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Dependent Interviewing and Seam Effects in Work History Data

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Dependent interviewing has been introduced by a number of panel surveys as a means of reducing measurement error, in particular the typically observed concentration of transitions at the seam between waves, the "seam effect." Little evidence exists, however, of the effects on survey estimates. We report on a large-scale randomised experiment comparing dependent interviewing with traditional independent methods. Proactive dependent interviewing improves the quality of work history data by reducing seam effects in estimates of monthly labour force transitions and eliminates differential seam effects across subgroups. Proactive interviewing does not have any effect on measures of cumulative experience and does not appear to lead to under-reporting of change. Seam transitions in continuous work histories are caused by response errors but can be either visible or hidden, depending on the editing rules used to reconcile reports from repeated panel observations. Proactive methods reduce seam effects by precluding overlapping noncorresponding reports. The potential for eliminating seam effects is, however, limited by item nonresponse to questions about dates.

Key words: Seam bias; recall error; labour force transitions.

1. Introduction

The collection of work history information in surveys is notoriously difficult – and yet increasingly of interest, due to the development of sophisticated methods for investigating dynamic causal relationships of life events. Retrospectively collected history data are affected by recall error, the inability of respondents to accurately recall events or circumstances from their past. Collecting information prospectively in panel or cohort studies attenuates the effect of recall error, by reducing the length of the recall period. Combining data from repeated panel observations to create continuous histories, however, typically leads to "seam effects:" an apparent concentration of transitions at the "seam" between two sources of data, or waves of a panel. In an attempt to reduce seam effects, many panel studies have introduced dependent interviewing methods. Little evidence exists, however, of the effects on estimates (see Mathiowetz and McGonagle 2000). Focusing on continuous work history data, we report results from the first large-scale

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randomised experiment comparing estimates from traditional independent and dependent interviewing.

The phenomenon that observed between-wave changes exceed within-wave changes was first noted for benefit receipt by Czajka (1983) in the U.S. Income Survey Development Program.[2] Seam effects have also been documented for gross changes in labour force status and occupational and industrial mobility in the U. S. Survey of Income and Program Participation (Hill 1994; Martini 1989; Ryscavage 1993), the Canadian Labour Market Activity Survey (Murray, Michaud, Egan and Lemaitre 1991) and the Canadian Survey of Labour and Income Dynamics (Cotton and Giles 1998). Lemaitre (1992) concludes that all current longitudinal surveys appear to be affected by seam problems, regardless of differences in the length of recall periods or other design features.

The increase in transitions at the seam is thought to be due to both under-reporting of within-wave changes and spurious transitions at the seam (Moore and Kasprzyk 1984). In both cases, reports for the period around the seam are inconsistent, leading either to a misdated or an entirely spurious transition at the date of interview between the two reference periods. Under-reporting occurs either deliberately or due to memory decay, in which case respondents tend to use their current situation as a heuristic to report about their past (Tourangeau, Rips, and Rasinski 2000). Rips, Conrad, and Fricker (2003) proposed a theory according to which respondents, who cannot recall events that occurred during the reference period, either guess or repeat answers about their current situation and show that the resulting "constant wave responses" can produce seam effects. Spurious transitions occur if the reports for adjacent waves of data collection do not correspond, although in reality no change has occurred. Inconsistencies can be caused by differences in the respondent's reports or differences in coding or editing (Halpin 1998). Respondents may reinterpret events, or remember the events correctly but not the dates at which changes occurred. Coding error is particularly problematic for classifications of occupation and industry, since open-ended questions may be coded differently if descriptions are ambiguous or if respondents describe the same activity in different ways on different occasions. The longitudinal inconsistencies resulting from any of these sources of error lead to seam effects and can bias key estimates derived from work history data, such as spell durations and transition rates (Murray et al. 1991).

With the advent of computer assisted interviewing (CAI), many panel studies have introduced dependent interviewing techniques as a means of reducing measurement error. Information from previous waves can be fed forward to remind the respondent of previous responses (proactive dependent interviewing) or to ask for clarification about inconsistencies between reports (reactive dependent interviewing). For questions on occupation and industry proactive methods are generally used. The expectation is that reminding the respondent of his or her response at the previous wave will reduce the omission of spells and misdating of events early in the reference period, as well as avoiding differences in the way that the same state is reported or coded. The effect should therefore be a reduction in the concentration of change at the seam. A common concern,

[2] Similar effects have since been documented for benefit data in other surveys, such as the Survey of Income and Program Participation (Burkhead and Coder 1985) and the Panel Study of Income Dynamics (Hill 1987).

however, is that providing respondents with their previous answer may lead to underreporting of change.

Proactive dependent interviewing is used to collect work history data, which is to say information about the respondent's history of spells in work, unemployment or out of the labour force, in the U. S. Current Population Survey (Mathiowetz and McGonagle 2000), the SIPP (Hill 1994) and the SLID (Hale and Michaud 1995). Hill (1994) reported that the rates of change in occupation and industry measures in the SIPP were significantly reduced when proactive dependent interviewing was used. Hill's study appears to be the only published comparison of estimates derived from dependent and independent interviewing for work history data. It is restricted, however, to employment spells and estimates of transition rates. Other related studies are Hale and Michaud (1995), who examined the effects of dependent interviewing on the recall of retrospective labour market histories, and Murray et al. (1991), who compared stock and flow estimates based on dependent interviewing with aggregate administrative records.

This article examines the extent to which proactive dependent interviewing can improve the quality of work history data, in particular by reducing seam effects. We make several contributions. First, we compare estimates of monthly transition rates from proactive and independent interviewing, as did Hill (1994), but include all possible transitions between employment, unemployment and inactivity (not looking for work or being out of the labour force). Second, we provide new evidence of the effects of dependent interviewing on other estimates of interest: spell lengths for different activities and measures of cumulative work or unemployment experience. Third, we examine whether seam effects are differential between demographic subgroups, and whether dependent interviewing counters or exacerbates differential errors. The results indicate that proactive methods reduce bias in estimates sensitive to seam effects (monthly transition rates) and also eliminate differential seam effects between subgroups. Measures of spell lengths and cumulative experience are comparable across interviewing methods, suggesting that these estimates are not sensitive to seam effects.

Fourth, unlike previous studies we report in detail the editing rules used to construct the continuous work histories and illustrate how these influence seam effects. We show that when combining data from different interviews, analysts set a large proportion of transitions at the seam for want of more accurate or complete information. Proactive dependent interviewing precludes overlapping noncorresponding reports by design. However, we show that proactive methods remain sensitive to missing date problems and cannot entirely eliminate seam effects.

The dependent interviewing experiment and the construction of continuous work histories are described in Sections 2 and 3. In Section 4 we examine the effect of dependent interviewing on estimates derived from the work histories. We compare monthly transition rates, spell lengths, cumulative experiences and correlates of seam effects, using independently and proactively collected data. In Section 5 we investigate the origins of seam transitions and the mechanisms through which dependent interviewing produces different results. We compare reports within a wave across interviewing methods and then focus on the role of recall errors in retrospective reports and item nonresponse to date questions. Section 6 discusses the findings and draws conclusions.

2. The Dependent Interviewing Experiment

Our analysis is based on an experiment carried out as part of the project "Improving Survey Measurement of Income and Employment" (ISMIE). The project aim was to compare dependent and independent interviewing techniques by assessing their effect on estimates from sets of questions that were asked in three different ways. Proactive dependent interviewing questions used answers from the previous wave in the formulation of the question; reactive dependent interviewing used information fed forward to generate follow-up questions if the current and previous reports were inconsistent. The third version consisted of standard independent questions.

The experiment was carried out on a subsample of the UK part of the European Community Household Panel Survey (ECHP). This sample was interviewed annually from 1994 to 2001 and from 1997 jointly with the British Household Panel Survey (BHPS) activities. Funding for the ECHP expired after Wave 8 in 2001, giving us the opportunity to interview Wave 8 respondents once more in early 2003 for purely methodological purposes. Due to attrition over eight waves, and the subsampling mechanism which skewed the sample towards lower-income households, the sample is not representative of the UK household population. Compared to estimates from the 2001 Census, respondents in the final ECHP interview were less likely to be in employment and had lower qualifications. The survey also under-represented younger age groups, especially those aged 20–29, and over-represented older age groups, especially above 70 (Jäckle, Sala, Jenkins, and Lynn 2004). The sample nonetheless covers a broad range of socio-demographic and economic characteristics and we believe provides a strong basis for generalisation of our findings, although the analysis is unweighted. The sampling design does not allow us to construct population weights, since the 1997 sample members who did not meet any of the criteria for selection had a zero probability of inclusion. We think our approach is nonetheless valid since the analysis does not rely on population inference, but on internal (to the sample) comparisons based upon random allocation of sample cases to treatments.

CAPI interviews were sought with all Wave 8 respondents (1,163 individuals in 781 households) of whom 1,033 (88.8%) were successfully interviewed. The CAPI interviews were based on the BHPS 2002 household and individual questionnaires.[3] These collected information about accommodation, tenancy, housing problems, household consumption, demographics and neighbourhoods, health and caring, employment (histories), values and opinions and household finances. At each wave, detailed information about current labour market activity was collected. To fill the gaps between interviews, respondents were asked to report any changes that had occurred since 1st September of the previous year. Wave 8 interviews took place from September 2001 to March 2002 and ISMIE interviews from February to May 2003.

For the ISMIE interview, Wave 8 respondents were randomly assigned to one of three treatment groups: proactive dependent interviewing (PDI), reactive dependent interviewing (RDI) and the usual independent BHPS questions (INDI). For the work history,

[3] Documentation for the BHPS is accessible at http://iserwww.essex.ac.uk/ulsc/bhps. For details of differences between the main BHPS and ISMIE questionnaires, see Jäckle et al. (2004).

the *independent* group was first asked about their current activity and when this started. If the start date was before 1st September 2001 the section ended. If the start date was after 1st September, the respondent was asked about previous activities, until a start date before 1st September was reported.

The *reactive* group was asked the same questions as the independent group. To verify changes since the Wave 8 interview, two sets of follow-up questions were added. First, all respondents who reported being in employment at the time of the ISMIE interview and had also, in the Wave 8 interview, reported being in employment at that time, were asked to state whether their occupation and employer were still the same. Apparent changes between self-employment and employment were also queried. Second, respondents were asked for clarification if their current activity reported at Wave 8 did not correspond to their ISMIE retrospective report for the same time period, or if the start dates of the Wave 8 activity reported at the two interviews conflicted.

In the *proactive* group, respondents in employment were reminded of their occupation and employer at Wave 8 and asked whether these had changed. If there had been no change the industry and occupation codes from Wave 8 were brought forward. If there had been a change the new responses were recorded and coded. Respondents were similarly asked whether they were still (self-)employed. For the work history, all PDI respondents were reminded of their activity at Wave 8 and asked to confirm this. They were then asked when this activity had ended. If it was still the current activity there were no further questions. If the respondent reported an end date, they were asked about subsequent activities until the current activity was reached.

Reactive dependent interviewing (RDI) is not a realistic option for the collection of work history data: if the follow-up question reveals an error in the retrospective reports, it is then not clear whether only the last spell was misreported or whether errors pervaded the entire sequence. All existing applications of dependent interviewing use proactive methods to collect work histories. We introduced the RDI group to obtain information about the sources of discrepancies and to explore the feasibility of obtaining such information. We did not, however, use the responses to the follow-up questions to construct work histories. Instead we only used the information given in response to the initial independent questions, which were identical to those administered to the INDI group, and combined these with the INDI group in the analysis (referring to the combined group as INDI). We have also carried out the analysis separating independent interviewing and RDI groups (without the follow-ups) and the results are comparable to those for the combined sample.

3. The Construction of Continuous Work Histories

Analysts wishing to use continuous work history data from the BHPS need to combine the successive current activity information and the between-interview histories. This requires decisions on how to deal with overlapping (and sometimes inconsistent) reports from different waves and with missing dates for changes in status. Maré (2006) provides an extensive discussion of the data structure and issues, and reviews other documented reconciliations of BHPS work history data by Halpin (1998), Oskrochi and Crouchley (2000) and Paull (2002).

To analyse the effects of dependent interviewing, we combined data from the ISMIE interviews with information from Wave 8. For each respondent, we created a continuous history relating to the period from 1st July 2001 until the date of the ISMIE interview in spring 2003, covering the seam at Wave 8. The activity spells were measured in months (day information was ignored) and spells starting and ending in the same month were ignored. The reconciliation of files from the two waves followed the main principles used by other analysts of BHPS history data. We set out the principles here as they are important in understanding some of the findings presented later.

With independent interviewing the activity histories were collected in reverse chronological order. Respondents were first asked when their ISMIE current activity had started and then asked to report previous spells until the entire period back to 1st September 2001 was covered. Figure 1 illustrates the following editing rules:

1 If there were reports from the two interviews which overlap in time but disagree in content, the Wave 8 report was treated as the correct description of activity during the overlap period since it was closer to the time being described. This overlap between the ISMIE and Wave 8 reference periods always occurred because the ISMIE reference period went back to 1st September 2001, while the Wave 8 interviews took place between 1st September 2001 and March 2002.
2 Wave 8 and ISMIE spells were merged at the seam and treated as a single spell if a) both spells were the same nonemployment status or b) both spells were employment spells which corresponded in terms of status (self-employment, full-time employment or part-time employment).[4]
3 If the Wave 8 and ISMIE spells did not correspond at the seam, the start date of the spell reported in ISMIE was set to the month after the Wave 8 interview.
4 Missing dates for changes in activity status were imputed as a) midway between preceding and subsequent transition dates, or b) set to the month after the Wave 8 interview, if the midway date was before the Wave 8 interview. Setting the date midway in this case would have meant overriding the more reliable Wave 8 information, violating Principle 1 above.

With proactive dependent interviewing the activity histories were collected in chronological order. The respondent was first reminded of the current activity reported at Wave 8 and asked when this had ended, before being asked about subsequent spells up to the current ISMIE activity. The problems of combining information from the two interviews were therefore somewhat different than with independent interviewing. Figure 2 illustrates the editing principles:

5 ISMIE respondents were first asked about their current activity and then about their activity history, in which the ISMIE current activity was again reported as the last

[4] In an earlier version, we also used sector (e.g., private firm, local government, not for profit organisation) and size of employing organisation as criteria for merging, since other authors use even stricter criteria such as correspondence of 1-digit SIC and SOC codes (Halpin 1998; Paull 2002). These definitions, however, led to extremely high transition rates in our data and we therefore chose an alternative, which is less affected by coding and reporting variability. Our rule, however, potentially under-represents true job-to-job transitions at the seam.

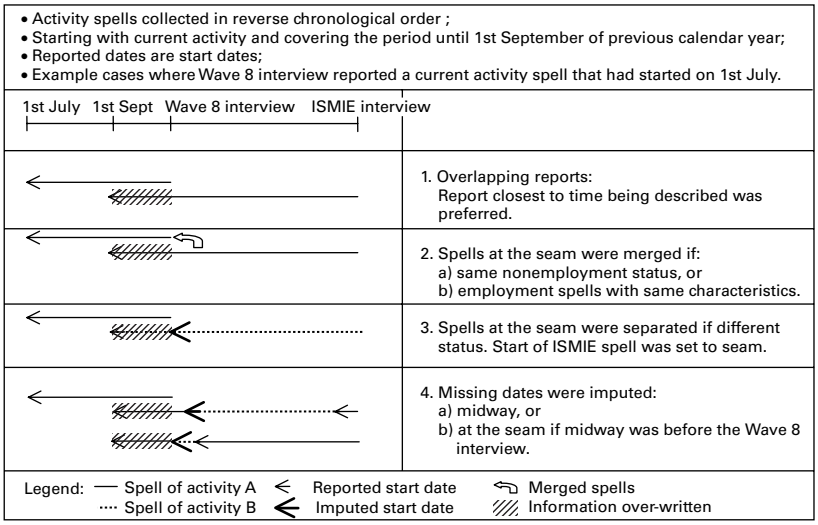

Fig. 1. Creation of continuous activity histories – independent interviewing

"not ended" spell. In case of inconsistencies in these reports, the current activity report overrode the job history for this spell. The reported end date of the previous spell in the history was treated as the start date of the current spell.

6 If the Wave 8 current activity was reported in the ISMIE interview as having "not ended," the spell was merged with the ISMIE current activity if the status reports corresponded, that is if the spells were the same nonemployment statuses, or

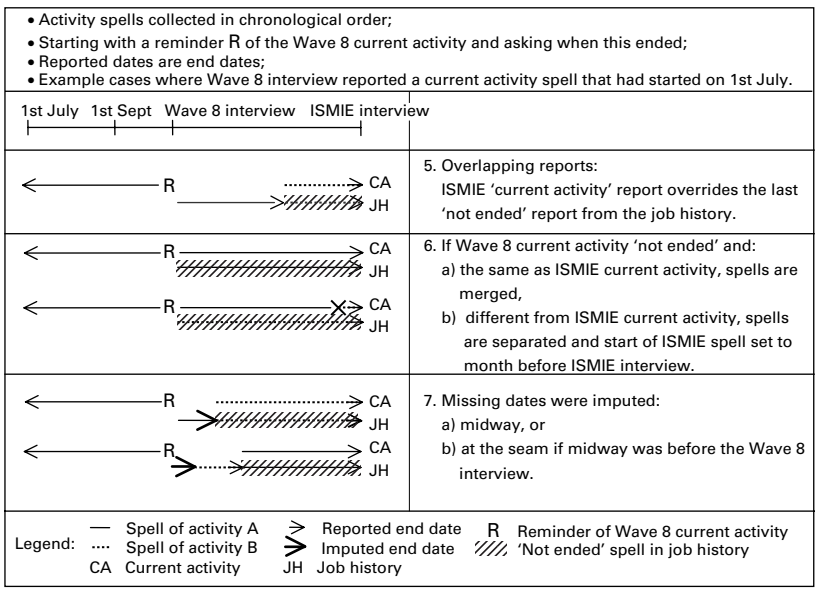

Fig. 2. Creation of continuous activity histories – proactive dependent interviewing

employment spells with the same characteristics in terms of employment status. If they did not correspond, the start of the ISMIE spell was set to the month before the ISMIE interview.

7 Missing dates were dealt with as for the independent group (see 4. above): missing dates were imputed as midway between preceding and subsequent transitions. If the imputed start date for an ISMIE spell lay before the Wave 8 date of interview, it was reset to the month after the Wave 8 interview.

These principles imply that there will be a concentration of transitions in the month after the Wave 8 interview for the independent group, if the current Wave 8 reports and the ISMIE retrospective reports do not correspond. Missing transition dates can also cause seam effects if the imputation rules lead to dates being set to the month after the Wave 8 interview. Transitions observed at the seam can, therefore, be either (1) true, (2) true but misplaced in time if the respondent misreported or did not remember the date, or (3) spurious if the Wave 8 and ISMIE reports refer to the same spell but were described or coded differently. In the proactive group it is not possible for the retrospective report to differ, since respondents were reminded of their current activity at Wave 8. Seam effects may, however, still occur if dates are missing and therefore have to be imputed.

The editing rules we have used highlight longitudinal inconsistencies; alternative principles could, however, be used, which would mask these problems and reduce the concentration of apparent transitions at the seam. Instead of cutting off the ISMIE history information at the seam (Rule 1), Oskrochi and Crouchley (2000) for example, would have used the entire job history, over-riding information from previous interviews until the start of the spell which was ongoing on 1st September 2001. The authors took this approach to reduce seam effects and make the data more suitable for duration analysis. The cost is that recall errors become more problematic, if the information collected closest to the actual event is discarded (see Paull 2002). As a result, under-reporting of spells is more likely and the inconsistencies at the seam are likely to be replaced by inconsistencies at the start date of the earliest activity in the history.

A second alternative would be to relax the rules for deciding whether spells at the seam correspond (Rule 2), for example by insisting only on correspondence of labour market status and not distinguishing between full-time and part-time employment or retirement and other forms of inactivity. This approach would increase the number of employment and inactivity spells merged at the seam (Halpin (1998) and Maré (2006) compared the effect of different matching rules) and is attractive if the reports of employment and inactivity characteristics are subject to recall error which would lead to the imputation of spurious transitions. Relaxing the rules for merging spells at the seam would, however, not have any effect on transitions between different labour market states, for example between employment and nonemployment spells.

Finally, the rules for imputing dates of transitions (Rules 3 and 4) could be chosen to avoid concentrations of transitions at the seam, for example by imputing a random date from the plausible period. This might be appropriate if transitions are true but misplaced in time and would have good properties for modelling durations (Halpin 1998). If transitions are spurious, such imputation rules would make longitudinal inconsistencies less visible, but not solve the underlying problem.

Compared to these alternative editing approaches, the seam effects we find might be more apparent, as no attempt is made to mask longitudinal inconsistencies. However, this does not necessarily imply that errors in estimates will be worse. Analysts of work histories need to gauge the relative advantages and disadvantages of alternative editing rules in light of the analysis methods they apply to the data.

4. The Effect of Proactive Dependent Interviewing on Estimates

We examined the effect of dependent interviewing on estimates of transitions between labour market activities, spell lengths and cumulative experience, using continuous work histories constructed for each respondent for the period from July 2001 until the ISMIE interview in spring 2003. The histories cover a period of 20.6 months on average and distinguish employment, unemployment and inactivity spells. (Employment includes full-time, part-time and self-employment; employment to employment transitions include promotions, changes in employer and changes between full-time, part-time and self-employment in the same occupation; inactivity includes retirement from paid work, on maternity leave, looking after family, in full-time education, long-term sick/disabled and on a government training scheme.)

4.1. Transition Counts and Rates

The mean number of transitions during the window of observation tended to be slightly larger with independent interviewing than with PDI, especially for transitions from employment (Table 1, Columns 1 and 2). The difference in means between treatment groups was not significant, however, for any type of transition ($P > 0.05$). This suggests that our conservative editing rule for identifying change in employment at the seam by changes between full-time, part-time and self-employment did not lead to an under-estimation of change as compared to PDI. In contrast, when we defined job to job transitions at the seam as additionally depending on correspondence of sector and size of organisation, there were more than twice as many job to job transitions with independent interviewing than PDI (see Jäckle and Lynn 2004, Table 1). Similarly, Hill (1994) found that the number of occupational changes during a 15-month period was nearly five times larger with independent interviewing than PDI and the rates of industry change were three times larger, when using the respective 3-digit codes to identify change. These findings illustrate that the editing rules can have a large effect on estimated transitions for job to job spells: the greater the extent to which potentially error-ridden characteristics are included in the definition of change at the seam, the more spurious changes result.

Although the mean numbers of transitions per respondent were comparable between the INDI and PDI groups, the dating of transitions was very different. The middle columns of Table 1 show monthly transition rates, that is, the percentage of respondents who were in an activity in a given month who had left this activity by the next month. Due to the small number of transitions observed in the PDI group, transitions were grouped as "from employment" or "from unemployment or inactivity." The rates are shown separately for seam and nonseam months. Nonseam months are adjacent months for which the information on activity stems from the same interview. The seam month is the month of

Table 1. Transitions between labour market activity statuses

Transition	Mean number of transitions per respondent		Monthly transition rates (%)				Percent of total transitions at seam	
			INDI		PDI			
	INDI	PDI	Seam	Nonseam	Seam	Nonseam	INDI	PDI
Emp → Emp	0.189	0.164	7.8	1.7***	–	–	18.5	–
Emp → Unemp	0.041	0.023	0.7	0.4	–	–	7.1	–
Emp → Inact	0.052	0.050	4.2	0.4***	–	–	36.1	–
Total from Emp	*0.282*	*0.238*	*12.7*	*2.5****	*5.5*	*2.3***	*20.1*	*11.1*
Unemp → Emp	0.038	0.038	18.9	3.0***	–	–	26.9	–
Unemp → Inact	0.020	0.015	21.6	0.9***	–	–	57.1	–
Inact → Emp	0.052	0.065	2.3	0.4***	–	–	22.2	–
Inact → Unemp	0.022	0.012	2.9	0.1***	–	–	66.7	–
Inact → Inact	0.048	0.059	7.5	0.1***	–	–	78.8	–
Total from U/I	*0.180*	*0.188*	*15.3*	*0.9****	*11.2*	*1.3****	*47.6*	*31.3**
Total	0.462	0.425	14.2	1.6***	8.5	1.7***	30.8	20.0**

Source: ISMIE Survey 2003 and BHPS 2001.

Notes: INDI = independent interviewing, PDI = proactive dependent interviewing.

Seam = adjacent months covered by different interviews, Nonseam = months covered by the same interview.

Transition rates for proactive group have been combined due to small numbers.

Asterisks (*) indicate significant difference compared to column on immediate left; *$0.01 < P \leq 0.05$, **$0.001 < P \leq 0.01$, ***$P \leq 0.001$.

the Wave 8 interview, as information on activity in the following month comes from the ISMIE interview. The spread of seam months over the Wave 8 fieldwork period (September 2001 to March 2002) means that any concentration of transitions at the seam does not occur in one particular calendar month. This reduces the visibility of the seam effect somewhat in analyses using calendar time as compared to panel surveys in which all interviews take place in the same calendar month, but does not get rid of it.

Transition rates in seam months far exceeded those in nonseam months, and this difference was larger with INDI than PDI. In nonseam months, the average monthly transition rate was 1.6% and 1.7% for INDI and PDI. At the seam, 14.2% of INDI respondents apparently changed their activity status in the month after the Wave 8 interview, as compared to 8.5% of the PDI group. The extent to which transitions in the INDI group were concentrated at the seam depended on the activities involved. The differences between seam and nonseam months were particularly large for unemployment to inactivity transitions (21.6% at the seam, 0.9% in nonseam months) and unemployment to employment transitions (18.9% and 3.0%), followed by job to job and inactivity to inactivity transitions. PDI considerably reduced the difference between seam and nonseam rates, mainly by reducing transition rates out of employment in seam months (5.5% compared to 12.7% in the INDI group). PDI had a smaller effect on the concentration at the seam of transitions out of unemployment and inactivity.

For each type of transition, the final columns in Table 1 summarise the percentage observed at the seam. With INDI, 30.8% of all transitions during the window of observation were observed at the seam. According to this measure, seam effects were particularly dominant for inactivity to inactivity, inactivity to unemployment and unemployment to inactivity transitions, for which between 57.1% and 78.8% of total transitions were at the seam. For job to job transitions, 18.5% were at the seam, but when sector and size of organisation were included in the definition, this concentration rose to 58.0%.

With PDI, 20.0% of all transitions occurred at the seam. Since the proactive questions were designed to reduce spurious transitions, this is still a large seam effect and suggests that PDI did not lead to under-reporting of change. A uniform distribution of transitions over months would yield approximately 5% of transitions at the seam. For this group, the main drivers were transitions from unemployment or inactivity, 31.3% of which were observed at the seam. PDI was, however, insensitive to the definition of job to job change: both the transition rate at the seam and the concentration of transitions were unchanged when sector and size of organisation were included in the definition of change (see Jäckle and Lynn 2004, Tables 2 and 3).

We are aware of few other studies with which we can compare our findings. Murray et al. (1991) examined the effects of PDI on estimates of transitions out of employment in the Canadian Labour Market Activity Survey (LMAS), a survey with annual interviews. They compared monthly transitions with comparable aggregate data from administrative records and the Labour Force Survey. Their findings suggested that PDI (coupled with the LMAS's editing rules for employment spells not confirmed by respondents) successfully removed the seam effect.

Martini (1989) and Ryscavage (1993) examined seam effects in independently collected work history data from the SIPP, a survey with a four-month recall period. We would expect seam problems to increase with the length of the recall period and therefore expect

Table 2. Mean spell length and cumulative experience (in months)

Activity	Mean spell length		Cumulative experience	
	Independent	Proactive DI	Independent	Proactive DI
Self-employed	13.8	16.6	1.0	1.2
Full-time employed	12.9	13.8	6.0	6.2
Part-time employed	12.0	12.3	2.3	2.5
Unemployed	8.8	10.1	9.3	9.8
Retired from paid work	19.0	18.5	1.0	0.9
Maternity leave	5.0	–	5.7	5.6
Looking after family	14.6	13.5	0.0	–
Full-time student	13.6	13.9	1.8	2.0
Long term sick/disabled	16.8	14.8	1.0	0.5
Government training scheme	4.0	17.0**	1.6	1.5
Other	8.9	9.5	0.0	0.0
Total	14.1	14.5	0.2	0.3

Source: ISMIE survey 2003 and BHPS 2001.
Notes: INDI = independent interviewing, PDI = proactive dependent interviewing. Number of spells: INDI: 1010, PDI: 486. '0.0' indicates nonzero number, but smaller than 0.05; – indicates zero occurrence. Asterisks (*) indicate significant difference compared to column on immediate left; **$0.001 < P \leq 0.01$.

larger seam effects in ISMIE than in SIPP data. Nonetheless, the transition rates at the seam in the ISMIE data are comparable to those reported by Martini (1989) Table 3). In fact, the SIPP rates are slightly higher. The concentration of transitions at the seam also exceeds our findings (own calculations based on Table 1 in Martini 1989). Ryscavage (1993) reported that around 55% and 44% of transitions from employment to inactivity or unemployment, and conversely transitions into employment, were concentrated at the seam, as were around 62% of job to job transitions. The equivalent proportions in the ISMIE data were 23%, 24% and 8%. (Although when sector and size of organisation were included for the identification of job to job changes, 58% of transitions occurred at the seam. See Jäckle and Lynn 2004, Table 2.) The seam effects in the ISMIE data therefore appear to be smaller than in the SIPP, and smaller than one might expect given the longer recall period.

4.2. Spell Lengths and Cumulative Experience

The previous section showed that PDI reduces seam effects in estimates of monthly transition rates. In the following, we investigate whether PDI has any effect on other estimates typically derived from work history data. Table 2 shows the average length (in months) of spells within the window of observation, from July 2001 until each respondent's ISMIE interview in spring 2003. Overall, mean duration of spells was comparable between INDI (14.1 months) and PDI (14.5 months). Mean durations of employment and unemployment spells tended to be somewhat shorter with INDI than PDI, although the difference in means was not significant for any of the activity types ($P > 0.05$), except Government training schemes. When sector and size of organisation were used as additional criteria to identify job to job transitions at the seam, employment

spells were significantly shorter with INDI: the average duration of self-employment, for example, was 8.8 months in the INDI group and 16.6 months in the PDI group (see Jäckle and Lynn 2004).

Another common use of work history data is to derive estimates of experience in certain activities, for example the total number of months spent in unemployment or self-employment over the life of a panel (e.g., Bardasi and Jenkins 2002; Taylor 1999). Estimates of time spent in employment may be less affected by spurious transitions than estimates of transition rates or spell durations, since it matters little whether the respondent has experienced one long spell or several short ones (Maré 2006). Estimates of employment experience might therefore be similar across interviewing methods, regardless of seam effects. For activities which tend to be redefined retrospectively by respondents (for example unemployment spells which are later reported as "looking after family"), there may still be differences in the distribution of time spent in each activity.

Columns 3 and 4 of Table 2 show the average number of months spent in each activity during the window of observation. The averages were similar across treatment groups for each of the activity types ($P > 0.05$). This finding is supported by Paull (2002, p. 33) who concluded that "spurious transition bias [. . .] does not significantly alter aggregate measures of the division of labour market time between states."

4.3. Differential Measurement Error

The analysis so far shows that work histories suffer from substantial seam effects and that these are reduced to some extent by PDI. The seam effects are likely to vary between demographic subgroups, if some groups have a greater tendency than others to omit or misreport spells in their work history (Lynn, Jäckle, Jenkins, and Sala 2006). If this is the case, differential seam effects may lead to misleading conclusions about differences in labour market dynamics between groups. In this section we test whether seam effects were differential across subgroups, and if *yes*, whether PDI counter-balanced differential errors, and if *no*, whether PDI introduced differential errors.

We used a probit model to estimate the probability that the Wave 8 current activity ended with a transition at the seam. Based on theories of recall and previous empirical studies, we included the following as predictors:

- *Factors affecting the saliency of the spell to be recalled*: shorter spells of any activity are more likely to be omitted from reports (forgotten) and, allowing for differences in spell length, unemployment spells are less likely to be recalled correctly than other types of spells (e.g., Paull 2002). We included indicators for the type of spell (unemployment and inactivity, where employment is the reference category) and length of the spell (1 if it had started since the previous interview and 0 if it had started before that date).
- *Factors affecting the difficulty of the recall task:* the number of intermittent events is thought to affect the difficulty of recalling any one event correctly (Eisenhower, Mathiowetz, and Morganstein 1991). Similarly respondents who had a second job at the time of the Wave 8 interview may in the following interview have reported that as their main activity at Wave 8, leading to an apparent transition. We included indicators of second job holding (1 if yes and 0 otherwise) and the number of spells

Table 3. Correlates of seam transitions

Probability of seam transition	Independent (I)		Independent (II)		Proactive DI (III)		Sample proportion
	dy/dx	*P*-value	*dy/dx*	*P*-value	*dy/dx*	*P*-value	
Age16-24	0.122*	0.015	0.165*	0.043	0.061	0.491	0.128
Age56 +	0.042	0.230	0.045	0.355	-0.052	0.328	0.377
Male	-0.016	0.352	−0.014	0.548	−0.046	0.266	0.415
A-level +	−0.018	0.361	−0.035	0.152	−0.026	0.607	0.217
Single-kids	0.144*	0.024	0.162	0.080	0.046	0.627	0.067
Couple-no kids	−0.005	0.825	0.022	0.531	−0.032	0.510	0.319
Couple-kids	0.077*	0.032	0.105	0.052	−0.059	0.321	0.226
Poor cooperation	−0.044	0.152	–	–	0.111	0.375	0.041
Unemployed	0.188*	0.023	0.112	0.257	0.078	0.658	0.049
Inactive	0.005	0.817	−0.007	0.799	0.090	0.193	0.496
Short spell	0.011	0.638	0.020	0.561	−0.105	0.055	0.220
2nd job	0.177*	0.016	0.234*	0.046	–	–	0.063
3 + spells	−0.028	0.311	−0.031	0.375	–	–	0.055
N	686	–	345	–	341	–	1,027
Predicted Pr($Y = 1$)	0.071	–	0.068	–	0.120	–	–
Pseudo *R2*	0.089	–	0.0993	–	0.102	–	–
Log-likelihood	−256.326	–	−130.170	–	−89.134	–	–

Source: ISMIE survey 2003 and BHPS 2001.

Notes: Model (II) excludes the reactive dependent interviewing sample.

dy/dx = marginal effects (change in probability of seam transition associated with change in binary indicator from 0 to 1) evaluated when all covariates set to 0.

Omitted categories are age 25-55, female, education lower than A-levels, single no kids, very good or good cooperation, employed or self-employed, seam spell had started before previous interview, long spell, no second job, only one or two spells between KDOI and LDOI.

Asterisks (*) indicate *P*-values: *$0.01 < P \leq 0.05$.

reported for the period between Wave 8 and the ISMIE interview (1 if three or more spells and 0 otherwise).

- *Measures of the respondent's cognitive ability and cooperativeness*: the quality of recall is also likely to depend on the respondent's ability and motivation. We include education (1 if A-levels[5] or higher and 0 otherwise) and the interviewer's assessment of how cooperative the respondent was during the ISMIE interview (1 if "fair" or "poor" cooperation and 0 if "good" or "very good" cooperation);
- *Respondent characteristics which may be associated both with the ability to recall and the labour market dynamics experienced*: under-reporting of unemployment spells is associated with both age and gender (Elias, 1997) and individuals with the most transient behaviour are more likely to give inconsistent accounts (Paull 2002). Both the younger and older age groups are likely to experience more and different types of transitions than the prime working age group. Similarly, family composition and the presence of children are likely to be associated with transitions into and out of inactivity. We included age (binary indicators for the age groups 16 to 24 and 56 and older, where the reference group is 25 to 55), gender (1 if male and 0 if female) and family type (binary indicators for single with children, couple with children and couple without children, where single without children is the reference group).

In addition to these factors, previous studies have consistently found that the length of the recall period is one of the most significant determinants of recall error (see Tourangeau, Rips, and Rasinski 2000). In our sample the recall period was roughly the same for all respondents and we therefore do not include a measure of time.

The last column in Table 3 presents the sample composition in terms of the explanatory variables. Since the allocation to treatments was random, the PDI and INDI samples did not differ with respect to these characteristics ($P > 0.2$ in Pearson's χ^2 tests of the independence of each characteristic across groups). The sample was predominantly of prime working age (49.6%), female (58.5%), with qualifications lower than A-levels (78.3%), single without children (38.7%), judged by the interviewer as having shown (very) good cooperation during the interview (95.9%), not active in the labour force (49.6%), experiencing a spell that had begun before the previous interview (78.0%), without a second job (93.7%), and reporting less than three spells during the ISMIE interview (94.5%).

We estimated the probability of seam transitions separately for the INDI and PDI samples. The estimates for the INDI sample show whether seam effects were differential and the estimates for the PDI sample show how this changed when dependent interviewing was used. Table 3 presents the marginal effects of the explanatory variables, that is, the percentage change in the probability of a seam transition, when the relevant indicator variable changes from 0 to 1. The marginal effects are evaluated holding all other explanatory variables at 0.

The findings suggest that seam effects were indeed differential, and that PDI removed differential errors. The probability of a seam transition was 12.2% higher for those aged

[5] A-levels are the UK exams required for entry to university, taken by students in their final two years of secondary education.

16-24 than for those aged 25-55; compared to singles without children, the probability was 14.4% and 7.7% higher for singles and couples with children; unemployment spells were 18.8% more likely to end at the seam than employment spells and those with a second job were 17.7% more likely to have a seam transition than those in employment without a second job (Model I in Table 3). The probability of reporting errors therefore seemed to depend on the nature and dynamics of labour market experiences, and those respondents with more complex histories were more prone to reporting error.

With PDI, none of the predictor variables were significant, suggesting that reporting error was not differential (Model III; the indicators for second job holding and three or more spells were dropped, because they predicted failure perfectly). The different results for the two treatment groups might, however, be due to differences in sample sizes: Model (I) included the INDI sample as well as independent responses from the RDI sample, so that the resulting sample size was twice as large as that for Model (III). To test whether the lack of significant effects in Model (III) was due to comparative lack of power, we repeated the analysis using only the original INDI sample, excluding the RDI group (Model II). In this smaller sample, only age and second job holding remained significant. The marginal effects and *P*-values of the unemployment and family composition indicators were however closer to those from Model (I) than those for Model (III), suggesting that the conclusion about differential errors in the INDI data is robust to the sample size.

5. Dependent Interviewing and Causes of Seam Transitions

5.1. The Effect of Dependent Interviewing on Reports Within a Wave

Reports within the ISMIE reference period may have differed across treatment groups, because the PDI group was asked about work history in forward chronological order and the INDI group was asked in reverse chronological order. Table 4 shows, however, that the different methods did not lead to differences in the mean number of transitions per respondent reported for the period between interviews (starting in the month after the

Table 4. Mean number of transitions per respondent between interviews

Transition	Treatment group		Total
	Independent (backward)	Proactive DI (forward)	
From (self-) employment	0.194	0.199	0.196
To (self-) employment	0.188	0.199	0.192
From unemployment	0.036	0.029	0.034
To unemployment	0.032	0.041	0.035
From inactivity	0.042	0.073	0.052
To inactivity	0.052	0.062	0.055
Total	0.272	0.302	0.282

Source: ISMIE survey 2003.
Notes: Window of observation is the period between the Wave 8 and ISMIE interviews, on average 17 months. Includes job to job and inactivity to inactivity transitions. Differences in means between the independent and proactive dependent interviewing groups were not significant at the 5%-level.

Wave 8 interview). We can therefore not say whether chronological or reverse order led to better quality of recall, in terms of less under-reporting of spell types typically more prone to being forgotten, for example, unemployment or short spells. There is no evidence that the method of interviewing makes a difference to the number of transitions, and hence spell lengths, reported within a wave. The main difference between methods lies in what happens at the seam.

5.2. Decomposing the Origin of Seam Transitions

Section 2 pointed out some of the decisions the analyst has to make in order to combine work history data from multiple waves of a panel survey such as the BHPS. The main issues evolve around how to deal with missing dates and overlapping reports. We examined the origin of transitions, separately for seam and nonseam months, according to whether the transition date was as reported by the respondent, imputed due to missing date information, or imputed due to inconsistent overlapping reports.

At the seam, only 6.1% of transitions in the INDI group were dated by the respondent; 28.6% were imputed because of missing date information and 65.3% were imputed because of overlapping noncorresponding reports, although the respondents had provided a date. For the PDI group, all transitions at the seam were imputed due to missing dates. By construction there were no overlapping spells.

Among within-wave transitions, 97.7% were at a reported date for the INDI group. In the PDI group only 27.0% of transitions were dated by the respondent, 73.0% had missing date information. Overlapping and noncorresponding reports were therefore the most important cause of (spurious) transitions at the seam for the INDI group, while missing dates had a large effect on the PDI group.

The high item nonresponse rate to the date question in the PDI group appears, in part, to be due to interviewers who were not careful about the change in question wording. In the INDI group, where work history questions were asked in reverse chronological order, transition dates were recorded as the start date of each spell ("On what date did you start doing that?"). With PDI, where histories were recorded in chronological order, respondents were asked for the end date ("On what date did you stop doing that?"). In some cases, the end date of the Wave 8 activity was before the Wave 8 date of interview, indicating perhaps that interviewers did not notice the change in wording. This led to a missing end date for the current Wave 8 activity, and by the same token to a missing start date for the subsequent spell.[6]

5.3. Overlapping Spells

We investigated reasons for inconsistencies between reports for the same time period from both waves using answers to follow-up questions administered to the RDI group. Two sets of follow-up questions were relevant. First, respondents whose current ISMIE activity began after the reference date (Sept. 2001) were asked a follow-up question if (1) their

[6] This problem occurred for 15 of the 341 respondents in the PDI group (4.4%), interviewed by 11 different interviewers.

retrospective activity report for the period of the Wave 8 interview differed from the Wave 8 report of current activity, or (2) activity reports corresponded but the start dates reported on the two occasions differed. In both cases, respondents were asked to clarify and indicate which report was correct. Second, all respondents who were in employment at both waves were asked to confirm whether their occupation and employer were still the same. We used these answers to calculate confirmed job to job transition rates and compared these with rates for the same respondents derived from the constructed work histories. This comparison exposes the extent of spurious job to job transitions in the constructed histories.

Table 5 shows the answers to the follow-up questions in the work history. A total of 80 respondents in the RDI group answered this section. In 11 cases, the retrospective report conflicted with the Wave 8 report of current activity. When asked to clarify, most respondents said the earlier report was correct. The verbatim answers indicated that some respondents had indeed forgotten about an activity and confirmed it when it was presented to them. In cases where respondents did not seem to remember, they tended to answer "if that's what I said last year it must be true." However, the answers given also indicated that conflicts in activity reports can be due to complex realities not easily broken down to the definitions of the survey. Both reports may be correct, for example if a respondent was officially retired but also working part-time, and reported one activity in the first and the other activity in the second interview. (The survey question is "Please look at this card and tell me which best describes your current situation?" where only one option may be coded.) In cases where the activity reports corresponded, but the reported start dates did not, similar proportions of respondents confirmed or rejected their earlier report.

All RDI respondents in employment at both waves were asked to confirm whether their occupation and employer were still the same. Of these respondents, 97 had started their current ISMIE employment before the reference date and were therefore not asked the work history questions. Nonetheless, when asked to confirm that their occupation (duties) and employer were still the same, 7 (7.2%) did report a change. According to the

Table 5. RDI follow-up questions in job history section

Which report is correct?	Noncorresponding reports for Wave 8	
	Activity	Start date
Wave 8 report	5	7
ISMIE report	1	5
Both reports	2	–
Don't know	1	1
Answer not codeable	2	–
N conflicting reports	11	13
Percent conflicting reports*	13.8	16.3

Source: ISMIE survey 2003 and BHPS 2001.

Notes: *80 respondents from the RDI treatment group were asked the job history section (i.e., had at least one transition since the Wave 8 interview). Follow-up question for conflicting start date only asked if activity reports correspond.

constructed histories, 5 (5.2%) of these respondents experienced a job to job transition. When sector and size of organisation were also used to identify change in employment, 49 (50.5%) of these same respondents appeared to have had a job to job transition. This illustrates that our editing rule was conservative and missed some true changes. Using additional (error-ridden) variables to identify change, however, led to considerable spurious change.

These findings are supported by Hill (1994), who reported that the majority of occupational and industrial changes observed with independent methods in the SIPP (defined as changes in the respective 3-digit codes) were not associated with changes in working hours, wages or employers. These associations were, however, much stronger with proactive dependent interviewing, leading him to conclude that "most of the observed 'change' with independent data collection methods is a result of variability in the response/coding process" (Hill 1994, p. 366).

5.4. Missing Dates

The second source of imputed transitions at the seam is item nonresponse to date questions. Nonresponse rates in the work histories were high as compared to other date questions in the ISMIE survey, especially for the PDI group. Compared to the start date of the current ISMIE activity, the nonresponse rates for history dates were comparable for the INDI group, but higher with PDI, for which around 37% of dates were missing (Table 6). The data also indicate that the incidence of missing dates increased with time since the interview.

These results imply that extra efforts to increase response to date questions in the work history section might help reduce the seam problem. More complete date information may, however, increase the incidence of overlapping noncorresponding spells. In addition, true transitions misplaced in time might be less biasing than spurious transitions created by noncorresponding descriptions of the same activity. If this is true, efforts would need to focus on the quality of reports of status and dates may not be that crucial. In either case, the analyst's decisions on how to impute missing dates and how to deal with overlaps are essential in the understanding of the source of seam effects.

Table 6. Item nonresponse rates for date questions in current activity and job history

Start/end date of	ISMIE current activity	ISMIE work history	
	Independent	Independent	Proactive
Work spell	8.4	9.9	37.6
Nonwork spell	32.9	27.3	36.1
N spells	869	1,005	486

Source: ISMIE survey 2003.

Notes: End dates asked in PDI job history; all other dates are start dates. Nonresponse rate is calculated as the number of missing dates divided by the total number of dates, multiplied by 100. Dates are considered missing if the month or month and year components are missing. Current activity "nonwork" spells exclude proactive group (no date question asked). "Work" spells include self-employment.

6. Summary and Conclusions

This article has provided new evidence of the effects of dependent interviewing on estimates derived from work history data, based on the first large-scale quantitative experiment comparing dependent and independent interviewing methods. As found in previous studies, work histories collected with independent interviewing suffered from substantial seam effects, caused mainly by longitudinal inconsistencies in reports. The answers to follow-up questions in the reactive treatment group provided some indication that inconsistencies were largely due to recall errors, in particular forgetting and misdating of spells. The findings also showed that the probability of seam transitions was differential, supporting conclusions by Rips, Conrad, and Fricker (2003, p. 545) that there is a "direct relationship between difficulty of the respondents' retrieval task and the size of the seam effect: The harder it is for respondents to recall the queried information, the larger the effect."

Proactive dependent interviewing reduced the difference between seam and nonseam transition rates but could not entirely eliminate seam effects, because of item nonresponse to spell dates. Proactive methods nonetheless eliminated differential errors, such that the probability of seam transitions was no longer related to the difficulty of the recall task. Proactive data were also insensitive to the definition of change: when additional variables were used to identify job to job changes, transition rates at the seam changed little with proactive dependent interviewing, while the rate of (spurious) transitions substantially increased with independent methods. The findings also showed that the reduction in seam effects does not necessarily lead to differences in estimates: spell durations and cumulative experience were comparable across interviewing methods. This new evidence suggests that seam effects matter if transitions are the focus of analysis, but may be ignorable if the focus is on cumulative experience.

Several implications emerge for the collection of work history data in repeated panel surveys. Proactive dependent interviewing is an effective way of reducing seam effects, especially for transitions for which (error-ridden) spell characteristics are used to identify change. For job to job transitions, for example, proactive dependent interviewing successfully reduced spurious change in reported employment characteristics (Lynn and Sala 2006), reducing spurious transitions when these variables were used to identify change. There is no evidence that the reduction in seam effects comes at the cost of under-reporting of change: transition rates at the seam still exceed transition rates for nonseam months. Further reductions in seam effects, however, require extra efforts to obtain complete reports of transition dates, even if these are approximate. The importance of the date questions could be emphasised in interviewer briefings and in the questionnaire script.

Alternative methods of reducing seam effects in repeated panel surveys, by aiding respondent recall, are also very promising. Event history calendars (see Belli, Shay, and Stafford 2001) may, for example, be very effective at reducing seam effects in labour market transitions, even without reminding respondents of previous reports.

Our study also provides some information about the influence of seam effects in independently collected data on estimates and on the correlates of seam effects, which analysts might take into account. We were not able, however, to test the effect on estimates from event history models, for which work history data are often used, due to the small

number of spells starting during the reference period. Similarly, we cannot identify the causes of differences (or similarities) as compared to proactive dependent interviewing. The fact that spell durations are comparable across methods in our study, for example, suggests that either dating errors are random or systematic but correlated for entry and exit events. Similarly, we do not know whether proactive methods reduce spurious change by reducing errors or by increasing the correlation in errors. Understanding the nature of errors leading to seam effects would not only benefit the development of data collection methods to reduce errors, but also the development and application of appropriate adjustment techniques by analysts. This would require external validation data covering at least two interviews and the intervening interval, which are rarely available in practice.

Theoretical models of the causes of seam effects are valuable in guiding research in this area. Rips, Conrad, and Fricker (2003), for example, proposed a model whereby respondents initially attempt to retrieve information from memory, and if recall fails, either guess or repeat a previous answer. The predictions from the model about seam effects fit their experimental data well. The model is limited, however, in that it focuses on "period status" questions, where respondents answer yes/no questions about whether a particular event occurred or a status applied during a particular interval of the reference period. In our study, the histories were derived from "sequential spell" questions, where respondents were asked to report dates of spells, rather than the status for each month. Furthering the understanding of seam effects will require incorporating such "sequential spell" questions into a theoretical model of seam effects.

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