Seam Bias in the 2004 SIPP Panel: Much Improved, but Much Bias Still Remains[*]

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ABSTRACT:
Panel surveys generally suffer to some extent from seam bias, 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. Seam bias strikes at a core utility of retrospective panel surveys, because it means that reports of the start and end dates of spells of important characteristics (e.g., program receipt, health insurance coverage, etc.) are likely to contain substantial measurement error. Much research has documented the existence of seam bias; attempts to reduce it, however, have generally met with only limited practical success.

The U.S. Census Bureau recently completed a multi-year research program to improve the Survey of Income and Program Participation (SIPP) questionnaire, a main goal of which was to reduce seam bias. The specific questionnaire revision intended to accomplish this was a much more extensive and focused use of dependent interviewing (DI) procedures. New DI procedures were incorporated into the SIPP questionnaire with the launch of the most recent SIPP panel in February 2004. This paper describes those procedures, and examines their impact on seam bias in the first several waves of the 2004 panel for a number of characteristics (e.g., government transfer program participation, school enrollment, employment, health insurance coverage, etc.), through a comparison with the same estimates derived from the 2001 panel. Initial analyses, using preliminary, unedited data files, found clear evidence of a positive change with the new procedures (Moore, et al. (2008)). This paper repeats those analyses on edited, public-use data files, the results of which point to the same three general conclusions: First, seam bias is substantially lower in 2004 than it was in 2001. Second, the seam bias improvements are attributable to the new DI procedures. Third, however, notwithstanding the clear improvements, seam bias still afflicts SIPP 2004 panel data to a substantial extent. While there is good reason to expect that additional refinements to the DI procedures would yield further seam bias improvements, the results also leave little doubt that even under the most optimistic scenario, those refinements would yield some additional reductions in seam bias but would by no means

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[*] This paper parallels (and draws heavily from) a previous paper prepared for the International Conference on the Methodology of Longitudinal Surveys, University of Essex, Colchester UK, July 12-14 2006 (see Moore et al., 2008). In essence it represents an updating and corroboration of the earlier paper, in that it uses final, edited data files in its analyses, rather than the preliminary, unedited, internal-use files which at the time were the only ones available. The present paper is released to inform interested parties of research and to encourage discussion of work in progress. The views expressed are the author's and not necessarily those of the U.S. Census Bureau.

eliminate it. An almost inescapable conclusion is that for most characteristics of interest to SIPP – school enrollment being a notable and instructive exception – the traditional question-by-question interviewing approach, using calendar months as cues, may simply be limited in its ability to capture high quality retrospective reports of spell start and end dates. Rather than focusing on any additional fine-tuning of that approach, future redesigns of the SIPP program should consider new alternatives such as event history calendar methods, which are more attuned to the basic organization of human memory, and which thus hold out promise as a better way to yield high quality retrospective spell data.

keywords: dependent interviewing, event history calendars, measurement error, memory cues, spells, transitions

1. Introduction

This paper 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, 2008). 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. Results suggest 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 questionnaire used in the 2001 SIPP panel. Section 5 offers general conclusions, and section 6 concludes the paper with a brief discussion of some implications of the current findings for future research.

2. Previous Research on Seam Bias

Seam bias began to draw the attention of survey methodologists in the early 1980's. Czajka (1983), 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" [p93]; 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 summarizing the first decade of seam bias research, uses 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) describe apparent change in income across successive survey waves as "grossly inflated" [p13]; similarly, Lynn and Sala (2006) label the amount of change they observe from one survey wave to the next in various employment characteristics as "implausibly high" [p8]; 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 (2008). For those concerned about seam bias, however, and the more general problem of accurate measurement of transitions, the need to control spurious change makes 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., 2006).

3. The U.S. Census Bureau's SIPP Program

3.1. SIPP's Basic Features

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 (2001a) for a more complete description of the current SIPP program. At present, SIPP is being re-engineered in an attempt to reduce respondent burden, deliver data products in a more timely manner, and reduce costs, while still continuing to meet key stakeholder data requirements. More information about the re-engineering effort and its schedule is available on the Census Bureau's SIPP website at: http://www.sipp.census.gov/sipp/dews.html .

In their basic design and structure, the 2001 and 2004 SIPP panels were equivalent. Most notably for purposes of this evaluation, both panels used automated questionnaires, constructed with the same instrument authoring language, administered via laptop computer, by Census Bureau interviewers ("field representatives") working under the direction of one of the Bureau's 12 regional offices. Each panel's interview schedule was identical in terms of the calendar months covered – that is, wave 1 interviewing began in February of the panel year (reference period: October, November, December, and January) and continued through May (January, February, March, April), wave 2 was conducted in June, July, August, and September, and so on for subsequent waves. In other words, corresponding waves of the two panels cover the exact same calendar months, three years apart.

3.2. Edited SIPP Data Files

As is the case with any survey, the raw data files derived directly from the SIPP questionnaire contain obvious imperfections. Logical inconsistencies represent one type of flaw in SIPP data – these can arise from a number of sources including mistakes in recording responses, instrument shortcomings (e.g., a failure to "clear" previously-entered fields appropriately when interviewers back up to change an earlier response), and simple response error. The most common type of imperfection, however, is missing data, of which there are two general types: item nonresponse, generally in the form of a "don't know" or a refusal to answer specific questions, and unit nonresponse, wherein whole households, or selected people within otherwise-interviewed households, fail to respond to a survey wave.

The SIPP program goes to considerable lengths to repair these imperfections in its public-use data files. The first repair stage is generally the most straightforward. It consists of an automated data editing procedure that is used when information exists on the sample person's record from which the true values for missing or inconsistent data can be logically inferred. More complex procedures are required for missing data which cannot be logically inferred; the general label for this is "imputation." In very general terms, the imputation process matches the nonresponse case with an appropriate responding case, based on sociodemographic characteristics, and then assigns the value from the "donor" record to the missing item. Whole-interview nonresponse is corrected through weighting adjustments. See U.S. Census Bureau (2001a) for more details about SIPP editing procedures.

3.3. Dependent Interviewing (DI) in SIPP

3.3.1. SIPP's pre-2004-panel 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 DI procedures 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 (2008) 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 on the immediate reporting period almost to the exclusion of prior circumstances.

3.3.2. 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 to have changed very little – and certainly not 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 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 most distant month of 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, in one sense, 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"[1]. 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 <u>not</u> 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

[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 new 2004 questionnaire across virtually all subject-matter areas, including jobs/businesses.

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 initial nonresponse. This "reactive" form of DI (see Lynn et al., 2006) 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, despite some problems in interviewers' administration of these procedures, they have been quite successful at reducing item nonresponse for income amounts (Moore, 2006a; Moore, 2006b). Moore et al. (2008) also report evidence of seam bias improvements for income amounts (specifically, earnings). The present evaluation does not further address seam bias for income amounts.

3.3.3. Testing and refining the new SIPP questionnaire

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; a paper by Moore, et al.(2004) also covers field test design issues, and provides 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 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 a set of program participation, health insurance coverage, and other "spell"-type characteristics. The analysis is by no means exhaustive of all characteristics included in SIPP, but it does include most of the "top tier" characteristics of primary interest to SIPP's key stakeholders. (For purposes of comparison, the selected characteristics also duplicate those examined in the preliminary work by Moore et al (2008) which, as noted, differs from the current work primarily in that it used preliminary, unedited, internal data files.) The analysis exploits the "natural experiment" of the design differences between the 2001 and 2004 SIPP panels – as noted above, the 2001 panel was mostly devoid of the DI procedures which were introduced extensively in 2004. The best evaluation method available from such a research design is a straightforward comparison of the 2004 seam bias results with those of the immediately preceding 2001 panel – fully recognizing that drawing conclusions from a natural experiment as opposed to a designed one requires additional strong

assumptions (e.g., that confounding factors such as sample design and field staff differences and the mere passage of time can be ignored). Although these limitations must be acknowledged, there is no evidence that they actually influence the findings in important ways, or affect overall conclusions.

4.1. Methodology

This evaluation examines month-to-month change in participation/coverage/receipt status – from "on" to "off" a particular program, for example, or from "off" to "on" – across the first four waves of the 2001 and 2004 SIPP panels for 17 characteristics. The 17 characteristics include 15 for which the 2001 and 2004 questionnaires differed markedly in their use of DI, as described above, and two others for which the 2001 and 2004 questionnaires did not differ. The expectation is that a reduction in seam bias will be evident in the 2004 results, compared to 2001, for the 15 characteristics for which DI procedures were introduced in 2004, and that this will stand in contrast to the results for the two characteristics whose 2001 and 2004 questionnaire procedures did not differ, which should show equivalent seam bias across the two panels. Additional analyses which look more closely at the different forms of change across the seam also address the issue of the role of DI in producing the observed seam bias reduction.

The 15 characteristics which are the focus of the seam bias reduction evaluation range across the spectrum of topics measured in SIPP, including participation in government transfer programs, health insurance coverage of various kinds, school enrollment, and receipt of various types of income – their breadth helps address the issue of how general the effects of DI are. In the reporting of results which follows I separate the characteristics into two general categories: "need-based programs," which consist of government-sponsored welfare-type programs intended to assist the economically disadvantaged, and other characteristics which have nothing to do with economic need (e.g., school enrollment, Social Security) or which are likely to be relevant to those at the upper end of the economic distribution (e.g., receipt of income from annuities/ estates/trusts).

The starting point for each of the analyses is the same: the set of SIPP-defined "adults" (age 15+) for whom data are available for each interview wave 1 through 4. The analyses thus exclude people who moved into sample households after wave 1, as well as those who were present and interviewed in wave 1 but for whom no interview data are available in one or more of the subsequent waves[2]. The following table shows, for each panel, the number of adults with interview data in each wave, the number of analysis cases interviewed in all four waves, and the number of analysis cases as a percentage of the initially-interviewed Wave 1 cases**:**

[2] This research also applied an alternative analysis strategy, one which includes noninterview and mover cases to the maximum extent possible. In essence, the alternative approach involved three separate seam bias examinations, one for each set of consecutive waves (1-2, 2-3, and 3-4), each of which included all cases interviewed in both waves. The three separate analyses were then averaged to form composite seam bias estimates. This approach was implemented for some (but not all) of the characteristics reported on in this paper; the results (not shown) differed trivially from the more exclusive approach described in this paper, and never to an extent that would alter any substantive conclusions.

SIPP 2001/2004 PANEL SEAM BIAS EVALUATION ANALYSIS CASES
Adults with Interview Data in All 4 Waves

SIPP Panel	Number of Adults (15+) with Interview Data, by Wave and for All Waves 1-4					
	Wave 1	Wave 2	Wave 3	Wave 4	**ANALYSIS CASES: ALL 4 WAVES**	Analysis Cases as % of Wave 1 Cases
2001	69,574*	56,574*	55,691	55,209	**47,462**	80.3%*
2004	85,847	81,337	78,837	77,357	**66,523**	77.5%

* The huge drop in interviewed cases from wave 1 to wave 2 in 2001 is mostly the result of a sample cut of approximately 15% following the wave 1 interview, which was implemented in response to budget constraints. For a technical description of the sample cut see U.S. Census Bureau (1999) and U.S. Census Bureau (2001b). The final column in the 2001 row of the table adjusts for the sample cut by calculating the all-4-waves analysis cases as a percentage of the Wave 1 interview cases reduced by 15% (estimated reduced n = 59,138).

The analysis for each characteristic is further restricted to those who reported a "yes" value (participated/covered/received/enrolled/etc.) in any month across the four interview waves. In other words, those with a value of "no" for all 16 months of the wave 1 through 4 reference periods were excluded. Among this final analysis group, the basic data for the analysis of each characteristic consists of a tally of changes in status from one month to the next – from "no" (off) to "yes" (on) or vice-versa – across each of the 15 month-pairs formed in linking together the 4 survey waves. These changes are examined separately for the 3 seam month-pairs, which straddle waves 1 and 2, 2 and 3, and 3 and 4, and the 12 "off-seam" month-pairs, in which both months fall within the reference period of the same interview wave. All analyses are carried out in terms of SIPP reference period months, as opposed to calendar months[3], which are numbered simply 1 through 16 for the first four SIPP waves. Thus, months 1 and 2, for example, correspond to October and November for the one-quarter of the sample which comprises rotation group 1 (wave 1 interview in February), and to November and December, December and January, and January and February for rotation groups 2, 3, and 4, respectively. Similarly, the wave1-2 interview seam is January-February for rotation group 1, and February-March, March-April, and April-May for the other rotation groups. All analyses use unweighted data.

4.2. Results – Characteristics with Differing DI Procedures in 2001 and 2004

4.2.1. Seam bias analysis for "need-based" programs

This evaluation of seam bias differences between the 2001 and 2004 SIPP panels examines six "need-based" programs designed to provide assistance of various kinds to the economically disadvantaged, as follows: Medicaid and other public-assistance-type health insurance plans,

[3] An exception is the special analysis of school enrollment changes, for which an analysis strictly in terms of reference period months presents certain problems – see section 4.2.3.

Federal Supplemental Security Income (SSI), Veterans' Compensation and Pensions, Temporary Assistance to Needy Families (TANF, often formerly referred to as AFDC), Women, Infants and Children (WIC) nutrition benefits, and Food Stamps. (Brief descriptions of each of these programs can be found in U.S. Census Bureau (2001a).) For each of these characteristics, month-to-month changes in status, plotted as a proportion of all analysis cases (as defined above), across the first four waves of the 2001 and 2004 SIPP panels, are shown graphically in Figures 1.A.1 through 1.A.6. (Figure designations correspond to the rows of Table 1, the Analysis Summary Table, which appears at the end of this paper, after the set of figures.) Using Figure 1.A.1 (Medicaid and other "public" health insurance coverage) as an example: of the cases for whom interview data were available in all waves 1 through 4, and who reported Medicaid/public coverage at any point during any of the four waves ($n_{(2001)}$ = 6,315; $n_{(2004)}$ = 9,354 – n's are shown in the analysis summary table), approximately 1-2 percent reported a change in coverage from one month to the next for months covered within a single interview. In 2001, that rate of change rises to 25-30 percent across seam month-pairs; at 15-20 percent, the seam "spikes" in month-to-month change in the 2004 data are still readily apparent, but are much less prominent than in the earlier panel. This same general pattern holds quite clearly for four of the five other need-based characteristics; much less so for Federal SSI, for reasons which are not clear.

The "eyeball" test of these results by itself is rather compelling evidence of reduced seam bias in 2004, but Table 1 provides further statistical corroboration. For each of the six need-based characteristics shown in part 1.A of the table, the proportion of all observed month-to-month changes that were comprised of changes observed at the seam (col. (4)) was significantly higher in 2001 than it was in 2004. Note also, however, that even the improved results for 2004 still reveal substantial seam bias. All of the estimates in the "% of all changes..." column are far higher than the 20% level which would be expected if month-to-month change were distributed evenly across the 3 seams and 12 off-seam month-pairs of waves 1 through 4.

The improved performance of the 2004 panel is also apparent in the comparison of the overall rates of change for off-seam and seam month-pairs (col. (5)). For all six of the need-based programs the rate of change across off-seam months was significantly greater in 2004 than it is in 2001, and the reverse is true across seam months (SSI being the lone exception to the latter). This evaluation has no access to any absolute standards for assessing the quality of the off-seam and seam change rates, which are mostly a function of the particular "volatility" of the characteristic in question. However, the direction in which they have moved from 2001 to 2004 is quite telling. The directions of the effects represent a direct antidote to what Moore and Marquis (1989) and others have found to be the essential nature of seam bias – that it is the net result of too little change across months measured within a single interview, and too much change at the seam. As a result of the forces acting on both seam bias components, the table reveals substantial differences between the two panels in the extent to which off-seam and seam change rates are out of balance. For Medicaid, for example, the rate of change observed at the seam in 2001 is more than 22 times the rate observed away from the seam (col. (6)); this imbalance is essentially cut in half (11.5) in the 2004 results; similar drops are evident for all of the need-based programs summarized in section 1.A. Again, however, while we seem to have

notable improvement here, we do not have a cure. 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. Even the best performing characteristic in 2004 – which among the need-based programs is Food Stamps – still exhibits a rate of change at the seam that is over four times the rate observed away from the seam.

4.2.2. Seam bias analysis for other, non-need-based characteristics (excluding school enrollment)

In addition to the need-based programs, the seam bias evaluation also includes 9 characteristics not associated with people in economic need, but for which DI procedures differed between the 2001 and 2004 panel questionnaires in exactly the same manner as described above. The "other, non-need-based" characteristics are as follows: private health insurance coverage, Social Security retirement benefits, workers' compensation benefits, child support payments, alimony, private retirement pensions, Federal Civil Service retirement pensions, receipt of "wealth" income from annuities, estates, or trusts, and school enrollment. Because school enrollment has unique features which require separate treatment, I set that characteristic temporarily aside and examine the results for the other 8 non-need-based characteristics first.

Again beginning graphically, Figures 1.B.1 through 1.B.8 show month-to-month changes in status for the non need-based measures in 2001 and 2004, plotted as a proportion of all analysis cases, across the 15 month-pairs in the first four waves of each panel. Despite the very different natures of these characteristics, their results are clearly quite similar to those for the need-based programs – an obvious and substantial reduction in the heights of the seam "spikes" in 2004 compared to 2001. Also as before, the summary data in part 1.B of Table 1 confirm what is clear to the naked eye: with notable consistency, the bias at the seam has been significantly reduced in 2004 compared to 2001 (col. (4)), and the overall effect is due to an increase in 2004 in the rate of change away from the seam, and a decrease in the rate of change across seam months (col. (5)). These latter effects are in the appropriate direction but fail to achieve statistical significance in the case of alimony, which has a very sparse set of analysis cases. In addition, the 2001 and 2004 off-seam change rates for private pensions are essentially the same. But these are the only anomalies among this set of characteristics. Among all 8 characteristics, the rate of change observed across seam months in 2001 was at least 5 times higher than the change rate away from the seam, and much higher than that in some instances – for Federal Civil Service pensions the two rates differ by a factor of 80 – and in most cases the extent to which the two rates are out of balance has been cut in half, approximately, in the 2004 results.

4.2.3. Seam bias evaluation for school enrollment

Figure 1.B.9, which shows month-to-month change in school enrollment, presents a very different picture than has been the case for the other characteristics examined thus far. The 2004 seam spikes are still of a lesser magnitude than the comparable 2001 estimates, but the entire pattern of the data is quite different – one major spike at the wave2-3 seam, two much smaller spikes at the other seams, and much greater off-seam change within waves 2 and 3. Despite the

fact that the statistical analysis summarized in Table 1 makes school enrollment look very much like the other non-need-based characteristics, it is clear that a different process is at work in these results.

The "different process" for school enrollment has to do with the increased relevance of calendar months for this particular characteristic, because school enrollment changes are not equally likely across all pairs of months. In the U.S. educational system, some month-pairs – specifically December-January, May-June, June-July, and August-September – are much more likely to see real changes in enrollment than other month-pairs – February-March, for example, or October-November. Furthermore, these "low-change" and "high-change" month-pairs are not evenly distributed across SIPP reference period months. In the legend at the bottom of figure 1.B.9, asterisks indicate how many "high-change" calendar month-pairs are included in each pair of reference period months. (Recall that, because of SIPP's rotation group interviewing schedule, each pair of reference period months consists of data from four different calendar month-pairs.) The first pair, month1-month2 within wave 1, for example, is comprised in approximately equal portions of data for October-November, November-December, December-January (a high-change pair), and January-February. Most other month-pairs also include a single high-change pair, but several include two such pairs, while one, the wave1-2 seam, includes no high-change pairs, and another, the wave2-3 seam, consists of three such pairs.

Thus, for school enrollment the assumption of equally likely change across all reference period months, which is a key assumption underlying a standard seam bias assessment of the type depicted graphically in Figure 1.B.9, is clearly not tenable. It is a reasonable conclusion that the reduced spike in 2004 at the wave1-2 seam indicates improved data quality, since that month-pair contains no high-change months, and thus should exhibit the lowest rate of change of any pair of months. But it is difficult to interpret the meaning of the differences at the other two seams, especially at the wave2-3 seam, where a large amount of enrollment change is to be expected.

Fortunately, the staggered interviewing schedule of SIPP's rotation group structure offers a route to a solution to this problem – that is, to unambiguous evidence concerning seam bias improvement in the 2004 panel. As a result of the rotation group design, every calendar month-pair throughout the year is a seam pair for one-quarter of the sample (i.e., one rotation group), and an off-seam pair for the other three-quarters. Figure 1.B.9.b organizes the school enrollment month-to-month change data to exploit this fact. Calendar month-pairs for the one-year period covered by waves 1 through 4 are arrayed along the X-axis. Plotted on the Y-axis is not the proportion of cases which change across that month-pair, as in the previous figures, but rather the proportion of all observed changes across that month-pair arising from the rotation group for which those months comprise an interview seam.

In the absence of any seam bias we would expect each bar to be at approximately the 25% level, which is marked on the figure with a dashed line. There are a couple of notable exceptions – specifically, May-June and June-July in the 2004 panel – but obviously most of the estimates far exceed the 25% target, indicating that seam months are over-represented in the set of cases in which enrollment change is observed. The good news, however, is that the 2004 estimates are

consistently closer to the 25% target than the 2001 estimates, significantly so for 9 of the 12 month-pair comparisons and for all 12 month-pairs combined. (Statistical significance is here defined as a t-value of 2.0 or greater in a t-test of the difference between two proportions, and is indicated in the figure by solid-filled bars.) Thus the evidence for school enrollment change is consistent with almost all of the other characteristics examined – it, too, indicates that a significant reduction in seam bias accompanied the new DI procedures introduced in the 2004 SIPP panel.

4.3. Results – Evidence Concerning the Role of DI in the 2004 Seam Bias Reduction

4.3.1. Characteristics with the same DI procedures in 2001 and 2004

The seam bias evaluation includes two characteristics whose measurement procedures did not differ across the two SIPP panels – employment at a job and Medicare coverage. (Medicare is the US government health insurance program for the elderly.) As suggested above, these characteristics were included in the evaluation to bolster the argument that the seam bias improvements in the other characteristics are attributable to the new DI procedures, and not some other difference between the two panels unrelated to how the questions were administered. This argument is supported to the extent that the seam bias results for these characteristics are the same across the two panels; to the extent that they differ, the argument of a major causal role for DI in the observed seam bias improvement in 2004 is weakened.

The Medicare program is designed such that once a person is eligible and enrolls he/she is for all intents and purposes covered for life. Thus in both the 2001 and 2004 questionnaires there were no DI procedures – a "yes/ covered" response in one wave was simply carried over automatically to all subsequent waves, without asking; "no/not covered" simply triggered another non-dependent question in the next wave. For employment at a job, on the other hand, both panels used the same fully dependent, "remind, confirm/still" DI procedures for any job held at the time of the previous interview: "Last time I recorded that you worked for XYZ Company. Do you still work for XYZ Company?"

The Medicare analysis follows the design for all of the other characteristics, described above. The "jobs" analysis, however, differs from the others in two ways. The first difference arises as a result of questionnaire problems which afflicted both panels, and which caused, in some cases, incorrect information about prior jobs to be fed forward from wave 2 to wave 3, and from wave 3 to wave 4. To avoid these problems, the seam bias analysis for jobs is limited to the first two interview waves. Second, because people can hold multiple different jobs across any given period of time – and even simultaneously – the analysis is carried out at the individual job level, not the person level. As with the other characteristics, the analysis file for each panel starts by excluding people with missing interview data in any relevant interview wave (waves 1 and 2, in this instance), and also excludes people who did not report a job in any relevant wave. But among the remaining cases, those with more than one job across the four interview waves contribute more than one "record" to the analysis file – one for each separate job.

The seam bias results for these two characteristics are shown graphically in Figures 2.1 and 2.2. The "jobs" results (Figure 2.1) conform to expectations – to a remarkable degree, at least visually. The 2001 and 2004 panels, using the same DI procedures, yield month-to-month change results that are virtually indistinguishable at every data point. Statistical analysis of these results, summarized in part 2 of Table 1, does reveal some significant differences – changes at the seam comprise a significantly higher proportion of all month-to-month change in 2004 than they did in 2001, and the off-seam change rate in 2004 is significantly lower[4]. In absolute terms, however, the effect sizes are very small, and their statistical significance seems to derive mostly from the very large SIPP sample and the correspondingly large number of jobs reported.

The seam bias results for Medicare (Figure 2.2) do not appear to conform so well to expectations. Despite identical questionnaire procedures, the spikes in month-to-month Medicare changes at the seam in 2001 consistently exceed those for 2004. In part, this may be a matter of scale, since a change in Medicare status, even at a seam month, is a relatively rare event. Statistical analysis of these results finds a non-significant difference in the overall extent of seam bias – in both the 2001 and 2004 panels virtually all observed month-to-month changes in Medicare status are found at an interview seam – but also confirms the visual evidence that, although the absolute difference is not great, the rate of change for seam months is significantly higher in the 2001 data. This finding, of course, would seem to weaken the argument that the consistent reductions in seam bias in 2004 (as reported in section 4.2) can be attributed to the revised 2004 questionnaire, and in particular to the new DI procedures, if in fact the same sort of reduction appears where DI procedures did not differ.

This initial evidence of more seam bias in the 2001 panel than in the 2004 panel is unexpected not only because the two panels' questionnaire procedures are virtually identical, but also because they run counter to the earlier findings of Moore et al. (2008), who found no seam bias difference in the 2001 and 2004 Medicare results. The earlier study used unedited "TransCASES" data derived directly from the 2001 and 2004 questionnaires, and that, in fact, appears to account for the discrepant results. A close examination of the data suggests that the editing process actually increased Medicare seam bias in both panels, by both reducing the frequency of off-seam transitions and increasing the frequency of seam transitions, and, furthermore, that the seam bias increase following the edit process was significantly greater in 2001 than in 2004 (data not shown) of the subsequent waves[5]. The point is that to the extent the edited Medicare results arise from some process outside the interview, they are less germane to the question of the role of DI in the 2004 seam bias reduction.

4.3.2. "Directional" change at the seam in 2001 and 2004

The new DI 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

[4] Note that both of these differences are in the opposite direction to the typical effects found for characteristics which used the new DI procedures in 2004.

[5] Ham, Li, and Shore-Sheppard (2008), in an analysis of employment transitions in SIPP data covering the years 1986-1995, also note that the editing process seems to have increased seam bias – see footnote 3, p2.

prior interview. Therefore, if those new procedures account for the reduction in seam bias in 2004, we should expect to see comparably asymmetrical effects for the two different forms of change that can happen at the seam, yes-to-no change and no-to-yes change. And, in fact, that is exactly what the data show. The reduced seam spikes in 2004 are due almost entirely to a reduction in yes-to-no change at the seam; differences between the 2001 and 2004 panels in no-to-yes seam changes are mostly small and inconsistent.

Once again, a graphical display offers very compelling evidence of these results. Figure 3.1a shows the month-to-month "yes-to-no" transition rates across all wave 1-4 month-pairs in Medicaid and other public-assistance-type health coverage, separately for the 2001 and 2004 panels. Each data point plots the proportion, among all those reporting Medicaid coverage in the first month of the pair, who switched to a report of no coverage in the second month of the pair. The two obvious features of this figure are the clear seam spikes – a "yes" at the beginning of a month-pair is much more likely to change to a "no" in the next month if that month-pair is at the seam – and the greatly reduced magnitude of those spikes in 2004 compared to 2001. This latter difference is in marked contrast to the pattern of results portrayed in Figure 3.1b, which shows transition rates for changes in the opposite direction – the proportion of "no" reports that changed to "yes" in the next month. Here the seam bias results for 2001 and 2004 are virtually indistinguishable.

Additional figures show the results of identical analyses carried out on one other need-based program, Food Stamps (Figures 3.2a and 3.2b), and two of the non-need-based characteristics, private health insurance coverage (Figures 3.3a and 3.3b) and "wealth" income (Figures 3.4a and 3.4b). These four characteristics were selected arbitrarily for display, but they are perfectly illustrative of a very general pattern, which is summarized in the final two columns of Table 1 (column (7)), which show the overall rates of yes-to-no and no-to-yes transitions at the seam, combined across all three seams in waves 1-4, for each characteristic and each panel. The 2004 row also shows the direction and magnitude of the change in the transition rate from 2001 to 2004, as a percentage of the 2001 "base rate." For 14 of the 15 characteristics for which DI procedures differed in the two panels, the rate of yes-to-no change at the seam is significantly lower in 2004 than it was in 2001. (The observed difference in the case of the single exception, alimony, is in the correct direction but does not reach statistical significance.) This is in marked contrast to the "no-to-yes" results, which in sum show no consistent difference between the two panels – four comparisons show greater rates of change in 2001 than in 2004, two show the opposite effect, and nine show no significant difference. In short, these results show strong and consistent seam bias outcome differences in precisely those circumstances where DI procedures were called forth, and no such differences where they were not.

5. Conclusions

This research points to several clear conclusions, as follows:

1. Seam bias has declined generally and substantially in the 2004 SIPP panel.

The evidence of a reduction in seam bias in the 2004 panel is both strong and consistent. Perhaps the clearest evidence in support of this conclusion (aside from the stark visual evidence of Figures 1.A.1 through 1.B.9) can be seen in the most basic seam bias indicator – the proportion of all month-to-month changes observed at the seam. Across a large number of characteristics, and a very wide range of types of characteristics, the analysis finds that the 2004 estimate is significantly less biased than the 2001 estimate in every single instance. Seam bias afflicts the measurement of characteristics associated with rich and poor alike, and the improvements in 2004 seem to have operated across-the-board as well.

2. The decline is attributable to the new DI procedures.

Although they differed in many ways, the primary difference between the 2001 and 2004 questionnaires had to do with the extensive use of true DI procedures in 2004. 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 evidence comes from two different observations. First, 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. Certainly this was the case for employment at a particular job; the Medicare coverage results are somewhat more problematic, although there is some indication that those results started out with essentially equivalent bias and only became non-equivalent as a result of post-collection data processing. The second source of evidence which points to DI as the cause of the 2004 improvement are the very obvious and highly consistent differences across the two different types of seam transitions, "yes-to-no" and "no-to-yes." As implemented in the 2004 panel, the new DI procedures are only invoked when the response at the end of the previous wave is "yes," so it is hardly surprising that the analysis only finds a measurable seam bias improvement for yes-to-no change (significant in 14 out of 15 cases), with no discernable difference between the panels for no-to-yes change (non-significant in 9 cases, mixed results in 6). Together, these findings offers strong support for the notion that the observed seam bias improvement in 2004 is 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. The improvement in seam bias in 2004 due to DI is due to both 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 "Month-to-Month Change Rates (%)" columns (5) of Table 1, for 13 of the15 characteristics the off-seam change rate is significantly higher in the 2004 panel than in the earlier panel, and for 13 of the 15 characteristics the rate of change at the seam in 2004 is significantly lower than in 2001.

4. Despite the improvements due to DI, much seam bias still remains in 2004 panel data.

Improvement in seam bias in the SIPP 2004 panel is unmistakable; that bias is far from having been eradicated is equally unmistakable. Every characteristic – notwithstanding its improvement relative to 2001 – still displays in 2004 an overabundance of changes at the seam. The visual

evidence from the figures makes this quite clear, but it is also clear statistically in Table 1. For example: in the proportion of all changes that are seam changes (column (4)), the very best outcome is for school enrollment, where seam changes account for "only" 37.1% of all changes in 2004, and for most other characteristics the estimate is well above 50%. In the absence of any seam bias we would expect seam changes to account for 20% of all month-to-month changes observed across four interview waves, since they comprise three of the 15 month-pairs of the four four-month reference periods. The same result surfaces in high profile in the "Change Rate Ratio" column of Table 1, where the best performing characteristic (again, school enrollment) shows a rate of change at the seam that is more than twice the rate observed between months within a single interview wave. For six of the fifteen cases the "improved" result in 2004 still leaves a rate of change at the seam that is more than 10 times the change rate across months away from the seam.

5. Additional improvements are possible...

The present results highlight an additional area in which there is still much untapped potential for further improvements, as has been alluded to already: "no-to-yes" changes at the seam. Asymmetrical DI as it has been introduced in the 2004 SIPP panel 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. The general form of such procedures seems fairly straightforward, and no more complicated than what currently exists. 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.

6. ...but those improvements are unlikely to "cure" seam bias entirely.

On the other hand, the present results also suggest the limits of expanding DI in this manner. For all their improvement, yes-to-no change at the seam in 2004 still exhibits substantial seam bias, as a glance at figures 3.1a, 3.2a, 3.3a, and 3.4a quickly reveals. Even if the DI procedures were made symmetrical, and even if the impact on no-to-yes change were every bit as positive as has been observed in the case of yes-to-no change, the data still suggest that seam bias would still be substantial.

6. Discussion

The results of this investigation, as with the earlier work which examined preliminary data files, are in one sense quite encouraging with regard to the quality of month-to-month change data in the new SIPP panel. 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, which was the specific intent of implementing them. On a less positive note: Despite the significant improvements, much seam bias still remains, and the prospects for further improvement, while clearly present, do not suggest that seam bias is close to being eliminated. This conclusion points to the need for more research directed toward a clear understanding of the effects of seam bias on the cross-sectional and longitudinal estimates that SIPP analysts typically produce, and the development of compensatory statistical adjustments for those effects (e.g., Ham, Li, and Shore-Sheppard, 2008). The fact that the "improved" SIPP 2004 transition data for program participation, income sources, and other characteristics are still subject to much error points to another distinct possibility – namely, that there are limits to the amount of improvement that can be achieved under a survey design that uses calendar months as the basis for reporting events of interest. Instructive in this regard are the results for school enrollment, 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 shown in Table 1 (see the "% of All Changes..." column (4)) are lower than for any other characteristic *after* the addition of improved DI procedures in 2004. And at key points in the calendar year we see, in Figure 1.B.9b, that the improved results in 2004 really are almost devoid of seam bias. It seems likely 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[6].

But school enrollment is clearly the exception – for the other characteristics months appear to be very ineffective cues for achieving high quality reporting. Certainly, they do not correspond to memory psychologists' current understanding of the manner in which most information is stored in human memory, which is through links and associations among memory elements (e.g., Conway, 1996). Future research efforts on improvements to the SIPP questionnaire, rather than focusing on continued refinements to the current standard questionnaire approach, may be better spent investigating alternative designs which do draw on and exploit relationships among events and circumstances. Event history calendar methods (e.g., Belli, 1998) represent one such approach, and as such are a prime candidate for future research attention on ways to elicit the best possible quality in SIPP transition data.

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[6] Interestingly, Moore and Kasprzyk (1984), in their seam bias investigation of the ISDP, SIPP's precursor, report high levels of bias for every type of characteristic examined, save one – receipt of educational benefits.

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FIGURES

1.A.1 through 1.A.6
(new DI procedures in 2004, "Need-Based Programs")

1.B.1 through 1.B.9
(new DI procedures in 2004, "Other (Non-Need-Based) Characteristics")

2.1 and 2.2
(same DI procedures in 2001 and 2004)

Figure 1.A.1: MONTH-TO-MONTH MEDICAID/PUBLIC HEALTH INS. COVERAGE CHANGES (unweighted) IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4

(edited data using cases with any Medicaid/public coverage interviewed in all 4 waves)

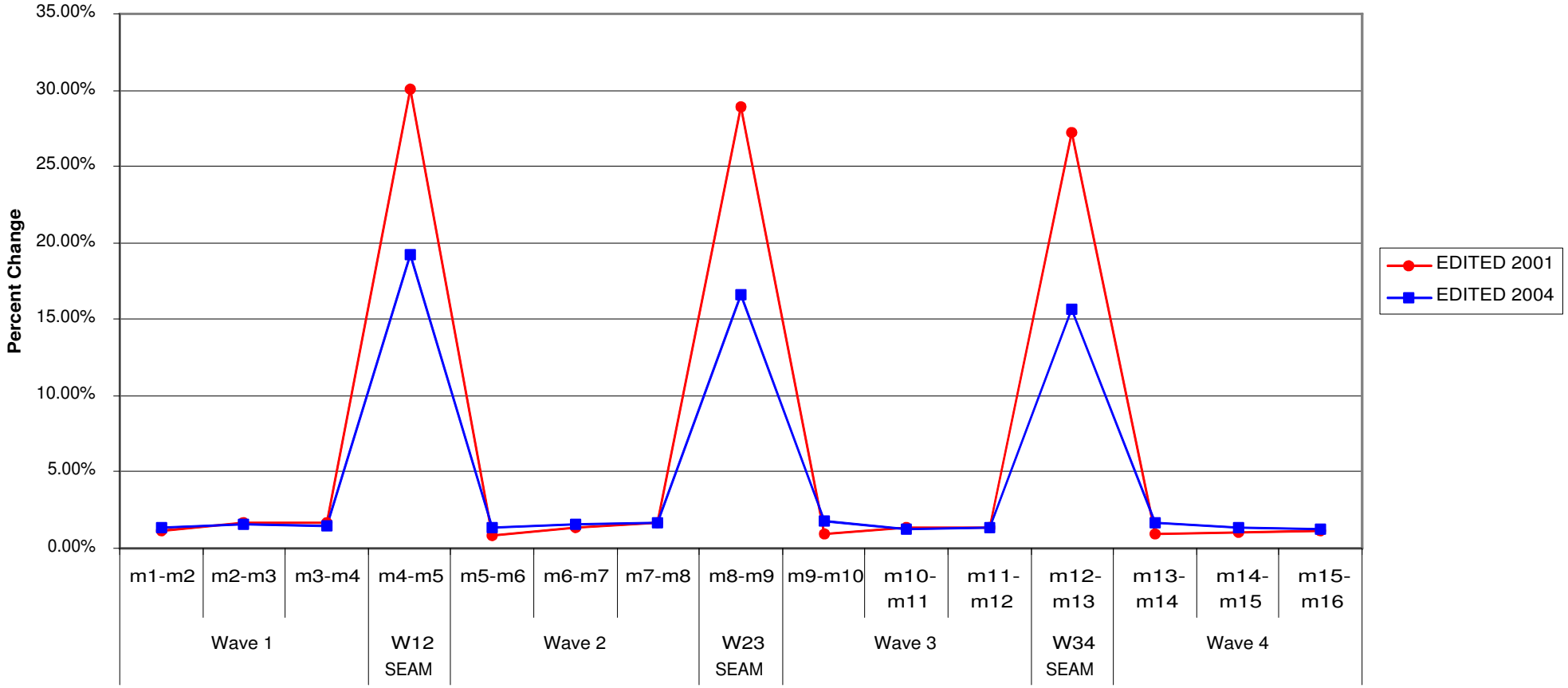

Figure 1.A.2: MONTH-TO-MONTH FEDERAL SSI CHANGES (unweighted) IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4

(edited data using cases with any Federal SSI receipt interviewed in all 4 waves)

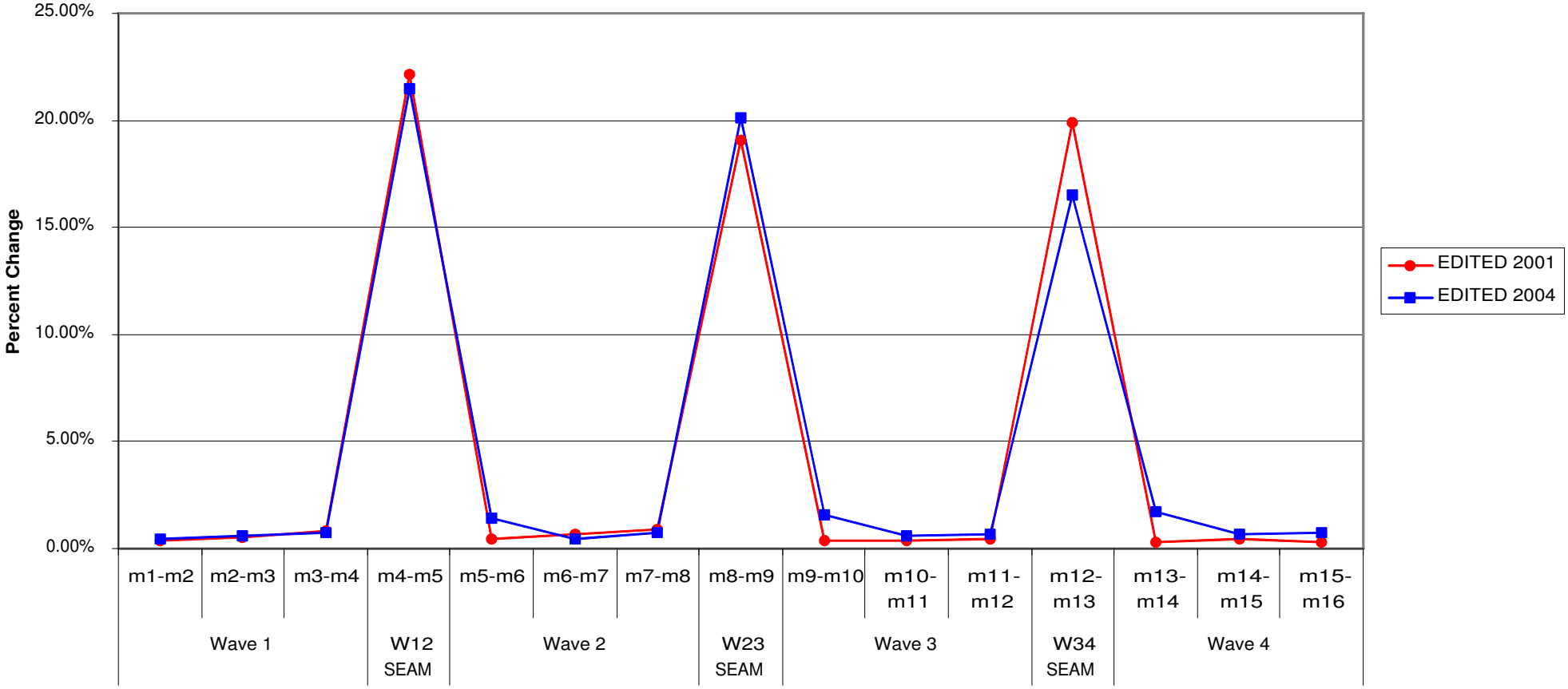

Figure 1.A.3: MONTH-TO-MONTH VETERANS' COMPENSATION/PENSION CHANGES (unweighted) IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4

(edited data using cases with any Vets comp/pension receipt interviewed in all 4 waves)

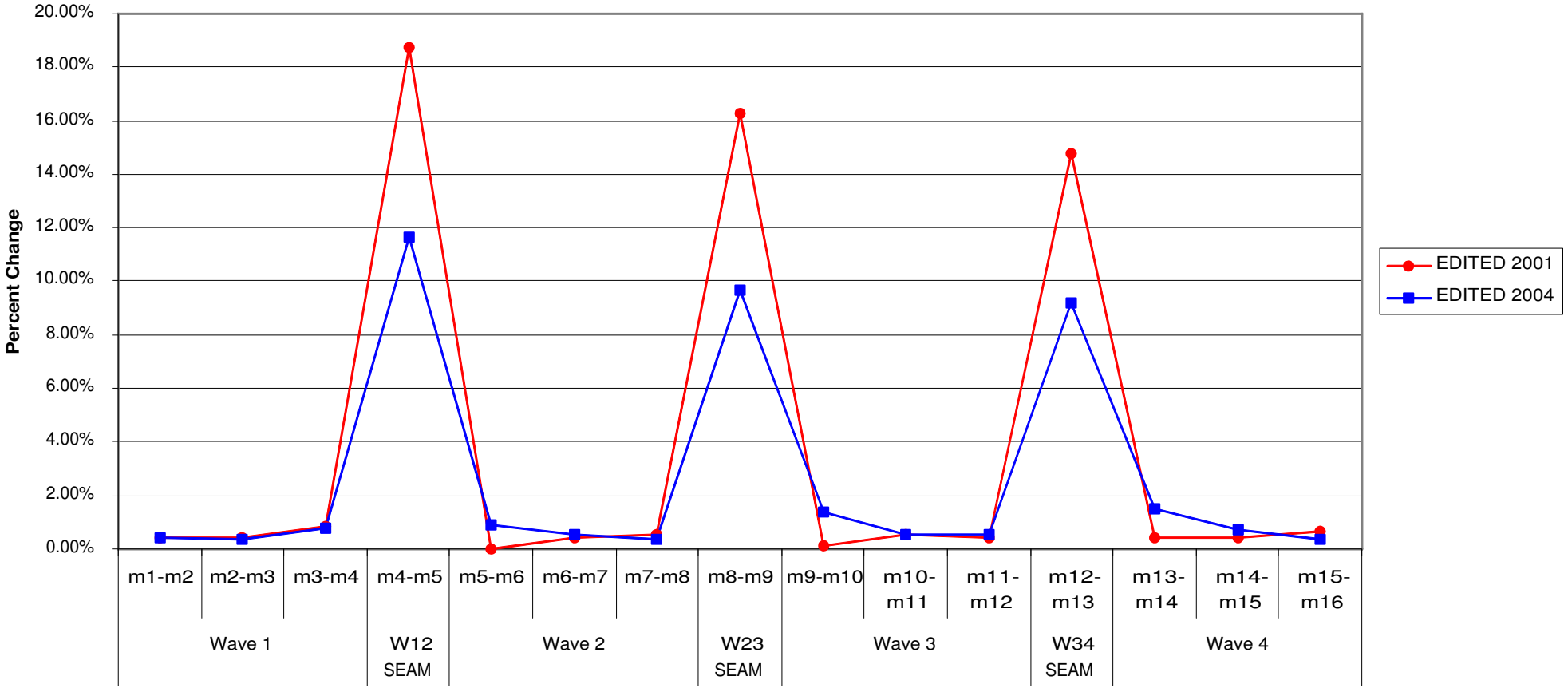

Figure 1.A.4: MONTH-TO-MONTH AFDC/TANF CHANGES (unweighted) IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4

(edited data using cases with any AFDC/TANF receipt interviewed in all 4 waves)

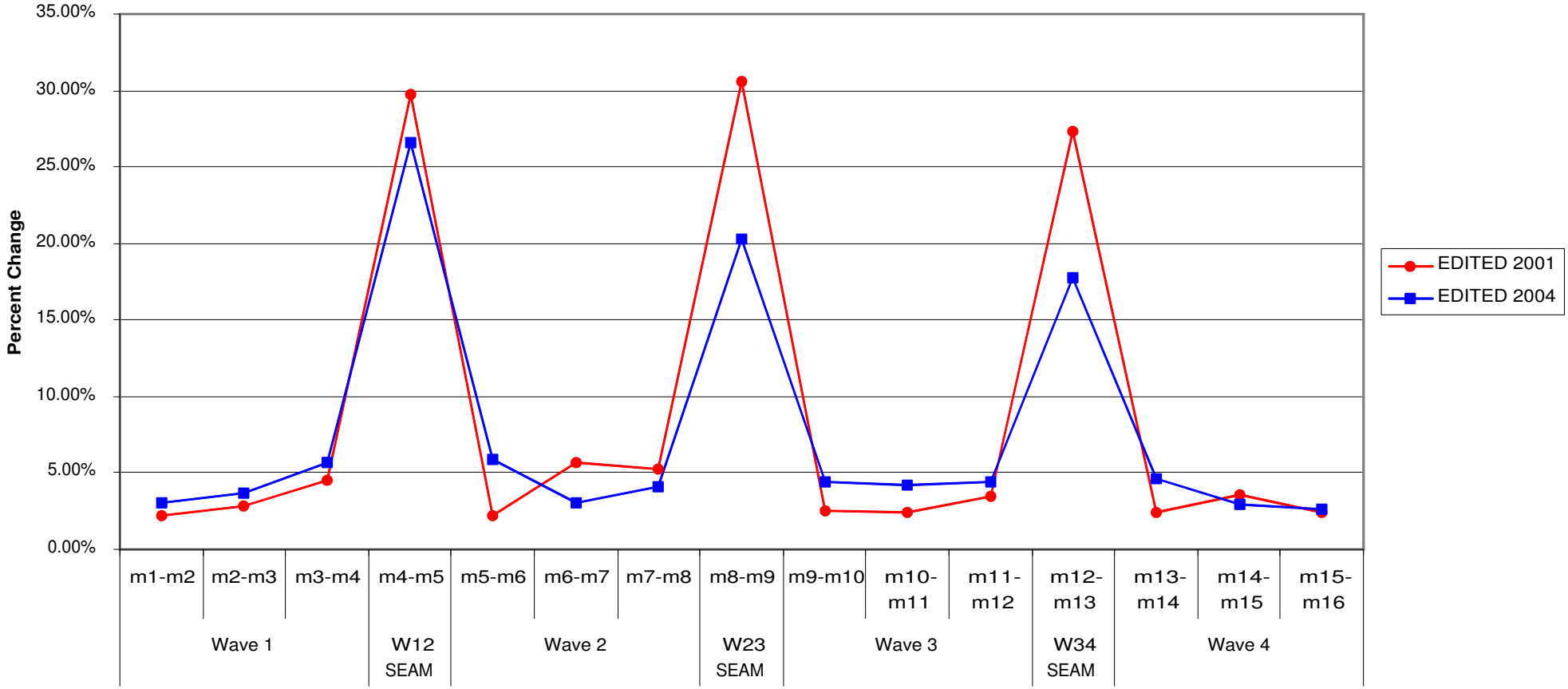

Figure 1.A.5: MONTH-TO-MONTH WIC CHANGES (unweighted)
IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4
(edited data using cases with any WIC receipt interviewed in all 4 waves)

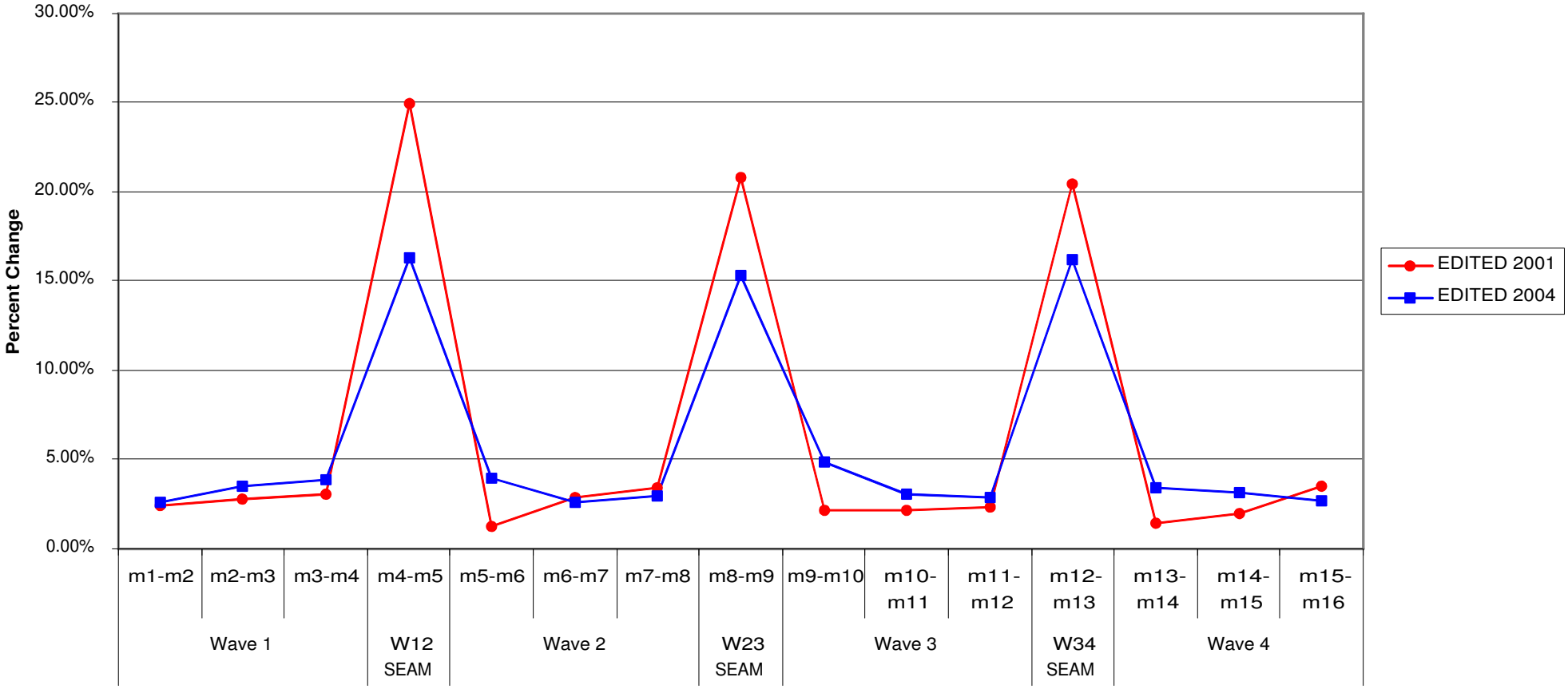

Figure 1.A.6: MONTH-TO-MONTH FOOD STAMPS CHANGES (unweighted) IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4

(edited data using cases with any Food Stamps receipt interviewed in all 4 waves)

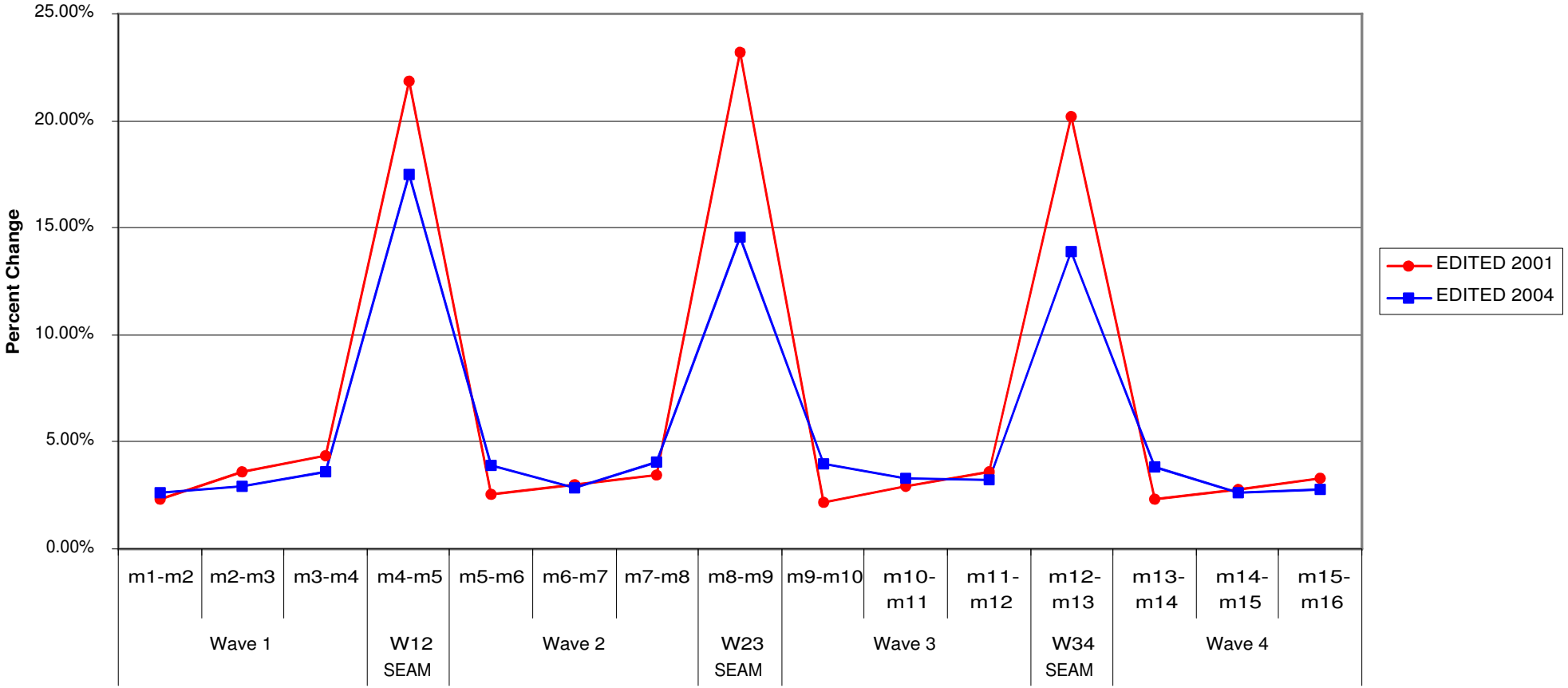

Figure 1.B.1: MONTH-TO-MONTH PRIVATE HEALTH INSURANCE COVERAGE CHANGES (unweighted) IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4

(edited data using cases with any private coverage interviewed in all 4 waves)

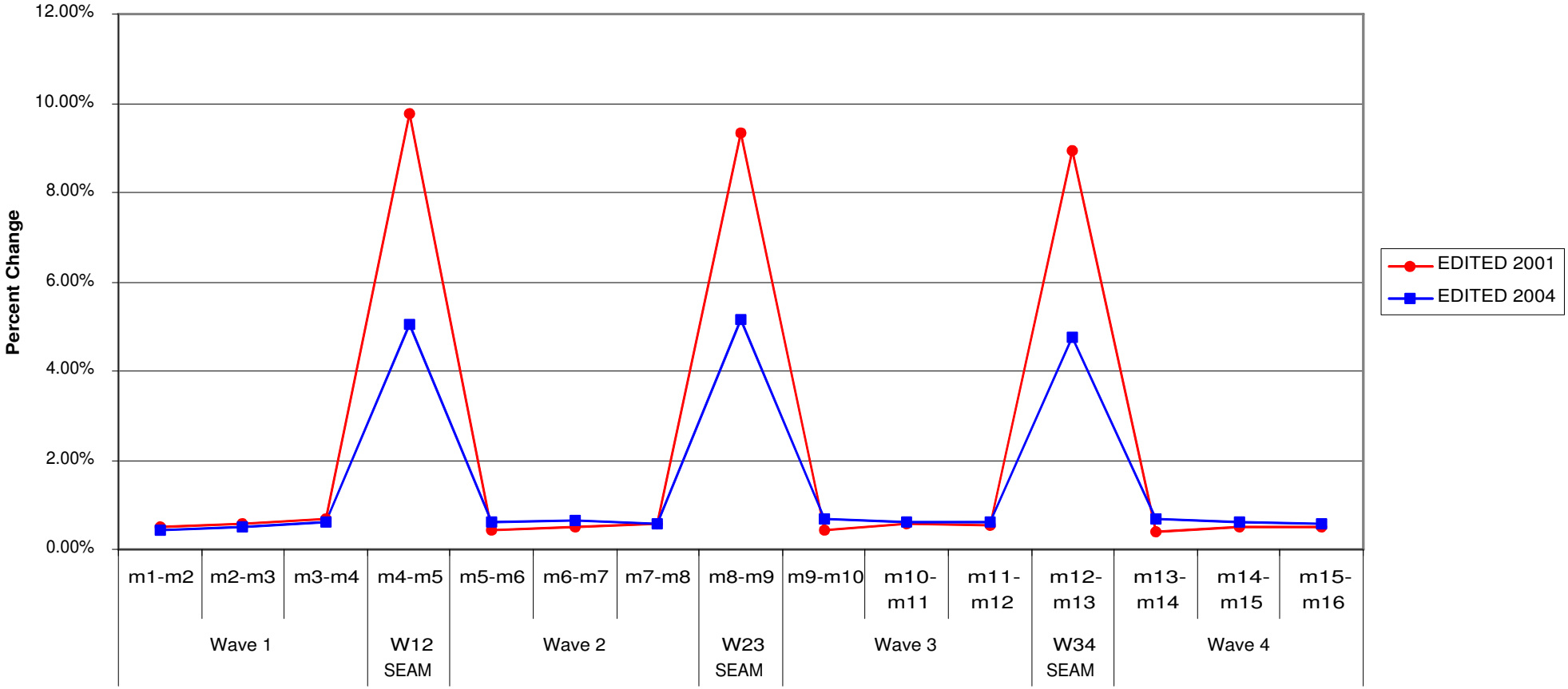

Figure 1.B.2: MONTH-TO-MONTH SOCIAL SECURITY CHANGES (unweighted) IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4

(edited data using cases with any Social Security receipt interviewed in all 4 waves)

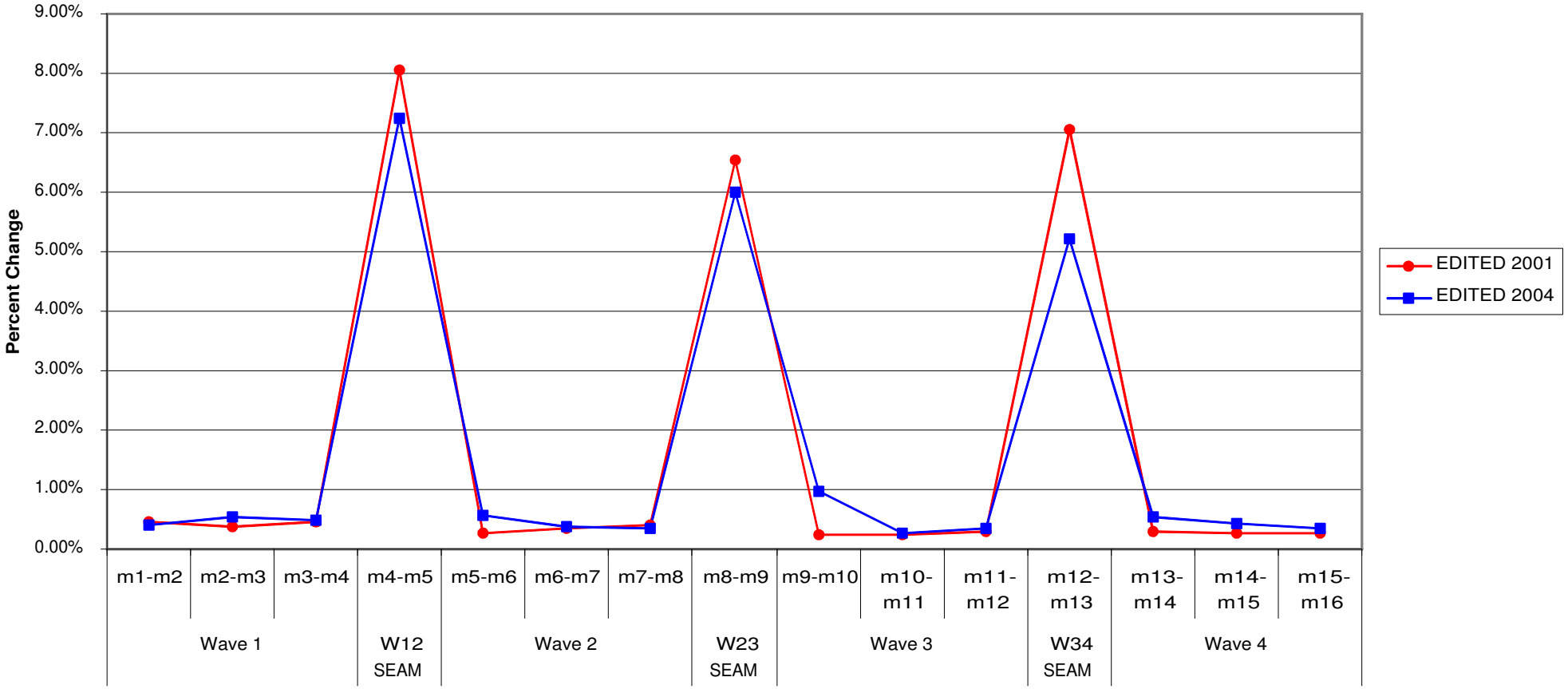

Figure 1.B.3: MONTH-TO-MONTH WORKERS COMPENSATION CHANGES (unweighted) IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4

(edited data using cases with any Workers Comp receipt interviewed in all 4 waves)

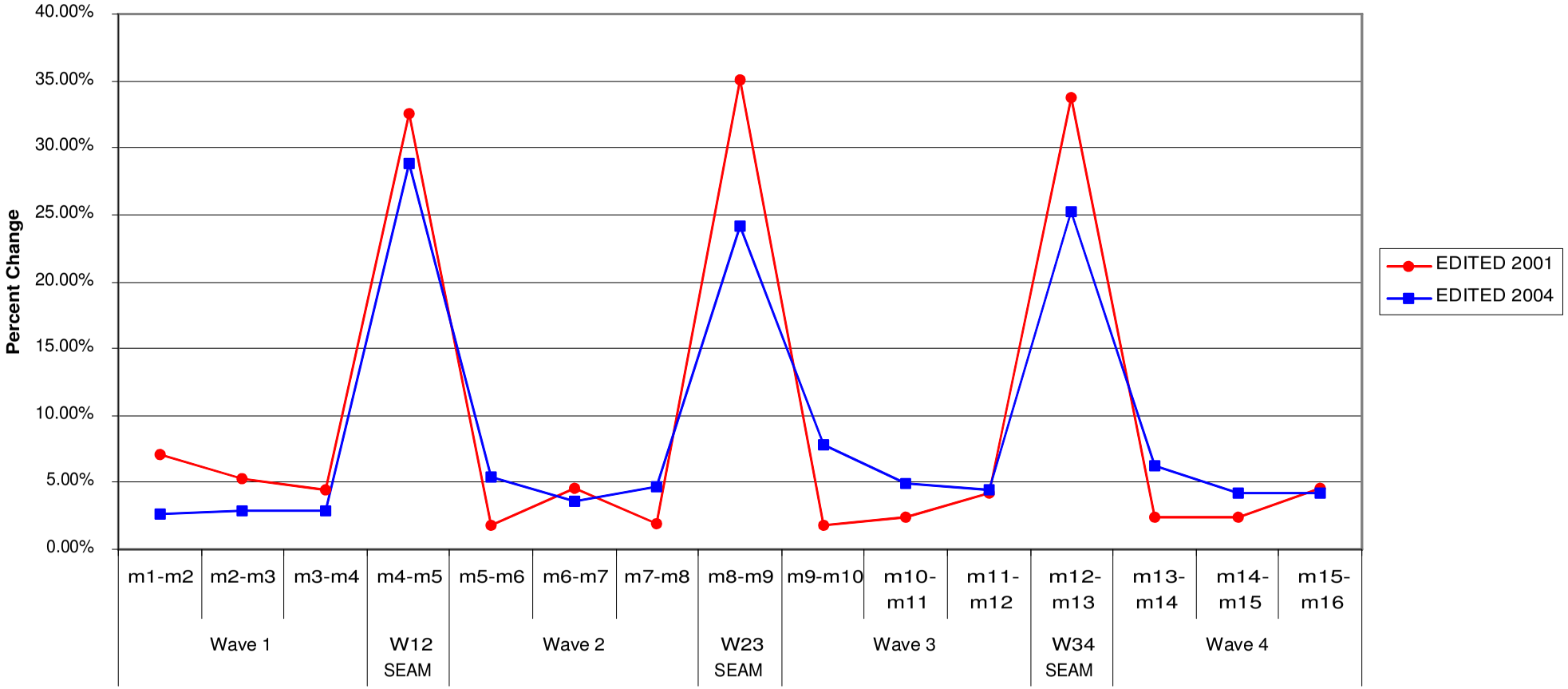

Figure 1.B.4: MONTH-TO-MONTH CHILD SUPPORT RECEIPT CHANGES (unweighted) IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4

(edited data using cases with any child support receipt interviewed in all 4 waves)

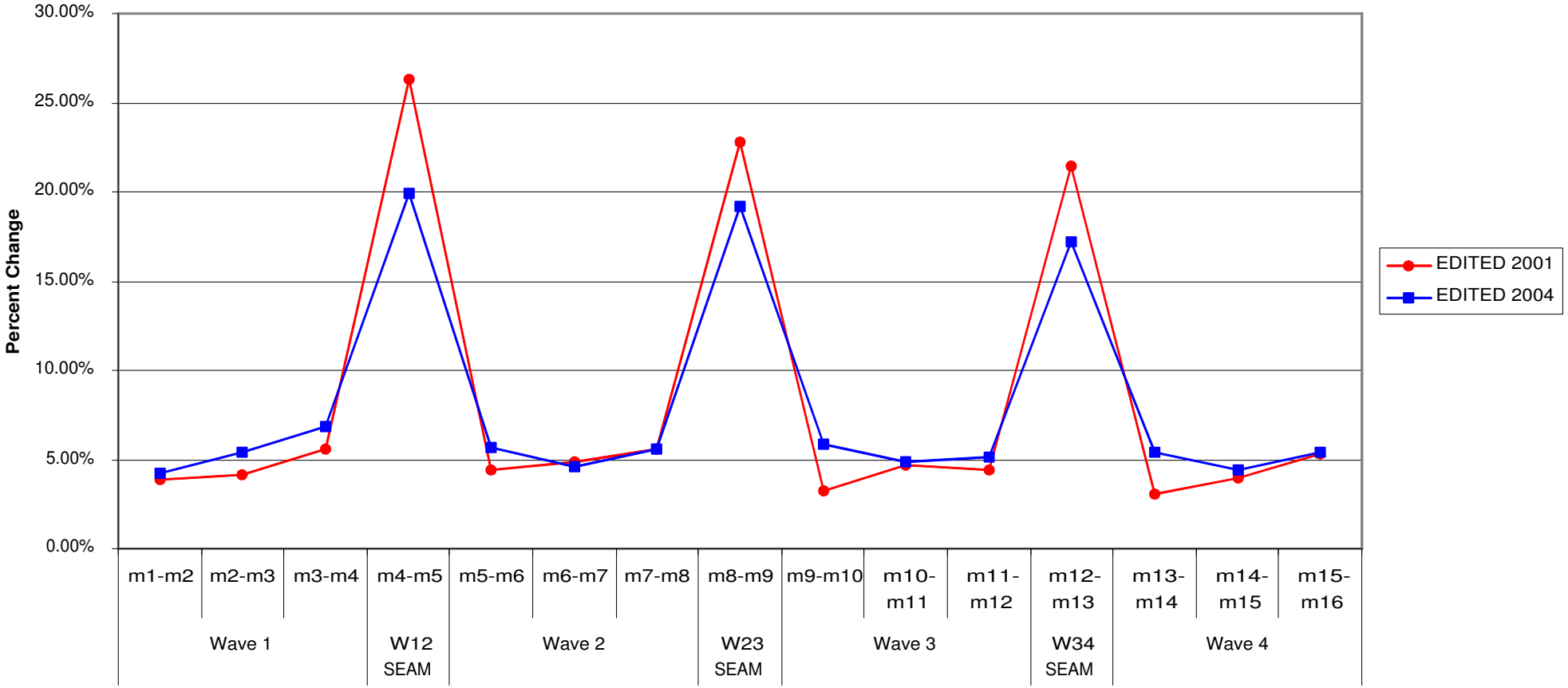

Figure 1.B.5: MONTH-TO-MONTH ALIMONY CHANGES (unweighted)
IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4
(edited data using cases with any alimony receipt interviewed in all 4 waves)

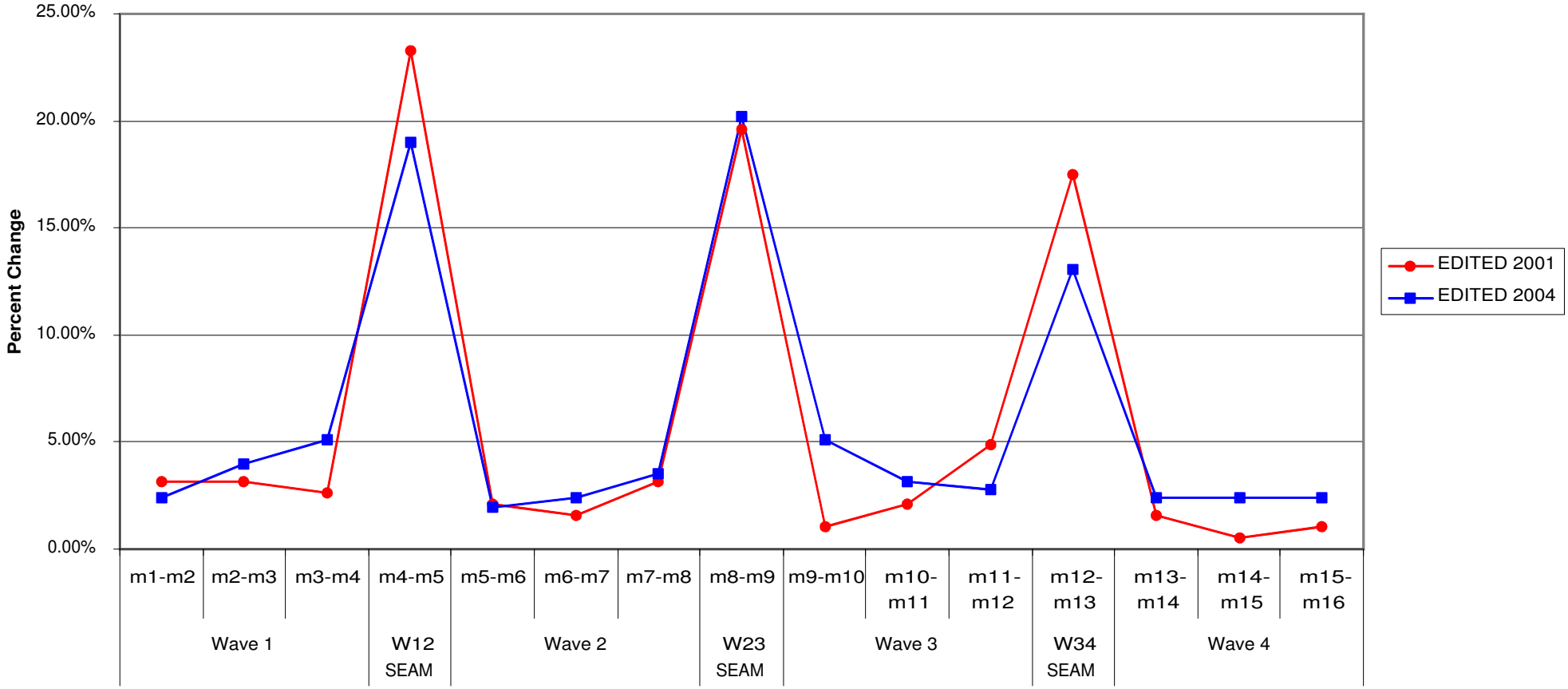

Figure 1.B.6: MONTH-TO-MONTH COMPANY/UNION PENSION CHANGES (unweighted) IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4

(edited data using cases with any company/union pension receipt interviewed in all 4 waves)

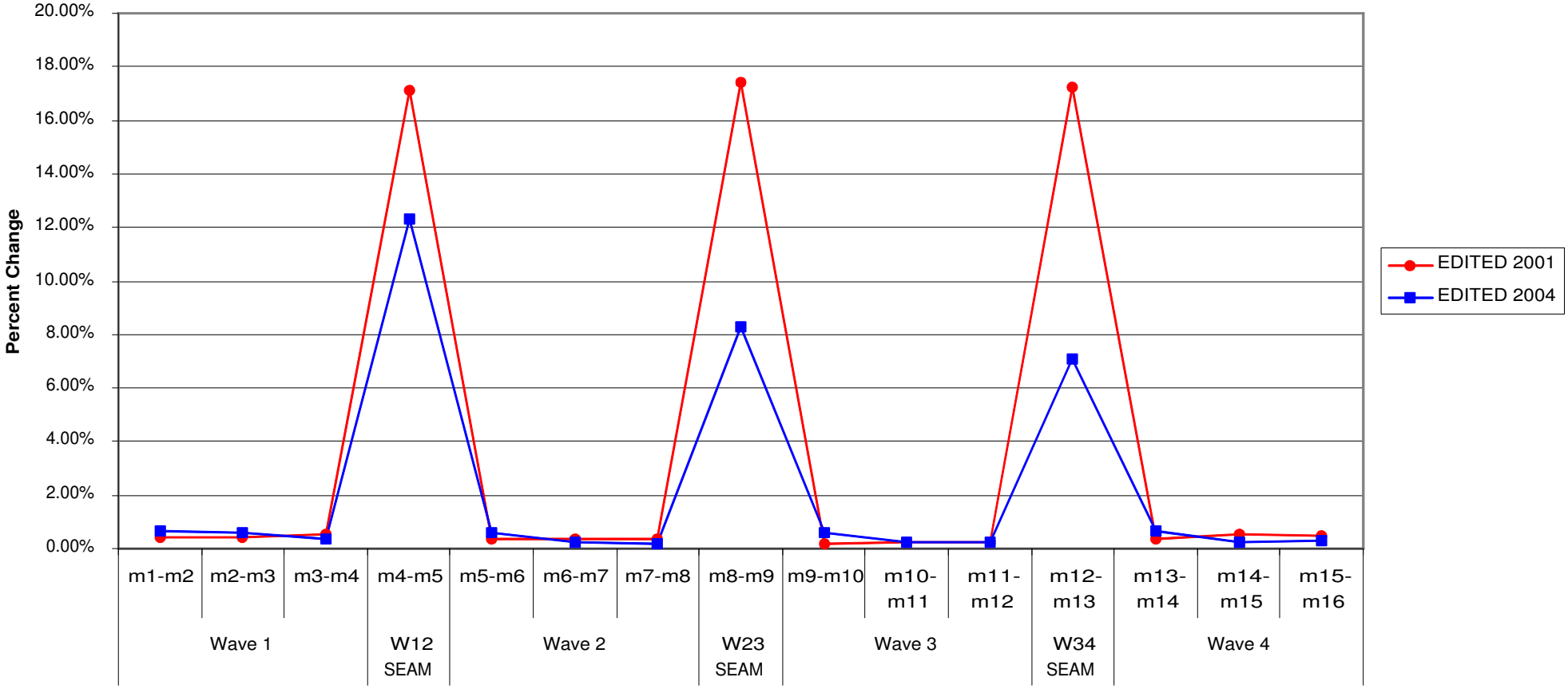

Figure 1.B.7: MONTH-TO-MONTH FED. CIVIL SERVICE PENSION CHANGES (unweighted) IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4

(edited data using cases with any Fed. Civil Service pension receipt interviewed in all 4 waves)

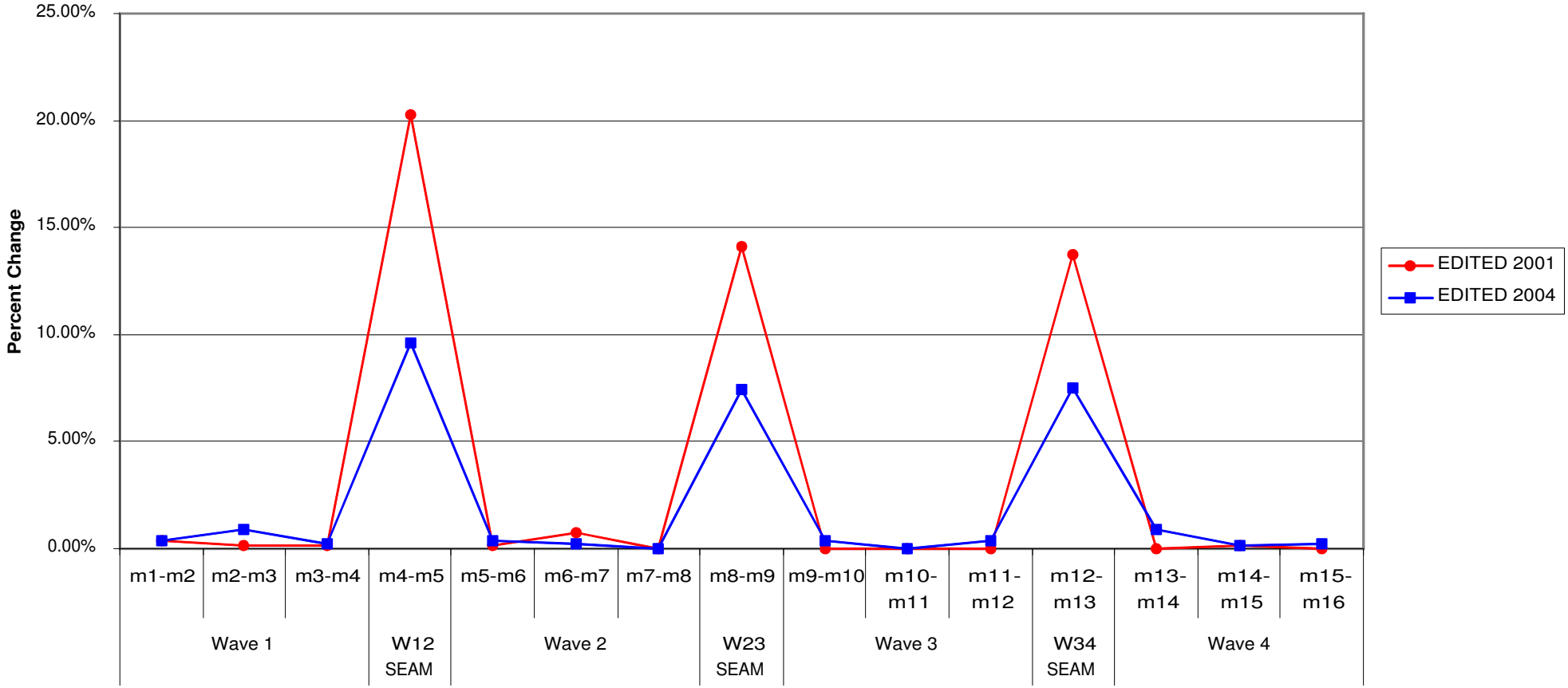

Figure 1.B.8: MONTH-TO-MONTH "WEALTH" INCOME CHANGES (unweighted) IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4

(edited data using cases with any "wealth" income receipt interviewed in all 4 waves)

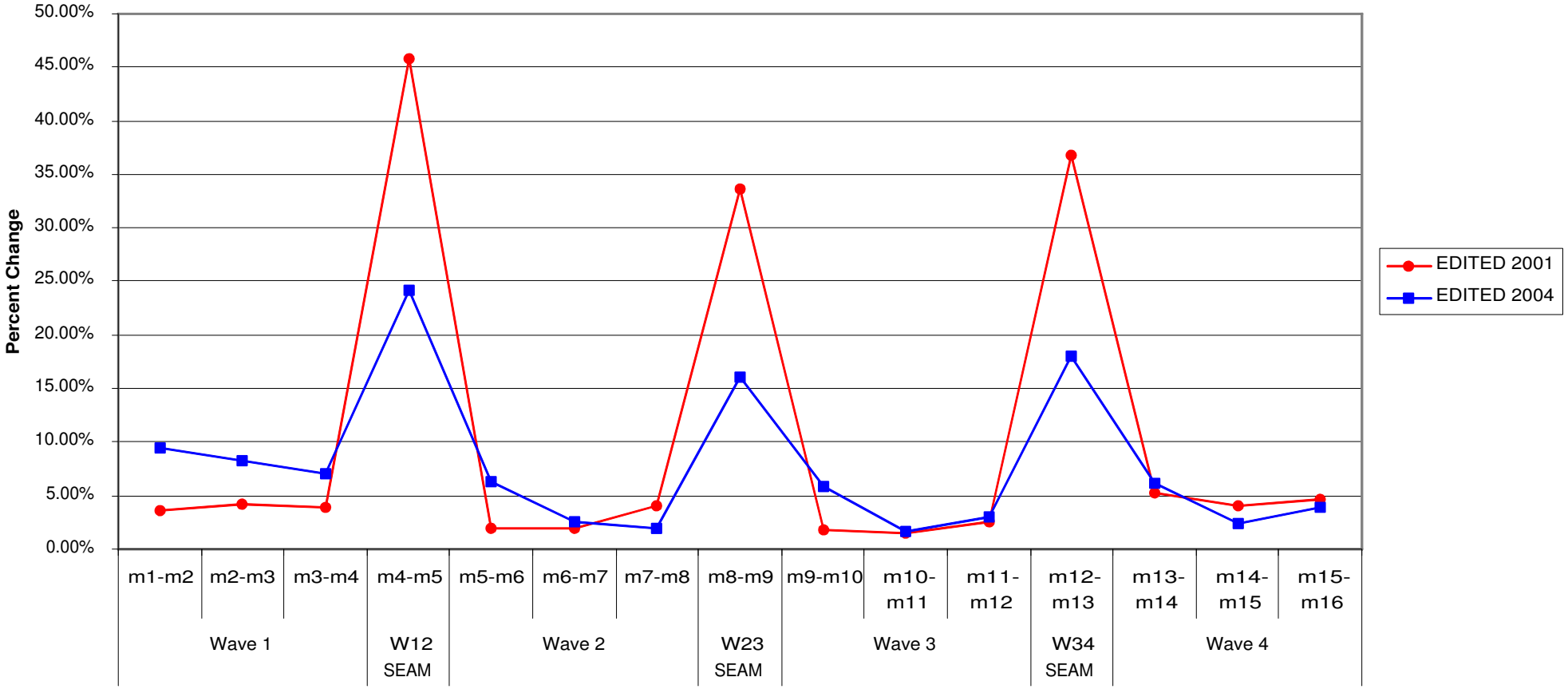

Figure 1.B.9: MONTH-TO-MONTH SCHOOL ENROLLMENT CHANGES (unweighted) IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4

(edited data using cases with any school enrollment interviewed in all 4 waves)

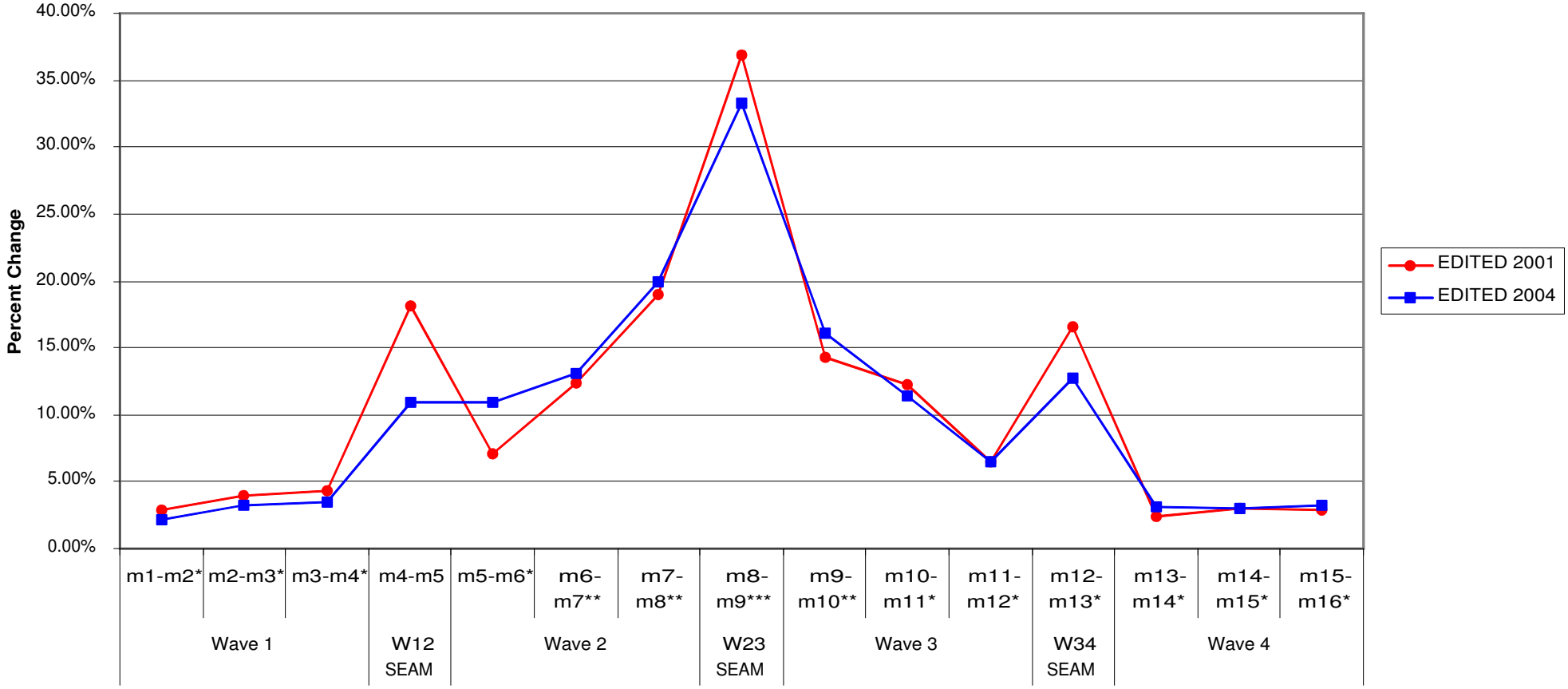

Figure 1.B.9b: SCHOOL ENROLLMENT: Of All Changes Observed in a Month-Pair, % Arising from the Rotation Group for Which that Month-Pair is at the Seam
(Edited Data, SIPP 2001 and 2004, Waves 1 - 4)

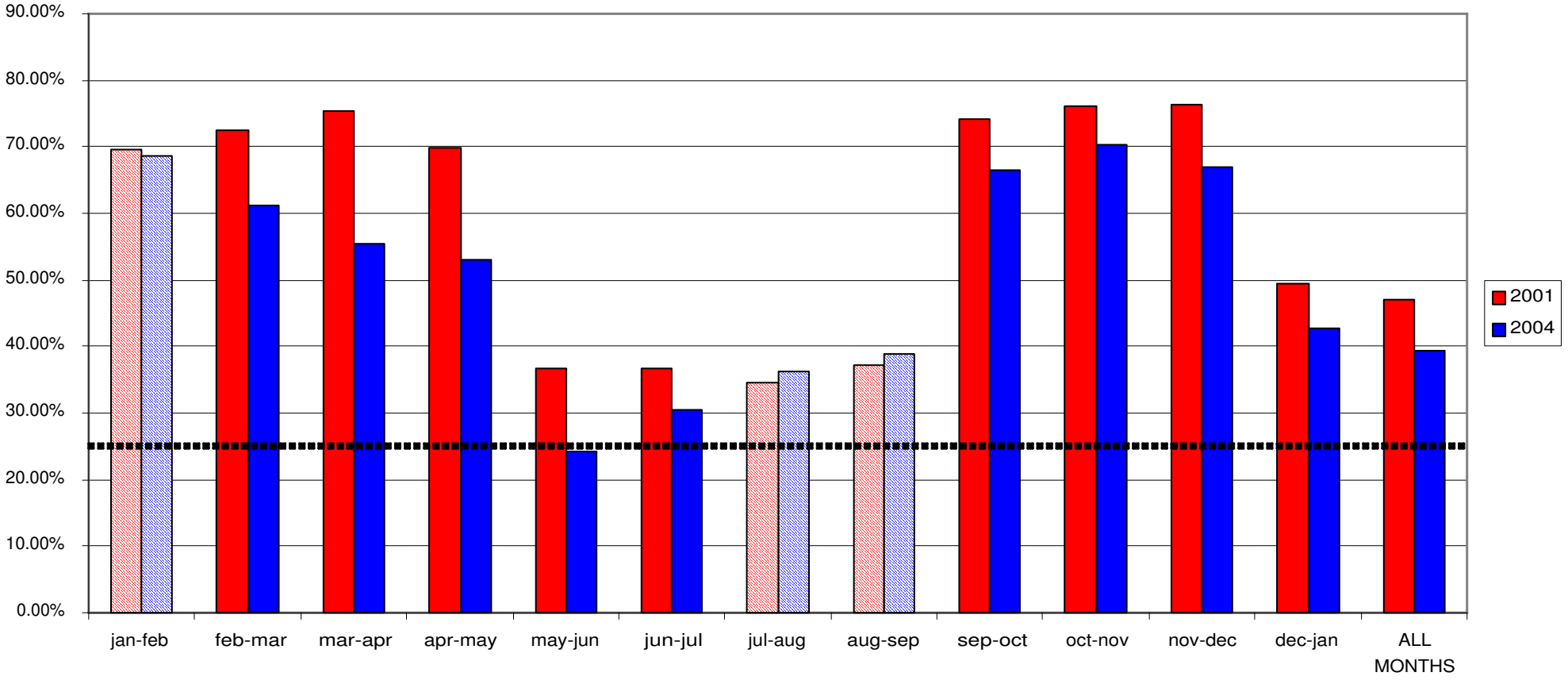

Figure 2.1: MONTH-TO-MONTH JOB CHANGES (unweighted)
IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 & 2 ONLY
(edited data using cases with any job interviewed in both waves)

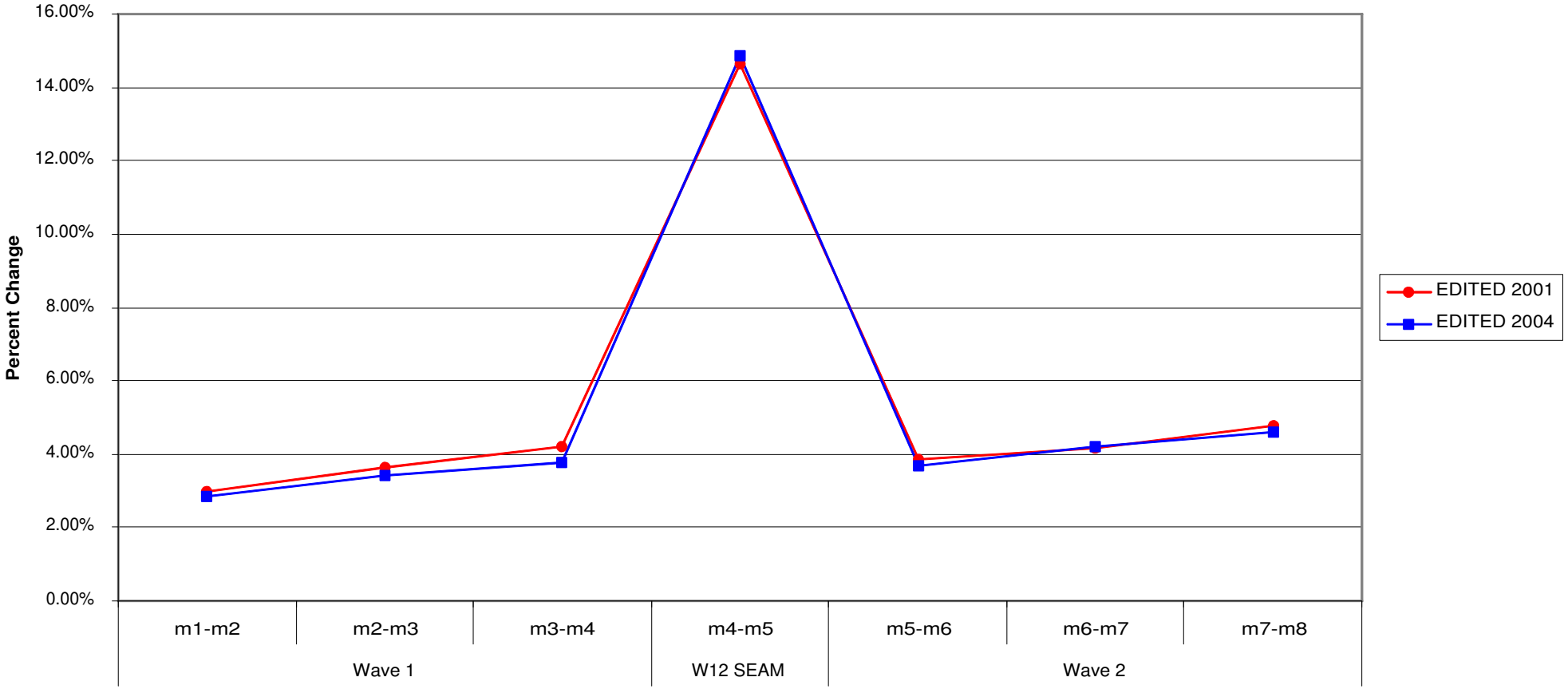

Figure 2.2: MONTH-TO-MONTH MEDICARE COVERAGE CHANGES (unweighted) IN THE 2001 AND 2004 SIPP PANELS, WAVES 1 - 4

(edited data using cases with any Medicare coverage interviewed in all 4 waves)

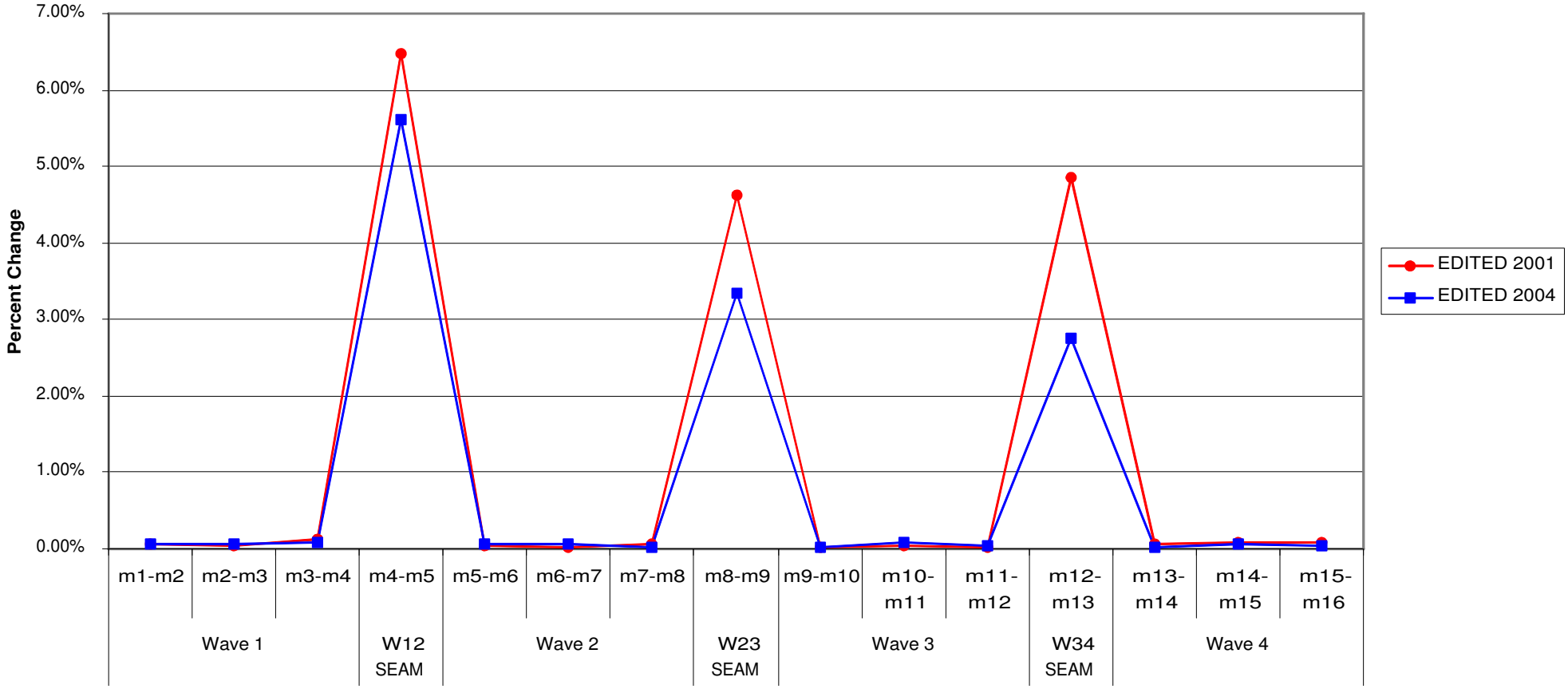

Figure 3.1a: MEDICAID/PUBLIC HEALTH INSURANCE COVERAGE (unweighted)
Of all "Yes's" in Month n, % that Changed to "No" in Month n+1
(edited data using cases with any Medicaid/public coverage interviewed in all 4 waves)

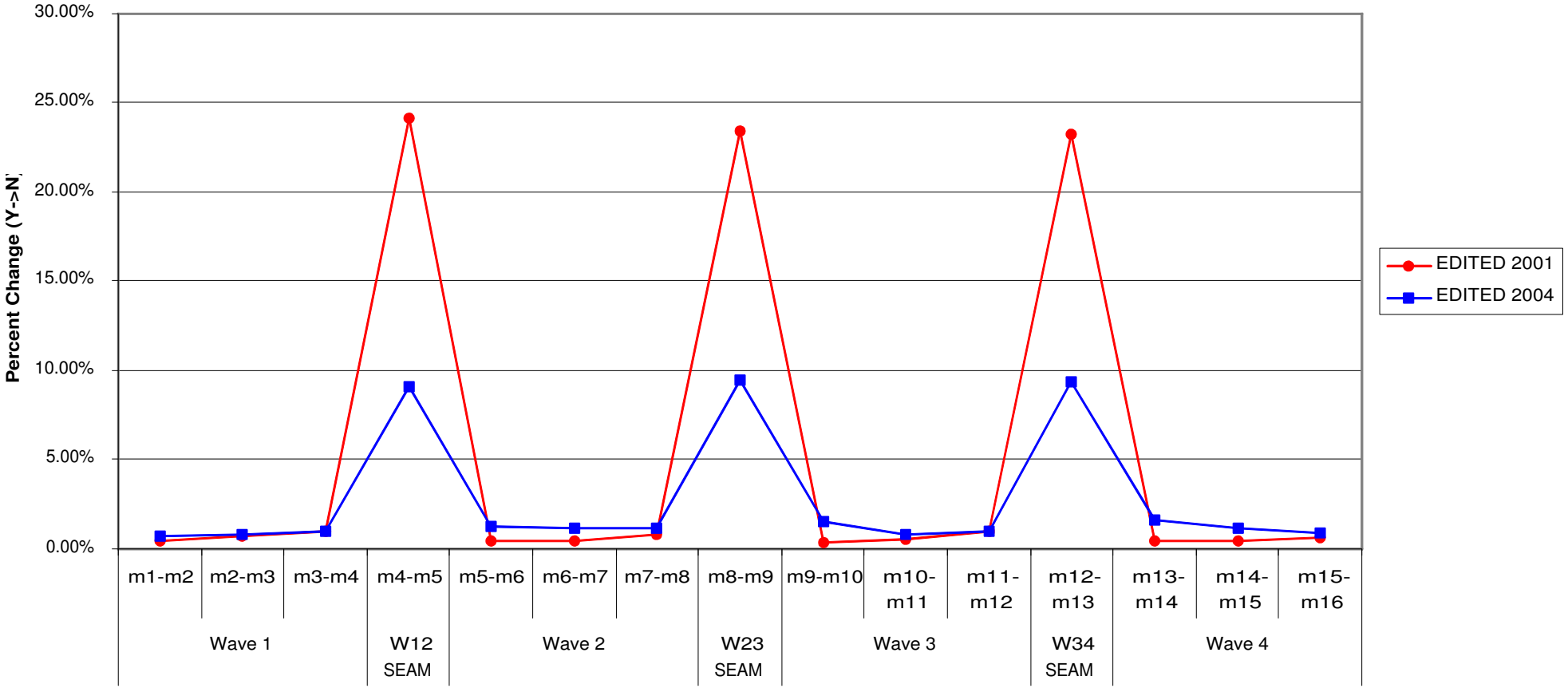

Figure 3.1b: MEDICAID/PUBLIC HEALTH INSURANCE COVERAGE (unweighted)
Of all "No's" in Month n, % that Changed to "Yes" in Month n+1
(edited data using cases with any Medicaid/public coverage interviewed in all 4 waves)

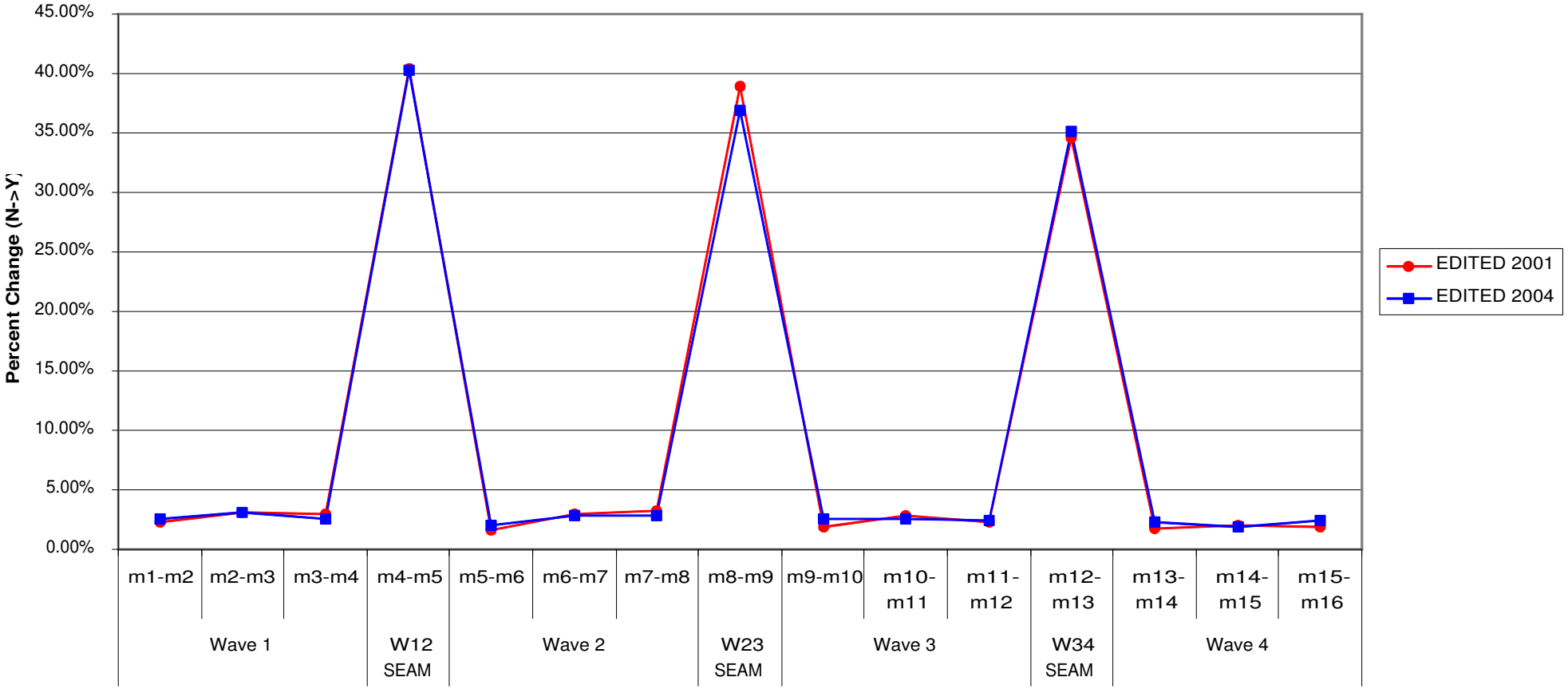

Figure 3.2a: FOOD STAMPS RECEIPT (unweighted)
Of all "Yes's" in Month n, % that Changed to "No" in Month n+1
(edited data using cases with any Food Stamps receipt interviewed in all 4 waves)

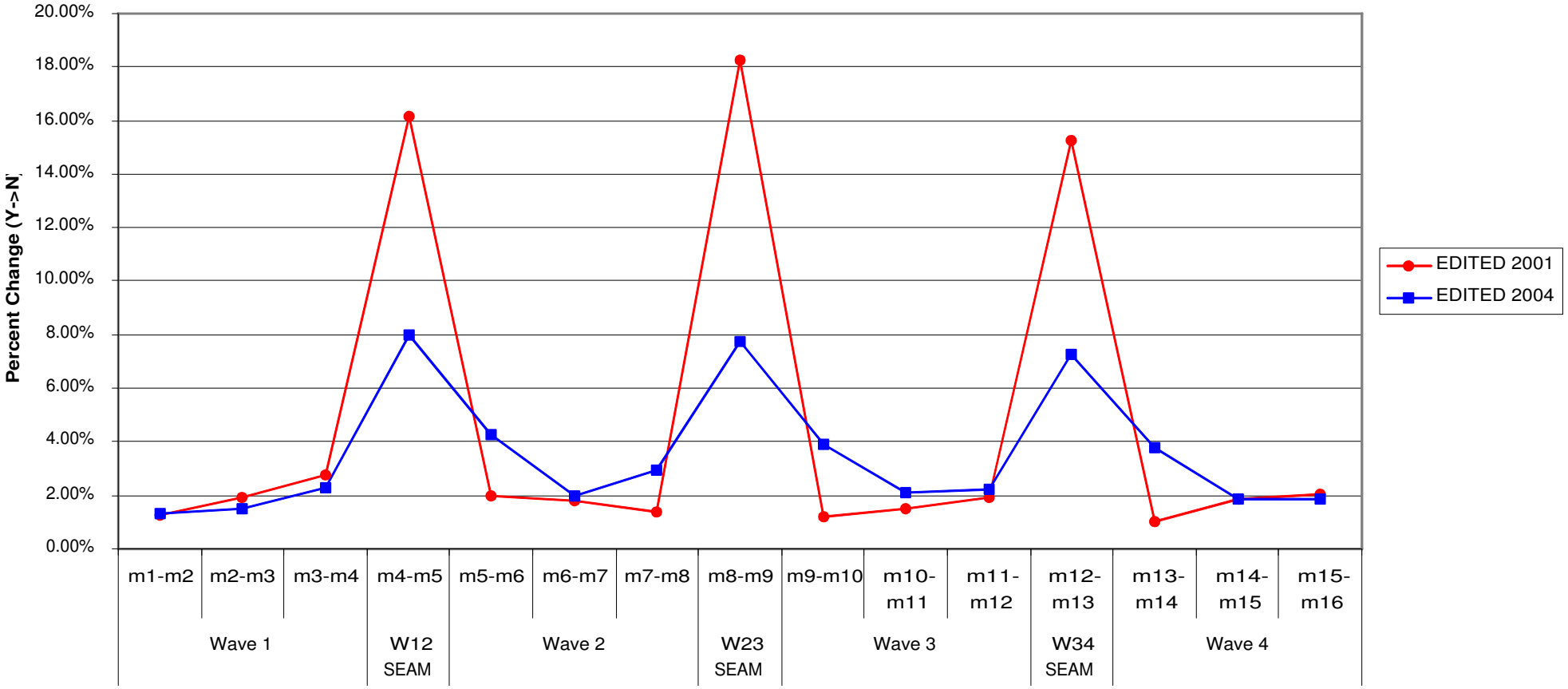

Figure 3.2b: FOOD STAMPS RECEIPT (unweighted)
Of all "No's" in Month n, % that Changed to "Yes" in Month n+1
(edited data using cases with any Food Stamps receipt interviewed in all 4 waves)

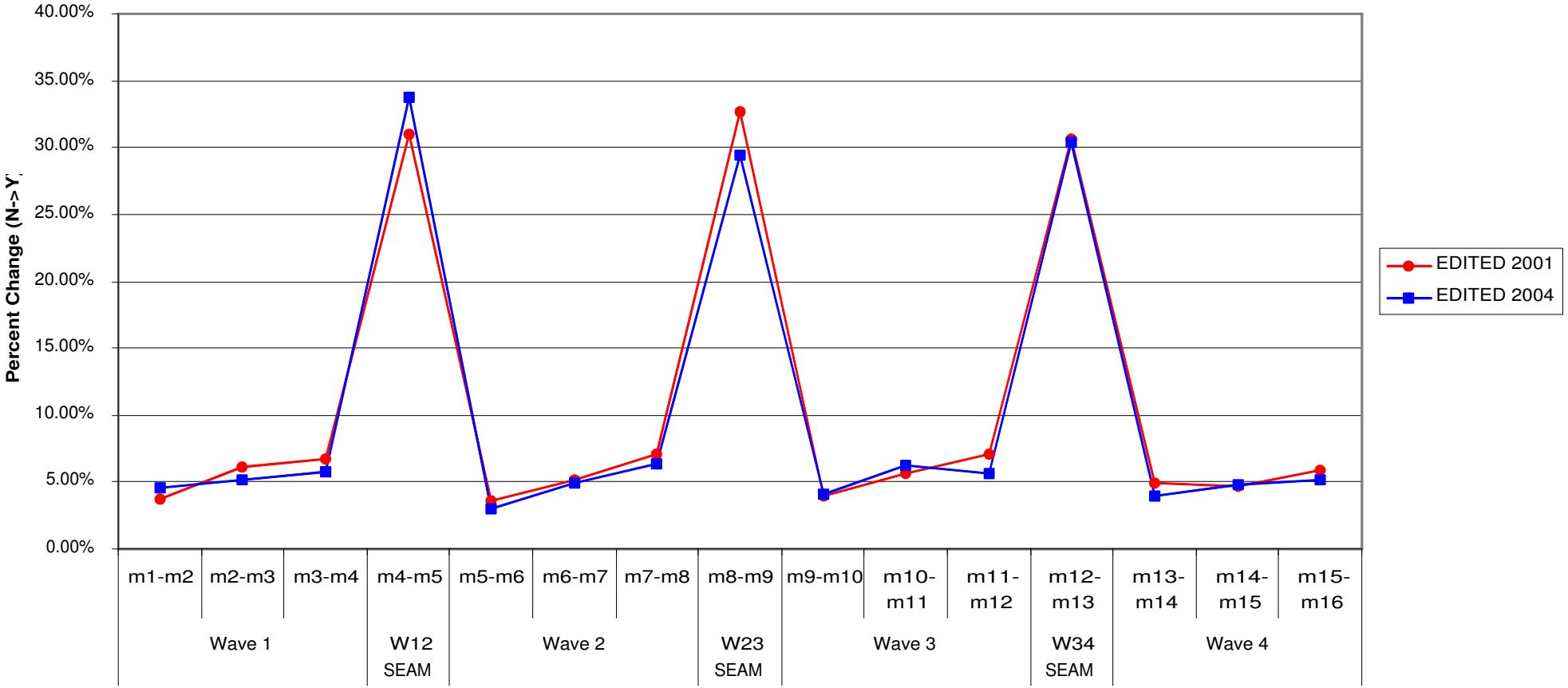

Figure 3.3a: PRIVATE HEALTH INSURANCE COVERAGE (unweighted)
Of all "Yes's" in Month n, % that Changed to "No" in Month n+1
(edited data using cases with any private coverage interviewed in all 4 waves)

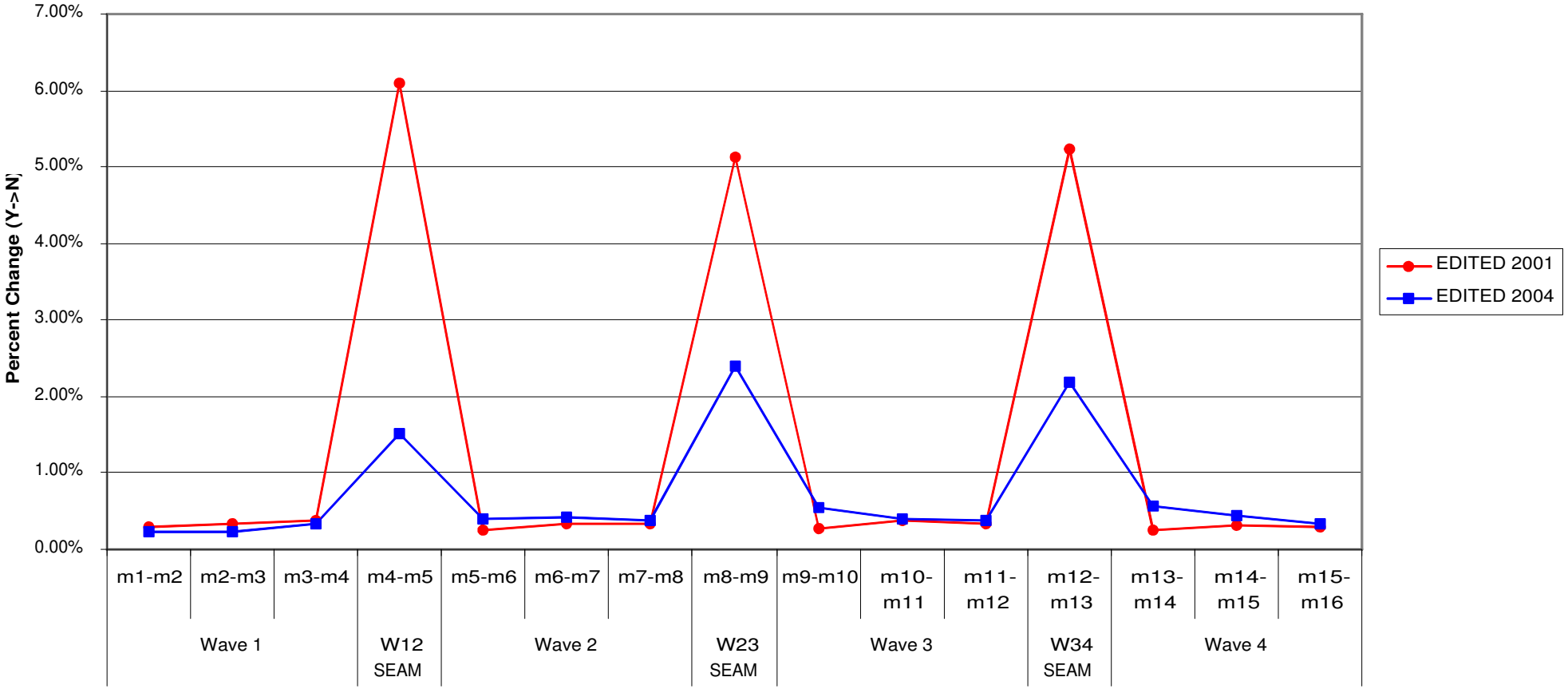

Figure 3.3b: PRIVATE HEALTH INSURANCE COVERAGE (unweighted)
Of all "No's" in Month n, % that Changed to "Yes" in Month n+1
(edited data using cases with any private coverage interviewed in all 4 waves)

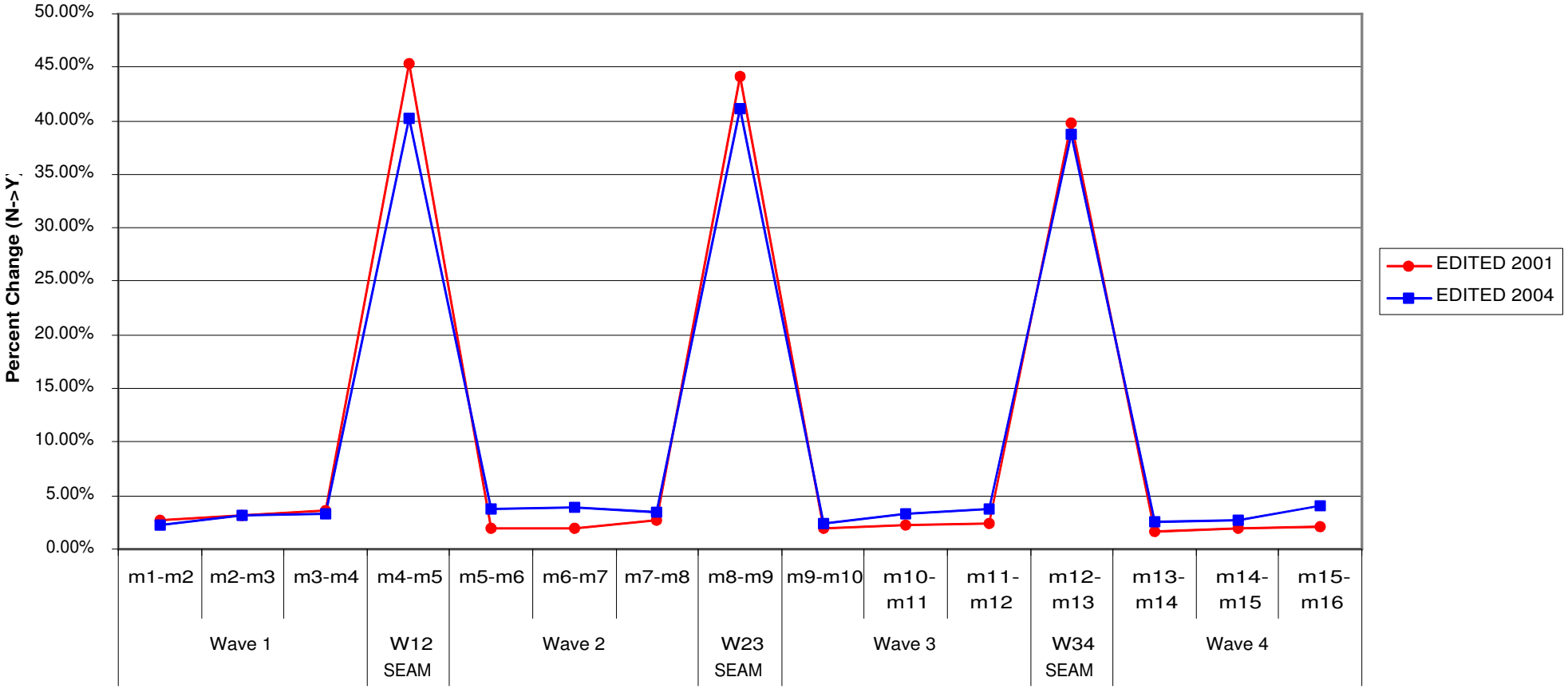

Figure 3.4a: "WEALTH" INCOME (unweighted)
Of all "Yes's" in Month n, % that Changed to "No" in Month n+1
(edited data using cases with any "wealth" income receipt interviewed in all 4 waves)

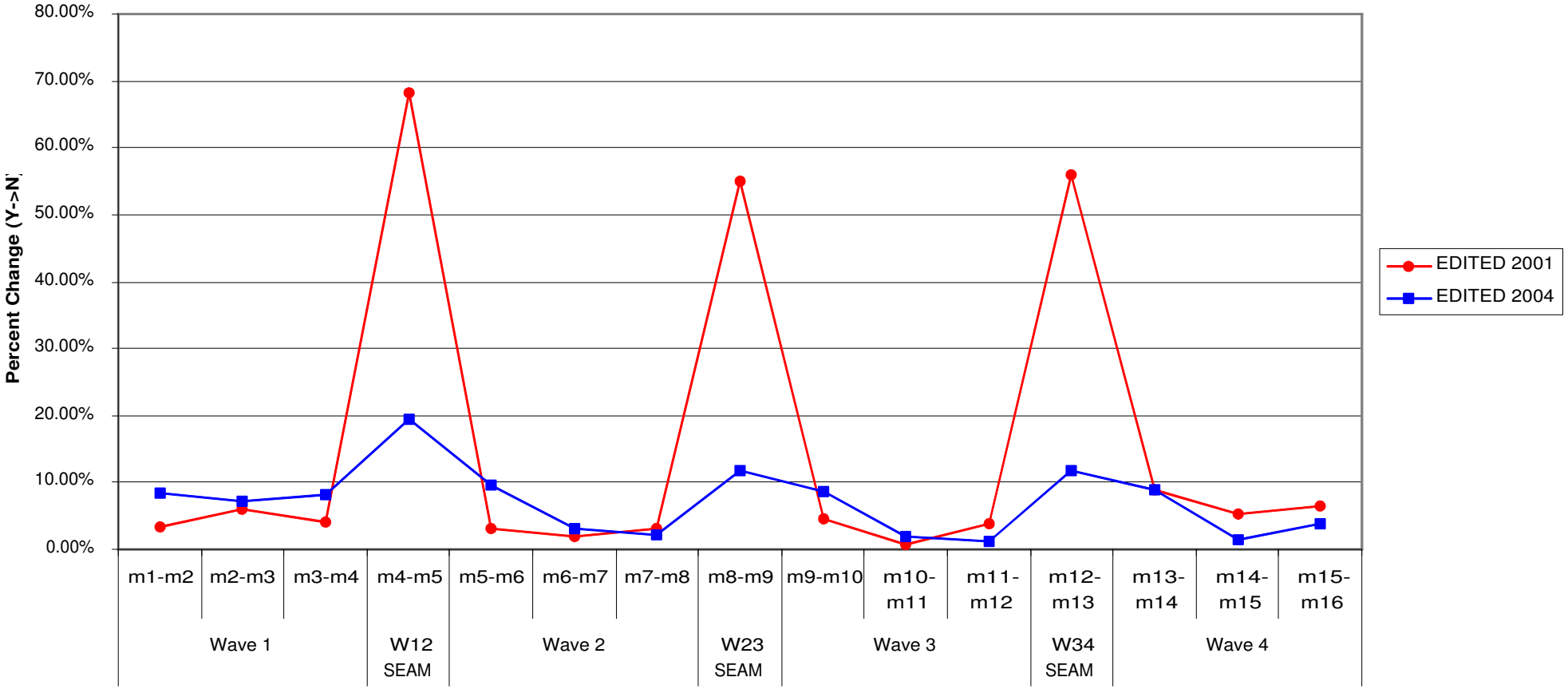

Figure 3.4b: "WEALTH" INCOME (unweighted)
Of all "No's" in Month n, % that Changed to "Yes" in Month n+1
(edited data using cases with any "wealth" income receipt interviewed in all 4 waves)

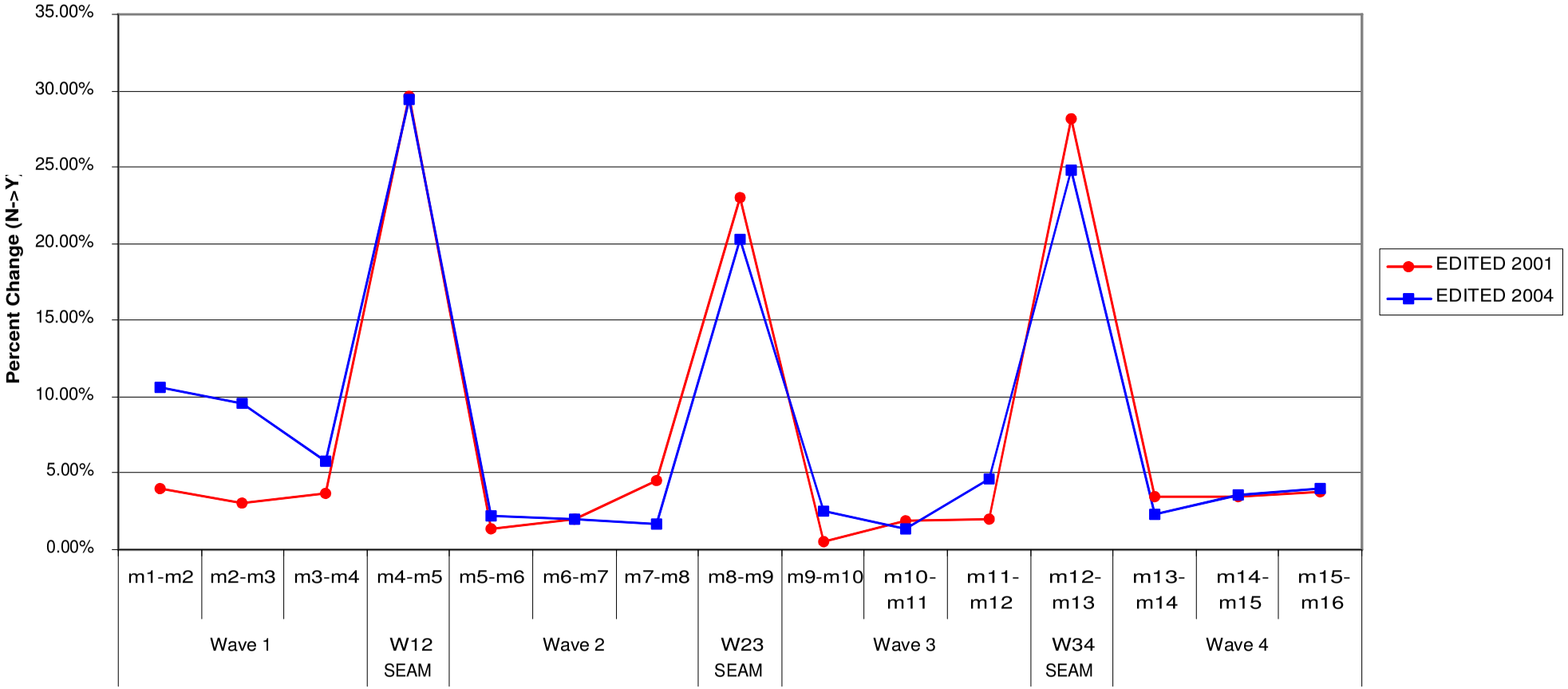

Table 1: ANALYSIS SUMMARY TABLE – SEAM BIAS IN THE 2001 AND 2004 SIPP PANELS ACROSS WAVES 1 THROUGH 4

Each analysis includes cases with interview data in all 4 waves and with any instance of the characteristic in any wave. Data source: edited SIPP 2001 and 2004 data files, unweighted.

(1)	(2)	(3)	(4)	(5)		(6)	(7)	
CHARACTERISTIC	PANEL	ANALYSIS N's Analyzed Interviews (Total Observed Changes)	% OF ALL CHANGES THAT WERE AT THE SEAM	MONTH-TO-MONTH CHANGE RATES (%)		CHANGE RATE RATIO[2/]: SEAM/ OFF-SEAM	"DIRECTIONAL" CHANGE RATES AT THE SEAM (% change in 2004 compared to 2001)	
				Off-Seam	Seam		% of "Yes's" that changed to "No"	% of "No's" that changed to "Yes"
PART 1: CHARACTERISTICS MEASURED WITH NEW DI PROCEDURES IN THE 2004 PANEL								
A. NEED-BASED PROGRAMS								
1. "Public" Health Insurance Coverage (e.g., Medicaid)	2001	6,315 (6,413)	**85.0%**[1/]	1.3	**28.8**	22.2	**23.6**	38.0
	2004	9,354 (6,480)	74.3%	**1.5**	17.2	11.5	9.3 (-61%)	37.7 (-0.8%)
2. Receipt of Federal SSI (Supplemental Security Income)	2001	1,866 (1,250)	**91.2%**	0.5	20.4	40.8	**13.2**	41.5
	2004	2,601 (1,781)	84.9%	**0.9**	19.4	21.6	10.0 (-24%)	**47.6** (+15%)
3. Receipt of Veterans' Compensation/ Pensions	2001	731 (389)	**93.6%**	0.3	**16.6**	55.3	**8.7**	42.1
	2004	1,131 (440)	78.4%	**0.7**	10.2	14.6	4.3 (-51%)	44.9 (+7%)
4. Receipt of AFDC/ TANF	2001	666 (848)	**68.9%**	3.3	**29.2**	8.8	**27.8**	**31.1**
	2004	938 (1,062)	57.1%	**4.1**	21.5	5.2	17.3 (-38%)	26.8 (-14%)
5. Receipt of WIC (Women, Infants, Children) Benefits	2001	1,462 (1,413)	**68.5%**	2.5	**22.1**	8.8	**16.2**	33.3
	2004	1,947 (1,704)	54.7%	**3.3**	16.0	4.8	8.5 (-48%)	31.2 (-6%)

CHARACTERISTIC	PANEL	ANALYSIS N's Analyzed Interviews (Total Observed Changes)	% OF ALL CHANGES THAT WERE AT THE SEAM	MONTH-TO-MONTH CHANGE RATES (%)		CHANGE RATE RATIO[2/]: SEAM/ OFF-SEAM	"DIRECTIONAL" CHANGE RATES AT THE SEAM (% change in 2004 compared to 2001)	
				Off-Seam	Seam		% of "Yes's" that changed to "No"	% of "No's" that changed to "Yes"
A. NEED-BASED PROGRAMS (continued)								
6. Receipt of Food Stamps	2001	2,558 (2,599)	**64.2%**	3.0	**21.7**	7.2	**16.6**	31.4
	2004	4,467 (3,824)	53.7%	**3.3**	15.3	4.6	7.7 (-54%)	31.3 (0%)
B. OTHER (NON-NEED-BASED) CHARACTERISTICS								
1. Private Health Insurance Coverage	2001	39,899 (13,690)	**81.7%**	0.5	**9.3**	18.6	**5.5**	**42.9**
	2004	54,300 (12,004)	67.9%	**0.6**	5.0	8.3	2.0 (-64%)	40.1 (-7%)
2. Receipt of Social Security	2001	10,134 (2,592)	**84.6%**	0.3	**7.2**	24.0	**3.2**	**44.5**
	2004	15,152 (3,652)	76.6%	**0.5**	6.2	12.4	2.3 (-28%)	42.1 (-5%)
3. Receipt of Workers' Compensation	2001	454 (655)	**70.2%**	3.6	**33.8**	9.4	**41.2**	28.3
	2004	447 (592)	59.1%	**4.5**	26.1	5.8	19.9 (-52%)	30.9 (+9%)
4. Receipt of Child Support Payments	2001	1,855 (2,288)	**57.2%**	4.4	**23.5**	5.3	**17.1**	34.9
	2004	2,955 (3,540)	47.0%	**5.3**	18.8	3.5	11.1 (-35%)	34.0 (-3%)
5. Receipt of Alimony	2001	189 (165)	**69.1%**	2.2	20.1	9.1	14.8	33.3
	2004	253 (227)	58.1%	3.1	17.4	5.6	12.1 (-18%)	27.5 (-17%)

CHARACTERISTIC	PANEL	ANALYSIS N's Analyzed Interviews (Total Observed Changes)	% OF ALL CHANGES THAT WERE AT THE SEAM	MONTH-TO-MONTH CHANGE RATES (%)		CHANGE RATE RATIO[2/]: SEAM/ OFF-SEAM	"DIRECTIONAL" CHANGE RATES AT THE SEAM (% change in 2004 compared to 2001)	
				Off-Seam	Seam		% of "Yes's" that changed to "No"	% of "No's" that changed to "Yes"
B. OTHER (NON-NEED-BASED) CHARACTERISTICS (continued)								
6. Receipt of Private Pensions	2001	3,892 (2,221)	**90.7%**	0.4	**17.2**	43.0	**7.4**	**47.8**
	2004	4,795 (1,569)	84.8%	0.4	9.3	23.3	3.1 (-58%)	44.3 (-7%)
7. Receipt of Federal Civil Service Pensions	2001	553 (276)	**96.4%**	0.2	**16.0**	80.0	**8.7**	44.3
	2004	804 (230)	85.7%	**0.3**	8.2	27.3	3.1 (-64%)	37.6 (-15%)
8. Receipt of "Wealth" Income (Annuities; Estates/Trusts)	2001	517 (805)	**74.7%**	3.3	**38.7**	11.7	**60.5**	26.8
	2004	612 (716)	49.7%	**4.9**	19.4	4.0	14.4 (-76%)	24.8 (-7%)
9. School Enrollment	2001	8,833 (14,364)	**44.1%**	7.6	**23.9**	3.1	**22.3**	26.5
	2004	12,116 (18,553)	37.1%	**8.0**	18.9	2.4	12.6 (-43%)	**30.3** (+14%)
PART 2: CHARACTERISTICS MEASURED WITH THE SAME DI PROCEDURES IN BOTH PANELS								
1. Employment at the Same Job	2001	41,048 [3/] (15,721)	38.2%	**3.9**	14.6	3.7	8.2	41.1
	2004	56,979 [3/] (21,320)	**39.7%**	3.8	14.9	3.9	8.2 (0%)	**42.7** (+4%)
2. Medicare	2001	8950 (1,490)	95.9%	0.06	**5.3**	88.3	**1.2**	52.3
	2004	12,934 (1,593)	95.1%	0.05	3.9	78.0	0.6 (-50%)	50.3 (-4%)

NOTES

[1/] Estimates shown in **bold** font are significantly higher ($t > 2.0$, $p < .05$) than the corresponding estimate for the other panel.

[2/] The "Change Rate Ratio" estimates are illustrative only – because they are derived directly from the "Off-Seam" and "Seam" month-to-month change rates in the preceding two columns, they have not been subjected to statistical testing.

[3/] Waves 1 and 2 only, due to instrument errors which affected dependent information about jobs after wave 2, especially in the 2004 panel.