Dependent Interviewing and Seam Effects in Work History Data

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Readers wishing to cite this document are asked to use the following form of words:

> **Jäckle, Annette and Lynn, Peter (December 2004) 'Dependent Interviewing and Seam Effects in Work History Data', ISER Working Paper 2004-24. Colchester: University of Essex.**

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ABSTRACT

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, and reduces bias in estimates of monthly labour force transitions and spell durations. 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 largely explained by editing rules used to reconcile reports from repeated panel observations. Proactive methods reduce seam effects by precluding overlapping non-corresponding reports. The potential for eliminating seam effects is, however, limited by item non-response to date questions.

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 impact of recall error, by reducing the length of the recall period. However, combining data from repeated panel observations to create continuous histories typically leads to 'seam effects': a 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 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 in the Income Survey Development Program.[1] Seam effects have also been documented for gross changes in labour force status and occupational and industrial mobility in the Survey of Income and Program Participation (Hill 1994; Martini 1989; Ryscavage 1993), the Canadian Labour Market Activity Survey (Murray et al. 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). Spurious transitions occur if the spells reported in adjacent waves of data collection do not correspond. Lack of correspondence can be caused by differences in the respondent's report (recall) or differences in coding or editing (Halpin 1998). Coding error is particularly problematic for classifications of occupation or industry. Open-ended questions may be coded differently if descriptions are ambiguous or if respondents describe the same activity in

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[1] 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 Survey of Income Dynamics (Hill 1987).

different ways over the years. Recall error occurs if respondents forget about events, reinterpret them, or remember the events correctly but not the dates at which changes occurred. In the presence of coding and recall errors, changes in status are typically observed at the point where two data sources meet: the date of the earlier interview. In work history data, spurious transitions lead to biases in key estimates 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/her response at the previous wave will reduce the omission of spells and misdating of events early in the reference period, thereby reducing spurious changes at the seam. A common concern, however, is that providing respondents with their previous answer may lead to under-reporting of change.

Proactive dependent interviewing is used to collect work history data in the 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 however, restricted to employment spells and estimates of transition rates. Hale and Michaud (1995) and Murray et al. (1991) also examined the effect of dependent interviewing on stock and flow estimates from work history data. They did not, however, compare estimates with independently collected data.

This paper 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. Second, we examine whether seam effects and dependent interviewing have differential impact on demographic subgroups. Third, 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. The results indicate that proactive methods reduce bias in estimates sensitive to seam effects (monthly transition rates and spell durations) and that the effects on subgroups are differential. Measures of 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 impact upon seam effects. We show that most transitions observed at the seam are imputed by the analyst – respondents do not have a tendency to report transitions as having happened at the seam. 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 non-corresponding 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 from independently and proactively collected data, repeat the analysis for demographic subgroups, and examine the effects on estimates of spell lengths and cumulative experiences. 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 non-response 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 impact 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 since 1997 jointly with the British Household Panel Survey (BHPS) activities. Funding for the ECHP expired in 2001, giving us the opportunity to interview wave eight 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. However, it covers a broad range of socio-demographic and economic characteristics and we believe provides a strong basis for generalisation of our findings. For further details of the sample composition, see Jäckle et al. (2004).

CAPI interviews were sought with all wave eight respondents (1,163 individuals in 781 households) of which 1,033 (88.8%) were successfully interviewed. The CAPI interviews were based on the BHPS 2002 household and individual questionnaires.[2] 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 in September 2001 to March 2002 and ISMIE interviews in 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, the *independent* group were 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 were 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 in employment at the time of the ISMIE interview were asked to confirm whether their occupation and employer were still the same. Apparent changes between self-employment and employment were also queried. Second, respondents were asked to clarify if

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[2] Documentation for the BHPS is accessible at http://iserwww.essex.ac.uk/ulsc/bhps. For details about changes compared to the main BHPS questionnaire, see Jäckle et al. (2004).

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 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.

The reactive treatment group was mainly introduced to obtain information about the sources of discrepancies and to explore the feasibility of obtaining such information. As mentioned in Section 1, existing applications of dependent interviewing exclusively use proactive methods to collect work history data. In this paper, we combine data from the reactive and independent treatment groups, although we have also carried out the analysis separating all three groups. The results are similar to those reported here.

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 (2002) 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 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:

1. If there were multiple reports for the same period, the earlier report was preferred since it was closer to the time being described.

2. The passage of time was measured in months (day information was ignored) and spells starting and ending in the same month were ignored.

3. Missing dates for changes in activity status were imputed as midway between preceding and subsequent transition dates.

For the reconciliation of reports around the seam for the independent and reactive groups, these principles implied that:

4. Adjacent non-employment spells of the same type were merged (e.g. two adjacent reports of unemployment were combined to one spell).

5. Adjacent employment spells were merged if they corresponded in terms of status (self-employment, full-time employment or part-time employment), sector (e.g. private firm, local government, not for profit organisation), size of employing organisation in terms of workforce *AND* the start date of the spell reported in ISMIE was before the wave 8 date of interview.[3]

6. Where adjacent spells could not be merged although the start date of the spell reported in ISMIE was prior to the wave 8 interview, the start date was set to the month after the wave 8 interview.

7. If a spell reported in ISMIE had a missing start date, could not be merged with the wave 8 current activity (4. or 5.) and 3. implied that this date was before the wave 8 month of interview, then the date was set to the month after the wave 8 interview, thereby overriding 3.

For the proactive group the following principles were used rather than 4. to 7.:

8. The most recent spell was the ISMIE current activity report – the last 'not ended' spell from the work history was ignored. The start date was the reported end date of the previous spell in the history.

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[3] Other authors (Halpin 1998; Paull 2002) used the correspondence of 1-digit SIC and SOC codes as their strictest criteria for merging employment spells at the seam. In our data this definition led to extremely high transition rates. We therefore chose an alternative which we expected to be less affected by coding and reporting variability.

9. Wave 8 and ISMIE reports were merged if the respondent reported in ISMIE that the wave 8 current activity had 'not ended' *AND* the status reports for the wave 8 and ISMIE current activities corresponded.

10. If the status reports did not correspond, the start month of the ISMIE current activity was set to one month before the ISMIE date of interview.

11. Missing dates were dealt with as for the independent/reactive groups (see 3. and 7. above): missing dates were imputed as half-way 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 at 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 have to be imputed.

4   The effect of proactive dependent interviewing on estimates

We analyse the effect of dependent interviewing on estimates of transition rates 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.

4.1   Transition rates

Table 1 shows the mean number of transitions per respondent between employment, unemployment and inactivity for the three treatment groups. (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.)

The occurrence and distribution of transitions in the independent (INDI) and reactive dependent interviewing (RDI) groups were comparable (P-value from a Pearson χ^2 test of association in the 8*2 table was 0.472). These two treatment groups were therefore combined in the following analysis and compared jointly with the proactive dependent interviewing (PDI) group.

With INDI and RDI the mean number of transitions was around one and a half times larger than with PDI. This difference is mainly driven by the large number of job to job transitions, of which there were more than twice as many in the INDI and RDI groups.

Table 1: Mean number of transitions per respondent

Transition	Treatment group			Total
	INDI	RDI	PDI	
Emp $\rightarrow$ Emp	0.376	0.362	0.164	0.902
Emp $\rightarrow$ Unemp	0.043	0.037	0.023	0.103
Emp $\rightarrow$ Inact	0.057	0.046	0.049	0.152
Unemp $\rightarrow$ Emp	0.046	0.029	0.037	0.112
Unemp $\rightarrow$ Inact	0.009	0.032	0.014	0.055
Inact $\rightarrow$ Emp	0.063	0.040	0.063	0.167
Inact $\rightarrow$ Unemp	0.026	0.017	0.011	0.055
Inact $\rightarrow$ Inact	0.049	0.046	0.057	0.152
Total	0.670	0.609	0.420	1.698
N respondents	348	341	341	1,033

Notes: The treatment groups are independent interviewing (INDI), reactive dependent interviewing (RDI) and proactive dependent interviewing (PDI). Comparison of all three groups: Pearson χ^2 (14) = 25.31, P = 0.032. Comparison of INDI and RDI: Pearson χ^2 (7) = 6.60, P = 0.472.

In comparison, Hill (1994) examined occupational and industrial change (measured by changes in the respective 3-digit codes),[4] comparing SIPP data collected independently in 1985 and proactively in 1986. During the 15 months for which the two panels overlapped, the number of occupational changes was nearly five times larger with independent interviewing, the rates of industry change were three times larger.

The differences in mean numbers of transitions in the ISMIE data are reflected in monthly transition rates. Table 2 shows the percentage of respondents who were in an activity

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[4] Our definition of job to job transitions is not based on SIC and SOC codes (see Section 2).

in a given month who had left this activity by the next month, separately for seam and non-seam months. Non-seam months are adjacent months for which the information on activity stems from the same interview. The seam month is the month of 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 compared to panel surveys in which all interviews take place in the same calendar month, but does not get rid of it.

Table 2: Monthly transition rates (%) by type of transition

Transition	INDI / RDI		PDI	
	Seam	Non-seam	Seam	Non-seam
Emp $\rightarrow$ Emp	48.5 ***	1.8		
Emp $\rightarrow$ Unemp	0.7	0.4		
Emp $\rightarrow$ Inact	4.2 ***	0.4		
Total from Emp	53.4 ***	2.6	5.5	2.3
Unemp $\rightarrow$ Emp	18.9 *	3.0		
Unemp $\rightarrow$ Inact	21.6 **	0.9		
Inact $\rightarrow$ Emp	2.3 *	0.4		
Inact $\rightarrow$ Unemp	2.9 **	0.1		
Inact $\rightarrow$ Inact	7.5 ***	0.1		
Total from U/I	15.3 ***	0.9	11.2 ***	1.3
Total	32.2 ***	1.6	8.5 ***	1.7

Notes: Transition rates for proactive group have been combined due to small numbers. Transition rates are calculated as the percentage of respondents in an activity in a given month which left this activity by the next month. Asterisks (*) indicate significant difference in transition rates at the seam compared to non-seam months. * indicates 0.01<P≤0.05, ** 0.001<P≤0.01, *** P≤0.001.

Due to the small number of transitions observed in the PDI group, transitions were grouped as 'from employment' or 'from unemployment or inactivity'. In non-seam months, the average monthly transition rate was 2% for all treatment groups, suggesting that PDI did not lead to under-reporting of change. Transition rates at the seam were substantially higher. Overall, 32% of INDI/RDI respondents apparently changed their activity status in the month after the wave 8 interview compared to 9% of the PDI group. The extent to which transitions in the INDI/RDI groups were concentrated at the seam depended on the activities involved. The differences between seam and non-seam months were particularly strong for job to job transitions (49% at the seam, 2% in non-seam months) and unemployment to inactivity transitions (22% and 1%). PDI considerably reduced the seam effect, mainly by reducing

transition rates out of employment at the seam (6% compared to 53% in the INDI/RDI group). PDI had little effect, however, on the concentration of transitions out of unemployment and inactivity.

For each type of transition, Table 3 summarises the percentage observed at the seam. With INDI/RDI, 50% of all transitions during the window of observation occurred at the seam. In addition to job to job transitions and unemployment to inactivity transitions identified as problematic in Table 2, seam effects were particularly dominant for inactivity to inactivity and inactivity to unemployment transitions. For these four cases, between 57% and 79% of total transitions occurred at the seam.

With PDI, 20% of all transitions occurred at the seam. Since the proactive questions were designed to reduce spurious transitions, this is still a large seam effect. 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% of which were observed at the seam.

Table 3: Percentage of transitions observed at the seam

Transition	Treatment group	
	INDI / RDI	PDI
Emp $\rightarrow$ Emp	58.0	
Emp $\rightarrow$ Unemp	7.1	
Emp $\rightarrow$ Inact	36.1	
Total from Emp	51.1 ***	11.0
Unemp $\rightarrow$ Emp	26.9	
Unemp $\rightarrow$ Inact	57.1	
Inact $\rightarrow$ Emp	22.2	
Inact $\rightarrow$ Unemp	66.7	
Inact $\rightarrow$ Inact	78.8	
Total from U/I	47.6 *	31.3
Total	50.1 ***	19.9

Notes: PDI transitions grouped due to small numbers. Asterisks (*) indicate significant difference compared to PDI; * $0.01 < P \leq 0.05$, ** $0.001 < P \leq 0.01$, *** $P \leq 0.001$.*

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'

editing rules for employment spells not confirmed by respondents) successfully removed the seam effect. Martini (1989) and Ryscavage (1993) examined seam effects in 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 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 tends to exceed our findings (own calculations based on Table 1 in Martini 1989). For the concentration of job to job transitions, Ryscavage (1993) reported similar results to ours, around 62%. For transitions from employment to inactivity or unemployment, and conversely transitions into employment, he found a concentration at the seam of around 55% and 44%. The equivalent proportions in the ISMIE data were lower, 23% and 24%. The seam effects in the ISMIE data therefore appear comparable to the SIPP, and lower than one might expect given the longer recall period.

4.2   Differential effects of proactive dependent interviewing

The seam effects in the INDI/RDI data 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 et al. 2004). In this section we test whether this is the case, and whether any differential bias remains when PDI is used. We have examined the concentration of transitions at the seam separately by gender, age, household composition and highest qualification. For the sake of brevity, we report the results for age and summarise the remainder.

Table 4 shows that seam effects varied significantly across age groups. The percentage of transitions observed at the seam in the INDI/RDI group increased from 32% for the youngest (aged 16 to 24), to 52% for those aged 25 to 55, to 77% for the oldest group (aged 56 plus). Differences were significant for job to job transitions and transitions between different inactivity states.

With PDI both the level of seam effects and the differences between age groups were reduced, although still significant. For the three age groups, 14%, 15% and 48% of transitions were observed at the seam. The differences for transitions from employment were no longer significant, while those for transitions from unemployment or inactivity remained significant, although the range was reduced.

Table 4: Percentage of transitions at the seam, by age and treatment group

Transition	INDI / RDI			PDI		
	16 to 24	25 to 55	56 +	16 to 24	25 to 55	56 +
Emp → Emp	37.5 ***	61.7 ***	76.9 ***			
Emp → Unemp	15.4	0.0	0.0			
Emp → Inact	44.4	31.6	37.5			
Total from Emp	34.6 **	54.8 **	65.7 **	8.3	13.0	0
Unemp → Emp	16.7	33.3	50.0			
Unemp → Inact	50.0	60.0	-			
Inact → Emp	23.5	21.1	-			
Inact → Unemp	33.3	85.7	100.0			
Inact → Inact	100.0 **	45.5 **	95.2 **			
Total from U/I	27.5 ***	42.4 ***	92.0 ***	25.0 **	18.2 **	57.9 **
Total	32.2 ***	52.1 ***	76.7 ***	13.9 ***	14.9 ***	47.8 ***
N respondents	91	342	259	41	170	130

Note: Asterisks (*) indicate significant differences within treatment groups. * indicates 0.01<P≤0.05, ** 0.001<P≤0.01, *** P≤0.001.

Classifying respondents according to their household composition (single or couple, with or without children) also showed significant differences in seam effects. The largest effects occurred for singles with children, followed by couples without children. With PDI, differences were no longer significant.

Between men and women, differences in the concentration of transitions at the seam were not significant. With PDI, the seam effect was reduced for both groups, although in the case of transitions from unemployment/inactivity more strongly for men than for women.

When distinguishing between respondents by their highest academic qualification (A-levels and above or below A-levels), the overall seam effect was again comparable for both groups. The only significant difference appeared in the percentage of inactivity to inactivity transitions at the seam. With PDI differences between the subgroups emerged, as was the case for men and women. They were, however, not significant.

4.3   Estimates of spell length and cumulative experience

The previous sections showed ample evidence 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 5 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. Spells were on average 2 months shorter in the INDI/RDI group (12.6) than in the PDI group (14.4). This is mainly due to significant differences in the

length of employment spells. The average duration of self-employment, for example, was 8.8 months in the INDI/RDI group and 16.6 months in the PDI group. These results mirror the ones reported in Section 4.1 for monthly transition rates.

Table 5: Mean spell length (months) within window of observation

| Activity | Treatment group | | Total |
	INDI / RDI	PDI	
Self-employed	8.8 ***	16.6	10.6
Full-time employed	10.6 ***	13.8	11.5
Part-time employed	10.0 *	12.1	10.6
Unemployed	8.8	10.1	9.2
Retired from paid work	19.0	18.5	18.9
Maternity leave	5.0	–	5.0
Looking after family	14.6	13.5	14.2
Full-time student	13.6	13.9	13.7
Long term sick/disabled	16.8	14.8	16.1
Government training scheme	4.0 ***	17.0	7.3
Other	8.9	9.5	9.2
Total	12.6 ***	14.4	13.1
N spells	1,137	487	1,624

Note: The window of observation is July 2001 to ISMIE interview in spring 2003. Significance test compares INDI/RDI with PDI; * indicates $0.01 < P \leq 0.05$, ** $0.001 < P \leq 0.01$, *** $P \leq 0.001$.

Another common purpose 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 2002). 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.

Table 6 shows the average number of months spent in each activity during the window of observation. The averages were similar across treatment groups, none of the differences were significant at the 5%-level. This suggests that seam effects are important in the analysis of estimates sensitive to spurious transitions, but may be ignorable if the focus is on measures of total experience in particular states. In the next section we turn to the mechanisms through which dependent interviewing produced different results.

Table 6: Cumulative experience: mean number of months in each state

Status	Treatment group		Total
	INDI / RDI	PDI	
Self-employed	1.0	1.2	1.1
Full-time employed	6.0	6.2	6.1
Part-time employed	2.3	2.5	2.3
Total (self-) employed	9.3	9.8	9.5
Unemployed	1.0	0.9	0.9
Retired from paid work	5.7	5.6	5.7
Maternity leave	0.0	-	0.0
Looking after family	1.8	2.0	1.9
Full-time student	1.0	0.5	0.8
Long term sick/disabled	1.6	1.5	1.5
Government training scheme	0.0	0.0	0.0
Other	0.2	0.3	0.2
Total	20.6	20.6	20.6

Notes: Average number of months spent in each activity within window of observation, from July 2001 until ISMIE interview in spring 2003. '0.0' indicates non-zero number, but smaller than 0.05; '-' indicates zero occurrence. None of the differences between INDI/RDI and PDI are significant at the 5%-level.

5   Dependent interviewing and causes of seam transitions

5.1   The effect of dependent interviewing on reports within a wave

Reports within the ISMIE interview may have differed across treatment groups because the PDI group were asked about their work history in forward chronological order and the INDI/RDI groups were asked in backward chronological order. Table 7 shows, however, that the different methods did not lead to differences in the mean number of transitions reported for the period between interviews (starting in the month after the wave 8 interview). We can therefore not say whether chronological or reverse order led to better quality of recall, in terms of less underreporting of spell types typically more prone to being forgotten, for example, unemployment or short spells. There is therefore 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.

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Table 7: Mean number of transitions between interviews

Transition	Treatment group		Total
	INDI / RDI (backward)	PDI (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

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 PDI and INDI/RDI group are not significant at the 5%-level.

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. Table 8 shows the origin of transitions, separately for seam and non-seam months, according to whether the transition date was as reported by the respondent, imputed due to missing date information, or imputed due to overlapping reports.

For the INDI/RDI group, three quarters of transitions at the seam were caused by overlapping non-corresponding reports, although the date information was complete. Only 3% of transitions at the seam were dated by the respondent and 21% had missing date information. For the PDI group, 97% of transitions at the seam were due to missing dates. By construction there were no overlapping spells. Among within-wave transitions, 97% were at an original date for the INDI/RDI group. In the PDI group only 24% of transitions were dated by the respondent, 73% had missing date information. Overlapping and non-corresponding reports were therefore the most important cause of (spurious) transitions at the seam for the INDI/RDI group, while missing dates had a large impact on the PDI group.

Table 8: Source of transition date

Origin of transition date	Treatment group (column %)		Total
	INDI / RDI	PDI	
Seam transitions			
Original date	2.7	3.5	2.8
Missing date	20.6 ***	96.6	29.4
Original but changed	76.7	-	67.9
N transitions	223	29	252
Within-wave transitions			
Original date	96.9 ***	23.9	71.7
Missing date	2.3 ***	72.7	26.6
Original but changed	0.9	3.4	1.8
N transitions	222	117	339

Notes: Window of observation is July 2003 until ISMIE interview in spring 2003. Asterisks (*) indicate significant difference in proportions between INDI/RDI and PDI for each row. * indicates $0.01 < P \leq 0.05$, ** $0.001 < P \leq 0.01$, *** $P \leq 0.001$.

The high item non-response rate 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/RDI 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?'). For 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.[5]

5.3 *Overlapping spells*

We investigated reasons for differences 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 retrospective activity report for the period of the wave 8 interview differed from the wave 8 report of

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

current activity, or (2) if activity reports corresponded but the start dates reported at 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 9 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, equal proportions of respondents confirmed or rejected their earlier report.

Table 9: RDI follow-up questions in job history section

Which report is correct?	Non-corresponding 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[1]	13.8	16.3

[1] 80 respondents from the RDI treatment group answered the job history section. Follow-up question for conflicting start date only asked if activity reports correspond.

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 constructed histories, 49 (50.5%) of these respondents experienced a job to job transition. This suggests that around 86% of the job to job transitions observed in the constructed histories are spurious, caused by differences in the employment characteristics used to reconcile reports at the seam.

These findings are supported by Hill (1994), who reported that the majority of occupational and industrial changes observed with independent methods in the SIPP 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).

5.4   Missing dates

The second source of imputed transitions at the seam is item non-response to date questions. Non-response rates in the work histories were high compared to other date questions in the ISMIE survey. Compared to the start date of the current ISMIE activity, the non-response rates for history dates were twice as high for work spells and 1.3 times higher for non-work spells (Table 10). Non-response rates were particularly high in the PDI group, around 36%. In the INDI/RDI groups non-response was two to three times higher for non-work than work spells. 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 non-corresponding spells. In addition, true transitions misplaced in time might be less biasing than spurious transitions created by non-corresponding 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 10: Item non-response rates for date questions in current activity and job history

Start/end date for	ISMIE current activity	Work history			
		INDI	RDI	PDI	Total
Work spell	8.4	12.2	9.5	37.4	18.3
Non-work spell	23.1	23.2	31.5	36.1	30.2
N spells	869	581	556	487	1,624

Notes: End dates asked in PDI job history; all other dates are start dates. Non-response 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 'non-work' spells exclude proactive group (no date question asked). 'Work' spells include self-employment.

6   Summary and conclusions

This paper 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, we examined a range of estimates of interest to analysts. As in previous studies, we found that proactive methods reduced seam effects, in particular for job to job transitions. We showed, however, that the reduction in seam effects does not necessarily lead to differences in estimates. Proactive methods reduced bias in estimates of monthly transition rates and spell durations. Estimates of cumulative experience, however, did not appear sensitive to seam effects and were comparable across interviewing methods. This new evidence suggests that seam effects matter if durations or transitions are the focus of analysis, but may be ignorable if the focus is on cumulative experience.

We have also shown that seam effects are differential: some demographic subgroups, in particular those of older age, were more likely than others to provide erroneous reports or missing data leading to transitions at the seam. Proactive interviewing had differential effects on different types of transitions, and thereby also on different subgroups, depending on the prevalence of transition types across groups.

In examining the impact of dependent interviewing, our study highlighted the role of editing rules in the creation of continuous histories from repeated panel observations. The main origin of seam transitions in the independent data were non-corresponding overlapping reports, for which respondents had reported complete start dates. The answers to follow-up questions in the reactive treatment group provided some indication that conflicting reports were largely due to recall errors, in particular forgetting and misdating of spells. In addition, most job to job transitions in the constructed histories were spurious, caused by differences in

the employment characteristics used by the analyst to determine whether a transition had taken place. Although proactive dependent interviewing reduced seam effects extensively, concentrations remained for transitions out of unemployment or inactivity, mainly caused by a high incidence of item non-response to spell dates.

Several implications emerge for the collection of work history data in repeated panel surveys. Proactive dependent interviewing is an effective way of reducing spurious seam effects, especially for job to job transitions. There is no evidence that the reduction in seam effects comes at the cost of under-reporting of change: proactive dependent interviewing yields similar within-wave estimates as independent interviewing. 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.

An implication for analysts of independently collected panel data is that they might want to take into account the likely relationship between measurement error (seam effects) and age, education and household composition.

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