Discussion Papers in Economics

Discussion Paper
No. 11/13

Mixed Signals: to what extent does male wage scarring vary with the characteristics of the local labour market in which unemployment was experienced?

Philip Ball

December 2011

Mixed Signals: To what extent does Male Wage Scarring vary with the characteristics of the local labour market in which unemployment was experienced?*

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Abstract

I test the hypothesis that unemployment experienced in high unemployment regions is less likely to be viewed by employers as a negative productivity signal, and more as a characteristic of the region. This predicts that unemployment's short-run negative wage effects will be mitigated if experienced in high unemployment regions. If so, then what long-term implications does this have for future wage growth (Wage Scarring)? How important is regional heterogeneity in driving wage outcomes? Continuous work-life histories are matched to the regional context in which individuals reside. This novel data set permits control for the timing of career disruptions, as well as regional location at the time of displacement, whilst searching and at re-employment. Persistent wage penalties are found, conditional on previous labour market status. Seminal UK research concludes that the first spell of non-employment carries the highest penalty. Considering unemployment and inactivity, no reduction in the penalty associated with incidence of inactivity is found. Strong regional differences are found in the impact of redundancy on wage growth. This is contingent on labour market tightness and urbanity of the region in which unemployment was experienced. Redundancy followed by unemployment in areas of high economic activity is equally damaging for future earnings potential, independent of age. Moreover, robust evidence is found supporting the main hypothesis in the UK, on average and for over 45s made redundant in their previous jobs.

Keywords: job displacement, wage scarring, regional heterogeneity, work-life histories.
JEL: J24, J31, J61, J63.

*This work is supported by the Economic and Social Research Council through the PhD quota award. Ralf Wilke, Peter Wright, Sarah Bridges, Wiji Arulampalam, Mark Taylor, Richard Upward, Bernt Fizenberger, as well as seminar participants at the Society for Longitudinal and Life Course Studies 2010, IAB PhD workshop 2010,the Office for National Statistics Labour Market Statistics Conference 2011, and the European Association for Labour Economists Conference 2011 are thanked for their comments.

†University of Nottingham, E–mail: lexpbb1@nottingham.ac.uk

1 Introduction

Economists have had a long standing interest in the impact of unemployment on individuals' labour market outcomes. Whilst extensive work has been conducted on the persistence of unemployment, less exists on the long-term implications for future earnings trajectories. Wage Scarring refers to the long-term impact of individual unemployment experience(s), hypothesised to increase the likelihood of future unemployment and decreasing future earnings potential. Economic theory provides ambiguous predictions with respect to this question. Job loss, and subsequent unemployment, may be linked to wage scarring through various mechanisms. There may be stigma effects of unemployment (since productivity is imperfectly observed, unemployment may be viewed as a negative productivity signal by prospective employers) which feed into lower wage offers. Firm-specific human capital is lost when a job is terminated, implying that, if returns to specific human capital are shared between the firm and worker(s) and human capital accrues with tenure on the job, longer tenure workers are at risk of losing the most due to job loss if this human capital is not transferable across employers. Independent of whether returns to specific capital are shared, firms have less incentive to layoff high tenure workers than their low tenure counterparts. Whilst there are many potential mechanisms at work, with some operating in different directions, human capital theory provides a tractable framework in which to operate as well as generating testable predictions (Becker, 1962).

Individuals may quit their job, or be fired due to low productivity. To avoid this selection issue the literature has tended to focus on the impact of employer initiated job displacement, that can be reasonably assumed to be unrelated to a worker's characteristics. This approach is taken in order to approximate a natural experiment. A direct test of human capital theory is that displaced workers earn less on the post- than pre-displacement jobs (Farber, 1999), the first generation of papers investigated short-term implications conducting before and after comparative studies on North American data. Addison and Portugal (1989) and Houle and van Audenrode (1995) are examples of before and after studies employing Displaced Worker Surveys for the US and Canada respectively. Looking at longer-term viewpoint, Jacobson et al. (1993) employed Pennsylvanian administrative data, whilst Ruhm (1991) drew on the Panel Study of Income Dynamics (PSID). Results for the more flexible labour markets of the UK and US have found substantial and persistent earnings losses which remain to the order of 10% to 18% even 10 years after re-employment (Ruhm, 1991; Jacobson et al., 1993; Gregory and Jukes, 2001; Arulampalam, 2001), whereas the evidence in Europe is less marked (Kunze, 2002). More recent contributions to the debate have replicated the approach of Jacobson et al. (1993), implementing newly developed econometric techniques (propensity-score matching) to extend the analysis, using administrative data for the United Kingdom (Hijzen et al., 2010), Sweden (Eliason and Storrie, 2006) and the US state of Connecticut (Couch and Placzek, 2010). Eliason and Storrie (2006) highlight the increased sensitivity of displaced workers' earnings losses to recessionary pressures. Furthermore, Couch and Placzek (2010) cast doubt over the generalisability of JLS's results for the US as a whole, given changes in State and time period. The existing literature suggests that variation in

institutional context may help to explain cross-country differences in the impact of unemployment on wage growth. However, Gangl (2006) finds that enough institutional heterogeneity exists to generate marked differences in wage scarring across EU members.

Although institutions may vary across countries, there is generally not enough variation in institutional context within a country to generate the observed differences in wage outcomes across regions (Carrington, 1993)[1]. van Dijk and Folmer (1999) hypothesize, and provide cross-sectional evidence, that longer unemployment periods carry a significant negative productivity signal in regions with low unemployment rates whereas in periphery regions where unemployment rates are high, this is attributed to the characteristics of the regional labour market This raises the question of whether this hypothesis holds when applied to Great Britain in a dynamic longitudinal context. Given the interconnectedness of Britain's regions, it is puzzling why persistent differences in average regional earnings remain. This analysis may shed some light on the cause of these regional differences. Do the substantial and persistent earnings losses found by UK studies like Arulampalam (2001) remain when adding the extra waves now available in the BHPS, and how important is regional heterogeneity?

In order to address these questions, the British Household Panel Survey (BHPS) is used to construct continuous work-life histories, following individuals from when they first left full-time education. In constructing the dataset, a rules-based approach is adopted to minimise measurement error and ensure consistency of the data. Individuals' labour market histories are prone to overlap due to the timing of interviews in the BHPS varying over the period of the survey (Halpin, 1997; Upward, 1999; Paull, 2002; Maré, 2006). Retrospective lifetime histories are constructed, spanning the period since first leaving full-time education. This information allows for a direct measure of general labour market experience, rather than using a potential experience proxy, thus reducing measurement error. The analysis uses interview dates as reference points due to better data coverage at these intervals, as well as being integral to the rules-based approach adopted. This dataset is linked to the Labour Force Survey at the Travel-to-Work Area & Local Authority levels of aggregation in order to incorporate time-varying unobserved heterogeneity not adequately captured in the model, as well as to address the research question. For more information on the dataset construction please consult Ball (2010).

The paper is organised as follows. Selected contributions to the existing literature are summarised in the Appendix, Table 13. Section 2 describes the data. Section 3 describes the methodology in the context of the exisiting literature. Section 4 examines descriptive statistics relating to the consequences of job displacement for future wage growth. Section 5 presents results from an initial replication of Arulampalam (2001). Section 6 extends the basic results in terms of observation period and regional-level effects. Sensitivity checks are briefly detailed in section 7, whilst Section 8 concludes.

[1]Federal countries are a notable exception, however the extent to which this impacts on observed wage outcomes across regions is an empirical question.

2 The Data

The British Household Panel Survey (BHPS) Detailed individual-level information is sourced from the BHPS. The version of the BHPS used in this study covers 11 waves of the survey, from 1991-2001. Unfortunately data coverage over the full survey period, 1991-2008, is incomplete for a key indicator at the sub-regional level. 'Labour market tightness' is commonly proxied by the vacancy/unemployment ratio, key to the Matching literature. The vacancies series is available from NOMIS (www.nomisweb.co.uk) at the sub-regional level. Vacancy statistics are likely to suffer severe downward bias due to the fact that vacancy posting is not obligatory for firms (Folmer and van Dijk, 1988). The existence of internal labour markets, implies that vacancy statistics will tend to underestimate the true level of labour demand as firms may recruit internally as a first option (Atkinson and Micklewright, 1991). In addition to being plagued by data quality issues, there is a one year gap the series due to significant changes to Jobcentre Plus procedures for handling vacancies in 2001. Moreover, the effect of this change was that vacancy statistics are not comparable over time (Bentley, 2005). The extended time-frame under investigation was reduced from 1991-2008 (for which data was available at the time of writing) to 1991-2001 in order to account for this issue. Socio-Economic data available in the BHPS at the individual & household level. The survey provides an annual nationally representative sample of 5000+ household and over 10000 individual-level observations per wave. Retrospective job history information is collected for the 12 months prior to the current wave interview. In addition, the survey contains information on complete work-life histories since leaving further education. Appendix 8.4 illustrates the structure of the BHPS, whilst full data preparation steps are detailed in Ball (2010).

Unlike the US Displaced Workers Survey (DWS), the BHPS contains regional location information relating to the time of displacement, the time an individual was searching for a job, *and* the time of re-employment. This allows control for the timing of moves across regional entities, and thus the identification of regional effects. This regional information is available on a spell-by-spell basis for spells lasting less than a year. For spells which have lasted for more than one year, regional location is coded at survey date. The date a move took place is also available, as well as whether an individual moved for employment reasons. Location information is only collected at the beginning of each labour market spell in the pre-1.9.90 data. 1.3% (153) of the sample move travel-to-work area between labour market spells, whereas 4.3% (527) move travel-to-work area over the sample period as a whole. 1.6% (190) of the sample move local authority between labour market spells, whereas this figure is 5.92% (718) over the sample as a whole. These figures are based on the Original Sample Members (OSM), using the current sample selection strategy, for the full 11 waves. These figures do not change markedly once the window is increased to 2 years around job take-up, to capture tied moves. They suggest that selection into a move *across* regional boundaries (LAD, TTWA) between labour market spells is less of a concern. Furthermore, household moves which are confined within the geographical entity of

interest -local authority, travel-to-work area - are ignored.

Local Area Quarterly Labour Force Survey (LAQLFS) The LAQLFS is available for the period 1992q2-2006q1. Quarter 3 waves of the Local Area Quarterly Labour Force Survey are used to link the regional-level data to the BHPS. 20 out of the 323 local authority areas could not be matched, due to changes in the way regions are classified in 1996. In 1996, 46 Unitary Authorities were introduced in the UK. Initial attempts to acquire a concordance table from ONS Geography failed. The strategy adopted was to match regions by name. This may not be the most accurate procedure, as there are cases where pre-1996 regions were split into smaller administrative entities. However, given the tools at my disposal this seemed the best approach[2]. Leaving these regions out of the individual-level data (dropping anyone who ever lived in them) does not seem to have a significant impact on the composition of the sample, suggesting that results are likely to be robust to this restriction. Furthermore, pairwise t-tests of the null hypothesis that dropping problematic does not have an impact on the means cannot be rejected. Ball (2009) can be consulted for a detailed discussion of the dataset construction.

In order to investigate the importance of the regional dimension, controls are introduced for the ILO Unemployment, the Vacancies-to-Unemployment Ratio, Accessibility, and whether the respondent is living in an Urban area at the time of interview. Labour market tightness is defined at the TTWA level of aggregation. Accessibility and Urban indicators in the BHPS are acquired from the National Statistics Postcode Directory (NSPD) and measured at the Output Area level of aggregation. Local authorities may contain a mixture of urban and rural Output Areas, thus using a classification at this level of aggregation is likely to be inappropriate implying that these measures are unlikely to capture the local labour market environment very well. Adopting the approach implemented in Ball (2009), Urban combines the Department for Environment, Food & Rural Affairs (DEFRA) local authority-based urban/rural classification, valid for England only, with the output area-based NSPD classification for Scotland & Wales (implying some measurement error). Unemployment incidence and the length of previous interruption are controlled for in all regional level regressions. Detailed continuous sub-regional data is unavailable from most standard sources over the period of interest. Furthermore, the Special Access LFS is only available from 2003, which would not allow me to construct full work-life histories given that this information is only collected in the second and third waves. Thus the Local Area Quarterly Labour Force Survey (LAQLFS) is drawn upon in order to construct these measures. The LAQLFS is available for the period 1992q2 - 2006q1. Given the rotating nature of the LFS, 1991q3 values are assumed to be the same as those in 1992q3. The BHPS and LAQLFS are linked at the local authority level of aggregation using the concordance scheme developed in (Ball,

[2]The 20 non-matches include: *Redcar & Cleveland; East Riding of Yorkshire; North East Lincolnshire; North Somerset; South Gloucestershire; Swindon; Medway Towns; West Berkshire; Conway; Debighshire; Flintshire; Bridgend; Caerphilly; Aberdeenshire; West Dunbartonshire; East Ayrshire; East Dunbartonshire; North Ayrshire; North Lanarkshire; South Lanarkshire*

2009).

Focus is limited to a sample of males aged between 16 and 58 and directly interviewed at Wave 1, excluding proxy interviews[3]. These individuals are followed from when they first leave full-time education until 65. In order to be able to derive full employment biographies for the Original Sample Members (OSM) used in the study, multiple data sources needed to be drawn on. This raised awareness of the inherent complexities in the survey design. Clearly defined, well justified data preparation steps are required in order to ensure further biases are not imparted on the final data. This sensitivity of the BHPS work-life histories to data preparation steps is well highlighted in (Paull, 2002). For this reason an extensive technical appendix to this paper was created, (Ball, 2010). This details the rules-based approach adopted to minimise the major sampling issues. Furthermore, the study aims to test the van Dijk and Folmer (1999) hypotheses by matching the individual-level data to the regional context in which these individuals reside.

3 Methodology

Ideally our treatment, job displacement, would be randomly assigned (Angrist and Pischke, 2009). Lack of experimental data means that most studies investigating job displacement have employed administrative or survey data. Since from a policy point of view this study's interest is in heterogeneity across separation types, careful attention to potential sources of endogeneity is called for. Unobserved heterogeneity, Selection Bias, Omitted Variable Bias, Measurement Error, systematic Recall Bias and Attrition Bias are of particular concern. Simultaneity bias is less of a concern given that timing is directly controlled for. "In the absence of experimental evidence, it is very difficult to know whether the higher earnings observed for better-educated workers are *caused* by their higher education, or whether individuals with greater earnings capacity have chosen to acquire more schooling (Card, 1999, pp. 1802)." By exploiting the longitudinal nature of the BHPS the time-invariant component of ability can be "differenced" out using the fixed effects estimator, leading to consistent estimates under OLS when controlling for a rich set of observed characteristics. The fixed effects estimator is inconsistent under the presence of omitted variable bias (OVB) and measurement error, which is likely to be present in survey data due to systematic Recall Bias. In extensions of this study, the regional variation in the BHPS is exploited as an extra dimension for identification.

[3]The initial interest was in replicating Arulampalam (2001)'s results and then extending the observation window with the extra survey waves now available. Due to lack of information about the exact data preparation steps, an exact replication was not possible. Replication is made harder due to regular updating of the panel due to coding errors, etc. I am grateful to Professor Arulampalam for providing her SPSS code detailing her data prepartion steps. Unfortunately this code referred to a preliminary version of the paper and the preparation steps used to construct certain key variables were missing. Granted, I developed alternative proxies due to these inherent ambiguities. Alternative proxies for key indicators were developed in order to address these ambiguities. This exercise raised valid questions about the robustness of findings to these data preparation steps.

The following Mincerian Earnings function is estimated:

$$ln(w_{it}) = x'_{it}\beta + (d'_{it}Z_{it})'\gamma + \lambda_t + \alpha_i + \varepsilon_{it} \qquad \forall\ i = 1 \dots n,\ \forall\ t = 1 \dots T \tag{1}$$

Where:

$w_{it} =$ Hourly wage of individual i at time t, deflated by CPI in 1991 prices.

$x_{it} =$ Matrix of observed personal and workplace characteristics.

$d_i =$ Dummy variable, taking the value 1 if individual i entered the current employment spell via a spell of interruption.

$Z_{it} =$ Matrix of selected individual characteristics (interacted with d_{it}).

$\lambda_t =$ Time dummy.

$\alpha_i =$ Time-invariant individual-specific error component.

$\varepsilon_i =$ Idiosyncratic error component.

The Within-Groups estimator, $ln(w_{it} - \bar{w_i}) = (x_{it} - \bar{x_i})'\beta + (d_{it} - \bar{d_i})'\gamma + (\lambda_t - \bar{\lambda}) + (\varepsilon_{it} - \bar{\varepsilon_i}) \qquad \forall\ i = 1 \dots n,\ \forall\ t = 1 \dots T$, is still consistent in a model with the inclusion of endogenous regressors, provided that the source of endogeneity is time-invariant, e.g. Due to ability bias (Cameron and Trivedi, 2005). The parameters in specifaction 1 are estimated as deviations from their individual-specific means, with appropriate adjustments made to the standard errors. Unobserved heterogeneity, υ_i, is modelled as $\upsilon_i = \alpha_i + \varepsilon_{it}$. When parameters are estimated as deviations from their means, the individual-specific error component drops out, given its time invariant nature, leaving us with only the idiosyncratic error component to deal with. By construction ε_{it} is uncorrelated with the explanatory variables. A Random Effects estimation strategy is not implemented due lack of an appropriate instrument for ability, implying that the assumption $E(\varepsilon_{it}|x_{it}) = 0$ is inappropriate in this case. Furthermore, Fixed Effects relies on the identifying assumption that $E(\varepsilon_{it}|x_{it}, \alpha_i) = 0$, i.e. Conditional exogeneity. Fixed effects estimates are susceptible to attenuation bias due to measurement error. If a variable is persistent, incidence this year makes incidence next year more likely, and changes from year-to-year are misreported/miscoded, although there may be measurement error in a sub-sample of the population in each year observed year-to-year changes in the variable will be mostly noise (Angrist and Pischke, 2009). This implies more measurement error in differenced estimates than in their levels, explaining one reason why fixed effects estimates are generally smaller than their OLS counterparts(Angrist and Krueger, 1999).

The sample appearing in the wage equation is unlikely to be a random sample of the underlying population. For individuals that do not appear in the wage equation, the wage distribution will be truncated at zero. However, this truncation is non-ignorable since it is the product of a underlying deterministic process influencing the labour market participation decision. List-wise deletion of cases in which real wages are not observed would lead OLS to produce biased estimates of the true extent of Wage Scarring, due to sample-selection bias, as these cases cannot be assumed missing at random. This incidental truncation is corrected for using the Heckman selection model (Heckman, 1979).

Selection Rule: For an individual to appear in the wage equation they must be continuously present in the survey for at least two wave since the beginning (1991); a positive real wage must be observed (defined only for those in employment and continuously present) and they must be in employment at least twice. Since the Within-Groups estimator applied to an earnings regression requires an individual to be in employment at least twice, individuals appearing in the wage equation are not representative of all workers in employment. This fact is explicitly taken into account when formulating this selection rule (Arulampalam, 2001). Sensitivity of the results to the selection rule is formally tested in the robustness checks for attrition bias.

Identification Strategy: Fixed Effects relies on the identifying assumption that $E(\varepsilon_{it}|x_{it}, \alpha_i) = 0$, i.e. the Conditional exogeneity/ Conditional Independence Assumption (CIA). Strategies are adopted in an attempt to satisfy this criterion. The advantage of using survey data is that this allows a rich set of regressors commonly thought to impact on wage outcomes to be controlled for, including regional-level interactions with state dependence. Regional-level identifiers in the BHPS allow time-varying regional characteristics to be incorporated. Heteroscedasticity is controlled for using White's heteroscedasticity robust standard errors. Furthermore, a rules-based approach to data preparation is taken in order to minimise measurement error and thus omitted variable bias (see Ball, 2010). How to control for the fact that most factors that influence unemployment also influence accepted wages is the fundamental identification challenge faced when attempting to control for sample selection (incidental truncation) using the Heckman two-step approach (Heckman, 1979). Exclusion restrictions in the first stage participation strengthen identification, however a fundamental challenge is that of finding valid instruments. Exclusion restrictions include whether an individual has children, which is expected to impact on re-employment probability through mobility costs, but not on wage offers (Stevens, 1997). This instrument may not have the appropriate properties of a good instrument, as the relationship between fertility and labour supply suggests the presence of endogeneity between the presence of children and labour market outcomes (Angrist and Krueger, 1999). Father's occupation when 14, whether they were self-employed at 14, and current housing tenure are also included as identifying variables in the first stage participation decision. As in Arulampalam (2001), the Travel to Work Area (TTWA) unemployment rate in 1991 is included as in as a further exclusion restriction, the identifying assumption being that historic local labour market unemployment rates impact directly on the current unemployment rate and only indirectly on the current wage, through the current unemployment rate. Ideally 1981 Census would have been used to gauge the TTWA unemployment rate. This proved impossible due to severe lack of concordance between the TTWA classification methods used[4]. A χ^2 test for the joint significance of the identifying variables is significant at the 99.9 percent level. Redundancies are more likely to be orthogonal to individual characteristics, relative to other

[4]No guidance is provided in Arulampalam (2001) on how she defined certain key variables, including the 1991 Travel to Work Area (TTWA) unemployment rate. In the subsequent analysis this is defined as the total of male and female unemployment as a proportion of the resident economically active population.

separation types. However, even within the redundancy category, there is likely to be heterogeneity. Unfortunately this cannot be controlled for, since the BHPS does not distinguish between mass layoffs and plant closures.

4 Descriptive Analysis

In the analysis that proceeds, previous labour market history is considered in relation to current labour market status, given that the individual reporting is currently employed. Preliminary analysis - not reported - highlighted close similarities between the sample of individuals directly interviewed in 1991 and the sub-sample used in the wage analysis (continuously present for at least 2 waves). These similarities persisted when the sample was further conditioned to exclude those reporting themselves as self-employment status at interview date, as well as when problematic regions are dropped. Moreover, the composition of the non-extended and extended samples are very similar (Table 9, Appendix Section 8.1). Thus the descriptive analysis mainly focusses on the extended 1991-2001 period.

Table 1: Current Employer Tenure, by previous labour market status, 1991-2001(§).

	Previous Status			
Tenure	**EMP**	**UNEMP**	**OLF**	**Total**
<1 year	790	375	98	1263
1-2 years	649	263	102	1014
2-3 years	556	191	87	834
3-4 years	488	152	72	712
4-5 years	407	126	64	597
5-10 years	1596	437	260	2293
>10 years	2623	532	1058	4213

§ - Excluding: Redcar & Cleveland; East Riding of Yorkshire; North East Lincolnshire; North Somerset; South Gloucestershire; Swindon; Medway Towns; West Berkshire; Conway; Debighshire; Flintshire; Bridgend; Caerphilly; Aberdeenshire; West Dunbartonshire; East Ayrshire; East Dunbartonshire; North Ayrshire; North Lanarkshire; South Lanarkshire.
Previous labour market states considered (since leaving full-time education): Employment/Self-Employment; Unemployment; OLF (Out of the Labour Force).

Table 1 excludes problematic regions. Previous "Out of the Labour Force"(OLF) includes previous full-time education (see Arulampalam, 2001, Table 2: pp. F594), with the sample restricted to individuals who have left full-time education for the first time. Differences between Table 2, Arulampalam (2001), and Table 1 can be explained not just by the extended period but also by her definition of previous status only capturing the last five years of work-life history. Using all information since respondents left full time education captures a significant proportion of continuous 'first job' employment spells which experienced no interruption over the observation window[5]. Existing studies in the literature have generally restricted their attention to high tenure individuals, thus excluding most of the early career workforce.

Previous labour market status of 'first job' spells is recorded as "out of the labour force", given the OECD definition. Arulampalam (2001) used the BHPS-supplied "current spell length" indicator, recorded at interview date, to construct her tenure variable. This indicator is likely to suffer from

[5]No interruption pre-1.9.90 that lasted longer than 1 month, given that the pre-1.9.90 data does not capture very short spells by design. I include a control for whether individuals are in their first job as a control in the regression analysis.

recall bias, leading to inconsistencies with the spell length measure used in this analysis. Summary

Table 2: Comparison of Means of Sample used in Regional Wage Analysis, 1991-2001.

PREV_STAT:	**EMP.** [1]	**UNEMP.** [2]	**OLF** [3]
Region			
SE	0.20	0.17	0.20
SW	0.08	0.11	0.11
East Anglia	0.05	0.05	0.03
E.Midlands	0.09	0.07	0.09
W.Midlands	0.12	0.11	0.09
N.West	0.13	0.13	0.11
Yorksire & Humber	0.06	0.13	0.10
North	0.07	0.07	0.10
Wales	0.03	0.03	0.01
Scotland	0.06	0.06	0.04
Total	7109	2076	1741

Statistics refer to sample used in the Wage analysis, which excludes the problematic regions defined in Table 1

statistics for the 1991-1997 and 1991-2001 period are presented in Table 9, Appendix Section 8.1. According with intuition, tables 9 suggests that individuals younger than 30 are relatively more likely to have come into their current employment spell via a spell of non-employment than those over 30. They are more likely to be single, have an employed spouse, be less qualified, and a private tenant. In terms of workplace characteristics, these individuals are less likely union members, more likely to be in part-time and temporary employment, and more likely to be in unskilled manual/non-manual jobs. On the contrary, over 30s are more likely to have made an employer-to-employer transition.

General experience levels are significantly lower for those who came into their current employment via a spell of unemployment, 223 months versus 265 months (1991-1997). This was lower at 198 months for previous OLF and carries over to the extended sample. However, previous status is not an accurate predictor of current employer tenure when labour market history since leaving full-time education is considered and non-employment is considered as a grouped category (107 months for employer-to-employer transitions, and 116 months for those from non-employment, 1991-1997). Those entering current employment from unemployment had 72 months of employment tenure on average, whereas those entering from OLF spells had accumulated 173 months of current employer tenure over 1991-1997. This pattern carries itself over to the 1991-2001 period on which the descriptive analysis focusses. Granted, those with previous interruption are consistently worse off in terms of earnings, regardless of assumed rate of overtime pay (not reported), suggesting a lack of catchup of wages to counterfactual levels. These tables highlight considerable differences within the category of Non-Employment, motivating this studies approach separating this labour market state into Unemployment and Out of the Labour Force (OLF).

Table 2 shows substantial regional heterogeneity in the incidence of job interruption as well as job-to-job transitions, motivating this studies interest in regional variation in wage scarring. State dependence aside, if one is interested in how wage profiles of individual change over their career, then taking into account the nature of those separations is key given differences in their productivity signalling effect for future employers. Table 3 suggests that men who entered their current job via an

employer-to-employer transition, without interruption, are 67% more likely to have quit their previous job voluntarily. A significant proportion of individuals who were made redundant in their previous jobs experienced no interruption (50.64%). This figure drops to 49.97% when the information used to construct previous status is restricted to the last five years. Arulampalam (2001) cites a larger figure, with 81% of redundancies experiencing no interruption in her sample. Given that I could not establish some of her data preparation steps, it is hard to reconcile these differences. No mention is made in Arulampalam (2001) of the difficulties of constructing continuous work-life histories, and how she dealt with overlapping data sources. It could be that differences in the approach to this issue explain some of the differences. If the methodology adopted in Arulampalam (2001) does not adopt strategies to minimise systematic recall bias, then one could expect a general underreporting of non-employment periods especially for frequent job changers and/or if these periods were short in duration (Paull, 2002). However, given that the main BHPS uses a 12 month recall period, this recall problem would not be as much of an issue as in the DWS for example. Using BHPS data Paull (2002) does show that different methods for dealing with the recall issue does lead to economically significant differences in results.

Table 3: Reason for leaving previous job by previous status, 1991 - 2001.

	EMP	**UNEMP**	**OLF**
	% (Obs.)	% (Obs.)	% (Obs.)
Redundant	0.12 (888)	0.41 (846)	0.02 (43)
Sacked/ Dismissed	0.01 (83)	0.05 (105)	0.00 (5)
Temporary Job Ended	0.03 (205)	0.11 (230)	0.02 (31)
Voluntary Quit	0.68 (4,827)	0.00 (0)	0.00 (0)
Missing	0.06 (419)	0.04 (93)	0.04 (68)
Other Reason	0.10 (688)	0.27 (567)	0.15 (262)
N/A	0.00 (0)	0.11 (235)	0.77 (1332)
Total	7109	2076	1741

Sample selection: Individuals never in self-employment at interview date. Statistics refer to sample used in the Wage analysis, which excludes the problematic regions defined in Table 1

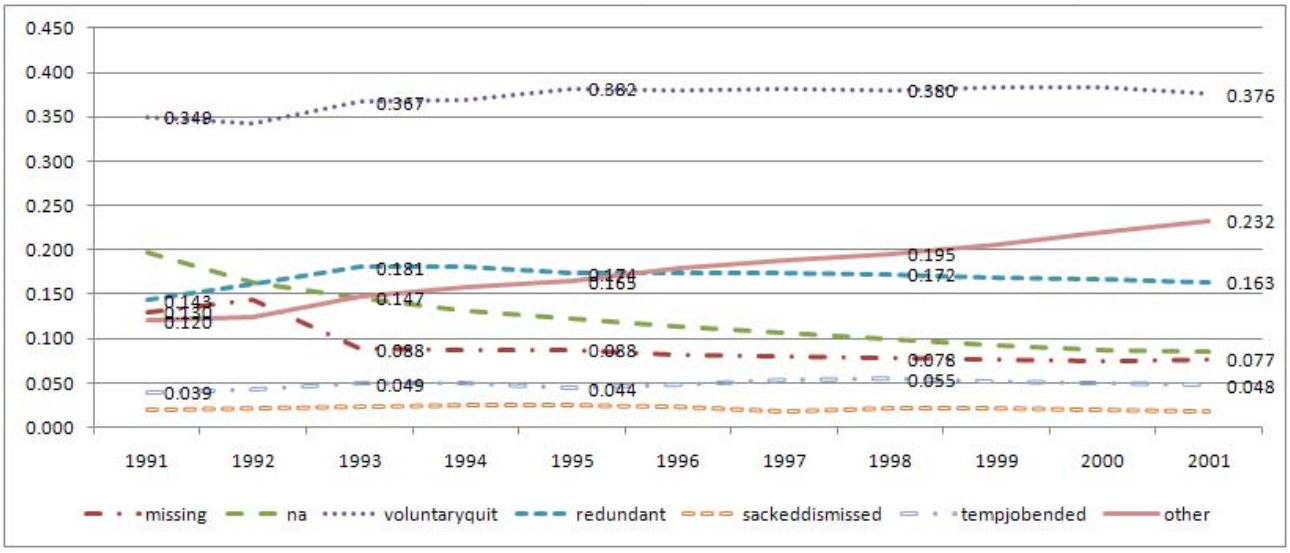
Figure 1 illustrates how displacement rates varied over the survey years by displacement type. Figure 1's rates are as a percentage of the population 'at risk' of displacement Following Farber (1999), those 'at risk' are proxied by the number of employed workers at survey date. These rates are likely to understate between survey-date dynamics in the sample.

Included in the analysis are workers that lost their jobs for "other" reasons. This category includes separations for health reasons, maternity leave, and family care, etc. Individuals that lost their jobs for unidentified/'missing'[6] reasons are also included as a separate category. The "not applicable" category captures people who have never been displaced since leaving full-time education[7]. Figure 1 highlights a trend increase in the proportion of people ending their jobs for "other" reasons. These

[6]Due to the inclusion of individuals present at Wave 2 and never after, 'reason for leaving previous job' is systematically missing for a significant proportion of the sample that never contributed to the wave 3 job history file, i.e. exited the sample at wave 2, as reason for leaving previous job is not asked in the wave 2 labour market history. Thus this heterogeneous category cannot be considered missing at random.

[7]Estimates are likely to be sensitive to the observation period over which labour market history is considered when constructing the control group.

Figure 1: Reason for Previous Job Ending (as % of total displacements), 1991 - 2001.



figures rose from 12% (1991), 18.8% (1997), to 23.2% (2001). Due to the nature of the data in the BHPS, one is unable to identify whether these individuals were subsequently recalled to their previous employer. However, recall is less of a common practise in the UK than in the US (Farber, 1999). A sizeable increase in the proportion of redundancies is also apparent (14.3% in 1991, peaking at 18.1% in 1993 and dropping to 16.3% in 2001).

In addition to genuine cases, the missing category includes individuals who were left the sample at wave 2, as well as individuals for which there was no lifetime job history (CLIFEJOB file) even though according to the survey design there should be[8]. A significant proportion of individuals continuously present, according to our definition, did not seem to contribute to the CLIFEJOB file even though they were present in waves 2 & 3. This category is likely to be considerably heterogenous and thus one does not expect any precise results regarding the wage implications of this separation type.

5 Empirical Results

5.1 Probit selection equation

Of the 3,516 individuals that were directly interviewed at Wave 1, 3,444 were included in the selection equation after dropping problematic regions. This figure reduced to 2,140 when those who were ever self-employed were dropped. This figure drops to 2029 after observations with missing real wage values are dropped. These individuals were not all subsequently followed, implying that some of these individuals may drop out of the sample at a later date, something -attrition- that the sample used in the wage analysis is conditioned to not include. Following Arulampalam (2001), the Inverse Mills Ratio $\lambda(X\delta_2) = \phi(X\delta_2)/\Phi(X\delta_2)$ is then interacted with year dummies in order to model how selection

[8]Issue with rule for dealing with pre-1.9.90 data as still remains an issue even after dropping individuals not present for at least 3 waves.

varies across the years. Current labour market status is also conditioned to exclude self-employment. Individuals reporting themselves in self-employment at interview date are excluded from the main analysis. This robustness of the results to this restriction is tested in subsequent sections.

Probit results for the first stage participation decision are presented in Table 10, in Appendix, Section 8.2. In a wage equation which only controls for tenure, experience and individual fixed effects, selection is significantly negative. However, inline with both Arulampalam (2001) & Gregory and Jukes (2001), I find that in a fully specified model sample selection is insignificant and does not seem to play a major role in the data considered, once observed characteristics are controlled for. The Inverse Mills ratio $\lambda(X\delta_2) = \phi(X\delta_2)/\Phi(X\delta_2)$ is consistently insignificant across specifications under consideration. Furthermore, its inclusion does not have any bearing on the estimated coefficients in the full specified model.

5.2 Replication of Arulampalam (2001).

Empirical results highlight persistent average wage penalties that depend on previous labour market status. These results replicate those of Arulampalam (2001) very closely, with previous inactivity estimated to carry an 11.6% average wage penalty into future employment. The 1991-1997 penalty is estimated to be roughly 3% lower at 8.7% for unemployment spells. Relative to a job-to-job transition, the penalty associated with previous unemployment is estimated to be 12.5% after four years of tenure. These results are qualitatively similar in both the 1991-1997 and 1991-2001 samples and thus not detailed here in the interest of brevity. The estimates are lower than many found in the existing US/UK literature due to the fact that they are averaged over all possible separation types, and are estimated relative to individuals who came into their current employment spell via a job-to-job transition. Studies focussing on involuntary job displacement are likely to find larger estimates of the impact of job loss, especially if they focus on high seniority/highly attached workers who stand to lose the most from job displacement. Furthermore, substantial heterogeneity exists across countries. However, the wage losses found in this study are consistent with those reported by UK longitudinal studies. See Table 13 for the results of selected studies.

6 Extensions

The evidence of State Dependence in individuals' Wage-Tenure profiles on re-employment raises the question of how this varies across reasons for leaving previous employment. Focus is limited to impact of involuntary displacement (redundancies). The definition of a displacement implemented in this analysis is consistent with Arulampalam (2001). The impact of involuntary displacements (redundancies) is gauged relative to a reference group which includes all other separation types (dismissals, temporary job ended, other reasons), job-to-job transitions (which are not considered as job displacement) and the group of never displaced (who were in their first labour market spell). I control for the missing category in all specifications and do not include this in the base category. Where an individual left

their previous employment for a better job and subsequently experienced a spell of non-employment, this is treated as a move for undefined (other) reasons. This affects 0.035% of separations.

6.0.1 The Effect of Redundancy & Previous Unemployment/ Inactivity

Contrary to Arulampalam (2001), who finds a positive average impact of 1.8%, specification 1, Table 4, suggests that being made redundant carries an average wage penalty of around 7.0% in the subsequent job. This is not a new observation in the literature, and accords with intuition more closely than the aforementioned result. "A surprising fraction of job changes (with and without on-the-job search) involve wage cuts (Devine & Kiefer, 1993)." Holding reason for leaving previous job constant, coming into the current employment spell via a spell of unemployment carries a large wage penalty of 10% (significant at the 5% level). Although insignificant at conventional levels, for those that were made redundant the penalty associated with previous unemployment is non-linear with age, being lower for those under 45. This suggests that on average, the impact of being made redundant and experiencing a spell of disruption does not carry a significantly different wage penalty to other separation types over and above the impact of experiencing a spell of unemployment.

Data limitations implied that Arulampalam (2001) was not able to identify the impact of previous unemployment and non-employment separately. Specification 1 separates out previous unemployment and inactivity (out of the labour force). Both the incidence and the duration of unemployment carry a significant negative wage penalty into subsequent employment. Controlling for duration, previous unemployment carries a wage penalty of 6.2% into subsequent employment spells. Moreover, unemployment spells lasting between six and twelve months carry an additional 8.9% penalty into subsequent employment relative to those lasting less than six. Specification 3 suggests that the wage penalty associated with unemployment reduces with incidence. However the penalty associated with inactivity does not diminish with incidence in the same fashion. The first spell carries the same wage penalty as the next. This should not be interpreted as suggesting that experiencing more unemployment spells is better than less. It may be that unemployment leads to re-employment in lower paying jobs, implying a lower wage penalty due to future incidence.

The general story seems to be robust to extensions of the observation period, bar from the duration effect (specifications 4 to 6, table 4). Whilst previous inactivity remains insignificant once holding duration constant, the impact of unemployment duration loses significance over and above the impact of a state dependence in the extended sample. Moreover, OLF spells lasting between six and twelve months are estimated to carry a significant 19.5% wage penalty relative to those lasting less than six. The estimates from the 1991 to 2001 period are likely to be more precisely estimated, suggesting that the impact of inactivity runs primarily through the duration effect. However, extending the observation period strengthens the argument that the penalty associated with inactivity doesn't diminish with incidence.

Table 4: Log Real Hourly Wage Equations for Male Sub-Sample, Individual-level observed heterogeneity controls, Previous Status unrestricted.

	1991-1997			1991-2001		
	[1]	[2]	[3]	[4]	[5]	[6]
Reason for leaving job§**.**						
redundant	-0.070*			-0.071**		
	(0.039)			(0.030)		
Previous Status (ref. Previous Employment.)						
Unemployment	-0.100**	-0.062*	-0.145**	-0.102**	-0.084**	-0.150**
	(0.041)	(0.034)	(0.036)	(0.031)	(0.028)	(0.031)
Inactivity	-0.082	-0.054	-0.135**	-0.079	0.009	-0.142**
	(0.066)	(0.084)	(0.057)	(0.049)	(0.070)	(0.049)
Reason for leaving job by previous labour market status.						
(ref. previous employment/no interruption)						
Unemployment						
Redundant	0.105			0.086		
	(0.065)			(0.054)		
Redundant*45+	-0.065			-0.076		
	(0.055)			(0.048)		
Inactivity						
Redundant	-0.094			0.004		
	(0.141)			(0.102)		
Redundant*45+	0.217			0.176		
	(0.220)			(0.211)		
Length of previous interruption (ref. < 6 months.)						
Unemployment						
6-12 months		-0.089*			-0.060	
		(0.049)			(0.039)	
12 months+		-0.033			-0.026	
		(0.074)			(0.053)	
Inactivity						
6-12 months		-0.078			-0.195**	
		(0.114)			(0.091)	
12 months+		-0.091			-0.129	
		(0.112)			(0.092)	
Number of previous unemployment spells (>1).						
1+ spell			-0.074			-0.057
			(0.059)			(0.047)
Previous Unemployment						
1+ spell			0.119**			0.107**
			(0.058)			(0.044)
Previous Inactivity						
1+ spell			0.033			0.072
			(0.144)			(0.101)
N	7666	7666	7666	10912	10912	10912
LL	1430	1418	1427	1468	1459	1469
$\bar{R}^2$	0.369	0.367	0.368	0.513	0.513	0.514
RMS error	0.202	0.202	0.202	0.212	0.213	0.212
AIC	-2.7e+03	-2.7e+03	-2.7e+03	-2.7e+03	-2.7e+03	-2.8e+03

§ Relative to quits to better job, temporary contract ended, other reasons & individuals who never experienced a displacement (first job spells). Dummy variable for missing reasons included in all specifications. **Sample selection:** Individuals never in self-employment at interview date. **Full set of control variables:** Current tenure, cumulative experience, age dummies, time dummies, a dummy for men whose current job if the first since leaving full time education, labour market experience dummies, marital status, health disability, temp/fixed-term contract, part-time job, employment sector, firm size, received training in current job, job type, regional dummies and industry dummies. Correction for selectivity interacted with time dummies also included.
Full results are available from the author on request.
* p<0.10, ** p<0.05, *** p<0.01

6.1 The Effect of Redundancy & Regional Heterogeneity

The results so far could be subject to a heterogeneity explanation if, conditional on regional mobility, the wage penalty due to job displacement varies within a country. The van Dijk and Folmer (1999) hypothesis would imply that the short-run wage penalty faced by individuals who experience unemployment in high unemployment regions will be relatively lower than that faced by the equivalent individual in low unemployment regions, due to their unemployment being seen as more a characteristic of the region rather than an individual productivity signal. In accordance with the predictions of job search theory, one would expect the average wage penalty associated with disruptions to be higher in slack than in tight labour markets.

Job search theory would predict that individuals displaced in tight labour markets will face lower job search costs due to more vacancies being available relative to the stock of job seekers (Cahuc and Zylberberg, 2004). In slack labour markets, the prospects of a successful match are lower as there will be more unemployment job seekers applying for a small pool of job vacancies. Of course, highly mobile (young and skilled) workers will be more able to mitigate this phenomenon by migrating to a tight labour market, however this may be less likely for less skilled workers and older workers with more regional attachments. Granted, individuals migrating to another region may face larger wage penalties than the equivalent worker finding reemployment in their pre-displacement region (Carrington, 1993).

In the UK context, the closest approximation to a self-contained local labour market is the Travel-To-Work Area (TTWA) level of aggregation (see Ball, 2009 for more information). The criterion on which TTWAs are defined is that: at least 75% of the resident economically active population actually work in the area, and that of everyone working in the area, at least 75% actually live in the area (Office for National Statistics, 2008). An important limitation of the TTWA measure is that: "[a]s some, predominantly professional and managerial, workers have increased their travel to work distance the self containment factor has been reduced. In effect this removes the extreme cases, so the TTWA definition has moved closer to a manual/ semi-skilled based definition (NOMIS, 1998)." The underlying difficulty of defining self-contained labour markets implies that an argument relating to the regional-specificity of human capital will be confounded by the fact that highly mobile young and/or skilled workers are less likely to work in their region of residence than their less skilled counterparts. Conducting this analysis at the Local Authority aggregation level makes the local labour market story even more implausible. A more plausible explanation may be that region of residence act purely as a signal of potential ability in the recruitment process. Individuals may select into a move *across* regional boundaries (LAD, TTWA) between labour market spells. However, evidence from the BHPS suggests that this is less of a concern given the low incidence (see Section 2).

6.1.1 TTWA Labour market tightness

Inconsistent with our priors, table 5 suggests that the average wage penalty associated with a job interruption is slightly higher in slack than tight local labour markets. In tight local labour markets, a spell of unemployment carries an 8.5% wage penalty into subsequent employment. This figure rises to 9.4% in slack Travel-to-Work Areas. Spell of inactivity carry an 8.5% wage penalty if experienced in tight labour markets. However, this penalty is insignificant at conventional levels. A significant 10.2% wage penalty is associated with inactivity spells experienced in slack labour markets. Whilst the impact of inactivity remains robust over the 1991-2001 period, no significant differences in the penalty associated with unemployment persist, conditional on labour market tightness. This result could be confounded by heterogeneity across separation types. Controlling for reason for leaving previous job, the average wage penalty associated with a spell of unemployment experienced in a tight local labour market is marginally larger over both periods (Table 6). But the average only tells part of

Table 5: Employment spells by average previous labour market characteristics ("Reason for leaving previous job" not controlled for).

Time Period	Tight	Slack	Urban	Rural	High U	Low U
	Average Previous Labour Market Characteristics.					
	Previous Labour Market Status (ref. Employment).♭					
Unemployment						
1991 - 1997	*-8.5%***	*-9.4%***	*-5.2%*	*-14.0%***	*-8.4%***	*-9.7%***
1991 - 2001	*-9.8%***	*-9.8%***	*-7.2%***	*-14.1%***	*-9.7%***	*-10.4%***
Inactivity						
1991 - 1997	*-8.5%*	*-10.2%**	*-6.7%*	*-11.4%**	*-9.8%**	*-5.6%*
1991 - 2001	*-7.8%**	*-10.2%***	*-7.3%*	*-8.9%*	*-8.9%**	*-6.2%*
	Previous Labour Market Status (ref. Employment).†					
Unemployment						
1991 - 1997	*-7.9%***	*-9.3%***	*-5.2%*	*-14.0%***	*-8.4%***	*-8.8%***
1991 - 2001	*-9.1%***	*-10.6%***	*-7.2%***	*-14.1%***	*-10.0%***	*-9.8%***
Inactivity						
1991 - 1997	*-8.5%*	*-9.2%**	*-6.7%*	*-11.4%**	*-8.3%*	*-9.4%**
1991 - 2001	*-6.4%*	*-10.2%***	*-7.3%*	*-8.9%*	*-7.8%**	*-8.4%*

♭ Definitions: Tight Labour Market - Vacancies/Unemployment ratio $\geq$ 2/3*Median. High Unemployment Region - Unemployment Rate $\geq$ 2/3*Median. Urban/Rural - Defined in text. * p<0.10, ** p<0.05, *** p<0.01
† Definitions: Tight Labour Market - Vacancies/Unemployment ratio $\geq$ Median. High Unemployment Region - Unemployment Rate $\geq$ Median. Urban/Rural - Defined in text. Full results in Appendix 8.3. * p<0.10, ** p<0.05, *** p<0.01
Sample selection: Individuals never in self-employment at interview date. **Full set of control variables:** age dummies, time dummies, a dummy for men whose current job if the first since leaving full time education, labour market experience dummies, marital status, health disability, temp/fixed-term contract, part-time job, employment sector, firm size, received training in current job, job type, regional dummies and industry dummies/ Correction for selectivity interacted with time dummies also included.

the story. Redundancy followed by unemployment in tight labour markets implies a 2.2% wage gain (13.7%-11%) on average, with no significant age variation in this effect. However, unemployment in slack local areas carries the full 9.4% wage penalty, with no variation by age. This story is robust to extensions of the observation period, although the wage gain becomes marginal (0.1% gain) in tight labour markets. Being made redundant and then experiencing a spell of inactivity in a slack labour market has a large positive impact on wage growth for the over 45's, whereas the impact is negative but insignificant at conventional levels for those under this age threshold. However these estimates loses significance when the observation window is extended. This may be capturing the fact that higher levels of human capital and less financial constraints imply that the older workforce are more likely to engage in productive search (Lippman and McCall, 1976). Productive search suggests a positive relationship between non-employment duration and re-employment wages. This could also be due to an inappropriate control group, as wages are generally higher for those over 45 (Kletzer and Fairlie, 2003).

Panel 3 looks at the time pattern of wage scarring whilst holding reason for leaving previous job constant. No significant variation by age is found. This specification suggests that a higher variance in the wage penalty associated with a spell of unemployment experienced in tight relative to slack local labour markets over the 1991 -1997 period. The penalty associated with previous redundancy, and subsequent unemployment, increases roughly monotonically with time on the job in both tight and slack labour markets. Over the 1991-1997 period, this decreases from a 13.7% gain in the first year (6.7% penalty, insignificantly different to the average effect), a 1.9% gain in the second (13.7%-11.8%), a 2% penalty in the third (13.7%-15.7%), and a long-run 1.3% wage penalty (13.7%-15%) relative to the counterfactual. However, all the short-term gains are in tight labour markets. The biggest wage

losses are associated with unemployment spells experienced in slack TTWAs. This wage penalty is 12.4% in the first year of tenure, 22.1% in the second, and 11.5% in the long-run. This story carries over to extensions of the observation period. However, being made redundant and then having a spell of inactivity is found to carry an insignificant wage penalty into subsequent employment spells, relative to the baseline, regardless of where it is experienced.

Table 6: Employment Spells: Labour Market Tightness†.

Previous Labour Market Status (ref. Employment).					
		Model A		Model B	
1.		*1991 - 1997*	*1991 - 2001*	*1991 - 1997*	*1991 - 2001*
Unemployment					
Tight		-11.0%**	-10.5%**		
Slack		-9.4%*	-10.4%**		
Inactivity					
Tight		-3.9%	-5.9%		
Slack		-5.8%	-7.4%		
Reason for leaving prev. job					
2. Redundancy§		-8.1%*	-7.2%**	-8.3%*	-7.3%**
Prev. Labour Market Status X Prev. Redundancy§ (ref. Employment).					
Unemployment	Age	*1991 - 1997*	*1991 - 2001*	*1991 - 1997*	*1991 - 2001*
Tight	ALL	13.7%**	10.6%*	13.4%**	10.5%**
	$\geq$ 45	-5.1%	-7.8%	-5.5%	-8.0%
Slack	ALL	7.6%	6.3%	5.3%	5.8%
	$\geq$ 45	-3.1%	-3.7%	-2.2%	-3.3%
Inactivity					
Tight	ALL	-7.5%	3.8%	-10.7%	2.4%
	$\geq$ 45	12.1%	10.9%	12.4%	11.6%
Slack	ALL	-16.2%	-5.9%	-5.9%	1.0%
	$\geq$ 45	47.4%**	23.0%	57.6%**	35.2%**
Model B: Prev. Unemployment X Tenure (years) on Current Job §.					
3. *Unemp*		[0,1)	[1,2)	[2,3)	[4,∞)
		1991 - 1997			
Tight		-6.3%	-11.8%*	-15.7%**	$\rightarrow$ -15.0%**
Slack		-12.4%**	-22.1%**	-7.0%	$\rightarrow$ -11.5%**
		1991 - 2001			
Tight		-7.6%**	-11.4%**	-12.4%**	$\rightarrow$ -13.3%**
Slack		-15.2%**	-18.8%**	-6.3%	$\rightarrow$ -10.6%**

† Tight labour market - Vacancies/Unemployment ratio > 2/3*Median. * p<0.10, ** p<0.05, *** p<0.01

§ Relative to quits to better job, temporary contract ended, other reasons & individuals who never experienced a displacement (first job spells). Holding missing reasons for leaving previous job constant in all specifications. **Sample selection:** Individuals never in self-employment at interview date. **Full set of control variables:** age dummies, time dummies, a dummy for men whose current job if the first since leaving full time education, labour market experience dummies, marital status, health disability, temp/fixed-term contract, part-time job, employment sector, firm size, received training in current job, job type, regional dummies and industry dummies/ Correction for selectivity interacted with time dummies also included.

NB. Previous inactivity * time dummy interactions mostly insignificant. Full results available from author on request.

Time-varying Regional Heterogeneity Accessibility enters positively and significantly into specification 2 (These results, not presented here, are available from the author on request). Individuals living in accessible regions earn on average 20-24% more than individuals living in inaccessible regions, holding all else constant and depending on time-period considered. Although negative, the impact of living in an urban area on earnings is insignificant in all specifications, and halved in the extended sample. The 12 month moving average of the change in ILO unemployment rate enters positively and significantly. Individuals living in local labour markets with higher longer run unemployment growth rates earn more on average, whilst the quarterly unemployment rate is insignificant. A one standard deviation change in the 12 month average change in quarterly ILO unemployment increases real wages

by 78%, everything else held constant. Although inconsistent with a priori expectations, this effect is not robust to extensions of the observation period as is likely a feature of the economy during the 1991-1997 period the first half of which was characterised by recession with unemployment levels peaking in the first quarter of 1993. These results are robust to the inclusion of both local authority and travel-to-work area fixed effects, suggesting that the main story seems to be robust for this period of observation. These results are generally invariant to the choice of specification. Therefore, in the interest of brevity, I do not discuss them further in subsequent sections.

Table 7: Employment Spells: URBAN/RURAL†.

Previous Labour Market Status (ref. Employment).					
		Model A		Model B	
1.		*1991 - 1997*	*1991 - 2001*	*1991 - 1997*	*1991 - 2001*
Unemployment					
Urban		-5.8%	-7.5%**		
Rural		-19.3%**	-16.5%**		
Inactivity					
Urban		-0.0%	-4.0%		
Rural		-7.9%	-7.3%		
Reason for leaving prev. job					
2. Redundancy§		-7.8%*	-7.4%**	-7.8%*	-7.4%**
Prev. Labour Market Status X Prev. Redundancy§ (ref. Employment).					
Unemployment	Age	*1991 - 1997*	*1991 - 2001*	*1991 - 1997*	*1991 - 2001*
Urban	ALL	7.6%	7.7%	7.5%	7.7%
	≥ 45	-2.4%	-4.3%	-2.2%	-4.0%
Rural	ALL	21.7%**	16.7%*	21.4%**	17.3%*
	≥ 45	-9.5%	-12.4%	-9.7%	-13.5%
Inactivity					
Urban	ALL	-14.3%	0.2%	-15.6%	1.0%
	≥ 45	38.0%**	27.0%**	34.1%*	27.1%*
Rural	ALL	-8.3%	-7.9%	-3.9%	3.9%
	≥ 45	9.0%	18.8%	7.1%	14.6%
Model B: Prev. Unemployment X Tenure (years) on Current Job §.					
3. *Unemp*		[0,1)	[1,2)	[2,3)	[4,∞)
		1991 - 1997			
Urban		-1.6%	-10.3%*	-7.2%	$\rightarrow$ -9.6%*
Rural		-18.6%**	-15.5%**	-24.3%**	$\rightarrow$ -21.9%**
		1991 - 2001			
Urban		-4.4%	-11.2%**	-7.7%**	$\rightarrow$ -9.6%**
Rural		-16.7%**	-15.0%**	-16.1%**	$\rightarrow$ -20.4%**

† Urban/Rural - Defined in text. * p<0.10, ** p<0.05, *** p<0.01

§ Relative to quits to better job, temporary contract ended, other reasons & individuals who never experienced a displacement (first job spells). Holding missing reasons for leaving previous job constant in all specifications. **Sample selection:** Individuals never in self-employment at interview date. **Full set of control variables:** age dummies, time dummies, a dummy for men whose current job if the first since leaving full time education, labour market experience dummies, marital status, health disability, temp/fixed-term contract, part-time job, employment sector, firm size, received training in current job, job type, regional dummies and industry dummies/ Correction for selectivity interacted with time dummies also included.

NB. Previous inactivity * time dummy interactions mostly insignificant. Full results available from author on request.

6.1.2 Local Authority-level characteristics

Due to lack of detailed controls at the travel-to-work area (TTWA) level of aggregation, I disaggregate the study to the local authority (LAUA) level in order to control for detailed regional-level characteristics. This exercise is carried out whilst maintaining a one-to-one link between the LAUA and TTWA levels of aggregation. Controlling for the length of interruption, unemployment incidence and regional-level characteristics, Table 5 suggests that, relative to a job-to-job transition, the impact of experiencing both inactivity and unemployment carry higher wage penalties for individuals

living in rural local authorities. Coming into the current employment spell via unemployment in a rural local authority carries a 14% wage penalty over the 1991-1997 period, relative to a job-to-job transition. This compares to an insignificant 5.2% penalty associated with the same experience in urban LAUAs. Likewise, experiencing a spell of inactivity in a rural LAUA carries a 11.4% relative wage penalty, whereas the penalty associated with urban local authorities is lower at 6.7% and insignificant at conventional levels. Average results are robust to extensions of the observation period. This may be driven by the fact that there are less jobs in rural areas, so an individual would have to search wider in order to find re-employment. However, local authorities cannot credibly be considered self-contained labour markets. It may be the case that less skilled workers are more likely to find local re-employment, however this is less likely for the mobile skilled workforce for whom even travel-to-work areas may be inappropriate. If distance is a factor when considering job offers, this may manifest itself in a negative correlation between urbanity and unemployment duration given that urban areas are generally characterised by higher levels of economic activity. However, Ball and Wilke (2009) showed that urban conurbations were amongst the worst places in Great Britain to live in terms of unemployment experiences. The time pattern of Wage Scarring suggests that being made redundant and then experiencing unemployment in an urban area is equally damaging for future earnings potential, independent of age. Taken together, these results suggest profound negative implications of unemployment experience for those living in urban areas, lending further support to Government initiatives like New Deal for Communities targeting these locations.

Controlling for reason for leaving previous job, only the impact of an unemployment spell experienced in a rural local authority remains significant at conventional levels over the 1991-1997 period (Panel 1). Whilst a spell of rural unemployment carries a 19.3% wage penalty into subsequent employment, relative to a job-to-job transition the penalty associated with urban unemployment spell is insignificant at conventional levels. Moreover, the impact of rural unemployment is non-linear with age (see panel 2, column 3). For those under 45, a spell of unemployment experienced in a rural local authority after being made redundant carries a 2.4% (21.7% -19.3%) wage *gain* into future employment relative to a other separation types. However, over 45s experience the full 19.3% wage penalty regardless. Since the over 45s are more likely to be mortgaged home owners, this result may be due to financial and residential mobility constraints implying that displaced mortgaged home owners are more likely to lower their reservation wages and accept local re-employment than renters who have more flexibility to widen their job search (Coulson and Fisher, 2009). Although the average wage penalty associated with inactivity is insignificant over the 1991-1997 period, being made redundant and then experiencing a spell of inactivity in an urban local authority carries a substantial wage gain into future employment for the over 45s (38%). These results are robust to time-period extensions.

The wage scar associated with previous inactivity is insignificant at conventional levels over the 1991-1997 period. Granted, a large and persistent wage penalty is associated with unemployment both in urban and rural local authorities. The magnitude of this effect is twice as large in rural areas on average (see panel 3). Moreover, this story carries over to extensions of the observation

period to 1991-2001. Over the 1991-1997 period, for the under 45s redundancy and subsequent rural unemployment implies a 2.8% wage gain (-18.6% + 21.4%) in the first year of tenure, a 5.9% gain (-15.5% + 21.4%) in the second, a 2.9% wage loss (-24.3% + 21.4%) in the third and a long-run wage penalty of 0.5% (-21.9% + 21.4%). However, the over 45s the same scenario implies the full penalty of 18.6% in the first year, 15.5% in the second, increasing to 21.9% in the long-run. Redundancy and subsequent urban unemployment implies an insignificant 1.6% wage loss in the first year, rising to a significant 10.3% penalty in the second, and a 9.6% wage penalty in the long-run, relative to the job-to-job transitions. No significant age variation is found in the impact of redundancy followed by urban unemployment, and these results carry over to extensions of the time frame.

Table 8: Employment Spells: High Unemployment/Low Unemployment†.

Previous Labour Market Status (ref. Employment).					
		Model A		Model B	
1.		*1991 - 1997*	*1991 - 2001*	*1991 - 1997*	*1991 - 2001*
Unemployment					
High U		-10.8%**	-10.8%**		
Low U		-13.1%**	-12.1%**		
Inactivity					
High U		-4.9%	-6.7%		
Low U		-1.8%	-4.8%		
Reason for leaving prev. job					
2. Redundancy§		-7.8%*	-7.2%**	-7.8%*	-7.3%**
Prev. Labour Market Status X Prev. Redundancy§ (ref. Employment).					
Unemployment	Age	*1991 - 1997*	*1991 - 2001*	*1991 - 1997*	*1991 - 2001*
High U	ALL	11.9%*	10.1%*	11.5%*	10.2%*
	≥ 45	-2.2%	-6.9%	-2.1%	-6.5%
Low U	ALL	19.9%**	13.4%**	19.9%**	13.7%**
	≥ 45	-16.6%**	-12.0%*	-15.7%**	-11.9%*
Inactivity					
High U	ALL	-12.2%	-1.5%	-1.0%	0.0%
	≥ 45	9.8%	7.9%	13.7%	15.1%
Low U	ALL	22.6%	-4.9%	27.2%*	28.8%**
	≥ 45	-5.4%	13.4%	-8.6%	-10.3%
Model B: Prev. Unemployment X Tenure (years) on Current Job §.					
3. *Unemp*		[0,1)	[1,2)	[2,3)	[4,∞)
		1991 - 1997			
High U		-6.6%	-12.1%**	-13.1%**	$\rightarrow$ -15.2%**
Low U		-13.7%	-17.8%**	-16.0%**	$\rightarrow$ -16.0%**
		1991 - 2001			
High U		-8.0%	-12.3%**	-11.3%**	$\rightarrow$ -13.5%**
Low U		-13.2%*	-16.0%**	-10.4%	$\rightarrow$ -14.7%**

† High Unemployment labour market - ILO unemployment rate > 2/3*Median. * p<0.10, ** p<0.05, *** p<0.01

§ Relative to quits to better job, temporary contract ended, other reasons & individuals who never experienced a displacement (first job spells). Holding missing reasons for leaving previous job constant in all specifications. **Sample selection:** Individuals never in self-employment at interview date. **Full set of control variables:** age dummies, time dummies, a dummy for men whose current job if the first since leaving full time education, labour market experience dummies, marital status, health disability, temp/fixed-term contract, part-time job, employment sector, firm size, received training in current job, job type, regional dummies and industry dummies/ Correction for selectivity interacted with time dummies also included.

NB. Previous inactivity * time dummy interactions mostly insignificant. Full results available from author on request.

van Dijk and Folmer (1999) hypothesis: Robust support for the van Dijk and Folmer (1999) hypothesis is found for the UK (Table 8, Panel 1). The strongest support being in the case of redundancies. In a specification without controls for heterogeneity across separation-type, on average unemployment experienced in areas of low unemployment is found to carry a higher wage penalty into subsequent employment, all else constant (Table 5). Holding duration of interruption, unemployment

incidence *and* reason for leaving previous job constant, unemployment spells experienced in high unemployment regions carry an average 10.8% wage penalty into subsequent employment. This figure is higher at 13.1% in low unemployment regions (Column 1). The economic significance of these two labour market states remains robust to extensions of the observation period, however, these estimates may be weakened/confounded by *efficiency wage* arguments (Shapiro and Stiglitz, 1984). This argument suggests that if firms use higher wages as a means of decreasing turnover and the incentive to shirk on-the-job, then higher unemployment levels make losing a job more costly. This cost is predicted to decrease with the level of unemployment benefits and increases with the level of unemployment. As long as the incentive to pay efficiency wages remains constant over time, then fixed effects will control for this. However, if this is related to the business cycle then this won't be the case, although time dummies will help to absorb most of the business cycle effect.

Panel 2 demonstrates the age variation in wage scarring, by previous labour market status. In all specifications previous inactivity is insignificant at conventional levels. Over the 1991-1997 period, redundancy and subsequent unemployment in high unemployment regions carries a significant 1.1% average wage gain into subsequent employment for under 45s (-10.8% + 11.9%). For those over 45, the same scenario implies a 1.1% average wage gain. Whilst redundancy and subsequent unemployment in low unemployment regions implies a 6.8% average wage gain (-13.1% + 19.9%) for the under 45s, the over 45s experience a 9.8% penalty (-13.1% + 19.9% - 16.6%) in subsequent employment. This result is robust to extending the time period, however redundancy-unemployment spells in high unemployment regions imply a -0.7% average wage penalty, independent of age.

Redundancy and subsequent unemployment in high unemployment regions is associated with a 6.6% wage penalty which is insignificantly different to the 11.5% average wage gain in the first year of employment. This would imply a 4.9% wage gain, although this linear combination would be insignificant at conventional levels (Panel 3). This drops to a 0.6% wage penalty (11.5% - 12.1%) in the second year, with a long-run wage penalty of -3.7% after 4 years in employment. These penalties are not found to vary with age group. In the case of low unemployment regions, the first year penalty for the under 45s is insignificantly different to the average wage gain of 19.9% associated with previous redundancy and subsequent unemployment for this age category. This drops to a 2.1% gain (19.9% - 17.8%) in the second year, and a 3.9% wage gain (19.9% - 16%) in the long-run. For the >45 age category, previous redundancy and subsequent unemployment is associated with an 13.7% wage penalty in the first year (insignificantly different to the significant 4.2% average wage gain (19.9%-15.7%)), dropping to 13.6% (4.2% - 17.8%) in the second and 11.8% (4.2% - 16%)in the long-run. This time profile of wage scarring carries over to extensions of the observation period, see table 8. Whilst the differences across region-types are robust but relatively small on average, at around 2%, the age differences in the impact of redundancy remain large with significant wage losses associated with unemployment spells in low unemployment regions for the over 45s (11.8% in low-, versus 3.7% in high unemployment regions, an 8.1% difference). The Local Authority-level results are robust to the inclusion of TTWA fixed effects, allowing for correlation across LAs within each TTWA.

7 Sensitivity Analysis

Heterogeneity in human capital investment is important when considering the impact of career interruptions on future wage growth (Kunze, 2002). The OECD-defined 'Out of the Labour Force' (OLF) indicator includes full time education, a productive investment in general human capital complementary to human capital accumulated in the labour market. This measure is likely to be confounded by differences across labour market states within the OLF category if the sample is not conditioned to exclude individuals who have not permanently left full-time education, or full-time education is not defined as a separate labour market state[9]. The average effect of an unemployment spell seems to be robust to classifying full-time education as a separate labour market state, however the impact of previous inactivity becomes insignificant in all specifications. This result is corroborated in the both the 1991-1997 and 1991-2001 samples, including controls for length of previous interruption and unemployment incidence. Although evidence of a persistent impact of previous unemployment on future wage growth is evident in both samples, the penalty associated with inactivity is much more variable. The long-run penalty loses significance when the observation period is extended. In the case of previous unemployment, controlling for regional heterogeneity, the general story remains robust to defining full-time education as a separate labour market state. However, previous inactivity is insignificant in all specifications. Redefining the thresholds used in the main analysis to the median, instead of 2/3rds of the median, does not produce qualitatively large changes to the main story, however the result supporting the van Dijk and Folmer (1999) hypothesis loses strength on average, with mixed long-run support at best (see Table 5, as well as Appendix Table 11 & 12. Interpretation is provided in the footnotes.). If we are to accept the definition employed in the main analysis (2/3rds of the Median as a threshold) over a more liberal definition (Median threshold) then support for the hypothesis under test is strengthened. The choice of the former definition can be justified, given that the Labour Economics literature tends to employ the former definition rather than the latter, e.g. the "Stepping Stone" literature (Stewart, 2007). However, whilst this definition retains the property of being objective, it could be criticised for being to an extent arbitrary.

Individuals may use self-employment as a way of cushioning the wage penalty associated with job loss. Thus for these individuals, wage losses may be kept to a minimum. However, there are likely to be systematic unobserved differences between individuals that pursue the self-employed route, and those that pursue full-time employment. Including the self-employed in the analysis is likely to impart downward bias on the estimated average earnings losses associated with involuntary displacement if this fact is not controlled for. In the main analysis, consistent with Arulampalam (2001), individuals were allowed to be previously self-employed, as long as they never reported themselves in self-employment at survey date. Here self-employment is treated as a separate previous as well as

[9]Results for the sensitivity analysis are available from the author on request. Previous labour market status is redefined to only consider disruptions which occurred in the last 5 years of labour market history when constructing this indicator. However, this approach is dropped in favour of the unrestricted version based on information criterion.

current labour market state. Although wages whilst self-employed are unavailable due to the difficulty of reporting self-employment hours, this approach allows one to capture whether the wage scarring effects of job displacement are mitigated for those entering a self-employment spell. This sign effect is essentially an empirical question, as it is possible that individual moving into self-employment in a declining industry may face lower earnings prospects, e.g. the 80's mining sector in the UK. Full-time education is treated as a separate previous labour market state, the aim of this exercise being is to shed further light on the representativeness of the main results.

Previous self-employment carries a large wage penalty into current employment, relative to a job-to-job transition. However, this high initial penalty proves to be very temporary when contrasted with the permanent wage penalties associated with previous unemployment and inactivity. The wage effect of previous self-employment is insignificant on average, but positive in the long-run. There is a positive long-run impact on wage growth for individuals entering employment via a spell of self-employment, having been made redundant in their last spell of full-time employment. However this effect is only positive in the long-run, with a temporary penalty in the short-run. Consistent with the previous robustness check, the effect of previous unemployment seems robust to the regional heterogeneity extensions.

The final robustness check addresses the representativeness of the sample used in the analysis. To what extent is the sample representative of the individuals interviewed in the BHPS? If this is the case, then since the BHPS is a representative survey, the results can be extrapolated to the population as a whole. If not, then they are unlikely to be generalisable. Whilst it is common practise to restrict attention to the OSM who are continuously present over the observation period, recent studies have cast doubt over the validity of BHPS-based estimates when attrition is assumed random (Bradley et al., 2007). Results restricted to continuously present OSM are contrasted with the existing (main analysis) results, where the OSM are followed until the first instance of attrition. Results for the continuously present OSM are generally very similar to those presented in the main analysis, notably in the extended sample. The results for the 1991-1997 period are very close to the basic and extended results presented in Arulampalam (2001). Furthermore, the regional heterogeneity story seems invariant to this restriction.

8 Summary and Conclusion

Although institutions may vary across countries, there is generally not enough variation in institutional context within a country to generate the observed differences in wage outcomes across regions (Carrington, 1993). The aim of this exercise is to shed some light on the potential underlying mechanisms at play. The main hypothesis under test is whether *unemployment* spells experienced in high unemployment regions are seem by future employers as more a characteristic of the region than a negative productivity signal (van Dijk and Folmer, 1999). If so, then what long-term implications does this have for future wage growth (Wage Scarring)? In order to address this question, the British

Household Panel Survey (BHPS) is used to construct continuous work-life histories following individuals from first entry into the labour market and capturing spells of employment, unemployment and inactivity. Furthermore, this novel dataset allows for the importance of regional heterogeneity to be gauged in the Wage Scarring context.

Strong evidence of Wage Scarring is found, with no sign of earnings recovery. Arulampalam (2001) concludes that the first spell of non-employment carries the highest penalty. Separating non-employment into unemployment and inactivity spells, no evidence of a reduction in the wage penalty associated with incidence of inactivity is found. Large regional differences, with respect to labour market tightness and urbanity, are found in the impact of redundancies on future wage growth, which could not be accurately accounted for without the data structure employed. Pronounced age differences in the wage scarring effect of redundancies are also found in the extensions to the study. The wage scarring effect of being made redundant is negligible for *all* unemployment spells experienced in tight local labour markets, with a short-run wage gain, whilst under 45s' with the same experience in rural areas face a marginal long-run wage *gain*. Experiencing a spell of unemployment in slack labour markets implies a substantial wage scar, independent of age. For over 45s, with higher levels of regional attachment, the wage penalty associated with rural unemployment spells is substantial with no sign of recovery. Whilst skilled workers are prone to engage in wider job search, the over 45s are more likely to be mortgaged home owners and thus are more likely to accept lower reservation wage jobs locally in order to maintain mortgage payments than those without these financial constraints. The impact of accepting 'low quality' employment, rather than waiting for a higher quality match, may have far reaching consequences for future human capital accumulation and subsequent wage growth. Ball and Wilke (2009) showed that urban conurbations were amongst the worst places in Great Britain to live in terms of unemployment experiences. Estimates imply that being made redundant and then experiencing unemployment in areas of high economic activity is equally damaging for future earnings potential, independent of age. Taken together, this suggests that the negative implications of urban unemployment experience are long lasting, lending further support to Government initiatives like New Deal for Communities targeting these locations.

Robust long-run evidence is found supporting the van Dijk and Folmer (1999) hypothesis, on average and for over 45s made redundant in their previous jobs. Redundancy implies a 3.7% wage loss if unemployment spells were experienced in high unemployment regions, independent of age. However, if experienced in low unemployment regions this implies a 3.9% wage *gain* for those under but an 11.8% wage *loss* for those over 45. This story is independent of whether controls for reason for leaving previous job are introduced, full-time education is treated as a separate labour market state, whether the self-employed are included and whether the sample is restricted to the Original Sample Members continuously present over the observation period. However, the penalty associated with previous inactivity is found to be less robust. Given that inactivity is a very heterogenous state, this is not surprising. Addressing the impact of inactivity sub-states is of interest, however this is not pursued given that this would result in imprecise results for some categories due to small cell size.

Cockx and Picchio (2009) model the joint distribution of unemployment duration, accepted wages, and subsequent employment duration using detailed Belgian Administrative data. Incorporating the impact on subsequent wage growth, using a similar approach, would be a promising avenue for future research.

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Appendix

8.1 Data Descriptives

Table 9: Male sub-sample by previous labour market status.

PREV_STAT:	1991-1997. EMP. [1]	UNEMP. [2]	OLF [3]	1991-2001. EMP. [4]	NON-EMP. [5]	OLF [6]
Personal Characteristics						
Age < 25	0.07	0.13	0.24	0.05	0.10	0.18
Age 25 - 29	0.12	0.15	0.17	0.11	0.14	0.15
Age 30 - 34	0.17	0.17	0.15	0.15	0.16	0.16
Age 35 - 39	0.16	0.15	0.12	0.16	0.16	0.14
Age 40 - 44	0.15	0.12	0.10	0.15	0.14	0.11
Age > 45	0.35	0.28	0.22	0.37	0.32	0.26
White	0.97	0.98	0.95	0.97	0.98	0.96
Married	0.79	0.70	0.63	0.80	0.72	0.67
Spouse Employed	0.61	0.52	0.47	0.62	0.54	0.50
Children	0.38	0.36	0.37	0.39	0.37	0.39
Health limits type of work	0.06	0.09	0.05	0.07	0.10	0.06
Disabled	0.01	0.02	0.02	0.01	0.02	0.02
School Type Attended						
Grammar School	0.15	0.14	0.15	0.15	0.14	0.16
Private School	0.06	0.05	0.06	0.06	0.05	0.06
Technical	0.08	0.08	0.04	0.07	0.07	0.05
Highest Qualification						
Degree	0.15	0.17	0.17	0.16	0.17	0.18
Other Higher	0.29	0.20	0.25	0.32	0.23	0.29
A'Levels	0.12	0.17	0.18	0.12	0.17	0.17
O'Levels	0.18	0.20	0.23	0.17	0.17	0.22
Other Qualifications	0.06	0.08	0.04	0.06	0.09	0.03
Apprenticeship	0.03	0.02	0.01	0.03	0.02	0.01
Housing Tenure						
Owned	0.12	0.13	0.13	0.13	0.15	0.13
Mortgage	0.73	0.65	0.71	0.73	0.65	0.72
Council tenant	0.05	0.11	0.07	0.05	0.10	0.06
Housing Association	0.02	0.03	0.02	0.02	0.03	0.02
Workplace Characteristics						
Public Sector	0.03	0.03	0.03	0.03	0.02	0.03
Public Services	0.19	0.22	0.23	0.19	0.22	0.24
Charity	0.02	0.01	0.02	0.02	0.01	0.02
Other Sector	0.02	0.01	0.02	0.02	0.01	0.02
Missing	0.00	0.01	0.00	0.00	0.00	0.00
Workplace Size						
50 - 99	0.14	0.13	0.12	0.14	0.14	0.12
100 - 199	0.13	0.12	0.09	0.13	0.12	0.10
> 200	0.37	0.36	0.40	0.37	0.36	0.40
Workplace Union Presence	0.57	0.51	0.60	0.56	0.49	0.60
Union Member	0.41	0.31	0.43	0.39	0.30	0.43
Contract						
Current job is part-time	0.01	0.07	0.06	0.01	0.07	0.06
Current temp.	0.04	0.13	0.07	0.04	0.13	0.07
Occupation						
Skilled Non-Manual	0.30	0.29	0.27	0.29	0.28	0.27
Unskilled Manual	0.14	0.23	0.11	0.14	0.25	0.11
Non-manual	0.25	0.27	0.30	0.25	0.27	0.31
Professional/ Managerial	0.29	0.18	0.27	0.29	0.17	0.26
Industry						
Energy & Water Supplies	0.04	0.03	0.05	0.04	0.03	0.05
Extraction of Metals, etc. Manufacture of Metals	0.05	0.03	0.05	0.05	0.03	0.05
Metal goods, engineering & Vehicles	0.17	0.17	0.16	0.16	0.16	0.16
Other Manufacturing	0.13	0.16	0.07	0.12	0.16	0.07
Construction	0.05	0.04	0.05	0.05	0.04	0.05
Distribution, Hotels & Catering, Repairs	0.12	0.13	0.16	0.13	0.12	0.15
Transport & Communications	0.10	0.07	0.07	0.10	0.07	0.07
Banking, Finance, etc.	0.12	0.10	0.13	0.13	0.11	0.12
Other Services	0.21	0.25	0.23	0.21	0.26	0.25
Income						
Deflated Real Wage[♯]	9.62	7.98	9.02	10.72	8.96	10.10
Usual hours worked	39.82	37.78	37.73	39.89	37.82	37.80
Usual paid overtime hours	3.37	2.89	2.69	3.17	2.90	2.61
Cumulative Employment Experience (months)	265	223	198	276	240	216
Current Spell Length (months)	108	72	173	111	81	185
Total	4917	1437	1312	7109	2076	1741

[♯]: Assumes overtime is paid at 1.5 times normal rate. Full labour market history since leaving full-time education used to construct indicators. Specifications 1 to 6 are from the sample used in the Wage analysis which excludes the problematic regions: Redcar & Cleveland; East Riding of Yorkshire; North East Lincolnshire; North Somerset; South Gloucestershire; Swindon; Medway Towns; West Berkshire; Conway; Debighshire; Flintshire; Bridgend; Caerphilly; Aberdeenshire; West Dunbartonshire; East Ayrshire; East Dunbartonshire; North Ayrshire; North Lanarkshire; South Lanarkshire.

8.2 Selection Equations

Table 10: Marginal Effects from Sample Selection Probit for Full (Male) Sample.

Variables.	dy/dx
Individual Characteristics	
Age (ref. <25)	
Age 25 - 29	0.140**
Age 30 - 34	0.071**
Age 35 - 39	0.144**
Age 40 - 44	0.061
Age > 45	-0.025
White	0.198**
Married/Cohabiting	-0.002
Spouse Employed	0.132**
Children	-0.090
Children And Married/Cohabiting	0.091
Disabled	-0.269**
Health Limits Type Of Work	-0.178**
School Type Attended (ref. Comprehensive, other)	
Grammar School (no fee)	0.010
Private School	-0.003
Technical	-0.016
Highest Qualification (ref. No Formal Qualifications)	
Degree	0.188**
Other higher	0.096**
A'Levels	0.129**
O'Levels	0.072**
Apprenticeship	0.096*
Other Qualifications	0.082**
Housing Tenure (ref. Private renter)	
Owned	0.123**
Mortgage	0.196**
Council tenant	0.011
Housing Assoc	-0.005
Father's Occupation when 14 (ref. to Army, Agriculture, Unskilled manual, unknown/invalid).	
Skilled manual	-0.054**
Non-manual	0.012
Professional/Managerial	-0.071*
Self-Employed	-0.076*
1991 Economically Active TTWA Unemployment Rate	-1.851**
Government Office Region (Ref. London)	
SE	-0.143**
SW	0.093*
E.Anglia	0.047
E.Midlands	-0.002
W.Midlands	-0.101**
N.West	-0.055
Yorkshire & Humber	-0.052
North	-0.012
Wales	0.001
Scotland	-0.134**
N	2029
LL	-1034.265
LL_int	-1235.435
Pseudo R^2	0.163
$\chi^2(12)$	73.10***
AIC	2150.531

* p<0.05, ** p<0.01, *** p<0.001 (TTWA98 Cluster Robust Standard Errors)
Marginal Effects evaluated at the sample means of the explanatory variable in question.
$\chi^2(12)$ tests joint significance of 12 identifying variables (exclusion restrictions).
Excludes individuals ever in self-employment at interview date plus missing real wage observations (reducing count from 2140 to 2029.)
LL_int - Likelihood ratio of intercept only model.

8.3 Alternative Threshold Definition: $\geq$ Median.

Table 11: Employment Spells: Labour Market Tightness†.

		Model A		Model B	
Previous Labour Market Status (ref. Employment).					
1.		*1991 - 1997*	*1991 - 2001*	*1991 - 1997*	*1991 - 2001*
Unemployment					
Tight		-7.6%	-8.4%**		
Slack		-13.7%**	-13.4%**		
Inactivity					
Tight		-3.9%	-5.0%		
Slack		-4.8%	-8.0%		
Reason for leaving prev. job					
2. Redundancy§		-7.9%*	-7.5%**	-7.9%*	-7.5%**
Prev. Labour Market Status X Prev. Redundancy§ (ref. Employment).					
Unemployment	Age	*1991 - 1997*	*1991 - 2001*	*1991 - 1997*	*1991 - 2001*
Tight	ALL	9.9%	8.2%	10.0%	8.6%
	≥ 45	-7.4%	-9.7%*	-7.6%	-9.6%*
Slack	ALL	16.7%**	12.9%*	16.8%**	13.0%**
	≥ 45	-3.7%	-5.5%	-3.8%	-5.5%
Inactivity					
Tight	ALL	-5.9%	6.1%	-5.3%	8.8%
	≥ 45	0.1%	7.7%	-0.3%	2.7%
Slack	ALL	-13.7%	-2.4%	-12.2%	0.8%
	≥ 45	38.3%**	18.2%	36.5%**	16.2%
Model B: Prev. Unemployment X Tenure (years) on Current Job §.					
3. *Unemp*		[0,1)	[1,2)	[2,3)	[4,∞)
		1991 - 1997			
Tight		-1.7%	-9.2%*	-12.1%**	$\rightarrow$ -12.2%**
Slack		-12.6%**	-16.7%**	-15.0%**	$\rightarrow$ -16.1%**
		1991 - 2001			
Tight		-6.2%	-9.3%**	-10.2%**	$\rightarrow$ -11.3%**
Slack		-11.6%**	-16.3%**	-12.2%**	$\rightarrow$ -15.7%**

Interpretation: On average there is a higher penalty for unemployment spells experienced in *tight* labour markets, once reason for leaving previous job is controlled for (panel 1). *1991-1997:* Redundancy followed by unemployment carries a 1.7% penalty & 0.1% wage gain if experienced in tight and slack labour markets respectively. This increases to a long-run penalty of 12.2% for the former case, and a .7% wage gain for the latter. No significant age variation is found, over and above the average effect. *1991-2001:* Redundancy followed by unemployment carries a 6.2% & 9.6% wage penalty for under and over 45s respectively, if experienced in tight labour markets. This increases to a long-run penalty of 11.3% and 20.9% for under and over 45s respectively. In slack labour markets, the same scenario implies a 1.4% wage gain in the first year of tenure, decreasing to a 2.7% long-run penalty relative to a job-to-job transition.

† Tight labour market - Vacancies/Unemployment ratio > Median. * p<0.10, ** p<0.05, *** p<0.01

§ Relative to quits to better job, temporary contract ended, other reasons & individuals who never experienced a displacement (first job spells). Holding missing reasons for leaving previous job constant in all specifications. **Sample selection:** Individuals never in self-employment at interview date. **Full set of control variables:** age dummies, time dummies, a dummy for men whose current job if the first since leaving full time education, labour market experience dummies, marital status, health disability, temp/fixed-term contract, part-time job, employment sector, firm size, received training in current job, job type, regional dummies and industry dummies/ Correction for selectivity interacted with time dummies also included.

NB. Previous inactivity * time dummy interactions mostly insignificant. Full results available from author on request.

8.4 Continuous Work-life histories

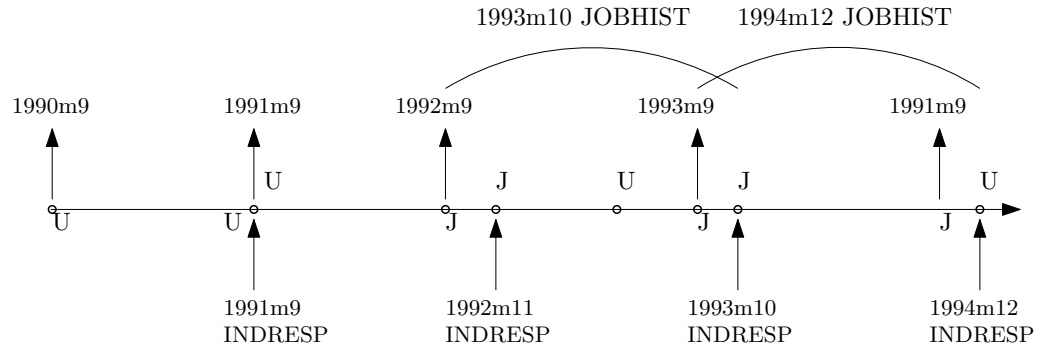
Figure 2 illustrates the structure of the British Household Panel Survey. In addition to the basic structure, retrospective job and employment status information, covering the period since first leaving full-time education, is collected at Waves 2 & 3. There is a developing literature on the systematic construction of continuous work-life histories, including Halpin (1997), Upward (1999), Paull (2002) & Maré (2006). Maré (2006) provides an extensive review of these studies, highlighting the benefits and limitations of each approach. Given that a direct measure of experience was desired and given the lack of a satisfactory data source, steps were taken to develop continuous work-life histories independently. A systematic, rules-based approach was adopted in order to minimise highlighted issues in the literature as well as to aid replication. A companion paper, Ball (2010) documents and justifies the steps taken.

Table 12: Employment Spells: High Unemployment/Low Unemployment†.

		Previous Labour Market Status (ref. Employment).			
		Model A		Model B	
1.		*1991 - 1997*	*1991 - 2001*	*1991 - 1997*	*1991 - 2001*
Unemployment					
High U		-10.8%**	-10.6%**		
Low U		-10.9%**	-11.2%**		
Inactivity					
High U		-2.5%	-5.3%		
Low U		-4.8%	-6.6%		
Reason for leaving prev. job					
2. Redundancy§		-7.7%*	-7.3%**	-7.7%*	-7.3%**
Prev. Labour Market Status X Prev. Redundancy§ (ref. Employment).					
Unemployment	Age	*1991 - 1997*	*1991 - 2001*	*1991 - 1997*	*1991 - 2001*
High U	ALL	13.4%**	10.2%**	12.8%*	10.1%*
	≥ 45	-5.2%	-7.7%	-5.3%	-7.7%
Low U	ALL	12.7%*	11.4%*	12.5%*	11.4%*
	≥ 45	-5.4%	-8.2%	-5.5%	-8.2%
Inactivity					
High U	ALL	-14.9%	-1.5%	-16.7%	-0.0%
	≥ 45	-12.4%	7.9%	-8.5%	9.4%
Low U	ALL	1.5%	-4.9%	-2.0%	2.9%
	≥ 45	16.8%	13.4%	-19.3%	14.6%
Model B: Prev. Unemployment X Tenure (years) on Current Job §.					
3. *Unemp*		[0,1)	[1,2)	[2,3)	[4,∞)
		1991 - 1997			
High U		-7.9%*	-11.9%**	-8.6%	→ -15.2%**
Low U		-7.0%	-13.1%**	-18.6%**	→ -13.6%**
		1991 - 2001			
High U		-9.6%**	-12.4%**	-7.7%*	→ -13.1%**
Low U		-8.2%*	-12.9%**	-15.1%**	→ -13.6%**

Interpretation: On average there is some *weak* evidence (0.1%-0.6%) in support of the van Dijk and Folmer (1999) hypothesis, once reason for leaving previous job is controlled for (panel 1). In terms of redundancies, this evidence is more mixed. *1991-1997:* Redundancy followed by unemployment carries a long-run 2.4% and 1.1% wage penalty if experienced in high and low unemployment regions respectively. *1991-2001:* For the 1991-2001 period, these figures are 3% and 2.2% after 4 years in high and low unemployment regions respectively. No significant age variation is found, over and above the average effect of being made redundant and then experiencing unemployment when the median threshold is employed.
† High Unemployment labour market - ILO unemployment rate > Median. * p<0.10, ** p<0.05, *** p<0.01
§ Relative to quits to better job, temporary contract ended, other reasons & individuals who never experienced a displacement (first job spells). Holding missing reasons for leaving previous job constant in all specifications. **Sample selection:** Individuals never in self-employment at interview date. **Full set of control variables:** age dummies, time dummies, a dummy for men whose current job if the first since leaving full time education, labour market experience dummies, marital status, health disability, temp/fixed-term contract, part-time job, employment sector, firm size, received training in current job, job type, regional dummies and industry dummies/ Correction for selectivity interacted with time dummies also included.
NB. Previous inactivity * time dummy interactions mostly insignificant. Full results available from author on request.

Figure 2: BHPS Data Structure: Time line showing data collection points and data source coverage.



Job history (JOBHIST) file is a retrospective data source, covering the last 12 months (since the first of September of the previous year). Individual response (INDRESP) file is a snapshot of labour market activity at interview date. JOBHIST data only collected if labour market status changed in the last 12 months. For an alternative illustration of the overlap (Halpin, 1997, see Figure 1).

Table 13: Summary of selected job displacement studies.

Paper	Data	Sample	Key Findings
Cross-sectional Studies:			*Associated Earnings Penalties:*
Addison and Portugal (1989)	US DWS	1979-1984♮. full-time males aged 20-65. Permanently displaced (plant closure/layoffs/shift abolished), Non-Agriculture.	*Unemployment incidence:* 15% in next job (per 10yrs. pre-displacement tenure); *Unemployment Duration:* .8-1.4% p/yr. Large losses for industry (16.1%-19.8%) & occupation (5.4%-13.9%) movers.
Houle and van Audenrode (1995)	Canadian DWS	1981-1986♮. full-time males aged 25-60. Permanently displaced (plant closure/layoffs/shift abolished), Non-Agriculture/Construction.	*Unemployment incidence:* 20% in next job (per 10yrs. pre-displacement tenure); *Unemployment duration:* 1.5% p/yr. Suggests lack of transferability of firm-specific skills as possible reason for larger penalty than in US.
Carrington (1993)	US DWS	1984, 1986 & 1988 waves♮ (1979-1988), full-time males aged 21-63, Permanantly displaced, Private Sector. Excluding inter-state post-displacement migrants.	*Penalty:* 21.6% industries with low *vs.* 3.9% industries with high state-specific employment growth. 20.5% av. penalty (per 10yrs. pre-displacement tenure), mitigated if labour market expanding. 4.5% (per 10yrs. pre-displacement tenure) if re-employed in same industry and occupation.
Neal (1995)	US DWS	1984, 1986, 1988 & 1990 waves♮ (1979-1990), full-time males aged 20-61. Permanently displaced (plant closures).	*Log wage change:* 23.4% industry switchers; 11.6% industry stayers. Strongest returns to pre-displacement tenure for stayers: 20% next job (per 10yrs. tenure).
Panel data Studies:			
Ruhm (1991)	US PSID	Mid 70's. Household heads age 21-65. Permanently displaced. Earnings losses 1yr. pre-separation♭.	16-18%, 4yrs. later△.
Jacobson et al. (1993)	Matched Pennsylvanian Admin. Data	1974-1986. Prime aged: ≥6yrs. *continuous* tenure by 1980. Earnings losses 5yrs pre-displacement♭. Involuntary displacements†.	*Penalty:* 40% immediately. 25% 6 yrs. later△. Largest losses in heavily unionised industries. Manufacturing sector leavers: 38%. Within Manufacturing sector: 20% if no 4-digit industry change; 18% if industry change.
Kunze (2002)	German Regional IAB	Aged 16-37, male and female, full time, skilled, highly attached. Excluding Civil Servants, Self-employed, unpaid & those ineligible for benefits.	*Previous Unemployment:* No penalty; *Previous OLF:* 1% LR penalty (males only)
Eliason and Storrie (2006)	Matched Swedish Admin. Data	1983-1999, Earnings losses 4 yrs. pre-displacement♭. Displaced 1986-1987, followed until 1999. Aged 21-50 in 1986. Excluding Self-employed & Construction sector. Involuntary displacements†.	*Earnings differential:* US$1,088 (1987), US$723 (1990), US$1,117 (1993, peak of recession), decreasing to 1990 levels 12 years later (1999). Worst for older displaced. NB. Control group: Employed in Nov. 1986.
Couch and Placzek (2010)	Matched Connecticut Admin. Data	1993-2004. Prime aged: ≥6yrs *continuous* tenure by 1999. Involuntary displacements†.	*Penalty:* 32%-33% immediately, 13%-15% 6yrs. later△. Largest earnings losses in highly paid service sector. Mitigated if don't change 4-digit industry.
Hijzen et al. (2010)	UK NESPD-IDBR-ARD	Aged 21 - 59; Earnings losses 8yrs. pre-displacement. Involuntary displacements†.	*Penalty:* 18%-35% firm closures; 14%-25% mass layoffs (to 5yrs later)△. Short-Run: Manufacturing (38.6%); Service (32.6%) sector. Long-Run: Both sectors: 24.1%, 5yrs later.
Longitudinal Studies:			
Arulampalam (2001)	BHPS	1991-1997, Cohort of males aged 16-58 in 1991. Controls for reason for leaving previous job.	*Unemployment incidence:* 6-14% 4 years+; *Incidence of Inactivity:* 8.6-13.6% up to 3 years; *NON-EMP incidence:* 6.4-10% 4 years+. *Unemployment duration:* Insignificant, **but** low quality indicator used from retrospective survey date questioning. Redundancy implies lowest penalty of ∀ separation types.

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Table 13 – continued from previous page

Paper	Data	Sample	Key Findings
Gregory and Jukes (2001)	NESPD-JUVOS	1984-1994, males aged 18-64. *Registered* unemployment spells. No info. on reason for leaving previous job.	*Unemployment incidence:* 10% - 2% 3 years+. *Unemployment duration:* 5% (6-months), 11% (12-months).

[♮] 5-year retrospective window. NB. DWS: Displaced Workers' Survey. Agri/Cons: Agricultural/Construction Sector.
[♭] Controls for unobserved heterogeneity ("Ashenfelter's dip", Ashenfelter, 1978). [†] Define 'involuntary displacement' window around firm closures/ mass layoffs (as lack exit reason). The wider the window, the more likely one is to pickup voluntary quits (Kunze, 2002). [△] Relative to *control group* of non-displaced. NB. PSID: Panel Study of Income Dynamics; NESPD: New Earnings Survey Panel Data; IDBR: Inter-Departmental Business Register; ARD: Annual Respondents Database.
NB. BHPS: British Household Panel Study; NESPD: New Earnings Survey Panel Data; JUVOS: Joint Unemployment and Vacancies Operating System.