THE SURVEY OF INCOME AND
PROGRAM PARTICIPATION

RESEARCH AND EVALUATION CONDUCTED ON THE SURVEY OF INCOME AND PROGRAM PARTICIPATION

No. 89

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U.S. DEPARTMENT OF COMMERCE

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ABSTRACT

Because the Survey of Income and Participation (SIPP) is relatively new, research and evaluation to identify problems and develop improvements in methods and procedures is of utmost importance. The research will provide a wealth of valuable information for the SIPP and to other methodologists designing longitudinal and cross-sectional surveys. The paper discusses six experiments and five research and evaluation projects designed to evaluate or identify ways to improve the quality of the SIPP data, gather information about a specific problem so that further research or experimentation would be more focused, and explore new procedures.

KEYWORDS

quality improvement, methods, procedures, experiments

I. INTRODUCTION

The Survey of Income and Program Participation (SIPP), a national household survey of the noninstitutionalized United States population, is designed to provide information on income and government program participation. Person and family characteristics that may influence income and program participation are also available from the SIPP. The survey collects data which helps explain the socio-economic process in the U.S. and aids federal agencies in formulating and evaluating policies and programs in the areas of income and social welfare.

The SIPP offers a unique opportunity to researchers for testing new survey methodology and procedures. The longitudinal methodology of the survey is new and more complicated, compared to standard cross-sectional survey designs conducted in the public and private domains. As a result, research to identify problems and develop improvements in methods and procedures is and has been of the utmost importance. In addition to benefits to the SIPP, results from experiments and research and the development of new methodologies will be useful to survey methodologists in designing/altering other surveys, both cross-sectional and longitudinal.

Several experiments have been conducted during the normal course of the SIPP interviewing to test alternative procedures of data collection and estimation. These experiments were designed either to identify ways to improve the quality of the SIPP data or to gather information about a specific problem so that further research or experimentation would be more focused. Results from six of these experiments, including asset and liability feedback, telephone interviewing, gift to respondent, employer-provided benefits, debriefing of respondents, and missing wave data, will be discussed in Sections II through VII. Some variation of these experiments has been carried out by other survey organizations.

Additionally, the SIPP has initiated many research and evaluation projects on an ongoing basis to evaluate or improve quality of the SIPP data. Results from these projects could have a great impact on the direction of the survey in the next few years and on survey methods in general. Five of these projects will be discussed in Sections VIII through XII. They include evaluation of transitions and spells, record check study, time-in-sample analysis, use of administrative data in longitudinal weighting, and noninterview adjustment research.

Because little time was available to test and analyze the methodology of SIPP's predecessor, the Income Survey Development Program, before implementation of the SIPP, much was unknown about the methodology and procedures used for the SIPP. As a result, the SIPP is especially sensitive to identifying and reducing problems in methodology and procedures at this time since much of it is in the first stages of implementation. The SIPP is taking the lead in

areas of research to improve data quality that will provide a wealth of valuable information to other methodologists designing longitudinal and cross-sectional surveys. The applicability of the SIPP experiments and evaluation to other surveys is discussed in Section XIII.

It will be helpful to present some background information on the design of the SIPP before discussing the experiments and other research. For the SIPP a new sample (or panel) of households is introduced each year. Each eligible person in the sample is interviewed once every four months for about 2½ years (generally, eight interviews). To even out field and processing work loads, households in sample are divided into four subsamples of nearly equal size called rotation groups, and one rotation group is interviewed each month. Thus, four months are required to complete one interview of the entire sample. The reference period of a questionnaire for a given rotation is the four month period prior to the interview month. Thus, a household interviewed in August would be asked questions relating to April through July and would not be interviewed again until December. Generally the four months required to complete each interview of the entire sample are referred to as a wave.

II. ASSET AND LIABILITY FEEDBACK EXPERIMENT

During Wave 4 of the 1984 panel, questions on types of assets and liabilities held during the previous year and the amount of each were administered to respondents. New assets and liabilities were identified and amounts updated one year later at Wave 7. For all the SIPP panels some of these questions are repeated yearly.

This yearly collection of asset and liability information makes it possible to estimate annual changes in asset and liability equity. However, as a result of response errors and variances it is possible, for example, for the same individual to have an underestimate of an asset value in the first interview and an overestimate in the second interview, resulting in an overestimate of the change in the asset value. Thus, it is important that the data be collected in a manner that will make the computation of yearly differences of amounts for individuals as accurate as possible. One potential way to enhance accuracy is to provide each respondent information about values reported in the previous year.

In Wave 7 of the 1984 panel a test of this methodology was implemented. Persons in half of the sample households were eligible to be reminded of the amounts of individually and jointly held assets reported in Wave 4. A computer listing of amounts (i.e. feedback form) was generated for each of these persons who were interviewed at Wave 4. During the Wave 7 interview, to assure confidentiality, the feedback form was given only to self respondents and to proxy respondents when the Wave 4 and Wave 7 proxies were the same. When a question in the Wave 7 interview corresponded to one of the items on the feedback form, the respondent was referred to the entry on the form and was asked to take this amount into consideration to answer the question for the Wave 7 reference period. The year-to-year changes for the "feedback" group were then compared to changes in the "nonfeedback" group. (Carmody, et al, 1988; Weidman, et al, 1988; and Lamas and McNeil, 1987).

Results of this experiment give no statistical evidence of consistent differences in the measure of annual changes in asset and liability amounts due to the use of the feedback procedure. It was expected that mean annual changes and correlations between responses provided at Waves 4 and 7 would be lower and higher respectively for the "feedback" than the "nonfeedback" group. However, table 1 shows no strong statistical increase in correlations for the whole population or for subpopulation 2 (i.e. households with maximum Wave 4 or Wave 7 assets or debts less than $200,000) through the use of feedback. For subpopulation 1 (i.e. households with maximum Wave 4 or Wave 7 assets or debts less than $50,000), the consistency of mostly nonsignificantly higher nonfeedback correlations suggests no effect of feedback in this subpopulation. Table 2 shows no strong statistical decrease in the mean annual changes for the whole population and each of the subpopulations for the feedback group. ($\alpha = .10$). (Weidman, et al, 1988).

Because a substantial portion of the asset and liability questions asked twice in the 1984 panel are asked only once in later panels, the feedback procedure is not currently being pursued for later panels. However, if it were pursued again, the approach would be modified.

Other survey methodologists interested in pursuing this approach should keep in mind that a possible explanation for these nonsignificant results is that feedback of amounts could have a relatively large effect on the reported percentage amounts of gradual changes such as interest accumulation or depreciation, but a relatively small effect on the lump sum changes via purchases or transfer between sources. If, as is probably the case, most mean annual differences are dominated by the lump sum changes, the effect of feedback on the reporting of gradual changes will be masked. For the asset and liability types which are identified and reported on after the feedback form is made available to the respondent, the feedback of amounts could prevent erroneous lump sum changes from being reported. However, to verify such an hypothesis one would need to track actual transfers, purchases, etc., via administrative records. (Weidman, et al, 1988) Of course, it is also possible that feedback dampens true change. Again this could only be determined through a match to administrative records.

III. TELEPHONE EXPERIMENT

When the SIPP was implemented in October 1983, it was believed that telephone interviewing was not feasible since the questionnaire is complex and requests data which are sensitive. However, in a continuing effort to make the SIPP design more efficient, a study was designed to assess whether maximum telephone interviewing (interviewing by telephone unless circumstances prevent it) could be used in place of maximum personal visit interviewing while maintaining data quality.

The test was conducted in three parts. In June 1985, the feasibility of doing a telephone experiment was tested using 279 households from two of the Bureau's regional offices. (The households had been dropped from the regular 1984 panel sample due to budget constraints.) The experiment was then carried out in two phases. In the first phase, half of the national sample at the second or third interview of the 1986 panel was designated to be interviewed by telephone. Data quality of the telephone and non-telephone groups was to be compared. Secondly, costs were to be evaluated. In the second phase, some interviews designated for telephone in the first phase were to be interviewed by telephone again. As a result, one-half of all phase I telephone designated cases were designated to be interviewed by telephone at interviews 3 and 4, one-fourth at interviews 2 and 4, and one-fourth at interview two only. This phase was designed to (1) determine how well households would respond to telephone interviews in two consecutive interviews and (2) evaluate differences in costs between the two interview modes. (Gbur and Durant, 1987; Carmody, et al, 1988).

Interviewers comments were solicited after the pretest, as well as after the first phase. Following the feasibility test, most interviewers indicated that telephone interviewing worked as well as personal visit interviewing. If they were able to make contact with the household, they were usually successful in obtaining the interview. Interviewers who participated in the first phase were generally positive about telephone interviewing. While 74.7% felt it could be successful in the SIPP, 29.3% believed it would not work due to the sensitive nature of the questions and because respondent trust is difficult to build and maintain over the telephone (the two percentages sum to over 100 because some interviewers gave reasons for and against the use of the telephone.) Additionally, 83% of interviewers believed that household size should be a deciding factor in whether to use the telephone since interviews are conducted with all eligible persons age 15 or more. A majority indicated that two persons is the maximum that can be interviewed during one sitting.

Currently, some persons designated for personal interview are interviewed by telephone. If the SIPP were to move to maximum telephone interviewing, some persons would by necessity be interviewed in person. Thus, to compare maximum telephone interviewing to the current methodology, personal visit and telephone designated groups are compared rather than actual personal visit and telephone groups. In the first phase, 14 percent of personal visit designated cases were telephone interviewed. A maximum of 46 percent of telephone designated cases were interviewed in person.

To obtain an early indication of whether data quality would be adversely affected by using maximum telephone interviewing, unweighted household, person, and item nonresponse rates, and self vs. proxy interview rates for the telephone and personal visit modes were compared from the first phase. All

hypothesis tests were conducted using $\alpha = .10$. When weighted data from the first phase become available, comparisons of estimates from the two modes will be made. Additionally, analysis of the second phase will be completed when the data are processed.

From the early analysis it appears that the use of maximum telephone interviewing after one or two personal visits can be done in the SIPP with little effect on household nonresponse rates, although there may be an adverse affect on item nonresponse rates and self vs. proxy interview rates. It was thought that refusal would be easier over the telephone, leading to higher nonresponse rates. However, neither the household refusal rate nor the household nonresponse rate (including no one home, temporarily absent, refusals, unable to locate) for the telephone group (3.8% and 7.3%, respectively) were significantly different from the corresponding rates for the personal visit groups (4.1% and 8.2%, respectively). Additionally, the person nonresponse rate within interviewed households was not significantly different.

It was also expected that asking sensitive questions over the telephone could increase item nonresponse rates. For 32 income and labor force items, no significant differences were found. However, for 29 of these, the nonresponse rates were numerically higher in the telephone group, raising the possibility that use of the telephone increases item nonresponse rates. Additionally, the telephone group had a significantly higher percentage of proxy respondents than the personal visit group (37.8% vs. 35.3%). This may partially explain the possible higher item nonresponse rates.

Until analysis is carried out on the SIPP estimates, affect on data quality can only be speculative. Although the differences in item nonresponse and self vs. proxy response rates are small, the findings suggest the potential for data quality of estimates to be somewhat adversely affected.

Although primary concern was on the issue of data quality, there was some interest in considering the impact of maximum telephone interviewing on costs. Because little additional burden could be placed on interviewers, data necessary for an appropriate cost analysis could not be collected. As a result, the cost analysis for the first phase could not provide conclusive results. Results from the second phase suggested that telephone interviewing may save time and reduce cost. However, the increased workloads during this phase may be at least partially responsible for this finding. (Gbur, 1987).

The early results agree with findings by Parmer, Huang, and Schwanz (1987) in a study using American Housing Survey (AHS) data. They found that item nonresponse for income items may be somewhat higher with telephone interviewing. Whether the differences are large enough to have a measurable impact on the SIPP estimates is yet to be investigated. No measurable affects on AHS published estimates were found. After results of the SIPP analyses become available, a decision will be made about whether more research should be done, whether to use maximum telephone interviewing, or whether the current interviewing rules can be relaxed to allow more use of the telephone.

IV. GIFT EXPERIMENT

The SIPP sample persons are followed and interviewed every four months over a two and one half year period. During this time sample loss occurs as some households for various reasons no longer participate in the survey or move and cannot be followed. Because this nonresponse increases as the SIPP panel ages and is over 20% by the end of a panel, bias and variance of estimates increase as the panel ages and may become substantial. (Petroni and King, 1988). Since household nonresponse is of great concern, attempts to reduce it were initiated and an experiment was conducted to explore whether giving an inexpensive gift as a token of appreciation for participation in the survey will reduce it.

For this experiment, small solar-powered calculators were given at the first interview to households in rotation 4 (April interview) of the 1987 SIPP panel. Following the interviewer's introduction, the calculator was presented to the respondent as a token of the Census Bureau's appreciation for the household's participation in the survey. (Carmody, et al, 1988). Because interviewers' perceptions effect how well a method works, interviewer reactions to the experiment were evaluated, as well as household nonresponse rates.

Interviewers completed an evaluation form for the gift experiment after the interview in which the gift was given. Although many (122) of the 352 interviewers who completed the form thought that giving a calculator was a good idea and that they should be given to future sample households, only 41 (12%) stated that giving it helped to gain cooperation. About 18 percent (65) of the interviewers believed that interviewers' skills had more impact on the cooperation of the respondents. Additionally, some interviewers felt they were "buying the interviews" and that the survey was becoming "too commercial". (Carmody, et al, 1988). About 1½ years after this initial evaluation, interviewers' reactions to the gift experiment were less positive. (Chapman, 1988). However, this reaction may have been influenced by the fact that these feelings were expressed in group meetings of interviewers.

Table 3 shows that at each of the first three interviews of the 1987 panel, national cumulative type A noninterview (household which is eligible to participate in the survey, but does not) rates are significantly lower ($\alpha = .10$) for the group designated to receive the calculators (Rotation 4). However, data from the 1985 and 1986 panels suggest that the differences may at least be partially due to seasonality. (The first interview of rotation 4 occurs in April, while the first interview of rotations 1-3 occurs in May, February, and March, respectively.) Additionally, table 4 shows that the rates for the gift receipt group are lower than those projected from the 1985 and 1986 panels, although they are significantly lower only for the second and third interviews of the 1985 panel. Thus, through the first three interviews a definite conclusion about the effectiveness of the token gift in reducing household nonresponse cannot be made.

Beyond doing a similar analysis for later waves, nonresponse rates for subgroups of the population such as race, metropolitan/nonmetropolitan status, and household size may be analyzed to identify subgroups whose response rates may be improved by token gift receipt.

Currently, there are no plans to pursue the experiment on an upcoming panel because the variance and bias gains are expected to be minimal at the national level. (Gbur, 1988). However, if the remaining analysis indicates that gains in mean square error may be substantial, the decision will be reconsidered. Additionally, the experiment could be narrowed and implemented on subgroups of the population, if, as is suggested by Ferber and Sudman (1974), it is found that reduction in nonresponse for certain subgroups may be substantial by providing a gift.

V. EMPLOYER-PROVIDED BENEFITS STUDY

During the past few years, employer contributions to health insurance plans, retirement plans, and life insurance plans have been the focus of national attention by Congress, other policy makers, and researchers in areas such as health care, the elderly, and tax reform. While the SIPP collects information on respondents' contributions to retirement plans, it does not collect information on employers' contributions. Also, the SIPP determines whether respondents are covered by insurance plans and whether employers pay for the plans, but the survey does not obtain the amounts that respondents and employers pay.

An experiment was conducted during the eighth interview of the 1985 SIPP panel to determine whether (1) respondents will sign a form authorizing the Bureau to contact employers for data and (2) employers will provide the requested data. The objective was to collect amounts paid by the employee and the employer for health insurance, pension plans and life insurance coverage. This information has the potential to enhance the survey data.

Even though the usefulness of collecting employer information appeared obvious, there was concern that the rate of response by employers might be very low, leading to inaccurate estimates. The intent of the experiment then was to obtain an indication of the expected response rate with a full-scale implementation of an employer questionnaire. In addition, experience with operations and field procedures could be gained to better implement an employer questionnaire with the SIPP in the future.

Approximately 500 households were randomly selected for the experiment. A short employer questionnaire (attachment A) was developed to obtain information on employer-provided benefits. This form included a statement to be signed by the SIPP respondents authorizing their employers to provide the

requested information to the Census Bureau. Only those persons in selected households who were at least 18 years old and employed at the time of interview were eligible for the experiment.

The procedure for collecting the data was as follows:

1. After completion of the eighth interview, the interviewer determined (or verified) whether the person was eligible for the experiment.

2. After an explanation of the purpose of the employer questionnaire to eligible respondents, the interviewer obtained authorization signatures, employer addresses, and the contact to which the employer questionnaire could be mailed.

3. The authorized employer questionnaires with cover letters were mailed to employers.

4. Follow-up on employer questionnaires was conducted first by letter and then by telephone with any businesses or firms which did not return a questionnaire.

For those SIPP interviews conducted by telephone or by personal visit but with a proxy respondent, interviewers mailed (or left) the employer questionnaire/authorization form, along with a letter of explanation and an envelope to mail the form to the Census Bureau. However, a follow-up of the respondents who did not return an authorization form that was mailed (or left) or who declined to sign at a personal interview was not conducted.

The following table summarizes the rates of return on signed authorization forms by type of interview.

Type of Interview	Number of Persons Eligible	Number (%) of Signed Forms Returned by Employers
Self	748	312 (41.7)
Proxy/ Telephone	604	233 (38.6)
Total	1,352	545 (40.3)

In addition to the interview nonresponse rates provided in the table above, item nonresponse exists for each question. For example, of the 545 signed and returned authorization/questionnaires, 37 (6.8%) contained no response for employee's contribution to health insurance, 38 (7.0%) contained no response for employer's contribution to a pension plan and 12 (2.2%) contained no response for employer's contribution to life insurance.

From results of the experiment, it was determined that, if the employer provided questionnaire is administered in another panel, the design of the form should be changed to better capture certain information. For instance, a question about whether health insurance coverage is at the individual or family level should be added to the questionnaire since the amount of contribution depends on this classification. Additionally, the size of the firm should be requested and more pre-coded categories should be provided to accommodate time periods of payment other than weekly, monthly or yearly.

Since response to the questionnaire by the SIPP respondents was very low, the most important change in a future implementation of the questionnaire will be to conduct normal SIPP callback procedures on those forms that were to be mailed in by proxy and telephone respondents and on those self respondents who refused to participate. As a result, we estimate , based on Census Bureau field experiences, that the overall rate of return on signed authorization forms could be greater than 50% compared to 40% obtained in the experiment. (Adams, 1988).

At the Census Bureau, the employer provided benefits data are expected to be used primarily for the development of models for predicting the value of

employee fringe benefits based on known characteristics of the employer and employee. Another set of models will be developed to attempt to identify factors which cause higher or lower benefit amounts.

Prior to use of the data for this purpose, the representativeness of the data [those who responded versus those who did not respond] will be examined. Also the effects of possible nonrandomness of responses on the modeling will be evaluated.

The most valuable knowledge gained from the experiment is that employers appear to be very accommodating to inquiries on benefit coverages for their employees. About 96% of the questionnaires sent to employers were completed and returned.

The Medical Provider Survey of the National Medical Care Expenditure Survey also used a signed release form to obtain data. (Cox and Cohen, 1985) Perhaps other surveys will also make use of this avenue for collecting employer information about persons in their surveys. In particular, surveys concerned with total personal income, assets, medical expenditures and its affect on income and retirement plans should investigate the applicability of this procedure to their survey objectives.

VI. DEBRIEFING OF SIPP RESPONDENTS

The debriefing of SIPP respondents was implemented primarily to determine why respondents do or do not refer to their records (such as bank statements, tax forms, etc.) in answering interview questions. The debriefing has also helped to determine reasons for continuing respondent participation in the SIPP, sources of bias and nonsampling errors, and whether respondent comments would suggest ways in which the SIPP questionnaire and data collection procedures could be improved. (Carmody, et al, 1988).

Statistical analyses of the debriefing data were performed using $\alpha = .10$. Differences between percentages that are not significant are noted.

A group of 516 SIPP respondents who completed the eighth interview of the 1985 panel made up the debriefing sample. After each SIPP interview, a small sample of respondents are selected at random for reinterview by supervisors to evaluate the quality of the interviewer's work. As a means of defraying expenses, the reinterview sample for rotation groups 2, 3, and 4 of the eighth interview of the 1985 panel underwent the debriefing.

The questions asked during the debriefing regarded: reasons for continuing with the SIPP; reasons for not using records during interviews; suggestions for making record use easier; clarity of government program, income, and asset questions; learning of other government programs through the SIPP; and overall comments and suggestions. Since the questions were open-ended, the response categories were generally determined after the debriefing took place. Some of the possible response categories, however, were preprinted on the questionnaire, so it is conceivable that the respondents may have been prompted with possible response categories in some cases. However, this was not intended. By looking at the frequencies and types of responses given, we hoped to gain a further understanding of the respondents' reactions to the SIPP program and how nonsampling error might be reduced.

The overall response rate for the debriefing experiment was 89.5 percent. The item nonresponse rate for the majority of the questions was 5 percent or less. However, the person(s) responding to the debriefing may not necessarily be the same person(s) who responded to the SIPP throughout the panel. This is a known problem for reinterviewing in general (Meier, 1988).

The tax and W-2 form record use rate for the SIPP obtained from debriefing data was compared to a similar rate estimated from 1984 panel tax and annual roundup topical module data. The tax and annual roundup module, a set of questions concerning calendar year income and types of tax returns filed, is asked during Waves 6 and 9 of the 1984 panel. The debriefing rate was determined by asking respondents if they had used tax and W-2 forms during the final 1985 panel SIPP interview. In contrast, the tax and annual roundup rate was determined from items that were checked off by the interviewer to indicate whether the respondent was referring to records.

In all, 56.4 percent of the debriefing respondents claimed that they used tax and W-2 forms during collection of data at their final interview. Of the persons responding to questions in the tax and annual roundup module of Wave 6, only 30 percent used W-2 forms and 34 percent of the respondents who filed a federal tax form used the tax form. Thus, the percent using both should be 30 percent or less. With this in mind, the debriefing rate is much higher than would be anticipated. However, if the debriefing respondents interpreted the debriefing question as asking whether they used tax or W-2 forms (as hypothesized), then the debriefing rate of 56.4 percent may be reasonable. In any case, increased record use rates are preferable for the SIPP (Meier, 1988).

The first two reasons given for not using tax and W-2 forms were that records were not available (32.3%), and that respondents did not have or file tax forms or did not work (20.2%). Also, 16.7 percent of respondents felt they knew the information without referring to their records. It is also possible that these first two categories may overlap somewhat (Meier, 1988).

Based on debriefing responses, 60.6 percent of the respondents reported that they routinely referred to records such as bank statements and pay stubs during regular SIPP interviews as compared with the 56.4 percent who said that they used tax or W-2 forms during the final interview (note that these two percentages are not significantly different). Although a large majority of the respondents who did not use records claimed that nothing could be done to encourage them to use records (about 80%), their reasons for not using records were quite varied. The major reason given for not using bank statements and pay stubs during regular interviews was that they knew the information without referring to records (54.1%). This same reason was the third most frequently stated reason for not using tax and W-2 forms during the annual roundup part of the last interview (16.7%) (Meier, 1988).

The two major reasons respondents gave for continuing to participate in the SIPP were that they liked the interviewer(s) (27.8%) and that they felt it was their patriotic duty to participate (22.6%). It was also determined that bias could exist in the SIPP estimates due to SIPP respondents' learning of other government programs through the SIPP and then applying for them. About 2.2 percent of the respondents (almost all of the 462 respondents answered this question) claimed that they had learned of a government program through the SIPP and then applied for it. However, it is not known how many of these applications were accepted. A 90-percent confidence interval for this 2.2 percent is 0.9 percent to 3.5 percent, inclusive. To put this percentage in perspective, suppose that 2.2 percent of all SIPP sample persons were receiving benefits from a government program which they learned of through the SIPP. This number of government program participants would constitute 12 percent of all SIPP sample persons on government programs. Such learning-effects bias may introduce further biases since the skip pattern of the questionnaire and answers to other questions are dependent upon the answers to government program questions (Meier, 1988).

As a final step in the debriefing, respondents were asked if they had any other comments or suggestions about the survey. The most frequently given comment was that the interviewers were of good caliber. It should be noted that the respondents' liking the interviewers was also the major reason that respondents said they continued to participate in the SIPP. The interviewers were said to be accommodating, persistent, cooperative in arranging schedules, friendly, and personable (Meier, 1988).

Since valuable information was learned from the debriefing, it was recommended that further research be done. Some suggestions for increasing record use were to have interviewers call before the interview to remind respondents to gather records, incorporate a statement into the "Dear Friend" letter that is sent to the respondents prior to each interview (not just the two containing extra income and asset questions) asking the respondents to refer to records, or adding a statement to the SIPP questionnaire asking the respondents to retrieve records. A cognitive laboratory approach was also recommended for determining which portions of the SIPP questionnaire are unclear and what might aid the interviewers in eliciting accurate responses. (Meier, 1988).

VII. MISSING WAVE DATA

Since the SIPP is a longitudinal survey, multiple visits are made to the same individual. This provides the opportunity to obtain retrospective data for

any previously missed interviews. This information could be used directly for imputation of missing interviews or to better classify noninterviewed persons in noninterview adjustments made to the data in the weighting procedures.

To understand the loss of missing interviews to SIPP longitudinal estimation, sample persons can be classified into the following three types:

1. Persons who respond to zero interviews of a reference period;

2. Persons who respond to some but not all interviews of a reference period; and

3. Persons who respond to every interview of a reference period.

In longitudinal weighting, a noninterview adjustment is used to correct for persons in the first category. Persons in the second category can be handled by imputation or as noninterviews in longitudinal weighting. (Kalton, 1986)

Currently, in the SIPP, the longitudinal weighting procedure treats persons in categories one and two as noninterviews and inflates the weights of persons in category three to account for them. As a result, none of the data collected for category two persons is directly included in the survey estimates (i.e. a large amount of data is discarded.) For example, approximately 28% of Wave 1 interviewed persons 15 years and older in interviewed households in the 1984 SIPP panel missed one or more interviews. Thus, 28% of the 1984 panel SIPP sample is discarded in computing estimates derived from the panel longitudinal weight. [Note, however, that portions of these data are used for cross-sectional estimation (Hubble, 1984) and longitudinal estimation for shorter time periods such as calendar years (Huggins, 1987).]

The alternative, imputation for person nonresponse, allows full use of all conducted interviews. However, it could be very complicated to implement depending on the patterns of interviews for which imputation would be employed and the imputation methodology selected. Assuming here that imputation based on retrospective data were implemented for persons who never miss consecutive interviews, the data discard rate of 28% in longitudinal estimation would be reduced to approximately 21% for the 1984 SIPP panel. The primary concern with using retrospective data for either imputation or weighting adjustment, however, is that response bias can be introduced or increased since it involves a longer recall period for respondents.

To study the potential for improving longitudinal estimates, a set of questions, titled "Missing Wave", was developed for persons whose response pattern over a set of three interviews was response-nonresponse-response and was introduced into SIPP interviewing starting with the fourth interview of the 1984 panel. The set of questions was asked in an attempt to fill the gap in interviewing. The missing wave section of the SIPP questionnaire contained a skeleton set of SIPP core questions relating to labor force status, receipt of income and assets and program participation.

The objective of the missing wave experiment was to determine the usefulness of retrospective data to longitudinal imputation and/or weighting procedures and whether the quality of and increase in the amount of data collected justified the respondent burden imposed by the additional set of questions.

Based on examination of missing wave data obtained from the last interview of the 1984 panel, the missing wave section was dropped from the SIPP questionnaire. Justification for this action stemmed from the belief that imputation and weighting adjustment for noninterviews based on missing wave data would not likely provide significant improvement over imputation and weighting adjustment procedures that utilize prior and past waves of collected data.

Detecting transitions in receipt of income, assets and government assistance is one of the primary functions of the SIPP. The missing wave questions detected only a small number of changes in receipt of income and assets between interviews. Tables 5 and 6 summarize the number of transitions reported by 512 eligible persons between interviews 7 and 8 and interviews 8 and 9 using missing wave data collected at the eighth interview. Estimates of the number of transitions normally expected for each type are also provided in the tables as benchmarks to assess the quality of reporting. More detailed results and explanation of the procedure to obtain the estimated benchmarks

are found in Huggins (1987a).

Of the persons eligible to respond to the missing wave in the last interview, 38 reported a change in receipt of one income type and one person reported a change in receipt of two income types. For receipt of assets, 68 reported a change in one asset and only one person reported a change in two assets.

Comparing the actual number of transitions seen in tables 5 and 6 to the estimated benchmark, it appears that the quality of missing wave data is questionable. The potential decrease in data quality could result from the longer recall period of the missing wave questions and the burden imposed by the missing wave section. In obtaining the estimated number of transitions, it was assumed that noninterviewed persons are like interviewed persons and that reporting of transitions is not affected by respondents' time in sample, even though it is known that there is a higher level of reporting certain characteristics at the first interview than for subsequent interviews due to respondents time in sample. The latter assumption is necessary because SIPP transition estimates were only available from the early waves of the panel. Even if the assumptions used to derive the estimated number are incorrect, the reported numbers of transitions for receipt of income and assets at the missing wave are small whether they are compared to the estimated benchmarks or not. With the proportionately small number of changes in receipt of income and assets detected by the missing wave form, it is believed that the SIPP core data collected before and after missing interviews can be used to fill gaps in reporting for many items. The number of transitions lost by not using missing wave data for imputation and weighting should be negligible and the respondent burden appears unjustified.

Missing wave data from only the last interview in the 1984 SIPP panel were analyzed. However, it is believed that analysis of missing wave data for other interviews would yield similar results. Missing wave data was collected for up to six interviews in the 1984 panel, up to seven interviews in the 1985 panel, and up to four interviews in the 1986 panel. These data are included in the SIPP data files for further analysis by interested SIPP data users. Other uses for this data may also be identified.

Surveys with a shorter recall period than the SIPP may obtain better retrospective data on transitions. Also, objectives in collecting retrospective data may differ such that it is vital that no gaps in interviewing occur. Overall, implementation of the missing wave questions went smoothly for the SIPP and person response was fairly good (94% of eligible respondents answered at least one question). Therefore, at least in an operational sense, other surveys can feel encouraged to try to collect retrospective data if it is vital in meeting their objectives.

VIII. EVALUATION OF TRANSITIONS AND SPELLS

Two longitudinal estimates from the SIPP could serve as powerful tools in explaining socioeconomic processes. These are gross flow estimates (i.e., transition from one state of economic or labor condition to another state) and the distribution of the length of spells (i.e., time with a given condition such as time unemployed or time receiving food stamps). However, there is evidence that problems may exist with these estimates as it does for other surveys such as the Current Population Survey (Abowd and Zellner, 1985) and the Panel Study of Income Dynamics (Hill, 1987).

Coder (1986) shows that month-to-month intrawave (within one interview) transitions are dramatically understated and month-to-month interwave (between interview) transitions are overstated for food stamps, railroad retirement, child support payments, state unemployment compensation, etc. This reporting pattern could have significant adverse affects on analysis of spell lengths, and if it distorts the covariance structure, on multivariate analysis of spells and transitions. (Recent evidence by Young (1989) suggests that at least for program participation variables at the national level, the correlational structure may not be distorted.)

Maher (1987) showed that transitions rates for food stamps calculated from pairs of months from all four SIPP rotations are not significantly different from those obtained from administrative records. This suggests that, at least for food stamps, macro level estimates of transitions for a given month will be satisfactory. Also the distribution of the length of spells at the macro

level will be acceptable. Furthermore, if the time-in-sample effect is small (compared to estimates), the estimate of change in number (or rate) of transitions and in length of spells would also be satisfactory (Singh, et al, 1986). (The study by Maher does not indicate that macro level transitions are a problem for Aid to Families with Dependent Children and no conclusions could be made about Federal Supplemental Security Income.)

Since many factors contribute to problems with these estimates, the SIPP staff has begun an intensive effort to improve transition and spell estimates via improved data collection instruments, procedures, and methodology. Short term efforts include introducing procedural changes and evaluation using currently or soon to be available data to gain further insights into areas where improvements could be made. Long term efforts include cognitive research and the designing and testing of a time line data collection instrument (Kasprzyk, 1988).

A. Changes

Recent changes have been made in interviewer training, emphasis on data quality, data collection methodology, and the questionnaire. In January 1989 a section describing the problem and its impact was included in the interviewer training package. Additionally, interviewers' comments and suggestions on the problem were elicited. Data quality was emphasized more by stressing its importance in the January training. In the future it will also be stressed in the introductory letter to respondents and throughout the questionnaire.

In February, 1989, the calendar as a data collection aid (see attachment B) was implemented in one regional office. At each interview data is recorded on the form and is provided to the respondent at the subsequent interview to serve as a reminder of what was previously reported. It is too early to evaluate the impact of these changes and interviewer comments.

Finally, two changes were made to the 1988 panel questionnaire in an attempt to reduce the interwave transitions (i.e., seam transitions). First, if a person received a given income at the last interview and reported not receiving it for the current reference period, the person is now asked to report the last month it was received to verify that he or she did not receive it this reference period. In previous questionnaires such probing was not done. Second, if a person reports the receipt of a given income this reference period, but did not at the previous interview, the person is now asked to state the month he or she began receiving it. In previous questionnaires the respondent was asked to report for each reference month individually the amount received (Hill, 1987).

To obtain an early indication of the effect of these changes, minimally edited data was used to compare transitions for Wave 2 of the 1988 panel to transitions for Wave 5 of the 1987 panel. The comparisons do not indicate improvements in the amount of seam transitions reported as a result of the changes. However, after the data are subjected to the complete SIPP edit, the results could change. If the analysis suggests that the changes do not lessen the seam problem, consideration will be given to providing interviewers with more cues and probes and performing an extensive edit and follow-up in the field of cases reporting a transition of the seam.

B. Evaluations

Three evaluations of currently or soon to be available data will be carried out to judge whether other changes could be made to improve transition and spell estimates. First, a small scale experiment involving 300 cases was conducted to (1) compare the impact of reporting transitions between waves by feeding back previous wave's data to current procedures and (2) compare the impact of reporting program participation within a wave when record use is emphasized to current procedures (Gbur, 1989 and Matchett, 1988). Results are not yet available.

A second evaluation will examine the effect of different combination of self and proxy respondents (i.e. self-self, self-proxy, proxy-self, proxy-same proxy, proxy-different proxy) on the proportion of transitions reported at the seams (i.e., between interviews). In addition, interactions of demographic characteristics with interview status will be examined. If the analysis indi-

cates differences, statistical comparisons of the proportions reported by the various respondent combinations will be carried out. The analysis may suggest whether current proxy rules should be reconsidered as a way to improve transition estimates.

The final evaluation will examine the effect of the same interviewer for consecutive waves vs. different interviewers on the reported proportion of seam transitions. If the study identifies demographic characteristics of interviewers that are related to the reporting of significant numbers of transitions at the seam, effort will be concentrated on using this knowledge to find ways to reduce the problem.

C. Cognitive Research

A four year, three phase cognitive research plan was developed to generate and test ideas about respondent cognition and behavior. The final product will be a questionnaire and procedures which are expected to substantially reduce the seam bias in the SIPP for implementation in 1995 and beyond.

The first phase, to begin in fiscal year 1989, consists of exploratory studies to generate testable models about the cognitive processes that the SIPP respondents use to answer questions. This phase will include laboratory interviews, focus groups, reinterview of potential seam problem households, and expert panels on cognitive science and human performance.

Phase II, scheduled to begin after completion of Phase I, will be devoted to experimental studies for developing ideas, from Phase I and elsewhere, into concrete procedures and evaluating how well these procedures reduce the bias associated with the seam problem.

During Phase III, the overall design arrived at through empirical research and other activities will be tested before being implemented in 1995 (Kasprzyk, 1988).

D. Time Line Data Collection Instrument

A prototype data collection instrument using a time-line calendar will be considered as a replacement for the current SIPP questionnaire if research suggests that data quality can be significantly improved. This instrument would be developed in conjunction with the cognitive research which will explore whether provision of such a visual aid which identifies major life events and their occurrences in time will substantially improve reporting of transitions.

Results from these intensive efforts to improve the SIPP transition and spell estimates are expected to make major contributions to survey methods.

IX. RECORD CHECK STUDY

This project was designed to evaluate the quality of the SIPP estimates by matching survey data to administrative records data on an individual basis. Results from this evaluation are being used to suggest areas where the SIPP questionnaire and interviewing procedures could be improved. Ultimately, survey data for SIPP sample persons from the 1984 panel in four states will be matched to administrative data from nine government transfer programs.

The record check is being done for all 1984 panel sample households in Florida, New York, Pennsylvania, and Wisconsin (about 5,000 sample households in all). These states were chosen because they: (1) have complete and accessible record systems for all target programs, (2) have a large SIPP sample, (3) are geographically diverse, and (4) were willing to share information. The time period for the study was the first two interviews of the 1984 panel. The reference period for these interviews covered the months June 1983 to April 1984 (Moore and Marquis, 1988).

Administrative records were obtained for five federally-administered programs: Federal Civil Service Retirement, Pell Grants, Social Security, Veteran's Compensation and Pensions, Supplemental Security Income (SSI); and four state administered programs: Aid to Families with Dependent Children (AFDC), Food Stamps (FS), Unemployment Compensation, and Worker's Compensation. For each of these programs, data on the identity of the respondent, receipt, and amount received were obtained for all persons who received income from a program at

any time between May 1983 and June 1984. Two of the administrative records files have problems with incomplete coverage. The New York Worker's Compensation file excludes an unknown number of "closed" cases; the Veteran's Compensation file for all four states excludes about one percent of the cases where the benefit was not paid directly to the individual. The other files have no identifiable coverage problems. Administrative data were given the same confidentiality protection as regular Census Bureau data (Moore and Marquis, 1988).

An accurate matching procedure is necessary to obtain good estimates of response bias and other measurement error parameters. To produce accurate matches, the variables used to match should be accurately reported and uniquely identify an individual. Social Security number (SSN) comes close to this ideal on both points (about 95 percent of SSNs on the SIPP file are correct and SSN comes close to uniquely identifying an individual). Name, address, sex, and date of birth are among the other variables that are being used as match variables. Therefore, the matching should be of high quality. Complex computer algorithms are used to perform the matching operation (Moore and Marquis, 1988). The matching procedures used here are based on the method described in Fellegi and Sunter (1969).

The process of matching the SIPP and administrative records data and creating research files has been completed for AFDC, FS, SSI, unemployment compensation, and workers compensation in Wisconsin and for AFDC, FS, and SSI in Pennsylvania. Some early results are available which, as mentioned, cover only the first two interviews of the 1984 panel.

An initial investigation by Moore and Marquis (1988) analyzed the matches for AFDC and FS for Wisconsin on a household basis. Comparisons of monthly SIPP data to AFDC records showed no pattern of upward or downward bias in the number of recipiency households reported in the SIPP. Also, no significant differences between the number of SIPP and AFDC recipients were found. For FS recipiency, there does appear to be a pattern of underreporting. Six of the eight months of data show significantly fewer households reported in the SIPP than in administrative records. (However, for simplicity all comparisons used t-tests that assume simple random sampling. If design effects were taken into account, some differences probably would not be significant).

Additionally, Moore and Marquis (1988) compared reported amounts for households in Wisconsin where both the SIPP and administrative records show recipiency. For both AFDC and FS, no significant differences were found between the average benefit amount reported in the SIPP and administrative records and no consistent pattern of over or under reporting was seen.

Third, Moore and Marquis (1988) considered the quality of SIPP month-to-month transition estimates for household level AFDC and FS recipiency and amounts for Wisconsin. Briefly, SIPP reporting of AFDC monthly transitions in recipiency did not show any significant biases. However, reporting of FS recipiency transitions and reporting of both AFDC and FS amount transitions showed very significant overreporting of transitions between interviews (seam transitions) and apparent underreporting of within interview transitions. The seam effect was more pronounced for FS estimates than for AFDC estimates. Graphs of the amount transition results are given in tables 7 and 8 for AFDC and FS, respectively.

Further investigation by Marquis and Moore (1989), analyzed the matches of AFDC, FS, SSI, and employment compensation for Wisconsin and AFDC, FS, and SSI for Pennsylvania at the person level. This investigation considered response biases, response error variances, response error correlations, and the seam effect for program participation and benefits amounts. (Program participation and amounts estimates were based on all eight months.) Only three of the eight participation estimates (e.g., AFDC and FS for Wisconsin and AFDC for Pennsylvania) of net response bias were significantly different from zero and all bias estimates were negative. For net response bias of benefits amounts, only unemployment compensation for Wisconsin was significantly different from zero.

Most of the error variances for program participation and benefit amounts were large. For the eight participation estimates, the median error variance was 45 percent of measured variance. For the benefits estimates, the median was 36 percent.

The response error correlations between consecutive months reported at the same interview tended to be larger than the correlations between consecutive months reported at different interviews. However, both the between and within correlations were greater than +.7 for most of the eight participation and amounts estimates.

Month to month participation transitions reported within and between interviews were generally biased with the between rates usually being too high and the within rates tending to be too low.

Analysis of the nine programs for the four states will continue at both the person and household level to gain further insights into the quality of the SIPP data.

Marquis and Moore (1989) conclude from the initial results that use of a time line calendar to minimize the seam effect and a reinterview to estimate the response errors may not be the solutions to the SIPP bias and seam problems. They believe, however, that the results support using cognitive research as a preliminary tool to develop hypotheses about other potential ways to reduce response biases and response error variances in the monthly participation reports and to reduce the correlations of these errors across time.

The methodology used for and insights gained from this research should prove valuable to survey methodologists who are considering alternative questionnaire and interviewing procedures.

X. TIME-IN-SAMPLE BIAS

The sources of bias in a panel survey can be put into one of two broad categories depending upon whether the bias is independent of or dependent on the length of time the panel has existed. Bias in the latter category is referred to here as time-in-sample (TIS) bias. The purpose of this research project is to determine if a significant amount of TIS bias is present in the SIPP.

Accumulating nonresponse is one possible source of TIS bias. SIPP nonresponse typically increases from 5 percent in the first interview to over 20 percent in the eighth interview. Therefore, it is likely that nonresponse bias increases over the life of a panel. Various kinds of learning effects are another possible source of TIS bias. For example, a respondent may begin participating in a government program which he or she learned about because of the survey. This would give an upward bias to program participation estimates. Also, a respondent may, after several interviews, avoid mentioning the receipt of an income source to avoid having to answer additional questions about that source of income. This would give a downward bias to recipiency estimates. Note that nonresponse and learning effects should lead to increasing (or, at least, nondecreasing) amounts of bias during a panel. Therefore, it will be assumed that net TIS bias, if present, is nondecreasing in absolute value as the panel progresses.

Determining the amount of TIS bias could have important consequences for the SIPP's design. The SIPP design could consist of overlapping or nonoverlapping panels. From a cost and data collection perspective, the sample sizes of two overlapping panels could only be about half the sample size of one nonoverlapping panel. Thus, data covering the same time period from two overlapping panels would need to be combined to obtain cross-sectional estimates having roughly the same reliability provided by data from one panel of a nonoverlapping design. As a result of time in sample, cross-sectional data from later waves of a panel may be biased. With overlapping panels, data from the same time period from early in a panel and later in another panel can be combined to reduce the TIS bias associated with waves occurring late in a panel. If this bias were found to be negligible, then a nonoverlapping panel design could be used for the SIPP without damaging the quality of cross-sectional estimates. With a nonoverlapping panel design, a larger sample size could be used for each panel, thus increasing the reliability of longitudinal estimates from the same panel.

Cross-sectional data are currently available from all interviews of the 1984 panel and from the first four interviews of the 1985 panel. Some idea of the amount of TIS bias can be obtained by comparing estimates from these two panels for the same time periods. For a limited number of items, SIPP estimates can also be compared to independent estimates taken from administrative

records.

Before discussing the planned comparisons to evaluate TIS bias in the SIPP, some notation and assumptions will be introduced:

$Y_{i,j}$ = true value of an item for the ith quarter of year j (i = 1,2,3, or 4)

$a_{i,j}$ = administrative records estimate of an item for the ith quarter of year j (assumed to have zero variance)

b_a = bias of administrative records estimate (assumed to be constant over time)

$x_{i,j}$ = 1984 panel SIPP estimate of an item for the ith quarter of year j

$z_{i,j}$ = 1985 panel SIPP estimate of an item for the ith quarter of year j

t_i = TIS bias for SIPP estimates of the ith quarter of a given panel. (Assume t_i is constant across panels for a given i. Also assume the t_i are either a nonincreasing or nondecreasing sequence of numbers)

b_x = panel bias for 1984 panel (assumed to be constant over time)

b_z = panel bias for 1985 panel (assumed to be constant over time)

e = random error term

Now, estimates from each source of data can be written in terms of these parameters. Further discussion will be simplified if a concrete example is considered. Therefore, throughout the rest of this section, the proposed methodology will be explained using the first and fourth quarters of 1985 as an example. Estimates from the first and fourth quarters of 1985 can be written as follows:

1. Administrative Records:
   $a_{1,85} = Y_{1,85} + b_a$
   $a_{4,85} = Y_{4,85} + b_a$

2. SIPP 1984 Panel:
   $x_{1,85} = Y_{1,85} + t_7 + b_x + e_{1,85}$
   $x_{4,85} = Y_{4,85} + t_{10} + b_x + e_{4,85}$

3. SIPP 1985 Panel:
   $z_{1,85} = Y_{1,85} + t_1 + b_z + e_{1,85}$
   $z_{4,85} = Y_{4,85} + t_4 + b_z + e_{4,85}$

By taking differences among the estimates in 1, 2, and 3, we will obtain estimates of differences among the t_i. Significantly large differences among the t_i would indicate that bias is increasing during the panel (i.e., indicate that TIS bias is present).

For items where the $a_{i,j}$ are known, an estimate of the difference in TIS bias between any two quarters will be obtained from a single panel. As an example, consider the first and fourth quarters of 1985. From the 1984 panel an estimate of $t_{10} - t_7$ is obtained by taking the difference $(x_{4,85} - a_{4,85}) - (x_{1,85} - a_{1,85})$. In the 1985 panel the difference $(z_{4,85} - a_{4,85}) - (z_{1,85} - a_{1,85})$ yields an estimate of $t_4 - t_1$. Assuming zero variance for $a_{1,85}$ and $a_{4,85}$, we can obtain variance estimates for $t_{10} - t_7$ and $t_4 - t_1$ using SIPP generalized variance parameters and previously computed estimates of quarter-to-quarter correlations. If these differences are not significantly large then there is no evidence that TIS bias is a problem. In this project, for items where administrative record estimates are available, all possible combinations of quarters for which data are available will be compared for both panels. This will help validate or dispute the assumption that the t_i are either nonincreasing or nondecreasing.

For items where the $a_{i,j}$ are unknown, an estimate of the difference in TIS bias for several quarters combined will be obtained by taking differences between 1984 and 1985 panel estimates at two points of time. Again, consider estimates of the first and fourth quarters of 1985. By taking the difference $(x_{1,85} - z_{1,85}) - (x_{4,85} - z_{4,85})$, all the extraneous terms cancel out and we

are left with an expression involving only TIS bias terms, namely, $(t_7 - t_1) - (t_{10} - t_4)$. If this difference is significantly large, then the presence of TIS bias is indicated. However, no conclusion can be drawn from a nonsignificant difference because this quantity will be zero if the t_i are all zero <u>or</u> if the t_i are linearly increasing or decreasing. (Note that if the t_i were all constant and nonzero they would be considered to be part of the panel bias terms, b_x and b_z). If it can be assumed that the amount of panel bias is constant across panels (i.e., $b_x = b_z$), then it is possible to estimate the difference in TIS bias between any two quarters by taking the difference between panels for the same period of time.

The TIS bias estimates described above will be obtained for a variety of items. Estimates such as earnings, labor force activity, poverty, and participation in government programs will be examined for demographic groups based on age, sex, race and other categories. Results from this study are not yet available.

Current plans are to extend the project to cover three or four years of data for three or four panels. The list of items may be changed based on the results of this initial project.

XI. USE OF ADMINISTRATIVE INCOME DATA IN LONGITUDINAL WEIGHTING

The accuracy of survey estimates may be improved by ratio estimation to certain population controls. In fact, many demographic surveys at the Census Bureau, including the SIPP, regularly make use of controls in their estimation procedures. The controls used are generally derived by updating information from the most recent decennial census.

Use of controls in the SIPP's longitudinal estimation procedures reduces mean square error for many demographic characteristics by reducing sampling variability and bias. Current controls used in the SIPP longitudinal estimation are cross-classifications of age, race, sex and householder/not householder status. The accuracy of other characteristics correlated with this set should also be improved.

Additional ratio adjustment to administrative income data could significantly improve longitudinal estimates of income and program participation, which is a focus of interest for the SIPP. A research project was initiated to determine whether additional adjustment to controls derived from administrative income data improves the SIPP longitudinal estimates of income and program participation by reducing the sampling variability while maintaining or reducing the level of bias in selected SIPP estimates. The focus of the research is on improving SIPP longitudinal estimates, specifically calendar year estimates. However, the methodology is applicable to cross-sectional estimation as well.

The first step in the research was to identify the availability and feasibility of using specific sets of administrative income data as control information. Several administrative sources such as IRS, Social Security, Food Stamp and AFDC files were considered. However, the timing to obtain certain files and the size of their respective populations limited use to only IRS data. The IRS file covers a large portion of the U.S. population and was expected to indicate the degree of improvement that might be obtained by using administrative income data in general.

For controls, a one-percent sample 1984 IRS file was employed. Although the controls are based on a sample, the sampling variability of the IRS one-percent file is small relative to the sampling variances of the SIPP characteristics. Consequently, all controls were treated as if they were free from sampling error.

Some inconsistencies between the SIPP and IRS universes exist to the extent that some IRS returns in the controls represent persons not in the SIPP universe. For example, some institutionalized persons file tax returns, but the SIPP excludes institutionalized persons from its sample. The controls used in research consequently cover a slightly different population than the SIPP and may introduce some bias. The maximum amount of bias is projected to be 2.4% for estimation of total population. Ideally, 1984 calendar year estimates from the SIPP should be ratio adjusted to 1984 IRS controls. However, the SIPP 1984 calendar year data were not available. A 12-month longitudinal data file called the SIPP 3-interview research file with appropriate longitudinal

weights was available. The SIPP 3-interview file covers the months June 1983-August 1984. Because of the survey design, the same twelve months of data are not included for each person on the file.

To use 1984 IRS income controls, the SIPP cases from the 3-interview file were classified by matching the SIPP 3-interview file to a second 1984 IRS data file. This second IRS file was close to a 100% file and approximately 56% of the SIPP persons matched to an IRS record on this file. Husbands and wives in the SIPP who filed joint IRS returns received the same IRS data on their records if there was a match to IRS.

The remaining SIPP population, those who did not match to IRS data are referred to as nonmatches. These nonmatches are a result of persons who did not file IRS returns, persons who filed but whose returns were filed too late for inclusion in the IRS file used in the analysis and persons for whom social security numbers were not available or were incorrect.

The ratio estimation procedure employed for the Census Bureau current surveys involves all of the sampled cases. The situation is different with respect to IRS data, however, since many SIPP respondents are legitimately not in the IRS universe. Consequently, the weighting adjustment under study was for only the SIPP sample cases linked to an IRS return. The weights of SIPP respondents not linked to a return remained unchanged.

The SIPP population was divided into groups by type of return: joint, single and non-joint. Ratio adjustment to IRS controls was performed separately on the three populations. For example, table 9 lists the marginal controls used in the ratio adjustment of joint returns. Similar dimensions were selected for the other types of returns. (Huggins and Fay, 1988).

Once the SIPP data was ratio adjusted to IRS controls by type of return, selected SIPP estimates for the entire SIPP population, SIPP matches and nonmatches to IRS data, and their variances were calculated before and after the ratio adjustment to IRS controls.

Table 10 presents the results of the adjustment for the distribution of total annual income and mean annual income of all persons (Huggins and Fay, 1988). The statistic presented in each cell of the table is the ratio of the variance of the SIPP estimate after ratio adjustment to the variance of the estimate before ratio adjustment to IRS controls. Ratios close to 1 indicate very little or no reduction in variance. Ratios greater than 1 indicate an increase in the variance of the estimate after ratio adjustment. For example, for total persons 25+ and with total personal annual income $10,000 or less, the variance of the estimate after ratio adjustment is 49% of the variance before ratio adjustment which is a 51% reduction in variance.

Overall, there are substantial reductions in variance for estimates of income for the population 25 and over. In particular, the sampling variance of the estimated mean income for the overall population is reduced by an estimated 54 percent. The most dramatic gains for males occur at the relatively higher end of the income distribution while gains for females are more evenly spread among the categories shown.

The adjustments generally benefit the estimates for Blacks, but less consistently. For males, there is overall improvement for the income distribution. The estimated income distribution for Black females shows little improvement. For both sexes the variance of mean income is reduced. For both sexes combined, there are improvements across income and for mean income.

Results for Hispanics are mixed. However, variance on the mean income for all Hispanics age 25+ is reduced by an estimated 27%. The coarseness of the raking for characteristics by ethnicity necessitated by the relatively small sample sizes most probably prevented substantial effects at the cell level that are seen for the overall distribution.

In addition to income characteristics, the effect of the adjustment to IRS controls on the variances of several program variables and on poverty status measure was examined. Table 11 presents the ratios of variances for these estimates. (Huggins and Fay, 1988). The results here for poverty are promising, especially for the overall population and for Blacks. Results for Hispanics are mixed but show minor gains for the overall Hispanic population age

25+, but not for either sex separately.

For the Food Stamp recipiency estimate, results are mixed, yielding some improvements in the overall distribution and for Blacks, but higher variances for Hispanics. AFDC recipiency is even more mixed, with no overall gain and poorer results for Hispanics.

Good overall reductions in variance of person level SIPP characteristics is observed from this research. It is possible that even greater reductions can be obtained for the SIPP estimates at the family and household levels since the IRS annual gross income values often reflect family and household income.

Current plans are to examine the potential gains in variance reduction for several labor force characteristics such as the proportion of persons unemployed and to reintroduce demographic controls into the procedure to adjust the SIPP population that did not match to IRS data. In addition, estimates derived from the research will be compared to administrative data to assess the bias in estimates after the use of the ratio adjustment procedures.

In addition to improving the SIPP longitudinal estimation, SIPP cross-sectional estimation may be improved with a similar procedure. More directly, March CPS income estimates from the Annual Demographic Supplement could benefit from the same procedure, since these estimates are calendar year estimates. A continuation of research to further develop the procedure described in this paper for the SIPP longitudinal estimation appears justified and its usefulness to other types of estimation and other surveys appears attractive.

XII. NONINTERVIEW ADJUSTMENT RESEARCH

A study was done to evaluate how well the SIPP household noninterview adjustment procedure accounts for nonresponse at later waves in the panel. The reason for the concern is that the nonresponse rate at later waves is substantially higher than that attained by the other demographic surveys of the Census Bureau which use similar noninterview adjustment procedures. Analysis of marital history and migration estimates obtained at the eighth wave of the 1984 panel suggests that nonresponse may be adversely affecting these estimates (Petroni and King, 1988). For example, comparisons to vital statistics estimates show the SIPP 1984 and 1985 yearly estimates of the number of marriages to be 20-25% lower. Similar comparisons using Wave 4, 1985 panel data, show the SIPP estimates to be 15% lower. The estimated number of persons who moved to a different residence using 1984 panel, Wave 8 data of the SIPP was 14% lower than the March 1986 CPS estimate. Similar estimates using 1985 panel, Wave 4 data were 7% lower. (O'Connell, 1988). If nonresponse is responsible for these differences, other SIPP estimates may also be adversely affected.

The study examined the ability of the classificatory variables currently used in the SIPP's cross-sectional nonresponse adjustment procedure to account for attrition in the context of the entire weighting scheme. To evaluate how well the adjustment is doing in a later wave, ideally data for the later wave noninterviews should be available so estimates calculated with actual data could be compared to the SIPP estimates. Since these data are missing by definition this is not possible. However, the comparisons were approximated using two estimates based on Wave 2, 1984 panel data. First, it was assumed that a household's Wave 2 characteristics are similar to its characteristics at Wave 6 and estimates were based on the Wave 2 interview status (call these W2/W2). Second, Wave 2 interviewed households that were missing at Wave 6 were identified. In the second Wave 2 estimate (call these W2/W6) these households were treated as missing (Petroni and King, 1988).

The results (see tables 12 and 13) suggest that consideration be given to the use of "monthly household cash income" categories, "metropolitan/nonmetropolitan" categories, and a further breakdown of the current "race/Spanish origin" categories in the SIPP nonresponse adjustment. However, these results need to be explored further since the weighting for 1985 and future panels includes an Hispanic adjustment which the 1984 panel weighting did not. Since this adjustment could affect Hispanic as well as other estimates such as income and metropolitan/nonmetropolitan, a similar study will be carried out using 1985 panel data. The feasibility of using identified categories in nonresponse adjustment from an operational and data processing viewpoint will also be considered (Petroni and King, 1988). Additionally, since

findings in O'Connell (1988) suggest that a separate adjustment may be needed for noninterviewed households which move, exploratory research in this area is underway.

Initial research provides evidence that some bias exists at later waves due to nonresponse and indicates that further research be done to explore whether there is a set of classifying variables which could reduce this bias. Although this research specifically addressed cross-sectional nonresponse adjustment, the knowledge gained may affect decisions on longitudinal nonresponse adjustment.

XIII. APPLICABILITY TO OTHER SURVEYS

The experiments and research projects discussed in this paper deal with collecting data and assessing and improving data quality. These are important issues for all surveys. Of course, the extent to which the methodologies and results of these SIPP research projects can be applied to other surveys must be assessed by those with a detailed knowledge of each particular survey. Some general comments about the applicability of the SIPP research projects to other surveys may be made, however.

The Telephone, Employer Provided Benefits, and Asset Feedback experiments were designed to test various data collection procedures. The methodology used on all three studies could be implemented for longitudinal surveys and the methodology of the first two studies could also be implemented for cross-sectional surveys. If it is considered desirable to test telephone interviewing, the SIPP results suggest that special attention should be given to the collection of cost data. As mentioned, surveys concerned with total personal income, assets, medical expenditures and their effect on income and retirement plans may want to consider the described approach to collecting data from employers. Longitudinal surveys interested in measuring changes in financial assets may want to test the effectiveness of feeding back previous answers to respondents. Analysis of the SIPP results led to the hypothesis that the feedback of amounts may improve the reporting of gradual changes in assets more than lump-sum changes. If true, the evaluation of a feedback test for assets and liabilities could be a difficult matter.

Several aspects of the SIPP data quality have been or will be evaluated through the Transition/Spell, Time-in-Sample, Noninterview Adjustment, Debriefing, and Record Check studies. The methodology of all these studies, except the last two, are restricted to being implemented in longitudinal surveys. Also, the Time-in-Sample study requires that the survey design include overlapping samples. However, the Record Check and Debriefing studies could be implemented in either cross-sectional or longitudinal surveys. Surveys which are interested in running tests to assess their data quality may gain some insight into how to approach this problem based on the methodologies and results described here. For example, experience with the SIPP debriefing questionnaire suggests the usefulness of respondent debriefing for insights into respondent attitudes and into how to improve the data collection instrument. Many results obtained from the SIPP studies may not apply directly to other surveys because the structure of the nonsampling errors may vary widely from survey to survey. For example, the Transition/Spell results may be sensitive to the SIPP questionnaire design.

The Missing Wave, Gift, and Administrative Records studies evaluate the effectiveness of alternate interviewing and weighting procedures. The first of these studies is restricted to use in longitudinal surveys, but the last two are not. Again, the methodologies of these studies may be of interest to other surveys which want to consider similar procedures for evaluating their data quality. The results should be used cautiously, however, because of differences in areas such as questionnaire design and interviewing procedures.

XIV. CONCLUSIONS

Research and evaluation carried out on the SIPP has been successful in investigating potential problems or in gathering information about a specific problem to focus further research or experimentation, and in identifying whether particular methodologies and procedures could improve the data quality. Evaluation yet to be completed is expected to contribute further to this body of knowledge.

Current research and evaluation of the SIPP indicates that further research be done to determine specific methodologies which improve transition and spell estimates and on the cross-sectional noninterview adjustment to determine if bias can be reduced. The evaluation has also shown that feeding back previously reported asset and liability information to respondents does not result in estimates which are statistically different from those obtained without feedback and that the amount of information obtained from collecting missing wave data is too minimal to affect the quality of estimates. The methodology for collecting employer provided benefits appears to be worth pursuing provided proxy respondents are followed up. Early results of the gift experiment are inconclusive about the impact on response rates by giving a small token gift. Results from evaluations yet to be completed are expected to have important implications for the future survey design of the SIPP.

Since the data quality issues faced by the SIPP are similar to those of other surveys, results from the SIPP research and evaluation can be used to identify methodologies and procedures which may improve the quality of data for these other surveys. Results, especially from the Transition/Spell, Record Check, and Administrative Records studies are expected to contribute substantially to survey methods.

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TABLES

Table 1. Correlation of Wave 4 and Wave 7 Amounts; Standard Errors of Correlation Differences Between Feedback and Nonfeedback Groups

	Correlations		Standard Errors
	NF[1]	F	
Total Population			
Home Equity	.444	.515	.120
Vehicle Equity	.587	.628	.046
Business Equity	.257*	.064	.101
Interest Assets at Fin. Inst.	.535	.458	.067
Other Interest Earning Accounts	.452	.390	.117
Stocks and Mutual Fund Shares	.239	.644*	.122
Other Real Estate	.375	.470	.119
Other Assets	.218	.575	.285
IRA or KEOGH Accounts	.491	.666*	.094
Secured Debt	.385	.560*	.102
Unsecured Debt	.178	.212	.090
Total Net Worth	.508	.605	.105
Subpopulation 1[2]			
Home Equity	.488	.500	.047
Vehicle Equity	.533	.521	.034
Business Equity	.016	.000	.123
Interest Assets at Fin. Inst.	.589	.576	.056
Other Interest Earning Accounts	.213	.735*	.168
Stocks and Mutual Fund Shares	.368*	.105	.148
Other Real Estate	.167	.154	.142
Other Assets	.242	.323	.136
IRA or KEOGH Accounts	.301	.390	.121
Secured Debt	.731	.722	.031
Unsecured Debt	.387	.380	.053
Total Net Worth	.707	.699	.022
Subpopulation 2[3]			
Home Equity	.566	.625*	.027
Vehicle Equity	.640	.605	.047
Business Equity	.246	.149	.090
Interest Assets at Fin. Inst.	.605	.606	.037
Other Interest Earning Accounts	.314	.413	.110
Stocks and Mutual Fund Shares	.311	.296	.064
Other Real Estate	.210	.354*	.070
Other Assets	.279	.359	.063
IRA or KEOGH Accounts	.305	.542*	.100
Secured Debt	.663	.624	.027
Unsecured Debt	.339	.319	.063
Total Net Worth	.733	.752	.015

1: NF=Nonfeedback, F=Feedback
2: Households with maximum Wave 4 or Wave 7 assets or debts less than $50,000.
3: Households with maximum Wave 4 or Wave 7 assets or debts less than $200,000.
*: Correlations significantly different at alpha=.10. Group with larger correlation is denoted.

Table 2. Mean Wave 7 Minus Wave 4 Values and Their Standard Errors: Asset/Liability Held in At Least One Wave

	Mean Values (Wave 7 - Wave 4)		Standard Errors	
Total Population	NF[1]	F	NF	F
Home Equity	2113	2181	1078	938
Vehicle Equity	-346	-325	91	62
Business Equity	-859	-12736	10067	8466
Interest Assets at Fin. Inst.	927	-700*	684	500
Other Interest Earning Accounts	9696	5360	2084	2181
Stocks and Mutual Fund Shares	2902	637	2426	4592
Other Real Estate	-563	-2299	3567	2940
Other Assets	1274	-952	944	1706
IRA or KEOGH Accounts	602*	1491	389	262
Secured Debt	1214	163	2046	1148
Unsecured Debt	-89	20	217	360
Total Net Worth	2842	-990	1849	2282
Subpopulation 1[2]				
Home Equity	-648	-5	343	364
Vehicle Equity	-282	-229	62	59
Business Equity	200	-274	928	739
Interest Assets at Fin. Inst.	193	-155*	103	147
Other Interest Earning Accounts	963	206	701	416
Stocks and Mutual Fund Shares	-95*	1102	243	410
Other Real Estate	-1068	1022*	851	745
Other Assets	-128	-31	58	58
IRA or KEOGH Accounts	860	867	264	181
Secured Debt	470	72	296	278
Unsecured Debt	-19	-4	21	85
Total Net Worth	-399	-93	193	219
Subpopulation 2[3]				
Home Equity	46	50	506	410
Vehicle Equity	-337	-297	61	60
Business Equity	3408	-413*	1378	1289
Interest Assets at Fin. Inst.	480	217	231	291
Other Interest Earning Accounts	1782	1530	564	923
Stocks and Mutual Fund Shares	29	372	690	566
Other Real Estate	-1125	-451	1088	759
Other Assets	-290	-219	134	131
IRA or KEOGH Accounts	1478	1405	253	235
Secured Debt	1528	974	372	437
Unsecured Debt	-167	135*	103	116
Total Net Worth	650	67	398	468

1: NF = Nonfeedback, F = Feedback
2: Household with maximum Wave 4 or Wave 7 assets or debts less than $50,000.
3: Households with maximum Wave 4 or Wave 7 assets or debts less than $200,000.
*: Mean values significantly different at alpha=.10. Group with absolute value closer to 0 is denoted.

Table 3. Cumulative National Type A Noninterview and Refusal Rates by Panel and Interview for Rotation 4 and Rotations 1-3 of the 1985, 1986, and 1987 Panels

Panel	First Interview				Second Interview				Third Interview			
	Rot 1-3		Rot 4		Rot 1-3		Rot 4		Rot 1-3		Rot 4	
	Base	Rate (%)	Base	Rate (%)	Base	Rate (%)	Base	Rate (%)	Base	Rate (%)	Base	Rate (%)
Type A Noninterviews												
1987	9493	6.9	3035	5.9 ++	9633	11.4	3094	10.1 ++	9797	11.8	3156	10.4 ++
1986	9358	7.5	3067	6.9	9579	12.0	3151	11.3	9723	12.8 *	3204	12.1 *
1985	11022	6.8	3592	6.4	11095	8.3 ++	3694	9.4 +	11279	10.2 ++	3755	11.6 +
Refusals												
1987	9493	5.2	3035	4.5	9633	8.6	3094	7.3 ++	9797	9.3	3156	7.8 ++
1986	9358	5.5	3067	5.5	9579	8.9	3151	8.9 *	9723	10.0	3204	9.9 *
1985	11022	4.7	3592	4.7	11095	6.9 ++	3694	7.4	11279	9.0	3755	10.1 ++

Note: A gift was presented to households at the first interview in rotation 4 of the 1987 panel.

For the 1985 panel, interviewers attempted to convert Wave 1 Type A noninterviews (which include refusals) in Wave 2. This conversion was not attempted in the 1986 or 1987 panels.

A design effect (DE) of 2.0 was used for all statistical testing. This DE is an adjusted CPS DE. All comparisons were done at the 10% level of significance and statistically significant differences are identified as follows.

- * The type A (refusal) rate is significantly higher than the 1987 rate.
- ++ The type A (refusal) rate is significantly lower than the 1987 rate.
- \+ The rotation 4 type A (refusal) rate is significantly higher than the rotation 1-3 rate.
- ++ The rotation 4 type A (refusal) rate is significantly lower than the rotation 1-3 rate.

Table 4: Cumulative National Type A Noninterview Rates for the 1985, 1986, and 1987 Panels and 1987 Panel Projected Rates by Interview for Rotation 4 and Rotations 1-3

	Interview 1		Interview 2		Interview 3	
	Rot 1-3	Rot 4	Rot 1-3	Rot 4	Rot 1-3	Rot 4
1985 Rate (%)	6.8	6.4	8.3	9.4	10.2	11.6
1986 Rate (%)	7.5	6.9	12.0	11.3	12.8	12.1
Proj 87-85 based (%)	NA	6.5	NA	12.9 *	NA	13.4 *
Proj 87-86 based (%)	NA	6.3	NA	10.7	NA	11.2
1987 Rate (%)	6.9	5.9	11.4	10.1	11.8	10.4
Base	9493	3035	9633	3094	9797	3156

Note: NA: Not applicable

Proj 87-85(86) based: projected cumulative national type A noninterview rate for the 1987 panel based on the rate for the 1985 (1986) panel.

A gift was presented to households at the first interview in rotation 4 of the 1987 panel.

For the 1985 panel, interviewers attempted to convert Wave 1 Type A noninterviews in Wave 2. This conversion was not attempted in the 1986 or 1987 panels.

A design effect (DE) of 2.0 was used for all statistical testing. This DE is an adjusted CPS DE. All comparisons were done at the 10% level of significance and projected type A rates which are significantly higher than the 1987 rate are identified by "*."

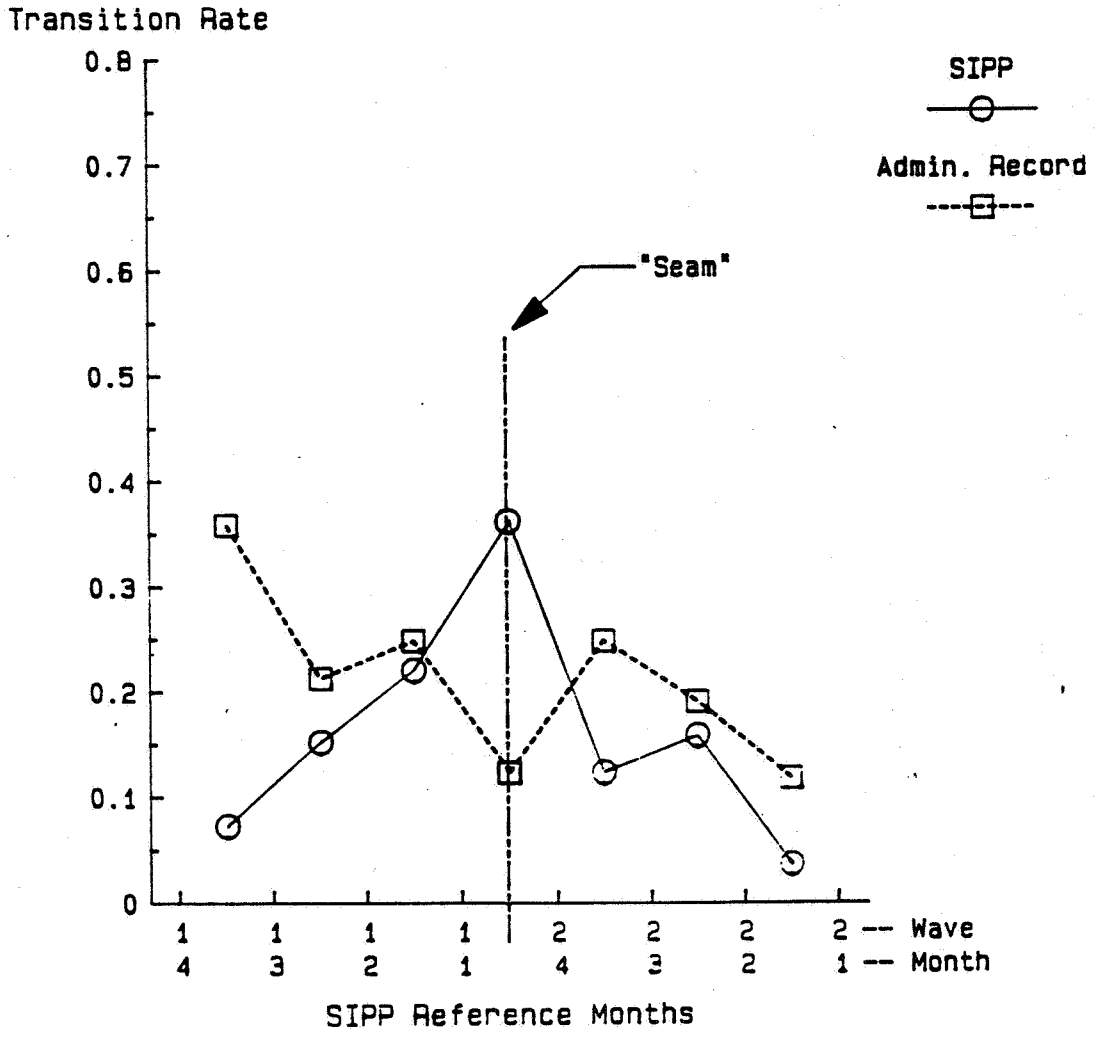
Table 5: Transitions Between Interviews for Receipt of Income Types

Source	Detected	Estimated Benchmark
Social Security	4	7
Railroad Retirement	0	0
Federal Supplemental Security Income	0	2
State Supplemental Security Income	0	0
Unemployment	7	16
Veteran's Compensation or Benefits	1	1
Work Compensation	1	3
Aid to Families with Dependent Children	2	3
Women, Infants and Children Nutrition Program	2	2
Food Stamps	3	9
Child Support	2	3
All others (27 types)	18	33
Total	40	79

Table 6: Transitions Between Interviews for Receipt of Asset Types

Source	Detected	Estimated Benchmark
Savings, Money Markets, C.D.'s, Now Accounts	57	157
Money Market Funds, U.S. Securities Municipal Bonds	0	17
Stocks, Mutual Funds	6	47
Rental Property	6	27
Mortgages	1	6
Royalties & Others	0	14
Total	70	268

Comparison of SIPP and Administrative Records on AFDC Benefit Amount Transitions (Table 7)



Comparison of SIPP and Administrative Records on Food Stamps Benefit Amount Transitions (Table 8)

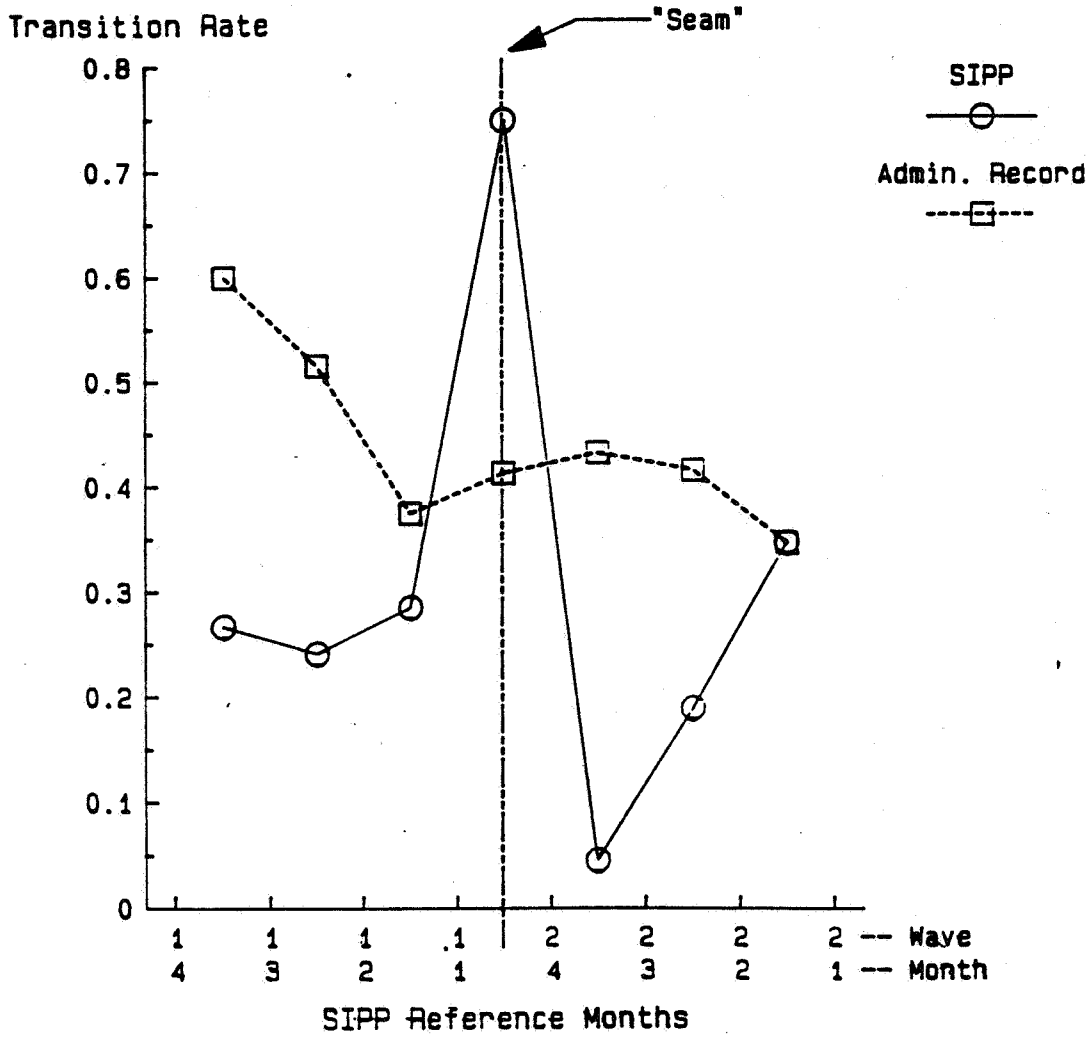


Table 9. Marginal Tables for the Adjustment of Joint Returns

1. Age2 by AGI2
2. Age2 by Race
3. Age2 by Hispanic
4. Age4 by Number of Exemptions (1-2/3/4/5/6+)
5. Age4 by AGI1
6. Number of Exemptions (1-2/3/4/5/6+) by Race
7. Number of Exemptions (1-2/3/4/5/6+) by Hispanic
8. Number of Exemptions (1-2/3/4/5/6+) by AGI1
9. AGI2 by Race
10. AGI2 by Hispanic

Notes: AGI - Adjusted Gross Income,
Race (Black/Non-Black),
Hispanic (Hispanic/Non-Hispanic),
AGI1 (Under $2500/$2500-4999/$5000-7499/$7500-9999/$10-15K /$15-20K/$20-25K/$25-30K/$30-35K/$35-40K/$40-45K/$45-50K /$50-75K/$75K+)
AGI2 (Under $10K/$10-20K/$20-30K/$30K+)
Age2 (-24/25-34/35-44/45-54/55-64/65+
Age4 (-34/35-44/45-54/55+)
Age is the age of the primary filer. For joint returns, this person is generally, but not exclusively, the husband.

Table 10. Ratios of Estimated Variances After and Before Adjustment to Administrative Totals

	Percentages of Income Distribution					Mean
	Loss-10K	$10K-$20K	$20K-$30K	$30K+	$20K+	Income
Total Age 25+	.49	.80	.58	.41	.38	.46
Males	.53	.93	.70	.38	.35	.46
Females	.48	.58	.61	.78	.54	.49
Black Age 25+	.74	.91	.87	.80	.75	.69
Males	.68	.93	.87	.74	.65	.61
Females	.81	.97	1.15	1.12	1.15	.74
Hispanic Age 25+	1.03	.83	.82	1.01	.69	.83
Males	1.23	.86	.77	.91	.68	.86
Females	.79	.81	.81	1.07	.83	.94

Table 11 Ratios of Estimated Variances After and Before Adjustment to Administrative Totals

	Months in Poverty	Food Stamp	AFDC Recip.
Total 25+	.74	.89	1.00
Males	.71	1.01	1.13
Females	.80	.81	.99
Black 25+	.71	.76	.89
Males	.65	.81	1.42
Females	.78	.77	.87
Hispanic 25+	.89	1.21	1.15
Males	.99	1.13	1.11
Females	1.00	1.23	1.17

Table 12. Number of Households (In Thousands) Receiving Benefits or with Low Monthly Income, First Quarter 1984

	Unemployment Compensation		Cash Benefits		Food Stamps		Low Monthly HH Income[1]	
	W2/W2	W2/W6	W2/W2	W2/W6	W2/W2	W2/W6	W2/W2	W2/W6
Race/Spanish Origin								
All Races	2707	2712	7246	7350	6582	6582	11819	11504+
White	2231	2217	4879	4986*	4238	4244	8659	8374+
Black	385	399	2155	2142	2133	2119	2890	2832*
Hispanic[2]	208	186*	779	767	728	682*	1132	1096
Metro/Non-Metro								
Metro	1861	1852	5355	5360	4671	4556*	8194	7790+
1,000,000+	917	897	2844	2752*	2444	2370*	4278	3978+
<1,000,000	944	955	2510	2608+	2227	2186	3916	3812*
Non-metro	846	860	1892	1989+	1911	2026+	3625	3714
Family HHs	2270	2284	5348	5401	5001	4982	7363	7127+
MC HHs[3]	1814	1799	2366	2463+	1859	1877	3838	3677+
Other Fam.	310	331	932	887	795	742+	884	834*
FHHerNSPW/C[3]	147	153	2051	2052	2347	2363	2640	2616
Nonfamily HHs								
Male HHer	272	271	582	590	466	463	1536	1471*
Female HHer	165	158	1316	1359	1115	1138	2921	2907

[1] Households with low monthly income are households below the poverty threshold for that month.
[2] Persons of Spanish Origin are also included in White or Black.
[3] MC = Married couple and FHHerNSPW/C = Female Householder, No Spouse present, with own children under 18 years of age.
\+ Indicates that w2/w2 and w2/w6 are significantly different for $\alpha = .05$.
* Indicates that w2/w2 and w2/w6 are significantly different for $\alpha = .10$.

Table 13. Monthly Cash Income for Households, First Quarter 1984

	Number of HHs (In Thousands)		Mean Income		Median Income	
	W2/W2	W2/W6	W2/W2	W2/W6	W2/W2	W2/W6
Race/Spanish Origin						
All Races	83845	83871	2210	2203	1707	1717*
White	72681	72718	2299	2295	1791	1803*
Black	9314	9347*	1454	1427+	1165	1165
Hispanic [1]	4118	4091	1661	1702*	1391	1434*
Metro/Nonmetro						
Metro.	63763	63206+	2301	2291	1797	1813+
1,000,000+	34348	33489+	2448	2463	1893	1924+
<1,000,000	29415	29717+	2129	2097+	1697	1704
Non-Metro.	20083	20665+	1920	1933	1485	1491
Age Groups						
<25	5633	5654	1459	1476	1278	1289
25-34	19618	19557	2104	2120	1812	1845+
35-44	16420	16360	2660	2654	2244	2255
45-54	12127	12197	2934	2959	2386	2419
55-64	12635	12656	2489	2406+	1787	1767
65+	17412	17447	1439	1432	1000	986*
Married Couple HHs						
All Races	48847	48857	2772	2762	2257	2265
White	44229	44229	2816	2812	2298	2312
Black	3454	3483	2096	2014+	1807	1807
Hispanic[1]	2483	2490	2034	2096+	1723	1775*

[1] Hispanic persons are also included in Black or White.
\+ Indicates that w2/w2 and w2/w6 are significantly different for $\alpha = .05$.
* Indicates that w2/w2 and w2/w6 are significantly different for $\alpha = .10$.

Attachment A

OMB No. 0607-0425: Approval Expires August 31, 1989

FORM ET-5814(X) (3-11-87)

U.S. DEPARTMENT OF COMMERCE
BUREAU OF THE CENSUS

EMPLOYER QUESTIONNAIRE AND AUTHORIZATION FORM

NOTICE — Your report to the Census Bureau is confidential by law (Title 13, U.S. Code). It may be seen only by sworn Census employees and may be used only for statistical purposes.

REGIONAL OFFICE ADDRESS

To: Name of employer

Street address

City State ZIP code

Attention: _____ *(Name of official or office)*

Telephone: ( ) — *(Area code)* *(Number)*

By this signed statement, I hereby authorize you to furnish the United States Bureau of the Census with the information requested below.

Signature _____ Date _____

INSTRUCTIONS FOR EMPLOYER

▶ If you no longer employ this person mark (X) the box to the right and return this form in the envelope provided. ☐ No longer employed

▶ If you currently employ this person please answer the following questions. Mark (X) the appropriate boxes in columns a, b, and c and enter the requested information. Then return this form in the envelope provided.

Name of employee

Social Security number

	Medical Insurance Plan (a)	Pension Plan (b)	Life Insurance Plan (c)
1. Do you provide a medical insurance plan, pension plan, or life insurance plan for your employees? *(Mark Yes or No for each plan in columns a, b, and c.)*	1☐ Yes 2☐ No	1☐ Yes 2☐ No	1☐ Yes 2☐ No
	If No is marked for all 3 plans, skip questions 2—5 and complete questions 6 and 7.		
2. As of July 31, 1987 was this employee ELIGIBLE to be included in a provided plan? *(Mark Yes or No for each plan provided.)*	1☐ Yes 2☐ No — *SKIP to next plan or item 6*	1☐ Yes 2☐ No — *SKIP to next plan or item 6*	1☐ Yes 2☐ No — *SKIP to item 6*
3. As of July 31, 1987 was this employee PARTICIPATING in a provided plan? *(Mark Yes or No for each plan provided.)*	1☐ Yes 2☐ No — *SKIP to next plan or item 6*	1☐ Yes 2☐ No — *SKIP to next plan or item 6*	1☐ Yes 2☐ No — *SKIP to item 6*
Please answer questions 4 and 5 for each plan in which the person participates. **4. What is the employer's cost for including this employee in the plan?**	$ _____ .00 PER 1☐ Week 2☐ Month 3☐ Year 4☐ Other — *Specify* _____ OR 5☐ Does not contribute	$ _____ .00 PER 1☐ Week 2☐ Month 3☐ Year 4☐ Other — *Specify* _____ OR 5☐ Does not contribute	$ _____ .00 PER 1☐ Week 2☐ Month 3☐ Year 4☐ Other — *Specify* _____ OR 5☐ Does not contribute
5. How much does the employee contribute to the plan?	$ _____ .00 PER 1☐ Week 2☐ Month 3☐ Year 4☐ Other — *Specify* _____ OR 5☐ Does not contribute	$ _____ .00 PER 1☐ Week 2☐ Month 3☐ Year 4☐ Other — *Specify* _____ OR 5☐ Does not contribute	$ _____ .00 PER 1☐ Week 2☐ Month 3☐ Year 4☐ Other — *Specify* _____ OR 5☐ Does not contribute
6. What is your Employer Identification Number (EI Number)?	☐☐ — ☐☐☐☐☐☐☐		

7. Name of person completing this report	Title	Telephone	Area code	Number	Ext.

Thank you for your assistance. All information you provide to the Census Bureau is held in strictest confidence and may be used only for statistical purposes. The Census Bureau never releases information that would identify individual persons or businesses. Our guarantee of confidentiality is assured under the provisions of law (Title 13, United States Code).

REGIONAL OFFICE USE ONLY

Case No. ☐☐☐ ☐☐☐☐ ☐☐ ☐

P.I. No. ☐☐ ☐☐☐ R.O. Code ☐☐

Copy distribution: WHITE — Business YELLOW — Regional Office PINK — Respondent

Attachment B

NOTE: The SIPP survey studies the changes in persons' lives and their financial situation over time. For most people, important changes only occur now and then. Whenever a change does occur, however, it is extremely important that we know exactly in which month it happened. This calendar records the answers given in the past by you or another household member for some of the most important events in the survey, to help you think about the answers for the current interview period.

1. R.O. Code	2. Control number →	PSU	Segment	Seg. suf.	Serial	Sample	Check digit	3. Address I.D.	4. Entry Address I.D.	5. Person No.
__ __		__ __ __	__ __ __ __	__ __	__ __	S 0 9	__	__ __	__ __	__ __ __

Person number (or name)	1988			1989										1990												1991								
	OCT	NOV	DEC	JAN	FEB	MAR	APR	MAY	JUN	JUL	AUG	SEP	OCT	NOV	DEC	JAN	FEB	MAR	APR	MAY	JUN	JUL	AUG	SEP	OCT	NOV	DEC	JAN	FEB	MAR	APR	MAY	JUN	JUL	AUG	SEP
Respondent person number																																				
Address change?																																				
Number of persons in household																																				
Mental status																																				
Employer 1:																																				
Wages/Salary																																				
Employer 2:																																				
Wages/Salary																																				
Self-employed (income)																																				
Health insurance coverage: Private																																				
Medicaid																																				
Medicare																																				
Programs: Workers' compensation																																				
Unemployment payments																																				
Child support																																				
Food stamps																																				
SSI																																				
AFDC/ADC																																				
General assistance																																				
WIC																																				
Pensions/Retirement: Social Security Retirement																																				
Private pension																																				
State/Local govt. pension																																				