Tackling Seam Bias Through Questionnaire Design

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1. Introduction

This chapter examines the impact of dependent interviewing procedures on “seam bias,” a phenomenon peculiar to longitudinal panel 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 – often by a factor of 10 or more. The presence of seam bias almost always signals 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 its essential nature – too little change within the reference period of a single interview, and too much at the seam. Attempts to control seam bias have met with some success, but have been limited primarily to employment-related characteristics.

The U.S. Census Bureau recently implemented new procedures in the 2004 panel of the Survey of Income and Program Participation (SIPP) in an attempt to significantly reduce seam bias for a wide variety of characteristics. The primary tool for accomplishing this was a more extensive and more focused use of dependent interviewing (DI) procedures, wherein “substantive answers from previous interviews are fed forward and used to tailor the wording and routing of questions” in the next interview (Jäckle, this volume, pXX). This chapter describes those procedures, and examines their impact on estimates of month-to-month change across the initial waves of the new panel for reports of participation in government transfer programs, school enrollment, employment, earnings, and health insurance coverage, through a comparison with similar estimates derived from the most recent prior SIPP panel, the 2001 panel. We find evidence of significant improvement with the new procedures – estimates of month-to-month change from the initial 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: Section 2 briefly describes the seam bias phenomenon, and summarizes work which has attempted to understand and ameliorate it. Section 3 provides a brief background on SIPP, and describes and contrasts its old and new DI procedures. 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. Section 5 offers our conclusions, including implications of the current findings for future research.

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2. Previous Research on Seam Bias

Seam bias 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) document the effect quantitatively. 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 Panel Study of Income Dynamics (Hill, 1987), and the U.S. Census Bureau’s quasi-longitudinal labor force survey, the Current Population Survey (CPS) (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; Jäckle and Lynn, 2004). LeMaître (1992), in an excellent general review, summarizes the first decade of seam bias research in terms that still seem apt: “seam effects would appear to be a general problem with current longitudinal surveys, regardless of differences in design [p5].” Marquis and Moore (1990) confirm that seam bias severely compromises the statistical utility of estimates of change.

Since the very beginning, 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, Lynn and Sala (2006, 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 LeMaître (1992). Rips, Conrad, and Fricker (2003) tie these phenomena to a combination of 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., Lynn and Sala, 2006).
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), Mathiowetz and McGonagle (2000), and Jäckle (this volume). 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 CPS 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 LeMaître (1992). More recently, in Great Britain, Lynn and colleagues have experimented with different forms of DI for labor force and other types of questions; they find somewhat inconsistent effects in different circumstances for different forms of DI, but in all cases find the level of spurious change to be consistently highest under conditions of non-dependent interviewing (Jäckle and Lynn, 2004; Lynn and Sala, 2006; Lynn et al., forthcoming).

3. SIPP and its Dependent Interviewing Procedures

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 conducted with a computer-assisted questionnaire; the first interview is administered in-person, subsequent interviews are generally 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 the 2004 panel, SIPP made much use of DI in its “control card” questions about the household roster and the demographic characteristics of household members, but little in the main body of the questionnaire. In the survey’s early panels this was in part a function of its paper-and-pencil interview mode, which is much less conducive to a smooth and accurate administration of dependent questions than is computer-assisted interviewing (CAI) (Brown, Hale, and Michaud, 1998; Corti and Campanelli, 1992). However,
even after the introduction of CAI in the 1996 SIPP panel, neither that panel nor those that followed made much more use of such questions than did their predecessors. In the 2001 SIPP panel, for example (the most recent SIPP panel before the 2004 redesign), 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 without any reference to past reports.

Other areas of the 2001 questionnaire employed dependent-like procedures which offered respondents general reminders of their prior reports, but then fell back on completely non-dependent wording for the actual question regarding the current wave. For example: “Last time I recorded that you received Foster Child Care payments. Did you receive any Foster Child Care payments at any time between [MONTH 1] 1st and today?” Extending Jäckle’s (this volume) terminology, we might label this the “remind, ignore” or “remind only” approach. This form of DI offers one clear advantage over fully dependent (Jäckle: “remind, continue,” or “remind, confirm/still”) questioning: it is simple to implement, because it does not require any restructuring of the initial questionnaire beyond the simple addition of the “Last time I recorded...” introduction. Major drawbacks of the form, however, are that it only weakly anchors the respondent’s current report to the known past, does little to invite consideration of whether that past state has continued or changed, and – not unlike a non-dependent question – leaves the respondent focused primarily on the immediate reporting period.

3.2. Development of new DI procedures

In the mid-1990’s, concerns about increasing nonresponse and attrition led the U.S. Census Bureau to launch a research and development program to redesign the SIPP questionnaire for the 2004 SIPP panel. The main focus of this effort was “interview process” improvements that would yield a less burdensome interview. Data quality improvements were also targeted, however, including a reduction in seam bias, which was found not to have changed for the better with the introduction of CAI procedures in the 1996 panel (Moore et al., 2004). Thus, we designed new procedures to reduce seam bias, primarily through an increased emphasis on the use of DI, as follows:

(1) With the advent of computer-assisted interviewing in 1996, SIPP expanded its traditional, strict four-preceding-calendar-months reference period to also include the current month, up to the date of the interview. This change was motivated more by aesthetic, rather than substantive, considerations – it permitted simpler question wording (“Since [MONTH1] 1st...” rather than “At any time between [MONTH1] 1st and the end of [MONTH4]...”), and made for a more natural response process, since it allowed respondents to report on very recent events. The “month 5” data were largely ignored, however. No attempt was made to exploit 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, because the interview month of one interview wave is the first month of the next wave’s reference period. That situation changed in the 2004 panel questionnaire; interview month information from one survey wave is now used in the next wave to decide whether to ask a dependent question, and, if so, the specific form of that question.

(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 actually led us to be more restrictive about when to use dependent procedures. In SIPP’s 2001 questionnaire, an event that occurred in any month of the previous interview’s reference period was sufficient to trigger the “Last time I recorded...” question introduction 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.” The new instrument, in contrast, only considers the previous interview’s months 4 (“last month”) and 5 (the interview month) in determining 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 reported in the interview month of the prior wave (i.e., the first month of the current wave’s reference period), triggers an initial confirmatory question in the next interview, e.g.: “Last time I recorded that you received Food Stamps in April. Is that correct?” A “yes” confirms the person’s status for the current reference period, and 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 questionnaire asks about the remainder of the current reference period, e.g.: “Did you receive any Food Stamps since May 1st?”

- A different strategy is used for events of interest reported in “month 4” of the prior wave (the last month of that wave’s reference period), but not in the prior wave’s interview month. In almost all cases the interview month report covers only a portion – and often a very small portion – of that month, so a “no” report actually could mean “not yet.” Thus, where the “month 4” report is a “yes,” and “month 5” is a “no” (“not yet”), the next wave’s interview recalls the “month 4” circumstances and asks whether they continued into the current wave: “Last time I recorded that you received Food Stamps in March. Did you continue to receive Food Stamps after April 1st?” The response establishes the person’s status for the current reference period; a “yes” triggers later questions about each individual month.

- 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) New DI techniques are also used in the 2004 questionnaire as a follow-up procedure, to reduce nonresponse to income amount questions. Questions about income amounts now begin as non-dependent questions, exactly as before, but switch to a dependent format in the event of an

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'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.'
initial nonresponse. This “reactive” form of DI (see Lynn et al., forthcoming) is in place for all income amount questions in the 2004 questionnaire beginning in wave 2; no such procedures had been employed in any previous SIPP panel. Initial evidence suggests that these procedures have been quite successful at reducing item nonresponse (Moore, 2006a; Moore, 2006b). We also thought, however, that we might see some impact of these new procedures on income amount transitions, although perhaps to only a limited extent due to their more limited use. The results of this investigation, too, are summarized briefly in section 4.

3.3. Testing and refining the new procedures

The project to develop the new questionnaire included a series of three field experiments to evaluate and refine the revised procedures. Doyle, Martin, and Moore (2000) describe the design of the field experiments; Moore, et al.(2004) also cover field test design issues, and provide information concerning the full array of 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 examines the impact of the new DI procedures on seam bias for program participation and other “spell”-type characteristics. 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 does not cover all characteristics included in SIPP; those selected were chosen with an eye toward breadth and importance, but primarily because they could be analyzed reasonably easily with preliminary, internal data files. Our use of preliminary files means that the results presented here may differ from those obtained from future analyses using final, edited data. We use the best evaluation 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 (e.g., that sample design and field staff differences and the mere passage of time can be ignored). While we acknowledge these limitations, we have no reason to believe that they actually influence our findings in important ways, or 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, so corresponding waves of the two panels cover the same calendar months, three years apart. For each panel we carried out three separate seam bias analyses, 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 unnecessary loss of otherwise useful cases absent from only one or two of the four waves (e.g., attritors and in-movers). Each analysis excludes 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 months at the seam.

Table 1 summarizes the results of these analyses, showing, by panel, the simple average of the three separate estimates for each statistic. The simple average accords equal weight to the three estimates, 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 to avoid giving extra weight to the wave 1-2 pair, which differs from the others in that it includes the only completely non-dependent interview (wave 1) in the entire panel, and to avoid giving extra weight to some periods of the calendar year at the expense of others just because of SIPP’s arbitrary interview schedule. We doubt that the decision to treat the three wave-pairs equally has any important impact on our results or conclusions, primarily because the differences among the three for any of the characteristics examined are quite minimal.

Table 1 is in two parts. Part 1 summarizes the results for characteristics which were captured with very 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 income sources and characteristics. Part 2 presents results for two characteristics whose measurement procedures did not differ across the two SIPP panels. In the case of Medicare (the US government health insurance program for the elderly), once a person is eligible and enrolls he/she is covered for life. Thus in both 2001 and 2004 there were no DI procedures – a “yes” response in one wave was simply carried over automatically to all subsequent waves without asking, and a “no” simply caused the non-dependent question to be re-asked in the next wave. For jobs, on the other hand, both panels used the same fully dependent, “remind, confirm/still” DI procedures.

The summary statistics (column headings) in Table 1 are defined as follows:

“Analysis N’s” – This shows, for each characteristic and each wave-pair, the total number of cases included in the analysis, and the total number of month-to-month transitions observed in 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\(^3\)) characteristics it is also limited to those who provided at least one “yes” value for any month in either wave.

“Average % of All Changes That Were At the Seam” – This column shows the number of month-to-month changes – in either direction – observed at the seam as a percentage of all observed changes (averaged across the three separate analyses). In the absence of seam bias, we would expect about one-seventh (14%) of all changes to be seam changes.

\(^3\)The definition of the analysis sample for each characteristic simply follows SIPP’s conventions for how the questionnaire data are stored (as a “person” record, a “job” record, a “program” record, etc.). The only practical impact of this decision is whether cases with all “no” values across all eight months are included or excluded.
since the seam is one of seven month-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 analyses.

“Average Month-to-Month Change Rates (%)” – These columns show the likelihood of observing a change from one month to the next, in either direction, for all month-pairs combined, and separately for “off-seam” and seam month-pairs. We have no absolute standard for assessing the quality of these rates, which are a function of both the particular “volatility” of the characteristic in question and how the analysis universe is defined.

“Change Rate Ratio: Seam/Off-Seam” – This column divides the seam change rate by the off-seam rate to produce an estimate of how much the seam change rate is inflated relative to 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.) Here we can apply an objective quality standard: in the absence of any seam bias the likelihood of a change across the seam would be about the same as for any other pair of months, and thus the change rate ratio would be close to 1.0.

“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/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 each characteristic; the no-to-yes rate, in 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 or dependent-like (“remind, ignore”) 2001 procedures were significantly less effective at controlling seam bias than are the fully dependent (“remind, continue” or “remind, confirm/still”) procedures introduced in 2004. This is readily apparent in the proportion of all month-to-month changes observed at the seam: across all 15 characteristics subject to the new DI procedures 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. The same result can be seen in the “Change Rate Ratio” column, where the ratio of the seam change rate to the off-seam change rate is always closer to 1.0 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. 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 and employment at a particular job, the use of very similar interviewing procedures yields very similar seam bias results. This finding offers strong support for the notion that the differences shown in Part 1 are due to the new DI procedures, 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 across a wide range of characteristics. Seam bias afflicts the measurement of characteristics associated with rich and poor alike, and the improved DI procedures introduced in 2004 were similarly unrestricted in their impact. We divided Part 1 of Table 1 into two categories of characteristics – “need-based” programs to assist the low income population, and other income sources and characteristics which apply to the general population (or which in some cases are skewed toward the wealthy) – 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.

4. The positive effects of DI are a result of reduced change at the seam and increased change off the seam. As noted, seam bias has been shown to be the net effect of too many changes observed at the seam and too few changes observed elsewhere (Moore and Marquis, 1989). The new DI procedures 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 all but two of the characteristics subject to 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 pattern makes sense because the dependent procedures employed in 2004 are “asymmetrical” (Murray, et al., 1991) – they only apply to those who are in a “yes” status (enrolled, covered, participating, etc.) at the end of the prior interview.

Given that focus, why did the new DI procedures have any impact at all on no-to-yes transitions? That they did is obvious: with the sole exception of school enrollment, 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, as contrasted with the indiscriminate approach that it replaced. 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 old DI format, no mention was made of 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 masked the fact that the respondent had already, in the prior wave, reported the spell’s termination, thus subtly encouraging him or her to mis-recall a new spell as a continuation of an old one, resulting in a false no-to-yes change report at the seam. (LeMaitre (1992) notes this flaw in the previous SIPP design, and its possible negative consequences.)
Interestingly, 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. Kalton and Miller (1991), using data from an early SIPP panel, apply this same definition to examine seam bias in Social Security payment amounts. They find a large seam effect, consisting of almost nonexistent month-to-month change within a single wave, contrasted with change at the seam about two-thirds of the time.

6. Despite the improvements due to DI, much seam bias still remains. Improvement in seam bias in the SIPP 2004 panel is unmistakable; that bias is far from having been eradicated is equally unmistakable. With the single exception of school enrollment, every characteristic – notwithstanding its improvement relative to 2001 – still displays, in 2004, an overabundance of changes at the seam. For example: in the proportion of all changes that are seam changes, the best outcomes (again, excluding school enrollment) are still above 30%, which is more than twice as high as would be expected if there were no seam bias. Similarly, in the “change rate ratios,” 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.

Although our purpose here is to examine general trends, rather than the results for particular characteristics, we want to focus briefly on the school enrollment results, which stand out from the others in the much lesser extent to which they are afflicted with seam bias. Even “pre-improvement,” in 2001, the seam bias estimates for school enrollment are lower than for any other characteristic after the addition of improved DI procedures in 2004. And the “post-improvement” results in 2004 arguably contradict the notion that “much” seam bias remains in the DI-improved estimates. We suspect that the unique profile for school enrollment is due to its familiar seasonal patterns, which makes reporting in terms of calendar months a relatively easy task compared to other characteristics. This suggests that, given reasonable cues to begin with, respondents can report transitions with reasonable accuracy. And with the addition of other useful cues – specifically, carefully-designed DI procedures – it is possible for respondents to produce reports of transitions that are largely devoid of error.

4.2. Seam bias evaluation for income amount transitions

Beginning in the second interview wave, the redesigned 2004 panel SIPP questionnaire introduced dependent questions into its procedures for capturing income amounts. As noted, these new DI techniques are used in a “reactive” manner (Lynn et al., forthcoming), as a follow-up to an initial nonresponse to an income amount question. Although the main purpose of this change was to reduce item nonresponse (Moore, 2006a; Moore, 2006b), here we examine its impact on income amount transitions, focusing specifically on monthly earnings from a job. We use an arbitrary definition of an amount “change” – namely, a difference in earnings amounts between two adjacent months of plus or minus 5 percent. We again analyze each pair of waves separately, and we restrict the analysis sample to those interviewed in each wave of the pair who held the same job in each wave. Occasional job changes within a wave, and occasional nonresponse to the amount question in one or the other of adjacent months, result in some month-to-month fluctuation in the number of cases available for analysis.

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The earnings change analysis is complicated by other questionnaire differences, specifically the new procedures in the 2004 panel which encourage respondents to select the “most convenient” method for reporting their earnings – as monthly totals (the traditional standard), as sub-monthly gross pay amounts, as an annual salary, or by reporting hourly pay rates and hours worked. In the 2001 panel, most workers – about 85% – reported their earnings using monthly amounts; in the new panel the proportions have flipped almost exactly, with about 85% reporting their earnings using something other than monthly amounts. Both panels transform non-monthly data into gross monthly earnings amounts. Unfortunately, however, the transformation algorithms used in the 2001 and 2004 panels are not consistent, resulting in important differences in how monthly amounts are created from non-monthly reports. Because we sought to assess the impact of DI on respondents’ reports of change, apart from the impact of processing decisions on change patterns, we decided to exclude from the analysis those who reported in a non-monthly fashion, and to limit our analysis to those who reported monthly amounts, either by reporting monthly totals directly, or by reporting all of their individual, sub-monthly payment amounts, which could then be summed to produce monthly totals.

Figures 1a, 1b, and 1c summarize the results of our analysis of seam bias for earnings amounts in waves 1-2, 2-3, and 3-4, respectively. They offer stark visual evidence that DI significantly reduced the change “spike” across the interview seam. Figure 1a, for example, shows that almost 70 percent of earnings amounts reportedly changed (according to our ±5% definition) across the wave 1-2 seam in 2001, compared to about half that rate in 2004, when DI was 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, in contrast to the post-seam change rates in 2004, which are only about half of what they were before the wave 1-2 seam. Figures 1b and 1c 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, 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.

Because nonresponse rates for earnings amount questions are not generally excessive (Moore, Stinson, and Welniak, 2000), we expected that use of the DI procedures would be a fairly rare event. Thus, the apparent magnitude of their impact on change at the seam is surprisingly large. In fact, use of the DI procedures was quite common – they were invoked over half the time in waves 2 and 3 when asking about job earnings, a rate which far exceeds typical rates of nonresponse to earnings amount items. Other evidence also suggests that the DI questions were not used strictly as a nonresponse follow-up tool. Several 2004 panel interview observation reports by Census Bureau staff note interviewers’ tendency to use the DI follow-ups as a means

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6Concern about the possible impact of these analysis sample restrictions led us to repeat the investigation for three other income sources – Food Stamps, Social Security, and child support – which did not require any such restrictions, since the survey procedures in both panels only permitted monthly-type reporting. These analyses produced very similar results to those reported here for job earnings (data not shown).

7Waves 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.
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 change rates in 2004 compared to 2001 also deserve comment. We suspect that this effect is mostly due to processing and programming decisions, not respondents’ reporting behaviors. The particular design of the 2004 DI procedures obtains a single amount (either the confirmed “last time” amount or a repaired figure), which is then assigned to each month in the current wave, allowing no chance of variation in an off-seam month-pair.

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 improved dependent interviewing (DI) procedures 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 not 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, Julia Klein Griffiths, Elaine Hock, and Heather Holbert. We also thank our Census Bureau colleagues Aref Dajani, for assistance with data file preparation, and John Boies, Kathy Creighton, and Chuck Nelson, who provided many useful comments on early drafts of this chapter. The final version also benefitted greatly from the comments of the editor of this volume, Peter Lynn, and two anonymous reviewers.

References


### 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

<table>
<thead>
<tr>
<th>CHARACTERISTIC</th>
<th>ANALYSIS N’s</th>
<th>AVERAGE % OF ALL CHANGES THAT WERE AT THE SEAM (see Notes)</th>
<th>AVERAGE MONTH-TO-MONTH CHANGE RATES (%)</th>
<th>CHANGE RATE RATIO:</th>
<th>AVERAGE “DIRECTIONAL” CHANGE RATES AT THE SEAM (% change in 2004 compared to 2001)</th>
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<td>ANALYZED INTERVIEWS (&lt;em&gt;Total Observed Changes&lt;/em&gt;)</td>
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<td>W1-W2</td>
<td>W2-W3</td>
<td>W3-W4</td>
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<tr>
<td>PANEL</td>
<td>All Month-Pairs</td>
<td>Seam</td>
<td>Off-Seam</td>
<td>% of “Yes’s” that changed to “No”</td>
<td>% of “No’s” that changed to “Yes”</td>
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#### PART 1: CHARACTERISTICS WITH NEW DEPENDENT INTERVIEWING PROCEDURES IN THE 2004 PANEL

**A. Need-Based Programs**

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<td>“public” health insurance coverage&lt;sup&gt;1&lt;/sup&gt;</td>
<td>45,560 (2,208)</td>
<td>41,572 (1,831)</td>
<td>40,799 (1,627)</td>
<td>57.6%***</td>
<td>0.4</td>
<td>0.21</td>
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<td>7.6</td>
<td>5.0 (-82%)</td>
<td>1.2 (-33%)</td>
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<td></td>
<td>67,211 (2,008)</td>
<td>62,227 (1,625)</td>
<td>60,473 (1,493)</td>
<td>61.9%***</td>
<td>0.6</td>
<td>0.13</td>
<td>3.6</td>
<td>27.7</td>
<td>27.1</td>
<td>1.8</td>
</tr>
<tr>
<td>receipt of Federal SSI (Supplemental Security Income)&lt;sup&gt;2&lt;/sup&gt;</td>
<td>1,596 (334)</td>
<td>1,563 (352)</td>
<td>1,587 (352)</td>
<td>84.6%**</td>
<td>3.1</td>
<td>0.55</td>
<td>18.5</td>
<td>33.6</td>
<td>13.9</td>
<td>88.6</td>
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<tr>
<td></td>
<td>1,826 (500)</td>
<td>1,917 (618)</td>
<td>1,929 (687)</td>
<td>77.6%**</td>
<td>4.5</td>
<td>1.2</td>
<td>24.7</td>
<td>20.6</td>
<td>19.4 (+40%)</td>
<td>86.1 (-3%)</td>
</tr>
<tr>
<td>receipt of Veterans’ Compensation/ Pensions&lt;sup&gt;2&lt;/sup&gt;</td>
<td>641 (118)</td>
<td>619 (103)</td>
<td>621 (94)</td>
<td>82.2%***</td>
<td>2.4</td>
<td>0.50</td>
<td>13.7</td>
<td>27.4</td>
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<td>82.4</td>
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<td>1,096 (118)</td>
<td>1,041 (110)</td>
<td>1,006 (96)</td>
<td>47.5%***</td>
<td>1.5</td>
<td>0.90</td>
<td>4.9</td>
<td>5.4</td>
<td>3.8 (-63%)</td>
<td>52.1 (-37%)</td>
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### A. Need-Based Programs (cont’d)

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<td>AfD/TANF (AFDC/TA NF) receipt</td>
<td>2001</td>
<td>575</td>
<td>345</td>
<td>534</td>
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<td>479</td>
<td>267</td>
<td>55.5%***</td>
<td>8.4</td>
<td>4.4</td>
<td>32.6</td>
<td>7.4</td>
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<td>2004</td>
<td>835</td>
<td>499</td>
<td>786</td>
<td>461</td>
<td>734</td>
<td>416</td>
<td>36.3%***</td>
<td>8.3</td>
<td>6.2</td>
<td>21.2</td>
<td>3.4</td>
<td>18.1 (-38%)</td>
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<td>WIC (Women, Infants, Children) benefits</td>
<td>2001</td>
<td>1,172</td>
<td>546</td>
<td>1,130</td>
<td>436</td>
<td>1,147</td>
<td>443</td>
<td>52.7%***</td>
<td>5.9</td>
<td>3.3</td>
<td>21.7</td>
<td>6.6</td>
<td>17.0</td>
</tr>
<tr>
<td></td>
<td>2004</td>
<td>1,713</td>
<td>672</td>
<td>1,703</td>
<td>648</td>
<td>1,667</td>
<td>635</td>
<td>34.6%***</td>
<td>5.5</td>
<td>4.2</td>
<td>13.3</td>
<td>3.2</td>
<td>9.0 (-47%)</td>
</tr>
<tr>
<td>Food Stamps</td>
<td>2001</td>
<td>2,104</td>
<td>976</td>
<td>2,033</td>
<td>884</td>
<td>2,008</td>
<td>823</td>
<td>52.2%***</td>
<td>6.2</td>
<td>3.5</td>
<td>22.7</td>
<td>6.5</td>
<td>18.1</td>
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<td></td>
<td>2004</td>
<td>3,844</td>
<td>1,512</td>
<td>3,837</td>
<td>1,435</td>
<td>3,802</td>
<td>1,377</td>
<td>34.4%***</td>
<td>5.4</td>
<td>4.1</td>
<td>13.0</td>
<td>3.2</td>
<td>9.2 (-49%)</td>
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### B. Other (Non-Need-Based) Characteristics

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<tr>
<td>School Enrollment</td>
<td>2001</td>
<td>50,948 (6,188)</td>
<td>47,458 (9,629)</td>
<td>46,920 (5,086)</td>
<td>28.9%***</td>
<td>2.1</td>
<td>1.7</td>
<td>4.3</td>
<td>2.5</td>
<td>23.3</td>
<td>2.1</td>
<td></td>
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<tr>
<td></td>
<td>2004</td>
<td>73,919 (8,082)</td>
<td>67,729 (13,549)</td>
<td>65,758 (6,189)</td>
<td>17.7%***</td>
<td>1.9</td>
<td>1.8</td>
<td>2.7</td>
<td>1.5</td>
<td>8.7 (-63%)</td>
<td>2.1 (0%)</td>
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<tr>
<td>Private Health Insurance Coverage</td>
<td>2001</td>
<td>51,378 (5,607)</td>
<td>47,856 (4,971)</td>
<td>47,064 (4,616)</td>
<td>73.4%***</td>
<td>1.5</td>
<td>0.46</td>
<td>7.6</td>
<td>16.5</td>
<td>5.4</td>
<td>14.1</td>
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<tr>
<td></td>
<td>2004</td>
<td>78,162 (6,483)</td>
<td>73,217 (6,627)</td>
<td>71,128 (5,991)</td>
<td>62.8%***</td>
<td>1.2</td>
<td>0.53</td>
<td>5.4</td>
<td>10.2</td>
<td>3.2 (-41%)</td>
<td>11.3 (-20%)</td>
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</tr>
<tr>
<td>Receipt of Social Security</td>
<td>2001</td>
<td>9,929 (780)</td>
<td>9,569 (753)</td>
<td>9,442 (655)</td>
<td>79.4%***</td>
<td>1.1</td>
<td>0.26</td>
<td>6.0</td>
<td>23.1</td>
<td>3.8</td>
<td>92.8</td>
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<tr>
<td></td>
<td>2004</td>
<td>14,999 (1,321)</td>
<td>14,610 (1,378)</td>
<td>14,403 (1,173)</td>
<td>62.2%***</td>
<td>1.3</td>
<td>0.55</td>
<td>5.5</td>
<td>10.0</td>
<td>3.0 (-21%)</td>
<td>87.3 (-6%)</td>
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### B. Other (Non-Need-Based) Characteristics (cont’d)

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<tr>
<td>receipt of Workers’ Compensation²</td>
<td>259 (222)</td>
<td>352 (242)</td>
<td>194 (151)</td>
<td>323 (220)</td>
<td>178 (128)</td>
<td>326 (238)</td>
<td>49.3%**</td>
<td>10.0</td>
<td>7.4</td>
<td>25.3</td>
<td>3.4</td>
<td>23.8 (-36%)</td>
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<td>receive of child support payments²</td>
<td>1,460 (860)</td>
<td>2,476 (1,403)</td>
<td>1,470 (817)</td>
<td>2,408 (1,298)</td>
<td>1,479 (750)</td>
<td>2,289 (1,177)</td>
<td>41.6%***</td>
<td>7.8</td>
<td>5.4</td>
<td>22.9</td>
<td>4.2</td>
<td>17.2</td>
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<td>receipt of alimony²</td>
<td>172 (63)</td>
<td>215 (84)</td>
<td>158 (57)</td>
<td>220 (93)</td>
<td>157 (53)</td>
<td>207 (85)</td>
<td>56.3%**</td>
<td>5.1</td>
<td>2.6</td>
<td>19.9</td>
<td>7.7</td>
<td>16.6</td>
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<td>receipt of private pensions²</td>
<td>3,146 (612)</td>
<td>4,681 (417)</td>
<td>2,894 (489)</td>
<td>4,570 (305)</td>
<td>2,877 (480)</td>
<td>4,523 (287)</td>
<td>87.6%***</td>
<td>2.5</td>
<td>0.37</td>
<td>15.5</td>
<td>41.9</td>
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<tr>
<td>receipt of Federal Civil Service pensions²</td>
<td>478 (104)</td>
<td>761 (50)</td>
<td>397 (65)</td>
<td>745 (35)</td>
<td>375 (54)</td>
<td>728 (55)</td>
<td>94.8%***</td>
<td>2.5</td>
<td>0.15</td>
<td>16.6</td>
<td>110.7</td>
<td>14.0</td>
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<td>receipt of “wealth” income (annuities; estates/trusts)²</td>
<td>271 (241)</td>
<td>456 (320)</td>
<td>199 (181)</td>
<td>378 (159)</td>
<td>185 (158)</td>
<td>363 (165)</td>
<td>67.7%***</td>
<td>12.6</td>
<td>4.8</td>
<td>59.8</td>
<td>12.5</td>
<td>60.3</td>
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### PART 2: CHARACTERISTICS WITH THE SAME DEPENDENT INTERVIEWING PROCEDURES IN BOTH PANELS

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<tr>
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<th>2001</th>
<th>2004</th>
<th># Employment at the same job</th>
<th>40.684 (15,410)</th>
<th>52,799 (17,510)</th>
<th>36.2%</th>
<th>36.2%</th>
<th>37.7%</th>
<th>5.4</th>
<th>4.0</th>
<th>13.7</th>
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<th>8.8</th>
<th>33.6</th>
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<td>Medicare</td>
<td>51,950 (706)</td>
<td>75,713 (868)</td>
<td>67.9%*</td>
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<td>0.12</td>
<td>0.12</td>
<td>0.12</td>
<td>0.12</td>
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<td>0.12</td>
<td>0.12</td>
<td>0.71</td>
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<td>48,508 (429)</td>
<td>70,152 (483)</td>
<td>48,809 (404)</td>
<td>47,809 (404)</td>
<td>68,067 (445)</td>
<td>69.5%*</td>
<td>69.5%*</td>
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**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 AT THE SEAM**” – 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 1a. Percent of Wave 1-2 Cases with a Month-to-Month “Change” (± 5%) in Job Earnings
(approx. avg. N (see text): 18,000 (2001); 7,200 (2004))

Figure 1b. Percent of Wave 2-3 Cases with a Month-to-Month “Change” (± 5%) in Job Earnings
(approx. avg. N (see text): 12,200 (2001); 6,300 (2004))

Figure 1c. Percent of Wave 3-4 Cases with a Month-to-Month “Change” (± 5%) in Job Earnings
(approx. avg. N (see text): 15,800 (2001); 5,600 (2004))