RESEARCH REPORT SERIES
(Survey Methodology #2006-02)

Use of Dependent Interviewing Procedures to Improve Data Quality in the Measurement of Change

Jeffrey Moore, Nancy Bates[1], Joanne Pascale,
Julia Klein Griffiths, Aniekan Okon[2]

Statistical Research Division
U.S. Census Bureau
Washington, DC 20233

[1]Demographic Surveys Division, U.S. Census Bureau

[2]Demographic Surveys Division, U.S. Census Bureau

Report Issued: January 13, 2006

Note: An early version of this paper was presented at the 60th Annual Conference of the American Association for Public Opinion Research, Miami Beach, FL, May 12-15, 2005; this version is a draft of an article accepted for publication (under the title "Tackling Seam Bias Through Questionnaire Design") in Methodology of Longitudinal Surveys (ed. P. Lynn), forthcoming in 2006 from John Wiley & Sons, Ltd.. This paper is released to inform interested parties of research and to encourage discussion of work in progress. The views expressed are the authors' and not necessarily those of the U.S. Census Bureau.

Use of Dependent Interviewing Procedures to Improve Data Quality in the Measurement of Change

Jeffrey Moore, Nancy Bates, Joanne Pascale, Julia Klein Griffiths, Aniekan Okon
U.S. Census Bureau

1. Introduction

This chapter examines the impact of dependent interviewing procedures on "seam bias," a form of measurement error peculiar to longitudinal surveys. "Seam bias" refers to the tendency for estimates of change measured across the "seam" between two successive survey administrations to far exceed change estimates measured within a single interview. Except in very special circumstances, the presence of seam bias is a clear signal of measurement error. Much research over the past two decades has documented the existence of seam bias in longitudinal surveys, and has also shed light on the essential nature of the phenomenon – too little reporting of change within the reference period of a single interview, and too much reporting of change at the seam. Attempts to control seam bias have met with some success, but have been limited primarily to employment-related characteristics.

Following an extensive research and development program, the U.S. Census Bureau recently implemented new procedures in the Survey of Income and Program Participation (SIPP) 2004 panel questionnaire in an attempt to significantly reduce seam bias. The primary questionnaire revision which was intended to accomplish this was a more extensive and more focused use of dependent interviewing (DI) procedures than had been employed in prior SIPP panels. This chapter describes those procedures, and examines their impact on estimates of month-to-month change across the initial waves of the 2004 panel for a number of characteristics (e.g., participation in government transfer programs, school enrollment, employment, earnings, and health insurance coverage) through a comparison with similar estimates derived from 2001 SIPP panel data. We find evidence of significant positive change with the new procedures – estimates of month-to-month change from the initial interview waves of the 2004 panel are in general much less afflicted with seam bias than their 2001 counterparts. Even with the improvement, however, much seam bias still remains.

The remainder of this chapter is organized as follows: In Section 2 we briefly describe the seam bias phenomenon, and summarize work which has attempted to understand and ameliorate it. Section 3 provides background on SIPP, the U.S. Census Bureau survey that served as the vehicle for the research presented here. We describe the DI procedures used in SIPP in the most recent panel (2001) before the current one, and the development and refinement of new DI procedures via a research and development effort called the SIPP Methods Panel project. Section 4 presents the primary research results, which consist of comparisons of 2004 SIPP panel seam bias results, across a variety of characteristics, with results for the same characteristics derived from the old questionnaire used in the 2001 SIPP panel. Finally, in Section 5 we present our conclusions, including implications of the current findings for future research.

2. Seam Bias

Seam bias as a measurement problem in longitudinal surveys began to draw the attention of survey methodologists in the early 1980's. Czajka (1983, p93), for example, describing data from a survey which was the precursor to the U.S. Census Bureau's SIPP, notes "a pronounced tendency for reported program turnover to occur between waves more often than within waves." Moore and Kasprzyk (1984) provide a quantitative assessment of the extent and magnitude of the seam bias effect in the same dataset. Soon the phenomenon was identified in the SIPP itself (Burkhead and Coder, 1985; Coder et al., 1987), and in other ongoing longitudinal survey programs such as the University of Michigan's Panel Survey of Income Dynamics (Hill, 1987), and the U.S. Census Bureau's quasi-longitudinal Current Population Survey (Cantor and Levin, 1991; Polivka and Rothgeb, 1993). In its subsequent panels, SIPP has continued to provide much evidence of seam bias (Hill, 1994; Kalton and Miller, 1991; Martini, 1989; Ryscavage, 1993; Weidman, 1986; Young, 1989 – see Jabine, King, and Petroni (1990), and Kalton (1998) for summaries of SIPP seam bias research), so much so that Weinberg (2002) lists it as a key unresolved research issue for the survey. Michaud and colleagues have produced numerous papers documenting seam bias and its attempted amelioration in Statistics Canada's longitudinal surveys (e.g.: Brown, Hale, and Michaud, 1998; Cotton and Giles, 1998; Dibbs et al., 1995; Grondin and Michaud, 1994; Hale and Michaud, 1995; Michaud et al., 1995; Murray et al., 1991); and in recent years researchers on the other side of the Atlantic have demonstrated that European longitudinal surveys are by no means immune (Holmberg, 2004; Hoogendoorn, 2004; Lynn et al., 2004). LeMaitre (1992) provides an excellent general review; his summary (p5) of the first decade of seam bias research results still seems apt: "seam effects would appear to be a general problem with current longitudinal surveys, regardless of differences in design and the length of the reference period." Marquis and Moore (1990) confirm that seam bias severely compromises the statistical utility of estimates of change.

Since the very beginning, seam effects researchers have considered it almost axiomatic that the amount of change measured between interview waves is overstated. Collins (1975), for example, speculates that between two-thirds and three-quarters of the observed change in various employment statistics (as measured in a monthly labor force survey) were spurious; Polivka and Rothgeb (1993) estimate a similar level of bias. Michaud et al. (1995, p13) describe apparent change in income across successive survey waves as "grossly inflated;" similarly, Sala and Lynn (2004, p8) label the amount of change they observe from one survey wave to the next in various employment characteristics as "implausibly high;" see also Cantor and Levin (1991), Hill (1994), Hoogendoorn (2004), and Stanley and Safer (1997). Other researchers have focused on the other side of the equation – the understatement of change within an interview wave – sometimes called "constant wave responding" (Martini, 1989; Rips, Conrad, and Fricker, 2003; Young, 1989). Moore and Marquis (1989), using record check methods, confirm that both factors – too little change within the reference period of a single interview, and too much at the seam – operate in concert to produce the seam effect. Kalton and Miller (1991) offer supporting evidence for that assessment, as does LeMaitre (1992). Rips, Conrad, and Fricker (2003) tie these phenomena to a combination of memory processes – specifically, memory decay over time – and strategies that respondents invoke to simplify a difficult reporting task. In support of these positions they cite

evidence of increasing seam bias with an increase in the interval between the interview date and the to-be-recalled change (see, for example, Kalton and Miller, 1991), and with increasing task difficulty in general (e.g., Sala and Lynn, 2004).

Along with a better appreciation of the pervasiveness of seam bias, and a better understanding of its underlying nature, came increased calls for possible remedies, among which DI procedures were often mentioned.(e.g., Corti and Campanelli, 1992; Kalton and Miller, 1991). Excellent summaries of the pros and cons of DI can be found in Holmberg (2004), Murray et al. (1991), and especially Mathiowetz and McGonagle (2000). For those concerned about seam bias, however, and the more general problem of accurate measurement of transitions, the need to control spurious change made DI very attractive. This has been especially true with regard to the measurement of employment-related phenomena. After tests of DI in the Current Population Survey (CPS, the official source of labor force data in the U.S.) showed great promise (e.g., Cantor and Levin, 1991), DI was introduced permanently into CPS procedures in the early 1990's, and has greatly reduced the overestimate of between-interview change in various labor force characteristics (Polivka and Rothgeb, 1993). Hill (1994), in a comparison of successive SIPP panels, one of which did not use DI for employment-related questions, the other of which did, reports similar results. Use of DI in Statistics Canada's Labour Market Activity Survey, and later its Survey of Labour and Income Dynamics, has virtually eliminated seam bias for employment characteristics, according to Brown, Hale, and Michaud (1998), Cotton and Giles (1998), and LeMaitre (1992). More recently, in Great Britain, Lynn and colleagues have experimented with different forms of DI in labor force and other surveys; they find somewhat inconsistent effects in different circumstances for "proactive" versus "reactive" DI, but in all cases the level of spurious change is consistently the highest under conditions of independent interviewing (Jackle and Lynn, 2004; Lynn et al., 2004; Sala and Lynn, 2004).

3. SIPP

SIPP is a nationally-representative, interviewer-administered, longitudinal survey conducted by the U.S. Census Bureau. It provides data on income, wealth, and poverty in the United States, the dynamics of program participation, and the effects of government programs. Each SIPP panel consists of multiple waves (or rounds) of interviewing, with waves administered three times a year, at four month intervals. The SIPP sample is split into four equivalent subsamples, called "rotation groups;" each rotation group's interview schedule is staggered by one month, in order to maintain a constant workload for field staff. All SIPP interviews are now conducted with a computer-assisted questionnaire; the first interview is administered in-person, subsequent interviews are often conducted via telephone. The SIPP core instrument, which contains the survey content that is repeated in every survey wave, is detailed, long, and complex, collecting information about household structure, labor force participation, income sources and amounts, educational attainment, school enrollment, and health insurance over the prior four-month period. A typical SIPP interview takes about 30 minutes per interviewed adult. See U.S. Census Bureau (2001) for a more complete description of the SIPP program.

3.1. SIPP's pre-2004 use of DI

Throughout its twenty-year history prior to 2004, SIPP always relied heavily on dependent questions for its "control card" material – i.e., the household roster, and the demographic characteristics of household members – but such procedures were never prominent in the main body of the SIPP questionnaire. In the survey's early panels this was in part a function of its paper-and-pencil interview mode. Computer-assisted interviewing (CAI), which was introduced in the 1996 SIPP panel, is much more conducive to a smooth and accurate administration of dependent questions than is the case with a paper instrument (Brown, Hale, and Michaud, 1998; Corti and Campanelli, 1992). Nevertheless, despite the expanded potential for the use of dependent techniques with CAI, neither the 1996 SIPP panel nor those that followed made much more use of such questions than did their paper-and-pencil predecessors. In the 2001 SIPP panel, for example (the most recent SIPP panel before 2004), some key subject-matter areas, such as health insurance coverage, did not use any dependent procedures; each wave of the survey asked about health insurance coverage without any reference to past reports. In fact, the survey's approach to asking about jobs and businesses after the initial interview wave comprised almost the complete set of truly dependent questions used in the 2001 panel: "Last time I recorded that you [had a job with/owned a business named] [employer name/business name]. Do you still [have that job/own that business]?"

Other areas of the 2001 questionnaire employed dependent-like questions, reminding respondents of their prior reports, but then simply repeating a wave-1-style question regarding the current wave: "Last time I recorded that you received Supplemental Security Income (SSI) payments. Did you receive any SSI payments at any time between [MONTH 1] 1st and today?" These dependent-like questions offer one clear advantage over the more standard form of dependent questioning: they are simple to implement, because they do not require any restructuring of the initial questionnaire beyond the simple addition of the "Last time I recorded..." introduction. A major drawback of the form, however, is that it only weakly anchors the respondent's current report in the past, and does little to actually invite consideration of whether a characteristic has continued in that past state or changed. Simply putting the information from the prior interview "out there," without tying it to the response task, still leaves the respondent focused primarily on the immediate reporting period, probably to about the same extent as a non-dependent question.

3.2. Development of new DI procedures

Motivated primarily by concerns about increasing nonresponse/attrition, and by a desire to bring known data quality problems under better control, in the mid-1990's the U.S. Census Bureau launched the SIPP Methods Panel project, a research and development program to create an improved SIPP questionnaire for implementation in the new SIPP panel to begin in 2004. For the most part, the Methods Panel's efforts focused on "interview process" improvements that would yield a less burdensome interview. Another goal, however, was to improve data quality, one specific aspect of which was a reduction in seam bias. This became an important focus of the Methods Panel when a comparison of seam bias in the 1993 and 1996 panels confirmed that the CAI procedures introduced in 1996 had not had any impact on the problem (Moore et al., 2004).

Armed with evidence that the need to address this measurement error issue remained as strong as ever, we designed and implemented new procedures to reduce seam bias, and tested them in the Methods Panel's experimental SIPP questionnaire. Because of the positive track record of DI with regard to reducing spurious change, our main focus was on strengthening those procedures in SIPP, which we did in the following ways:

(1) We expanded the survey's reference period to include not only the four preceding calendar months, but also the current month, up to the date of the interview. This had been a feature of many of the questions in the 2001 SIPP panel; in the new questionnaire it became universal when asking about the timing of events. The more fundamental change, however, was to store the interview month information and carry it forward to the next interview wave, where it is featured prominently in the logic used to decide whether to ask a dependent question, and, if so, the specific form of that question. As noted above, the interview month ("month 5") of one interview wave is the first month of the next wave's reference period. Before 2004 the SIPP questionnaire had not exploited the fact that when an interview month event is reported, a basic fact about the *next* wave's four-month reference period is already known.

(2) We framed the new questionnaire's dependent questions in truly dependent language, explicitly linking the current wave report to what is known from the last interview, and focusing the cognitive task on whether or not the prior circumstances did or did not continue on into the current wave. The concentration on whether something *continued* from one interview's reference period to the next also led us to restrict the range of circumstances under which a dependent question would be asked. In SIPP's 2001 questionnaire, an event that occurred in any month of the previous interview's reference period was sufficient to trigger a dependent-like question in the next wave – even if the event happened only early in the previous interview's reference period, and was no longer appropriate to the notion of "continuing" into the next reference period[1]. The new instrument, in contrast, concentrates on what was happening as of the end of the previous reference period. Thus, only the previous interview's months 4 and 5 – "last month" and the interview month – are used to determine whether to ask a dependent question. Events that happened only before those months trigger a non-dependent question in the subsequent interview wave, with no mention at all of pre-month-4 events or characteristics.

More specifically, we instituted the following new procedures, with some slight variations, throughout the 2004 SIPP questionnaire:

– An event or circumstance reported in "month 5" of the prior wave (i.e., the first month of the current wave's reference period), triggers an initial question in the next interview wave to confirm that prior event or circumstance: "Last time I recorded that you [received X, were enrolled in school, had health insurance, etc.] in [MONTH 1]. Is that correct?" If the respondent confirms the prior wave report, then his/her status for the current reference period is confirmed; a later question fills in the details about the remaining months of the reference period. If the respondent does not confirm the prior wave report, then the

[1]The 2001 instrument's dependent questions about prior-wave jobs and businesses were an exception to this rule, and in fact closely mirrored the procedures implemented in the 2004 questionnaire for other subject-matter areas.

questionnaire asks about the remainder of the current reference period, e.g.: "Did you receive any income from [X] since [MONTH 2] 1st?"

– Otherwise, if an event or circumstance was reported in "month 4" of the prior wave (the last month of the previous interview's reference period), but not in the interview month, the questionnaire recalls that fact and asks about continued participation the current wave: "Last time I recorded that you [received X] in [prior wave month 4]. Did you continue to [receive X] after [MONTH 1] 1st?" If the respondent reports that [X] did continue into the current wave, then his/her status for the current reference period is confirmed; a later question identifies specific months of receipt/enrollment/etc..

– Finally, if an event or circumstance was not reported in the prior wave, or was only reported in a month other than month 4 or month 5, then the respondent is asked a non-dependent question about the current wave.

(3) The final basic change involving DI techniques was to use them as a follow-up procedure, to reduce nonresponse to income amount questions. This "reactive" form of DI (see Lynn et al., 2004) was implemented for all income amount questions in the 2004 questionnaire beginning in wave 2; no such procedures had been employed in any previous SIPP panel. Questions about income amounts began in 2004 exactly as they had in the previous panel, independent of the information provided in the previous interview. The new questionnaire invoked the reactive DI questions when the respondent volunteered that his/her income had not changed, said "don't know," or refused to answer. Initial evidence suggests that these procedures have been quite successful at reducing item nonresponse in the 2004 SIPP panel (Moore, forthcoming a; forthcoming b). However, we also sought also to understand their impact on income amount transitions; the results of this investigation are also summarized briefly in section 4.

3.3. Testing and refining the new procedures in the SIPP Methods Panel field tests

The Methods Panel project included a series of three field experiments to evaluate and refine the revised SIPP questionnaire in preparation for the 2004 panel. For a description of the design of the field experiments see Doyle, Martin, and Moore (2000); Moore, et al.(2004) also covers field test design issues in some detail, and provides more general information concerning the changes implemented in the SIPP questionnaire. The results of these experiments were sufficiently positive (see, e.g., Moore and Griffiths, 2003) that the new DI procedures for seam bias reduction and for the reduction of income amount nonresponse were implemented in the redesigned instrument used in the 2004 SIPP panel.

4. Seam Bias Comparison – SIPP 2001 and SIPP 2004

This section presents the results of our evaluation of the impact of the new DI procedures on seam bias for program participation and other "spell"-type characteristics in the 2004 SIPP panel. We also look beyond the immediate goal of the DI nonresponse follow-up procedures and examine

their impact on transitions in income amounts. We note that our analysis is limited to a subset of all characteristics included in SIPP that are captured at a monthly level, chosen with some eye toward breadth and importance, but primarily because their results could be analyzed reasonably easily with preliminary, internal data files. Our use of preliminary files also means that the results presented here may differ from those obtained from any future analyses using final, edited data. We use the best method available to us – a comparison of the 2004 seam bias results with those of the immediately preceding 2001 panel – recognizing that drawing conclusions from a "natural experiment," as opposed to a designed one, requires additional strong assumptions. Although we acknowledge these limitations, we have no evidence to suggest that any of the potential confounds actually *did* influence our findings. From the strength and consistency of our findings we also draw confidence that any differences between our results and those of later investigations looking at other characteristics, or using other data files, will be at the margins, and are unlikely to affect overall conclusions.

4.1. Seam bias analysis for program participation and other "spell" characteristics

Our analysis uses data from the first four interview waves of the 2001 and 2004 SIPP panels. Each panel started its wave 1 interviewing in February of the panel year, and thus the first four waves of the two panels cover the exact same calendar months, three years apart. We carried out three separate seam bias investigations, one for each successive pair of waves – waves 1-2, 2-3, and 3-4 – in effect treating each pair of waves as if it provided an independent set of eight months' worth of data, with one seam in the middle. We chose this approach for its simplicity, for the ease it offered with regard to linking sample cases across waves, and also to avoid as much as possible the loss of otherwise useful analysis cases due to their absence from only one or two of the four waves (e.g., attritors and in-movers). In each analysis we exclude cases for which an interview was obtained in only one of the two waves, and within each characteristic we further exclude cases for which data are missing for either of the seam months.

We summarize the results of these analyses in Table 1, which presents the simple average of the three separate estimates we computed for each statistic. Use of the simple average of the three estimates gives them all equal weight, ignoring the fact that the number of cases from which they are derived generally declines slightly across the three tests, due primarily to attrition. We opted for this approach primarily to avoid giving extra weight to the results for waves 1 and 2, which differs from the other wave-pairs in that it includes the only completely non-dependent interview (wave 1) in the entire panel. Another consideration was to accord equal weight to all periods of the calendar year, so as not to over- or under-weight any seasonal effects just because of SIPP's arbitrary interview schedule. Although we have done no formal sensitivity analysis, we doubt that the decision to treat the three wave-pairs in this manner has any important impact on the results and conclusions presented here, primarily because the differences among the three for any of the characteristics examined appear to be minimal.

Table 1 is subdivided into two parts. Part 1 summarizes the results for characteristics which were captured with different procedures in the two panels – i.e., where the 2004 questionnaire used the new DI procedures. Part 1A presents the results for "need-based" public-assistance-type

programs, and Part 1B for non-need-based programs and characteristics. Part 2 presents results for two characteristics whose measurement procedures did not differ across the two SIPP panels. The summary statistics (column headings) are defined as follows:

"Analysis N's" – This shows, for each characteristic and for each pair of waves, the total number of cases included in the analysis, and the total number of month-to-month transitions observed across the eight months of the two waves. The analysis sample is limited to those who were interviewed in both waves of the pair, and for most (but not all[1]) characteristics it is also limited to those who provided at least one "yes" value in one month within either wave.

"Average % of All Changes That Were Seam Changes" – This column starts from a tally of all observed month-to-month changes across the eight months included in two adjacent survey waves; the table shows the percentage of the total comprised of changes at the seam. In the absence of any seam bias, we would expect about one-seventh of all changes – around 14% – to be seam changes, since the seam month-pair is one of seven such pairs in a two-wave analysis. We also summarize here the results of significance tests comparing the 2001 and 2004 estimates in each of the three separate analyses.

"Average Month-to-Month Change Rates (%)" and "Change Rate Ratio: Seam/Off-Seam"– The change rates express the number of observed month-to-month transitions (across all types of month-pairs, and separately for "off-seam" and seam month-pairs) as a percent of all relevant observations. Typically, each individual in the analysis sample for a pair of waves provides seven month-pair observations for each characteristic – one at the seam and six off of the seam. The "Change Rate Ratio" then divides the seam change rate by the off-seam rate to produce an estimate of how many times larger the seam change rate is than the off-seam change rate. (This statistic, unlike the others in Table 1, is not an average of the three separate analyses, but is calculated directly from the average change rates shown in the table.) We have no immediate standard against which to assess the quality of the change rates themselves, which are in part a function of how the analysis universe is defined, and in part a function of the particular "volatility" of the characteristic in question. However, in the absence of any seam bias, we would expect the change rate ratio to be close to 1.0, indicating that the likelihood of a change across the seam is about the same as for any other pair of months.

"Average 'Directional' Change Rates at the Seam" – This pair of columns displays the observed percentage of "yes" (on a program, enrolled in school, covered by health insurance, etc.) cases in the last month of one wave's reference period that changed to a "no" (off, not enrolled, not covered, etc.) in the first month of the next wave's reference period, and, similarly, the observed percentage of "no" cases that changed to "yes." These rates, too, are affected by differences in the volatility of the various characteristics; the no-to-yes rate, in

[1]The decision as to how to treat the analysis sample for a particular characteristic appears arbitrary, but in fact simply follows SIPP's conventions for how questionnaire data are stored (as a "person" record, a "job" record, a "program" record, etc.). The only real practical impact of this decision is whether cases with all "no" values across all eight months are included in or excluded from the analysis.

addition, is also highly sensitive to the definition of the analysis universe. To permit a comparison between the two SIPP panels in the likelihood of observing "yes-to-no" and "no-to-yes" changes at the seam, we also show, in the 2004 row, the change in the statistic from 2001 to 2004 as a percent of the 2001 estimate.

Despite the large amount of information in Table 1, and the wide variations among the different characteristics in the levels of the estimates presented, we find the essential features of the results to be remarkably consistent. We see those essential features as follows:

1. Seam bias has declined substantially in the 2004 SIPP panel. The non-dependent 2001 procedures were significantly less effective at controlling seam bias than are the dependent procedures introduced in 2004. This conclusion is readily apparent in the first data column, which shows the proportion of all month-to-month changes observed at the seam. Scanning down that column in Part 1 reveals a completely consistent trend across all 15 analyzed characteristics subject to the new DI procedures: in every case the 2004 panel estimate is lower than the 2001 estimate. As noted earlier, the 15 pairs of estimates in Part 1 represent 45 separate comparisons, of which 42 showed a statistically significant difference (see Table 1 notes) according to a simple t-test of the difference between two proportions; 36 of the 42 significant differences were significant at the $p<.001$ level or beyond. Not surprisingly, the "Change Rate Ratio" column presents a similar consistency: across all of the Part 1 characteristics, the ratio of the likelihood of a seam change to the likelihood of an off-seam change is always lower in 2004 than it was in 2001. In many cases the 2004 estimate is less than half its 2001 counterpart.

2. The decline is attributable to the new DI procedures. Just as clearly as seam bias declined in 2004 where SIPP implemented new DI procedures, it did not decline where the interview procedures were the same in both panels, as shown in Part 2. For both Medicare coverage (the US government health insurance program for people over 65), and employment at a particular job, the questionnaire procedures in use in the 2004 panel are very similar to the procedures used in 2001. Not surprisingly, the use of very comparable interviewing procedures yields effectively identical seam bias results. This finding offers strong support for the essential argument of this chapter, which is that the seam bias differences in Part 1 are due to the new DI procedures introduced in 2004, and not to different samples, different interviewing staffs, the different times that the measurements were collected, or other artifacts.

3. DI shows positive seam bias effects for all types of characteristics and respondents. Seam bias is not a problem that affects one class of respondent, or one type of characteristic, and the improvements observed with the implementation of DI seem similarly general. This is evident in the consistent, positive results, already noted, across all of the diverse characteristics included in Part 1. We divided Part 1 into two general subcategories of characteristics – "need-based" programs to assist the low income population, and other programs and characteristics which apply to the general population (or which are in fact skewed toward the upper end of the economic spectrum) – primarily to make it easy to see that there was really no need to do so. The impact of DI appears to have been the largely the same in both categories. On some dimensions, in fact, it was nearly identical. For example, the average 2001-to-2004 decline in the percent of changes

observed at the seam across the need-based programs summarized in Part 1A is 20 percentage points, compared to 19 percentage points for the non-need-based characteristics in Part 1B; in terms of the percent decline observed in 2004 relative to the 2001 baseline the average figures are 31 percent and 30 percent, respectively.

4. The positive effects of DI are a result of reduced change at the seam and increased change off the seam. As noted earlier, seam bias has been shown to be the net effect of too many changes observed at the seam and too few changes observed within the months of a single wave's reference period (Moore and Marquis, 1989). The DI procedures introduced in the 2004 SIPP panel directly countered those tendencies. As shown in the "Average Month-to-Month Change Rates (%)" columns, for every characteristic the off-seam change rate is higher in the 2004 panel than in the earlier panel, and for 14 of the 15 characteristics the rate of change at the seam in 2004 is lower than in 2001. DI reduced spurious change reports at the seam, and reduced spurious *non*-change reports across the months within a wave.

5. SIPP's new DI procedures acted primarily to reduce spurious "yes-to-no" change at the seam. As shown in the right-most columns of Table 1, across almost all but two of the characteristics which were captured with the new DI procedures in 2004, the decline in "yes-to-no" changes at the seam is greater, in percentage terms, than the decline in "no-to-yes" changes at the seam. This general pattern makes great sense, because the new dependent procedures employed in 2004 are "asymmetrical" (Murray, et al., 1991), in that they only apply to those who are in a "yes" status (enrolled, covered, participating, etc.) at the end of the prior interview – preventing false yes-to-no change at the seam is what they were designed for.

Given that focus, the question arises as to why the SIPP 2004 DI procedures had any impact at all on no-to-yes transitions. That they did is obvious: with the sole exception of school enrollment, where the rate of no-to-yes change at the seam stayed the same in 2004, the amount of no-to-yes change at the seam was consistently higher in 2001, before the new DI procedures were implemented. We suspect that the key is the careful targeting of DI in the new SIPP panel, especially as contrasted with the indiscriminate, quasi-DI approach that it replaced. Recall that the new DI procedures were only triggered by spells known to be in progress at the end of the previous wave's reference period. In contrast to the format used in the 2001 panel, no mention was made of any spells which were known to have ended before the end of the reference period. We suspect that the old format's irrelevant reminders to respondents about already-ended spells may have served to mask the fact that the respondent had already, in the prior wave, reported the spell's termination, thus subtly encouraging them to mis-recall a new spell as a continuation of an old one – thus resulting in a false no-to-yes change report at the seam. (LeMaitre (1992) also notes this flaw in the previous SIPP design, and its possible negative consequences.)

6. Despite the improvements due to DI, much seam bias still remains. Improvements in seam bias due to the addition of DI are unmistakable; the fact that seam bias is far from having been eradicated is equally unmistakable. With the single exception of school enrollment, every characteristic with improved measurement of transitions in the 2004 SIPP panel still displays an overabundance of changes at the seam. For example: in the proportion of all changes that are

seam changes, the lowest average proportion (again, excluding school enrollment) is still above 30%, which is more than twice as high as would be expected if there were no seam bias. In the "change rate ratios" even the best performing characteristics show a rate of change at the seam that is more than twice the rate observed between months within a single interview wave, and in most cases the improvement still leaves at least 3-4 times more seam changes than there should be – still leaves substantial measurement error, in other words.

Although our purpose here is to examine general trends, rather than the results for particular characteristics, we do want to focus brief attention on the school enrollment results, which stand out from all the others in the much lesser extent to which they are afflicted with seam bias. Even "pre-improvement," in 2001, before the addition of DI, the seam bias estimates for school enrollment are lower than for any other characteristic *after* the addition of DI in 2004. And the "post-improvement" results in 2004 arguably contradict the notion that "much" seam bias remains in the DI-improved estimates[2]. We suspect that the unique profile for school enrollment is due to its familiar seasonal patterns, which makes months much more effective memory cues than they are for other characteristics. This suggests that given half a chance – that is, given reasonable memory cues to begin with – respondents can report their status transitions with reasonable accuracy. Given more than half a chance, with the addition of other useful memory cues – in this case in the form of appropriate dependent questions – respondents can produce reports of status transitions that appear largely devoid of error.

4.2. Seam bias evaluation for income amount transitions

People who report having worked during the SIPP reference period are asked to report their earnings. The 2001 panel questionnaire directed most respondents (about 86 percent) to report their earnings on a monthly basis. The rest provided an annual amount or an hourly wage and number of hours worked (these options were allowed in the 2001 questionnaire but were not explicitly offered in the question text). In an effort to reduce burden, the 2004 SIPP questionnaire allows respondents to define the reporting period, asking explicitly whether they would prefer to report monthly amounts, weekly/biweekly/bimonthly income amounts, an annual salary, hourly pay rates, or quarterly business earnings. In 2004, approximately one-third of all workers reported their earnings using weekly/biweekly/bimonthly income amounts, another one-third reported an annual amount, and the remainder were about equally divided between hourly rate reporters and those who reported on a monthly basis.

In cases where any amount other than monthly is selected, the SIPP instrument calculates a gross monthly amount, based on a variety of other known information. Unfortunately, however, the transformation algorithms used in the 2001 and 2004 panels were not consistent, resulting in important differences in how monthly amounts were created from non-monthly amount reports. Because we sought to address the impact of DI on respondents' *reports* of change in their circumstances (apart from the impact of processing decisions on change patterns), the non-monthly transformation discrepancies forced us to exclude from the analysis those who reported

[2]It is interesting to note that Moore and Kasprzyk (1984), in their very early seam bias investigation, report high levels of bias for every type of characteristic examined, save one – receipt of educational benefits.

in a non-monthly fashion, and to limit our analysis to those who reported individual pay period amounts[3] or monthly totals.

Our analysis of seam effects uses an arbitrary definition of an amount "change" – namely, a difference in earnings amounts between two adjacent months of plus or minus 5 percent[4]. Here again we analyze each pair of waves separately, focusing on the proportion of cases that fell outside the 5% threshold for each month-pair in waves 1-2, 2-3, and 3-4. We restrict the analysis sample to those interviewed in each wave of the pair who held the same job in each wave, and who provided an income amount for each month. Figures 1, 2, and 3, which summarize the results of our analysis of seam bias for earnings amounts in waves 1-2, 2-3, and 3-4, respectively, offer stark visual evidence that DI significantly reduced the "spike" of differences between monthly earnings reported for the same job across the interview seam. Figure 1, for example, shows that almost 70 percent of earnings amounts reportedly changed (according to our definition) across the wave 1-2 seam in 2001, compared to about half that rate in 2004, when DI was made available as a nonresponse follow-up procedure. Note also that when DI was not available in either panel, in the three month-pairs within wave 1, the line graphs are virtually identical, with a constant change rate of about 20 percent. After the seam spike in 2001, the picture within wave 2 returns to a pattern almost identical to that of wave 1 – a change rate that is fairly constant at about 19 percent – in contrast to the post-seam change rates in 2004, which are only about half the rates observed in the earlier panel. Figures 2 and 3 present strikingly similar results. In all three analyses the percent of cases falling outside the threshold at the seam is significantly lower in the 2004 panel than it was in 2001, in some cases reduced by over half[5], and the change rate for off-seam month-pairs (with the exception of those in wave 1) is also consistently lower in the 2004 panel compared to 2001.

The magnitude of the DI procedures' impact on change at the seam is surprisingly large, given that their intended use is restricted to a quite set of circumstances,. It appears, however, that intended use and *actual* use may have been very different matters. We find, for example, that the DI follow-up questions were invoked over half the time in waves 2 and 3 when asking about job earnings, and about 40 percent of the time when asking about business earnings[6]. These rates far exceed typical rates of nonresponse to earnings amount items (Moore, Stinson, and Welniak,

[3]We further restricted those reporting pay period amounts in the 2004 panel to those reporting pay amounts that varied. We excluded those reporting paychecks that did not vary because the new instrument systematically suppressed month-to-month variation when it aggregated to monthly amounts.

[4]Kalton and Miller (1991) use this definition of change to study seam bias in the 1984 SIPP panel for Social Security payment amounts. They find a large seam effect, consisting of almost nonexistent month-to-month change within a single wave, contrasted with amount change at the seam about two-thirds of the time.

[5]Waves 1-2: 69.5% vs. 37.8%, $t=47.2$, $p<.001$; Waves 2-3: 69.1% vs. 31.0%, $t=52.2$, $p<.001$; Waves 3-4: 66.4% vs. 30.1%, $t=50.6$, $p<.001$.

[6]Unfortunately, we cannot determine which condition (same as last time, don't know, or refused) prompted the DI screens to come up most often. For some reporting options, the original amount entry was overwritten when dependent data was later presented and verified as correct. In these cases it is impossible to distinguish whether a DI screen was invoked due to the "same as last time" "don't know" or "refused" trigger.

2000). Other evidence also suggests that the DI questions were not used strictly as a nonresponse follow-up tool. Several 2004 panel interview observation reports (Bruun, 2005; Davis, 2005; Gilbert, 2005; Moore, 2004) note interviewers' tendency to use the DI follow-ups as a means to "peek ahead" at the answers reported in the last interview, and to help respondents answer the amount questions without even giving them a chance to report on their own. Thus, the greater than anticipated seam effect reduction may have been the result of interviewers' tendency to transform the intended reactive DI follow-ups into proactive-style questions.

The consistently lower off-seam month pair change rates in 2004 compared to 2001 also deserves comment. We suspect that this effect is mostly due to instrument programming, not respondent behavior in response to DI. In cases where DI is invoked in 2004, one of two paths produces a monthly total. The respondents might verify the fed-forward amount, in which case it is assigned to each month of the current wave. Or he/she might not verify the old amount, but offer a corrected one in its place, in which case it, too, is allocated to each month in the current wave. Either way, there was no chance of variation in an off-seam month-pair. Thus, the reduced level of month-to-month earnings changes within a single wave's reference period is a function not of respondents' responses to dependent questions, but rather how the survey's designer's have chosen to implement them.

5. Conclusions and Discussion

Despite the limitations noted earlier, we find the results quite encouraging with regard to the quality of month-to-month change data from the new SIPP questionnaire. They offer strong and consistent evidence, across many diverse characteristics, of the significant positive impact of dependent interviewing (DI) on the measurement of month-to-month transitions. In the earnings amount results we even find evidence of "byproduct" positive effects, where nonresponse reduction, not improvement in transition data, was the primary intent. The new, more precise and focused dependent interviewing procedures employed in the 2004 SIPP panel with the specific intent of improving data on transitions appear to have reduced reports of change at the seam, and to have increased reports of off-seam changes. Both trends address what have been shown to be the major error tendencies in the measurement of change in longitudinal surveys – overreporting of change at the seam, and underreporting of off-seam changes (Moore and Marquis, 1989). As a result, the likelihood of recording a transition at the interview seam in the current SIPP panel is, for virtually every characteristic examined, significantly more in line with what would be expected in the absence of measurement error than is the case with the previous panel. Despite the significant improvements, however, much seam bias still remains.

Fortunately, the results presented here also highlight an additional area in which there is still much untapped potential for further improvements: "no-to-yes" changes at the seam. DI as it has been introduced in SIPP focuses exclusively on the *presence* of some characteristic – being enrolled in school, receiving Food Stamps, etc. – in the last months of the prior wave's reference period. A previously-identified, likely-to-continue spell is carefully addressed in the new post-wave-1 questionnaire; the same attention is <u>not</u> paid, however, to the onset of a new spell at the

seam. We recommended that such procedures be developed and tested. Their general form seems fairly straightforward. When a respondent reports that a new spell of some characteristic has started – that is, reports a "yes" for a characteristic that was not a "yes" at the end of the previous wave's reference period – then questioning about the start of that spell should refer to what is known from the previous wave, e.g.: "When we interviewed you back in early March you weren't receiving Food Stamps. When did you start to receive them?" Addressing, in this or some similar manner, the continuation of the *absence* of some characteristic across the seam is likely to produce additional gains in the overall quality of transition data.

Acknowledgments
Many people provided assistance with various aspects of the work presented in this chapter. At the risk of inadvertently omitting key names, we specifically note the following: our immediate colleagues for many years on the SIPP Methods Panel project – Anna Chan, the late, great Pat Doyle, Tim Gilbert, Elaine Hock, Heather Holbert, and Ceci Villa; Aref Dajani, for assistance with data file preparation; and our Census Bureau colleagues who provided many useful comments on early drafts of this paper – John Boies, Kathy Creighton, and Chuck Nelson.

References

Brown, A., Hale, A., and Michaud, S. (1998), "Use of Computer Assisted Interviewing in Longitudinal Surveys," in M. Couper, R. Baker, J. Bethlehem, C. Clark, J. Martin, W. Nicholls, and J. O'Reilly (eds.), *Computer Assisted Survey Information Collection*, New York: John Wiley & Sons, 185-200.

Bruun, M. (2005), "Fifth Wave SIPP Observation," unpublished U.S. Census Bureau memorandum to Jeff Moore, Statistical Research Division, July 12, 2005.

Burkhead, D. and Coder, J. (1985), "Gross Changes in Income Recipiency from the Survey of Income and Program Participation," Proceedings of the American Statistical Association, Social Statistics Section, 351-356.

Cantor, D. and Levin, K. (1991), "Summary of Activities to Evaluate the Dependent Interviewing Procedure of the Current Population Survey," Westat, Inc.: report submitted to the Bureau of Labor Statistics (Contract No. J-9-J-8-0083).

Coder, J., Burkhead, D., Feldman-Harkins, A., and McNeil, J. (1987), "Preliminary Data from the SIPP 1983-84 Longitudinal Research File," SIPP Working Paper No, 8702, Washington, DC: US Census Bureau

Collins, C. (1975), "Comparison of Month-to-Month Changes in Industry and Occupation Codes with Respondent's Report of Change: CPS Job Mobility Study," U.S. Census Bureau, Response Research Staff Report No. 75-5, May 15, 1975.

Cotton, C. and Giles, P. (1998), "The Seam Effect in the Survey of Labour and Income Dynamics," Statistics Canada: SLID Research Paper Series, Catalogue No. 98-18, November 1998.

Corti, L. and Campanelli, P. (1992), "The Utility of Feeding Forward Earlier Wave Data for Panel Studies," in Westlake, A. et al. (eds), Survey and Statistical Computing, London: Elsevier, 109-118.

Czajka, J. (1983), "Subannual Income Estimation," chapter 7 in David, M. (ed), Technical, Conceptual, and Administrative Lessons of the Income Survey Development Program (ISDP), New York: Social Science Research Council, 87-97.

Davis, J. (2005), "Observation Notes of Personal Interviews for the SIPP Wave 5: Evanston, Skokie, and Wilmette, IL," unpublished U.S. Census Bureau memorandum to Doug Geverdt and Kurt Bauman, Population Division, June 7, 2005.

Dibbs, R., Hale, A., Loverock, R., and Michaud, S. (1995), "Some Effects of Computer-Assisted Interviewing on the Data Quality of the Survey of Labour and Income Dynamics," Statistics Canada: SLID Research Paper Series, Catalogue No. 95-07, April 1995.

Doyle, P., Martin, E., and Moore, J. (2000), "Methods Panel to Improve Income Measurement in the Survey of Income and Program Participation," *Proceedings* of the Section on Survey Research Methods, Washington, DC: American Statistical Association, 953-958.

Gilbert, T. (2005), "SIPP 2004 Panel Wave 4 Observation," unpublished U.S. Census Bureau report, May 6, 2005.

Grondin, C. and Michaud, S. (1994), "Data Quality of Income Data Using Computer Assisted Interviewing: SLID Experience," Statistics Canada: SLID Research Paper Series, Catalogue No. 94-15, August 1994.

Hale, A. and Michaud, S. (1995), "Dependent Interviewing: Impact on Recall and on Labour Market Transitions, Statistics Canada: SLID Research Paper Series, Catalogue No. 95-06 (Product Registration Number 75F0002M), March 1995.

Hill, D. (1987), "Response Errors Around the Seam: Analysis of Change in a Panel with Overlapping Reference Periods," Proceedings of the Section on Survey Research Methods, Washington, DC: American Statistical Association, 210-215.

Hill, D. (1994), "The Relative Empirical Validity of Dependent and Independent Data Collection in a Panel Survey," *Journal of Official Statistics* 10: 359-380.

Holmberg, A. (2004), "Pre-Printing Effects in Official Statistics: An Experimental Study," Journal of Official Statistics, 20: 341-355.

Hoogendoorn, A. (2004), "A Questionnaire Design for Dependent Interviewing that Addresses the Problem of Cognitive Satisficing," *Journal of Official Statistics* 20: 219-232.

Jabine, T., King, K., and Petroni, R. (1990), "Survey of Income and Program Participation Quality Profile," Washington, DC: U.S. Census Bureau.

Jackle, A. and Lynn, P. (2004), "Dependent Interviewing and Seam Effects in Work History Data," Working Paper #2005-24 of the Institute for Social and Economic Research, Colchester, UK: University of Essex.

Kalton, G. (1998), "SIPP Quality Profile 1998," SIPP Working Paper #230, Washington, DC: U.S. Census Bureau.

Kalton, G. and Miller, M. (1991), "The Seam Effect with Social Security Income in the Survey of Income and Program Participation," *Journal of Official Statistics* 7: 235-245.

LeMaitre, G. (1992), "Dealing with the Seam Problem for the Survey of Labour and Income Dynamics," Statistics Canada: SLID Research Paper Series, Catalogue No. 92-05, August 1992.

Lynn, P., Jackle, A., Jenkins, S., and Sala, E. (2004), "The Effects of Dependent Interviewing on Responses to Questions on Income Sources," Working Paper #2004-16 of the Institute for Social and Economic Research, Colchester, UK: University of Essex.

Marquis, K. and Moore, J. (1990), "Measurement Errors in the Survey of Income and Program Participation," Proceedings of the U.S. Census Bureau's Annual Research Conference, 721-745.

Martini, A. (1989), "Seam Effect, Recall Bias, and the Estimation of Labor Force Transition Rates from SIPP," Proceedings of the American Statistical Association, Section on Survey Research Methods, 387-392.

Mathiowetz, N. and McGonagle, K. (2000), "An Assessment of the Current State of Dependent Interviewing in Household Surveys," *Journal of Official Statistics* 16: 401-418.

Michaud, S., Dolson, D., Adams, D., and Renaud, M. (1995), "Combining Administrative and Survey Data to Reduce Respondent Burden in Longitudinal Surveys," Statistics Canada: SLID Research Paper Series, Catalogue No. 95-19 (Product Registration Number 75F0002M), August 1995.

Moore, J. (2004), "SIPP 2004 Panel Wave 2 Observation Report," unpublished U.S. Census Bureau report, September 27, 2004.

Moore, J. (forthcoming a), "The Effects of Questionnaire Design Changes on Asset Income Amount Nonresponse in Waves 1 and 2 of the 2004 SIPP Panel," Washington, DC: U.S. Census Bureau, Research Report Series (Survey Methodology), forthcoming.

Moore, J. (forthcoming b), "The Effects of Questionnaire Design Changes on General Income Amount Nonresponse in Waves 1 and 2 of the 2004 SIPP Panel," Washington, DC: U.S. Census Bureau, Research Report Series (Survey Methodology), forthcoming.

Moore, J. and Griffiths, J. (2003), "Asset Ownership, Program Participation, and Asset and Program Income: Improving Reporting in the Survey of Income and Program Participation," *Proceedings* of the American Statistical Association [CD-ROM], Alexandria, VA: American Statistical Association.

Moore, J. and Kasprzyk, D. (1984), "Month-to-Month Recipiency Turnover in the ISDP," Proceedings of the American Statistical Association, Section on Survey Research Methods, 726-731.

Moore, J. and Marquis, K. (1989), "Using Administrative Record Data to Evaluate the Quality of Survey Estimates," Survey Methodology 15: 129-143.

Moore, J. Pascale, J., Doyle, P., Chan, A., and Griffiths, J. (2004). "Using Field Experiments to Improve Instrument Design: The SIPP Methods Panel Project." In Presser, S., Rothgeb, J., Couper, M., Lessler, J., Martin, E., Martin, J., and Singer, E. (eds.), Methods for Testing and Evaluating Survey Questionnaires. New York: Wiley, Chapter 10, pp. 189-207.

Moore, J., Stinson, L., and Welniak, E. (2000), "Income Measurement Error in Surveys: A Review," Journal of Official Statistics, 16: 331-361.

Murray, T., Michaud, S., Egan, M., and LeMaitre, G. (1991), "Invisible Seams? The Experiences with the Canadian Labor Market Activity Survey," Proceedings of the US Census Bureau's Annual Research Conference, 715-729.

Polivka, A. and Rothgeb, J. (1993), "Redesigning the CPS Questionnaire," Monthly Labor Review, September 1993, 10-28.

Rips, L., Conrad, F., and Fricker, S. (2003), "Straightening the Seam Effect in Panel Surveys," Public Opinion Quarterly, 67: 522-554.

Ryscavage, P. (1993), "The Seam Effect in SIPP's Labor Force Data: Did the Recession Make it Worse?" SIPP Working Paper #180, Washington, DC: U.S. Census Bureau.

Sala, E. and Lynn, P. (2004), "Measuring Change in Employment Characteristics: The Effects of Dependent Interviewing," Working Paper #2004-26 of the Institute for Social and Economic Research, Colchester, UK: University of Essex.

Stanley, J. and Safer, M. (1997), "'Last Time You Had 78, How Many Do You Have Now?' The Effect of Providing Previous Reports on Current Reports of Cattle Inventories," Proceedings of the American Statistical Association, Section on Survey Research Methods, 875-880.

Weidman, L. (1986), "Investigation of Gross Changes in Income Recipiency from the Survey of Income and Program Participation," Proceedings of the American Statistical Association, Section on Survey Research Methods, 231-236.

Weinberg, D. (2002), "The Survey of Income and Program Participation – Recent History and Future Developments," SIPP Working Paper Series No. 232, Washington DC: U.S. Census Bureau, June 2002.

U.S. Census Bureau (2001), *SIPP Users' Guide*, Washington, DC: U.S. Census Bureau.

Young, N. (1989), "Wave-Seam Effects in the SIPP," Proceedings of the American Statistical Association, Section on Survey Research Methods, 393-398.

TABLE 1: PRELIMINARY SEAM BIAS ANALYSIS RESULTS FOR VARIOUS NEED-BASED AND OTHER CHARACTERISTICS (AVERAGED ACROSS INDIVIDUAL ANALYSES OF WAVES 1 AND 2, WAVES 2 AND 3, AND WAVES 3 AND 4) IN THE 2001 AND 2004 SIPP PANELS

Data source: preliminary, internal, unedited ("TransCASES") questionnaire data files, unweighted

CHARACTERISTIC	PANEL	ANALYSIS N's n = Analyzed Interviews Δ = Total Observed Changes			AVERAGE % OF ALL CHANGES THAT WERE SEAM CHANGES (see Notes)	AVERAGE MONTH-TO-MONTH CHANGE RATES (%)			CHANGE RATE RATIO: SEAM/ OFF-SEAM	AVERAGE "DIRECTIONAL" CHANGE RATES AT THE SEAM	
		W1-W2	W2-W3	W3-W4		All Month-Pairs	Off-Seam	Seam		% of "Yes's" that changed to "No"	% of "No's" that changed to "Yes"
PART 1: CHARACTERISTICS WITH NEW DEPENDENT INTERVIEWING PROCEDURES IN THE 2004 PANEL											
A. Need-Based Programs											
"public" health insurance coverage[1/]	2001	n = 45,560 Δ = 2,208	n = 41,572 Δ = 1,831	n = 40,799 Δ = 1,627	**81.9%*****	0.6	0.13	3.6	27.7	27.1	1.8
	2004	n = 67,211 Δ = 2,008	n = 62,227 Δ = 1,625	n = 60,473 Δ = 1,493	**57.6%*****	0.4	0.21	1.6	7.6	5.0 (-82%)	1.2 (-33%)
receipt of Federal SSI (Supplemental Security Income)[2/]	2001	n = 1,596 Δ = 334	n = 1,563 Δ = 352	n = 1,587 Δ = 352	84.6%**	3.1	0.55	18.5	33.6	13.9	88.6
	2004	n = 1,826 Δ = 500	n = 1,917 Δ = 618	n = 1,929 Δ = 687	77.6%**	4.5	1.2	24.7	20.6	19.4 (+40%)	86.1 (-3%)
receipt of Veterans' Compensation/ Pensions[2/]	2001	n = 641 Δ = 118	n = 619 Δ = 103	n = 621 Δ = 94	**82.2%*****	2.4	0.50	13.7	27.4	10.4	82.4
	2004	n = 1,096 Δ = 118	n = 1,041 Δ = 110	n = 1,006 Δ = 96	**47.5%*****	1.5	0.90	4.9	5.4	3.8 (-63%)	52.1 (-37%)

A. Need-Based Programs (cont'd)											
receipt of AFDC/ TANF[2/]	2001	n = 575 Δ = 345	n = 534 Δ = 326	n = 479 Δ = 267	**55.5%*** **	8.4	4.4	32.6	7.4	29.0	52.8
	2004	n = 835 Δ = 499	n = 786 Δ = 461	n = 734 Δ = 416	**36.3%*** **	8.3	6.2	21.2	3.4	18.1 (-38%)	38.1 (-28%)
receipt of WIC (Women, Infants, Children) benefits[2/]	2001	n = 1,172 Δ = 546	n = 1,130 Δ = 436	n = 1,147 Δ = 443	**52.7%*** **	5.9	3.3	21.7	6.6	17.0	68.3
	2004	n = 1,713 Δ = 672	n = 1,703 Δ = 648	n = 1,667 Δ = 635	**34.6%*** **	5.5	4.2	13.3	3.2	9.0 (-47%)	49.0 (-28%)
receipt of Food Stamps[2/]	2001	n = 2,104 Δ = 976	n = 2,033 Δ = 884	n = 2,008 Δ = 823	**52.2%*** **	6.2	3.5	22.7	6.5	18.1	62.7
	2004	n = 3,844 Δ = 1,512	n = 3,837 Δ = 1,435	n = 3,802 Δ = 1,377	**34.4%*** **	5.4	4.1	13.0	3.2	9.2 (-49%)	47.9 (-24%)
B. Other (Non-Need-Based) Characteristics											
school enrollment	2001	n = 50,948 Δ = 6,188	n = 47,458 Δ = 9,629	n = 46,920 Δ = 5,086	**28.9%*** **	2.1	1.7	4.3	2.5	23.3	2.1
	2004	n = 73,919 Δ = 8,082	n = 67,729 Δ = 13,549	n = 65,758 Δ = 6,189	**17.7%*** **	1.9	1.8	2.7	1.5	8.7 (-63%)	2.1 (0%)
private health insurance coverage	2001	n = 51,378 Δ = 5,607	n = 47,856 Δ = 4,971	n = 47,064 Δ = 4,616	**73.4%*** **	1.5	0.46	7.6	16.5	5.4	14.1
	2004	n = 78,162 Δ = 6,483	n = 73,217 Δ = 6,627	n = 71,128 Δ = 5,991	**62.8%*** **	1.2	0.53	5.4	10.2	3.2 (-41%)	11.3 (-20%)
receipt of Social Security[2/]	2001	n = 9,929 Δ = 780	n = 9,569 Δ = 753	n = 9,442 Δ = 655	**79.4%*** **	1.1	0.26	6.0	23.1	3.8	92.8
	2004	n = 14,999 Δ = 1,321	n = 14,610 Δ = 1,378	n = 14,403 Δ = 1,173	**62.2%*** **	1.3	0.55	5.5	10.0	3.0 (-21%)	87.3 (-6%)

B. Other (Non-Need-Based) Characteristics (cont'd)											
receipt of Workers' Compensation[2/]	2001	n = 259 ▵ = 222	n = 194 ▵ = 151	n = 178 ▵ = 128	49.3%**	11.2	6.7	38.4	5.7	37.3	41.8
	2004	n = 352 ▵ = 242	n = 323 ▵ = 220	n = 326 ▵ = 238	36.1%**	10.0	7.4	25.3	3.4	23.8 (-36%)	30.6 (-27%)
receipt of child support payments[2/]	2001	n = 1,460 ▵ = 860	n = 1,470 ▵ = 817	n = 1,479 ▵ = 750	**41.6%*** **	7.8	5.4	22.9	4.2	17.2	63.1
	2004	n = 2,476 ▵ = 1,403	n = 2,408 ▵ = 1,298	n = 2,289 ▵ = 1,177	**31.0%*** **	7.7	6.4	15.5	2.4	10.6 (-38%)	48.6 (-23%)
receipt of alimony[2/]	2001	n = 172 ▵ = 63	n = 158 ▵ = 57	n = 157 ▵ = 53	56.3%**	5.1	2.6	19.9	7.7	16.6	60.6
	2004	n = 215 ▵ = 84	n = 220 ▵ = 93	n = 207 ▵ = 85	41.1%**	5.8	4.0	16.8	4.2	14.7 (-11%)	33.8 (-44%)
receipt of private pensions[2/]	2001	n = 3,146 ▵ = 612	n = 2,894 ▵ = 489	n = 2,877 ▵ = 480	**87.6%*** **	2.5	0.37	15.5	41.9	11.9	87.4
	2004	n = 4,681 ▵ = 417	n = 4,570 ▵ = 305	n = 4,523 ▵ = 287	**58.5%*** **	1.1	0.44	4.7	10.7	3.5 (-71%)	71.2 (-19%)
receipt of Federal Civil Service pensions[2/]	2001	n = 478 ▵ = 104	n = 397 ▵ = 65	n = 375 ▵ = 54	**94.8%*** **	2.5	0.15	16.6	110.7	14.0	97.2
	2004	n = 761 ▵ = 50	n = 745 ▵ = 35	n = 728 ▵ = 55	**70.2%*** **	0.9	0.31	4.4	14.2	3.3 (-76%)	96.7 (-1%)
receipt of "wealth" income (annuities; estates/trusts)[2/]	2001	n = 271 ▵ = 241	n = 199 ▵ = 181	n = 185 ▵ = 158	**67.7%*** **	12.6	4.8	59.8	12.5	60.3	51.9
	2004	n = 456 ▵ = 320	n = 378 ▵ = 159	n = 363 ▵ = 165	**31.1%*** **	7.5	6.1	15.9	2.6	13.6 (-77%)	28.8 (-45%)

PART 2: CHARACTERISTICS WITH THE SAME DEPENDENT INTERVIEWING PROCEDURES IN BOTH PANELS											
Medicare	2001	n = 51,950 ▵ = 706	n = 48,508 ▵ = 429	n = 47,809 ▵ = 404	67.9%#	0.15	0.05	0.71	14.2	0	0.86
	2004	n = 75,713 ▵ = 868	n = 70,152 ▵ = 483	n = 68,067 ▵ = 445	69.5%#	0.12	0.04	0.59	14.8	0.02	0.71
employment at the same job3/	2001	n = 40,684 ▵ = 15,410	n = 29,533 ▵ = 11,811	n = 36,562 ▵ = 13,140	36.2%@	5.4	4.0	13.7	3.4	8.8	33.6
	2004	n = 52,799 ▵ = 17,510	n = 50,862 ▵ = 22,052	n = 48,873 ▵ = 20,879	37.7%@	5.7	4.1	15.3	3.7	9.4	44.8

Table 1 Notes

Table entries represent the simple mean of three estimates derived from individual analyses of each of three pairs of SIPP waves – waves 1 and 2, 2 and 3, and 3 and 4. Each individual analysis included only "adults" (people age 15+) for whom a completed interview was obtained in both waves, and excluded cases with missing data in either seam month. Additional analysis restrictions are as follows:

1/ Analysis restricted to people aged 20+.

2/ Analysis restricted to people who reported at least one month of receipt of this income type in either of the two successive survey waves.

3/ The unit of analysis is the job, rather than the person; the analysis includes all jobs held in at least one month in either of the two successive survey waves. Note that an error in the 2001 wave 3 rotation 1 instrument resulted in the inability to link wave 3 jobs with wave 2 jobs. The error only affected rotation group 1 – the instrument operated correctly for rotation groups 2, 3, and 4. Therefore, the data from rotation 1 are excluded from the 2001 W2-W3 analysis.

"AVERAGE % OF ALL CHANGES THAT WERE SEAM CHANGES" – statistical analysis notes:

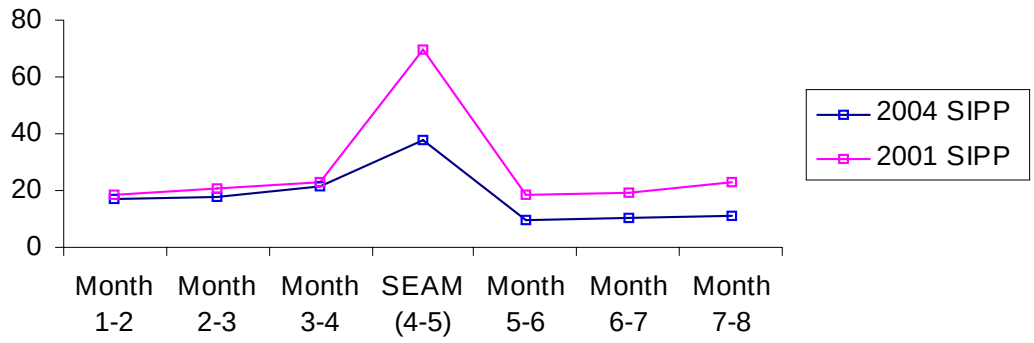
*** [and estimates in bold font] According to a simple t-test of the difference between two proportions, the 2001-2004 difference was statistically significant in each of the three individual analyses.

** According to a simple t-test of the difference between two proportions, the 2001-2004 difference was significant in two of the three individual analyses, and in the same direction (but non-significant) in the third.

All three individual 2001-2004 differences were non-significant.

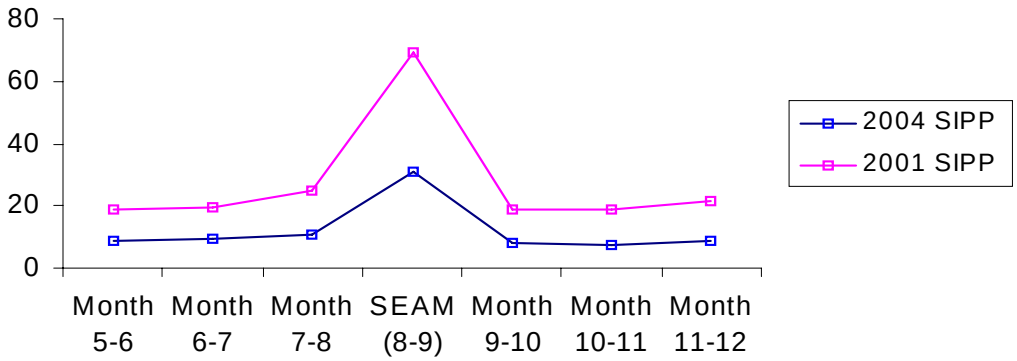
@ All three individual 2001-2004 differences were statistically significant, but the sign of the difference was inconsistent across the three comparisons.

Figure 1. Waves 1-2: Percent of Cases with +/- 5% Difference in Month-to-Month Job Earning Amounts
(same job across waves - monthly or paycheck reporters only)



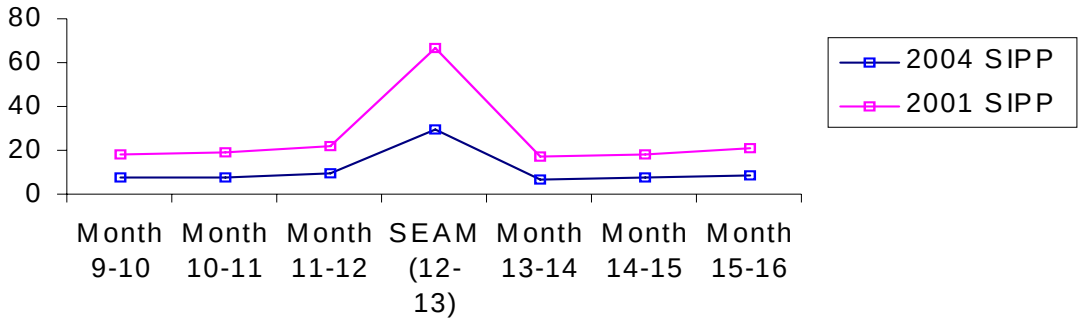
	M1-2	M2-3	M3-4	M4-5	M5-6	M6-7	M7-8
2004 (N)	6783	6952	7114	7141	7714	7557	7315
+ /- 5% △	1176	1217	1538	2696	721	768	788
2001 (N)	18092	18519	19060	17250	18202	17680	17136
+ /- 5% △	3297	3883	4323	11986	3428	3419	3917

Figure 2. Waves 2-3: Percent of Cases with +/- 5% Difference in Month-to-Month Job Earning Amounts
(same job across waves - monthly or paycheck reporters only)



	M5-6	M6-7	M7-8	M8-9	M9-10	M10-11	M11-12
2004 (N)	6475	6417	6316	6205	6299	6192	6071
+ /- 5% △	580	617	675	1926	489	475	539
2001 (N)	11966	12245	12550	11542	12665	12332	12108
+ /- 5% △	2223	2370	3119	7970	2367	2338	2605

Figure 3. Waves 3-4: Percent of Cases with +/- 5% Difference in Month-to-Month Job Earning Amounts
(same job across waves - monthly or paycheck reporters only)



	M9-10	M10-11	M11-12	M12-13	M13-14	M14-15	M15-16
2004 (N)	5748	5766	5753	5678	5598	5503	5413
+ /- 5% △	461	463	562	1708	395	431	459
2001 (N)	15377	15882	16464	15205	16299	15896	15576
+ /- 5% △	2756	3073	3606	10100	2866	2936	3208