey (FNS) of administrative records of food stamp participants in August 1983. Those who were identified as food stamp recipients in SIPP for September



1983 were analyzed in their study. (They felt this time difference should not adversely affect their study since their comparison between the SIPP August and September 1983 reference month files showed trivial differences.) They found that the differences in income characteristics between the SIPP and the FNS estimates were relatively small for the households with only one food stamp unit and no subunit. However, SIPP showed considerably fewer households (36%) with both Aid to Families with Dependent Children (AFDC) and food stamps than the FNS (46%).

When households with subunits were included in the analysis, they found larger differences for selected income characteristics. Some of the differences could be explained by the relative influence of characteristics of the members in subunits. However, the differences were not entirely explained.

The quality of the SIPP poverty rate was evaluated by comparing it with the CPS rate. Note that the concepts and the procedures for the CPS are different from the SIPP and the comparison of their estimates is not totally valid. Coder et. al. (1987) obtained the CPS type income estimates for the SIPP in order to compare SIPP with CPS. Annual SIPP household income was determined using the household composition as it was for the twelfth reference month on the longitudinal research file consisting of the first three interviews in the 1984 SIPP panel. He showed that SIPP estimates lower poverty rates than CPS for all persons, white and black. The poverty rates for all persons from the SIPP and the CPS were 13.0% and 14.8%, respectively. The rates for white and black also showed similar differences. Ruggles and Williams (1986) also found lower poverty rates by family type for the SIPP than the CPS using the CPS type income estimates and the SIPP data for waves 2 through 5 from the 1984 SIPP panel. We believe the poverty rates from the SIPP may be better since SIPP captures income from transfer programs better than CPS (see table 1). SIPP is also more successful in capturing persons with marginal income because of a shorter recall period.

Vaughan (1988) compared interest and dividend income amounts from the SIPP with the CPS and independent estimates. The SIPP provided better dividend amount data than the CPS. However, the estimates from both surveys were way too low compared to the independent estimates. The SIPP and the CPS both underestimated income amounts from interest. Data did not show which of the two was better.

Evaluation of the estimates produced from the longitudinal data file is in its early stages. Tables 3-6 present a few selected estimates from Coder (1986b). These estimates have been compared with estimates from independent

sources. Some estimates appear to be of good quality - for example, persons receiving AFDC, food stamps in fourth quarter of 1983, mean annual income amounts from rents and royalties - although more research is needed.

## 2. Quality of Estimates for Change

SIPP also provides estimates of change in level (or percent) for many characteristics, such as the number of food stamp participants and the number of households by source of income. As a part of the SIPP evaluation, estimates of changes between the third quarters of 1983 and 1984 were examined for certain characteristics. Table 7 presents relative change estimates from the SIPP and independent sources. Differences in these estimates are also presented in the table. These differences between estimates from the SIPP and independent sources for Social Security, SSI, AFDC, and food stamps appear to be large for analytical purposes but they are not statistically different due to small SIPP sample size. (The changes in level estimates were also not statistically different.) However, the numbers of total households with four (out of five) selected assets are significantly lower for the third quarter of 1984 than for that of 1983. (see Table 8.) Further analysis utilizing either estimates for a longer period or estimates from independent sources will shed light on whether or not the change estimates are influenced by nonsampling errors such as time-in-sample bias, learning effects, etc.

Hill (1987) studied marital status and its changes over time as reported for the SIPP and independent data sources. Independent national estimates were based on either pertinent information in the Statistical Abstract (1986), a combination of published vital statistics and the CPS, or obtained from the Panel Survey of Income Dynamics (PSID). SIPP estimates were based on waves 1 through 3 data of the 1984 panel for rotation groups 1 through 3, individuals aged 15 and over responding in all three waves. Wave 3 weights were used since longitudinal weights were not available. Hill found significantly lower proportions of changes in marital status reported in SIPP over the course of the year than for the other sources. For example, for persons 15 years or older, SIPP reported 1.4% becoming married, while the Statistical Abstract (1986) indicated 2.6% becoming married. SIPP reported 0.6% becoming divorced while a combination of Vital Statistics and the CPS reported 1.3%. Lower changes were reported for all status changes except into widowhood.

### B. Quality of Estimates from Topical Modules

SIPP is designed to provide data on a number of special topics. The data on these special topics (usually called

topical modules) are not collected during each interview. The evaluation of the topical module data is not completed and it would be difficult to discuss here the quality of data from each topical module evaluated so far. However, the quality of data for selected modules will be discussed. Since the quality of data for selected modules will be discussed. Since the quality of the data from a topical module depends on its topic, no general conclusions about the quality of topical module data is possible at this time.

SIPP collected data in Wave 5 of the 1984 panel on child care arrangements. The data analyzed were averages of the usual child care arrangements from December 1984 through March 1985 and the results were presented in the Current Population Reports, Series P-70, No. 9, of the Census Bureau. The report also compared the SIPP data with May 1985 data from the CPS and 1984 individual income tax returns. A few of these comparisons are presented here. SIPP estimates about 900,000 children under 15 years of age were cared for by unmarried men while CPS estimates that 671,000 children under age 12 and 528,000 children 12 to 17 years old were with unmarried fathers. Assuming a uniform distribution for children 12 to 17 years old that were cared for, the CPS estimate for children under 15 years of age that were cared for is 935,000. Thus, SIPP and CPS estimates appear to be comparable. SIPP and CPS estimate that 5.5% and 4.6%, respectively, of working women were absent from work due to failure in childcare arrangements.

SIPP estimates of employed women with at least 1 child under 15 and of child care arrangements don't seem to be that inconsistent with IRS estimates. (See Current Population Reports, Series P-70, No. 9.) However, inconsistencies between SIPP and IRS universes preclude any definite conclusions.

During Wave 4 of the 1984 SIPP panel, data on household wealth and asset ownership were collected. A comparison of the SIPP aggregate asset amounts with estimates derived from the Flow of Fund data of the Federal Reserve Board (FRB) along with the detailed analysis of the SIPP data is presented in the Current Population Reports, Series P-70, No. 7 of the Census Bureau. Curtin et al (1987) compared the SIPP wealth data with the 1983 Survey of Consumer Finances (SCF) and the 1984 Wealth Supplement to the Panel Study of Income Dynamics (PSID). One should be cautious in interpreting their results. This is due to the fact that the SIPP data file has wealth top-coded. In addition, there are some conceptual and logical differences among these surveys.

Table 9 presents the estimates from the SIPP and the FRB data published in the Current Population report. The differences in estimates from the two sources are large, but one should be careful in drawing conclusions from this table due to the following limitations. 1) The household sector in FRB data include nonprofit organizations and private trusts not

covered under the SIPP. 2) The SIPP universe consists of noninstitutionalized resident population living in the United States and at least 15 years of age. The FRB Balance Sheet includes the asset holdings of the institutionalized population. 3) The household sector of the FRB balance sheet is estimated as a residual after allocations are made to farm business, nonfarm noncorporate business, nonfinancial corporate business and private financial institutions. As a result, accuracy of household sector estimates is reduced.

The Annual Roundup topical module was administered in Wave 6 of the 1984 panel. Coder (1987d) found that the SIPP estimate of 111.9 million recipients of wage and salary for calendar year 1984 is lower than the CPS estimate of 114.4 million (the SIPP and the CPS estimates include imputed data.) Furthermore, the overall nonresponse rate (including household, person and item nonresponse) for wage and salary amounts was about 40 percent. This rate is much higher than the CPS rate of 24%. Also, only 30% of the amounts were taken from W-2 forms even though its use was encouraged in SIPP. The data from the remaining respondents were based strictly on their recall. Table 10 presents median wage and salary income of those who used W-2 forms and those who did not. The table shows that, in general, the median income of those who used W-2 forms is higher than those who did not. Furthermore, SIPP estimates of wage and salary based on the core data are lower than the CPS estimate. (See table 1). Overall, the quality of wage and salary data from the SIPP is not as good as from the CPS.

Coder (1987e) also found that the distribution of tax returns by return type in the SIPP is different from the IRS. He indicated that the number of single returns are underreported in the SIPP. Also the SIPP adjusted gross income (AGI) medians by return type are higher than for the IRS.

Kominski (1987) analyzed the data for educational financing collected in Wave 6 topical module of the 1984 panel and found that the estimates in general do not come close to independent estimates of financing for the period these data reference. (The topical module data he used was not edited.) He also observed large discrepancies in reporting the same phenomenon in the core and the topical module. Thus, the overall quality of the SIPP data for educational financing is poor in the 1984 Panel. Starting with the 1985 Panel, the questions related to educational financing were changed substantially so that the core questions closely mirror topical module questions.

#### C. Quality of Gross Flow and Length of Spell Estimates

Let us first discuss the measurement of gross flows between any pair of consecutive months. For example, in table 11, gross flows between January 1984 and February 1984 are

observed from a single interview (i.e., second interview) for rotations 2, 3, and 4. For rotation 1, they are observed by linking two interviews (the second and third interviews). Thus, the SIPP design produces four measurements, one for each rotation group. Three of them come from a single interview (within reference period) and one measurement comes from a pair of consecutive interviews.

The preliminary analysis of unweighted data from the SIPP [Coder 1986a] presents evidence that gross flows differ for pairs reported by the same interview from those reported from two consecutive interviews. Some selected results are presented in table 12, which shows month-to-month changes in reciprocity and amounts for food stamps. Month-to-month changes for fourth to fifth and eight to ninth correspond to the seams where reference periods join (i.e., two consecutive interviews). All other pairs are from the same interview. Note that there are many more transitions between the eighth and ninth months and the fourth and fifth months than between other pairs of months. This pattern also holds for other characteristics such as railroad retirement, child support payments, state unemployment compensation, etc. [Coder 1986a]. Moore and Kasprzyk (1984) also observed similar results in ISDP-79 data for these and other characteristics. These differences are clearly due to nonsampling error in reporting. This reporting pattern affects estimates of the covariance structure and has significant adverse effects on multivariate analyses dealing with transitions or length of spells.

The problem with gross flow estimates is not unique to SIPP. Hill (1987b) also reported problems with gross flow estimates in the Panel Survey of Income Dynamic (PSID). Similar problems for the Current Population Survey have been known to analysts for over twenty years and are discussed in the proceedings of the Conference on Gross Flows in Labor Force Statistics (1985).

A large proportion of the research on transitions at the Census Bureau has concentrated on government benefit programs and labor force status. This work includes comparisons of SIPP with CPS and administrative data in order to evaluate the quality of reported transition rates, and examination of relationships between demographic characteristics and the months in which transitions are reported. In this section we review the results of the comparison studies.

Ryscavage and Feldman-Harkins (1988) compared gross flow and stock (level) estimates for labor force status from the SIPP and the CPS. In their study they found that the SIPP provided lower gross flow estimates than the CPS. The study found that the gross flow estimates from the SIPP were more consistent with the corresponding estimates of stocks (levels). They pointed out that this is bound to be the case because of the SIPP design. The larger inconsistency in the

CPS estimates was attributed to the fact that the gross flow estimates from the CPS for any pair of two consecutive months are obtained from two different interviews. They reserved their judgement about the quality of the SIPP labor force flows at this point since the survey designs in the SIPP and the CPS are very different and suggested further investigation before reaching any judgement.

Burkhead and Coder (1985), and Coder (1986a) show that transitions are dramatically understated most months and/or overstated every fourth month. If transitions are overestimated at the seams and underestimated within reference periods, then the combination of these for a given pair of months or over an interval of months may be less biased. With this in mind, studies to evaluate the bias in reporting for participation for food stamps (Judkins 1986), AFDC (Maher 1987b) and supplemental security income (SSI) (Maher 1987c) have been completed. In these studies, start up and exit rates (transition rates) for SIPP were computed using unweighted data from the SIPP longitudinal file (Coder 1986a). Noninterviewed cases were excluded and imputed data were used for item nonresponse.

Food stamp start-up and exit rates were computed from administrative record data prepared by the Urban Institute (1985) for the Food and Nutrition Service. These data were obtained using a two-stage stratified sample (with equal probability of selection) of local food-stamp offices in the 48 coterminous states and the District of Columbia. Complete case histories on subsamples of cases active between October 1, 1980 and December 31, 1983 were collected. Data from the last six months were used in the comparison study. Due to internal inconsistencies, about eight percent of the cases from the administrative records were discarded.

The start-up rate is defined to be the percent of active participants who are in the first month of a participation spell. Similarly, the exit rate is defined as the percent of active participants who are in the last month of a participation spell. The average rates were compared for four pairs of reference months for SIPP with six pairs of reference months (covering the same calendar months) for the administrative records. These results are presented in table 13. This study, even with its limitations, was very encouraging. Transition rates based on measures for all four rotation groups provide no evidence of differences between SIPP and Administrative Records Data. The results may be different if weighted data are used, but it seems unlikely.

For AFDC, estimates of the administrative record rates were obtained from several issues of Quarterly Public Assistance Statistics (1983, 1984) which present data from complete sets of administrative records. Comparisons of average start-up and exit rates were made for the periods July-December 1983, October 1983-June 1984 and July 1983-June 1984 (see table

14). The average start-up rates are slightly lower for SIPP and the average exit rates are 20-30% lower for SIPP. When these differences are tested, they are not significant at the 10% level. (The tests were performed as if the estimates from each of the three periods are independent, but they have considerable overlap in data.) Standard errors on the SIPP estimates are very large, so no conclusions on the accuracy of transition rates are really possible. It is desirable to examine these estimates over a longer period of time in order to assess the bias in them.

For SSI, issues of the Social Security Bulletin (1984,1985) provide estimates of start-up rates for complete sets of administrative records, including people who are institutionalized or under age fifteen. Since SIPP does not include receipt of benefits for these people, adjustments to estimates from the bulletin were made based on the Social Security Administration's December 1983 1% file. Comparisons of average start-up rates are made for periods similar to those used in the AFDC study, and they indicate problems with the SIPP estimates (see table 15). Most of the within reference period rates for SIPP are as high or higher than all of the administrative rates, and the rates at the seams are still several times higher than those within waves. This results in tests that show significant differences at the 10% level between the two sources.

This higher start-up rate reported in SIPP could be a result of some confusion on the part of interviewed recipients between regular social security and SSI. If this is the case, then a comparison of exit rates should show the same pattern of monthly over-reporting as for start-up rates.

The results from these 3 studies suggest that each benefit source should be individually evaluated before using longitudinal estimates of transitions from SIPP. Similar types of studies should be extended to receipt and amount of income from various assets, as they show the same kind of within reference period vs. seam reporting pattern (Coder 1986a).

The reporting of more changes at the seam could have adverse effects on covariance structures and hence on micro-level analysis. The study of Young (1989) sheds some light on transition correlations between a number of different events and amount change status. Table 24 presents some of the correlations he computed. The number 1 in column 2 of the table refers to the pair of seam months, and numbers 2, 3, and 4 refer to the other 3 pairs formed by reference months within the interview. The correlations corresponding to these pairs are presented in their respective rows. Except for correlations of 'marital status' and 'married spouse present' with other characteristics, they did not show a pattern of distortion in bivariate relationships. These results are very encouraging. However, until more analysis is completed we should be careful reaching a definite conclusion.

Let us optimistically assume that other evaluation studies yield results similar to those for food stamps. Does it mean that our gross flows and length of spell estimates can be used by policy makers and social scientists? It depends on their goals. For some purposes they will be useful while for others they will not. For example, estimates of transitions based on measures for all four rotation groups for a given month at the macro level will be satisfactory. Furthermore, the estimate of change in number (or rate) of transitions and in length of spells based on measures for all four rotation groups would also be satisfactory if time-in-sample effect is small (compared to estimates). Such estimates would be worthwhile for policy makers and could assist them in evaluating their policies. On the other hand, more evaluation of covariance structures is needed to judge the usefulness of micro level multivariate analysis whose goal is to understand economic processes.

At present, very little is known about the bias in SIPP estimates. We need extensive research in this area to understand the problem better. Some possible research areas for determining the causes of the problem and how to correct it are discussed in Section IV.

### III. ERROR SOURCES

#### A. Identification of Sources

In order to conduct research into alleviating the problems discussed previously, we first attempt to identify causes for the observed response patterns. These causes can be separated into two types: those related to the respondents and those related to the survey instrument and its processing. Of course, there is some overlap between these types. The latter type includes questionnaire wording/design, interviewer coding and data keying errors, changes in interviewers, and imputation procedures. The former type includes respondent bias and variability, which may be affected by length of recall, learning effect of previous interviews, proxy respondents, demographic characteristics, and nonresponse. Each of these possible causes except the last will be discussed briefly here. Nonresponse is discussed in Section III.B.

##### 1. Interviewer Coding/Data Keying

Errors can be made by interviewers and keyers in transcribing the responses in order to produce a computer data file. A monthly verification of SIPP data keying in the regional offices based on a random selection of questionnaires and data fields yields error rates of about .3%. (See, e.g., Linebarger, 1986.) The effect of these errors on reported transitions can only be determined by



examining the individual errors more closely to see if they tend to introduce or mask transitions. If we assume that the interviewer coding rates are of the same magnitude, the overall effect of these sources on the reported patterns is minimal.

## 2. Change in Interviewer

The respondents in a household become familiar with an interviewer after one or more visits, establishing a rapport that is either beneficial or harmful to accurate response. When a new interviewer arrives the respondents may be more or less willing to reveal receipt of sources such as unemployment compensation. In either case, any change in response would most likely occur for the entire wave, thus introducing false transitions between waves. On the other hand, continuing with the same interviewer may cause under-reporting of transitions.

When new interviewers begin work they do not have the same familiarity with the questionnaire and respondents that more experienced interviewers have. This probably results in some differences in recorded responses, but it is difficult to quantify. The extent of this problem could be investigated by comparing the proportions of between wave transitions reported with the same and different interviewers, as well as with new and experienced interviewers.

## 3. Imputation

Imputation is used to provide values for items missing from an interview, which usually occurs simultaneously for all four months of a wave. As an example, incorrect imputation of receipt would cause transitions to be recorded when they did not happen, or vice versa. An examination of four waves of data has shown that the proportion of between wave transitions is higher for records with at least one of the waves having imputed data than when both are observed (Weidman, 1987). (See the next section for a more complete description of this work.) However, the nonimputed transitions also exhibit the problem pattern. Thus imputation magnifies an already existing problem.

## 4. Questionnaire Wording/Design

There are many aspects of the questionnaire and the interview process that affect errors. One general issue is the amount of effort made by respondents and interviewers to provide accurate data. On an interest amount question, for example, at one extreme a respondent might give a top-of-the head guess rounded to the nearest hundred dollars. At the other extreme, a respondent might thoroughly check their records, do some computa-

tions, and add interest across different accounts. How a respondent answers between these extremes is a function of many things, including the specific questions asked, to what extent the questionnaire and training encourage interviewers to probe and to ask for record checking, and the length and complexity of the interviews as a whole.

Another area of concern is the month(s) of receipt for income. Sources of income, assets, etc. received at some time during the wave are determined in the interview before the actual months of receipt are. During the probe for sources, the respondent may forget (or not consider important) a source that was received in only one month of a wave, the interviewer or respondent may lack an understanding of the correct source and misreport it, or the respondent may answer without thinking. These and other sources of response variance are related to the questionnaire format.

The specific months of receipt for each source of income, assets, etc. are determined later in the interview when the amounts are recorded. The months of receipts are queried for beginning with the last month of the wave. If this query began with the first month instead, the respondent might think more carefully about the actual months of receipt and avoid some of the above problems, because a longer recall would be required immediately. This could be a major cause of the observed pattern of transitions, since many people are affected in the same way by the questioning.

#### 5. Length of Recall

This problem is related to the queries about specific months of receipt of sources proceeding from the most recent to the most distant month. A person may report a transition in the wrong month by not remembering the exact month of occurrence. It may be easier to report the receipt state as being the same for all four months in a wave than trying to remember whether it changed 3 or 4 months ago, or if the receipt state in the first month was different than in the other three months the respondent may forget it.

#### 6. Learning Effect

After one or more interviews a respondent may determine that a receipt="yes" requires more additional questions than does a receipt="no". This would lead to excessive between wave transitions from receipt to nonreceipt. At a later time point a person may begin receipt and not report it for this same reason. This would lead to too few transitions from nonreceipt to receipt being reported regardless of the month in which they occurred.

## 7. Proxy respondents

Changing between proxy and self response may cause reported transitions that did not occur or misplace their month of occurrence. If the change is from self to proxy to self in successive waves, then errors in reporting by the proxy can be corrected through the source roster questions. However, if the proxy response continues this correction will probably not occur. Within wave transitions may be omitted or misplaced because of inadequate knowledge of the proxy.

Weidman (1986) has shown that proxies report a smaller percentage of receipt for many sources than do self respondents. This may cause errors in both between and within wave transition counts. However, there could be legitimate causes of this result other than proxies lacking knowledge about the missing respondents. A further investigation of the characteristics of proxies is required, but because the proportion of self respondents is so high, these errors can only be a minor cause of the observed pattern.

## 8. Demographics

It may be that respondents with certain combinations of demographic variables report a smaller proportion of receipt of certain sources than actually occur. Identification of such effects would allow us to adjust the data to allow for them or to alter the questionnaire in order to improve respondent accuracy. An investigation of certain demographic variables was made and showed only small effects of some combinations for some sources (Weidman, 1986).

## B. Nonresponse and Coverage of Population

Knowledge of rates and causes of nonresponse is important in evaluating the quality of SIPP. This section discusses SIPP nonresponse rates and compares them with those of other surveys. Before discussing this in detail, it is worth mentioning various type of nonresponse.

Every household survey includes individuals who do not respond or respond partially to the questions posed. This nonresponse can be divided into the following categories:

**Household Nonresponse:** Every member of the household is a noninterview.

**Person Nonresponse:** A member of an interviewed household could not be interviewed and a proxy interview is not obtained. It is called a type Z noninterview.

**Item Nonresponse:** A response to a given question is not available.

Table 16 presents response rates for the 1984 SIPP Panel, the National Medical Care and Utilization Expenditures Survey (NMCUES) and the PSID. These rates are not directly comparable due to differences in contents of the surveys, recall periods, frequency of interviews, etc. However, they do provide a general idea about the range of person response rates in multiple interview surveys.

Ongoing statistics have been kept on the distribution of non-interviews and their causes. There are 32,985 persons who were interviewed in wave 1, did not leave the universe, and were not cut from the sample. 69.8% of these were interviewed in each wave through the eighth and 20.2% became and remained noninterviews (including missing both waves 7 and 8). The importance of adjustment becomes important when this attrition is taken into account.

Dahmann and McArthur (1987) studied all persons at least 15 years old who were interviewed in the first wave and survived the fifth-sixth wave sample cut. They looked at differences in characteristics between persons with different interview response patterns. One of the comparisons was between people who responded in all waves and those who were missing at least the last two interviews. Persons who left the universe were not included in these calculations. For each of 23 variables recorded in the first interview, the distributions of these two groups were compared using chi-square tests adjusted by a factor of 3 to take account of the sample design. Significant differences at the 10% level were detected for most of these variables: regional office, size of SMSA, ownership of living quarters, interview status, length of interview, relationship to reference person, household size, age, sex, race, ethnicity, mover status, marital status, hours worked per week, employment status, household and person monthly income, having savings account, and having other types of assets.

McArthur and Short (1986) looked at the relationship between changes in these characteristics at an interview and whether or not a person became a noninterview for the next interview and all interviews through the fifth. There appeared to be relationships for changes in the number of persons in the household, employment status, household income and residence. The results of these studies have led to further work which is currently being pursued. That is, what combinations of variables differentiate persons who become and remain nonrespondents, and what variables and responses at one interview are related to a person becoming a nonrespondent at the following interview? It is hoped that the results of this work will lead to improved adjustments for nonresponse.

Item nonresponse rates for asset amounts were compared for the SIPP and the ISDP in the Current Population Reports, Series P-70, No. 7. It shows that SIPP item nonresponse rates are very large for some items such as value of own business (38%) and market value of stock and mutual fund shares (41%), but they are significantly lower than the ISDP rates for all the items.

Table 17 presents overall item response rates in the SIPP and the CPS for selected income types. These rates for the SIPP are based on core data. The overall item response rate is derived based on household, person and item nonresponse rates. These overall item response rate (100-nonresponse rate in %) for the SIPP are lower than for the CPS for all items presented in the table.

Undercoverage in a survey has an adverse effect on the quality of survey estimates. As a part of the evaluation of the SIPP data quality, the SIPP coverage of the target population by age, race and sex was examined. (Coverage is the ratio of the SIPP estimates of number of people in a specific demographic group to the corresponding independent estimate. Note that the SIPP estimate used is after adjustment is made for noninterviews. This adjustment increases the estimates according to the number of noninterviews, and therefore the indicated undercoverage is not explained by noninterviews. Also, the independent estimates are updated 1980 Census figures, without adjustment for Census undercount. Undercoverage is worse when Census undercount adjustment is included.) The examination showed that, like other household surveys, the SIPP also has a differential coverage by age, race and sex. The coverage ratios for the SIPP and CPS are about the same and are lower for blacks than whites, lower for males than females and are worst for black males 22-24 years of age in both surveys. As examples, SIPP undercoverage as compared to the Census is about 7% for nonblack females and about 15% for Black males.

Nonresponse and undercoverage in surveys are compensated for by complex imputation and/or weighting procedures. These procedures are developed on the assumption that within a demographic group, the persons who respond are similar to those who do not respond. In real life this is not true. Therefore, the quality of the survey estimates including estimates from the SIPP is affected adversely due to lack of complete coverage and nonresponse, and biases exist in estimates to the extent that persons in missed households or missed persons in interviewed households have different characteristics than the interviewed persons.

#### C. Examination of Error Sources

Several studies at the Census Bureau have examined one or more of the error sources identified in the previous section. In this paper we summarize the results of four of them. They

include a brief look at recall lag, a look at some possible causes of observed transition patterns, an examination of some possible causes of attrition, and an approach to modeling respondent error. The first two of these are presented here, the third in the previous section, and the last in section IV.

The recall effect study (Petroni, 1986) used data from September 1983 to attempt to determine if the number of months between occurrence and reporting of an event affects the reported value. For individuals three benefit sources, labor force activity and monthly income categories were tested. Eight benefit sources and monthly income categories were tested for households. Only one of twenty categories tested significant for recall lag effect at the .05 level, using chi-square tests adjusted for weighted data. This lack of recall lag effect is supported by examination of the data performed as part of the second study. There were extremely few cases where a change in receipt status was reported as occurring within a wave for the several income sources examined. This indicates that for many questions respondents give the same response (perhaps the current state) for all four months of a wave and thus only report changes at the beginning of a wave.

The transition pattern study (Weidman, 1986) examined three possible causes that could contribute to the reported between/within wave pattern of transitions for eight income sources: demographics, interview status (self or proxy respondent), and imputation procedures. We give a brief description of this study and its results.

The income sources examined were social security, unemployment compensation, private pensions, VA compensation and pension, supplemental security income, child support and AFDC. Demographic characteristics that were examined as possible causes of the reported patterns were age, sex, race, marital status, education, relationship to principal person, household size, tenure, and standard metropolitan statistical area (SMSA) size. The distribution of gross flows in receipt status between consecutive months for each income type was computed with respect to all pairs of demographic characteristics and interview status. There are four possible gross flow states for each pair of consecutive months: RR, RN, NR, and NN, where R=receipt and N=nonreceipt. RN and NR denote transitions between receipt states.

In light of the patterns reported by Burkhead and Coder (1985), how is it determined if any relationships exist? For any combination of demographic variables to be a determinant of this change, we would have to observe a huge difference in the number of transitions reported in the first month of a wave as compared to the last three months, but a much smaller difference for other combinations.

Within each cell defined by a particular pair of demographic characteristics, we calculate the probability of each receipt state,  $P_iAB = P(\text{receipt state AB for cell } i)$ . Let  $P_iAB_w$  denote such a probability within waves and  $P_iAB_b$  the corresponding between wave probability. Compare  $P_iNR$  and  $P_iRN$  for between waves to those for within wave. If this demographic combination has no relationship to gross changes, the ratios  $P_iNR_b/P_iNR_w$  should be fairly constant for all  $i$ , as should the ratios  $P_iRN_b/P_iRN_w$ . If one and/or both of these sets of ratios differ "greatly" between cells, this indicates the type of relationship we are looking for.

For the second part of this study there are four possible interview statuses of interest for two consecutive months: SS, SP, PS, and PP, where S=self and P=proxy. When examining interview status the situation is somewhat different than for combinations of demographic characteristics. This is because two of the interview status pairs, PS and SP, cannot occur within waves. In this case we look for large differences in the distributions of  $P_iNR_b$  and  $P_iRN_b$  between cells.

In either case we must be careful about looking at differences for probabilities based on very small numbers of observations because of the resultant large variances in proportions. We present two pairs of tables to represent the results of these comparisons. Tables 18 and 19 give the results for food stamps for sex by interview state. Tables 20 and 21 give the results for food stamps gross flows computed for race by sex. These tables are typical of the results obtained.

A result was noted for interview status, although no major influences on the reported pattern were identified based on the ratio and probability comparisons. For food stamps and social security, larger proportions of receipt of sources were reported by self-respondents than by proxies. Also, there is usually a higher proportion of transitions between waves when at least one of two consecutive months has a proxy response than when both of the months are self-reported.

In the last part of this study the proportion of gross flows that were transitions were calculated for consecutive months without imputation and with imputation. (See tables 22 and 23.) They show a larger proportion of between wave transitions when at least one of two consecutive months is imputed than when both of the months are reported. It may be that people with transitions are more likely to be nonrespondents, so we should not reach any conclusions regarding imputation without a closer examination of the data.

#### IV. HOW ESTIMATES CAN BE IMPROVED

In this part of the paper we briefly discuss a number of research areas. The first set of 12 topics use general research

to improve our knowledge in some aspect of SIPP quality. The second set of 14 topics goes further in that the research is intended to lead to changes that would improve quality. This is of course not a complete list of possible research, but we have attempted to be fairly comprehensive, possibly including some topics that are not very promising.

Due to limited resources, we anticipate doing work only in a few of these areas at the Census Bureau, and thus strongly encourage others to also work in these areas. We would be happy to talk to anyone with ideas for one or more research projects they would like to conduct.

#### A. Research for Improved Understanding

##### 1. Time-in-Sample Bias

A very little information about this bias is available from a single study (Coder, 1987a) using only a limited amount of SIPP data. It is generally important to know how large this bias is. In particular, a suggestion has been made to have only one panel in the field at a time. Thus, in one year all addresses would be in their first set of interviews and in the following year would be in their second set of interviews. This is an attractive idea if there is little or no time-in-sample bias but has obvious major problems if bias is high.

##### 2. Improvement of Independent Estimates

For several types of income, SIPP estimates of number of recipients and of amount have been compared to other estimates such as from the Bureau of Economic Analysis (BEA) and the Social Security Administration (SSA). As discussed earlier in the paper, these comparisons generally show SIPP estimates as too low, sometimes by small amounts and sometime by large. The independent estimates are usually for a slightly different universe, use slightly different definitions for the income source, and are subject to some biases of their own. Thus, especially for income sources where SIPP estimates are only a little lower, it is not clear if SIPP is underestimating recipients and amounts. Investigation into the independent sources could be done. For example, we may be able to adjust some BEA estimates for definition differences in some income types. In some cases, such adjustments have already been made to independent source estimates, but they were prepared in 1979 and may be out of date for the purpose of comparison.

##### 3. Recall Errors

The only investigation of recall errors used September 1983 data (Petroni, 1986). That month was in the first wave of the survey and may not be representative of other



waves. Thus, a series of comparisons should be made, including comparisons for population subgroups. Better knowledge about recall errors will be particularly needed if the reference period is lengthened.

#### 4. Direct Analysis of Gross Flow and Spell Data

The simplest form of analysis is subjective analysis of gross flow and spell data. One looks for illogical patterns and anomalies and postulates possible or likely causes for problems found. Much work of this type has of course been done (see, Burkhead and Coder (1985) for example), but more could profitably be done.

A follow-up to this subjective analysis is to identify individual cases where incongruous situations occur and then carefully examine the questionnaires to try to understand what might have happened. Examples of incongruity are no increase in social security income at a time when a cost of living increase in benefits occurs (Kalton and Miller, 1986) or a pattern of frequent changes in receipt/non-receipt for an income source. Little of this type of analysis has been pursued.

Another relatively simple type of analysis is the comparison of gross flows within an interview period to those between interview periods. As discussed above, this has already been done for a number of characteristics, but it could be done for many more characteristics for the 1988 panel to understand effect of changes in the questionnaire.

#### 5. Response Variance Estimation from Reinterview

The reinterviews conducted in SIPP allow for estimates of response variance. Simple estimates of response variance can be made for status characteristics which are used to produce gross flow and spell estimates. One would anticipate some large response variances for characteristics for which the seam flows are much greater than the non-seam flows.

Of greater potential value, however, is a detailed analysis of response variance by demographic characteristics and survey procedures. For example, one can compare response variances for different kin relations (head of household, spouse, and other relative), different ages, and self vs. proxy response on both original interview and reinterview. This type of analysis can indicate that problems exist in only certain situations, e.g., response variances are low for self reporters or for some age groups. O'Muircheartaigh (1986) did exactly this type of reinterview analysis for the Current Population Survey (see especially sections 4 and 5 of his paper). Note, however, that caution must be used in drawing conclusions

because of weaknesses in reinterview data and because there is no experimental control over items like self response versus proxy. Again, see O'Muircheartaigh (1986).

In principle, this analysis could be done with already collected SIPP reinterview data. There are however, three major problems. 1) All reinterviews have been done with reconciliation. It has been well documented (see U.S. Census Bureau (1968, p.25) that the estimated response variance in CPS is much lower with reconciliation than without. The reconciliation estimates are believed to be substantially underestimated. 2) Only a small proportion of all the questions have been included in reinterviews, and thus there is only limited data to analyze. Thus, to get a lot of value from this type of analysis, changes will be required in the reinterview program (see 7. below). 3) Reinterview questions are generally incomplete, i.e., reinterview asks only about receipt during the last four months without asking about specific months.

## 6. Response Variance Estimation Without Reinterview

### a. Use of Single Rotation Groups and Reference Months

A proposal has been made to estimate response variance in SIPP without use of reinterview data. Judkins (1985) suggests a complex estimator based on squared differences for single rotation groups and single reference months. The proof that the estimator is an unbiased estimate of response variance requires the assumptions that length of recall does not affect response bias, that response error is perfectly correlated within wave, and that response error is uncorrelated across waves. Though none of these probably hold exactly, they may be close enough to provide useful response variance estimates.

### b. Modeling

Another possible approach is to model the distribution of gross changes using either multivariate normal or logit models (Weidman, 1986). For CPS, it has long been known that there is a relationship between the responses to a question and (i) the amount of time that has elapsed between the month of interest and the month of interview, and (ii) the length of time a person has been in the sample. Work on SIPP has shown a relationship of certain self and proxy responses with interview status. Models were proposed for gross flows that make use of similar relationships.

The dependent variable of interest for a given income type is the receipt state identified with the second of two consecutive months. The possible receipt states for month  $t$  are (1)RR, (2)RN, (3)NR, (4)NN. Let  $Y_{ijkt(m)}$  be the number of responses in receipt state  $m$  in month  $t$  where

$i$  = number of times a person has been interviewed,

$j$  = number of months between month  $t$  and month of interview,

$k$  = interview status for months  $t-1$  and  $t$ ; PP,PS,SP and SS with S=self, P=proxy.

Then the vector  $Y_{ijkt}$  =

$(Y_{ijkt}(1), Y_{ijkt}(2), Y_{ijkt}(3), Y_{ijkt}(4))$ ,

represents the gross flow counts for the combination  $ijkt$ .

- (i) Multivariate Normal Models. Since the  $Y_{ijkt}$  are vectors of counts, they have a multinomial rather than a multivariate normal distribution. But because of the large sample sizes on which they are based (the total number of counts in  $Y_{ijkt}$ ), they have that distribution asymptotically. We propose a multivariate analysis of variance (MANOVA) model of the form:

$$E(Y_{ijkt(m)}) = \mu(m) + N_i(m) + M_j(m) + S_k(m) + NM_{ij}(m) + NS_{ik}(m) + MS_{jk}(m) + \hat{a}_t \quad (1)$$

where the terms are

$N_i$  = interview  $i$ ,

$M_j$  = months of recall between month of interview and month of occurrence,

$S_k$  = interview status,

$NM_{ij}$ ,  $NS_{ik}$ ,  $MS_{jk}$  are interactions of these effects, and

$\hat{a}_t$  = month  $t$ .

- (ii) Polytomous Logit Models. Alternatively, the probabilities of the receipt states could be estimated using logit models. In this method, the likelihood function is the

product of terms of the form

$$P(Y_{ijkt}(m')) = (\mathbf{X}'_{ijkt} \hat{\beta}_m) / \sum_{m=1}^4 \exp(\mathbf{X}'_{ijkt} \hat{\beta}_m).$$

Here  $\mathbf{X}_{ijkt}$  is a vector of 0-1 variables that indicate which main effects and interactions are present for a particular  $ijkt$  combination (as in the right hand side of (1)). Thus, we only need the  $Y_{ijkt}$  in order to determine the likelihood function and the resulting maximum likelihood estimates  $\hat{\beta}_m$ .

When using either of these methods, tests for main effects and interactions being zero would be carried out in order to determine which of them influence the reporting of changes in receipt state. There are some technical difficulties that must be addressed when using either of these models.

#### 7. Expanded Reinterview

It is desirable to keep the respondent burden to a minimum for a complex and lengthy survey like SIPP. Therefore, the reinterview program for the SIPP was designed to discourage fabrication of interviewing and to identify those interviewers who fabricate data. The program is very successful in achieving its goal. Unfortunately, it does not provide a good measure of response variance. Considering the problem with gross flows, it is important to explore all avenues that could help in improving these estimates even if it increases respondent burden and the risk of higher nonresponse in subsequent interviews.

As a starting point, the reinterview program could be expanded to measure response variance for selected items. These items may be selected only from one or two sections of the SIPP questionnaire. When sufficient data are available for these, we could replace them with another set of questions to provide response variance measures for items in another part of the questionnaire. This approach does not attempt to provide the response variance for all estimates at the same time and in a short period. However, it does provide valuable information while still keeping the respondent burden moderate and hence minimizing the risk of increasing nonresponse in subsequent interviews.

Beyond a simple expansion, the reinterview could be used as the vehicle for various experiments.

## 8. Use of Administrative Records

Administrative records could be very useful in increasing understanding in order to improve estimates of gross flows and length of spells. The administrative records could be used at the macro or micro level.

At the macro level, studies similar to validation of food stamp turnover (Judkins, 1986), AFDC turnover (Maher, 1987b) and supplemental security income (Maher, 1987c) would provide information on the quality of additional transition estimates at the macro level. Transition and spell estimates for longer time periods should also be evaluated to assess their quality.

To make the best use of the SIPP, it is extremely important to utilize micro level data. The gross flow estimates suggest problems with the data at the micro level. A micro level match of SIPP data with administrative records has begun at the Census Bureau (Singh, 1986 and Moore, 1986). This study plans to evaluate the SIPP data by matching individual records on reciprocity of nine government transfer programs in four states - Florida, New York, Pennsylvania, and Wisconsin and develop a model of SIPP response and imputation errors in measures of program participation and amount received (Moore, 1986). This is a good step in the right direction, but more efforts are needed to evaluate and develop models for other characteristics and/or other states.

## 9. Special Samples With Known Income Sources

The preceding section discussed getting information on reporting errors through matching of survey data with administrative data. One can also select particularly interesting cases from administrative records to include for evaluation purposes in the SIPP. We might, for example, select some households with multiple reciprocity of income/program sources that occur infrequently, e.g., supplemental security income and unemployment compensation, to explore whether we particularly tend to get reporting errors in such cases. We could also plan special reinterviews for households selected from administrative data when sample and administrative records data disagree. No plans for this type of research have been made.

## 10. Cognitive Research

Cognitive research can be important in a number of areas. Research would be intended to examine cognitive processes of respondents during interviews, to explore outside influences affecting respondent behavior, and to develop improved questions, procedures, etc. Areas of application include coverage problems (especially for Black and

Hispanic males), timing of events (gross flows) and respondent willingness to participate and to consult records.

One way to obtain information is through debriefing of respondents. A debriefing of some respondents after completing all SIPP interviews was done in a reinterview in 1987 (Matchett, 1987). Respondents were asked why they continued to participate and whether they had comments to improve data collection. Analysis is continuing, but some preliminary information is already available. The main reasons for participation are wanting to be sociable, liking the interviewer, and having nothing to hide. Further debriefing should be done, correcting some problems discussed in the initial debriefing, using open-ended instead of fixed response questions and addressing different problems.

#### 11. Basic Coverage Research

As previously discussed, the SIPP and other demographic surveys have much worse coverage than the Decennial Census. One partial explanation is that the Census includes a number of erroneous inclusions, such as duplicates, that are not included in the SIPP. The project here would be to adjust the controls used in forming coverage ratios by excluding the erroneous inclusions. Analysis of such ratios by age-sex-race would improve our knowledge about differences between the SIPP and Census coverage.

Another area of research involves comparisons of survey and Census tabulations. Valentine and Valentine (1971) concluded from a small-scale study on one area that most of the omitted Black males in Census Bureau surveys are household heads. Since the Census has much better coverage than our surveys, the Valentine hypothesis would lead us to expect some significant household composition differences between the Census and our surveys. To examine this, we would compare April 1980 Current Population Survey (CPS) tabulations to Census tabulations. We would use special CPS tabulations that exclude the normal ratio estimation to population control figures.

#### 12. Imputation

Not much is known about the accuracy of SIPP imputation. The imputation may be overcompensating or undercompensating for nonrandom differences, if any, between respondents and non-respondents. Also, the frequency of transitions for imputed cases is much greater than for non-imputed cases for many income sources, suggesting possible deficiencies in the imputation methods (see tables 22 and 23). Also, persons who are nonrespondents because they move to an unknown address appear to have different

characteristics than other non-respondents. Thus, it may be that adding a variable about movers would improve the imputation system. In general, research is needed into how well the imputation system is working.

## B. Research for Improving Estimates

### 1. Reducing Complexity

There are 3 panels in SIPP from February through August and 2 panels from September through January. This makes for a variable workload, resulting in some regional office clerks working only part of the year on SIPP and in difficulties for interviewers. More importantly, each panel has a somewhat different questionnaire, so that interviewers have to deal with up to 3 different questionnaires at a time. This necessitates multiple clerical and supervisory procedures. Training is made more difficult.

If the SIPP questionnaires were short and simple, having 3 versions would be less of a problem. But the basic questionnaire is complex and requires considerable interviewer knowledge in order to administer it correctly. As an example, interviewers must know the difference and distinguish in the interview between a bank certificate of deposit and a statement savings account to collect data of good quality.

It is believed that questionnaire length and complexity, together with having as many as 3 questionnaires simultaneously in use, results in interviewing errors, less probing than desired, and infrequent checking of records for income amounts.

There are several things that would reduce complexity. First, we could redesign SIPP so that only 1 or 2 panels would be interviewed at a time. Four such options have been mentioned. The simplest of these options would have each panel in sample for exactly 3 years and a new panel would be introduced only once every 3 years. Its main disadvantage is that comparisons of estimates would be adversely affected by time-in-sample bias. The other 3 options have new panels introduced at one to two year intervals. They would be less affected by time-in-sample bias, but would have 2 panels being interviewed simultaneously all of or part of the time.

A second way to reduce complexity is to shorten the core questionnaire. A major decrease in length could help substantially. Interviewers would have less to learn and remember, and shorter interviews would be conducive to more probing, more use of records by respondents, and higher response rates. Of course, a major disadvantage is less data and information from the survey.

Another related way to reduce complexity is to reduce the number or/and size of topical modules. This would have the same advantages and disadvantages as would shortening the core questionnaire.

## 2. Improving Field Procedures

Beyond initial training, interviewers are monitored through observation, reinterview, and administrative data. Periodically all interviewers are observed by their supervisor or a Supervisory Field Representative. The interviewer receives positive and negative feedback, as appropriate during the observation, and further action is taken if serious problems are uncovered. Reinterview is used primarily to ensure that interviewers do not fabricate interviews. Interviewers are informed about the reinterview results. Finally, data are kept on productivity and noninterview rates. Appropriate action is taken when there are indications of low productivity or high noninterview rates.

Over the last year or two, significant improvements have been made in the monitoring programs. Through the use of microcomputers and data base systems, historical data on interviews is much more readily accessible to the supervisors. There have been changes towards more positive feedback to interviewers. Previously, somewhat rigid standards for acceptable interviewer performance have been changed to flexible guidelines, with emphasis on supervisors making their own decisions on when an interviewer has a serious performance problem that requires corrective action. However, further improvements are still needed. Supervisors need more training on how to use the data available to them for evaluation and coaching. There is still a need for more communications, especially positive feedback, by supervisors.

## 3. Improving Training

Training is particularly important in SIPP since it is such a complex survey. Holt (1986) has made some specific recommendations for improvements in training that should be pursued. The Bureau is currently evaluating these recommendations for possible implementation.

## 4. Reducing Nonresponse

A gift experiment was conducted on the SIPP 87 panel to see if it reduces nonresponse in SIPP. According to the experiment, a token gift of solar calculators was given to those households who were eligible for interview in April 1987. The complete results of this experiment will not be available until after the panel retires. Three additional ideas are presented below.



First, there can be follow up experimentation to the earlier discussed experiment in which calculators are given to respondents. This would involve different gifts or multiple gifts, or gifts given at different times in the interview cycle.

Secondly, thank you notes handed to respondents at the end of an interview might improve cooperation in future interviews.

Third, providing interviewers and respondents with more information on the survey objectives may be helpful, although some of this is already being done. This would address interviewer observations that some respondents have stopped participation because they don't see a need to answer the same questions over and over again.

#### 5. Dependent Interviewing

Asset and liability questions are asked in the seventh interviews. During a feedback experiment in 1986, some seventh interview respondents were given information on their wave 4 responses. Analysis is still continuing, but preliminary results do not show any evidence of feedback affecting the data (Lamas and McNeil, 1987). Nonetheless, feedback and/or more dependent interviewing may still have potential. For example, Coder (1987c) has suggested that when there is an indicated transition from reciprocity to nonreciprocity at the seam, the respondent could be asked how many months it was since the last receipt of that income source. If the answer is not 4 months, the transition may not really have occurred at the seam. Even a different type of feedback on assets and liability might show improvements. Thus, additional experimentation with dependent interviewing would be worthwhile.

#### 6. Reference Period

Various studies (for example, Kobilarcik, et. al. 1983) have shown that the length of recall affects the data quality. As the length of recall varies, the quality of data varies. A better understanding of the gross flow estimates will help in identifying important estimates with large problems. For these estimates, a shorter reference period would be desirable. On the other hand, a longer reference period could be used for items with small problems. However, consideration to the importance of these items needs to be given in deciding the length of the reference period. One suggestion is to have various (differential) recall lengths for different core questions during the same interview. The topical modules already have differential (mixed) reference periods. The mixed reference period approach has also been used for the Consumer Expenditures Survey.

Another suggestion involves frequent brief telephone interviews interspersed with less frequent full interviews. For example the basic interviewing frequency could be increased from 4 to 6 months (with a reference period also of 6 months). In addition, there could be one or two short telephone interviews between the full interviews. The telephone interviews might only ask whether there have been any changes in reciprocity status or amounts for types of income.

The main potential advantage is that the 2 month recall would result in more accurate transition data and greatly reduce the seam effect. On the other hand, it is unknown whether such a methodology is feasible; there are several potential disadvantages, and details of the methodology have not been determined.

#### 7. Reducing Response Variance

For transitions that have particularly high response variances, specific efforts can be made to reduce the response variance. In particular, attempts can be made to determine improvements in the questionnaire and/or in the data collection procedures. Proposed methods can then be compared with present methods in experiments that use carefully conducted reinterviews to measure the response variances. This type of undertaking has been started for the American Housing Survey (Schwanz, 1986).

#### 8. Improving Transition and Spell Estimates

There is interest in pursuing any procedural, design or questionnaire changes that could lead to improved transition and spell estimates. One such change that could possibly improve estimates of transition from nonreciprocity to reciprocity is to reverse the order in which months of reciprocity are asked. Reciprocity in the most distant month would be asked first and the most recent month last.

Another potentially helpful procedural change is to present respondents with calendars or diaries that they can keep and use to record relevant dates and income amounts.

Changes can be made for programs that have cost-of-living increases at fixed times during the year. For example, food stamp increases occur in July and October. Reports of such increases could be improved by reminding recipients of cost-of-living increases in the appropriate months.

One suspected cause of false transitions at the seams is inconsistent classification of income sources between interviews. For example, in one interview a respondent

may report Aid to Families With Dependent Children (AFDC) income and the next interview General Assistance (GA) income, whereas in reality the income source was unchanged. The inconsistencies could be reduced if recipients were reminded of some characteristics that uniquely identify a particular program (such as color of check, date mailed, or where it is mailed from). Also, a program edit that was developed for the Income and Survey Development Program (ISDP) to reduce misclassification between AFDC and GA income could possibly be used in SIPP. The ISDP edit "corrected" classifications based on respondent reports on monthly payment amount, unit size, state of residence, WIN participation and Medicaid coverage. The weakness to this edit is that actual survey answers are changed, some of which may have been correct.

#### 9. Increasing Respondent Effort

Improvement of respondent effort could improve data. We could stress to respondents that it's important to us to know the exact months of reciprocity, and could ask respondents to make a commitment to answering the questions as well as possible and to think about their answers.

#### 10. CPS Gross Flow Conference Proposals

At the Conference on Gross Flows in Labor Force Statistics organized by the Census Bureau and the Bureau of Labor Statistics, several methods for adjusting for errors in transition estimates were presented. Two papers, Fuller and Chua (1985) and Poterba and Summers (1985) present reasonable and viable adjustment procedures for response error, using reinterview data for estimating response errors. (See also Fuller and Chua, 1986.) Abowd and Zellner (1985) also present a viable procedure which adjusts for missing data (nonresponse in one interview or non-match between interviews) as well as response error. Any of these three procedures could be applied directly to SIPP transition estimates with the availability of estimates of response variance from reinterview or other sources.

The main research required at this point is an in-depth comparison of the three methods, both theoretical and empirical, which might result in one or more new procedures which combine their best features. The goal of such research would be to determine the 'best' adjustment procedure for SIPP transitions. One problem that at least some of the present adjustment procedures have and that needs to be addressed is that adjustment yields negative transitions in some situations. In practice, research is likely to conclude that at least two different adjustment procedures are about equally good. If two or three "best" procedures result in substantially dif-

fering transition estimates from each other, it will be impossible to have much confidence in adjusted transition estimates even if there is a consensus that the adjusted estimates are better than the unadjusted.

#### 11. Imputing versus Weighting Adjustments

How to handle missing data for longitudinal analysis is an important issue, especially when the sample unit is a noninterview for only some of the interviews. Kalton (1986) discusses various alternatives to deal with such situations. The preliminary evaluation of the missing wave data for wave 8 suggests that, in certain situations, imputation could be used with little affect on gross flow estimates (Huggins, 1987). However, more research should be performed in deciding when and which of the two procedures should be used.

#### 12. Improving Wage and Salary Income

One possible problem contributing to wage and salary income underestimates is that some respondents report take-home pay instead of gross pay (Coder et. al. 1987). One possible improvement may be to ask for both take-home pay and gross pay.

#### 13. Improving Interest Income

SIPP clearly underestimates interest income recipients and amounts. There are several ways to improve the reporting of interest income.

One approach is to use IRS records instead of respondent answers, although this may make subannual estimates impossible. Since interest income data on IRS records is not available by source, this approach has the potential to improve only an estimate of aggregate interest income for federal tax filers. Another approach is to give respondents a notebook in which to record the information. Perhaps the notebook could be made useful for other things as well, and so function as a token reward for cooperation. A third approach is to provide more training to interviewers on the various sources of interest income so that interviewers might more effectively probe. A fourth approach is for respondents to tell us the principal and interest rate for each source of income rather than the amount of interest.

#### 14. Improving Child Care Questions

In the child care topical module, questions are asked about child care arrangements. Among other things, estimates are produced on the number of children, both young and old, who care for themselves after school while their parents work. We have asked about child care arrange-

ments directly. These questions can be very sensitive for parents whose child care arrangements are not very good for young children, and thus such parents may frequently mis-report on our questions. Research on this may lead to better questions and better data.

#### 15. Improving Assets Data

Obtaining accurate information on assets and liabilities is very difficult for all surveys. Assets is an area where many respondents are leery of providing information or are not knowledgeable. It is possible to get at least some assets data from administrative sources by matching on social security number. However, there are major problems of administrative data not being consistent with survey definitions and categories. The work required to be able to use each data source will be substantial. Thus, we may be able to improve assets estimates by substituting administrative data for survey data.

### V. SUMMARY

In this paper we have taken a brief but wide-ranging look at studies that have been carried out to evaluate many aspects of SIPP data quality, and we have proposed additional areas of study aimed at improving and further evaluating data quality. It is not possible to make a general statement about the results of the studies, but we can summarize them for different types of data.

Estimates were classified as belonging to two groups --cross-sectional and gross flow/spell. SIPP cross-sectional estimates of the number of recipients for and amounts received from several government programs by quarter are lower than for administrative sources, but for amounts SIPP's generally higher than for the CPS. However, the number of people receiving and the amounts received for unemployment compensation show a decreasing trend compared to independent sources. Estimates of annual income of various types using the SIPP longitudinal file were comparable for the SIPP and the CPS, but poverty rates are lower for the SIPP and thought to be somewhat closer to the actual because of SIPP's better coverage of transfer program income and shorter recall period.

Estimates of rates of change in table 7 show differences between the SIPP and administrative sources, but only one of them is statistically significant. Comparisons of differences in estimates one year apart of the number of households having certain income sources are statistically significant for 4 out of 5 sources. Further investigation of these differences is needed.

Much work has been done on gross flow estimates because of the observed problem of a large percentage of transitions being reported as occurring between waves. Validation of exit and

start-up rates for food stamps, AFDC, and SSI has produced mixed results for macro level use of the data, suggesting that each benefit source should be individually evaluated. A study of the relationship of demographics, imputation procedures and interview status with this pattern of reporting showed no large-scale results. However some small-scale results indicated that proxy respondents and imputation contribute to overestimates of numbers of transitions between waves. To understand the effect of gross flow patterns on the micro-level analysis, Young (1989) computed correlations between a number of different events and amount change status. Except for correlation of 'marital status' and 'married spouse present' with other characteristics they did not show patterns of distortion in bivariate relationships. However, until more analysis is completed, one should be careful in judging the utility of the data for multivariate analysis at the micro-level.

Nonresponse takes various forms including household, person and item. One serious problem with the SIPP is the number of people who become and remain nonrespondents, approximately 20% of the sample by the eighth interview. A study comparing those who missed the last two waves with those responding in all waves shows many variables related to this nonresponse. Further investigation of this data is being carried out. Item response rates for selected income types are given in table 17 and show lower rates for the SIPP than the CPS.

As this summary indicates, the SIPP data quality compares favorably with other sources in some cases and not so favorably in others. This is not surprising since the SIPP uses such an extensive questionnaire, as well as topical modules, that attempts to collect accurate information for many constituencies. Further studies should be carried out to evaluate variables and error sources that have not yet been treated. In addition, research should be carried out on methods for directly improving the quality of data through better interviewing procedures.

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Table 1. Comparisons of Estimated Numbers of Income Recipients and Estimated Aggregate Income Amounts Received for Selected Income Types: SIPP vs Independently Derived Estimates vs the Current Population Survey

	SIPP as a Percent of the Independent Estimates of Monthly Average Recipients for Selected Income Types by Quarter	SIPP as a Percent of the Independent Estimates of Aggregate Income Amounts Received for Selected Income Types by Quarter	CPS (1983) as a Percent of the Independent Estimate Aggregate Income Amounts Received
<b>Wage and Salary</b>			
3rd Quarter 1983	(x)	95.0	99.0
4th Quarter 1983	---	94.3	
1st Quarter 1984	---	93.2	
2nd Quarter 1984	---	94.4	
3rd Quarter 1984	---	95.2	
4th Quarter 1984	---	94.5	
<b>Federal Supplemental Security</b>			
3rd Quarter 1983	92.0	89.8	84.9
4th Quarter 1983	91.3	93.5	
1st Quarter 1984	94.8	96.4	
2nd Quarter 1984	98.2	97.4	
3rd Quarter 1984	98.3	98.6	
4th Quarter 1984	98.1	99.2	
<b>Social Security</b>			
3rd Quarter 1983	99.2	99.6	91.7
4th Quarter 1983	96.3	100.6	
1st Quarter 1984	97.3	100.5	
2nd Quarter 1984	97.7	101.1	
3rd Quarter 1984	97.5	101.3	
4th Quarter 1984	97.5	101.6	
<b>Aid to Families with Dependent Children 1/</b>			
3rd Quarter 1983	78.5	76.2	76.0
4th Quarter 1983	79.2	78.5	
1st Quarter 1984	84.5	85.3	
2nd Quarter 1984	86.0	86.0	
3rd Quarter 1984	82.0	80.2	
4th Quarter 1984	80.7	78.8	
<b>Food Stamps</b>			
3rd Quarter 1983	89.5	90.1	71.2
4th Quarter 1983	91.1	83.1	
1st Quarter 1984	90.8	85.2	
2nd Quarter 1984	90.5	86.2	
3rd Quarter 1984	90.3	84.6	
4th Quarter 1984	91.7	83.6	
<b>Veterans' Compensation or Pension</b>			
3rd Quarter 1983	89.2	78.9	63.3
4th Quarter 1983	89.7	79.9	
1st Quarter 1984	90.6	78.0	
2nd Quarter 1984	90.8	74.5	
3rd Quarter 1984	89.8	76.3	
4th Quarter 1984	93.3	79.7	

1/ The amount excludes dependents covered by payments.

(x) Not Applicable

Table 2. Comparisons of SIPP State Unemployment Compensation with Estimates Derived from Independent Sources

(Monthly Averages for Specified Quarter. Recipients in thousands, aggregates in millions)

Period	SIPP as a Percent of Independent Estimate <sup>1</sup>		CPS 1983 Estimates as a Percent of the Independent Estimates Aggregate Income Amounts Received
	Recipients	Aggregate Amount Dollars	
1983			
Third Quarter	100.9	102.2	75.5
Fourth Quarter	103.4	106.8	
1984			
First Quarter	82.6	85.2	
Second Quarter	82.5	83.1	
Third Quarter	78.5	80.3	
Fourth Quarter	95.1	100.9	
1985			
First Quarter	85.5	94.8	
Second Quarter	77.3	77.7	
Third Quarter	72.8	72.6	
Fourth Quarter	79.1	77.4	

<sup>1</sup>Independent estimates exclude Federal Supplemental Compensation

Source: Coder, J. (1987b)



Table 3. Comparison of Annual Aggregate Income Estimates from the  
March CPS and SIPP 1983-1984 Longitudinal Research File

(In millions of dollars)

Income source	SIPP 1983-1984	March CPS	
		1984	1983
Cash transfers, total.....	216,326	200,620	197,975
Social Security.....	153,958	147,503	138,293
Railroad Retirement.....	5,603	3,973	3,975
Federal SSI.....	8,859	8,444	7,647
Public assistance, total.....	14,643	13,407	12,878
AFDC.....	11,881	10,972	10,523
Other.....	2,762	2,435	2,355
Unemployment Compensation, total.....	14,911	12,169	19,720
State Unemployment Compensation.....	14,060	(NA)	(NA)
Other.....	851	(NA)	(NA)
Veterans' Payments.....	10,978	8,349	8,831
Worker's Compensation, total.....	7,374	6,775	6,631
"State" worker's compensation.....	6,041	(NA)	(NA)
Other Compensation.....	1,333	(NA)	(NA)
Pensions, total.....	92,619	85,448	79,718
Private pensions, total.....	40,319	37,266	34,636
Company or union pensions.....	32,874	(NA)	(NA)
Other private pensions.....	7,445	(NA)	(NA)
Federal pensions.....	19,593	17,154	17,720
Military pensions.....	15,556	15,328	14,095
State and local pensions, total.....	17,151	15,700	13,267
State.....	12,201	(NA)	(NA)
Local.....	4,950	(NA)	(NA)
Interest income.....	115,687	138,661	118,800
Dividends.....	38,251	30,657	27,286
Rents and royalties.....	16,834	17,725	16,483
Estates and trusts.....	5,085	7,835	6,666
All other income, total.....	36,720	30,487	27,258
State SSI.....	101	(NA)	(NA)
Foster child care.....	207	(NA)	(NA)
Child support and alimony.....	8,551	9,401	8,323
Income from charity.....	58	(NA)	(NA)
Money from friends or relatives.....	6,441	4,757	5,358
Income from roomers or boarders.....	165	(NA)	(NA)
Financial investments.....	16,389	(NA)	(NA)
Other income not included elsewhere...	4,808	16,329	13,577
Food Stamps.....	9,267	7,555	7,471

NA Not available.

Source: Coder (1986b)

Table 4. Comparison of SIPP and March CPS Estimates of Persons Ever Receiving Benefits from Selected Programs

Selected income sources	SIPP 1983-1984	CPS	
		1984	1983
Social Security.....	34,122	32,182	31,731
Federal SSI.....	3,941	3,568	3,442
State Unemployment Compensation <sup>1</sup> .....	9,082	7,693	10,109
Veterans' Payments <sup>2</sup> .....	3,790	2,865	3,156
AFDC.....	3,987	3,561	3,468
Worker's Compensation.....	2,329	2,478	2,382
Private pensions.....	8,499	7,951	7,618
Federal pensions.....	1,937	1,555	1,609
Military pensions.....	1,297	1,493	1,337
Interest income.....	123,135	99,045	99,005
Dividends.....	26,807	19,858	18,690
Rents and royalties <sup>3</sup> .....	14,040	12,461	11,836
Estates and trusts.....	521	1,384	1,239

<sup>1</sup> CPS estimates may include a small number of persons receiving other types of "unemployment" benefits but no State unemployment compensation.

<sup>2</sup> CPS estimates include G.I./VEAP beneficiaries who do not receive cash veterans payments. The SIPP figure excludes this group.

<sup>3</sup> The SIPP estimates excludes persons receiving royalties but not rental income.

Source: Coder (1986b)

Table 5. Comparison of Cross-Sectionally Derived Quarterly Estimates with Fourth Quarter 1983 Estimates Derived from the Longitudinal Research File

(Recipients in thousands. Monthly averages)

Selected income sources	SIPP Cross-sectional estimates				SIPP Q4-83 estimates based on longitudinal file
	Q4-83	Q1-84	Q2-84	Q3-84	
<b>Recipients</b>					
Social Security	31,854	32,370	32,432	32,376	31,924
Federal SSI	3,216	3,362	3,492	3,549	3,346
State Unemployment Compensation	2,878	2,982	2,212	1,927	3,013
Veterans' Payments	3,568	3,546	3,503	3,435	3,527
AFDC	2,894	3,129	3,171	2,973	3,065
Food Stamps	6,746	6,917	6,775	6,416	6,916
<b>Median Amount</b>					
Social Security	\$385	\$398	\$402	\$402	\$385
Federal SSI	209	211	208	206	214
State Unemployment Compensation	400	396	379	361	364
Veteran's Payments	131	126	124	125	120
AFDC	285	289	293	287	285
Food Stamps	99	101	99	96	99
<b>Mean Amount</b>					
Social Security	\$395	\$405	\$409	\$411	\$396
Federal SSI	216	221	218	218	217
State Unemployment Compensation	414	405	406	395	410
Veterans' Payments	235	229	226	232	236
AFDC	314	316	318	319	319
Food Stamps	111	113	113	111	111

Source: Coder (1986b)

Table 6. Comparison of Mean Annual Income Amounts from the March CPS and SIPP 1983-1984 Longitudinal Research File

Income Source	SIPP 1983-1984	March CPS	
		1984	1983
Social Security	\$ 4,512	\$ 4,583	\$ 4,358
Railroad Retirement	6,448	6,190	6,098
Federal SSI	2,248	2,366	2,221
AFDC	2,980	3,072	3,034
Federal Pensions	10,115	11,032	11,013
Military Pensions	11,586	10,267	10,538
Dividends	1,427	1,543	1,459
Estates and Trusts	9,709	5,660	5,379
Food Stamps	954	1,070	1,042

Note: This limited list of income types includes only those for which directly comparable mean income could be derived given the data available at the time of preparation.

Source: Coder(1986b)

Table 7. Rates of Change in the Number of Program Participants from SIPP and Independent Sources

Comparison*	Characteristic	Estimates from		Difference
		SIPP (84-83)/83	Other Source* (84-83)/83	
1	Social Security	.029	.010(A)	.019
2	SSI	.096	.028(A)	.068
3	AFDC	-.018	-.013(A)	-.005
4	Food Stamps	-.050	-.047(A)	-.003
5	Average house- hold income	-.066	.081(C)	-.015
6	Average monthly earnings of married, spouse present, male working fulltime	-.036	.033(C)	-.069**

\*"A" stands for the administrative record and "C" stands for CPS.

\*\*Stands for significant difference.

Source: Kim, J. (1985)

Table 8. Differences of SIPP Estimates Between 1983 and 1984 3rd Quarters

Characteristics	NUMBER OF HOUSEHOLDS		(3) Difference (2) - (1)
	(1) 1983	(2) 1984	
Interest generating Assets	57,170,000 (30,278)**	56,249,000 (372,632)	-921,000*
Cash Dividends	11,317,000 (258,430)	10,506,000 (250,301)	-811,000*
Rental Income	6,457,000 (201,253)	5,981,000 (194,254)	-476,000*
Income from Mortgage	2,185,000 (120,079)	2,213,000 (194,254)	28,000
Other Type of Financial Asset	2,319,000 (123,610)	2,019,000 (115,538)	-300,000*

\*\* The number in the parentheses is the standard deviation of the number just above it.

\* Indicates that the calculated test statistic is significant at the 5-percent significance level.

Source: Kim, J. (1985)

Table 9. SIPP Asset and Liability Estimates Compared to Federal Reserve Board Balance Sheet Data for the Household Sector: 1984

(Number in billions except for median networth)

Category	FRB balance sheet	SIPP	Ratio of SIPP to FRB balance sheet
A. Equity in owner-occupied housing	\$ 2,174.2	\$2,823.6	1.30
Gross value	3,482.7	3,958.2	1.14
Debt	1,308.5	1,134.6	0.87
B. Equity in motor vehicles	287.0	410.5	1.43
Gross value	459.6	558.8	1.22
Debt	172.6	148.3	0.86
C. Equity in noncorporate business	2,229.7	1,680.2	0.75
D. Financial assets	3,812.0	2,826.1	0.74
1. Interest-earning assets <sup>1</sup>	3,195.2	1,635.7	0.51
2. Corporate equities <sup>2</sup>	1,456.7	1,062.7	0.73
3. Other financial assets <sup>3</sup>	160.4	127.8	0.80
4. Less: Financial assets held by nonprofit sector or in personal trusts	(840.0)	X	X
E. Installment and other consumer debt <sup>4</sup>	379.9	241.5	0.64
F. Net Worth (A+B+C+D-E)	8,122.9	7,498.8	0.92
G. Median Networth	30,550.0	32,670.0	1.07

NA Separate estimates not available.

X Not Applicable.

<sup>1</sup> Includes passbook savings accounts, money market deposit accounts, certificates of deposit, checking accounts, money market funds, U.S. Government securities, municipal or corporate bonds, saving bonds, IRA and KEOGH accounts, and other interest-earning assets.

<sup>2</sup> Includes equities in stocks, mutual fund shares, and incorporated self-employed businesses or professions.

<sup>3</sup> Includes mortgages held by sellers and other financial assets not otherwise specified.

<sup>4</sup> Excludes debt for automobile and mobile homes.

Source: U.S. Bureau of the Census, Current Population Reports, Series P-70, No. 7, 1986

Table 10. Median Wage and Salary Income in 1984 From the WAVE 6 Topical Module

(Based on unweighted observations)

Record usage and respondent type	Total	Men	Women
<u>Used W-2 Form</u>			
Total.....	15,222	20,990	10,825
Self.....	14,422	17,967	11,255
Proxy.....	17,897	21,031	7,107
<u>Did not use W-2 Form</u>			
Total.....	11,515	15,963	8,542
Self.....	11,168	16,009	8,897
Proxy.....	12,273	15,896	6,632

Source: Coder, J. (1987d)



Table 11. Time-in-Sample by Rotation Covering a Reference Month for SIPP 1984 Panel

Reference Month	Rotation Group			
	1	2	3	4
November "83	2	2	1	1
December "83	2	2	2	1
January "84	2	2	2	2
February "84	3	2	2	2
March "84	3	3	2	2
April "84	3	3	3	2
May "84	3	3	3	3
June "84	4	3	3	3

Note: The numbers in the table indicate the Time-in-Sample. For example 2 means the second time interviewed.

Table 12. Month-to-Month Changes in Reciprocity and Amounts of Food Stamps for Fully-Interviewed Persons Age 15 Years and Older

TYPE OF CHANGE	-----MONTH-TO MONTH CHANGES-----										
	1ST TO 2ND	2ND TO 3RD	3RD TO 4TH	4TH TO 5TH	5TH TO 6TH	6TH TO 7TH	7TH TO 8TH	8TH TO 9TH	9TH TO 10TH	10TH TO 11TH	11TH TO 12TH
TOTAL WITH INCOME IN AT LEAST ONE MONTH..	1927	1927	1927	1927	1927	1927	1927	1927	1927	1927	1927
RECEIVED INCOME IN BOTH MONTHS.....	1287	1306	1325	1211	1334	1341	1351	1224	1327	1326	1329
AMOUNT DECREASED BY 75.0 TO 99.0 PERCENT..	4	3	7	12	0	3	6	6	2	4	4
AMOUNT DECREASED BY 50.0 TO 74.9 PERCENT..	6	7	10	31	5	9	10	36	1	7	12
AMOUNT DECREASED BY 25.0 TO 49.9 PERCENT..	12	24	17	56	10	21	22	68	12	13	10
AMOUNT DECREASED BY 10.0 TO 24.9 PERCENT..	12	22	22	89	15	22	24	123	16	20	21
AMOUNT DECREASED BY 5.0 TO 9.9 PERCENT....	12	9	14	58	5	7	13	69	8	7	16
AMOUNT DECREASED BY LESS THAN 5.0 PERCENT.	9	9	9	103	9	13	18	90	2	6	6
AMOUNT DID NOT CHANGE.....	1166	1131	1108	444	1241	1194	1176	505	1262	1207	1190
AMOUNT INCREASED BY LESS THAN 5.0 PERCENT.	0	34	38	149	7	18	16	84	4	11	7
AMOUNT INCREASED BY 5.0 TO 9.0 PERCENT....	6	16	23	64	7	11	8	34	2	7	10
AMOUNT INCREASED BY 10.0 TO 24.9 PERCENT..	17	11	28	76	9	7	20	63	5	19	14
AMOUNT INCREASED BY 25.0 TO 49.9 PERCENT..	6	12	16	49	11	16	16	46	6	10	13
AMOUNT INCREASED BY 50.0 TO 74.9 PERCENT..	11	9	6	31	5	8	8	35	1	6	9
AMOUNT INCREASED BY 75.0 TO 99.9 PERCENT..	5	6	5	10	3	2	5	20	8	3	1
AMOUNT INCREASED BY 100.0 PERCENT OR MORE.	13	13	22	47	7	10	9	45	6	6	16
FROM POSITIVE AMOUNT TO LOSS.....	0	0	0	0	0	0	0	0	0	0	0
FROM LOSS TO POSITIVE AMOUNT.....	0	0	0	0	0	0	0	0	0	0	0
LOSS BOTH MONTHS.....	0	0	0	0	0	0	0	0	0	0	0
FROM RECEIVING TO NOT RECEIVING INCOME.....	44	48	43	177	25	42	45	180	36	39	33
FROM NOT RECEIVING TO RECEIVING INCOME.....	67	62	63	148	49	55	53	139	38	36	45
DID NOT RECEIVE INCOME BOTH MONTHS.....	529	511	496	391	519	489	478	384	526	526	520

Source: Coder (1986a)

**Table 13. Start-up and Exit Rates (Percentages) for Food Stamp Participation**

SIPP 84 Panel-Reference Month *i* to *i*+1 Across All Four Rotations

	1 to 2	2 to 3	3 to 4	4 to 5	Avg.
Start-up Rate	4.9	4.7	4.5	10.9	6.2
Standard Error <sup>1</sup>	.8	.8	.7	1.1	0.5
Exit Rate	3.3	3.5	3.1	12.8	5.7
Standard Error <sup>2</sup>	.7	.7	.6	1.2	.5

Urban Institute data-Calendar Month *i* to *i*+1 in 1983

	6 to 7	7 to 8	8 to 9	9 to 10	10 to 11	11 to 12	Avg
Start-up Rate	6.7	6.9	6.1	6.2	6.7	5.0	6.3
Standard Error <sup>1</sup>	.6	.6	.5	.5	.6	.5	.3
Exit Rate	7.3	5.8	6.7	7.0	6.1	5.1	6.3
Standard Error <sup>2</sup>	.6	.5	.6	.6	.5	.5	.3

<sup>1</sup> For individual pairs of months, a design effect of 1.8 is assumed. For the average, a design effect of 2.6 is assumed to reflect the correlation between the individual pairs reduced by being in the same set of PSUs. The monthly sample sizes were around 1350. For the average, the sample size is quadrupled.

<sup>2</sup> For individual pairs of months, a design effect of 1.3 is assumed. For the average, a design effect of 2.0 is assumed. The monthly sample sizes were around 2600. For the average, the sample size is to be sextupled.

Source: Judkins (1986)

Table. 14 Start-up and Exit Rates (Percentages) for AFDC Participation

## SIPP 84 Panel-Reference Month i to i+1 Across All Four Rotations

	1-2	2-3	3-4	4-5	5-6	6-7	7-8	8-9	1-5 Avg.	5-9 Avg.	1-9 Avg.
Start-Up Rate	2.9	2.7	2.3	9.6	2.9	2.1	1.6	10.2	4.4	4.2	4.3
Standard Error <sup>1</sup>	.9	.9	.8	1.6	.9	.8	.7	1.6	1.3	1.3	1.5
Exit Rate	1.4	1.9	1.5	8.1	1.0	1.4	2.1	9.9	3.2	3.6	3.4
Standard Error <sup>1</sup>	.6	.7	.7	1.5	.5	.6	.8	1.6	1.2	1.2	1.4

## AFDC Quarterly Averages

	Quarter 3 1983	Quarter 4 1983	Quarter 1 1984	Quarter 2 1984	July-Dec.83 Avg.	Oct.-June(83-84) Avg.	July-June(83-84) Avg.
Start-Up Rate	4.9	4.8	4.5	4.1	4.8	4.5	4.6
Exit Rate	4.7	4.6	4.2	4.8	4.7	4.5	4.6

1. The design effect is assumed to be 1.8 for individual pairs of months, 2.6 for half year averages, and 3.4 for the 12 month averages.

Sources: Coder (1986a), U.S. Department of Health and Human Services (1983, 1984)

Table 15. Start-Up Rates for SSI Participation (Percentages)

SIPP 84 Panel-Reference Month *i* to *i*+1 Across All Four Rotations

	1-2	2-3	3-4	4-5	5-6	6-7	7-8	8-9	1-5 Avg.	5-9 Avg.	1-9 Avg.
Start-Up Rate	1.4	1.2	.9	5.5	1.4	1.6	1.3	6.8	2.3	2.8	2.6
Standard Error	.8	.7	.6	1.4	.7	.8	.7	1.5	.5	.5	.4

SSI Calendar Month *i* to *i*+1

	6-7	7-8	8-9	9-10	10-11	11-12	12-13	13-14	14-15	15-16	16-17	7-12 <sup>2</sup> Avg.	10-17 <sup>2</sup> Avg.	7-17 <sup>2</sup> Avg.
Start-Up Rate	.8	.6	1.1	1.0	.9	.9	.9	1.0	1.0	1.4	1.1	.9	1.0	1.0

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<sup>1</sup>The design effect is assumed to be 2.6.

<sup>2</sup>Months 7-12 correspond to July through December of 1983 and months 13-17 correspond to January through May of 1984.

Sources: Coder (1986a), U.S. Department of Health and Human Services (1984, 1985)

Table 16. Responses for Interviews Two Through Five as a Percentage of Initially Responding Persons for 1984 SIPP Panel, NMCUES<sup>1</sup>, and PSID.

INTERVIEW	% of Response		
	NMCUES	SIPP	PSID
(Base)	(16902)	(25138)	(18387) <sup>2</sup>
Second	99.5	94.4	86.6
Third	97.9	89.7	83.5
Fourth	97.1	85.9	81.5
Fifth	96.5	83.2	79.3

<sup>1</sup> Percentages for NMCUES include ineligible individuals, and are based on all persons in initially responding, reporting units.

<sup>2</sup> 1979b persons are described in most recent releases of the PSID data. An adjustment to this number was made to make it more compatible with the SIPP.

Sources: Cox, B. and S. Cohen (1985); Short, K. and E. McArthur (1986); Beckett, S., W. Gould, L. Lillard, and F. Welch, (1983)

Table 17. Overall Item Response Rate for CPS and SIPP 1985  
Calendar Year Estimates<sup>1</sup>

Income Types	SIPP	CPS
Wage or Salary	76.1%	78.8%
Self-Employment Income	68.9	73.7%
Federal Supplemental Security Income	75.5%	78.8%
Social Security Income	72.7%	76.2%
Aid to Families with Dependent Children	77.1%	80.8%
Unemployment Compensation	72.6%	76.8%
Company or Union Pensions	70.8%	74.6%
Food Stamp Allotment	77.1%	83.9%
Veterans Compensation or Pensions	72.4%	76.7%

<sup>1</sup> Calendar Year item response rates are for estimates based on monthly averages.

Source: Maher, S. (1987a)

Table 18. Between Wave Transitions for Food Stamps

## Sex x Interview Status

Sex	Interview State	Response Response	Response Nonresponse	Nonresponse Response	Nonresponse Nonresponse
Male	Self/Self	54.5 (456)	9.4 (79)	6.0 (50)	30.1 (252)
	Self/Proxy	45.7 (106)	12.5 (29)	8.6 (20)	33.2 (77)
	Proxy/Self	38.2 (76)	16.1 (32)	8.0 (16)	37.7 (75)
	Proxy/Proxy	37.7 (171)	12.4 (56)	5.7 (26)	44.2 (200)
Female	Self/Self	65.5 (2326)	6.8 (240)	5.2 (184)	22.6 (802)
	Self/Proxy	53.9 (125)	9.1 (21)	8.5 (20)	28.4 (56)
	Proxy/Self	43.1 (103)	9.2 (22)	9.2 (22)	38.4 (92)
	Proxy/Proxy	55.4 (92)	11.4 (19)	6.6 (11)	26.5 (44)

First entry in each cell is percent of total responses in row.  
Second entry is number of responses in cell.

Table 19. Within Waves Transitions for Food Stamps

## Sex x Interview State

Sex	Interview State	Response Response	Response Nonresponse	Nonresponse Response	Nonresponse Nonresponse
Male	Self/Self	57.3 (1782)	1.5 (47)	2.5 (77)	38.7 (1202)
	Proxy/Proxy	45.7 (939)	2.2 (46)	2.7 (56)	49.3 (1014)
Female	Self/Self	68.1 (7750)	1.7 (198)	2.1 (236)	28.0 (3189)
	Proxy/Proxy	59.8 (714)	1.7 (20)	1.3 (15)	37.3 (445)

First entry in each cell is percent of total responses in row.  
Second entry is number of response in cell.



Table 20. Between Waves Transitions for Food Stamps

## Race x Sex

Race	State	Response Response	Response Nonresponse	Nonresponse Response	Nonresponse Nonresponse
white	male	44.3 (547)	11.8 (146)	6.1 (75)	37.9 (468)
	female	59.7 (1560)	2.2 (205)	2.7 (163)	49.3 (684)
nonwhite	male	54.0 (262)	10.3 (50)	7.6 (37)	28.0 (136)
	female	68.9 (1086)	6.2 (97)	4.7 (74)	20.3 (320)

First entry in each cell is percent of total responses in row.  
Second entry is number of responses in cell.

Table 21. Within Waves Transitions for Food Stamps

## Race x Sex

Race	State	Response Response	Response Nonresponse	Nonresponse Response	Nonresponse Nonresponse
white	male	49.3 (1830)	2.0 (73)	3.1 (116)	45.6 (1695)
	female	64.2 (5031)	2.0 (154)	2.2 (172)	31.6 (2479)
nonwhite	male	61.2 (891)	1.4 (20)	1.6 (23)	35.8 (521)
	female	72.6 (3433)	1.4 (64)	1.7 (79)	24.4 (1155)

First entry in each cell is percent of total responses in row.  
Second entry is number of responses in cell.

Table 22. Distributions of Transitions and Non-Transitions  
Between Waves

<u>Source</u>	<u>Imputes Involved</u>		<u>Imputes Not Involved</u>	
	<u>Trans</u>	<u>Non- Trans</u>	<u>Trans</u>	<u>Non- Trans</u>
Social Security	.039 (88)	.961 (2191)	.027 (499)	.973 (17785)
Veterans Compensation	.045 (11)	.955 (235)	.035 (67)	.965 (1854)
AFDC	.347 (43)	.653 (81)	.113 (257)	.887 (2014)
Food Stamps	.312 (74)	.688 (163)	.135 (773)	.865 (4940)
Child Support	.22 (24)	.78 (85)	.131 (296)	.869 (1966)
Private Pension	.103 (76)	.897 (663)	.049 (218)	.951 (4261)
Supplemental Security Income	.156 (23)	.844 (124)	.057 (128)	.943 (2113)
Unemployment Compensation	.459 (174)	.541 (205)	.192 (1119)	.808 (4720)

Trans = Transitions

Table 23. Distributions of Transitions and Non-Transitions Within Waves

<u>Source</u>	<u>Imputes Involved</u>		<u>Imputes Not Involved</u>	
	<u>Trans</u>	<u>Non-Trans</u>	<u>Trans</u>	<u>Non-Trans</u>
Social Security	.0003 (3)	.9997 (8594)	.008 (590)	.992 (75660)
Veterans Compensation	0 (0)	1.0 (711)	.004 (34)	.996 (8009)
AFDC	.003 (1)	.997 (301)	.030 (286)	.970 (9138)
Food Stamps	.007 (4)	.993 (596)	.043 (997)	.957 (22214)
Child Support	0 (0)	1.0 (335)	.052 (480)	.948 (8776)
Private Pension	0 (0)	1.0 (2130)	.01 (182)	.99 (18694)
Supplemental Security Income	0 (0)	1.0 (326)	.014 (125)	.986 (9121)
Unemployment Compensation	.232 (212)	.768 (701)	.108 (2616)	.892 (21656)

Trans = Transitions

Table 24. SIPP Transition Correlations

	WM	MS	MSP	PPEARNS	FFINC	FFPOV	ESR	CAIDCO	AFDC	FOODST	WELFAR	FDSTA
marital	1	1.000	0.755	0.028	0.071	0.222	0.043	0.076	0.094	0.079	0.019	0.004
status	2	1.000	0.825	0.027	0.144	0.379	0.083	0.291	0.332	0.240	0.039	0.014
MS	3	1.000	0.891	0.023	0.139	0.350	0.086	0.310	0.330	0.205	0.009	0.028
	4	1.000	0.843	0.025	0.145	0.365	0.088	0.338	0.367	0.261	0.063	0.035
married	1	0.755	1.000	0.028	0.074	0.217	0.036	0.052	0.063	0.062	0.025	0.009
spouse	2	0.825	1.000	0.022	0.132	0.282	0.055	0.138	0.148	0.118	0.011	-0.002
present	3	0.891	1.000	0.025	0.129	0.270	0.067	0.187	0.199	0.128	0.011	0.032
MSP	4	0.843	1.000	0.018	0.142	0.294	0.066	0.225	0.253	0.181	0.065	0.043
person	1	0.028	0.028	1.000	0.291	0.032	0.414	0.005	0.010	0.007	0.006	0.017
earnings	2	0.027	0.022	1.000	0.323	0.032	0.532	0.025	0.022	0.030	0.018	0.036
PPEARNS	3	0.023	0.025	1.000	0.321	0.027	0.523	0.014	0.014	0.033	0.011	0.050
	4	0.025	0.018	1.000	0.318	0.041	0.510	0.033	0.027	0.024	0.018	0.024
family	1	0.071	0.074	0.291	1.000	0.141	0.127	0.085	0.091	0.095	0.052	0.026
income	2	0.144	0.132	0.323	1.000	0.175	0.152	0.121	0.127	0.119	0.055	0.047
FFINC	3	0.139	0.129	0.321	1.000	0.167	0.163	0.084	0.120	0.089	0.042	0.039
	4	0.145	0.142	0.318	1.000	0.175	0.152	0.108	0.141	0.095	0.063	0.033
family	1	0.222	0.217	0.032	0.141	1.000	0.115	0.323	0.406	0.299	0.024	0.020
need	2	0.379	0.282	0.032	0.175	1.000	0.082	0.301	0.334	0.248	0.029	0.006
std.	3	0.350	0.270	0.027	0.167	1.000	0.082	0.303	0.313	0.190	-0.002	0.012
FFPOV	4	0.365	0.294	0.041	0.175	1.000	0.099	0.310	0.323	0.230	0.016	0.003
job	1	0.043	0.036	0.414	0.127	0.115	1.000	0.080	0.096	0.093	0.045	0.050
status	2	0.083	0.055	0.532	0.157	0.082	1.000	0.090	0.100	0.081	0.026	0.043
recode	3	0.086	0.067	0.523	0.163	0.082	1.000	0.089	0.091	0.077	0.016	0.054
ESR	4	0.088	0.066	0.510	0.152	0.099	1.000	0.102	0.107	0.079	0.031	0.036
medicaid	1	0.076	0.052	0.005	0.085	0.323	0.080	1.000	0.565	0.366	0.212	0.068
coverage	2	0.291	0.138	0.025	0.121	0.301	0.090	1.000	0.743	0.354	0.269	0.039
CAIDCO	3	0.310	0.187	0.014	0.084	0.303	0.089	1.000	0.590	0.279	0.186	0.037
	4	0.338	0.225	0.033	0.108	0.310	0.106	1.000	0.601	0.367	0.248	0.071
AFDC	1	0.094	0.063	0.010	0.091	0.406	0.096	0.565	1.000	0.408	0.214	0.053
coverage	2	0.332	0.148	0.022	0.127	0.334	0.100	0.743	1.000	0.411	0.291	0.079
AFDC	3	0.330	0.199	0.014	0.120	0.313	0.091	0.590	1.000	0.323	0.319	0.071
	4	0.367	0.252	0.027	0.141	0.323	0.107	0.601	1.000	0.424	0.321	0.091
foodstamp	1	0.079	0.062	0.007	0.095	0.299	0.093	0.366	0.408	1.000	0.080	0.365
coverage	2	0.240	0.118	0.030	0.119	0.248	0.081	0.355	0.411	1.000	0.103	0.419
FOODST	3	0.205	0.128	0.033	0.089	0.190	0.077	0.279	0.323	1.000	0.071	0.458
	4	0.261	0.181	0.024	0.095	0.230	0.079	0.367	0.424	1.000	0.093	0.372
welfare	1	0.019	0.025	0.006	0.052	0.024	0.045	0.212	0.214	0.080	1.000	0.159
income	2	0.039	0.011	0.018	0.055	0.029	0.026	0.269	0.291	0.103	1.000	0.182
WELFAR	3	0.009	0.011	0.011	0.042	-0.002	0.016	0.186	0.319	0.071	1.000	0.163
	4	0.063	0.065	0.018	0.063	0.016	0.031	0.248	0.321	0.093	1.000	0.148
foodstamp	1	0.004	0.009	0.017	0.026	0.020	0.050	0.068	0.053	0.365	0.159	1.000
allotment	2	0.014	-0.002	0.036	0.046	0.006	0.043	0.039	0.079	0.419	0.182	1.000
FDSTA	3	0.028	0.032	0.050	0.039	0.012	0.054	0.037	0.071	0.458	0.163	1.000
	4	0.035	0.043	0.024	0.033	0.003	0.036	0.071	0.091	0.372	0.148	1.000

Months 5-32, Full Panel Research File: Observations fully interviewed

Source: Young (1989)

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