Individual Unemployment & its Consequences:
A Regional Perspective

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Declaration of Authorship

The author hereby declares that the chapter 4 is an extended version of a joint paper co-authored with Ralf Wilke. The author hereby declares that he compiled the rest of this thesis independently, including the technical appendices to chapter 4, using only the listed resources and literature.

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Nottingham, October 15, 2012

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Abstract

Unemployment has a strong influence on the economic prospects of the UK economy as a whole. The effect of unemployment can be long-lasting, and as an experience can imply lasting effects on future employment outcomes. In order to avoid unemployment, individuals may decide take jobs they are overqualified for as a stepping stone to a better match when such positions become available. If over-qualification is a negative productivity signal, then this could reduce future career mobility. This thesis aimed to gain some insights into the impact of where individuals live, within the UK, on their unemployment and employment experiences. With that in mind, detailed data sets were constructed in order to answer the questions of interest. Moreover, flexible econometric techniques were employed.

Contents

Dedications

Dedicated to the memory of my mother and the dedication of my father.

Chapter 1

Introduction

1.1 Introduction

Unemployment is a key policy measure of the health of the economy. In an age of austerity, coupled with post recessionary pressures and the looming threat of a "double dip", high levels of unemployment are even more likely to be a public "litmus test" for the perceived operational efficiency of government policies. Efficient management of unemployment levels is a must in order to credibly manage public perceptions, both in the short- and long-term. High aggregate unemployment is generally considered a sign of low levels economic activity, provoking demand-side intervention with expansionary policies to stimulate aggregate demand and thus increase employment (*expansionary fiscal policies:* lowering tax rates or increasing government spending in order to boost aggregate demand (job creation), or *expansionary monetary policies:* lowering interest rates or expanding the money supply in order to increase the incentive to spend and thus boosting aggregate demand). As section 3.1 highlights, it is likely that expansionary fiscal policies are only complementary to targetted individual-level supply-side side interventions, Active Labour Market Policies (ALMP), aimed at 'reactivating' the workforce through education and training schemes[1]. Moreover, fiscal policy is unlikely to be able to target the re-employment prospects of the long-term unemployed.

Unemployment is persistent, in that unemployment today makes unemployment tomorrow more likely (Arulampalam *et al.* 2000). Furthermore, unemployment carries implications into future employment. Human capital depreciation, 'hysterisis' (as skills and re-employment prospects dwindle), and the stigma of being unemployed are likely to lead to lower wage offers, lower wage bargaining power and thus lower starting wages. Unless wage growth is significantly faster than growth of equivalent non-displaced workers, a permanent wage penalty will persist when compared to a situation where no unemployment had occurred.

Human capital depreciates whilst unemployed. Moreover, skill mismatch and over-qualification leads to further depreciation in jobs where human capital is underutilised. What is important, from a policy point of view, is whether this mismatch is temporary or of a permanent nature. Are mismatched jobs stepping stones to better matches?

1.2 The Economics of Unemployment

Types of unemployment Frictional unemployment, the focus of chapter 4, is a result of imperfect information leading to unemployment inflow/outflow imbalance, as it takes time to match workers and firms. This implies the simultaneous presence of unemployed persons and vacant jobs. On the demand-side, the rate of job creation and job destruction affects the level of frictional unemployment, which is affected by firms' hiring and firing costs . These costs

[1]Other supply-side policies include benefit or income tax reform in order to provide incentives to work, as well as trade union reform. Since minimum wages introduce wage rigidities, implying that the unemployed cannot lower their reservation wages in order to get into the job market, reforming minimum wage laws might help the least skilled that are priced out of the labour market. Labour market frictions could be decreased by introducing centralised vacancy posting where this did not formerly exist, e.g. Job Centre Plus.

are in turn affected by strong Employment Protection Legislation (EPL) laws (Marinescu 2009). On the supply-side a key driver of this type of unemployment is the reservation wage, which is generally determined by the generosity of unemployment benefits as well as eligibility constraints (Lancaster 1979). The reservation wage is a crucial concept as it interacts with other features of the welfare system, e.g. Working Families Tax Credit (UK), by changing marginal tax rates and thus the incentive to work an extra hour. This has direct impacts on the labour supply decision, which may not be captured in an unemployment duration study. Whilst the term "frictional unemployment" is fairly negative, unemployment can be a productive experience for some. Job search can be viewed as an investment in information, improving subsequent job matches (Lippman & McCall 1976). However, there is limited empirical evidence of this.

Other important types of unemployment is real wage/classic unemployment. This is caused when wages are above perfectly competitive market equilibrium levels, which prices some people out of the labour market. A common cited example is the effect of the minimum wage on labour demand. Demand-deficient unemployment usually results from fluctuations in aggregate demand, e.g. the business cycle. If wages are rigid, in that wages don't adjust downwards equally due to demand decreases, then this type of unemployment will occur. Unionised wage bargaining is an important example of a wage rigidity driver, and is one reason for large scale UK Public sector job cuts in 2011/2012.

Given the recent recession caused by the collapse of the Sub-Prime Mortgage industry, there is currently a vibrant debate in the US about whether the current problem of persistently high levels of unemployment and low economic growth are due to cyclical demand-deficiencies or due to structural misallocation of resources in the economy. Structural unemployment results due to a mismatch between skills demanded and supplied, and implies substantial costs to both occupational and regional mobility. Structural unemployment can also results from Technological Change and International Trade, which reallocate resources in the economy resulting in winners and losers. An often cited example is the decline of the UK's shipping, mining and manufacturing base. Given that these industries tended to be geographically concentrated, this drove persistent regional differences in unemployment outcomes. Structural unemployment may also arise from firms' profit maximising behaviour. Monitoring is costly. Firms have an incentive to pay above market (efficiency) wages in order to decrease the incentive to shirk on the job/"Moral hazard" (Shapiro & Stiglitz 1984). However, if all employers did this then supply would exceed demand, leading to structural unemployment[2].

Voluntary and involuntary unemployment result from layoffs and quits, each of which have different implications for future wage trajectories. However, seasonal unemployment is more predictable as it results from systematic, predictable fluctuations in labour demand. Since these fluctuations are known in advance, compensation for periods of unemployment is usually build into the remuneration structure.

Until recently, for decades the US has always had lower levels of unemployment than Europe. Machin & Manning (1999) attribute this to generous unemployment benefits as well as higher levels of long-term unemployment in Europe. Whilst ALMP are more pervasive and in Europe, they are almost non-existent in the US. Unemployment benefits are less generous in the US, not linked to past earnings, as well as being time limited. Although in Europe time-limited unemployment insurance coexists with means tested unemployment assurance, the latter implies a higher likelihood of exposure to "hystere-

[2]Since higher unemployment rates decrease the outside option and decrease the incentive to pay efficiency wages, this leads to a well established inverse relationship between the unemployment *rate* and the wage *level* across a country called the Wage Curve (Blanchflower & Oswald 1990; 1995).

sis" due to lower work incentives. However, the main difference is in the cost of job creation and job destruction. Strong EPL laws in Europe increase firing costs, making firms more selective about who they hire. This in turn leads to higher levels of unemployment (Borjas 2010.).

Measures of Unemployment A major difficulty in measuring the unemployment rate is how to define the unemployed. The two main measures of unemployment employed in the UK are:

International Labour Organisation (ILO) definition: This definition is employed in the Labour Force Survey (LFS). By this definition, an individual is unemployed if they have been actively searching for a job in the last 4 weeks and are ready to work in the next two.

Claimant Count: The first requirement is to be claiming unemployment benefits (Jobseekers' Allowance, JSA), which implies that this definition is more restrictive. However, an individual can still claim JSA if they are working up to 16 hours a week (which would disqualify them from the ILO-based series).

Whereas the UK government once calculated unemployment rates using the Claimant Count series, since JSA was introduced in 1996 the two series have diverged considerably (Machin & Manning 1999). Individuals can be ILO unemployed but not appear in the Claimant Count series, and *vice versa*. Moreover, the phenomenon of 'hidden unemployment' suggests that even the ILO series may not capture everyone.

Consequences of Unemployment Governments have an incentive to credibly manage the unemployment level due to the impact on public opinion as well as the long-run implication for the economy. Unemployment has both negative and positive consequences, however the former tend to far outweigh the latter and get wider public attention. Unemployment can lead to market failure as well as lower Economic Growth. Consumer confidence and spending can be reduced by higher unemployment levels, and Government revenues are likely to be reduced due to lower tax revenues and higher benefit expenditure. High levels of unemployment may induce a "Hysterisis Effect" as the skills and employability of the long-run unemployment deteriorate. It may also increase Social Deprivation and crime. From a more social point of view, unemployment has been shown to lower health levels, increase divorce rates and reduce life expectancy.

On a slightly more positive note, higher levels of unemployment can lead to lower and more stable inflation rates. They can also increase industry competitiveness by lowering the bargaining power of workers over wages. Moreover, higher levels of unemployment could imply an Environmental gain, due to lower levels of Economic Growth.

Due to the impact on reelection prospects, Government policy is more likely to be geared towards reducing unemployment rates. But, there is a tradeoff between low unemployment rates and high inflation. Price inflation will feed into wage inflation, increasing costs to employers and reducing their competitiveness. Thus maintaining a credible policy of low unemployment is difficult. Expectations need to be managed. However, short-term party politics can get in the way of the economy's long-term economic needs. Excess volatility of inflation between 1970 and 1990 affected consumer and investor confidence. Since the mid 1990s, Bank of England (BoE) independence has meant a separation of monetary and fiscal policy, with the BoE targetting inflation by setting the former and government using the latter. This has helped to curb inflation expectations as well as increasing confidence in the UK economy. Price stability also helps the UK achieve its long-term targets of high and sustainable levels of economic growth and employment.

1.3 Features of the UK unemployment benefits system

The UK welfare system is complex, and it is beyond the scope of this study to incorporate these inherent complexities into a unified framework. As with most studies investigating the operationalisation of the welfare system at the individual level, focus is limited to Job Seekers' Allowance claimants.

Job Seekers' Allowance (JSA) replace unemployment benefit & income support for unemployed people from October 1996. There are two main types of JSA:

1. Contributions-based JSA: Paid to those who have satisfied the national insurance (NIC) contributions criteria (referred to as Unemployment Insurance, UI).

2. Income-based JSA: Paid to claimants who satisfy a *family* income-based means test (referred to as Unemployment Assurance, UA)[3].

JSA tightened work search requirements, increasing monitoring restrictions relative to the previous system. To qualify individuals must (be):

- Under 60 years old and not receiving Income Support.

- Working *less than* 16 hours a week.

- Capable of starting work immediately.

[3]The complexity of the system means that it is possible to receive means-tested JSA with an income-based top-up.

- Actively taking steps to find a job, e.g. attending interviews, writing applications or seeking job information.
- Have a current "job seeker's agreement" with Jobcentre Plus, including information like: hours available for work; desired job; any steps the claimant is willing to take to find work.
- Prepared to work 40 hours a week.
- Cannot place too many restrictions on the type of work that they are willing to take. Refusal to take up a job offer without good reason may result in loss of benefit eligibility (the "Work Test").

In general UI is linked to the wage earned in the previous job, however in the case of the UK this is paid at a flat rate. Eligibility for UI receipt is restricted to 6 months from sign-on, however UA eligibility is not restricted in the same way. UA functions through the social security system and is unlimited in duration.

A major limitation of studies based on the UK Claimant Count is that they pool both UI and UA claimants. This ignores substantial compositional differences between these two groups, both in their work incentives and the interactions with other elements of the welfare system. For instance, UA claimants are more likely to be eligible for other benefits targetted at those on lower incomes. By employing a definition that restricts the data to UI recipients with adequate NIC contributions, we side step these issues in the first chapter. This definition of unemployment is based on work by Wilke (2009). This paper provides an important contribution to the unemployment duration literature, developing empirical bounds for the true unemployment rate using UK administrative data.

1.4 Statement of the research problem

This thesis sets out to answer the following questions. What drives individuals' unemployment experiences? The composition of individuals in a region, or regional characteristics that drive that composition? What impact do career interruptions have on individuals' earnings prospects? Do earnings recover, and is there a permanent "wage scar" carried into future employment (relative to the counterfactual, individuals whose wages were growing continually). Human capital depreciates whilst in unemployment. Moreover, human capital also depreciates if underutilised in jobs one is overqualified for. What impact does over-education have on the persistence of low-skilled employment? Are low-skilled jobs a stepping stone to better matches for the overqualified? How does this vary by local labour market characteristics and over the local business cycle?

1.5 Outline of thesis

The thesis is comprised of three main chapters and is outlined as follows. Chapter 2 details and justifies the main methodological approach adopted, as well as inherent limitations and ways in which this could be extended given the resources. Chapter 3, Sections 3.1, 3.2. and 3.3, cover the contributions to the literature directly relevant to the three main chapters. The first chapter of the thesis, chapter 4, draws on a novel data set linking individual Job Seekers' Allowance (JSA) claimants to the regions in which they reside in order to ask whether it is the composition of individuals in a region, or regional characteristics that drive that composition which determines observed unemployment durations. Flexible econometric techniques are employed. The key result being that regional characteristics were only found to impact on unemployment durations up to 150 days. In all cases, data set construction was a non-trivial task. Appendix chapters C and D describe how the regional data set used in the first chapter was constructed, as well as how the individual level was linked to the regional level.

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. However, economic theory does not provide clear cut predictions of the impact of unemployment on future earnings. The main hypothesis under test in Chapter 5 is that proposed and tested by van Dijk & Folmer (1999) with cross-sectional data for the Netherlands: Unemployment experienced in high unemployment regions is seen as more of a characteristic of the region in which that unemployment was experienced, and less of a negative productivity signal. What implications does this have for the UK and for Wage Scarring? This chapter finds robust long-run evidence is found supporting the van Dijk & Folmer (1999) hypothesis, on average and for over 45s made redundant in their previous jobs. Moreover, important sources of regional variation in Wage Scarring are found. Being made redundant and then experiencing unemployment in areas of high economic activity is equally damaging for future earnings potential, independent of age. This chapter draws on Continuous Work-life Histories, the construction of which is detailed and justified in the Appendix, Section E.

The Overeducation literature is motivated by the observation of an increasing proportion of highly skilled workings in jobs that were once low-skilled (Borghans & de Grip 2000). Chapter 6 asks whether low-skilled jobs are stepping stones to better matches for the overqualified, how this varies by local labour market characteristics and over the business cycle. Much of the existing literature assumes that over-qualification has the same impact, independent of the skill-composition of tasks performed on the job. Is over-qualification is worse for those in skilled or low-skilled employment? A dynamic discrete-choice Multinomial Logit model (MNL) is employed. This chapter finds robust evidence to suggest that over-qualification is more damaging for career mobility if experienced in low-skilled employment. Low-skilled employment is more of a Stepping Stone to skilled employment for females than males, independent of over-qualification. However, conditional on being overqualified, only women in low-skilled employment are more upwardly mobile than men. Important variation is evident, both in terms of previous industry and firm characteristics. The effect of being over-qualified is not invariant to the business cycle. Moreover, when contrasting results from a 1988 and 2008 based classification of occupational skill, estimates suggest that upward career mobility may have increased (decreased) between 1988 and 2008 for overqualified females (males) in general.

Finally, chapter 7 concludes, suggesting how the chapters in this thesis could be extended in the future.

Chapter 2

Methodology

This section introduces the theoretical framework as well as the econometric methods used to assess the re-employment prospects, the costs of unemployment and the implications of over-qualification for career mobility in the subsequent thesis chapters.

2.1 Theoretical Motivation

2.1.1 Chapter 4: Modelling Unemployment: Search & Matching

Job search theory describes the process in which individuals match with vacancies in the labour market. Seminal jobsearch papers include McCall (1970) and Mortensen (1970).

The Job Search Model The standard job search model assumes that the distribution of wages offers is exogenous/ determined outside the system (Atkinson & Micklewright 1991). An unemployed worker's strategy can be described by a reservation wage, above which wage offers result in job acceptance. Reservation wages increase with benefit levels, resulting in the prediction that increases in unemployment benefit generosity lead to lower employment transitions for the unemployed.

The standard model makes various restrictive assumptions. An individual is concerned with the present value of their future income stream (over an infinite horizon), discounted at a constant rate, δ. Once accepted, jobs are assumed to last forever, at a constant wage w. Job offers are assumed to arrive at a constant rate, α, per unit of time (which implies that the probability of a job's wage offer, w, exceeding the reservation wage, w^*, is $1 - F(w)$ at each point in time (where $F(.)$ is the cumulative distribution function of all possible wage offers, assumed known). Moreover, the job search intensity is assumed fixed. Past job offers cannot be recalled. When out of work, an individual has a value of leisure v. Furthermore, the level of unemployment compensation, b, is assumed constant over time (Atkinson & Micklewright 1991). Under these assumptions, there is a stationary reservation wage, w^*, which must satisfy:

$$w^* - (b + v) = \alpha(1 - F(w^*))[w^{**} - w^*]/\delta \tag{2.1}$$

where w^* is the expected wage, conditional on $w \geq w^*$. Choice of reservation wage boils down to a balance between increasing income by accepting w^* today, versus the expected improvement over w^* as a result of waiting for a better offer. In the limit ($n \to \infty$), the hazard rate, or the exit rate from unemployment, is $\alpha(1 - F(w^*))$. Thus the average duration of unemployment is $T_u = \frac{1}{\alpha(1-F(w^*))}$ which is an increasing function of the reservation wage w^*.

$$\begin{aligned} w^* =& f(z, \alpha, r, q, w) \\ =& x - z - \frac{\lambda}{r+q}\int_x^{+\infty}(w-x)dH(w) \end{aligned} \tag{2.2}$$

(Cahuc & Zylberberg 2004)

where z is the unemployment benefit rate, α is the wage offer arrival rate, r is the rate of interest, q the job loss rate, and w the real wage. Average duration of unemployment, T_u, is an increasing function of the benefit rate. However, it is a decreasing function of the interest rate and the job loss rate. A higher interest rate implies that future values are discounted more, lowering the reservation rate and the average time looking for work. A higher job loss rate implies lower demands of job seekers (decreased "pickiness"), thus decreasing average unemployment durations. A higher arrival rate of job offers implies an ambiguous effect on average unemployment durations. It may increase the reservation wage, leading to a decrease in the probability of accepting wage offers, thus the overall effect is an empirical question (Cameron & Trivedi 2005).

The first issue that could be raised about the theoretical model is that reservation wages are unobservable[1]. Moreover the overall distribution of wages is unknown. We only observe wages that exceeded reservation wages. These limitations have meant that the literature has adopted a reduced form approach, focussing on the probability distribution driving unemployment durations. In this context, the hazard function can be represented as $\phi(t).dt = \frac{f(t)}{S(t)}$ where $S(t) = 1 - F(t)$ (see Methodology for more information, Section 2). $\phi(t) > 0$ implies positive duration dependence (re-employment probabilities increase as unemployment duration increases), whilst $\phi(t) < 0$ implies negative duration dependence (re-employment probabilities decrease as unemployment duration increases).

The assumption of an unlimited benefit duration is inappropriate, as in reality the time to expiry of benefit entitlement (eligibility constraints) matter. In reality, Unemployment Insurance (UI) is paid for a fixed period, t. The reservation wage of UI recipients falls with length of unemployment spell until

[1]Where reservation wages are observable, these are almost invariantly self-reported and subject to serious measurement error.

time t (Mortensen 1977, pp. 511). If this is the case then post UI benefit exhaustion, the level of benefits will have no impact on re-employment probability.

Studies evaluating the effects of unemployment benefits are faced with numerous challenges, including poor data, the complexity of the system (confounding factors) and lack of true exogenous variation. In Germany, as in many other European countries, unemployment benefits are tied to previous earnings. For very low-income earners, income replacement rates (the rate at which unemployment benefits replace former income) can be up to 100%. This effectively eliminates any work incentive. Recent Hartz-IV reforms have aimed to reduce this issue, however means testing still implies this issue affects a sub-section of society. Even though in the UK UI is not tied to previous earnings, the effect of UI benefits on labour supply decisions is. Moreover, work incentives are likely to be impacted by other features of the welfare system, e.g. housing benefits, child benefits and working family tax credits (WFTC). Many studies find evidence of negative duration dependence (re-employment probabilities decrease as unemployment durations increase). However, Meyer (1995; 1996) draws on data from a natural experiment in the US, finding evidence of positive duration dependence during benefit eligibility: individuals are more likely to exit unemployment as unemployment duration increases. Evidence of both negative and positive duration dependence has also been found in Europe. So conclusions are not always clear cut as one may think.

There are definite problems regarding the assumptions of the basic model when applied to real world Unemployment benefit systems. Centralised vacancy posting, e.g. Job Centre Plus, lower the costs of job search by decreasing the level of imperfect information in the market. Alternative formal and informal search methods also exist on both sides of the market, and in some occupations may account for the bulk of recruitment. Job seekers may exploit alternative job advertisement channels such as local newspapers. Moreover, employers may recruit internally (internal labour markets) before looking to specialist recruitment agencies and centralised vacancy postings for prospective talent. In 1996, the arrival of JSA introduced stronger monitoring restrictions and eligibility constraints for job seekers. Monitoring restrictions & eligibility constraints affect job search intensity, resulting in a spike in exit rates just before benefit exhaustion (Meyer 1990). This suggests that parametric specifications of the reduced form hazard rate are likely to be inappropriate for capturing such non-linearities. Mortensen (1977) also suggests an indirect effect of the prospect of future eligibility, resulting in the increased attractiveness of employment as a substitute for uninsured unemployment. Moreover, there are generally restrictions on job offer acceptance, e.g. the "Work Test" in the UK.

This prediction, that increases in unemployment benefit generosity lead to lower employment transitions for the unemployed, is sensitive to personal & demographic characteristics, elapsed unemployment duration and the prevailing labour market characteristics. Moreover, there is a possible role for individual heterogeneity in driving unemployment outcomes. Distinguishing between true versus spurious state dependence is an important challenge. Genuine state dependence can explain the presence of negative duration dependence in the data, however unobserved heterogeneity will bias estimates towards spurious negative duration dependence. True negative duration dependence suggests that if we randomly select two people, we would expect the individual with the shorter unemployment duration to leave unemployment more quickly (Machin & Manning 1999). However, there is another possible explanation for this phenomenon. Individuals with higher re-employment probabilities will leave the sample first, resulting in the remaining pool of unemployed being dominated by jobseekers with low re-employment prospects (Machin & Manning 1999).

True and spurious state dependence carry different policy implications, and distinguishing between the two is key for Government policy. True state dependence suggests that policies should target the re-employment prospects of all jobseekers, whereas spurious state dependence suggests intervention to target those with the lowest re-employment prospects (Collier 2005). However, it is difficult to disentangle the two phenomena:

> It does not really seem possible to identify separately the effect of heterogeneity from that of duration dependence without making some strong functional form assumptions which have no foundation in economic theory (Machin & Manning 1999, pp. 3111).

The standard job search model has been extended to incorporate alternative destination states, such as inactivity, on-the-job search, and to endogenise firm behaviour, equilibrium jobsearch models for a detailed exposition see Rogerson *et al.* (2005) and/or Eckstein & van den Berg (2007).

The Matching Model So far we have concentrate on partial equilibrium job search theory. The matching model is an equilibrium model, which summarises, at the aggregate level, the process in which firms with vacancies and jobseekers meet. The matching model is a macroeconomic model with microeconomic foundations. Unlike classical macroeconomic models, this approach adds more institutional realism by incorporating labour market frictions.

Various formulations of the matching process exist. The "Balls and Urns" model assumes that job matches are random, with no possibility of strategic jobsearch behaviour (Pissarides 1979). Ranking models explicitly model firm behaviour, under the assumption that job applicants are ranked in terms of unemployment duration with those with the lowest durations having being the most attractive (Blanchard & Diamond 1994). Stock-flow matching models explicitly model heterogeneity between stocks and flows of jobseekers and vacancies, suggesting that what matters for the re-employment prospects of the long-term unemployed is the flow of new vacancies and not the stock of existing vacancies (Coles & Smith 1998). However, alternative mechanisms could explain some empirical observations. Blanchard & Diamond (1994) explain negative duration dependence as a demand-side phenomenon, however, this could equally be explained by lower levels of job search intensity as unemployment duration increases (Petrongolo & Pissarides 2001)[2].

Standard approaches assume job search intensity is constant. Moreover, the "Balls and Urns" model is a constant returns to scale matching function. However, the validity of this assumption has been called into question in the literature (Petrongolo 2001) when investigating matching across UK regional Jobcentres. This also ignores on-the-job search by assuming that only the unemployed are jobseekers.The empirical matching literature finds that incidence of unemployment, spatial allocation of vacancies and job seekers, as well as the demographical characteristics of the labour force matter in the matching process. However unemployment benefits are no found to be as important a determinant (Cahuc & Zylberberg 2004). As noted by Petrongolo & Pissarides (2001), the unemployment benefits indicator is likely to suffer from substantial measurement error due to data limitations and the difficulty of accurately capturing the complexity of the welfare system. However, these general findings are also consistent with the result that incidence rather than duration of unemployment matters most for employment outcomes (Kalwij 2004).

The matching process can be summarised by the ratio of vacant jobs to

[2] A recent contribution to the theoretical matching literature is that of "island matching" (see Mortensen 2009)

unemployed (job seekers) in the labour force.

$$\theta = \frac{V}{U} \tag{2.3}$$

(Cahuc & Zylberberg 2004)

Where V represents the stock of vacancies and U the stock of unemployed. θ is an indicator of "labour market tightness". Analysing the general implications of a change in θ is not so straightforward as firms and job seekers are affected differently. Firms will find it harder to fill vacancies the higher the number of vacancies for a given level of unemployed job seekers. However, job seekers will find that this same scenario increases their chances of finding a suitable job match. Chapter 4 focusses on the supply-side, unemployment duration, with job seekers' unemployment experiences being of direct interest.

2.1.2 Chapter 6: Human Capital Theory, The Mincerian Earnings Function & its Estimation.

The study of the distribution of earnings in the economy stretches back to Classical Economists including Adam Smith who considered the distribution of wealth between capital and labour (Polachek 2007). Later interest changed from the notion of homogenous labour inputs, to the distribution of income across heterogenous labour types. Earlier macroeconomic enquiries into the distribution of earnings across workers include Leontief 1946 and Schultz 1967. The Mincerian earnings function formalises the relationship between life cycle earnings and human capital accumulation with important policy and welfare implications (Mincer 1958; Mincer & Polachek 1974).

Mincer treats schooling and occupation as investments and that individuals invest up to a point where investment cost is just equal to present value of schooling gains, i.e. the point at which one is indifferent between the cost of investing more and the increased lifetime earnings. This was motivated by the empirical evidence of a non-uniform distribution of income across the population. Becker (1962) later extended this work, formalising the theory of Human Capital.

The basic Mincerian earnings function can be represented in its short-hand form:

$$\begin{aligned} \ln Y_i(t) = & a_0 + a_1 SCHOOLING_i + a_2 EXPERIENCE_i \\ & + a_3 EXPERIENCE_i^2 + \varepsilon_i \end{aligned} \tag{2.4}$$

where a_1 and a_2 are rates of return to schooling and post-schooling human capital investment (Mincer 1958)

In light of Human Capital theory's predictions about general and specific training, equation 2.4 has been augmented to include a quadratic in job tenure (where post-schooling human capital investment is divided into general - experience- and specific -job tenure- human capital):

$$\begin{aligned} \ln Y_i(t) = & a_0 + a_1 SCHOOLING_{+} a_2 EXPERIENCE_i \\ & + a_3 EXPERIENCE_i^2 + a_4 TENURE_i \\ & + a_5 TENURE_i^2 + \varepsilon_i \end{aligned} \tag{2.5}$$

(Mincer & Polachek 1974)

2.2 Econometric Methodology

2.2.1 Chapter 4: Unemployment Duration Models

The decision of how to model the transition process should be directed by the nature of the underlying data (Jenkins 2004). The Joint Unemployment & Vacancies Operating System (JUVOS) contains data on the number of days an individual was claiming Jobseekers' Allowance. Individual-level characteristics refer to the beginning of the spell in question. These characteristics are matched to regional-level information relevant to the month in which the claimant spell began. The data is arranged into single spell format, in order to aid comparison to the Censored Quantile Regression (CQR) estimator which cannot take into account time varying covariates (see Chapter 4, Section 4.4 for a motivation of the use of CQR for duration analysis). Since in chapter 4 the dependent variable retains its daily survival time structure, it seemed appropriate to pursue a continuous-time estimation strategy in this case. In the context of duration models, reformatting the data into a discrete-time format would imply making further assumptions about the nature of transitions at the boundary of each time interval and thus is not pursued in this chapter. The following describes methodological considerations.

Continuous-time Duration Models

If we let unemployment duration equal a nonnegative random variable, T, then the Cumulative Density Function (CDF) $F(t) = Pr(T \geq t) = \int_0^t f(s).ds$ where $f(s) = dF(t)/dt = F'(s)$ is the Probability Density Function (PDF), the instantaneous probability that unemployment duration equals t, Pr(T = t). Following the methodology in Cameron & Trivedi (2005), the Survivor Function, S(t) represents the probability that an unemployment spell last until time t: $S(t) = 1 - F(t) = Pr(T > t)$. The probability that a spell of length t ends in the next time period $t + \Delta t$, given that it has lasted up until time t, is $Pr(t \leq T < t + \Delta t \mid T \geq t)$. Thus the hazard rate, $\lambda(t)$, is defined as:

$$\lambda(t) = \frac{lim}{\Delta t \to \infty} . \frac{Pr(t \leq T < t + \Delta t \mid T \geq t)}{\Delta t} = \frac{f(t)}{1 - F(t)} = \frac{f(t)}{S(t)} \tag{2.6}$$

The hazard rate, $\lambda(t)$, is the instantaneous probability of leaving a state, conditional on survival until time t. Equation 2.6 highlights that the hazard rate is the ratio of the distribution function and the survival function of duration times. The hazard rate can be shown to be the change in the log-survivor function.

$$\begin{aligned}\lambda(t) &= \frac{1}{S(t)} . \frac{dF(t)}{dt} = \frac{1}{S(t)} . \frac{d(1 - S(t))}{dt} = \frac{1}{S(t)} . (-\frac{d(S(t))}{dt}) = -\frac{1}{S(t)} . \frac{dS(t)}{dt} \\ &= -\frac{d \ln(S(t))}{dt}\end{aligned} \tag{2.7}$$

The integrated hazard function, Λt, has computational advantages over the hazard, $\lambda(t)$. This function can be determined by summing all hazards until period t.

$$\Lambda(t) = \int_0^t \lambda(v).dv = \ln S(t) \tag{2.8}$$

Taking exponents, the survivor function can be represented:

$$S(t) = \exp(-\Lambda(t)) \tag{2.9}$$

Typically, an assumption is made about the form of the distribution function

$\Lambda(t)$, then its parameters are estimated. Common parametric functions used to model the hazard rate include the Exponential distribution, $\Lambda(t) = \gamma, \gamma > 0$. In this case the hazard is constant and independent of time. The Exponential survivor function is $S(t) = \exp(-\gamma.t)$. The Weibull distribution function is represented $\lambda(t) = \gamma.\alpha.t^{\alpha-1}, \gamma, \alpha > 0$. Unlike the exponential case, this hazard function depends on time. The Weibull survivor function is thus $S(t) = \exp(-\gamma.t^{\alpha})$.

Non-linearities cannot be accounted for using the exponential and Weibull approaches. The Log-Normal, Log-Logistic, and Generalised Gamma distributions are more flexible and allow for these non-linearities to be explicitly modelled, albeit under some assumptions. The main issue when estimating parametric models is misspecification. Consistency of parameter estimates requires a correctly specified model. The general approach is to adopt a non-parametric Kaplan-Meier approach to assess the distribution of duration dependence before deciding on the parametric form of this. This amounts to imposing a non-testable assumption for statistical convenience, as economic theory generally does not have anything to say about the exact shape of duration dependence.

Censoring & Estimation

Censoring is a major challenge when dealing with time to event data, requiring special methods which explicitly take into account this censoring when formulating the likelihood function. The Joint Unemployment & Vacancies Operating System (JUVOS) data used in this study contains an *inflow* of registered unemployment spells (claimants), up until a fixed point (end of sample window). This introduces the problem of right censoring, as some of the spells in the data are incompletely observed. Given n observations, $t_1, t_2, ..., t_n$, some of which are censored, the indicator function $\delta_i = 1[T_i^* < C_i^*]$, equals one if a complete spell is observed (no censoring) and zero if a spell is right censored. $T_i = min(T_i^*, C_i^*)$ is observed in the data. Estimation is carried out via Censored Maximum Likelihood. The common approach is to assume independent/random censoring, implying that the censoring mechanism δ_i is exogenous and can be ignored. Under independent censoring a closed form solution to the likelihood function exists.

The log-likelihood function is the logarithmic product of the joint density function of complete and incomplete spells, and can be represented as the weighted sum of the hazard and integrated hazard functions. As long as this density function is correctly specified, which relies on the hazard function being correctly specified, then the Maximum Likelihood Estimates achieve the Cramer-Rao lower-bound (asymptotic consistency). Misspecification implies a lack of robustness. The underlying identification assumption is that the same process is driving the observed and unobserved observations.

Sample attrition in survey data is an example of potentially non-random (endogenous) censoring, however this assumption is maintained given its widespread application, lack of identification results, and the lack of a Censored Quantile Regression estimator consistent under random censoring (Koenker 2008).

To be able to assess how particular variables impact on duration levels, as is convention in the Labour Economics literature, a Proportional Hazard approach is adopted. This breaks the conditional hazard rate into two components, a baseline hazard $\lambda_0(t, \alpha)$ (a function of time) and a vector of estimates, $\phi(x, \beta) = \exp(x'\beta)$ which is a function of the included covariates.

$$\Lambda(t) = \lambda_0(t, \alpha).\phi(x, \beta) \tag{2.10}$$

The class of Proportional Hazard (PH) models includes the Exponential and Weibull approaches. These hazard functions are proportional to the baseline hazard and covariates (x) increase or decrease the hazard function by a constant proportion relative to the baseline hazard (Cameron & Trivedi 2005). Another PH example includes the semi-parametric Piecewise-Constant Hazard, which makes very few assumptions about the distribution of duration times. This divides the exponential function into k segments within which the hazard of exiting unemployment is constant. $\lambda_0(t, \alpha) = \exp(\alpha_j), c_{j-1} \leq t < c, j = 1, \ldots, k$. The parameters $\alpha_1, \ldots, \alpha_k$ are estimated to uncover the baseline hazard. Once the baseline probability of exiting unemployment in any period is estimated, the impact of covariates would be to shift baselines up or down proportionally.

Unobserved Heterogeneity

As is the case for OLS, measurement error and omitted variable bias (OVB) are important in survival analysis. Lack of control for unobserved ability bias may lead to incorrect inferences. Moreover, measurement errors in survival times (most likely due to time interval aggregation) and in regressors will also impart important biases on results if not controlled for.
Consequences of ignoring unobserved heterogeneity include:

- Over-estimating negative duration dependence.
- The effect of a change in a variable on the hazard rate is no longer constant, but declines over time. The proportional hazards assumption imposes the restriction that covariates impact on the hazard rate by shifting it up or down at a constant rate (Cameron & Trivedi 2005).
- Under-estimating the effect of a change in a covariate on the re-employment hazard.

(Jenkins 2004)

A full-flexible non-parametric baseline hazard lends credence to the Cox model. However, penalized likelihood estimation restricts the researcher to controlling for shared frailty in the Cox PH model. Research looking at single spell data suggests that failing to take unobserved heterogeneity into account does not seriously bias results given a fully baseline hazard specification (see Meyer 1990, Narendranathan & Stewart 1993, Cameron & Trivedi 2005). Duration analysis techniques are restricted in the way the unobserved heterogeneity can enter the specification: "[A]llowing for a random disturbance term in each of the cause-specific hazards requires an additional assumption that imposes the independence of these disturbance terms across the cause-specific hazards (Petrongolo 2001, pp.728)." Unobserved heterogeneity can be incorporated into most continuous-time models using standard techniques in Stata. However more flexibility is available in the discrete-time setting. This can be done by modelling the destination-specific intercepts as normally or discrete mass point distributed using techniques such as HSHAZ and PGMHAZ8 in Stata (Jenkins 2004), GLLAMM (Rabe-Hesketh & Skrondal 2008) or the Halton draws based procedure whilst imposing normality of the residuals (Haan & Uhlendorff 2006). Whilst flexible, the discrete-time approach requires imposing assumptions about the nature of transitions at the boundaries of time intervals. Moreover, the choice of how to define an interval can be arbitrary.

Competing Risks, Time-Varying Covariates & Multiple Spells

It is common for studies using survival analysis techniques to treat transitions to inactivity as right censored or to drop them completely. This assumption implies that the partial likelihood can be considered the sum of the destination-specific hazards, without having to explicitly model the censoring mechanism as one would in a correlated risks setting (Cameron & Trivedi 2005). In the duration context, treating inactivity as a *censored destination state*, may lead to inconsistent estimates of the parameters determining the transitions of interest as this assumes away unobserved characteristics affecting both transitions of interest and those to censored states (van den Berg & van Ours 1994; van den Berg & Lindeboom 1998). Conditional on the validity of the independent risks assumption, the empirical strategy pursued in Chapter4 is consistent and takes into account the competing risks structure of the JUVOS. However, as far as I am aware to date identification has only been proved in the competing risks duration setting under independent risks. The independence assumption imposes restrictions on the nature of the error structure between competing risks. IF this restriction does not hold then relaxing it is likely to have a large impact on estimates. The multiple spell nature of the JUVOS could be exploited for identification purposes (Abbring & Berg 2000; Van den Berg 2001). The advantage of controlling for multiple spells is that identification in multiple spell context achieved under much weaker assumptions than in the single spell case (Honore 1993). Omitting time-varying covariates leads to an OVB problem, especially if these variables are correlated over time. Time-varying covariates can be incorporated using "episode splitting", aiding identification of individual- and regional-level effects, however this can lead to very large data sets (Jenkins 2004).

2.2.2 Chapter 5: Econometric Issues of Estimating the Mincerian Earnings Function.

The estimation strategy pursued in Chapter 5 is informed by lessons drawn from the literature estimating the Mincerian earnings function.

Omitted Variable Bias

The original Mincerian earnings function excluded controls for experience, estimating $\ln y_t = a_0 + a_1 S_t + \varepsilon_t$ (Mincer 1958). This produced an estimate of the return to an extra year of schooling of 7%, in a regression with an R^2 of 7%. Mincer (1974) augmented the original regression with a quadratic in experience, increasing the estimated rate of return to schooling to roughly 11%, *ceteris paribus*, and the R^2 to 29% (Cahuc & Zylberberg 2004). This highlights the important impact of Omitted Variable Bias (OVB). By omitting post-schooling experience, human capital accumulated after full-time education will be captured by the error term, thus imparting bias on regression estimates. Since schooling and experience are negatively correlated, excluding experience from the regression will mean that the returns to schooling are underestimated (Card 1999). OVB biased estimates and decreases the overall performance of the model.

To illustrate the problem, one can draw on the standard OLS estimator in matrix notation.

$$
\begin{aligned}
Y =& X'\beta + \underbrace{z'\alpha} + u \\
\hat{\beta_{OLS}} =& \beta + \underbrace{(N^{-1}X'X)^{-1}(N^{-1}X'z)}_{\delta}\alpha + (N^{-1}X'X)^{-1}(N^{-1}X'u)
\end{aligned}
$$

If $\quad Cov(X_k, z) = 0 \quad$ then OLS is consistent as $\quad \delta \to 0$

If $\quad Cov(X_k, z) \neq 0 \quad then:$

$$\hat{\beta} = \beta + \delta\alpha \tag{2.11}$$

(Cameron & Trivedi 2005)

Specification 2.11 represents the Omitted Variable Bias (OVB) formula. The term δ determines the direction of the bias due to the omitted variable under consideration, which depends on the correlation $Cov(X_k, z)$.

If not adequately controlled for, time out of the labour force can lead to an overestimate of the returns to experience, furthermore, aggregate economic conditions, like local unemployment rates and local labour market tightness, have been shown to impact on returns to schooling (Polachek 2007). Union membership (Kuhn & Sweetman 1998), race, gender[3], marital status and ethnicity are other examples of potential 'exogenous'[4] confounders. Crucially, measurement error in variables will also impart a form of OVB on estimates.

Unobserved Heterogeneity

Selection on unobservables is caused by endogeneity which cannot be controlled for due to lack of data, or poorly measured proxies. To illustrate this point,

[3]Investigating the labour supply decisions of women is only slightly complicated by the fact that womens' labour supply function tends to be discontinuous (Polachek 2007).

[4]"Fixed at the time the regressor of interest was determined (Angrist & Pischke 2009, pp. 64)".

schooling is considered to be endogenous to ability in an earnings regression as investing in higher levels of schooling is less costly for individuals with higher levels of ability (Spence 1973). Instrumental Variables (IV) and twin studies are potential options for dealing with this issue *in the context of cross-section data*, however they are not without their limitations. The main difficulty when implementing IV is finding valid instruments. A valid instrument for schooling would be correlated with schooling but not directly with earnings (Polachek 2007). Difference-in-difference approaches, e.g. Addison & Portugal (1987), have been implemented in the cross-section context, however in the absence of an adequate control group strong identifying assumptions imply that these estimates should be interpreted with caution. Furthermore, if earnings drop before displacement ("Ashenfelter's dip," Ashenfelter 1978) then estimates will be sensitive to the time period chosen. Even if a control group is available, some identifying assumptions may be invalidated, e.g. the common trends assumption.

Given the availability of panel data, the fixed-effects (within-groups) estimator can be used to control for time-invariant sources of endogeneity, e.g. the time-invariant component of ability bias. The within-groups estimator, a generalisation of the fixed-effects estimator (Arulampalam 2001), is attractive in the context of an earnings regression as whilst it does require at least two observations per individual, it does not require these observations to be consecutive as the first-differences estimator does.

To demonstrate the operation of the within-groups estimator, suppose we had the following regression:

$$Y_{it} = \alpha_i + X'_{it}\beta + \varepsilon_{it} \tag{2.12}$$

In equation 2.12, X_{it} represents a matrix of regressors, α_i a random individual-specific effect, and ε_{it} a random error term. Allowing time-invariant α_i to be correlated with X_{it}, the error term can be written as $u_{it} = \alpha_i + \varepsilon_{it}$. Thus u_{it} contains a time-invariant and time-varying component. The fixed-effect α_i can be eliminated be the within-groups estimator through de-meaning. By construction ε_{it} is uncorrelated with the explanatory variables, X_{it}.

$$(Y_{it} - \overline{Y_i}) = (X_{it} - \overline{X_i})'\beta + (\varepsilon_{it} - \overline{\varepsilon_i}) \tag{2.13}$$

Where:

$$\overline{Y_i} = T_I^{-1} \sum_{t=1}^{T_i} Y_{it}; \quad \overline{X_i} = T_I^{-1} \sum_{t=1}^{T_i} X_{it}; \quad \text{etc...}$$

OLS on equation 2.13 yields consistent estimates of β, the standard errors of which must be suitably adjusted for loss of degrees of freedom.

The drawback of this approach is the inability to estimate the effect of time-invariant regressors. Furthermore, if there is little variation in a regressor of interest, its fixed-effects estimates will no be precisely measured (Cameron & Trivedi 2005). Fixed effects estimates are susceptible to attenuation bias due to measurement error, whilst measurement error and ability bias work in different directions in terms of their impact on estimated returns to schooling. 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 & Pischke

2009, pp. 335). 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 & Krueger 1999).

Whilst fixed-effects may eliminate time-invariant sources of endogeneity, this estimator is based on the identifying assumption of strict exogeneity of the explanatory variables conditional on α_i: $E(u_{it}|X_i, \alpha_i) = 0 \quad \forall t$, where α_i is the unobserved individual effect (Cameron & Trivedi 2005). This implies that OLS estimates of 2.13 will still yield inconsistent estimates of β if this identifying assumption is not satisfied. Sample selection bias is another important sources of endogeneity in this context.

In the context of this study, the dependent variable, the real wage, is likely to be impacted by measurement error. Usual hours worked is likely to be quite stable over time in full time jobs, as individuals develop regular work patterns.

Sample Selection

When a random sub-sample of the population of the population of interest is not available, one needs to worry about the representativeness of the data at hand. Since earnings are only observed for those that work this implies that the sample of wage earners is not a random sub-sample of the working population as whether one works is determined by the participation decision. This phenomenon is commonly termed incidental truncation. Survey design, sample attrition and survey non-response are other examples of reasons why the the data may not be representative.

Gronau (1974) provided an early contribution to the issue of incidental truncation, in the context of labour supply. The problem can be laid out as follows. Suppose we want to estimate $E(W_i^0|x_i)$, where W_i^0 represents the hourly wage offer of individual i. Since W_i^0 is only observed for those that work, the participation decision can be represented as the point on a labour supply schedule where an individual is indifferent between working and not working. The weekly labour supply model can be represented as a utility maximisation problem: $max_h \quad util_i(W_i^0 h + a_i, h)$ subject to $0 \leq h \geq 168(24 * 7)$, where h represents the hours worked per week and a_i the non-wage income of individual i (Wooldridge 2001). Individual i will be indifferent between working and not working at the point where the marginal utility of income equals the marginal disutility of working. Gronau (1974) refers to this as the reservation wage (W_i^r). Thus we will only observe a positive wage for individuals if they received a wage offer that is bigger than their reservation wage ($W_i^0 \geq W_i^r$).

Under certain parametric assumptions[5], it can be shown that the W_i^0 will only be observed when:

$$\ln W_i^0 - \ln W_i^r = X_{i1}\beta_2 - X_{i2}\beta_2 - \gamma_2 a_i + u_{i1} - u_{i2} = X_i\delta_2 + v_{i2} \quad > 0 \quad (2.14)$$

Dropping i subscripts, letting $Y_1 = \ln W^0$, and Y_2 the participation indicator function, gives:

$$Y_1 = X_1\beta_1 + u_1 \qquad Y_2 = 1[X\delta_2 + v_2 > 0] \quad (2.15)$$

The sample selection model is estimable under the following assumptions: exogeneity of X, $v_2 \sim N(0,1)$ and $E(u_1|v_2) = \gamma_1 v_2 + \varepsilon \quad$ where $\quad \varepsilon \sim I.I.D.(0, \sigma^2)$,

[5] That (W_i^0) and (W_i^r) are exponentially distributed.

i.e. The error in the second stage is a linear function (γ_1) of the error in the first stage, plus some noise. Under these assumptions,

$$E(Y_1|X, Y_2 = 1) = X_1\beta_1 + \gamma_1 E(v_2|v_2 > -X\delta_2) = X_1\beta_1 + \underbrace{\gamma_1\lambda(X\delta_2)}_{*} \quad (2.16)$$

Equation 2.16 introduces the inverse Mills ratio $\lambda(X\delta_2) = \phi(X\delta_2)/\Phi(X\delta_2)$. If sample selection is not a problem then $\gamma_1 = 0$, u_1 and v_2 are uncorrelated, and OLS will produce consistent results since equation 2.16 reduces to $E(Y_1|X, Y_2 = 1) = X_1\beta_1$. However, if $\gamma_1 \neq 1$ then OLS of Y_1 on X_1, omitting * will lead to inconsistent estimates of β_1 in the same manner as OVB (Heckman 1979).

Heckman (1979) proposed a two-step procedure to correct for sample selection bias. The first stage participation decision is estimated as the Probit $P(Y_{i2} = 1|X_i) = \Phi(X_i\delta_2)$ on the all available observations including non-participants in the labour force. The $\hat{\delta_2}$ estimates are then used to obtain the inverse Mills ratio $\hat{\lambda_{i2}} \equiv \lambda(X_i\hat{\delta_2}) \quad \forall i$. In the second stage, equation 2.16 can be consistently estimated using $\hat{\lambda_i}2$ estimated from the first stage. The resulting estimates are $\sqrt{N}$-asymptotically normal (Wooldridge 2001).

A t-test for the null hypothesis $H_0 : \gamma_1 = 0$ serves as a test for the presence of sample selection. Identification is secured by exclusion restrictions in the first stage participation decision. Valid exclusion restrictions are factors which impact on the participation decision but not on the wage offer (essentially instruments in the IV sense). Finding valid exclusion restrictions is a major challenge for most studies adopting this technique. In the absence of exclusion restrictions, β_1 is only identified due to nonlinearity in the inverse Mills ratio (Puhani 2000). However, the two-step Limited Information Maximum

Likelihood (LIML) procedure imposes less distributional assumptions than the bivariate sample selection model which requires the additional assumptions of joint normality and homoskedasticity of u_1 and v_2 (Cameron & Trivedi 2005). The assumption of joint normality of the residuals is unlikely to hold in small samples, which could be a problem in the context of longitudinal survey data (Newey *et al.* 1990).

Attrition

The BHPS is not available continuously from wave 1 for all interviewees aged 16 to 58. This is mostly due to individuals entering the sample at later interviews once they turn 16, but also due to individuals exiting and then re-entering the sample due to attrition. If this attrition is assumed random and not systematically related to individual characteristics, then dropping individuals whom attrition affects will not have an impact on the representativeness of the resulting sub-sample. This strategy has been adopted by numerous studies using the BHPS, including Halpin (1997) and Dustmann & Pereira (2008). Although less restrictive than the aforementioned approach, the sample selection rule used in this study is likely to introduce bias in to estimates if attrition is systematically related to X_{it} in the population regression. This introduces a form of selection bias, the time-invariant component of which can be eliminated using fixed effects techniques. However, if there are time-varying factors influencing attrition then this will impart bias on the estimated results.

2.2.3 Chapter 5: Applications of the Mincerian Earnings Function to Job Displacement & Wage Scarring - Methodological Considerations.

The estimation strategy pursued in Chapter 5 is informed by lessons drawn from the literature estimating the Mincerian earnings function. The following outlines key methodological considerations based on the existing literature.

Cross-section studies: Short-run effects

Interest in the earnings effects of job displacement provided a natural learning ground for the application of the Mincerian earnings function to this important policy question. Early papers investigating this issue adopted the Mincerian earnings function to compare earnings in the pre- and post-displacement jobs, focussing their attention on layoffs and plant closures that can reasonably be assumed to be exogenous to individual characteristics (a 'natural experiment'). The following is adopted from Addison & Portugal (1987).

Equation 2.17 lays out the familiar equation of interest (individual and time subscripts are ignored in the interest of brevity).

$$\begin{aligned}\ln W_j =& \alpha_1 EXPERIENCE + (\alpha_j + \beta_j - \alpha_1) TENURE_j \\ &+ u_j\end{aligned} \tag{2.17}$$

However, the reduced form equation 2.17 compounds the effect of previous job and unemployment durations into one coefficient[6]. To see this let $TENURE_s$ and $TENURE_j$ represent completed job duration on job s and current tenure respectively; DUR_s, α and β represent unemployment duration before job s,

[6] The tenure coefficient captures the effect of tenure over and above the effect of experience (Addison & Portugal 1987)

transferable (general-training) and non-transferable (specific-training) components of the return to tenure; and u the idiosyncratic error term. This gives a more flexible representation of the Mincerian earnings function.

$$\begin{aligned}\ln W_j = &\sum_{s=1}^{j-1} \alpha_s TENURE_s + (\alpha_j + \beta_j) TENURE_j \\ &+ \sum_{s=1}^{j} \gamma_s DUR_s + u_j\end{aligned} \tag{2.18}$$

If investments in general-training vary from job-to-job, $\alpha_1 \neq \alpha_2 ... \neq \alpha_j$, then equation 2.18 can be rewritten:

$$\begin{aligned}\ln W_j = &\sum_{s=1}^{j} \alpha_s TENURE_s + \sum_{s=1}^{j-1} (\alpha_s - \alpha_1) TENURE_s \\ &+ (\alpha_j + \beta_j - \alpha_1) TENURE_j + \sum_{s=1}^{j} \gamma_s DUR_s \\ &+ u_j\end{aligned} \tag{2.19}$$

If investments in general-training are not allowed to vary from job-to-job, and γ_s is restricted to zero, then we arrive at equation 2.17. Equation 2.19 illustrates that the return to specific-training, β_j, is only identified under the assumption that $\alpha_j = \alpha_1$. Furthermore, the interpretation of the coefficient on tenure is sensitive to errors in the experience variable. If experience is measured with error, then since previous unemployment durations are not directly controlled for in the estimation, $\hat{\alpha_1}$ will be biased downwards, and thus the tenure coefficient in equation 2.17 will be biased upwards (Addison & Portugal 1987).

Equation 2.17 also ignores unobserved job match and individual heterogeneity. In an attempt to address the issue of unobserved heterogeneity, Addison & Portugal (1987) using the US Displaced Workers' Survey (DWS), as well as others using similar data, have restricted their attention to the pre- and post-displacement job. By ignoring the effects of earlier unemployment durations, this technique increases sensitivity to measurement error in the experience variable. However, this approach is generally motivated by data availability considerations. The DWS, a supplement to the Current Population Survey (CPS), contains a cross-section of individuals that were displaced from their jobs in the last 5 years. This allows researchers to compare pre- and post-displacement jobs under some strong assumptions [7].

Letting $W_{i,j-1}$ and W_{ij} represent wages on the pre- and post-displacement jobs, and X_i a matrix of individual and demographic characteristics:

$$\begin{aligned}\ln W_{i,j-1} =& \alpha_1 EXPERIENCE_{i,j-1} + (\alpha_{j-1} + \beta j - 1 - \alpha_1) TENURE_{i,j-1} \quad (2.20)\\ &+ X_{i,j-1}\Omega + u_{i,j-1};\\ \ln W_{ij} =& \alpha_1 EXPERIENCE_{i,j} + (\alpha_{j-1} - \alpha_1) TENURE_{i,j-1} \quad (2.21)\\ &+ (\alpha_j + \beta j - \alpha_1) TENURE_{ij} + \gamma_j DUR_{ij}\\ &+ X_{i,j}\Omega + u_{i,j};\end{aligned}$$

Combining equations 2.20 and 2.21 (taking 'quasi first-differences'):

[7] In the context of the DWS, which has a five year retrospective window, previous job information may be subject to systematic measurement error due to recall bias (see section Z for more information).

$$\begin{aligned}\ln W_{ij} =& \delta \ln W_{i,j-1} + (1-\delta)\alpha_1 EXPERIENCE_{i,j} \\ &+ [(1-\delta)(\alpha_{j-1} - \alpha_1) - \delta\beta_{j-1}]TENURE_{i,j-1} \\ &+ [\alpha_j + \beta j - \alpha_1(1-\delta)]TENURE_{ij} + \gamma_j DUR_{ij} \\ &+ (X_{i,j} - \delta X_{i,j-1})\Omega + (u_{i,j} - \delta u_{i,j-1});\end{aligned} \tag{2.22}$$

Equation 2.22 reduces to 2.23 when $\delta = 1$.

$$\begin{aligned}\ln W_{ij} =& \ln W_{i,j-1} + (\alpha_j + \beta j)TENURE_{ij} - \beta_{j-1}TENURE_{i,j-1} \\ &+ \gamma_j DUR_{ij} + (X_{i,j} - X_{i,j-1})\Omega + (u_{i,j} - u_{i,j-1});\end{aligned} \tag{2.23}$$

Equation 2.20 provides the return to tenure over and above experience, $(\alpha_{j-1} + \beta j - 1 - \alpha_1)$, equation 2.21 isolates the transferable component of this return, $(\alpha_{j-1} - \alpha_1)$, and equation 2.23 isolates the loss in specific-training investments, $-\beta_{j-1}$.

In the absence of a direct measure of experience, age (EXPERIENCE + SCHOOLING + 5/6) is generally used, where 5 or 6 is the school starting age. Potential experience (age - SCHOOLING - 5/6) is also another proxy widely used in the literature, however, this requires knowledge of school leaving age. Both of these measures imply measurement error in the analysis, as experience and schooling are negatively related whilst both being positively related to earnings (Card 1999). Moreover, returns to potential experience will likely be overstated for women who take time out of work during child birth (Chiswick 2003). Using age in an earnings regression without adequately controlling for previous unemployment duration could also lead to simultaneity problems (Addison &

Portugal 1987).

> "If the unexplained part of the pre-displacement wage is fully transferred to the new job, i.e. $\delta = 1$, then all individual characteristics of equal value to the two jobs should have no explanatory power. If, however, this restriction is improperly introduced, some unchanged individual characteristics will appear to valued differently across jobs, while they actually shouldn't.... [P]re-displacement tenure can appear in the equation with a negative sign, especially if δ is large and pre-displacement tenure poorly valued in the new job (Houle & van Audenrode 1995, pp. 82)."

However, this will depend crucially on whether the only difference between pre- and post-displacement wages is the "random" treatment (being laid off). If being laid off is truly exogenous to individual characteristics, then the value of the 'time-invariant' pre-displacement characteristics will be the same in the post-displacement job. If this is not the case, then the estimates will be biased upwards in general. Since the 'quasi first-differences' approach is essentially differences-in-differences, identification relies on exogeneity of the treatment. Moreover, lack of a comparison group implies that wage losses due to displacement are likely to be underestimated, as this ignores the fact that earnings of the non-displaced are growing (Arulampalam 2001). In an attempt to address this issue, Farber *et al.* (1993) estimated equation 2.23 using the DWS and a synthetic control group of non-displaced constructed from the CPS data. The common trends assumption assumes that the composition of the treated and untreated group are the same before and after treatment. This assumption is unlikely to be valid if the treatment, job displacement, is not truly exogenous as would be the case in a controlled experiment. Sample selection is an issue in the DWS, generally addressed using the Heckman (1979) procedure as the sample of individuals re-employed by survey date are unlikely to be a random sample of the displaced. In addition there may be non-random selection into treatment, however, given that the focus is usually on layoffs/plant closures this is less of an issue. Chapter 5, table 5.16 highlights selected contributions using cross-sectional data.

Panel data studies: Long-run effects

The availability of detailed panel data allows researchers to use more sophisticated methods to address both the short- and long-run economic consequences of job displacement in an integrated framework. Jacobson *et al.* (1993), a pioneering study in the field, used quarterly merged Pennsylvanian administrative panel data to consider whether the earnings losses of high tenure workers were permanent and thus a major policy concern given that these workers had the most to lose. Similar to the approach adopted in cross-sectional studies like Addison & Portugal (1989), this study focussed on a single displacement.

$$Y_{it} = \alpha_i + \gamma_t + X_{it}\beta + \sum_{k \leq -20}^{24} D_{it}^k \delta_k + \varepsilon_{it} \tag{2.24}$$

α_i captures the individual-specific fixed effect, whilst γ_t is a set of quarterly dummies capturing aggregate earnings growth. X_{it} is a matrix of observed time-varying characteristics (in the case of Jacobson *et al.* (1993) this was restricted to age (EXPERIENCE + SCHOOLING + 5/6), age^2 and sex) and D_{it}^k is a dummy variable that equals one if an individual has been displaced k quarters earlier (or later in the case of the pre-displacement period). Jacobson *et al.* (1993) allow earnings to vary with displacement status up to 20 quarters before displacement, tracking this growth for up to 24 quarters after.

Equation 2.24 is estimated via the within-groups estimator, relative to a control group of individuals who were never displaced over the observation period. This allows them to isolate the cost of displacement, relative to wage growth that would have occurred had the individual in question not been displaced. Since administrative data is generally hampered by a limited covariate set, Jacobson *et al.* (1993) adopt alternative strategies to control for unobserved heterogeneity and thus test the robustness of specification 2.24. As previously noted, sample selection is an issue when considering the earnings outcomes of the displaced. Due to lack of information about separation types (in administrative data in general), the authors are unable to accurately identify the impact of involuntary displacements. However, they limit their sample to individuals displaced from 'distressed' firms (that experience large employment changes/ mass layoffs) arguing that these individuals are at the highest risk of being laid off. This argument is likely to be weakened due to the potential endogeneity between individual productivity and firm productivity. The authors acknowledge that their approach is likely to be biased if firms selectively layoff individuals that have under-performed around the separation date (Jacobson *et al.* 1993). Moreover, the wider the window around a plant closure the more likely one is to capture early separators/voluntary quits (Kunze 2002).

A worker-specific time-trend is introduced in order to capture different trends in wage growth which may impact on the likelihood of job displacement (Jacobson *et al.* 1993). Trends in individual-specific wage growth are likely to be related to individual productivity. Thus low wage growth is likely to be indicative of low productivity. Equation 2.24 can also be extended to control for heterogeneity within observational groups by interacting whether they were displaced ($D_{it}^k\delta_k$) with the observed characteristics of interest. However, in the interest of a parsimonious specification, the authors introduce individual-time-specific dummy variables to summarize the impact of possible confounders before, during and after the displacement period.

Unlike previous studies in more flexible economies, Kunze (2002) finds little evidence of persistent wage penalties due to job displacement in Germany. 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 & Storrie 2006) and the US state of Connecticut (Couch & Placzek 2010). Eliason & Storrie (2006) highlight the increased sensitivity of displaced workers' earnings losses to recessionary pressures. Furthermore, Couch & Placzek (2010) cast doubt over the generalisability of JLS's results for the US, given changes in State and time period. Chapter 5, table 5.16 highlights selected contributions using panel data.

Longitudinal studies: Wage scarring

In addition to the question of the persistence of wage losses due to job displacement, longitudinal panel data also allows one to address the impact of multiple interruptions on wage growth as well as whether the incidence or duration of unemployment/non-employment matters more for re-entry wages. Longitudinal survey data usually forms the basis of such studies, e.g. the US Panel Study of Income Dynamics (PSID), the British Household Panel Survey (BHPS) and the German Socio-Economic Panel (GSEOP). These studies are motivated by previous research's conclusions that in order to examine the impact of unemployment on wages, accurate information on pre- and post-displacement wages and job characteristics before and after unemployment as well as the duration of unemployment are necessary. Furthermore, the exact timing of displacement as well as the number of previous displacements needs to be controlled for. Finally, a control group of non-displaced workers are needed in order to make inferences about the *counterfactual*: what would have happened to wage growth if the workers in question had not been displaced? All this requires large scale longitudinal datasets, allowing a large cross-section of individuals to be followed across an extended period of time.

Given a representative sample, the major challenge faced when using longitudinal data sources is controlling for unobserved heterogeneity[8]. The fixed effects estimator allows one to control for permanent ability differences between workers both on and off the job that may impact on promotion prospects, earnings, tenure and thus wage losses on displacement. If human capital depreciates with time out of the workforce, then since more experienced workers have accumulated more human capital they have more time to find a good worker-firm match. Good matches tend to last longer than bad matches (Farber 1999). Job tenure and experience are functions of past quit/layoff decisions and thus are correlated with unobserved job-/match-specific factors (Arulampalam 2001). Since OLS estimates of a standard earnings function using cross-section have been shown to produce biased estimates due to assuming that $E(u_{it}|X_{it}) = 0$, unobserved heterogeneity needs to be accounted for. Equation 2.24 shows a typical specification estimated in these approaches. This base equation is usually augmented to control for whether an individual entered their current employment via a spell of unemployment, quadratics or linear splines in current tenure and experience, and both a time-invariant individual-specific residual (α_i) and idiosyncratic error term (ε). Furthermore, the individual-specific time-trend and $\sum_{k\leq-20}^{24} D_{it}^{k}\delta_k$ unobserved heterogeneity controls are dropped in favour of

[8] If the sample cannot be considered representative then sample selection techniques will need to be pursued as a first step.

a rich covariate set. The within-groups estimator is typically used to eliminate the time-invariant fixed effect (α_i).

Panel data provides many advantages over cross-sectional data sets. Transitions rather than rates can be examined, thus allowing one to identify the persistence of labour market states. Detailed information on observable characteristics of individuals is generally available (not always the case with administrative panel data), and these individual characteristics can be tracked over time. Panel data techniques allow for the control of observable and unobservable heterogeneity. Most panel data sets provide information on the precise timing of events, allowing for the ordering of events to be taken into account as well as to establish causality. Furthermore, since individuals can be tracked over an extended period of time, this allows one to gauge the long-term impact of job displaced and the subsequent unemployment period on individuals' career outcomes. Despite these advantages, estimates are still susceptible to the limitations of Fixed Effects estimation. Chapter 5, table 5.16 highlights selection contributions using longitudinal data.

2.2.4 Chapter 6: Pooled & Dynamic Multivariate Discrete-Choice Models

This refers to a class of approaches to model decision making given multiple alternatives, where no natural ordering of possible choices exists. These models are also referred to as Random Utility Models, as outcomes are partially determined by factors and partially determined randomly. Unlike the Ordered Logit the Multinomial Logit (MNL) suffers from the Independence of Irrelevant Alternatives (IIA) problem (detailed later in this chapter). The **decision to implement a Multinomial Logit (MNL) based estimation strategy over the Ordered Logit** was influenced by ambiguities in the ordering of the considered labour market states. The dual labour market model by Evans (1999) assumes that only skilled workers can do a skilled job, imposing an ordering of less-skilled and skilled jobs that could be exploited by treating the outcome variable as ordinal. Ignoring ordinality would imply a loss of efficiency, increasing the risk of insignificant results (although parameter estimates will be unbiased). However, Léné (2011) extends this model, by relaxing this restriction which the author argues is inconsistent with recent empirical observations. Crucially, this study considers non-employment as a separate labour market state. Since transitions out of non-employment can be into less-skilled or skilled jobs, imposing this restriction would imply a significant loss of information and not be characteristic of individuals' contemporaneous choice set. Thus a Multinomial Logit (MNL) strategy is pursued in Chapter 6. The following outlines the MNL and key methodological considerations.

Structure

There exists an unordered discrete dependent variable y_i which takes an integer value in the set $m = 1 \ldots M$. We assume that there exists a set of latent propensities y^*_{im} for each discrete state, each assumed independent of exogenous characteristics. Moreover, all propensities depend linearly on a single common set of characteristics x_i, such that:

$$y^*_{im} = x'_i\beta_m + u_{im} \quad ,\text{for} \quad m = 1 \ldots M \tag{2.25}$$

β_m is a vector of parameters specific to state m, and u_{im} are state-specific random disturbances, with are potentially jointly distributed. These latent propensities are fundamentally unobservable. Granted, we observe outcomes which are related to the underlying propensities through observability criterion:

$$y_i = m \quad \text{if} \quad y^*_{im} = \max_{j=1\ldots m} y^*_{ij} \tag{2.26}$$

Given a set of parameters, and a distribution of the disturbance term, the observability criterion can be used to define a set of conditional probabilities $Pr(y_i = m|x_i) = Pr(y^*_{im} = \max_{j=1\ldots m} y^*_{ij}|x_i)$. However, there are limitations on the number of parameters that can be identified (only M-1).

To illustrate this, a 3-state model is drawn on:

$$\begin{aligned} y^*_{i1} &= x'_i\beta_i + u_{i1} \\ y^*_{i2} &= x'_i\beta_i + u_{i2} \\ y^*_{i3} &= x'_i\beta_i + u_{i3} \end{aligned}$$

From 2.26:

$$\begin{aligned} y_i =& 1 \quad \text{if} \quad y_{i1}^* > y_{i2}^* \quad \text{and} \quad y_{i1}^* > y_{i3}^* \\ y_i =& 2 \quad \text{if} \quad y_{i2}^* > y_{i1}^* \quad \text{and} \quad y_{i2}^* > y_{i3}^* \\ y_i =& 3 \quad \text{if} \quad y_{i3}^* > y_{i1}^* \quad \text{and} \quad y_{i3}^* > y_{i2}^* \end{aligned}$$

To see the problem of identification, consider probability of observing $y_i = 1$:

$$\begin{aligned} Pr(y_i = 1|x_i) =& Pr(y_{i1}^* > y_{i2}^* \quad \text{and} \quad y_{i1}^* > y_{i3}^*|x_i) \\ =& Pr \begin{pmatrix} x_i'\beta_1 + u_{i1} > x_i'\beta_2 + u_{i2} \\ x_i'\beta_1 + u_{i1} > x_i'\beta_3 + u_{i3} \end{pmatrix} \\ =& Pr \begin{pmatrix} u_{i2} - u_{i1} < -x_i'(\beta_2 - \beta_1) \\ \underbrace{u_{i3} - u_{i1}}_{\text{Random Component}} < \underbrace{-x_i'(\beta_3 - \beta_1)}_{\text{Deterministic Component}} \end{pmatrix} \end{aligned} \tag{2.27}$$

This shows that for whatever distribution of (u_{i1}, u_{i2}, u_{13}), the probability $Pr(y_i = 1|x_i)$ depends only on differenced parameter vectors $(\beta_2 - \beta_1)$ and $(\beta_3 - \beta_1)$. Analogously, it can be shown that $Pr(y_i = 2|x_i)$ and $Pr(y_i = 3|x_i)$ also depend on the same terms.

$$Pr(y_i = 2|x_i) = Pr \begin{pmatrix} u_{i1} - u_{i2} < -x_i'(\beta_2 - \beta_1) \\ \underbrace{u_{i3} - u_{i2}}_{\text{Random Component}} < \underbrace{x_i'(\beta_2 - \beta_1) - x_i'(\beta_3 - \beta_1)}_{\text{Deterministic Component}} \end{pmatrix} \tag{2.28}$$

$$Pr(y_i = 3|x_i) = Pr \begin{pmatrix} u_{i1} - u_{i3} < -x_i'(\beta_3 - \beta_1) \\ \underbrace{u_{i2} - u_{i3}}_{\text{Random Component}} < \underbrace{x_i'(\beta_3 - \beta_1) - x_i'(\beta_2 - \beta_1)}_{\text{Deterministic Component}} \end{pmatrix} \tag{2.29}$$

This illustrates the inherent problem of identification. Since β_2 and β_3 are only found in diffenced form, relative to β_1, we can no longer separately identify the parameters of interest. One possibility is to estimate the differences or choice normalisation.

The Multinomial Logit Model (MNL)

The MNL is preferable over estimating separate Logits, as this approach is more efficient, resulting in lower standard errors, due to the use of all observations in the sample. The IIA assumption can easily be relaxed, in a dynamic panel data context. Furthermore, this specification is more flexible, allowing for correlation between competing risks to be incorporated. In order to estimate a multivariate discrete choice model, additional assumptions are required about the distribution of unobserved heterogeneity. If we specify the individual disturbance term u_{im} as type-I extreme value, this implies a combined logistic distribution for the set of disturbances ($u_i m, \quad m = 1 \ldots M$) and the Multinomial Logit Model (MNL). The probabilities $Pr(y_i = m|x_i)$ take the form:

$$\begin{aligned} Pr(y_i = m|x_i) =& \frac{\exp(x'\beta_m)}{\sum_{j=1}^{m} \exp(x'\beta_j)} \\ =& \frac{\exp(x'\beta_m)}{1 + \sum_{j=2}^{m} \exp(x'\beta_j)} \end{aligned} \tag{2.30}$$

Where β_1 is normalised to 0, $\exp(x'0) = 1$. This implies M-1 parameters to estimate, and estimation is conducted via Maximum Likelihood. The likelihood is the product of the probabilities of each observation, conditional on the data, the parameters of the model, and the assumed distribution of the disturbance term. For the ith observation, the likelihood contribution is:

$$\begin{aligned} l_i =& \prod_{m=1}^{M} Pr(y_i = m|x_i)^{zim} \\ =& \prod_{m=1}^{M} P_{im}^{z_{im}}, \end{aligned} \tag{2.31}$$

where $z_{im} = 1(y_i = m)$ for m = 1...M, and where P_{im} represents the probability $Pr(y_i = m|x_i)$. For the parameter vector $\Theta = (\beta_2, ..., \beta_{M-1})'$, the full sample likelihood is:

$$l(\Theta) = \prod_{i=1}^{n} \prod_{m=1}^{M} P_{im}^{z_i m} \tag{2.32}$$

Whilst the Multinomial Logit generally has nice computation properties, and marginal effects of a change in a regressor on different state probabilities can be easily calculated, a potential drawback of this model is the relationship between probabilities: the Independence of Irrelevant Alternatives (IIA).

Extending the Standard Multinomial Logit Model (MNL)

The standard, pooled, model can be extended incorporating individual-specific random effects to take into account the dynamic nature of the underlying data. In extensions, these random effects are allowed to be correlated across alternatives. This strategy has the added advantage of relaxing the Independence of Irrelevant Alternatives assumption, formal tests of which are viewed with caution (Train 2009).

Equation 6.2 and 6.3 illustrate the assumptions required for identification of the parameters in the standard dynamic MNL. Whilst the MNL is more flexible than the standard Logit model, it places restrictions on the underlying choice structure. For instance, the MNL only captures variables that are constant across alternatives, e.g. race (Cameron & Trivedi 2005). The MNL also suffers from the Independence of Irrelevant Alternatives (IIA) restriction. Since the MNL coefficients are relative to a normalised base category, "discrimination amongst the m alternatives reduces to a series of pairwise comparisons that are unaffected by the characteristics of alternatives other than the pair under consideration (Cameron & Trivedi 2005, p. 593)." In other words, the ratio of probabilities of any two alternatives is independent of the probability of any other outcome. Thus adding an extra alternative to the range of outcomes has no impact on this ratio, an unrealistic feature of the model for describing most real world decision making processes. A natural alternative, if the IIA assumption is problematic, is to jointly model alternatives in order to break this restriction. The basic setup models outcomes as described in equation 6.2.

$$y_{ijt} = x_{it}\beta_j + y_{it-1}\gamma_j + (d'_{it-1}y_{it-1})'\phi_j + \varepsilon_{ijt} \tag{2.33}$$

Thus, the instantaneous probability can be represented as:

$$Pr(y_{ijt}|x_{it}, y_{it-1}) = \frac{exp(x_{it}\beta_j + y_{it-1}\gamma_j + (d'_{it-1}y_{it-1})'\phi_j)}{\sum_{k=1}^{m} exp(x_{it}\beta_k + y_{it-1}\gamma_k + (d'_{it-1}y_{it-1})'\phi_k)} \tag{2.34}$$

Where:

y_{ijt}	$=$	Individual i's labour market status (j) at time t.
m	ϵ	$1 \ldots M$ (Potential labour market states).
x_{it}	$=$	Matrix of covariates.
y_{it-1}	$=$	Previous labour market status (t-1), interacted with previous industry and workplace characteristics.
d_{it-1}	$=$	Indicator variable interacted with previous labour market status.
ε_i	$=$	Idiosyncratic error component.

Initial estimates estimate a pooled dynamic MNL, under the assumption that the error term is independent of the x_{it}. If the model is not fully specified then this assumption is unlikely to hold, and it will be impossible to distinguish between true and spurious state dependence. In order to disentangle true from spurious state dependence, a random individual-alternative-specific intercept term (α_{ij}) is introduced to control for time-invariant factors influencing an individual's probability of choosing a particular outcome, which varies across alternatives (random taste variation).

$$y_{ijt} = x_{it}\beta_j + y_{it-1}\gamma_j + (d'_{it-1}y_{it-1})'\phi_j + \alpha_{ij} + \varepsilon_{ijt} \tag{2.35}$$

Thus, the instantaneous probability can be represented as:

$$Pr(y_{ijt}|x_{it}, y_{it-1}, \alpha_{ij}) = \frac{exp(x_{it}\beta_j + y_{it-1}\gamma_j + (d'_{it-1}y_{it-1})'\phi_j + \alpha_{ij})}{\sum_{k=1}^{m} exp(x_{it}\beta_k + y_{it-1}\gamma_k + (d'_{it-1}y_{it-1})'\phi_k + \alpha_{ik})} \tag{2.36}$$

The contribution of each alternative labour market state is gauged relative to the normalised category ($J = 1$). The log-likelihood contribution of individual i can thus be represented (where $d_{ijt} = 1$ if individual i is in labour market state J at time t):

$$\begin{aligned} L_i &= \prod_{t=2}^{T}\prod_{j=2}^{m} Pr(y_{ijt}|x_{it}, y_{it-1}, (d'_{it-1}y_{it-1}), \alpha_{ij})^{d_{ijt}} \\ &= \prod_{t=2}^{T}\prod_{J=2}^{m} \left[\frac{\exp(x_{it}\beta_J + y_{it-1}\gamma_J + (d'_{it-1}y_{it-1})'\phi_J + \alpha_{iJ})}{1 + \sum_{k=2}^{m} \exp(x_{it}\beta_k + y_{it-1}\gamma_k + (d'_{it-1}y_{it-1})'\phi_k + \alpha_{ik})}\right]^{d_{iJt}} \end{aligned} \tag{2.37}$$

Individual i's contribution to the Log-Likelihood function can thus be represented as:

$$\log L_i = \sum_{t=2}^{T}\sum_{j=2}^{m} d_{ijt}\log[Pr(y_{ijt}|x_{it}, y_{it-1}, (d'_{it-1}y_{it-1}), \alpha_{ij})] \quad (2.38)$$

$$= \sum_{t=2}^{T}\sum_{J=2}^{m} d_{iJt}\log \quad (2.39)$$

$$\frac{\exp(x_{it}\beta_J + y_{it-1}\gamma_J + (d'_{it-1}y_{it-1})'\phi_J + \alpha_{iJ})}{1+\sum_{k=2}^{m}\exp(x_{it}\beta_k + y_{it-1}\gamma_k + (d'_{it-1}y_{it-1})'\phi_k + \alpha_{ik})}$$

The overall log likelihood is just the integral of the individual-specific likelihood function, $\log L_i$, over all i, w.r.t. α_{iJ} (equation 2.40).

$$\log L =$$

$$\int_i \sum_{t=2}^{T}\sum_{J=2}^{m} d_{iJt}\log\left[\frac{\exp(x_{it}\beta_J + y_{it-1}\gamma_J + (d'_{it-1}y_{it-1})'\phi_J + \alpha_{iJ})}{1+\sum_{k=2}^{m}\exp(x_{it}\beta_k + y_{it-1}\gamma_k + (d'_{it-1}y_{it-1})'\phi_k + \alpha_{ik})}\right]$$

$$f(\alpha_{iJ})d(\alpha_{iJ}) \quad (2.40)$$

In the empirical application, the alternative-specific intercepts, α_{ij}'s, are allowed to be potentially correlated by modelling their joint distribution as bivariate normal.

$$\alpha_{i1} \sim \left\{ \begin{matrix} a_2 \\ a_3 \end{matrix}, \begin{pmatrix} var_2 & cov_{23} \\ cov_{23} & var_2 \end{pmatrix} \right\} \quad (2.41)$$

This strategy allows for correlation with individual-specific unobservables, potentially correlated across the alternative states. The standard random effects estimator is a special case, where $cov_{23} = 0$. Equation 2.40 assumes that the random effects are uncorrelated with observed characteristics x_i. “Correlated” random effects can be introduced by specifying the individual-alternative-specific intercept as $\alpha_{ij} = \bar{x}_i\psi_j + \nu_{ij}$, following Mundlak (1978). A key advantage of allowing “correlated” (Mundlak 1978 corrected) random effects approach is that since this analysis conditions on the state of the local labour markets, this strategy will also capture the endogenous role of time-varying observed and unobserved regional heterogeneity in driving individual outcomes[9], without the need for Instrumental Variables (see for example Cockx & Picchio 2009b, for a similar argument). By specifying α_{ij} as a linear function of the average observed individual characteristics, this allows these characteristics to be correlated with unobservables over time, thus approximating "fixed" taste variation or a fixed effect. By modelling the alternative-specific intercepts as bivariate normal we allow alternative states to be correlated in unobservable characteristics, thus simultaneity between skilled and unskilled labour market transitions is controlled for instead of assuming that these transitions are independent. ν_{ij} maintains the IID assumption throughout.

According to Mundlak (1978), if ν_{ij} is a composite asymmetric matrix, then heterogeneity bias will be minimal, given that the correlation between the individual effects and the explanatory variables is partly captured in the model. However, if ν_{ij} is symmetric, GLS estimators of the full model will be similar to fixed effects ("within") estimators and thus unbiased. Granted that the empirical strategy employs the non-linear MNL, estimated via MLE, *since these conclusions were reached in the linear panel case* then the former conclusion is unlikely to apply. However, heterogeneity bias will still be minimal, as the correlations between individual effects and explanatory variables are controlled for. Moreover, this approach has the added advantage of allowing time-invariant variables to be included.

[9]Under the assumption that the model is correctly specified and Omitted Variable Bias is not an issue

MNL Marginal Effects

The formula for the marginal effect of a change in a regressor in a Multinomial Logit with alternative invariant regressors is:

$$\frac{1}{N}\sum_i \frac{\partial p_{ijt}}{\partial x_{jt}} \quad = \quad \frac{1}{N}\sum_i p_{ijt}(\beta_j - \bar{\beta}_i) \tag{2.42}$$

where $\bar{\beta}_i$ is the probability weighted average of β_i ($\bar{\beta}_i = \sum_j p_{ijt}\beta_j$) and $p_{ijt} = \frac{\exp(x'_{it}\beta_i)}{\sum_j \exp(x'_{it}\beta_j)} = \frac{\exp(x_{it}\beta_J + y_{it-1}\gamma_J + (d'_{it-1}y_{it-1})'\phi_J + \alpha_{iJ})}{\sum_{k=1}^{3}\exp(x_{it}\beta_k + y_{it-1}\gamma_k + (d'_{it-1}y_{it-1})'\phi_k + \alpha_{ik})}$ (Cameron & Trivedi 2005). This highlights the impact of all covariates on the reliability of the estimated marginal effects. Estimation of marginal effects and the associated standard errors is implemented using the delta method.

In this analysis the marginal effects were evaluated at the sample means of the independent variables. Recent work criticizes the use of the marginal effect evaluated at the sample means (MEM) as an approximation of the average partial effect, especially when contrasting the marginal effects of two differing sub-samples (in this case males and females). As the sample means of the male and female sub-samples will differ, this renders the MEM's of the two sub-samples incomparable. A better approach is to calculate the average marginal effects (AME). This argument is developed in Bartus (2005).

Solutions to the Initial Conditions Problem.

Given the dynamic context, the Initial Conditions Problem arises and needs to be accounted for (Hsiao 2003). Since with most labour market data an individual's complete labour market history is not observed, the initial state cannot be treated as exogenous: it is a product of an individual's previous labour market history. This initial state may either be determined by state dependence, or unobserved heterogeneity and can be considered as an endogenous selection problem (Mosthaf *et al.* 2009). In other words: we cannot draw inference about the causal effect of unemployment at time t-1 on the probability of being in low-skilled employment at time t if we don't control for why the individual was in unemployment at time t-1 in the first place. If unaccounted for, an estimation strategy is likely to result in inaccurate inferences about the degree of true versus spurious state dependence. Various methods have been proposed to deal with this issue, each with their relative merits. In this study I initially intended to implement the Wooldridge (2005) correction (specifying $\alpha_{ij} = \bar{x}_i\psi_j + x_0 + \nu_{ij}$ where x_0 is a vector of initial values, and $\bar{x}_i$ is a vector of Mundlak (1978) terms.) which has shown, through extensive Monte Carlo Experiments, to have favourable finite sample properties (Akay 2009). This approach has the additional advantage of being easier to implement than Heckman's reduced-form solution to approximating the conditional probability of these initial values (Heckman 1981). Moreover, the relative performance of these estimators has been shown to be satisfactory permitted the number of time periods in the sample is not small (Akay 2009; Arulampalam & Stewart 2009). Unfortunately, the structure of the data (allowing individuals to enter the sample at any point in time) made controlling for initial conditions difficult, relative to a cohort followed from one point in time. Thus in section 6 I do not control for initial conditions, a caveat to be taken into account when interpreting the results.

Estimation Technique

The basic Multinomial Logit model can be estimated using the delta method, via standard in-built Stata routines, as a numerical solution exists for the ML function. The Multinomial Logit with Random Effects can be estimated using GLAMM, with appropriate adjustments. Random effects are initially assumed to be uncorrelated, in the standard fashion. In extensions correlation between these random effects is allowed for by specifying the alternative-specific error components to be distributed bivariate normal. Moreover, a Mundlak (1978) approach is adopted to capture endogeneity between observed characteristics and observed outcomes. Rabe-Hesketh & Skrondal (2008) provides details, as well as an excellent exposition of data preparation steps in a dichotomous setting. This approach uses Gauss-Hermite or Adaptive Quadrature methods to solve integration of the likelihood function, due to lack of a closed form solution. Hole (2007) provides a routine for estimating this model using a Mixed Logit approach, however, extracting standard errors for the marginal effects proved problematic. An alternative approach to estimate the MNL with unobserved heterogeneity controls is suggested by Haan & Uhlendorff (2006). This Simulated Maximum Likelihood procedure constructs the likelihood function based on the method of pseudo-random Halton Draws (Cappellari & Jenkins 2006; Train 2009), and has computational advantages when attempting to solve higher dimensional integrals, notably once the number of alternative labour market states exceeds three (see Mosthaf *et al.* 2009, for an application). The simulated likelihood, see Eqn. 2.43, approximates that actual likelihood over R draws from the unobserved heterogeneity distribution (Haan & Uhlendorff 2006). However, extracting the marginal effects and standard errors can be tricky. I chose the number of Halton draws in the estimation according to the accepted rule of thumb: "The MSL estimator is consistent, asymptotically normal and efficient, and equivalent to ML if the number of draws tends to infinity faster than the square root of the number of observations does (Train 2009, pp. 259)."

$$
\begin{aligned}
SL &= \\
&\prod_{n=1}^{N}\frac{1}{R}\sum_{r=1}^{R}\prod_{t=2}^{T}\prod_{J=2}^{3} \qquad (2.43)\\
&\left[\frac{\exp(x_{it}\beta_J + y_{it-1}\gamma_J + (d'_{it-1}y_{it-1})'\phi_J + \alpha_{iJ})}{1+\sum_{k=2}^{3}\exp(x_{it}\beta_k + y_{it-1}\gamma_k + (d'_{it-1}y_{it-1})'\phi_k + \alpha_{ik})}\right]^{d_{iJt}}\\
&= \\
&\prod_{n=1}^{N}\frac{1}{R}\sum_{r=1}^{R}\prod_{t=2}^{T}\prod_{J=2}^{3} \qquad (2.44)\\
&\left[\frac{\exp(x_{it}\beta_J + y_{it-1}\gamma_J + (d'_{it-1}y_{it-1})'\phi_J + \bar{x}_i\psi_J + \nu_{iJ})}{1+\sum_{k=2}^{3}\exp(x_{it}\beta_k + y_{it-1}\gamma_k + (d'_{it-1}y_{it-1})'\phi_k + \bar{x}_i\psi_k + \nu_{ik})}\right]^{d_{iJt}}
\end{aligned}
$$

where α_{ij} are jointly distributed bivariate normal[10] and β_1 and α_{i1} are normalised to 0. This can be approach can be easily modified to include any individual-specific error distribution (equation 2.44 specifies substitutes in $\alpha_{ij} = \bar{x}_i\psi_j + \nu_{ij}$ where $\bar{x}_i$ are Mundlak (1978) terms), and implemented using Stata's Maximum Likelihood programming routines (d0 method) detailed in (Gould *et al.* 2010). Due to convergence problems, the Multinomial Logit with random effects could not be estimated. In Chapter 6, the standard pooled MNL with and AR(1) lag structure is estimated including Mundlak (1978) terms to approximate the fixed effects specification. This caveat should be taken into account when interpreting the results presented.

[10] Originally this was specified as multivariate normal, and self-employment was included as a separate labour market state. However, computational considerations meant that the dimensions of the integral to be solved had to be reduced.

Chapter 3

Literature Review

3.1 Regional heterogeneity and Unemployment Duration

3.1.1 Introduction

The empirical jobsearch literature is vast, having being extensively reviewed in Devine & Kiefer (1993), Rogerson *et al.* (2005) and Eckstein & van den Berg (2007). A major challenge is picking out the main contributions. However, the foundations of the literature, are two fold: job search theory and the matching model (see Chapter 2, Section 2.1.1 for a discussion of the theory, and 2.2.1 for a methodological discussion of the econometric challenges).

Early unemployment duration studies found, on average, a stronger impact of the duration than the level of unemployment benefits, e.g. Moffitt (1985), Meyer (1990), with re-employment probabilities spiking near benefit exhaustion. Since the probability of accepting first job offers is almost one, this suggests that average unemployment durations depend crucially on job offer arrival rates (Cahuc & Zylberberg 2004). Job offer arrival rates will in turn depend on the search efforts of the unemployed. Improving job arrival rates has been a strong driver of the many UK and international Welfare Reforms over the last 30 years. There is a well developed economic literature on Welfare reforms and the effects of jobsearch assistance, monitoring, sanctions and eligibility constraints (conditionality) on individuals' re-employment prospects, e.g. Dolton & ONeill (1996) who found a positive effect of RESTART in the 1980s on re-employment prospects in the UK. Manning (2009) and Petrongolo (2009) studied the 1996 introduction of Jobseekers' Allowance in the UK. Whilst Manning (2009) did not find a strong short-term effect of JSA on re-employment prospects, in the longer-run Petrongolo (2009) found a significant increase in transitions onto incapacity benefits of 2.5-3%, *ceteris parisbus*. Extending this theme, other studies have considered the impact of training and work experience schemes provided through Active Labour Market Programmes (ALMP), e.g. Adda *et al.* (2007) who find a positive effect of Swedish ALMP work experience schemes, and van Ours (2004) who finds a lock-in effect of Slovakian subsidised jobs. These mixed results highlight the importance of context in driving unemployment experiences, as well as the importance of individual-level heterogeneity.

Whilst this study primarily focuses on JSA recipients, job search is not exclusive to those who are outside employment. Job-search practices of non-working and working job seekers are likely to differ, in part due to differing compositions of these two groups (Green *et al.* 2011). Changing policy, economic and technological context have a crucial influence on jobseekers, e.g. recessionary pressures have an impact on the composition of jobseekers (Darby *et al.* 1986; Elsby *et al.* 2009; Shimer 2012). Technological advances have impacted on job search methods, and opened up alternative recruitment channels with varying effectiveness.

Regional variation in job offer arrival rates may help to explain regional variation in unemployment durations (Petrongolo 2001). This is typically proxied by single measures like local labour market tightness, which summarise the job matching process (Kalwij 2010). However, substantial measurement challenges as well as unsystematic variation in the efficiency in which Jobcentres collect and post vacancy data raise significant identification challenges when proxying regional variation in job offer arrival rates via a single measure.

Whilst an analysis of these phenomena is beyond the scope of this thesis, the importance of Social Interactions, Social Networks and Neighbourhood Effects in jobsearch has been highlighted in the literature, e.g. Lavezzi & Meccheri (2011) for the impact of social connections; Ioannides & Loury (2004) for job information networks and neighbourhood effects (physical and social proximity); Bauer *et al.* (2011) for the negative effect of neighbourhood unemployment on individual re-employment prospects; Topa *et al.* (2005) for the impact of informal hiring networks and Dustmann *et al.* (2011) for the impact of job referrals.

A recent contribution to the Spatial Jobsearch literature suggests that the cost of distance is relatively high, with utility of being offered a job declining dramatically with distance to the job (Manning & Petrongolo 2011). Moreover, this study finds that jobseekers are discouraged from applying for jobs in areas where they know they will face stiff competition. The focus of this literature is on frictional unemployment. The composition of the short-term and long-term unemployed is likely to diverge considerably, with different strategies required to active these sub-groups (Machin & Manning 1999).

Some Identification Considerations Relating to estimating Local Labour Market Effects (relevant to the thesis as a whole) This thesis approximates self-contained labour markets at the NUTS3 level in Chapter 4 and at the Travel-To-Work-Area level (1998 classification method) in Chapters 5 and 6. As long as individuals do not search across NUTS3/ TTWAs then this will provide exogenous variation which will allow for the identification of NUTS3 /TTWA effects. The larger these areas the more likely this is to hold, however this is less likely for skilled workers who have a higher propensity to commute and less likely given online recruitment channels. The NUTS3 area classification whilst large, was not explicitly designed to capture self-contained labour markets. The TTWA classification has gone through numerous revisions over time and is exogenous to the composition of the local labour force due to being determined by their propensity to commute (>75% of individuals that live in a TTWA also work in a TTWA). However a time-constant classification gets around this issue, albeit under some strong assumptions. Despite these limitations, the TTWA classification is widely used in UK studies of local labour markets. Lack of controls at the TTWA level meant that some indicators in this thesis were defined at the Local Authority (LAUA) level, which are less likely to be self-contained.

Even given exogenous variation in regional entities, it is still necessary to control for selection into regional location as this is unlikely to be an exogenous outcome. Studies investigating the impact of Neighbourhood Effects have resorted to differing techniques, including identifying treatment effects via natural experiments, instrumental variables (IV) or via aggregation to identify the average neighbourhood effect within a higher level area.

I exploit an aggregation strategy. A one-to-one link between the NUTS3/ TTWA and LAUA levels is defined to minimise aggregation bias. However, the Local Authority effect is unlikely to be well identified as individuals may live in an LAUA but work in neighbouring LAUA. Clearly if we aim to capture Local Authority effects then the current empirical strategy is unlikely to be robust to correlation across these sub-regional entities. In empirical applications I allow for arbitrary correlation across Local Authorities within a NUTS3/ TTWA in the regional effects. However the issues raised above suggest that this is unlikely to be satisfactory for identifying the true Local Authority effect. At most this captures an average Local Authority effect through the aggregation. The effects identified through aggregation, however, include not only the average Local Authorities effects in a NUTS3/TTWA but also any higher-level consequences of living in a particular NUTS3/ TTWA. Thus, to be able to interpret the estimated effects as Local Authority effects requires the assumption that the NUTS3/ TTWA level does not directly affect outcomes (Topa *et al.* 2005).

In the empirical applications I control for the fixed effect of living in a NUTS3/ TTWA, to capture correlation in unobservables amongst individuals living in the same NUTS3/ TTWA, e.g. due to positive sorting into local labour markets (individuals with similar characteristics gravitating to the same areas, Öberg & Oscarsson 1979). Strict identification of the Local Authority effect exists only under the assumption of no correlation in unobservables across Local Authorities within NUTS3/ TTWA groups (Chapter 4, Section 4.3 highlights the correlations amongst regional-level indicators which are measured at the LAUA and NUTS3 level of aggregation). Moreover, at the individual level this approach rules out correlation in unobservables between individuals across Local Authority boundaries as well as ruling out correlation in unobservables affecting the outcome variable of interest amongst residents within a Local Authority as key identifying assumptions (Topa *et al.* 2005). How reasonable the latter assumption is debatable as high pre-existing Local Authority unemployment levels are likely to influence an individual's likelihood of choosing to live in an area. On average this assumption may hold better at very low levels of aggregation, e.g. neighbourhoods, if these characteristics are not systemic of the Local Authority in question. Formal tests of these assumptions can be carried out, however this is left to future implementations of the study.

These considerations suggest that future extensions of this work need to take an alternative approach to strengthen identification if the issues of Spatial Jobsearch, Social Networks and Neighbourhood Effects are to be fully accounted for in the research design.

Whilst not exhaustive, the following reviews selected contributions to the literate directly related to Chapter 4. Chapter 2, Section 2.2.1 details methodological challenges to empirical applications.

3.1.2 Regional variation in job arrival rates: Related Unemployment Duration Literature:

Brown & Sessions (1997) use the British Social Attitudes survey to investigate the impact of region and individual composition on unemployment risk across the UK over the 1985-91 period. This provides a sample of 15,519 individuals, of which 1,224 experienced unemployment. The unemployment definition used in this study classifies the following individuals as unemployed: those who were unemployed and registered at a benefit office; those who were unemployed, not registered, but actively looking for a job; and those who were waiting to take up a paid job already accepted. Whilst limited by the lack of individual-level unobserved heterogeneity controls, the study finds that regional differences in unemployment risk remain *even* after controlling for a detailed set of individual characteristics (notably for men). Individual heterogeneity and variation in job offer arrivals rates are at least as important as reservation wages in driving individual unemployment experiences (van den Berg 1990). Contemporaneous local labour market conditions may impact on job opportunities and thus job offer arrival rates. Moreover, the incidence and duration of unemployment is not evenly distributed across the UK (Collier 2005). Significant differences in job search behaviour amongst observationally equivalent individuals suggests that unobserved heterogeneity plays an important role in driving individuals' unemployment experiences. Restrictions on job offer rejection ("Work Test") suggests that (regional) variation in job offer arrival rates may be a key factor in explaining the observed variation in unemployment experiences (Collier 2005).

Collier (2005) draws on a unique representative cross-sectional survey conducted drawn from Employment Services records (survey of individuals appearing in administrative unemployment/claimant count data) for the county of Kent in 1992. The sample used in the analysis covered 4,872 unemployed individuals, providing direct information on reservation wages and jobsearch activity. This data was used in order to distinguish whether individual heterogeneity or intra-regional variation (within the county) in employment opportunities are more important in driving observed unemployment durations.

Whilst the econometric strategy adopted does not account for competing risks, potentially biasing results if alternative destination states are potentially correlated. A standard competing risks model would not account for this as even if alternative destination states are accounted for, destination-specific hazards are still assumed independent (the proportional hazards assumption). Standard survival models assume independent censoring, however if this is not the case (if the determinants of censoring are correlated with the determinants of unemployment duration) then the processes must be modelled jointly. A structural model (jointly modelling reservation wage and unemployment duration determination) and reduced form 2-stage IV are employed, where the 2-stage IV tests for the importance of unobserved heterogeneity (selection on unobservables). However, formal Hausman (Hausman, 1978) tests for omitted variable bias could not reject the null that the IV and structural estimates did *not* suffer from measurement error. This finding was interpreted as suggesting that the reservation wage was insignificant, and that variation in job offer arrival rates was a more important driver of unemployment durations.

2-stage IV estimates suggest that regional location has a significant impact on reservation wages, in the region of 3%-10% across districts, within the county of Kent. Second stage unemployment duration estimates highlight the importance of demographic, personal characteristics (notably gender, with females experiencing significantly shorter durations), and previous occupation in determining durations. Mobility constraints play an important role in determining re-employability of the unemployed. Individuals willing to travel more than 1 hour to work experience durations that are on average 14% shorter than those of people only willing to travel 15 minutes. Moreover, have one's own transport significantly reduces unemployment length, regardless of travel time. Regional variation in labour market opportunities has a very strong and significant impact on unemployment experiences. This varies from average unemployment spells that are between 11% and 56% higher, and up to 42% lower than the reference district. However, Collier (2005) finds that districts within Kent with the shortest unemployment durations were also those districts with the highest incidence of unemployment, the lowest proportion of long-term unemployment, and the highest proportion of unskilled. He interpreted this as suggesting a high degree of labour market "churning" (job creation & job destruction) in these regions, implying poor long-term employment prospects. Whilst the reservation wage data in the survey is likely to suffer from substantial measurement error, encouragingly these results were robust to stratification of the sample by gender.

Folmer & van Dijk (1988) investigates the relative importance of personal characteristics and regional demand for individuals' unemployment durations, using cross-sectional data from the 1979 Dutch Labour Force Survey. The data is stratified into frictional unemployment (<4 months), medium-term unemployed (4-11 months), and long-term unemployed (≥12 months). The definition of unemployment used includes everyone looking for a job (independent of hours willing to work). The empirical approach controls for regional dummies, interacting these with personal characteristics, however, computational constraints restrict the econometric strategy to estimating a sequence of binary logits (pairwise comparisons) to approximate a multinomial logit specification. The key result of the study is that differences in unemployment durations are found to be mainly caused by personal characteristics, rather than regional variation in the demand for labour. This result is at odds with that of Collier (2005), however the two study differ in country as well as definition of unemployment (The eligibility threshold for unemployment benefits in the UK is 16 working hours, and not zero). Folmer & van Dijk (1988) find important differences across strata, with respect of personal characteristics. Older workers are over-represented amongst the long-term unemployed, moreover, most of the highly educated fall into the frictional unemployment group.

Arntz & Wilke (2009) investigates individual, regional and institutional characteristics jointly influencing Germans' unemployment durations in a competing risks framework (semi-parametric Cox Proportional Hazards model). The study draws on a sample of single males and females, and married males, using the German Integrated Employment Biographies (IEBS) (matched employer-employee administrative data) over the period 2000-2002 (pre-Hartz reforms period). Motivation came in the form of recent Government reforms geared towards reducing unemployment levels, by re-activating the unemployment through Active Labour Market Policies (ALMP). The aim of the exercise was to shed light on the effectiveness of ALMP versus Passive labour market measures (unemployment benefits and the welfare state) in driving re-employment probabilities. *Theoretical foundations of the approach:* The empirical strategy is based on a job search model which allows for simultaneous search in multiple local labour markets (see Arntz 2005, for more details). In this framework, probability of accepting a job offer depends on both the reservation wage and search intensity. Jobseekers set local labour market-specific reservation wages such that the marginal cost of continued search equates the marginal value of accepting a job offer. Moreover, jobsearch effort is allocated across labour markets such a way that the marginal value of search a labour market equals the marginal cost of searching (individuals search where they deem themselves most likely to get a satisfactory job offer). Effectively this model endogenizes job search strategy. Possible exit states considered include: local regular employment, non-local regular employment (migration), and subsidised employment. In this empirical set up, individuals are simultaneously searching for these three possibilities. The unemployment definition used, unemployment with permanent income transfers, excludes all unemployment spells without unemployment benefit receipt (UI/UA). However, like the UK Claimant Count, it under-represents the true unemployment level as it excludes the hidden unemployed, not registered at jobcentres.

Key results suggest that individual work history is the driving force behind unemployment durations in the sample, and that regional factors are not as significant. In general, older unemployed workers (56+) are less-likely to take up regular local employment (22% (22%) and 12% (21%) less likely for low and high wage singles (married men), respectively.), less likely to migrate (5% (6%) less likely for high wage singles (married men).) and more likely to end up in subsidised employment (1.7% (2.2%) less likely for low wage singles (married men). 1.2% less likely for high wage married men). Out of these, individuals with higher earnings capacities are also 5% more likely to migrate. In terms of work history variables, long unemployment benefit entitlement periods and previous employment in state-subsidised work both strongly decrease the likelihood of local regular employment. The latter increases subsequent subsidised employment probability in the range of 7% (7%) to 13% (15%), for low and high wage singles (married men). Unemployment spells >24 months reduce all exit hazards, with this effect being strongest for high earners. Arntz & Wilke (2009) interpret this as evidence that individuals are using the unemployment benefit system as a form of early retirement. However, employment in low-wage employment (<400 Euros per month) significantly increases local job finding and decreases migration probability, suggesting stronger local labour market attachment. Less support is found supporting the commonly purported positive effects of active labour market policies on reemployment hazards. Passive labour market measures seem to more significant in this context. Moreover, relative to *a priori* intuition, East and West German unemployment experiences seem to have converged over the observation period. Arntz & Wilke (2009) come to the damning, yet poignant, conclusion that since individuals with the lowest pre-unemployment earnings, and thus highest income replacement rates, have the lowest exit rates to non-subsidised employment, a reduction in unemployment benefits is likely to achieve Governmental objectives by substantially reducing unemployment durations. This accords well with the 2005 Hartz IV reforms, targeting the long term unemployed and welfare benefits recipients. However, it is unclear whether *in general* these individuals would actually enter employment, or end up squeezed out of the labour market all together.

The ranking model of Blanchard & Diamond (1994) predicts that the probability of exiting unemployment decreases with length of time unemployed (negative duration dependence), and increases with economic growth. Moreover, since employers will rank prospective job applicants by unemployment duration and average unemployment durations increase during recessionary periods, it also predicts that negative duration dependence will be stronger when the labour market is in a slump. Kalwij (2001) sets out to test these predictions, drawing on a sample of male unemployment benefit claimants (JUVOS) aged 18-59, over the period 1982q4-1998q1. A continuous-time Mixed Proportional Hazard model is employed, with Heckman & Singer (1984) discrete mass point distributed heterogeneity. Business cycle effects are controlled for using a quarterly GDP series. This allows Kalwij (2001) to separately identify the impact of ranking (unemployment duration effect, *a la* Blanchard & Diamond (1994)), sorting (individual heterogeneity effects) and the business cycle in order to assess which drives the aggregate observation of negative duration dependence. Key results suggest that *both* ranking and sorting effects explain the decrease the exit rate from unemployment with duration. On average, the likelihood of leaving unemployment within one quarter is 64% lower when economic growth is low than when high. Moreover, higher average individual unemployment durations during recessions seems to explain most of the increase in the national unemployment rate during these periods. However, the pattern of *genuine* negative duration dependence does not change over the business cycle as predicted by (Blanchard & Diamond 1994). In fact, entering unemployment during times of low economic growth implies, on average, a higher probability of *leaving* unemployment than those becoming unemployed in good times, all else equal. Kalwij (2001) draws the policy conclusion that unless this difference in composition of unemployment inflows over the business cycle is taken into account, active labour market policies will be biased towards success in periods of high- and failure in periods of low economic growth. What is not explained is whether during recessionary periods, these exits are actually exits into employment or inactivity. This criticism highlights a key limitation of the estimation technique, in that it does not separate out reason for leaving unemployment, by treating other exits as censored or in a competing risks framework. However, this artefact (lack of destination information) is a data limitation of the pre-

1996q4 JUVOS data collection strategy.

Drawing on a male sample from the JUVOS over the 1982q4-1998q1 period, Kalwij (2004) investigates whether male youth unemployment is a result of job search behaviour, due to structural employment instability experienced by certain socioeconomic groups, or a result of a combination of the two possible divers. This study is motivated by a high incidence of repeat unemployment within the UK, the job quality literature (getting the unemployed into stable jobs and avoiding repeat unemployment), as well as the youth unemployment experience. (Kalwij 2004) follows individuals on a quarterly basis, using both time on the claimant count, as a proxy for unemployment, and time in non-employment to assess the hazard of becoming unemployed for the first time (following individuals from their 18th birthday), the hazard of leaving unemployment, and the hazard of re-entering "unemployment". Men claiming unemployment benefits are defined as unemployed in this study. The study covers the pre- and post-JSA period. After the introduction of JSA, the ILO and claimant count based unemployment series have been shown to diverge considerably. Thus the composition of unemployment inflows post JSA is likely to differ substantially from that before. However, since this study follows individuals since 18, this is less of an issue as the pre-1996 inflows will dominate the sample.

A continuous-time multiple-spell mixed proportional hazard model is employed. Initial conditions are controlled for be modelling the hazard of entering unemployment for the first time using the strategy mentioned above. Lack of a detailed covariate set meant that time-invariant unobserved heterogeneity was controlled for using the approach of Heckman & Singer (1984). The multiple-spell structure of the JUVOS is exploited for identification purposes (Van den Berg 2001). Which has been shown to achieve identification under much weaker assumptions than in the single spell case (Honore 1993). However, identification still requires unobserved heterogeneity to be modelled as a random effect, retaining the proportionality assumption (Van den Berg 2001). In other words, this model does not incorporate dependent risks. Controls include a time-varying Government Office Region (GOR) Gross Domestic Product (GDP) matched to the region of claim, previous unemployment duration dummies (assumed independent of re-employment probability), region of claim dummies, as well as quarterly duration dependence dummies (grouping spells greater than 31 quarters). Due to data limitations this study implicitly assumes non-unemployment is employment.

Key results suggests strong evidence of negative duration dependence. Moreover, the hazard of re-entering unemployment decreases very quickly with time in non-unemployment, suggesting evidence of structural employment instability resulting in repeated unemployment. Contrary to conventional wisdom at the time, that incidence and not duration of unemployment is insensitive to business cycle fluctuations (Layard *et al.* 1991), Kalwij (2004) finds evidence that unemployment inflows and outflows respond to changes in the GDP series for the youth sample considered.

Repeat unemployment is found to be very common in the UK labour market. Moreover, Kalwij (2004) interprets the finding that individual heterogeneity affects the hazard of re-entering unemployment *more* than that of leaving as evidence that incidence rather than duration of unemployment is most important (this suggests that regional heterogeneity may have more impact on the probability of unemployment than on the probability of re-employment[1].). Stable unemployment is defined as being out of the claimant count for more than 2 years. 73% of young men find stable employment by 35, however the rest expe-

[1] I thank an anonymous referee for stressing this point. Given the data limitations at hand, an accurate assessment of this was not possible using Administrative data.

rience repeat frequent unemployment. Kalwij (2004) draws the conclusion that Active Labour Market Policies like the New Deal for Young People (NDYP), targeting youth long-term unemployment, would lead long-run benefits for the economy by getting these individuals out of unemployment and promoting job-retention. The robustness of these results to limiting the sample to a period when destination state is known was not tested in the study. Moreover, the implicit assumption that non-employment periods equate to employment spells is likely to be problematic when forming policies based on the study's conclusions.

Kalwij (2010) revisits the questions addressed in Kalwij (2001), using a monthly series of the Government Office Region TTWA Vacancies/Unemployment (V/U) rate in order to control for the Business Cycle. The main research question is whether individual duration dependence varies over the business cycle. By including regional dummies as well, the author argues that fixed differences in the effectiveness of jobcenter operation will be captured. This also implies that time-series variation is used to identify business cycle effects. Since multiple jobcenters exist in each GOR, with differing levels of effectiveness, this argument is likely to be subject to state aggregation bias. As with the previous papers by the same author, given the data limitations over the observation period considered, reason for leaving the claimant count cannot be controlled for. Thus, this study does not take into account the competing risks structure of the JUVOS. However, individual-level unobserved heterogeneity is directly controlled for using a support point approach (Heckman & Singer 1984). In this study a labour market is considered tight (slack) if the V/U ratio is greater (less) than or equal to the 90th (10th) percentile of the V/U distribution. This is contrasted with the baseline, being at the median of the V/U distribution.

Key results suggest that, on average, the likelihood of re-employment decreases by 65% in the first 2 years of unemployment. Sorting effects explain a third of this, whilst negative duration dependence explains two thirds of this effect. However, strong variation over the business cycle is found. Males entering unemployment in tight labour markets are estimated to be 21% less likely to leave unemployment than those becoming unemployed in slack labour markets. The unemployed are more likely to be re-employed when the labour market is tight (as there are more jobs than the stock of unemployed job seekers), and less likely to be when the labour market is slack. However, the composition of unemployment inflows depicted above suggests that a counteracting effect is also present. The former effect is found to dominate the later when the labour market is slack: longer aggregate unemployment durations when the labour market is loose are mostly driven by the business cycle and not changes in the composition of inflows into unemployment (Kalwij 2010). The counteracting effects imply that when the labour market is tight, leaving unemployment is 35% more likely than when slack. Moreover, inconsistent with the predictions of the Blanchard & Diamond (1994) model, changes in the composition of unemployment inflows over the business cycle, and not individual duration dependence, is found to be the main determinant of the systematic variation in average duration dependence (Kalwij 2010).

3.1.3 Conclusion

This section reviewed selected contributions relevant to the research questions in Chapter 4. One conclusion can be drawn from the selected literature, that there is no consistency in results across countries relating to the relative importance of individual and regional heterogeneity. UK studies, Brown & Sessions (1997) and Collier (2005), find that regional variation in job offer arrival rates is the main driver of average unemployment experiences. However, for the Netherlands, Folmer & van Dijk (1988) find that individual characteristics and not regional variation in labour demand to be the main driver of aggregate unemployment durations. This result is concurred by Arntz & Wilke (2009) for Germany. Differences in methods of controlling for the regional level may explain some of the differences in results. However, using UK Claimant Count data, Kalwij (2001; 2010) both find evidence suggesting that observed aggregate pattern of duration dependence over the business cycle is mostly driven by changes in the composition of unemployment inflows and not due to stronger negative duration dependence at the individual level (as would be predicted by the Blanchard & Diamond 1994 model). Kalwij (2001) draws on aggregate GDP, however, Kalwij (2010) uses a quarterly regional Vacancies/Unemployment series controlling for the fixed effect of living in a region. Consistency of this result across the two papers suggests an important role for time-varying regional heterogeneity in driving average unemployment outcomes in the UK, not adequately captured by regional fixed effects alone. This also lends support to the importance of regional variation in job offer arrival rates (Collier 2005). However, restricting this same sample to under 30 year old men, Kalwij (2004) finds less of a role for regional heterogeneity. Incidence, rather than duration of unemployment is found to be most important for young male career outcomes. This result is interpreted as suggesting that Active Labour Market Policies designed to encourage employment retention will have long-run benefits for the economy.

Although many existing UK studies suggest less of a role when compared to individual-level characteristics, regional variation in average unemployment experiences suggests that geography is may be more important than the existing studies have acknowledged. Disentangling its effect is of key interest from a policy perspective. Chapter 2, Section 2.1.1 points to the importance of job arrival rates. Regional variation in job offer arrival rates may help to explain regional variation in unemployment durations (Petrongolo 2001). This is typically proxied by single measures like labour market tightness (vacancies/unemployment, V/U, rates), which summarise the job matching process (Kalwij 2010). However, substantial measurement challenges, on-the-job search, and unsystematic variation in the efficiency in which Jobcentres collect and post vacancy data raise significant challenges when proxying the regional context via a single measure. These issues, further developed in Chapter 4, motivate the detailed approach to modelling the regional context adopted therein.

3.2 Wage Scarring Background

This section reviews main contributions to the Wage Scarring literature, as well as identifying gaps in the literature that raise interesting follow on questions. These questions provide a basis for the research questions in Chapter 5. The foundation of this literature is the Mincerian earnings function (see Chapter 2, Section 2.1.2). Earlier studies, mostly in the US, focussed on estimating the short-run impact of *involuntary* job displacement using cross-sectional data like the Displaced Workers' Survey (e.g. Addison & Portugal (1987), Topel (1990), Carrington (1993), Houle & van Audenrode (1995) and Seninger (1997)). The lack of longitudinal data on both displaced and non-displaced individuals is likely to be an important limitation. Arulampalam (2001) argues that longitudinal data on both pre- and post-displacement wages, as well as the timing of these displacements, is necessary to ensure that the impact of job loss on subsequent wages is not underestimated. This point is also raised in Gregory & Jukes (2001): Concentrating solely on displaced workers ignores the fact that non-displaced workers' wages may be growing whilst an individual is unemployed. Thus the wage the individual was earning when they were displaced may be less than the wage of an equivalent non-displaced individual at the time of re-employment. Taking into account the experiences of non-displaced workers allows one to address the counterfactual: *What would have happened to an individual's wage if they had not been displaced?*.

Panel studies investigating the long-run impact of *involuntary* job displacement in the US include Ruhm (1991), Jacobson *et al.* (1993), Neal (1995) for the impact of industry specific human capital, Stevens (1997) for the impact of multiple displacements, and Kuhn & Sweetman (1998) for the impact of union status in a time of declining unionisation. European studies include Borland *et al.* (2002) for the UK, Burda & Mertens (2001) and Kunze (2002) for Germany. Key contributions to the Job Displacement literature are briefly covered in the introduction to Chapter 5 whilst Table 5.16 in the same chapter summarises the methodology and key findings of selected contributions. Methodological issues around estimating Wage Equations in the context of Job Displacement and Wage Scarring are detailed in Chapter 2 , Sections 2.1.2, 2.2.2 and 2.2.3.

3.2.1 Wage Scarring:

'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. In combination with the persistent nature of unemployment experiences[2], if unemployment experiences impact on the slope of future wage-tenure profiles (earnings growth) then by implication Government Active Labour Market Policies (ALMP) targeting the re-employment prospects of the unemployed are paramount. A common identification strategy has been to focus on the earnings outcomes of individuals displaced due to reasons one can reasonably assume are unrelated to their individual characteristics. The strategy of focussing on involuntary separations is also motivated by the finding that the majority of earnings losses due to displacement can be attributed to these separation types. Relative to separations in general, there is a wider literature looking at the earnings losses associated with involuntary unemployment, e.g. see Fallick (1993), Kletzer (1998), & Farber (1999) for detailed surveys. Fewer papers have explicitly addressed the impact of unemployment incidence and duration on subsequent wage growth. Some recent papers looking at the impact of unemployment incidence and duration on subsequent wages are: Arulampalam (2001) and Gregory & Jukes (2001) for the UK, van Dijk & Folmer (1999) for the Netherlands, Lippi & Ordine (2002) for

[2] Arulampalam *et al.* (2000).

Italy and Fares & Tiongson (2007) for Bosnia & Herzigovina.

3.2.2 Longitudinal studies & fixed effects

Using the first seven waves of the British Household Panel Survey (BHPS), 1991 to 1997, and focussing on a sub-sample of 3516 males directly interviewed in 1991 who followed over subsequent waves[3], Arulampalam (2001) estimates the effects of interruption on re-employment wages. Controls were made for changes over time, sample selection via the Heckman selection model (Heckman 1979)[4], and time-invariant unobserved individual heterogeneity via the within-groups estimator[5]. Self-employment is excluded from the definition of current

[3] This excludes individuals who entered the sample after 1991 and remained in the sample continuously from then onwards, severely limiting the sample size. Focussing on individuals directly interviewed at Wave 1 and following their career trajectories introduces a sampling bias as at that point in time the sample will be dominated by individuals with long employment tenure (Farber 1999). This issue is compounded by the fact that new labour market entrants are only allowed to enter the sample in the first Wave, 1991 and not later. The fact, highlighted in Farber (1999), that most new jobs end early is likely to imply that this sampling framework is likely not to capture much of these short-run dynamics.

[4] Given the fixed effects estimator requires at least 2 outcome variable observations per individual, this implies some sample selection in the data construction. Arulampalam (2001) controls for this selection by modelling the first selection equation as a dichotomous variable, taking the value "1" if an individual has 2 wage observations in the data and "0" otherwise. In other words the study is not modelling selection into employment but selection into employment of more than one period. Individuals employed for only one period, and otherwise continually in the survey will be coded as "0" along with individuals that remain unemployed over the 7 waves. The problem is that wages are not randomly assigned, as they are only observed for the employed implying that the observed wage distribution will be incidentally truncated at zero due to the underlying participation decision. This may not be the best econometric strategy, as it requires first that an individual be employed, and then that they work for more than one period. It is likely to underestimate the impact of transitions from unemployment to employment, and especially inactivity to employment, if these individuals are over represented in the pool of unstable, short duration jobs. These estimates are also likely to be imprecise due to small cell sizes.

[5] The within-groups estimator -deviations from the mean- is analytically equivalent to the fixed effects estimator (Angrist & Pischke 2009). For unbiased results, one must assume common stochastic shocks and that all observed and unobserved confounding factors are controlled for Arulampalam (2001). In other words, this approach parameterises the nature of unobserved heterogeneity. However, since the Within-Groups estimator only controls for time-invariant individual-level fixed effects, time-varying factors not controlled for in the regressions could imply Omitted Variable Bias. This point is raised by Gregory & Jukes (2001), who admit that their specification cannot control for time-varying demographic and local labour market changes.

labour market status. However, the author groups previous employment and self-employment, stating that this is not possible using the retrospective job history information, as self employment is grouped with a change of employer (see Taylor *et al.* 2010, 13th wave Questionnaire, pp. 80). With regards to the current job status, it is unclear whether self-employed individuals are dropped from the sample or included in the control group. This is relevant for individuals that move between employment and self-employment over the period and were in employment enough times to appear in the wage equation. The advantage of the BHPS over data sources used in previous studies is the ability to distinguish between displacement types, construct a control group of non-displaced, as well as to control for the exact timing of displacement. Earlier literature almost exclusively focussed on involuntary job separations. The other main advantage is the ability to better control for general experience. Full employment histories are available - albeit with some inconsistencies due to the retrospective nature of this data (see Section E for details of how these inconsistencies can be minimised)- from the time an individual first left full-time education. This allows the researcher to calculate general experience by cumulating full-time employment experience rather than proxying this using age or potential experience (age - schooling). The study aims to address a number of research questions: the effect of job interruption on re-employment wages; the effect of type of interruption; the effect of the duration versus incidence of interruption; and the effect of multiple unemployment spells on re-employment wages.

Arulampalam (2001) adopts a ‘flexible’ Mincerian earnings function as the baseline specification, which controls for personal and job characteristics as well as regional and industry fixed effects. Specification 3.1 illustrates the flexible formulation of equation 2.5 used in Chapter 5.

$$\ln W_{it} = X'_{it}\beta + (d'_{it}Z_{it})'\gamma + \alpha_i + \varepsilon_{it} \tag{3.1}$$

where W_{it} represents to natural logarithm of individual i's real hourly wage at time t[6], X_{it} is a matrix of observed individual and firm characteristics including schooling, current tenure and experience, d_{it} equals one if the individual in question came into the current employment spell via a non-employment, Z_{it} is a matrix of observable individual characteristics (current tenure/ reason for leaving previous job/ non-employment duration/ unemployment incidence), α_i a time-invariant individual-specific error and ε_{it} an idiosyncratic error term. This specification also allows for constant individual-specific effects not captured by observed factors included in the model[7]. Equation 3.1 is estimated via the within-groups estimator.

The dependent variable in the outcome -wage- equation is likely to suffer from measurement error as self-reported usual hours worked, collected once a year in the BHPS, are likely to adjust more slowly than usual gross weekly pay which can be gleaned from individuals' pay slips (Taylor *et al.* 2010). In the BHPS, usual hours worked are collected once a year, in the individual response file pertaining to the current job at interview. In the case of an individual who has worked in two separate jobs over the past year, their pay information will be collected twice: once in the current response file and once in the work history file. The author's justification for using this variable is that scarring may not just be limited to weekly wage changes, as weekly wages may stay constant but hours may be doubled, a real wage scar not captured when analysing weekly wages (Arulampalam 2001).

[6] Usual gross weekly pay/Usual weekly hours worked, deflated by the Retail Price Index and measured in 1991 prices.

[7] An attempt to control for time-varying selection effects is made, by interacting the Inverse Mills ratio with a linear time trend, this is generally not significant (Arulampalam 2001).

Job-to-job transitions dominate the sample, with low labour market attachment found for individuals transitioning from unemployment or inactivity to employment (employment tenure is low for the majority of these sub-samples). This observation supports the suggestion, by Arulampalam (2001), that the impact of these transitions on subsequent wages is likely to be underestimated.

The base specification, which controls for previous labour market state, suggests that an individual entering employment from unemployment will on average suffer a 7% wage penalty, *ceteris paribus*. This figure is larger for individuals coming from "Outside of the Labour Force"(OLF), at 11%. When the effect of displacement is allowed to vary with type of displacement as well as time since interruption, the initial wage scar associated with unemployment to employment transition is initially 6%. Thus figure rises to 14% during the first 4 years, and subsequently levels off at 11%. Estimates for the penalty to previous OLF suggest very high initial penalties (8.6% to 13.6% 3 years later), however this is insignificant in the long-run. The author suggests that this could be due to small cell size with respect to this interruption type and thus creates a combined non-employment category for further analysis. The combined effect of unemployment and OLF on wage growth is 6.4% in the first year of employment, with a long run penalty of 10.5%.

The author finds a permanent re-employment wage penalty for individuals moving from unemployment to employment with previous employment history, with the first spell of unemployment carrying the highest penalty of roughly 22%. In contrast, the average penalty for non-first timers is 10%, decreasing to 9% in subsequent periods. Evidence of persistent scarring is also found, with penalties being carried into subsequent periods. However, the nature of job interruptions is found to be significant with involuntary unemployment (redundancies) carrying the lowest future penalty (a significant 3.5% average wage *growth* is found). This suggests that involuntary unemployment carries the lowest ‘stigma’ relative to other separation types, and that distinguishing between these exit reasons is likely to be important. This conclusion is strengthened by the observation that over 80% of individuals displaced from their former jobs by redundancy don’t experience an intervening non-employment spell. However, the inability to separate plant closures from layoffs using the BHPS suggests that heterogeneity within the ‘redundancy’ category may exist, if the US evidence is anything to go by. Arulampalam (2001) finds unemployment duration to be insignificant, attributing the entire wage penalty effect to incidence. This result does not change for individuals who were previously inactive. However, Arulampalam (2001) uses an indicator of spell length provided in the BHPS that contains a significant proportion of missing date (50% missing over the period), and it is not clear whether this factor is driving the results. Arulampalam (2001) attributes the difference between her results and the ones of Gregory & Jukes (2001) to the fact that the former study considers all types of unemployment -registered and non-registered-, whereas the latter only considers registered unemployment. This result is corroborated in recent work that develops ’bounds’ for the true rate of unemployment, using the Joint Unemployment & Vacancies Operating System (JUVOS). Whereas the definition in the BHPS is closer to the ILO definition, registered unemployment from the JUVOS claimant count is likely to be a ’lower bound’ to the true unemployment rate (Wilke 2009). Arulampalam (2001) results rely on the identifying assumption that the Travel-To-Work-Area unemployment rate at the first interview - 1991 - has a direct impact on individuals propensity to experience unemployment for more than two periods, whilst being independent of offered wages. Furthermore, lack of continuous monthly data implies that (Arulampalam 2001) cannot directly control for wage observations at the date of job offer acceptance or track wage growth. Wage observations at survey interview date are controlled for, but these are subject to similar criticisms raised by Houle & van Audenrode (1995) in relation to Addison & Portugal (1989). Survey date job does not necessarily refer to the first job offer accepted after a spell of non-employment. Controls for experience and tenure indirectly capture these between survey date dynamics, however they require the assumption that measures for tenure and experience accurately capture accumulation of general and firm-specific human capital and do not merely index them (Farber 1999).

Using a detailed linked British administrative dataset[8], covering the period 1984 to 1994 and focussing on a sub-sample of 150,000 males, Gregory & Jukes (2001) investigate the impact of registered unemployment experiences on subsequent earnings of British men. A 2-step Heckman procedure and the within-groups estimator are used to control for individual-level heterogeneity as well as sample-selection. Research questions addressed include: the impact of unemployment experience on subsequent earnings growth, relative to experiences of similar individuals without this experience; whether the wage penalty is temporary or permanent; whether incidence or duration of unemployment is more important for the penalty; and heterogeneity across groups (age and occupation) in terms of propensity to be unemployed.

The authors' only find a temporary wage penalty of incidence (10% in the first year, 7% in the second, and a long-run penalty of 2%) whereas duration is found to inflict a permanent wage penalty that is increasing with length of unemployment (from 5% for a 6-month spell to 11% for a 12-month period of unemployment). The large sample size allows the authors to stratified their data by age group. No penalty is found to exist for the initial job interruptions of younger workers, however, a penalty is found to exist for longer durations regardless of age profile. Additionally, the wage penalty is found to rise exponentially for prime-aged workers, peaking at mid-age (49) and then flattening

[8]The New Earnings Survey Panel linked to the JUVOS creating a 1% representative sample of all adults within the U.K.

out until retirement age (measured as 64 in the study). This negative wage effect is found to be a particular *curse of the "well-off"*, being highest amongst individuals with higher incomes and those in higher occupational groups.

A representative sample, and large sample size lend credence to the results of Gregory & Jukes (2001). However, given that the treatment being looked at is via a selection on observables this could imply possible omitted variable bias due to the lack of control for time varying covariates in the model. The estimation technique only captures time-invariant factors not captured by the variables included in the model. The administrative data used in the study lacks data on demographic and local labour market conditions. Furthermore, due to the lack of detailed retrospective information, Gregory & Jukes (2001) restrict registered unemployment to have an effect up to two years after incidence (Arulampalam 2001). The results in Gregory & Jukes (2001) highlight that long-term unemployment always carries a wage penalty, regardless of age. Granted, it would be of interest to gauge how relevant the relative age, occupational, and industrial structures of regions is in this regard. The observation that individuals with similar characteristics tend to gravitate to the same areas, (Öberg & Oscarsson 1979), would further motivate this enquiry.

None of these papers explicitly consider the regional context in which individuals' experience their unemployment spells. Gregory & Jukes (2001) include as one of their controls region of work, however they do not mention the sign or significance of this indicator in the paper. The regional controls in this paper enter in the first stage regression and not the second stage, suggesting that the regional context has an impact on re-employment probability but not on subsequent wage outcomes.

3.2.3 Cross-national studies:

Whilst consistent evidence of large and persistent earnings losses due to involuntary job separations has been found in the United States (Ruhm 1991; Jacobson *et al.* 1993; Farber *et al.* 1993; Seninger 1997), these results have not been corroborated in Europe as a whole. As highlighted in section 3.2.2, the findings for the UK are generally consistent with those of the US (Arulampalam 2001; Gregory & Jukes 2001). However, the lack of evidence of a persistent wage scarring effect of job disruptions in the rest of Europe (Kunze 2002; Burda & Mertens 2001) acted as a motivation for the study into cross-national differences in wage scarring by Gangl (2006). Focussing on the impact of unemployment on future wage prospects, Gangl (2006) develops and tests an "institutional hypothesis" wherein the consequences of unemployment are treated as endogenous to the institutional environment in which unemployment is experienced. Two major sources of cross-national differences in institutional context are taken into account: Employment Protection Legislation (EPL) and Unemployment Insurance (UI including Active Labour Market Programmes). EPL (for example severance pay and advance notice, which alter the overall wage structure in the economy) is argued to have negative implications for wage scarring, if it limits workers' mobility. The likely impact of UI is positive, however Gangl (2006) stresses a negative interaction of EPL and UI that implies that the overall effect of these institutional factors is an empirical question. Strict EPL may limit the positive effect of unemployment insurance systems (Gangl 2006).

In order to address the hypothesis, Gangl (2006) draws on the US Survey of Income & Program Participation (SIPP) and the European Community Household Panel (ECHP), covering the 1995-1999 and 1994-2001 periods respectively. Although the two data sources are modified to reflect each others design, the incidence of recall bias is likely to be higher in the ECHP given that the SIPP is conducted on a 4 monthly basis. The definition of unemployment used in the analysis is restricted to individuals without work and searching for a job at the time of interview. The sample is further restricted to all workers, male and female, aged between 25 and 54. A serious limitation of the study is that it doesn't control for heterogeneity across separation types. Whilst Gangl (2006) argues that lack of comparability between the SIPP and the ECHP motivated this, this limiting assumption implies that the empirical strategy adopted in the study is unlikely to be appropriate. A stratified difference-in-difference kernel matching estimator for the average treatment effect on the treated (ATT) is implemented. However, since the treatment is cannot reasonably considered as exogenous, this selection on unobservables cannot be accounted for in the implemented framework. A better strategy would have been to isolate the impact of involuntary layoffs. Given these problems, the estimation results are likely to suffer from what Rosenbaum refers to as "hidden bias" or selection bias (Rosenbaum 2002).

Significant cross-national differences in the persistence of wage scarring are found, with the biggest losses accruing in the first year of re-employment. Notably, the quickest recovery is found in the most flexible labour markets. The largest initial wage penalties are found in the US. The average wage penalty in the US is 15% in the first year, dropping to 6% in the second, and remaining at 5% 3 years later. This compares with a figure roughly 10% 3 years later in Belgium, and United Kingdom who share a similar wage recovery pattern to the US. The penalty is 3% to 4% 3 years later in Germany, Ireland, Austria and Finland however the displaced in these countries don't experience similarly large initial wage losses. Gangl (2006) argues that these contrasting results are likely driven by differences in the flexibility in working arrangements, allowing individuals to easily adjust their hours of work in countries like the US.

Controlling for EPL strictness, strength of the UI system and the interaction, Gangl (2006) finds evidence to suggest that the effectiveness of an UI regimes is highly contingent on the flexibility of the labour market, i.e. low EPL levels. The prospects of workers losing their jobs in highly institutionalised labour markets are found to be relatively worse off, even after controlling for job history and worker characteristics (composition of the workforce). Gangl (2006) finds that the most important factor dictating cross-national differences in wage recovery is the ability to adjust hours of work. Whilst intuitive, it is unclear to what extent this result is driven by omitted variable bias, given the possible inappropriateness of the research design and the fact that the study also comes to conclusions that are inconsistent with its priors. Another possible explanation for the quick recovery of wages in the US is that individuals may be able to lower their reservation wages more easily than in less flexible European labour markets with more centralised wage bargaining systems (Abbring *et al.* 2002).

3.2.4 Wage Scarring and Youth Unemployment:

> The best predictor of future unemployment, research shows, is previous unemployment. In Britain a young person who spends just three months out of work before the age of 23 will on average spend an additional 1.3 months in unemployment between the ages of 28 and 33 compared with someone without the spell of youth joblessness. A second stint of joblessness makes things worse (Economist 2011).

Since this study does not focus solely on high tenure, prime age workers, the age-profile of the displaced is likely to be important. Youth unemployment is traditionally seen to be less of an issue for subsequent wage growth, relative to the unemployment experiences of the high tenure prime age workforce. This position is likely motivated by the predictions of human capital theory, given that young workers have less specific human capital accumulated and thus less to lose. A common strategy in the literature is to focus solely on high tenure individuals, as exemplified in Jacobson *et al.* (1993), who exclude low tenure workers entirely from their sample. Furthermore, since the PSID oversamples household heads, the results from studies like Stevens (1997) using this resource are not likely to be representative for this age group. A further limitation of these studies is the use of a comparison group constructed over the entire age profile of the sample population. Kletzer & Fairlie (2003) argue that the main driver of young displaced workers' earnings losses is the rapid early career wage growth forgone as a result of not being in work. Using a counterfactual that includes all age groups will likely underestimate the earnings losses for young workers, given that average wage growth profiles are flatter than those specific to this age group. Employing the National Longitudinal Survey of Youth (NLSY), they find substantial and persistent earnings losses for young displaced workers in the US, circa 10% five years following job displacement (Kletzer & Fairlie 2003). The nature of unemployment experiences tend to differ by age group, with younger workers experiencing higher incidence and shorter durations relative to their more experienced counterparts (Nordstrom Skans 2004). This is reflected in statistics presented by Gregg & Tominey (2005) for the UK: roughly 60% of their sample experienced no youth unemployment, whilst 22% experienced 1.5 months. However, a small % of their sample, the top 8%, experienced 26 months of youth unemployment. On the face of it youth unemployment seems transitory, however, the experience may have far reaching consequences for individuals' future wage growth.

Fares & Tiongson (2007) use the longitudinal 2001, Bosnia & Herzegovina Living Standards Measurement Survey, and its subsequent 2002 to 2004 waves, to address the topical issue of youth unemployment experiences and its long-term effects within a transition economy. The authors' estimate separate equations for unemployment and joblessness scarring, as well as for the wage effect, allowing for the effect of previous unemployment spells on subsequent wages to vary by skill level. Youth are found to be more likely to move into inactivity or unemployment and less likely to be able to move out of inactivity. In line with Gregory & Jukes (2001), Fares & Tiongson (2007) do not find any significant impact of initial interruption on the youth, relative to older workers. However, contrary to the previous literature the authors do not find evidence that the wage penalty is higher for more educated workers. The very short panel and the lack of control for neither selection nor unobserved heterogeneity raises questions about the validity of the empirical predictions. Regional boundaries are less clear-cut than those of nations, however, heterogeneity across regions could exist due to their differing levels of economic development, as well as industrial- and individual-level compositional effects.

Investigating the long-term costs of job-displacement for younger workers Kletzer & Fairlie (2003) present evidence challenging the notion that youth unemployment is purely frictional. A representative sample of 12,686 men & women, aged 14 to 22, and first interviewed in 1979 is selected from the US National Longitudinal Survey of Youth (NLSY) to cover the 1984 to 1993 period (the maximum age in their sample is 30). The sample is restricted to exclude military enrollments, those attending school in any year, zero earnings observations, and individuals without 3 years of continuous pre-displacement tenure (to exclude recent school leavers). Individuals displaced and then reemployed by their previous employer within a year, recalls, are also dropped from the analysis. Due to data limitations, layoffs and plant closures are grouped.

A similar estimation strategy to that implemented in Jacobson *et al.* (1993)

is followed, including pre-displacement and post-displacement year dummies in a fixed effects Mincerian earnings equation controlling for quadratics in age and cumulative experience. In all specifications, no evidence of an initial dip of earnings is found for this age group. Displaced men face a 5.7% wage penalty in the year of displacement, increasing to 17.9% in the next year, and 9% four years after initial displacement. Larger wage losses are found, with a significant 7.8% penalty in the year of displacement rising to 10% the year after. Five years after wage losses are still 19.9%, relative to the counterfactual. Controlling for differences in experience reduces the estimated coefficients significantly in the case of earnings losses. Whilst no earnings losses are apparent in the post-displacement year, earnings are 16.9% relative to the counterfactual. After 5 years, relative earnings losses are just 3.3%, suggesting strong wage recovery, whilst relative wage losses are 16%. Larger earnings losses are found for women, which Kletzer & Fairlie (2003) argue are mostly driven by hours of work reductions. Controlling for experience, women face a 35% earnings penalty during the post-displacement year, lowering to 21% in the next. Significant wage penalties are found in the second year after displacement, where wage are estimated to be 6.8% less than the counterfactual. Five years after displacement, wages are found to be 7.1% lower that expected levels. Introducing experience controls renders the wage penalty insignificant. Displaced women are estimated to face a 35% earnings penalty in the first post-displacement year, dropping to 21% in the second.

Kletzer & Fairlie (2003) stratify the analysis into skill groups, in order to investigate whether the earnings losses vary with the underlying average wage growth rate of that specific demographic. For the male sample (Results are similar for females), they find earnings losses in the range of 28% to 34% 3 to 5 years post displacement for college graduates. Relative to the earnings losses of less skilled groups, they find college graduate losses to be mostly driven by working hour reductions whereas in the case of less skilled workers these are lower due to increased hours of work being used as a strategy to counter the wage drop.

The argument that the most rapid wage growth is experienced at the beginning of one's career is only valid under the assumption that wage-tenure profiles are not "back loaded" in order to discourage quits (Abraham & Farber 1988). Since Kletzer & Fairlie (2003) focus on the first observed job displacement, their study cannot say anything about the impact of subsequent interruptions on the wage growth of this age group and especially on their career earnings. Gregg & Tominey (2005) track individuals up to age 42 in order to control for early career displacement, as well as subsequent interruptions, on the individuals' entire prime age wage profiles.

Gregg & Tominey (2005) draw on the UK National Child Development Survey (NCDS) of a cohort of children born in the week March 1958, following them until age 42 (10 year intervals), in order to look at the *long-term effects* of youth unemployment -between 16 & 24- on subsequent wages. Whilst this isn't panel data, the longitudinal nature of this dataset allows the authors to analyse this phenomenon relative to a counterfactual group experiencing no youth unemployment using the retrospective content of the survey. In order to analyse the wage scar, they restrict their sample to individuals reporting positive wages in the relevant period[9]. This common strategy in the literature side steps the issue of "Attrition Bias". Their sample is further restricted to individuals with more than 2 years of employment history in order to exclude youths pursuing further education. Their dataset provides the benefit of a rich set of controls for demographic and regional characteristics, family background, as well as unemployment experience.

[9]This introduces a sample selection issue..... corrected for?

Gregg & Tominey (2005) adopt an Instrumental Variables (IV) approach as their estimation strategy. The local unemployment rate at age 16 is used to instrument for youth unemployment. The authors argue that, since at 16 individuals' decision about where to live is largely exogenous to their own personal characteristics, this should make the indicator a valid instrument (Gregg & Tominey 2005). As noted by Öberg & Oscarsson (1979), individuals with similar characteristics - i.e. income - tend to gravitate to similar regions, driving the evolution of the compositional mix of a region as well as unemployment. However, they note that since the decision about where to live at 16 is largely made by an individuals' parents, this implies that parental heterogeneity should be controlled for in order to avoid this as a potential source of bias in the results (Gregg & Tominey 2005).

Gregg & Tominey's (2005) results highlight a large wage penalty ('scar') for both males and females, with strong recovery over the next 10 years. However, this is only the case if further unemployment spells are avoided after 23. Individuals that experience >7 months of unemployment between 23 and 33 have wages that are 16% to 33% lower for men, whilst this is 10% to 19% lower in the case of women. They also find a long-lasting wage scar with lasts for 20 years, a result that is robust to initial controls for unobserved ability. Furthermore, results from the IV strategy adopted suggests that unobserved heterogeneity was not imparting downward bias on the results.

In the worst-case scenario, more than 13 months of youth unemployment exposure results in a 30% raw wage gap at 23[10], which increases to 42% at 33 & decreases to 42% at 42 for men. In the case of women this raw gap is 34% at 23, 42% at 33 but recovers at a faster rate to 25% at 42 for women. Once education and region of residence is controlled for, Gregg & Tominey (2005) find that the

[10]Relative to the counterfactual: the wage penalty associated with no youth unemployment, given similar subsequent unemployment experience (Gregg & Tominey 2005).

wage gap is quite similar at 23 (23%) and 33 (16%), suggesting persistence of the wage scarring effect of youth unemployment. The risk of experiencing further unemployment spells before 33 is heightened by frequent exposure to youth unemployment. However, after 33 the relationship between youth unemployment and unemployment disappears. Education accounts for most of the wage gap at 42, with qualification to degree level increasing wage by 65% to 70% relative to the counterfactual. Wages recover slowly and incompletely after substantial youth unemployment, plus further exposure to unemployment slows the recovery process. Whilst the possibility of recall bias is evident, the results support government interventions to target youth unemployment, lending support to Active Labour Market Programmes like New Deal For Young People.

3.2.5 Relevant Regional-Level Studies

van Dijk & Folmer (1999) employ the April 1985 Dutch OSA-Labor Market Survey in order to investigate the effect individual unemployment history on wage levels, taking into account the regional labour market. The 1985 Dutch OSA-Labor Market Survey contains data on 4,020 individuals aged 16-60, excluding students and those in military service (individuals with missing age information, and those still in full-time education, are excluded from the analysis). This provides a snapshot of labour market activity at the interview date. Retrospective data used to capture work life history over Jan. 1980 - April 1985 period, as well as to construct the controls used in the analysis. A sub-sample of 1,310 males is selected, 1,204 of which were employed at the interview date. A parametric Heckman Selection Model (Heckman 1979; Puhani 2000; Newey *et al.* 1990) is used to correct for sample selection[11].

[11] Controls in the first stage participation equation (probability of being employed in April 1985) are: education; marital status; age; age^2; duration of unemployment.

Given the cross-sectional nature of the data, with wage only being observed at interview date, individual heterogeniety cannot be adequately controlled for using individual-level fixed effects. The authors attempt to address this issue by stratifying their sample into high and low unemployment regions, in order to test whether the impact of previous unemployment experience varies with the level of unemployment in a region[12].

The Hausman test (Hausman 1978) is implemented to test for endogeneity of unemployment duration, frequency and number of job changes. The null hypothesis of exogeneity was rejected in the case of unemployment duration and frequency, however this could not be rejected in the case of number of job changes. Thus Tobit-predicted values of unemployment duration and frequency were used to control for simultaneity *between the two regressors and the dependent variable*, whilst number of job changes was entered in observed form. van Dijk & Folmer (1999) assume that the other controls are exogenous. The dependent variable used in the second stage wage equation is the natural logarithm of net hourly wage in April 1985 (weekly earnings after tax - excluding overtime payments, bonuses,etc - divided by weekly hours of work[13]. Education level, unemployment frequency & duration (months), number job changes, breadth of jobsearch (if regional migration was necessary), sex, marital status, occupation, temporary contract, supervisory role, and sectoral effects are all controlled for in the wage equation (These indicators are calculated using interview date as well as retrospective information covering the period Jan. 1980 - April 1985). Due to data limitations, previous labour market status is restricted to the last 5 years. Labour market states considered are employment and unemployment (the self-employed and those out of the labour force are

[12]This strategy is unsatisfactory, given that it imposes the assumption that the wage effect of unemployment history for those living in high unemployment regions is the same for all individuals living in those regions, irrespective of individual ability.

[13]Net hourly wage captures tax regime effects (van Dijk & Folmer 1999).

excluded from the analysis). No information is available about the nature of job separations, thus job separations are likely confounded by individual heterogeneity. Years with current employer (tenure) at interview date is used to proxy specific human capital. General human capital is proxied by years of labour market experience prior to the current job (age - tenure - 15). Both tenure and potential experience are entered as quadratics in order to capture non-linearities in their effect on observed wages. Furthermore, an tenure & potential experience are entered as an interaction.

A wage equation is estimated via OLS. Wages are found to increase with both tenure and experience prior to job offer acceptance. One year of tenure increases wages by 1% in core and 4% in periphery regions, both significant at the 1% level. In line with tenure effects, potential experience is found to have a positive impact of wages of 1% in core and 4% in periphery regions, significant at the 1% level. The returns to tenure peak earlier in core (37) than periphery regions (57), suggesting than in general individuals tend to move on to flatter tenure-wage profiles in periphery regions (van Dijk & Folmer 1999). Interestingly number of job changes is found to have a positive, significant impact on wages in periphery regions. An extra job change is found to carry a 6% wage gain in periphery, but only 2% and insignificant in core regions. Unemployment incidence is found to be significant in core regions but not periphery regions Experiencing one extra unemployment spell carries a significant wage gain of 40% in core regions, whereas in periphery regions the effect is insignificant. In both cases the coefficient is positive, leading the authors to argue that, controlling for unemployment duration, the benefits of career mobility combined with productive job search far outweigh the other mechanisms through which unemployment can impact on wage growth. Furthermore, duration of unemployment is found to be insignificant in periphery and negative and significant in core regions A one month increase in unemployment duration will decrease wages by 3%, *ceteris paribus* (van Dijk & Folmer 1999). This they argue highlights that the positive effects of unemployment incidence are reversed once an unemployment spell exceeds 13 months (van Dijk & Folmer 1999). Key to the paper's contribution, personal characteristics are only found to be significant in core regions. (van Dijk & Folmer 1999) argue that even within a country the relationship between unemployment and wages is likely to be heterogenous. They argue that this is more caused by differences in search and recruiting practises related to the regional labour market, than as a result of intra-national institutional differences which are likely to be small.

The validity of (van Dijk & Folmer 1999)'s results comes under question due to lack of exclusion restrictions. The Heckman Selection model is identified under the joint normality assumption when the same covariates appear in the selection equation and the equation of interest. However, identification will be difficult unless there are many observations in the tails where there is substantial nonlinearity in the Inverse Mills Ratio (Cameron & Trivedi 2005). An exclusion restriction - at least one variable which appears with a non-zero coefficient in the selection equation but does not appear in the equation of interest - is required for identification (Puhani 2000). Furthermore a small sample size makes satisfaction of the joint normality assumption less likely (Newey *et al.* 1990).

Using the Bank of Italy Survey of Italian Households Income & Wealth, 1993 & 1995, Lippi & Ordine (2002) set out to test their hypothesis that pre-existing high levels of unemployment will decrease the informativeness of the labour market status of unemployment as a productivity signal for future employers. Thus, they predict that high levels of unemployment in the region of residence should put downward pressure on the re-employment wage penalty, finding empirical evidence for this hypothesis. As an econometric strategy the Feasible

GLS (within/between) estimator was employed, however no controls were made for selection or gender differences. The key result of the paper is that individual unemployment experiences only carry a wage penalty in the Northern regions, where aggregate unemployment is low. This wage penalty is non-existent in Southern Italian regions where long-term unemployment is common. This result is in line with that of van Dijk & Folmer (1999) for the Netherlands who, correcting for selectivity bias, find that longer unemployment periods carry a significant negative productivity signal in core 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 rather than being seen as a negative productivity signal (van Dijk & Folmer 1999).

3.2.6 Conclusion

This literature raises the following questions:

- Do the van Dijk & Folmer (1999)/ Lippi & Ordine (2002) hypotheses hold when applied to Great Britain?
- How important is regional heterogeneity in determining individuals' wage outcomes?
- Do Arulumpalam's results still hold when adding the extra waves now available in the BHPS?

The van Dijk & Folmer (1999) hypothesis suggests that these individuals' employment experiences will be viewed as less of a negative productivity signal and more a characteristic of the region. The expectation is that individuals in tight labour markets (with high Vacancies/Unemployment ratios) that experience a spell of unemployment will be less scarred relative to observationally equivalent individuals who experience unemployment in loose labour markets. This prediction follows from the hypothesis that these individuals' employment experiences will be viewed as less of a negative productivity signal and more a characteristic of the region (van Dijk & Folmer 1999). This notion has less to do with these individuals' human capital accumulation, and more to do with how employers interpret their unemployment signal. If evidence is found supporting this, there is likely to be substantial heterogeneity across skill groups. Skilled workers are generally viewed as being more geographically mobile than their less-skilled counterparts. Furthermore, skilled workers are likely to have accumulated more general human capital, enabling them to mitigate the stigma effect of unemployment spells. The analysis in the Chapter 5 may shed some light on these possibilities.

3.3 Stepping Stones, Skill Mismatch & Over-education Background

This section reviews selected contributions to the Stepping Stones, Skill Mismatch and Over-education literature, as well as identifying gaps in the literature that raise interesting follow on questions. These questions provide a basis for the research question in Chapter 6. If the long-term economic prospects of the unemployed are to be ensured, then moving them out of unemployment and into high quality employment is key not just for their well being but also for the nation in terms of aggregate welfare gains. In what follows the selected literature is divided in terms of the relevant dimensions of match quality.

3.3.1 Employment Quality: Pay.

"Stability of the overall [earnings] distribution [overtime] does not [necessarily] imply stability for individuals (Stewart & Swaffield 1999)." Drawing on the first 5 waves of the BHPS, Stewart & Swaffield (1999) investigate the persistence of low pay for the bottom end of the income distribution. Of key interest is the extent of mobility within the earnings distribution, and the income groups for which this mobility is limited. Knowing whether low-paid employment leads to high-paid employment in the future, or whether certain groups get stuck in a "low-pay no-pay" cycle is relevant from a policy perspective, as well as being the main research question of this study.

Three low pay thresholds are defined in this study: half the median, half the mean and two thirds of the median of the overall distribution of gross hourly wages, including overtime, for full-time men and women (at adult rates). A pooled Bivariate Probit model is employed, with endogenous sample selection to control for the initial conditions problem. Six labour market states are distinguished: low paid employment; higher paid employment; employed but missing earnings information; self-employment; unemployment; out of the labour force (OLF).

Descriptives suggest the presence of a "low-pay no-pay" cycle. "The low paid are more likely to be out of employment in the next period than those higher up the earnings distribution (Stewart & Swaffield 1999, p.33)." Both males and females are more likely to be in low pay at time t if they were unemployment or Out of the Labour Force (4 times and 6 times more likely for males, respectively, and 2.5 time more likely for females in both incidences). Furthermore, moving from low pay into unemploymeny/OLF significantly increases the chances of moving back into low pay.

An extra year of education before t-1 decreases the likelihood of remaining low paid by 2-3% points, *ceteris paribus*, and the probability of dropping into low pay by 1% point. Training in the 12 months before t-1 decreases the probability of remaining low paid by 5-10% points, and that of dropping into low pay by 2-4% points. Low paid workers with union coverage at t-1 are less likely to remain low paid and less likely to drop into low pay at t. Employees of establishments with more than 25 employees are 7-10% points less likely to remain low paid. Furthermore, women are 15-20% points more likely to remain low paid and 2-3% points more likely to fall into low pay than men.

Controlling for endogenous selection (the initial conditions problem) via Instrumental Variables (instruments: parental variables; socioeconomic group of parents when 14) suggests that the coefficients above are inflated by a magnitude of 2, relative to a specification which controls for initial conditions. However, this result relies on the validity of the instruments: that they affect the level and not the change in low pay status. Granted, the general story remains robust although the magnitudes of estimated coefficients are reduced.

Stewart (2007) revisits this topic, drawing from the first 6 waves of the

BHPS. A dynamic discrete-choice framework is employed, with interview date used as a reference point. The sample is restricted to those in the labour force (employed or ILO unemployed) at the time of interview. The low pay threshold used is defined as two thirds of the median, based on weighted BHPS data. A dynamic random-effects Probit is estimated, using the Heckman (1981) and Wooldridge (2005) estimators to control for the initial conditions problem. Unobserved heterogeneity is controlled for by specifying this as discrete mass-points of unknown distribution (Heckman & Singer 1984). Moreover, correlated errors are allowed by specifying a bivariate error structure, which relaxes the independence assumption.

Main results suggest that, holding observed and unobserved characteristics constant, an individual that was unemployed at time t-1 is twice as likely to be unemployed at t than an individual who was employed at t-1. Moreover, being in low wage employment at time t-1 decreases the probability of being in employment at t as much as being in unemployed at t-1. Formal statistical tests of the differences in these estimates affirm these conclusions. Stewart (2007) argues that low-wage jobs are the main driver of repeat unemployment and that getting a better job substantially decreases the risk of repeat unemployment. Entering low-wage employment at time t from unemployment (t-1) triples the risk of re-entering unemployment, relative to someone whose previous status at t-1 was employment.

Investigating the "hidden brain drain" of *female* part-time work, Connolly & Gregory (2008) draw on two samples from the NESPD and the BHPS. Although quite different data sources, the NESPD and the BHPS have their relative merits. Whilst the former contains information on "actual" contractual, instead of self-reported "usual", hours of work, the NESPD under-samples both part-time workers as well as job movers (Connolly & Gregory 2008). To aid comparability, the samples are restricted to follow individuals aged 22 to 59 on an annual basis over the 1991-2001 period. Moreover, the analysis is restricted to job-to-job transitions. Whether women change occupation on moving from full-time to part-time work, and whether this involves downgrading to occupations with involve lower skill-levels on average is of main interest. To capture occupations skill composition, a detailed fixed ranking is developed (15 occupational groups) using the SOC90 data from the 2000 LFS. This ranking is based on the average level of (highest) qualifications employed in each occupation as well as the similarity of the tasks performed.

Connolly & Gregory (2008) estimate a standard Multinomial Logit for the probability of upgrading/ downgrading relative to remaining in the same occupation, and whether this involves a change in employer and/or full-time employment status. The odds of downgrading are found to be relatively higher for women previously in higher-skilled occupations, with the odds of upgrading found to be highest for those whose previous occupations were low-skilled. In this study the cost of occupational downgrading is emphasized in terms of the number of years of education underutilised. Furthermore, measurement error issues with their approach are highlighted.

> "Evaluating the extent of the underutilisation of formally acquired skills is a rather approximate exercise, as individual unit groups within each of the 15 occupations involve differing numbers of years of post-compulsory education, and the structure, particularly of vocational qualifications, has evolved across age-groups (Connolly & Gregory 2008, p. F69)."

This issue would not be faced to the same extent if the ranking of occupations were allowed to vary across time. The extent of downgrading on occupational change is gauged for each occupation in the 15-point ranking. This exercise is restricted to the NESPD due to small cell size limitations of the BHPS

(Connolly & Gregory 2008). 89% of Teachers and Nurses switched occupation between 1991-2001 without changing occupational group. Out of these, half of teachers and two thirds of nurses moved into lower-skilled jobs, for example, secretarial and carers respectively. The higher up the occupational ranking, the higher the likelihood of downgrading given that there are more occupations to downgrade into (Evans 1999). In terms of managerial posts, "Other managers" are found to have the highest probability of downgrading, 47%, whilst for "Corporate managers" this is slightly lower at 29%. Connolly & Gregory (2008) argue that since experience and on-the-job training usually trumps formal education when it comes to career progression into corporate management, a measure of the underutilisation of high-level skills is not very meaningful for this occupational category given that they are not often a prerequisite. Whilst that may be the case, if future career outcomes are of interest then the underutilisation of their accumulated specific skilled human capital may carry more serious implications for low-formal-qualification-members of this group especially in the face of limited transferability of skills.

Mosthaf *et al.* (2009) ask whether it is better for west German women to remain unemployed and wait for a better job offer than to take up low-wage employment, given that low-wage may increase the likelihood of future "low quality" employment or result in repeat unemployment. If human capital depreciates whilst unemployed, then one would expect that accepting "low quality" employment sooner rather than later would mitigate the future earnings impact of unemployment spells. However, since human capital accumulation is generally low in "low quality" employment, taking up this kind of work may prolong the negative earnings effects. If previous "low quality" employment is seen as a negative productivity signal for "high quality" employers, then engaging in this kind of work may limit future earnings mobility.

The aim of Mosthaf *et al.* (2009) is to investigate (true) state dependence of low-wage employment and how this differs by firm and individual characteristics. Furthermore, the extent of unemployment risk, upward mobility of low wage earners and when low-wage jobs can act as "stepping stones" to high-wage employment. 5 labour market states are distinguished: high-wage employment; low-wage employment (part-time and full-time); ILO unemployment and inactivity (OLF).

The 2000-2006 waves of the German Socio-Economic Panel (GSEOP) are used to investigate the labour market dynamics of western German Women. Women less than 20 in 2000 and greater than 55 in 2006, as well as full-time education to work and work to retirement transitions, are excluded from the analysis. Furthermore, the self-employed, trainees, students, women in disabled employment and women in agriculture are dropped. Monthly unemployment rates capture the effect of the Business Cycle. An unbalanced panel is constructed of all individuals observed in 2000 and 2001. These individuals are followed until the first instance of attrition or until a missing value of an independent variable is encountered. A low-paid job is defined as two thirds of the median hourly gross wage, calculated for each year for the whole German population using a weighted sample to correct for attrition bias. A dynamic Multinomial Logit Model with correlated random effects (Mundlak 1978 methodology) is implemented, using the Wooldridge (2005) estimator to deal with the initial conditions problem.

Being in a low-paid job at t-1 increases the probability of being in a low-paid job at t, and decreases the probability of being in a high-paid job. This is highest for those in low-paid part-time work at t-1. However, low-paid women are better off than unemployed or inactive women, where the probability of transition into high-wage employment is the lowest and the probability of becoming unemployed or inactive again is the highest. Mosthaf *et al.* (2009) argue that this is evidence in favour of the hypothesis that low-wage jobs can be stepping stones to high-wage employment. However, this should be taken in the context that low-wage women working in large firms are found to have lower probabilities of moving into higher paid jobs than their full-time counterparts, and this upward mobility is also found to be lower if these women live alone and/or have young children in their household. The authors acknowledge that this study ignores duration dependence. Taking low-wage employment may be more appropriate for the long-term than short-term unemployed. Furthermore, the effect of a low-wage job may also depend on its duration (Mosthaf *et al.* 2009).

3.3.2 Employment Quality: Stability.

The Stepping Stone hypothesis in its original formulation suggests that temporary jobs (fixed term contracts/temporary help agencies) are stepping stones to permanent employment (Boheim & Taylor 2000; Booth, Francesconi, & Frank 2002). Temporary contracts are generally seen as an instrument for labour market flexibility. However, human capital accumulation may be lower in temporary jobs, resulting in workers getting stuck in these jobs and not being able to progress into permanent employment due to human capital depreciation (Picchio 2008). Picchio (2008) uses the 2000, 2002 and 2004 waves of the Survey of Italian Household's Income and Wealth (SHIW) to assess the strength of the Stepping Stone effect of temporary jobs in Italy. The sample is restricted to individuals aged 15 to 64 in 2000, resulting in a balanced panel of 1677 individuals. Dynamic unobserved effects Probit models are estimated for the probability of having a permanent job. In order to distinguish between true versus spurious state dependence, unobserved heterogeneity is initially specified as a random effect where this is allowed to be correlated with average observed characteristics according to the Chamberlain (1980) methodology. Moreover, initial conditions are incorporated using the Wooldridge (2005) and Heckman (1981) Conditional MLE approaches. Relative to temporary work, a permanent job at time t-1 significantly increases the probability of permanent employment at time t. Furthermore, relative to a temporary job, unemployment at t-1 significantly decreases the chances of permanent employment at t-1. Wald tests confirm the significance of unobserved heterogeneity. The Wooldridge (2005) and Heckman (1981) solutions to the Initial Conditions problem result in marginal effects, evaluated at the sample means, of 16.2 and 13.7 percentage points respectively. Relative to sample descriptives suggesting an average Stepping Stone effect of 29 percentage points, controlling for observables reduces this estimate to 20 percent. Accounting for unobserved heterogeneity and initial conditions reduces this estimate further. This suggests that half of the average Stepping Stone effect is spurious (Picchio 2008). Specifying the unobserved heterogeneity as discrete mass point distributed, as well as using Efficient Generalised Methods of Moments (GMM) and first-differencing techniques (OLS and Instrumental Variables) results in estimates which closely bound the Stepping Stone effect found in the main analysis of 13.7 to 16.2 percentage points, once initial conditions are controlled for. Defining temporary employment as jobs with fixed term contracts and casual work, and employing the 1994-1999 waves of the European Community Household Panel, D'Addio & Rosholm (2005) found evidence that temporary work increases the probability of future labour market exclusion, notably for men. In contrast, for UK, Booth *et al.* (2002) only find a Stepping Stone effect of fixed term and not temporary (casual work) contracts which is strongest for women using BHPS data. Tougher Employment Protection Legislation (EPL) implies higher firing costs and thus a greater incentive for employers to use temporary jobs as a "screening device (Cockx & Picchio 2009a)." However, Cockx & Picchio (2009a) argue that a permanent contract is not necessarily a guarantee of job security, proposing that effective job tenure ($\geq$1 year) as a sign of job security. They also argue that temporary employment is not always short-lasting and thus not necessarily an indicator of "low quality" employment. The definition of a short-lived job used in this study is: $\leq$1 quarter and involuntarily interrupted. "This is likely a lower bound for the conversion of temporary jobs to long-lasting jobs (Cockx & Picchio 2009a, p.3)."

The main research question is whether by accepting short-lived jobs (the treatment), individuals are more likely to enter long-lived jobs than if they had remained unemployed, continuing to search for a stable position Cockx & Picchio (2009a). Micro-simulation techniques are then used to simulate the Average Treatment Effect on the Treated (ATT), based on the estimates in the first stage regressions. Three labour market states are considered: insured (registered) unemployment, employment, and an all absorbing censoring state. Moreover, job-to-job transitions are explicitly modelled in the analysis. The quarterly Belgian Crossroads Bank for Social Security (CBSS), a 100% administrative sample of school-leavers' labour market histories is drawn on. The sample is restricted to 18-25 year olds who in 1998 were still unemployed 9 months after graduation. Since in Belgium school leavers only become entitled to unemployment benefits after 9 months, this simplifies the initial conditions problem to a left censoring issue. The final sample contains the (un)employment histories of 8,921 women and 6,627 men, beginning in 1998 and ending in 2001 (max. 4 years). The detailed set of controls include: firm characteristics (at the beginning of the spell); nationality, region of residence, education (time-invariant); age, quarter of entry into spell, household position (head, single, cohabiting), monthly unemployment benefits, sector, firm size (spell-varying); time until benefit expiration/drop; unemployment rate (time-varying: state of labour market).

A discrete-time Mixed Proportional Hazard (MPH) model is estimated with a flexible piecewise constant baseline hazard. Possible sources of endogeneity include unobserved heterogeneity, endogenous censoring, endogeneity induced by time-varying covariates, and the initial conditions problem. Competing (correlated) risks, multiple spells and time-varying covariates are accounted for in the model specification. Selection on unobservables is controlled for by specifying this as a discrete distribution with unknown number of mass points (Heckman & Singer 1984), where the optimal number of mass points is chosen to minimise the Akaike Information Criterion (AIC) (Gaure *et al.* 2008). Cockx & Picchio (2009a) allow the order, type and duration of the previous spell to proportionally shift the baseline hazard. HORNY & PICCHIO (2009) prove that all parameters in such a model are identified in a continuous-time setting. The authors argues that their approach is appropriate as extensive Monte Carlo analysis by Gaure *et al.* (2008) suggests that the discrete-time approach still produces robust estimates, provided that the likelihood function is correctly specified.The “all absorbing” censoring state is modelled as a separate competing risk so as not to contaminate the destination-specific hazards of interest, in the event of the independent risks assumption failing (van den Berg & Lindeboom 1998). Moreover, the initial conditions problem is controlled for.

Lagged occurrence dependence is found to be more important than lagged duration dependence, however, unlike Doiron & Gorgens (2008) who draw on a survey of Australian youths, lagged duration dependence is also found to be significant. This impact of past unemployment occurrence is found to only increase the likelihood of future unemployment (employment-to-unemployment transitions), and not job-to-job transitions. Increasing previous unemployment spell length by a quarter (4 months) is found to decrease transitions out of employment and into unemployment by 3%, whereas job-to-job transitions decrease by 4% for women. For men, previous unemployment duration is only found to have a negative impact of 3% per quarter on job-to-job transitions.

Youn men and women entering employment from unemployment, rather than a job-to-job transition, are found to be 27%(35%) more likely to be *dismissed*.

The order of unemployment spells is found to be very important. Relative to the first spell (during which no work experience had been accumulated under assumption), the probability of re-employment in the second unemployment spell is 37% higher for men and 75% higher for women. This increases to 70% and 98% in the third spell, for males and females respectively. Cockx & Picchio (2009a) argue that this highlights the importance of work experience. The length of the past job is found to increase job duration. An increase in previous job duration by one quarter decreases job-to-unemployment transitions by 12% for men and 6% for women. However, this also decreases job-to-job transitions by 4% for women. Longer previous (registered) unemployment spells are found to increase the probability that the current job last longer. However, negative duration dependence is also found in the re-employment hazard ($U- >E$). Rejecting short-lived job offers now reduces the probability of finding a long-lived job later on. Moreover, length of previous job increases the probability of re-employment whilst unemployed. This effect increases with the number of previous job spells, but at a decreasing rate. Thus, Cockx & Picchio (2009a) argue the importance of finding a first job quickly, to maximise the Stepping Stone effect.

The simulated ATT lends further support for the Stepping Stone effect. However, this effect is likely to be very heterogenous. "40% of those who accepted short-lived jobs would have speeded up their labour market integration by rejecting these jobs (Cockx & Picchio 2009a, p. 29)." Moreover, the Stone Effect is smaller for the highly educated and those living in low unemployment rate districts. Cockx & Picchio (2009a) do not consider the impact on the level of wages, or on wage growth. Furthermore, the ATT is not generalisable to the whole population (Cameron & Trivedi 2005) and its simulated nature implies sensitivity to correct specification of the empirical model. The study restricts itself to very short-lived jobs, which involuntarily end in unemployment instead of a subsequent job (job-to-job transitions) (Cockx & Picchio 2009a). Thus the Stepping Stone effect is likely to be an underestimate of the population-wide effect if individuals select themselves into a job-to-job transition. The sample only captures the outcomes of long-term unemployed graduates, with no labour market experience (employment spells). The stigma attached to being long-term unemployed, as well as the associated general human capital depreciation, implies that these outcomes are unlikely to be representative. However, this study is restricted to the outcomes of the registered unemployed who are actively searching for work and qualify for active labour market participation. Thus productive search predictions (Lippman & McCall 1976) are more likely to apply for this sub-set of the population than for those who are ILO unemployed. As noted by the authors, if long-term unemployed graduates are more likely to enter minimum wage jobs, then this provides limited scope for downward mobility of wages. An obvious problem with the approach, in terms of the behavioural implications the type of job held, is the backward looking nature of the short-lived/long-lived definition. How does an employee tell how stable the job will be before accepting the wage offer? This information about the status of job security will only accumulate with length of time on the job, and thus the distinction between a secure and un-secure job is unlikely to be time-invariant. This issue would in turn made identify the fixed effect of taking a “short-lived” job, by this study’s definition, problematic at best.

A recent study, by the same authors, considers both the pay and stability dimensions of job quality. Cockx & Picchio (2009b) jointly model the length of unemployment, starting wages and subsequent job tenure in order to assess the impact of unemployment duration on job quality. Contrary to the existing literature, this study does not categorise jobs as low- or high-paid, instead modelling the starting wage as a separate, pre-employment spell, labour market state. By allowing for these competing risks to be correlated in both observables and unobservables, simultaneity between unemployment duration and starting wages is controlled for without the need for instrumental variables (e.g. Addison & Portugal 1989). Furthermore, the sample distribution of starting wages is captured by modelling this as a separate labour market state. The common definition used is two thirds of the median gross hourly wage. Stewart & Swaffield (1999) use the New Earnings Survey (NES) to establish this threshold. Stewart (2007) uses the BHPS survey weights to aggregate the individual-level data to the national level before calculating this threshold. Variation is the construction of this indicator may imply some sensitivity of results to the definitional choice. By only considering the impact on starting wages (post-wage bargaining wage offers), Cockx & Picchio (2009b) ignore the impact on subsequent wage growth. Cockx & Picchio (2009b) draw on the same sample used in Cockx & Picchio (2009a), implementing a similar identification strategy, in order to asses the *causal* impact of unemployment benefit receipt and unemployment duration on the quality (wage and duration) of subsequent work. The study is motivated by the general finding in the literature that the incidence, rather than the duration of unemployment matters more for the impact of unemployment on subsequent wages (e.g. see Arulampalam 2001).

Taking into account initial conditions, wage selectivity and state dependence, key results reject the presence of Unemployment Scarring in Belgium. Micro-simulations suggest that a 1 year increase in unemployment duration increases wages by 1.6% on average for men, with no significant effect for women. Possible explanations put forward include the possibility that being long-term unemployed is already enough of a negative signal to potential employers that any subsequent unemployment duration is unlikely to have a significant impact on future wages. The authors argue that limited flexibility of the centralised

Belgian wage bargaining system, and limited downward wage flexibility, is also likely to imply that wage penalties can only be incurred through occupational downgrading down the wage scale. Moreover, if long-term unemployed graduates are more likely to move into minimum wage jobs then wage reductions will be infeasible due to institutional constraints (Cockx & Picchio 2009b).

Unemployment duration is found to negatively impact on job-to-job transition intensities. An additional quarter of unemployment decreases transition intensities by 4% and 9% for men and women respectively. However, job-to-unemployment transitions are unaffected. Since the study is limited to registered unemployment spells, this is more likely to capture productive search due to monitoring restrictions and eligibility for Active Labour Market Policies. Cockx & Picchio (2009b) interpret this as indirect evidence for Wage Scarring, as by negatively impacting on job-to-job transition intensities unemployment duration could hamper long-term career mobility and the long-run growth of individuals' wage profiles. However, this subsequent impact on wage profiles is not modelled.

Whilst unemployment duration is found to be important, starting wages do not have a significant impact on job tenure for both men and women. "[This is likely due to] the type of bargained wage profile, rather than the starting wage, that might affect job tenure (Cockx & Picchio 2009b, p.27)." Economic theory provided conflicting predictions of this sign effect. If wages are used as an incentive device (Shapiro & Stiglitz 1984), then a positive relationship between starting wage and job tenure would be expected as high wage individuals would have a higher incentive to increase their productivity. However, if match quality is only observed *ex ante* (Jovanovic 1979) then high wage workers will have a higher probability of being in a non-profitable match, implying a predicted negative relationship between starting wages and job tenure (Cockx & Picchio 2009b). However, the authors argue that imperfect information considerations will increase employers' incentives to bargain for a "backloaded" wage profile with a tenure-based remuneration structure.

The general story presented is robust sensitivity checks including controls for Active Labour Market participation, allowing the baseline unemployment exit rate to differ for highly educated workers ($\geq$ university degree) to differ from lower educated workers (to capture endogenous hetereogenous time-variation of the reservation wage) and controls for age variation in the minimum wage (Cockx & Picchio 2009b). Data limitations generally imply that the starting wage is used as a proxy for the offered wage. However, these are likely to diverge for individuals with bargaining power as the starting wage is the product of the wage bargaining process. The results also rely on the MPH assumption for identification, however, initial specifications suggest that varying the identify assumptions leads to similar results when initial conditions are ignored and the censoring state is treated as exogenous.

3.3.3 Employment Quality: Skill Downgrading & Skill Mismatch.

A common premise of many mobility studies is that skilled workers are more mobile than their less-skilled counterparts. Whilst less-skilled workers are less likely to be upwardly mobile, the notion of downward occupational mobility implies that skilled workers are also more likely to take on less-skilled jobs during economic downturns/ downturns in their sectors.

Evans (1999) considers the probability of occupational change (either upgrading or downgrading), *given displacement*, as an alternative to remaining unemployed. Research questions include the characteristics which make downgrading more likely, as well as the how downgrading varies with the business cycle. Moreover, an additional aim is to determine whether all types of occupational change are a source of flexibility in the labour market. Downgrading/upgrading is defined as an occupational change that involves movement down/up the occupational hierarchy, where this ranking is determined by occupations' skill-content. Since this approach restrains individual to be unemployed in the first stage, it does not consider movements within a firm. Reder (1955) argues that firms may use lower hiring standards to "hoard" skilled labour during downturns. Skilled labour embody a higher investment in specific human capital, hence downgrading them into less skilled jobs within the firm may be cheaper if they can be upgraded when things pick up and less skilled workers can be hired externally to fill their places.

The study is based on the 1989 cross-section of the UK Labour Force Survey (LFS). Occupational status at the time of interview and *retrospective* status one year earlier are used to identify occupational change, given the cross-sectional nature of the data. This measure is likely to suffer from measurement error as short labour market spells will tend to be underreported. Furthermore, systematic recall bias is likely to be an issue due to the retrospective nature of the previous labour market status. A ranking of occupations is developed based on the characteristics of the jobs carried out. Six categories are defined: (6) Professionals; (5) Intermediate Professionals; (4) Skilled (non-manual); (3) Skilled (manual); (2) Semi-skilled; and (1) Unskilled. Movements up this scale are defined as upgrading, whereas movements down are defined as downgrading. Connolly & Gregory (2008) define this ranking based on the average highest qualification levels paid in each occupational band. Acemoglu (2001) develops a model of occupational change, where he defines good jobs as high paid and bad jobs as low paid. Manning & Petrongolo (2008) define this by average real hourly wages paid in each occupational band. Whether the skill-content, average pay structure, or stability of a job is the most appropriate measurement of its quality is an issue of controversy. If up-skilling of the workforce is a long-term economic growth strategy, assuming that highly paid jobs are "good" assumes that highly paid jobs are skilled by nature. Results will be sensitive to how the high pay/low pay threshold is defined. Moreover, the wider the classification of the occupational bands, the more likely measurement error is a to be an issue. However, finer bands introduce a tradeoff with numerical accuracy due to small cell size.

A Logit model for the probability of downgrading, given displacement and the individual in question was in full-time employment at t-1, is estimated relative to becoming jobless. Being older is found to decrease the probability of downgrading significantly, consistent with higher tenure individuals possessing higher levels of occupation-specific human capital (Farber 1999). Being white, as well as having a working spouse increases the probability of downgrading. Evans (1999) argues that, due to the family means test, since Unemployment Insurance is withdrawn if a spouse is working more than 24 hours this is essentially an eligibility constraint. Moreover, being ranked higher up the occupational scale developed increases the likelihood of downgrading. Displaced low skilled workers are more likely to move into non-employment than their high skilled counterparts higher up the scale, as the lower down the less jobs to downgrade into (Evans 1999). These results are robust to controls for (bivariate) sample selection and an alternative ranking of occupations based on average wages paid. Consistent with the previous argument, when the sample is stratified into 3 occupational groups (low-skilled, intermediate and high-skilled) education is found to have a significantly larger effect on downgrading the higher is the occupation in question in the ranking.

The probability of being re-employed in a job at the same level in the ranking is estimated, given displacement and full-time education at t-1, relative to downgrading or becoming jobless. Semi-skilled workers are four time more likely to move into non-employment than professionals. However, the more skilled the pre-displacement occupation, the lower the chances of finding an equally skilled job. The previous finding is driven by the fact that those in professional occupations at t-1 are twice as likely to move into semi-skilled work at time t than those that were semi-skilled at t-1 (Evans 1999).

Regional heterogeneity is likely to be relevant in determining this phenomenon. "Since downgrading often involves changing industry, if the propensity to downgrade differs across industries then regional disparities in downgrading could be driven by regional variation in the industrial mix (Evans 1999, p.87)." However this notion is not tested or controlled for.

According to theoretical predications, "skilled jobs become increasingly rationed during recessions, but if the secondary sector is market-clearing then unskilled jobs are freely available. That will increase the relative attractiveness of taking less skilled work, making downgrading counter-cyclical (Evans 1999, p.90)." Descriptives suggest that, contrary to expectation, both upgrading and downgrading are pro-cyclical in nature. This is based on a pseudo-panel (repeated cross-sections) drawn from the LFS, of 18 UK sub-regions from 1986 to 1992, recessionary years. A regional-level analysis is conducted to gauge the determinants of the inflow into upgrading/downgrading. Vacancy rates, wage rates as well as housing payments data are sourced at the sub-regional level. Since individuals cannot be followed over time, Evans (1999) stresses that migration will not be captured. However, no mention is made of commuting, and whether these sub-regional entities approximate self-contained labour markets as TTWA would. The analysis employs fixed and random effects techniques, which Evans (1999) argues are valid if the characteristics of regions evolve slowly over time. The main argument put forward is that migration between sub-regions of the UK is low. Despite these limitations, quantitative evidence is also found in support of the procyclicality of downgrading.

The level of regional inequality, measured by the ratio of the highest wage in the lowest decile of the regional-specific hourly wage distribution to the lowest wage in the top decile, is found to be inversely related to downgrading (significant at the $\simeq$10% level). "The more compressed the wage distribution, the more attractive is downgrading because the relative opportunity cost of unemployment search is high (Evans 1999, p.91)." In contrast, a positive correlation is found with upgrading. "Provided that displaced workers who chose not to search for less skilled work spend longer unemployed, inequality will be positively related to unemployment because it raises the incentive *not* to search for less skilled job (Evans 1999, p/92)." Moreover, in support of this arguement, the lagged unemployment rate is found to be inversely related to downgrading but unrelated to upgrading.

In conclusion, the Evans (1999) raises the following limitations of the study. Data limitations mean that the magnitude of wage losses cannot be gauged. Furthermore, the return to skilled work is likely to be heterogenous depending on match quality and the skill-level of the worker. Since individuals cannot be followed over an extended period, the scarring effect of downgrading down the occupational scale cannot be determined. If upgrading/downgrading is usually associated with industry changes, then it is important to gauge whether this phenomena is concentrated amongst a subset of the population and not evenly distributed. Since industry changes involve loss of industry-specific human capital, this is likely to have a significant impact on subsequent career trajectories and associated wage profiles.

Khalifa (2010) develops a dynamic stochastic general equilibrium model, featuring search frictions, in an attempt to explain both the persistence in total unemployment as well as unemployment across skills. The observation of higher persistence of unemployment over the business cycle for the unskilled acts as a motivation. A two-sector Job Competition model, similar in many respects to that described in Evans (1999), is developed. On-the-job search is allowed. High, at least college education, and low educated workers simultaneously search for high and low skilled vacancies. This framework implicity assumes that human capital is either high or low skilled. The highly educated unemployed, as well as the highly educated in low skill jobs, lose their skills whilst in these labour market states. Economic downturns increase the probability of being unemployed, resulting in skilled workers competing with unskilled workers for unskilled jobs. This competition results in the unskilled being "crowded out" of unskilled jobs and into unemployment. However, the longer mismatched jobs last the more likely that mismatched skilled workers lose their skilled human capital and become unskilled. The dynamic model of occupational downgrading developed in this study suggest a potential mechanism for the higher persistence of unemployment amongst the unskilled, as well as the unemployment persistence. However, it ignores some demand side interactions. For example, employers may anticipate higher turnover levels of the skilled in less-skilled jobs leading to lower demand for their skills and increasing their transition rates into unemployment (Evans 1999). Furthermore, if human capital is occupation-specific, a skilled worker may not be as productive as an unskilled worker at less-skilled tasks (unskilled does not necessarily imply routine).

The Reder (1955) hypothesis predicts that firms will lower their hiring standards during economic expansions and raise them during recessions, in order to keep their wage bill at steady state levels. This mechanism may explain why upgrading may increase during boom phases Employers may raise their recruitment standards during economic downturns. Furthermore, certain worker-types may benefit the most from occupational upgrading during upturns. If employers all raise their standards during downturns, this will have profound implications for the less skilled as pressure from skilled jobseekers squeezes them into unemployment/out of the labour market (Devereux 2002).

3.3.4 Employment Quality: Overeducation/Overqualification.

Over-qualification is an important phenomenon in the labour market, both in terms of future earnings potential but also in terms of career mobility. This affects all spectrums of the earnings and skills distribution, impacting on the prospects of both school leavers, university graduates and even PhDs. As highlighted by a recent Economist study:

> Even graduates who find work outside universities may not fare all that well. One OECD study shows that five years after receiving their degrees, more than 60% of PhDs in Slovakia and more than 45% in Belgium, the Czech Republic, Germany and Spain were still on temporary contracts. Many were postdocs. About one-third of Austria's PhD graduates take jobs unrelated to their degrees. In Germany 13% of all PhD graduates end up in lowly occupations. In the Netherlands the proportion is 21% (Economist 2010).

The Overeducation literature is motivated by the observation of an increasing proportion of highly skilled workings in jobs that were once low-skilled (Borghans & de Grip 2000). This literature argues for two potential underlying mechanisms: technological change has resulted in an upskilling of jobs that is biased towards skilled labour; Overeducation increases the supply of highly skilled workers and leads to 'the crowding out' of the less skilled. The second mechanism, overeducation, has widely been interpreted as skill underutilisation under the premise that higher levels of formal education imply higher levels of on-the-job productivity[14]. Granted, limited direct evidence of skill un-

[14]This is a human capital theory (HCT) based interpretation (Mincer & Polachek 1974). HCT implies that schooling, and not job characteristics, should determine earnings. Alternative interpretations are provided in the literature. The Job Competition model by Thurow (1975) implies that wages are directly determined by requirements of the job and only indirectly by attained schooling. Firms prefer to recruit individuals that are cheaper to train, ranking applicants by training cost. If highly educated workers are cheaper to train then Job Competition could explain why over-qualification arises. Moreover, Signalling Theory

derutilisation exists due to the inherent expense of collecting this information. "Unfortunately, much less is known about how workers' productivity is related to the way in which people use their skills, than about the allocation of workers in the labour market (Borghans & de Grip 2000, pp. 5)."

Overeducation is also important from a education policy viewpoint. If education is heavily subsidised, then gauging the returns to overeducation informs policy makes about the effectiveness of these investments in terms of aggregate welfare gains (Leuven & Oosterbeek 2011)[15]. The main focus of this literature has been on the incidence of, and returns to overeducation. The latter literature attempts to estimate the Duncan & Hoffman (1981) extended wage equation:

$$\ln W_i(t) = \delta_r S_i^r + \delta_o S_i^o + \delta_u S_i^u + x_i'\beta + \varepsilon_i \tag{3.2}$$

where S^r, S^o, and S^o are years of required, over- and under-education on the job. These components have been consistently shown in the literature to be different, however not all data sets have information on actual years of education, introducing potential measurement error issues. An accurate assessment of these components would be invaluable in policy setting, however the measurement of overeducation alone is a controversial issue. Here I give a brief overview, however, this is discussed at length in the chapter 3, section 6. The main problem is assessing job requirements from what is generally self-reported data. Various approaches have been proposed (see section 6), however, the incidence of over-qualification is shown to be highly sensitive to definitional choice. Unobserved heterogeneity is a major issue. Studies which

suggests that education may only have an allocative role, independent of productivity on the job (Spence 1973).

[15] The overeducation literature is reviewed extensively in Borghans & de Grip 2000, McGuinness 2006 as well as Leuven & Oosterbeek 2011.

have been able to construct measures of unobserved ability find ability to be negatively correlated with the likelihood of overeducation (Chevalier & Lindley 2009). Omitted Variable Bias makes it difficult to establish the causal effect of overeducation on earnings. However, since *both* actual and required years of schooling are not random outcomes, this complicates things. In terms of dynamics, over-qualification is not found to be permanent for all sections of the workforce. Sicherman & Galor (1990) develop a theory of career mobility in which individuals accept less-skilled jobs (that they are over-qualified for) with lower wages in exchange for higher promotion prospects. This study presents evidence supporting this notion, moreover, drawing on the 1976-1978 US Panel Study of Income Dynamics, Sicherman (1991) finds high levels of persistence for the majority of over-qualified but upward career mobility for a some. Interestingly, Sicherman (1991) also finds that the under-qualified are more likely to progress up the career lader than their well matched counterparts, interpreting this as evidence of higher unobserved ability levels. Traditionally career progression into skilled jobs through on-the-job experience has been an avenue open to even those with lower levels of qualifications. However, technological change and the increased supply of college educated workers has meant that getting a skilled job increasingly requires *both* education and experience (Léné 2011). A review of the literature relating to key definitional challenges facing the over-qualification literature, directly relevant to this thesis, is included in Chapter 6, Section 6.2.

3.3.5 Conclusion

The existing literature raises some interesting questions that have hitherto been unaddressed. Chapter 6 analyses the the Stepping Stone effect of less-skilled employment. Are less-skilled jobs Stepping Stones to better matches for the over-qualified? How does this vary with gender, the composition of the local labour market and over the business cycle? The impact of Occupational change and the associated Over-Education on labour market outcomes is not explicitly modelled in the literature. Focus is mainly consigned to the less-advantaged. However, increased Occupational Downgrading during economic downturns suggests that skilled workers pose a threat to the job stability of the less-skilled. By jointly modelling skilled and unskilled labour market transitions this endogeneity can be taken into account. Moreover, sub-regional differences in industrial composition suggest a differential impact of economic downturns within a country. Chapter 6 sets out to assess whether this is the case.

Chapter 4

Job Seeker's Allowance in Great Britain: How does the Regional Labour Market affect the length of Job Search?

4.1 Introduction

The design of successful labour market policies and their impact on the effectiveness of the social security system highlights the need for an accurate and rigorous analysis into the impact of regional and individual level determinants on individual labour market outcomes in an integrated framework. Whilst there are many existing studies investigating the influence of individual characteristics and unobserved heterogeneity in determining unemployment incidence and duration, there is a distinct lack of studies providing detailed evidence of the time-varying role of geographical location in driving unemployment experiences (some examples include Kalwij 2004; 2010). Although existing UK studies suggest less of a role when compared to regional level characteristics, regional variation in average unemployment experiences suggests that geography is may be more important than the existing studies have acknowledged. Disentangling its effect is of key interest from a policy perspective. Chapter 2, Section 2.1.1 points to the importance of job arrival rates. Regional variation in job offer arrival rates may help to explain regional variation in unemployment durations (Petrongolo 2001). This is typically proxied by labour market tightness (vacancies/unemployment, V/U, rates), which summarise the job matching process (Kalwij 2010). However, substantial measurement challenges, on-the-job search, and unsystematic variation in the efficiency in which Jobcentres collect and post vacancy data raise significant challenges when proxying the regional context via a single measure.

If socioeconomic groups are geographically concentrated, then lower economic prospects can be expected in mostly lower socioeconomic status areas due to higher unemployment levels, lower spending power, and thus lower levels of economic activity in the local economy. However, geographical location is not a random outcome. Individuals select into this, and changes in the composition of regions means that the relative positions of regions within a country evolves over time. Thus parameterising the regional heterogeneity via fixed effects alone is not a very satisfactory solution.

Establishing the pure regional effect is very challenging, possibly explaining the limited attempts at this. Whilst this aim is beyond the scope of this study, we attempt to give an answer in this direction by developing a comprehensive database matching individual-level unemployment benefit claimant periods from the Joint Unemployment & Vacancies Operating System (JUVOS) to a rich set of regional indicators from sources such as NOMIS and the Department of Work & Pensions (DWP) on a monthly basis. This data is then mapped to the UK geography using the National Statistics Postcode Directory (NSPD), available from UK Borders, allowing the spatial characteristics of regions to be identified. Data on further education institutions and unemployment benefit office locations is included to capture the relevant supply, demand, as well as structural, social and institutional factors of interest. This database allows one to conduct research at a highly disaggregated local authority level in order to answer policy relevant questions. In this paper we link the individual-level JUVOS data to the regional context in which unemployment benefit claimants reside, rather than parameterizing regional heterogeneity through fixed effects.

Differences in labour market institutions are cited as a major explanation of unemployment disparities between countries. However, although institutions do not vary markedly between regions, there is considerable variation in UK regional unemployment rates (incidence) and individuals' experiences (durations). For instance, regional -local authority- ILO[1] unemployment rates varied from 3.3% to 14% over the year 2005. Furthermore, the greater spread in unemployment rates at lower levels of aggregation in the UK is well documented in the literature (Brown & Sessions 1997; Collier 2005). Figure 4.1 contrasts the conditional and unconditional distribution of unemployment durations across Great Britain over the period of investigation: 1999 to 2005. The importance of the job offer arrival rate in explaining average unemployment durations has been highlighted in the theoretical literature (Cahuc & Zylberberg 2004). Moreover, there is strong evidence to suggest that matching varies across regional Jobcentres in the Great Britain (Petrongolo 2001). Unemployment duration has been extensively studied using individual-level unemployment duration data. However, Collier's results suggest the regional context to be significant (Collier 2005). Despite the vast unemployment duration literature, there are surprisingly few studies which explicitly take into account the regional context. Most use parametric approaches, and regional effects are implicitly accounted for in some studies (for the UK see Kalwij 2004;

[1]International Labour Organisation.

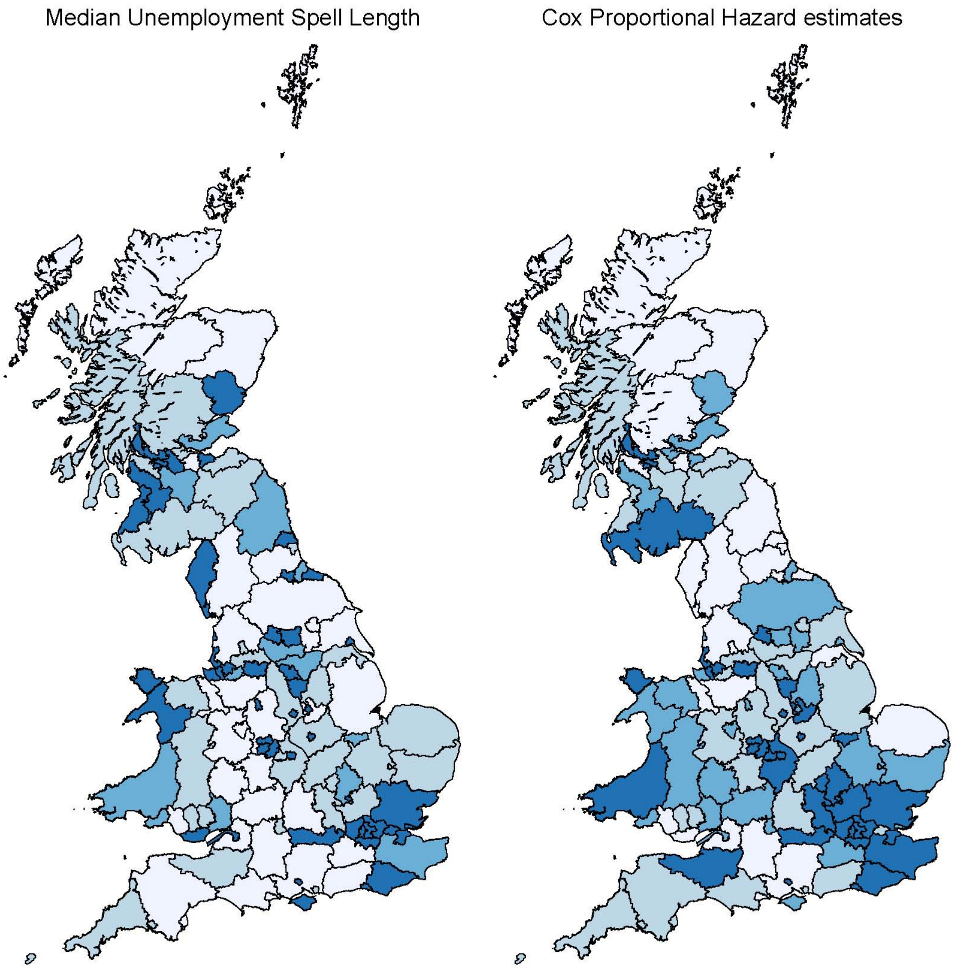


Figure 4.1: Unemployment Duration: Distribution of median unemployment spell length, versus conditional Cox Proportional Hazard estimates*, by NUTS3 region over the period 1999-2005. (*Darker = Worse in terms of unemployment experiences)

Brown & Sessions 1997; for the Netherlands see Folmer & van Dijk 1988) via fixed effects.

Studies looking at the impact of regional-level indicators like local unemployment rates and local labour market tightness on individuals' unemployment experiences include Meyer (1990) for the US and Petrongolo (2001) for the UK. However very few studies have analysed individual unemployment duration at the UK regional level. We are only aware of Collier's study which focusses exclusively on the county of Kent (Collier 2005). Adopting a structural job search model and using detailed (unique) individual-level survey data, the author concludes that differences in regional labour market characteristics (notably regional variation in job offer arrival rates) may matter more than individual heterogeneity for unemployment experiences. This result is in contrast to more recent results for other countries. Using detailed individual-level administrative data, Arntz & Wilke (2009) do not observe a strong effect of the regional labour market on unemployment duration in Germany. They conclude that regional policies may have a smaller effect than commonly thought.

The standard job search model assumes that the distribution of wages offers is exogenous (Atkinson & Micklewright 1991). Theoretical job search literature models the individual job finding probability as a function of the job offer arrival probability as well as the probability of job offer acceptance (Rogerson *et al.* (2005) provide a detailed survey. See Section 2 for more information). The former will be influenced by individual productivity, human capital accumulated, and local demand conditions whereas the latter will be influenced by individuals' reservation wages as well as (local) demand conditions (Petrongolo 2001)[2]. However, studies investigating the importance of geography have generally limited themselves to single indicators like the V/U ratio and fixed effects. Given the attempt to model the regional environment in which individuals live and conduct their job search, the relevant local demand conditions will be those of self contained local labour markets, which Petrongolo (2001) approximates by using regional indicators at the 'Travel-To-Work-Area' (TTWA) level of aggregation. Her study reaches the conclusion that regional labour market tightness is negatively related to, whereas the stock of jobseekers in the region of residence impacts positively on, individual re-employment probability (Petrongolo 2001). This result is found to be insignificant for females, which

[2]Lancaster (1979) proxies the former by the local unemployment rate in the region where the individual resides and the latter by the individual reservation wage.

the author suggest could be an artifact of the data source (unemployment benefits offices) and heterogeneity in job search strategies by gender resulting in males being over-represented in the sample of registered claimants. Given the observation period, 1987, this result may not be generalisable to future time periods, due to increased female participation in the labour force during the 90s.

Whilst higher local unemployment rates may have a significant spell lengthening effect, *ceteris paribus*, over time an increase in local unemployment rates could actually shorten spells as layoffs increase during economic downturns and these job separations are precisely the type that carry the least 'stigma' in terms of future re-employment probability (Meyer 1990). Furthermore, the advantage of the legislated requirement of two weeks written notice before termination of contract should give laid off individuals the added advantage of an early start to job search (Arulampalam 2001), relative to other job separation types.

Looking at regional unemployment in the UK, Martin (1997) provides suggestive evidence, via cointegration analysis, that the pattern of regional unemployment disparities exhibited significant geographical persistence since the 1960s. This is of great concern given that individuals experiencing unemployment earlier on in their life are more likely to experience it later on (Gregg 2001). Furthermore, Atkinson & Micklewright (1991) highlight that being unemployed at time t makes it more likely to be unemployed at time $t+1$ ('negative duration dependence'). Petrongolo (2001) finds strong evidence of negative duration dependence in the UK. However, it is important to distinguish between spurious & genuine state dependency (Collier 2005), as both genuine state dependency and unobserved ability of the unemployed can explain the observation of 'negative duration dependence'. Controlling for unobserved heterogeneity will avoid spurious correlations between the probability of leaving unemployment and elapsed duration (Lancaster 1979). Using a Mixed Proportional Hazard model, van den Berg & van Ours (1994) found evidence of negative duration dependence for UK men, whereas heterogeneity was insignificant. This result accords with those of Petrongolo (2001) for both UK men and women. Table 1 summarises selected unemployment duration literature in the context of regional effects.

We see scope to exceed the previous work in two aspects. Our data set is richer, using individual-level administrative unemployment benefit claim periods linked with to institutional & regional variables at a low level of aggregation. This allows us to better capture the regional context and thus *regional variation in job arrival rates*. As an empirical strategy Figure 4.1 suggests that our approach is informative as, after conditioning on observed factors in the Cox proportional hazard model, we observe quite a different distribution of unemployment durations relative to the unconditional distribution. From a methodological point of view, we adopt a flexible censored quantile regression approach to estimating conditional re-employment hazards. The quantile regression framework allows us to capture different effects on short- and long-term claimant periods in the same model. In addition, this approach is more flexible than standard techniques, as even in the case of the semi-parametric Cox proportional hazards (PH) model (Cox 1972) the sign of the effect of a regressor is restricted to be the same across all quantiles of the conditional distribution. Rather than the usual conditional mean, our approach employs a conditional quantile function which is unaffected by outlier observations. This implies that results are also robust to the shape of the error distribution.

Non-parametric conditional hazard rates are estimated from the quantile regression estimates using a resampling method similar to Machado *et al.* (2006). Since this econometric model imposes less structure, the resulting conditional hazard rates can be disproportional and they can even cross. Our estimation results obtained by the censored quantile regressions provide evidence of several violations of the proportional hazard assumption.

The structure of the paper is as follows. The next section provides a detailed account of the relevant institutional setup. Following this, we briefly cover the data set construction, variable selection[3], as well as the individual and regional level data included[4]. The methodology exploited as well as the empirical results are considered in the following sections. Subsequently, relevant policy implications are detailed in light of the analysis.

[3]For a detailed exposition of the data preparation steps, see Appendix D.

[4]The procedure for linking the individual & regional levels is documented in appendix A2.

Table 4.1: Summary of selected relevant literature.

Paper	Data	Sample & Regional Controls	Key Findings: Individual versus Regional Level?
Lancaster (1979)	UK Cross-section, Representative Survey of registered jobseekers, 1973.	479 unskilled jobseekers actively looking for work (self-reported). 18% found a job between selection and interview (5 weeks later). Sample male dominated, however includes 32 single females. *Unemployment definition:* Registered unemployment. *Regional controls:* State unemployment rate	A significant -VE effect of state unemployment is found, however this significance of this effect is sensitive to specification (significance is lost once controls for the baseline hazard and individual-level unobserved heterogeneity are included).
Meyer (1990)	US Continuous Wage & Benefit Histories UI Administrative data, 1978-83$^{\natural}$.	3,365 observations. *Unemployment definition:* Registered unemployment. *Regional controls:* State unemployment rate & fixed effects. Continuous-time duration model with nonparametric baseline hazard (Prentice-Gloeckler approach). Gamma distributed individual-level unobserved heterogeneity.	Regional effects are found to be significant. A 1 percentage point increase in the state unemployment rate reduces the re-employment hazard by 2.4%, *ceteris paribus.*
Folmer & van Dijk (1988)	Netherlands Cross-section, Representative Labour Force Survey, 1979.	Differentiates between frictional unemployment ($<$4 months), medium-term unemployed (4-11 months), and long-term unemployed ($\geq$12 months). *Unemployment definition:* Everyone looking for a job, independent of hours willing to work. *Regional controls:* Fixed effects.	Regional level is less significant. Personal characteristics drive unemployment experiences. Elders are over-represented amongst the long-term unemployment, whilst the highly educated mostly experience frictional unemployment.
Brown & Sessions (1997)	UK Non-random Cross-Sectional Survey, 1986-91.	15,519 individuals (1,224 experiencing unemployment). Lack of individual-level unobserved heterogeneity controls. *Unemployment definition:* Registered, actively seeking but not registered, or waiting to take job already accepted. *Regional controls:* Fixed effects.	Regional level is less significant, however regional variation in unemployment risks remains even after controlling for individual characteristics (especially for men).
Petrongolo (2001)	UK Cross-section, Representative Survey of British men & women beginning registered unemployment at 88 selected Unemployment Benefit Offices in the 4 weeks starting 16 March, 1987	1,239 men; 706 women. *Unemployment definition:* Registered unemployment. *Regional controls:* Travel-To-Work Area Unemployment/ Vacancies rate, stock of vacancies & stock of jobseekers.	Local labour market conditions affect the reemployment prospects of men but not women. Whilst the individual level is significant in both cases, it is the sole driver of reemployment hazards for females in the sample. Interestingly, unobserved heterogeneity is more significant for females (NB. sample includes both single and married females).

Continued on next page

Table 4.1 – continued from previous page

Paper	Data	Sample & Regional Controls	Key Findings: Individual versus Regional Level?
Kalwij (2004)	UK Joint Unemployment & Vacancies Operating System (Admin. data), Males, 18-34, 1982q4-1998q1	*Unemployment definition:* Registered unemployment (Claimant Count). Stable employment: out of the claimant count for $\geq$2 years. *Regional controls:* Fixed effects. Government Office Region (time-varying, regional) Gross Domestic Product. Data limitations mean that implicitly assumes non-employment is employment. Individual-level unobserved heterogeneity: Heckman & Singer (1984).	Strong evidence of negative duration dependence. Structural employment instability drives repeat unemployment. Individual heterogeneity affects the hazard of re-entering unemployment more than the hazard of re-employment, suggesting that the incidence rather than the duration of unemployment matters most. This suggests that regional heterogeneity may be more important for unemployment incidence♮. 73% of young men find stable employment by 35, however the rest experience repeat unemployment.
Kalwij (2010)	UK Joint Unemployment & Vacancies Operating System (Admin. data), Males, 18-34, 1982q4-1998q1	*Unemployment definition:* Registered unemployment (Claimant Count). Stable employment: out of the claimant count for $\geq$2 years. *Regional controls:* Fixed effects. Government Office Region (time-varying, regional) Vacancies/Unemployment (V/U) Ratio. Tight (Slack) labour market: $\geq$ (<) 90th (10th) percentile of V/U distribution (base = Median). Data limitations mean that implicitly assumes non-employment is employment. Individual-level unobserved heterogeneity: Heckman & Singer (1984).	Re-employment probability decreases by 65% in the first 2 years of unemployment. Males entering unemployment in tight labour markets are 21% less likely to find re-employment than those in slack labour markets. When the labour market is tight, re-employment probability is 35% higher than when it is slack. Changes in the composition of unemployment flows, and not individual duration dependence, is the main determinant of this systematic variation in average duration dependence.
Key studies: Detailed Individual & Regional-level Heterogeneity			
Collier (2005)	England (Kent). Unique cross-sectional Survey of Claimant Count, 1992.	4,872 unemployed. Direct information on reservation wages and job search activity. *Unemployment definition:* Registered unemployment. *Regional controls:* Regional-level time-invariant variables & fixed effects.	Duration model & 2-stage IV. Regional level more significant. Significant impact of regional location on reservation wages (3%-10% across districts of Kent). Regional variation in labour market opportunities implies very strong and significant impacts on unemployment durations (11%-56% higher & up to 42% lower than the reference district). But evidence of poor long-term employment prospects in regions with shortest average unemployment durations (due to high labour market "churning").
Arntz & Wilke (2009)	Germany IEBS (employer-employee matched admin. data), Males & Single Females, 2000-02	*Unemployment definition:* Registered unemployment. *Regional controls:* detailed time-varying regional-level variables & fixed effects	Individual work history is the driving force behind unemployment durations, regional factors are less significant.

See Chapter 3.1 for a more detail analysis of the relevant literature.

♮ I thank an anonymous referee for stressing this point. Given the data limitations at hand, an accurate assessment of this was not possible using Administrative data.

4.2 Institutional setup

Unemployment benefits (Job Seeker's Allowance, JSA) are administered by the Jobcentre Plus which is a part of the Department for Work and Pensions (DWP). As in many other countries, the number of people on unemployment benefits in the UK and the number of people unemployed according to the International Labour Organisation's (ILO) definition do not necessarily coincide.

Jobseeker's Allowance is the main benefit for people who are out of work. In order to get Job Seeker's Allowance, an individual must be able to work for at least 40 hours a week and have been actively looking for work. There are two types of JSA: The first is called 'Contribution-based Jobseeker's Allowance' and lasts for up to six months (182 days), subject to eligibility. An unemployed person gets Contribution-based Jobseeker's Allowance if he or she paid or was credited with class 1 National Insurance (NI) contributions in the preceding 2 tax years. The other is based on a family Means test, which includes personal and/or family income and savings, whichever is relevant given an individual's circumstances (single/married/cohabiting). Unlike the Contributions-based JSA, this Means-tested JSA can be granted for an indefinite period. This is called 'Income-based Jobseeker's Allowance'. Thus, type one requires that the individual has paid enough national Insurance on income and the second requires that current household income and savings are below a certain threshold.

Independent of the type of JSA, the level of unemployment benefits are the same and do not depend on the pre-unemployment wage. Since April 2008 the weekly level has been set at £47.95 for individuals aged 16 - 24 and £60.50 for those aged 25 or over. However, the level of benefits can increase, depending on household size. This implies that the JSA wage replacement rate is in general very low for previous high earners, an important difference when compared to many other European countries with more generous income-related benefits.

The receipt of other benefits may make an individual ineligible for JSA. Quitting a job voluntarily may lead to a benefit sanction of up to 26 weeks. In order to remain eligible for entitlements, the unemployed must visit the Jobcentre at least once every two weeks, and provide evidence that they have been actively looking for employment[5] and are ready to work. In the UK, eligible individuals must be normally between 18-65 years and have a jobseekers agreement with the jobcentre. For more details on the institutional setup see Jobcentre (2008).

The Jobcentre Plus operates across 8 major regions which cover the whole of Britain (excluding Northern Ireland). It maintains over 1,000 offices, amongst which includes back office branches and call centres. The administrative and institutional structure is generally the same across the country, whilst intermittent internal restructuring and the introduction of nationwide policies may lead to temporary regional disparities. For example, the New Deal Programme was first introduced in pilot regions before being implemented in the rest of the country. However, we are not aware of permanent regional differences in the institutional setup of the programme.

Jobcentres administer the main active labour market policy programme: the New Deal Programme. This is a programme that gives people on benefits additional support, including training and preparing for work, in order to improve their employment prospects. Whilst eligibility for this programme is the same nationwide, there are considerable regional and local disparities in the share of eligible individuals starting the scheme. The New Deal for Young People programme is compulsory for JSA claimants aged 18-24 after 6 months. Investigating the impact of RESTART, Dolton & ONeill (1996)

[5] There are various ways of providing this evidence, as highlighted in the JSA brochure: "You should do at least 3 things every week. This could include writing a CV or speaking to employers (Jobcentre 2008, pg.10)."

highlight that self-selection on to the scheme may be an issue due to perceived re-employment prospects. Tighter monitoring restrictions, as well as poor re-employment prospects, make exits to alternative labour market states, e.g. Income Support, a more attractive proposition.

4.3 Data

Our analysis bases on comprehensive linked individual-regional level data from Great Britain.

Individual data. We use the JUVOS (Joint Unemployment and Vacancies Operating System) cohort, which is a randomised 5% sample of all benefit claimants. This data is organised into daily spells relating to individual unemployment benefit claim periods. See Ward & Bird (1995) for a general description of the JUVOS. Our version covers the period 1982 to June 2007. The data is available as a scientific use file from the Office for National Statistics. We restrict our sample to spells starting from the 1st of January 1999 to 31st of December 2005, censoring all spells ongoing on the 8th of August 2007.

It is well known that the claimant count-based and ILO-defined unemployment measures diverge, notably following the 1996 introduction of JSA. Wilke (2009) proposes ways to deal with the limitation in the JUVOS of not being able to identify the true length of unemployment periods, as well as the gaps in individuals' employment histories due to lack of matched administrative data. In Wilke (2009) study, the author suggests several implementations of unemployment duration in the JUVOS as, in many cases, single claim spells will not coincide with the true duration of unemployment. By using the reason for leaving markers at the end of claim periods, it develops bounds for the true level of unemployment as well as enabling the use of a competing risks approach with respect to destination state.

In this paper we consider durations of continuous receipt of unemployment benefits (Concept 1 of Wilke 2009). This is a lower bound for the true unemployment duration and should not contain periods other than claimant unemployment. By restricting the sample to spells with sufficient foregoing employment duration, this should ensure that, in most cases, the start of an unemployment spell equals the start of a claimant spell. This selection limits our study to re-employment prospects of the registered unemployed entering the current claimant period via a spell of employment. Thus our results are not generalisable to the experiences of the full claimant population. Since JSA is means tested after six months, we face the problem of attrition in particular for individuals with an employed spouse[6]. Thus we restrict our sample to single males and single females aged 18-65 and 18-59 respectively since, in the case of singles, the benefit duration is more likely to bear a closer resemblance to the true unemployment duration for this sub-group of the population[7]. Removing individuals aged 16-18 minimises the likelihood of capturing dependent teenagers still living with their parents. Our duration analysis is therefore not an analysis of ILO unemployment duration. However, our sample of claim periods should be comparable to unemployment durations, as individuals are likely to be entitled for JSA for the duration of unemployment. 2.5% of spells in our data are right censored at the end of the observation period (8th of August 2007). Following Rosholm (2001), we also right censor observations with exits to states other than employment. Based on the definition, 39.1% of observations are right censored in the final sample. This approach is feasible given the independent risks assumption commonly employed when using the Cox Proportional Hazards model (Cox 1972). This assumption implies that the

[6]Due to the family Means test

[7]Using German data, Arntz & Wilke (2009) show that empirical results for single males and females are quite similar while married males and females possess different result patterns. This is likely due to the well documented labour market attachment differences between married males and females (Kalwij 2004).

partial likelihood can be considered the sum of the destination-specific hazards, without having to explicitly model the censoring mechanism as one would in a correlated risks setting (Cameron & Trivedi 2005). Conditional on the validity of the independent risks assumption, our empirical strategy is consistent and takes into account the competing risks structure of the JUVOS, without the need to separate these alternative risks [8]. Consistency is enhanced by allowing the destination-specific hazards to be fully flexible in the semi-parametric approaches adopted herein.

As with most administrative individual data, the JUVOS is handicapped by a limited covariate set. Information contained in the JUVOS includes: start & end date of claims, gender, age and marital/cohabiting status. Following Wilke (2009), we refer to the 2000 Standard Occupational Classification (SOC2000) aggregating the 4-digit occupational codes in the JUVOS to the 1-digit level. We then further group these into 5 representative categories: elementary. manufacturing, trade/services, technical and senior/professional (see Wilke 2009). van den Berg & van Ours (1994) find that the season of entry onto the unemployment register impacts significantly on exit probabilities, leading us to control for seasonal influences by including quarterly fixed effects. Calendar time effects are indirectly controlled for via year dummies, which should also account for business cycle effects over the period of observation (Lüdemann *et al.* 2006).

Using linked German administrative data, Arntz & Wilke (2009) found a strong influence of individual heterogeneity - notably work history - on unemployment durations, whereas regional factors were found to be less important. The importance of work history is also highlighted in the individual-level study by Lüdemann *et al.* (2006). Collier's (2005) results suggest the opposite, using unique individual-level survey data for the English county of Kent, finding

[8] See Chapter 2, Section 2.2.1 for a brief discussion of competing risks models

Table 4.2: Work History Variables

Variable Name	Description
Active Labour Market Participation	Individual engaged in at least one past Active Labour Market Programme participation.
Long-Term Unemployment	Individual experienced at least one period of long-term unemployment in the past (>365 days).
Incapacity Benefits	Individual claimed incapacity benefits on at least one occasion, on exiting claimant unemployment in the past.
Income Support	Individual claimed income support on at least one occasion, on exiting claimant unemployment in the past.

individual characteristics to be less important than regional macroeconomic environment. Taking this into account, we control for individual work history, using the measures defined in Table 4.2. Age and gender are also included in order to control for socio demographic factors.

Our final sample consists of about 187,000 spells, a descriptive summary of which can be found in the appendix, Table A.1.

Regional data. Regional variation in job offer arrival rates may help to explain regional variation in unemployment durations (Petrongolo 2001). Kalwij (2010) captures regional variation in job offer arrival rates via labour market tightness. Labour market tightness, which summarises the efficiency of the job matching process, is usually proxied by vacancies/unemployment (V/U) rates. However, substantial challenges exist in measuring these rates, due to underreporting of vacancy data to Jobcentres and the reliance on claimant count data to proxy unemployment rates. Moreover this measure ignores competition from employees engaged in on-the-job search. In addition to these measurement challenges, Jobcentres may vary unsystematically in the efficiency in which they collect and post vacancy data (implying that fixed effects will not control for this).

Geographical differences in aggregate vacancies to unemployment ratios could also be driven by regional variation in the average propensity to use alternative channels to advertise vacancies. This is likely to be related to the industrial mix of the region in question and thus the skill requirements of employers. Employers requiring employees with highly specialised skills are more likely to acquire these through specialist recruitment agencies. Öberg & Oscarsson (1979) observe that individuals with similar labour market characteristics tend to gravitate to specific regions. This suggests that the evolution of compositional changes as well as time-varying regional demographics are of interest in determining what is influencing individuals' unemployment experiences in these geographies. Given the challenges noted above, single measures like the V/U ratio, combined with fixed effects, do not lend themselves well to capturing the regional variation in job offer arrival rates. This motivates the detailed approach to modelling the regional context adopted herein.

Regional-level data was sourced from the quarterly Local Area Labour Force Survey, available from UK Data Archive. Regional data was also sourced from other providers, however missing values limited the final covariate set (e.g. NOMIS censors all observations with values less than 500, implying that small area data is likely to be affected). Continuous variables at the regional level were standardised across regions by month, the shortest interval in the regional dataset. We link the regional-level to the individual-level data by claimant spell start month, since we lack continuous daily data on regional characteristics and individual information is only captured at the beginning of a claimant spell[9]. The final data set consisted of 60 possible covariates at the individual and 160 at the regional level of aggregation.

[9]For a detailed exposition of the data preparation steps, see Appendix D. For details on how this link was created see Appendix A2.

In order to aid variable selection and to avoid including highly correlated regional indicators into the model, we implemented cluster analysis techniques to class regional variables into representative groups. The Clustering of Variables Around Latent Variables (CLV) routine by Vigneau & Qannari (2003) was used. This is a two-stage routine which implements hierarchical clustering analysis followed by a partitioning algorithm, thus capturing the benefits of both approaches. This method clusters highly correlated variables together, regardless of the direction of this correlation. This allows us to select a variable to represent the information captured by the other neighbouring covariates. Given data availability issues, certain variables would be more attractive than others. This approach implies that, in addition to motivation from previous literature, this selection is not arbitrary and based on economic and statistical criteria. The correlation matrix in Table 4.3 illustrates the performance of this process.

Following Arntz & Wilke (2009), regional data was sourced in order to completely characterise the local environment in which individuals reside. Regional variables were clustered into 5 representative groups, capturing the relevant supply, demand, as well as structural, social and institutional factors of interest.

Table 4.3: Correlation Matrix of Regional Indicators (Correlation coefficients over 0.3, and Local Authority level indicators, underlined).

Regional Vars	**Urban**	**Access**	**Uni**	**Skill**	**GDP**	**△GDP**	**Unemp**	**△Unemp**	**Unemp Flow**	**NREG**	**NDYP**
Accessible(*)	1										
Urban(#)	0.302	1									
University Present	0.059	0.133	1								
Skill Intensity	0.079	0.138	0.050	1							
GDPPH	0.121	0.250	0.124	0.403	1						
△ GDP(**)	0.087	0.132	-0.001	0.185	0.198	1					
ILO Unemp.	0.045	0.189	0.038	-0.149	0.042	0.016	1				
△ ILO Unemp.(**)	-0.071	-0.198	-0.079	0.097	-0.041	0.016	-0.141	1			
Flow of Unemp.(♠)	0.006	0.124	-0.002	-0.452	-0.154	-0.096	0.522	-0.326	1		
SME Startups(♠)	0.010	0.103	0.049	0.605	0.590	0.205	-0.108	0.097	-0.457	1	
18-24 New Deal Starters(§)	-0.027	-0.075	-0.006	-0.014	-0.039	-0.017	-0.090	0.074	-0.078	-0.012	1

National Statistics Postcode Directory (NSPD) population density based. * UK/Wales - DEFRA Local Authority based. Scotland - NSPD-based, Local Authority with >90% Output Areas Accessible (<30 mins drive from large (>10,000) urban conurbation).
** 3 year moving average. Medium-term evolution of region. §- NB. Interacted with eligibility at individual level (aged 18-24). ♠ - Proportion of resident population. SME - Small Business.
Time-varying variables standardised across region, by month. Evolution of where a region sits in regional distribution.
Variable selection to fully characterise time-varying regional heterogeneity: Economic theory + Clustering of Variables Around Latent Variables Vigneau & Qannari (2003).

Supply & Demand: Local ILO unemployment rates were used in order to indirectly capture regional 'labour market tightness'. An alternative proxy for 'labour market tightness' is the unemployment/vacancy ratio. However, this indicator is plagued by data quality issues due to significant changes to Jobcentre Plus procedures for handling vacancies in 2001[10]. The retrospective average 12 quarter change in ILO unemployment is included as a proxy for the medium-term evolution of local supply and demand imbalance.

Local economic performance: Regional Gross Domestic Product (GDP) and (3 year average) change in GDP proxy for the level and medium-term evolution in economic activity in a region. In addition, the rate of new business startups is a further indicator of economic activity. Since GDP data is unavailable at the aggregation level of interest, we use unadjusted quarterly Gross Value Added (GVA) as a proxy. This workplace-based measure, allocated to the region in which commuters live, is reported at basic prices[11]. The retrospective 3 year average change in GDP per head is used as a medium-term measure of this phenomenon, as annual changes are likely to be picking up the effects of transitory shocks at the national level. Table 4.3 shows only a weak correlation between these GDP indicators of .198. One would expect prosperous areas to have high levels of GDP, however the magnitude of the GDP growth effect for these areas is ambiguous. Due to this being a residence-based measure, areas with high levels of GDP may be not have high-levels of Economic Activity

[10]The effect being that vacancy statistics are not comparable over time (Bentley 2005). Regressions on a reduced dataset including the unemployment/vacancy ratio as a control find this indicator to be insignificant once other controls are included. Moreover, the aggregate Claimant Count is continuously available from NOMIS over the period of interest. Unfortunately a breakdown of the Claimant Count by occupation is not: availability is restricted to the 1996 to 2000 and 2005+ periods only. Furthermore, whilst there is only a one year gap in notified vacancy statistics by country or government office region, at lower levels of aggregation this is two years. Measurement error in the underlying vacancy statistics suggest that local ILO unemployment rates may be a better proxy.

[11]Deflated for changes in prices over time and across regions (ONS 2007).

due to commuting weakening this relationship. If these areas have high levels of GDP due to historical factors unrelated to economic activity, this will weaken the correlation between the two GDP indicators employed. The rate of business startups is proxied by the number of new businesses registering for VAT each year as a proportion of the resident population. Due to their size, this indicator will not include sole proprietors. However, since the majority of VAT-registered businesses employ less than 50 employees, this indicator is capturing small business activity. Less than half of UK businesses are registered for VAT (NOMIS 2009). It could be argued that this measure is more likely to capture the economic performance of a region than an indicator capturing the activity of larger enterprises. Small businesses are more likely to engage in more localized business activity relative to larger multi-plant firms. They are also more likely to respond to fluctuations in regional economic performance than larger firms with less liquidity constraints.

Social Structure: We define Skill Intensity as the proportion of all employees aged 16 & over working in the following occupational classifications: Managers & Senior Officials; Professionals; Associated Professionals & Technical; Admin. & Secretarial; & Skilled Trades [12]. This measure proved to be highly correlated with educational attainment rates, constructed from the same source. Education attainment and income levels are assumed to be linked through productivity by Human Capital Theory (Becker 1964). Since educational attainment and skill intensity are highly correlated, it would then be expected that individuals living in skill intensive areas would experience higher job offer arrival rates. Their unemployment spells would thus be expected to be shorter[13]. However, the impact of skill level on unemployment duration is likely

[12]It is acknowledged that this measure is likely to suffer from measurement error due to heterogeneity of skill-intensities within detailed occupational categories.

[13]The implicit assumption is one of *perfect information*, that an individual's education

to be endogenous due to the fact that higher job offer arrival rates are likely to push up reservation wages. The effect of this would lead affected individuals to be more selective about the job offers they accept and in turn lengthening unemployment periods (Mortensen 1970). Furthermore, the institutional context (section 4.2) and monitoring restrictions that the UK unemployment benefits system places on job offer acceptance/rejection, suggest that the impact of this covariate is an empirical question.

Institutional Organization: We tried to collect any kind of information about the internal structure of the jobcentre branches but our requests were rejected by the DWP. Given the shortage of information and given the nation-wide identical entitlements for participation in the New Deal Programme, it is therefore difficult to control for the institutional organization. However, we have constructed one indicator, the New Deal for Young People Starters as a proportion of the eligible claimant count. In our analysis this variable is interacted with individuals being aged 18-24. Note that we do not include the base effect of this variable due to multicollinearity. A negative -shortening- effect of this variable would indicate that local jobcentres are more likely to assign eligible individuals to the New Deal Programme if the local labour market offers better re-employment opportunities.

Structural indicators are included in order to characterise the type of region.

Unemployment Dynamics: Regions with high levels of seasonal employment,

level an accurate signal of true productivity and does not pick up unobserved heterogeneity, *viz.* Signal Theory (Spence 1973; Silles 2008). In support of the assumed link between income level and educational attainment, Silles (2008) finds that higher levels of education are always associated with higher earnings in the UK, however whether Human Capital or Search Theory can explain this as a causal relationship is a debatable given the influence of confounding factors like family background (Angrist & Krueger 1999).

proxied by the 'flow of unemployed as a proportion of the resident population', are more likely to be characterised by longer unemployment spells as the sample median unemployment duration is around two months.

Urban/Rural indicator: Two versions of this variable were sourced. One from the National Statistics Postcode Directory (NSPD) and one from the Department of Environment, Food & Rural Affairs (DEFRA). For England & Wales, the NSPD indicator, a population density-based measure, is derived using the 21st of July 2004 release of the National Statistics Rural & Urban Classification of Output Areas (NSPD 2007). This Output Area-based indicator is not valid for higher levels of aggregation which may include a mixture of rural and urban output areas based on the definitions used. For Scotland, areas are defined as rural if they have less than 3,000 inhabitants (NSPD 2007). The DEFRA classification is based on local authorities, but is only available for England[14]. The correlation between these two measures is low, .56 by our calculations. Given the superiority of the DEFRA classification, where it was available it was implemented, and where not the NSPD definition was used, implying that this indicator involves some measurement error for Scotland and Wales.

Accessibility: Exploiting the rich data available in the NSPD, the sparsity of the surrounding area was used in order to define whether a local authority was accessible or remote in the case of England and Wales. Driving distance to the nearest large settlement is used as a proxy in the case of Scotland[15]. One would expect that, on average, individuals' labour market outcomes would be better in regions that are urban and/or near large urban conurbations due to the positive job-prospect spill-overs as a result of higher levels of economic activity.

[14] See DEFRA 2007.

[15] Since this indicator is output area-based, this may be subject to some error. We assume that this error is small, given the lack of alternative local authority-based measures.

University Present: Information on Higher Education institution location was sourced from the Higher Education Statistics Authority (HESA). As a policy relevant variable, one would expect that the presence of higher education institutions would be a force for improved employment prospects for the local population, given the support services needed to run such an institution as well as the influx of young consumers into the local market. However, as pointed out by Arntz & Wilke (2009), the increased availability of a young flexible workforce willing to work at minimum wage rates may impact negatively on the labour market participation on a section of the local population. The overall impact of this indicator is likely to be an empirical question, given these confounding factors.

Table 4.4: Regional Indicators.

No.	Group	Indicator	Source	Mean	SD
I	**Demand/supply**	• Quarterly *ILO Unemployment rate* (%)	A	.048	.021
		• Average retrospective 12 quarter *change in ILO Unemployment rate* (%)	A	.047	.023
II	**Economic Performance**	• Annual *GDP per head (GDPPH)* (£)	B	14971.3	7604.48
		• Average retrospective 3 year *change in GDP per head (GDPPH)* (%)	B	.076	.139
		• New Small Business Startups/ Resident Population	E	.003	.001
III	**Social Structure**	• Quarterly *Skill Intensity*	A	.622	.072
IV	**Institutional Organisation**	• Monthly *18-24 New Deal Starters* (% of eligible claimant count) Interacted with eligibility at individual level.	A/E	.024	.051
V	**Structural Indicators**	• Accessibility	C	.933	.249
		Type of region: (ref. rural)			
		• Urban	C/D	.634	.212
		• University Present	F	.135	.342
		• Flow of U/ Resident Population	E	.007	.003
Number of obs = 187,032					

A: Local Area Labour Force Survey; B: Office of National Statistics; C: National Statistics Postcode Directory;
D: DEFRA (Department of Environment, Food and Rural Affairs; E: NOMIS; F: HESA (Higher Education Statistics Agency)

The linked data set matching the individual- and regional-level data to the UK geography is conditioned on the start of claimant spells. In order to match the continuous individual-level data to the regional information, individual spells were matched to the regional information pertaining to the month in which they started (see also Appendix 4).

The final data set contains the information of 963 unemployment benefit office (UBO) locations (full postcodes and postcode districts). This is then mapped to the existing data via the NSPD. Given the self-reported nature of the JUVOS postcode information, data quality issues were present with postcode information missing or wrongly imputed at times. In order to maintain some regional variation we only replaced this self-reported variable with the UBO postcode district when this variable was missing and no information could be obtained from previous spells (implemented in 2% of cases). If the postcode information was missing, the initial strategy was to replace this with the postcode reported in a previous spell if this existed, i.e. assuming that the individual did not move location between the spells. This was implemented in 2.8% of cases.

We omit Northern Ireland from proceedings, due to lack of coverage for some major regional indicators of interest at all levels. Our analysis thus focusses on Great Britain. The City of London and Isles of Scilly local authorities are dropped from the analysis, as data for these geographies is systematically missing at the aggregation level of interest (local authority level). However, in the case of randomly missing values we impute values for the variables of interest given the number of missings is so low for the selected variables. For each variable affected, the imputation method was to replace the variable by the data in the preceding period.

Due to the creation of 46 unitary authorities[16] over the period 1995 to 1998 in the regional data, including 13 extra units, we restricted our observation period to after 1998. This was due to a restructuring of local governments over the period, from a one-tier to two-tier (lower level) system in some areas. The resulting geography is a mixture of Local Authority Districts, Unitary Authorities and Metropolitan Districts. Restricting ourselves to the 1999 to 2005 period also avoids a concordance issue between the 1990 Standard Occupational Classification (SOC90) and the 2000 update (SOC2000), as the Local Area Labour Force Survey is available according to the SOC2000 methodology from the first quarter of 1999 (Beerten, Rainford, & Jones 2001).

Due to data limitations, we are unable to distinguish between New Deal participants on government supported training initiatives and those partaking in subsidised work-placements. Individual-level studies on the Swedish and the German labour markets highlight fundamental differences in the re-employment probabilities of these two population sub-groups (Adda *et al.* 2007; Arntz & Wilke 2009). Using individual-level Slovakian administrative data, van Ours (2004) found a significant locking-in effect of government subsidized jobs. Given the regional context of our study, this suggests that where these jobs occur may be of importance.

4.4 Econometric Model

We analyse the determinants of unemployment duration by means of censored quantile regression and the Cox proportional hazard model. Censored quantile regression is recently emerging as an attractive and powerful alternative to proportional hazard models (see for example Koenker & Geling 2001). The linear quantile regression model, introduced by Koenker & Bassett (1978) models the

[16]"Single-tier administrations with responsibility for all areas of local government (ONS 2004)"

conditional quantile function of the dependent variable as a linear functional of the regressors x_i, where x_i is $k \times 1$ with $x_{1i} = 1$ for all $i = 1, \ldots, N$. Let the dependent variable lny_i be the logarithm of the ith duration of unemployment y_i. Then the θth conditional quantile of the dependent variable given x is given by

$$\begin{aligned} Quant_\theta(lny_i|x_i) &= x_i'\beta^\theta \quad \text{or} \\ Quant_\theta(y_i|x_i) &= exp(x_i'\beta^\theta) \end{aligned}$$

where β^θ is a $k \times 1$ vector of unknown coefficients. Note that these coefficients are allowed to vary over the quantile $\theta \in (0, 1)$. This means that the framework is flexible enough to allow for different effects of the regressors at different quantiles of the conditional distribution of unemployment duration. In particular, as the sign of the coefficients can change, a regressor can have a shortening effect for a lower quantile θ_1 ($\beta_j^{\theta_1} < 0$) and a prolonging effect for a higher quantile θ_2 ($\beta_j^{\theta_2} > 0$) with $\theta_1 < \theta_2$. Since our sample of unemployment duration is partly right-censored, we apply censored quantile regression. Our sample is (lny_i, x_i, yc_i), $i = 1, \ldots, N$, where $yc_i = lny_i$ if the unemployment duration is not censored and $yc_i = \infty$ when it is right censored. We apply the censored quantile regression estimator of Powell (1984) and Powell (1986) and obtain $\hat{\beta}^\theta$ by minimising the following distance function

$$\frac{1}{N}\sum_{i=1}^{N} \rho_\theta(lny_i - min(x_i'\beta^\theta, yc_i)) \tag{4.1}$$

with,

$$\rho_\theta(u) = \begin{cases} \theta \cdot |u| & \text{for } u \geq 0 \\ (1-\theta) \cdot |u| & \text{for } u < 0. \end{cases} \tag{4.2}$$

For more details on censored quantile regression see the recent survey by Koenker (2008). We use the censored LAD procedure of TSP 5.0 to estimate the unknown coefficients at three quantiles $\theta = 0.1, 0.5$ and 0.7. We bootstrap the full sample 100 times to approximate the distribution of the estimator and therefore to obtain inference statistics. [17]

The Cox proportional hazard model is based on the idea that the conditional hazard rate is proportional for different values of the regressors x. For the ith observation let $\lambda_i(y|x) = f_i(y)/P(Y_i \geq y) = exp(x_i'\tilde{\beta})\lambda_0(y)$ be the hazard rate and $f_i(y)$ the conditional density of Y_i given x_i. λ_0 is the so called baseline hazard which is nonparametric. The Cox model gains its popularity from the fact that it is relatively simple to estimate.

We estimate the Cox model by using the implementation in STATA 10 and report hazard ratios, i.e. the proportionate change in the hazard rate relative to a reference group with $x_i = 0$ rather than the estimated coefficients itself. The Cox model has also several drawbacks. It ignores individual specific error terms, which can lead to a systematic bias of estimated coefficients even if the error is uncorrelated with the regressors. Moreover, the estimated baseline hazard is usually downward biased in the presence of unobserved heterogeneity in particular for longer durations. However, a flexible semi-parametric hazard rate, with no distributional assumptions, makes this model attractive over alternative parametric approaches. As a robustness check, mixed proportional hazard models are estimated which incorporate an individual specific error term as a random effect (unobserved heterogeneity). Alternative parameterisations of the baseline hazard were implemented, including a semi-parametric piecewise constant model both with and without unobserved heterogeneity controls.

[17] We do not bootstrap more often because of the extensive computational effort and we do not apply the bootstrap method of Bilias *et al.* (2000) as the degree of censoring in our data is rather high. Since our sample consists of dummy variables and standardised continuous variables only, we do not report marginal effects as interpretation is straightforward in this case.

While incorporating unobserved heterogeneity changed little (except the significance level of the accessibility indicator), the additional assumption of a piecewise constant hazard rate did not lead to a change in the order of relevance of the estimated coefficients. We also re-estimated the Cox model with single spell data by randomly drawing one spell for each individual. Again, this also led to very robust results, except to a change in the significance level of the accessibility coefficient (See Appendix B for a more detailed discussion of these checks). For these reasons we only present the Cox results for the estimated hazard rate model in this chapter.

While the marginal effect of a regressor on the conditional distribution of unemployment duration can vary over the quantiles, the Cox model implies a unique sign of this effect (see Lüdemann *et al.* 2006). Therefore, the censored quantile regression model offers an attractive alternative as it is robust with respect to the distribution of unobserved heterogeneity and it does not restrict the effect of the regressors over the distribution of unemployment duration. Moreover, it relaxes the Proportional Hazards assumption. Note that there is no one-to-one correspondence between the quantile regression model and the Cox proportional hazard model, the coefficients $\tilde{\beta}$ and β are not the same. We focus our comparison of estimation results therefore on the sign and relative importance of the regressors and whether we can observe different signs of the estimated quantile regression coefficients for different quantiles.

In order to provide a more complete insight in the effects of various regressors on unemployment duration, we also investigate conditional hazard rates. Since the nonparametric baseline hazard of the Cox model is likely to be biased, we estimate nonparametric conditional hazard rates based on quantile regression estimates. We apply the resampling procedure of Fitzenberger & Wilke (2006) for right censored duration data which is a modification of the approach by Machado, Portugal, & Guimaraes (2006) (henceforth denoted as MPG). The main idea of the MPG is to simulate data based on the estimated quantile regression coefficients given the regressors and to estimate the conditional density and the conditional distribution function of the dependent variable directly from the simulated data.

In detail the procedure is as follows:

1. Generate M independent random draws $\theta_m, m = 1, ..., M$ from a uniform distribution on (θ_l, θ_u), i.e. extreme quantiles with $\theta < \theta_l$ or $\theta > \theta_u$ are not considered here. θ_l and θ_u are chosen in light of the type and the degree of censoring in the data. Additional concerns relate to the fact that quantile regression estimates at extreme quantiles are typically statistically less reliable, and that duration data might exhibit a mass point at zero or other extreme values. The benchmark case with the entire distribution is given by $\theta_l = 0$ and $\theta_u = 1$.[18]

2. For each θ_m, estimate the censored regression model obtaining M vectors β^{θ_m}.

3. For a given value of the covariates x_0, the sample of size M with the simulated durations is obtained as,

$$Y_m^* \equiv \hat{q}_{\theta_m}(Y|x_0) = exp(x_0'\beta^{\theta_m}) \quad \text{with} \quad m = 1, ..., M \,.$$

4. Based on the sample $\{Y_m^*, m = 1, ..., M\}$, estimate the conditional density $f^*(y|x_0)$ and the conditional distribution function $F^*(y|x_0)$.

[18]In our application, $\theta_l = 0.05$ and $\theta_u = 0.7$. Random numbers are then drawn from a discrete uniform distribution which has the quantile grid points as support points. This increases computation time significantly at the cost of small approximation errors.

5. The hazard rate conditional on x_0 and conditional on the durations drawn in the interval (θ_l, θ_u)[19] is estimated by

$$\hat{\lambda}_0(y) = \frac{(\theta_u - \theta_l) f^*(y|x_0)}{1 - \theta_l - (\theta_u - \theta_l) F^*(y|x_0)} \quad .$$

MPG uses a kernel estimator for the conditional density

$$f^*(y|x_0) = \frac{1}{M\,b} \sum_{m=1}^{M} K\left(\frac{y - Y_i^*}{b}\right)$$

where b is the bandwidth and $K(.)$ the kernel function. Based on this density estimate, the distribution function estimator is

$$F^*(y|x_0) = \frac{1}{M} \sum_{m=1}^{M} \mathcal{K}\left(\frac{y - Y_i^*}{b}\right) \quad \text{with} \quad \mathcal{K}(u) = \int_a^y K(v)\,dv\,.$$

We follow Fitzenberger & Wilke (2006) and use a kernel density estimator based on log durations. The estimates for density and distribution function for the duration itself are easily derived from the density estimates for log duration by applying an appropriate transformation.

4.5 Empirical Results

Table 4.5 reports estimation results for the duration models as described in the previous section. It shows the estimated coefficients of the censored quantile regression model and the estimated hazard ratios for the Cox model. Cox model A is a model which includes the reported variables only while model B also contains dummy variables for the 128 NUTS3 regions in Great Britain. Estimated conditional hazard rates based on the resampling procedure are presented in Figures 4.2 and 4.3.

[19]Simulating the full distribution ($\theta_l = 0$ and $\theta_u = 1$), it follows by definition: $\hat{\lambda}_0(y) = f^*(y|x_0)/[1 - F^*(y|x_0)]$.

Table 4.5: ESTIMATED COEFFICIENTS OF THE CENSORED QUANTILE REGRESSION MODEL AND ESTIMATED HAZARD RATIOS OF THE COX PROPORTIONAL HAZARD MODEL. NUTS3 CLUSTER ROBUST STANDARD ERRORS.

	Censored Quantile Regression			Cox Model	
	Quantile 0.1	Quantile 0.5	Quantile 0.7	A	B
Constant	2.315***	4.088***	4.755***		
	(0.031)	(0.022)	(0.023)		
Socio-demographics					
age< 25	0.244***	0.148***	0.073***	0.893***	0.894***
	(0.012)	(0.005)	(0.007)	(-0.008)	(-0.008)
age≥ 56	-0.094	0.086*	0.14***	0.848***	0.847***
	(0.063)	(0.05)	(0.03)	(-0.033)	(-0.033)
female	-0.042***	-0.048***	-0.048***	1.024**	1.033***
	(0.016)	(0.011)	(0.009)	(-0.011)	(-0.011)
Occupation(ref:Elementary)					
Manufacturing	-0.148***	-0.19***	-0.167***	1.161***	1.157***
	(0.026)	(0.015)	(0.019)	(-0.018)	(-0.018)
Trade, services	-0.061***	-0.178***	-0.19***	1.159***	1.168***
	(0.016)	(0.01)	(0.009)	(-0.010)	(-0.009)
Technical	0.016	-0.112***	-0.146***	1.111***	1.129***
	(0.032)	(0.02)	(0.019)	(-0.021)	(-0.022)
Senior, professional	-0.1***	-0.28***	-0.305***	1.270***	1.288***
	(0.025)	(0.012)	(0.015)	(-0.025)	(-0.026)
Unknown	-0.177***	-0.333***	-0.347***	1.342***	1.348***
	(0.02)	(0.013)	(0.014)	(-0.018)	(-0.019)
Work History variables					
Active Labour Market Programme Participation	0.071*** (0.019)	0.261*** (0.007)	0.276*** (0.012)	0.801*** (-0.010)	0.802*** (-0.01)
Long-Term Unemployment	0.31*** (0.014)	0.501*** (0.007)	0.518*** (0.012)	0.647*** (-0.007)	0.654*** (-0.006)
Incapacity Benefits	-0.051**	0.009	0.008	0.971	0.955***
	(0.025)	(0.017)	(0.019)	(-0.018)	(-0.017)
Income Support	0.024	0.07	0.067***	0.936* (-	0.943
	(0.047)	(0.047)	(0.024)	0.034)	(-0.034)
Calendar time(ref: 1999)					
y2000	0.01	-0.009	-0.021**	1.017* (-	1.018* (-
	(0.019)	(0.014)	(0.01)	0.010)	0.010)
y2001	0.072***	-0.03***	-0.054***	1.023* (-	1.029**
	(0.021)	(0.014)	(0.014)	0.013)	(-0.013)
y2002	0.12***	0.016	-0.01	0.975	0.985
	(0.021)	(0.014)	(0.016)	(-0.015)	(-0.015)
y2003	0.209***	0.07***	0.044***	0.925***	0.935***
	(0.022)	(0.014)	(0.013)	(-0.015)	(-0.016)
y2004	0.23***	0.119***	0.065***	0.879***	0.889***
	(0.024)	(0.012)	(0.015)	(-0.015)	(-0.015)
y2005	0.438***	0.312***	0.273***	0.746***	0.754***
	(0.023)	(0.013)	(0.016)	(-0.011)	(-0.011)
Quarter(ref: q1)					
q2	-0.029*	-0.015	-0.016	0.99	0.991
	(0.015)	(0.011)	(0.012)	(-0.008)	(-0.008)

Continued on next page

Table 4.5 – continued from previous page

	Censored Quantile Regression			Cox Model	
	Quantile 0.1	Quantile 0.5	Quantile 0.7	A	B
q3	-0.026	-0.041***	0.028**	0.974***	0.971***
	(0.016)	(0.012)	(0.012)	(-0.010)	(-0.009)
q4	0.118***	0.193***	0.111***	0.906***	0.902***
	(0.014)	(0.011)	(0.011)	(-0.011)	(-0.011)
Regional variables					
Accessible♯	0.11***	0.088***	0.082***	0.948* (-	0.987
	(0.022)	(0.017)	(0.014)	0.030)	(-0.033)
Urban*	-0.026	-0.013	-0.034	0.901***	0.947***
	(0.024)	(0.017)	(0.022)	(-0.019)	(-0.020)
University Present	0.023	0.049***	0.061***	0.966* (-	0.919***
	(0.018)	(0.011)	(0.014)	0.020)	(-0.016)
Skill Intensity	0.016**	0.024***	0.028***	0.979**	1.000
	(0.008)	(0.007)	(0.005)	(-0.009)	(-0.006)
GDPPH	0.013**	0.025***	0.022***	0.986	0.972
	(0.006)	(0.006)	(0.004)	(-0.008)	(-0.069)
ILO unemployment rate	0.056***	0.072***	0.068***	0.944***	0.980***
	(0.006)	(0.004)	(0.006)	(-0.009)	(-0.004)
Change in GDPPH**	0.01*	0.015***	0.017***	0.991	1.003
	(0.006)	(0.003)	(0.004)	(-0.011)	(-0.006)
Change in ILO unemployment rate**	-0.018**	-0.035***	-0.039***	1.025***	1.000
	(0.008)	(0.004)	(0.006)	(-0.009)	(-0.007)
Flow of Unemployed/ Resident Population	0.014*	0.023***	0.038***	0.986	0.976**
	(0.007)	(0.004)	(0.007)	(-0.010)	(-0.011)
New Small Business Startups/ Resident Population	0.098***	0.091***	0.081***	0.918***	0.978**
	(0.011)	(0.007)	(0.006)	(-0.014)	(-0.010)
18-24 New Deal Starters/ Eligible Population (§)	-0.012**	-0.029***	-0.028***	1.021***	1.012***
	(0.006)	(0.005)	(0.005)	(-0.005)	(-0.004)
NUTS3 fixed effects					✓
Joint significance of regional variables:					
$\chi^2(11)$				212.83	88.41
				(0.000)	(0.000)
Number of obs = 187,032					

Standard Errors: Robust bootstrapped standard errors - Bilias *et al.* (2000) methodology (CQR); Cluster (NUTS3) robust standard errors (Cox PH). Significance levels: ***: 1% **: 5% *: 10%

Interpretation (CQR Ests): $\exp(\beta^{\theta}) - 1$. Marginal effects not drawn on due to lack of comparability in existing methodologies.
Note: For regional dummy results see Figure 4.1(Cox model B only)
Robustness: Full robustness checks are detailed in Appendix B
♯ National Statistics Postcode Directory (NSPD) population density based. * UK/Wales - DEFRA Local Authority based. Scotland - NSPD-based, Local Authority with >90% of Output Areas Accessible (<30 mins drive from large (>10,000) urban conurbation).
** 3 year moving average. Captures the medium-term evolution of a region. §- NB. Interacted with eligibility at the individual level (aged 16-24).
NB. Time-varying variables standardised across region, by month. Captures the evolution of where a region sits in the regional distribution.

Continued on next page

Table 4.5 – continued from previous page

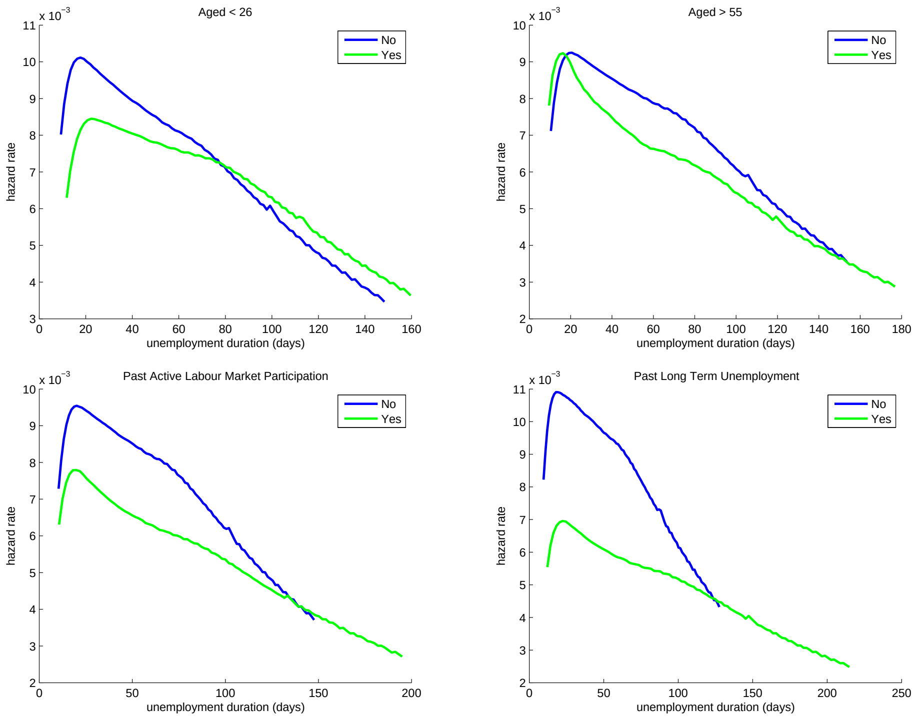
	Censored Quantile Regression			Cox Model	
	Quantile 0.1	Quantile 0.5	Quantile 0.7	A	B
NB. Variable selection to fully characterise time-varying regional heterogeneity: Economic theory combined with Clustering of Variables Around Latent Variables (CLV) routine by Vigneau & Qannari (2003).					

In what follows we discuss and compare the estimation results in more detail. The Cox model estimates are similar to those of Wilke (2009) although the time period differs. Whilst the presence of region dummies in the Cox model (B) does not impact significantly on the magnitude of coefficients, several regional variables lose significance. The regional dummies in this model are also insignificant, suggesting that the regional variables perform well in capturing the regional variation in the data. This is further supported by the significance of the region dummies in a model without regional controls. This result is robust to the inclusion of Travel-To-Work Area fixed effects, as well as to changes in specification (Exponential, Weibull, Gompertz, Piecewise-Constant, see Appendix Section B).

The CQR coefficients represent the effect at each quantile of the distribution, whilst the Cox estimates average out this effect under the assumption of proportional hazards. The CQR framework is flexible enough to allow for different effects of the regressors at different quantiles of the conditional distribution of unemployment duration, without restricting the sign of these effects to be the same. Moreover, the CQR estimator has been shown to be robust in various situations, including non-normality of the error distribution (Fitzenberger & Wilke 2006). Whilst several quantile regression coefficients change their sign over the quantiles, we observe a significant change of the sign for only two variables (y2001, q3). These cases imply an immediate violation of the proportionality assumption. Moreover, hazard rates may be disproportionate in absence of sign changes of quantile regression coefficients and it is therefore of interest to look directly at the estimated conditional hazard rates to obtain a clearer picture.

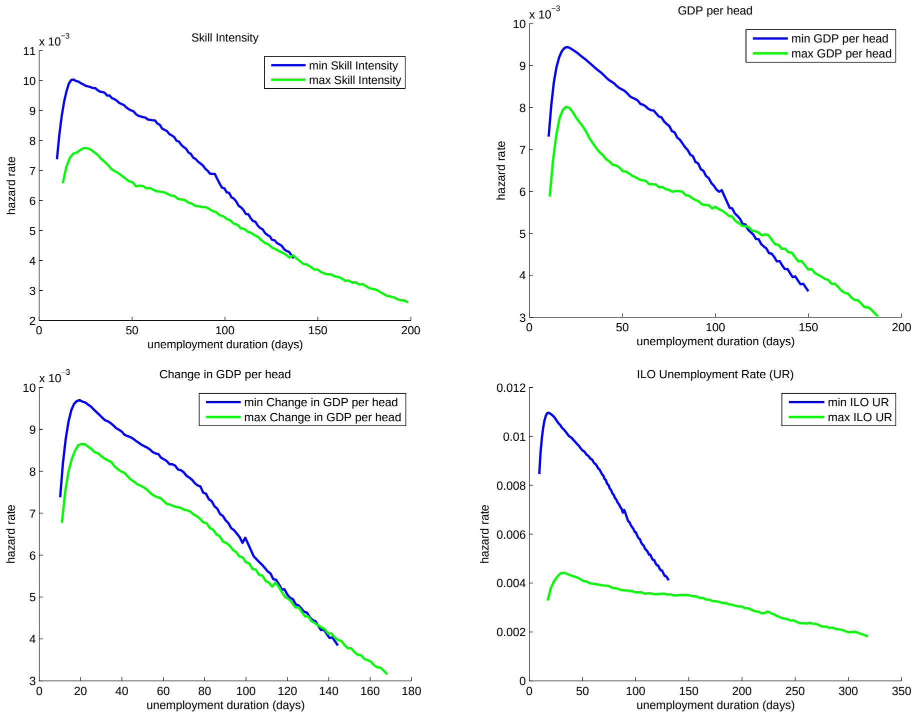
Figures 4.2, 4.3 and 4.4 present a selection of the estimated conditional hazard rates, where we do not display results for the calender time and if the effect of a regressor is very small. The figures provide a more complete image of the impact of the CQR coefficients in Table 4.5, by illustrating how the impact varies depending on the value of the variable in question. The support of the estimated hazards is limited to a certain interval as we have only estimated the quantile regression model for $\theta \in [0.05, 0.7]$. The Figures suggest that the covariate effect is mainly limited to shorter durations of up to about 150 days, and these effects are largely consistent with the CQR estimates in Table 4.5. For instance Figure 4.3 suggests that, as we move from regions with the maximum to the minimum level of Skill Intensity there is a *ceteris paribus* drop in re-employment probability and on average this difference lasts until roughly 150 days. This suggests mismatch between the pool of JSA claimants and the vacancies posted in areas of high Skill Intensity. However, these figures provide evidence that conditional hazard rates often appear disproportionate (see for example the conditional hazards cross for the aged < 26 category). Unfortunately, since higher moments of the hazard rate estimator are unknown, we cannot test for this type of shape regularity. However, given the very large number of observations we believe that it is likely that some of the non-proportionalities cannot be rejected. Due to the more restrictive nature of the Cox model, the following discussion is based mainly on the quantile regression estimates. In general Table 4.5 suggests that if a variable has an economically and statistically significant effect, then this will be reflected in both models. However, if there is a change of sign in the quantile regression model, then the Cox estimator is more likely to produce the effect at higher quantiles. Thus conflicting effects are more likely for shorter durations.

Figure 4.2: Estimated conditional hazard rates: change from 0 (blue line) to 1 (green line) in one individual level variable; sample means of all other variables.



Individual variables and calender time (Table 4.5) Being aged <26, previous Active Labour Market Programme (ALMP) participation, and previous long term unemployment experience all have a strong positive impact on the length of JSA claimant periods. Moreover a clear time pattern is evident, with longer durations in later years. Being aged <26 and being aged ≥56 display a reverse trend across the quantiles, relative to being prime-aged. This is not reflected in the Cox estimates. Relative to elementary occupations, previous employment in every other 1-digit standard occupational group significantly shortens claimants' spells. This effect is significantly more pronounced the longer the claimant spell. Data limitations implied that skill levels could not be directly proxied by observed qualifications. However, the observed divergence of reemployment prospects suggests that the negative signal of previous elementary occupation increases as spells lengthen. As some individual variables have a stronger association with the dependent variable, relative to the other controls, the direct implication is that the individual-level seems to be more important than the regional-level of aggregation. Estimated effects of the individual level coefficients are generally similar to the estimates of Wilke (2009) and for this reason we omit here a more detailed discussion.

Figure 4.3: Estimated conditional hazard rates: changes from sample min (blue line) to sample max (green line) in one regional variable ; sample means of all other variables.

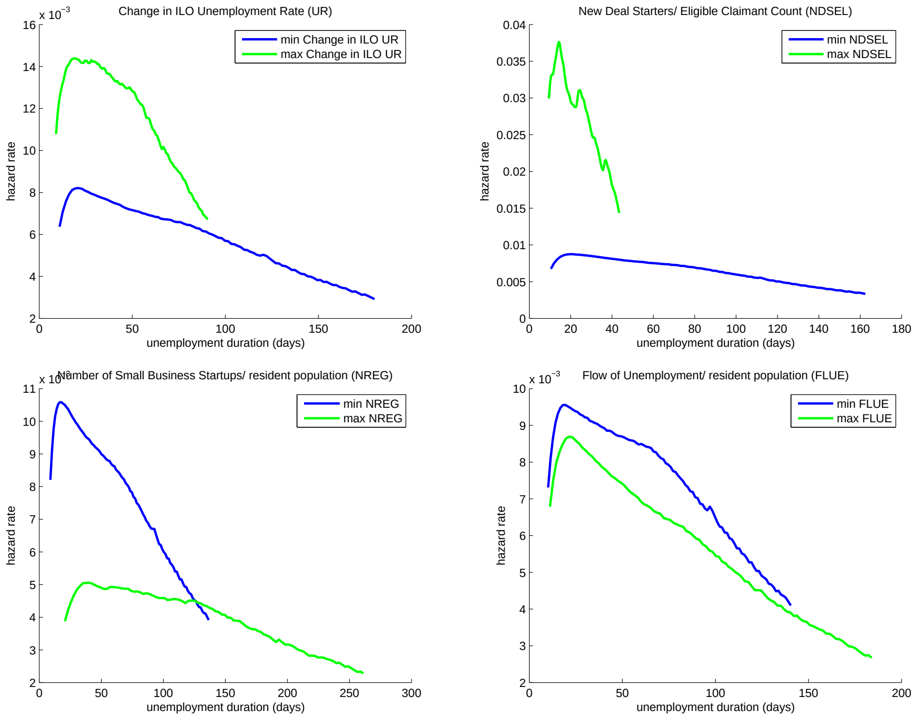


Regional variables (Table 4.5) Although regional labour market conditions generally possess a significant association with the length of claim periods, the size of these effects is often considerably smaller than for the individual level variables. This pattern is not unique to Britain as the same observation was made with data from Germany after controlling for institutional factors (Arntz & Wilke 2009). This contrasts the findings of Collier (2005) which suggest that regional labour market conditions are more important. For the set of regional variables, we do not observe any change of sign of the quantile regression coefficients over the quantiles.

Better accessibility of a region increases the length of JSA claim periods, in particular for very short durations. This is roughly compatible with the findings of Arntz & Wilke (2009) for Germany, who observe that a longer driving time to a higher level city increases the job finding probability for singles. However, the sign of this effect is inconsistent with our previous hypothesis. Urban regions are associated with relatively shorter claimant periods, a result which concords with our expectations from economic theory and is consistent with Arntz & Wilke (2009). Although consistent with out priors, this result is insignificant at conventional levels. Given that urban regions are also accessible, the accessibility indicator is capturing the effect of being accessible conditional on being urban. As in the case of Germany, over the time period of observation, the presence of a university lengthens JSA claim periods whilst the relationship is not significant for shorter durations. The presence of more skill-intensive jobs increases the length of claim periods. This suggests that a better social environment is related with poorer employment prospects. This finding contrasts the results of Arntz & Wilke (2009) and the interpretation of the effect is unclear and may be affected by endogeneity as the individual level variable suggests the contrary.

Higher local GDP per head has a positive association with the length of claim periods although this effect is small in economic terms. Although surprising, this result pattern is also compatible with the observation of Arntz & Wilke (2009) for Germany. Similar to the results of Petrongolo (2001), our analysis suggests that a higher local unemployment rate is related with longer claim periods. The effect increases over the quantiles and it is one of the most important regional variables. In addition to the unemployment rate, Arntz & Wilke (2009) also control for the share of long term unemployed and their

Figure 4.4: Estimated conditional hazard rates: changes from sample min (blue line) to sample max (green line) in one regional variable ; sample means of all other variables.



results suggest that this indicator significantly increases spell lengths. For the reasons mentioned earlier, we have not included the share of long term unemployed in our final model. However, other model specifications suggest that the indicator has a comparable effect when used instead of the unemployment rate. It is not confirmed by our results that emerging regions (in terms of increase in GDP per head and decrease in unemployment rate) improve JSA claimants' job finding probabilities. However, the estimated effects are small in terms of economic significance, notably in the lowest quantiles. Granted, the impact of higher unemployment rates may suggest a 'stigma' effect of living in high unemployment regions that increases with the duration of unemployment.

The share of New Deal programme starters amongst the eligible claimant count (18 - 24) has a negative association with the length of JSA claimant durations. Although this unlikely to be causal, it suggests that assignment activity in local jobcentres may be related to regional labour market outcomes and not fully random.

In line with our expectations we find that the rate of unemployment flows - which proxies Seasonal Unemployment - has a positive impact on JSA claimant durations. This effect increases across the quantiles.

Arntz & Wilke (2009) find that the rate of new business startups in an area - which proxies local 'business activity' - has a positive, and significant, impact on the prospects of low-wage earners being re-employed in the local area. The effect on high-wage earners is found to be insignificant, which may be due to higher levels of job mobility. Due to lack of earnings information, we are unable to make this distinction. However, this indicator is one of the most economically significant amongst the regional variables. The estimates suggest that higher levels of 'business activity', relative to the resident population, have a lengthening effect on claimant spells which is most notable in the bottom quantile. This estimate is difficult to interpret, given our priors. Although most of the estimated effects of the regional variables are rather small in magnitude, being accessible, the local unemployment rate and 'business activity' in a region turn out to be the most important among them. There are strong shifts in the estimated conditional hazards for changes from the sample minimum to the sample maximum in these continuous regional variables. This suggests that extreme regional labour market conditions do have strong effects, although Table 4.5 suggests that sample effects on the conditional quantiles are mainly limited as they are in response to a shift by one standard deviation.

Table 4.6: Top and worse performing regions in Great Britain. Results from a Cox regression with individual variables, calender time variables and 128 NUTS3 region dummies.

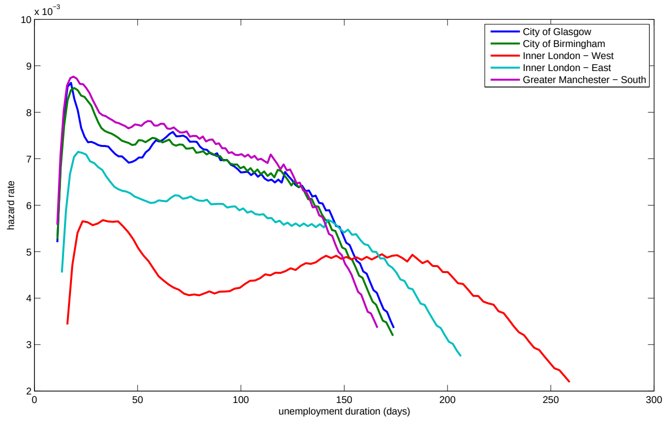
Rank	Top Performer	Worst Performer
1	Eilean Siar (Western Isles)	Inner London - East
2	Caithness/Sutherland/ Ross/Cromarty	Inner London - West
3	Shetland Islands	Outer London - West and North West
4	East Cumbria	Birmingham
5	Aberdeen/Aberdeenshire/ North East Moray	Berkshire

Comparison of regions As a next step we directly compare selected regions. Table 4.6 reports a ranking of region dummies obtained by the Cox model with individual variables and region dummies only (e.g. omitting the other regional variables). The region dummies capture both the observable and unobservable region specific effects and thus provide us a simple performance ranking in terms of the length of claim periods by controlling for individual specific characteristics and calendar time. We refer to the Cox estimates as it was technically impossible to obtain censored quantile regression estimates when regional dummies were included in the model.

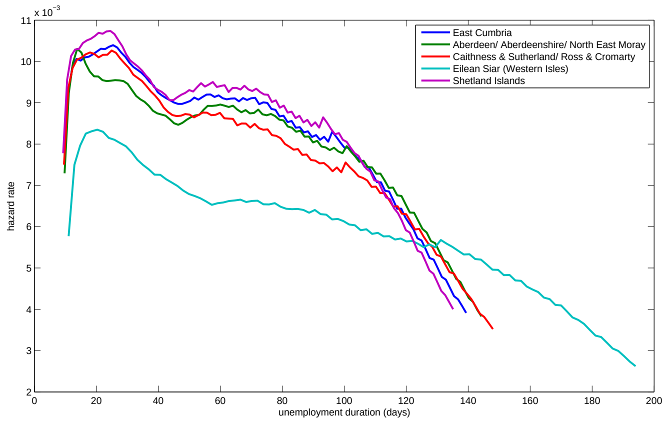
The table suggests that large cities such as London and Birmingham and the London commuter belt have the strongest positive association with the length of claim periods. In contrast, remote regions such as the Western Isles and Shetland Islands are among the regions with the shortest conditional claim duration. It is remarkable that four out of the five top performing regions are located in Northern Scotland. Since the results in the table are based on the simple Cox model's regional dummies, it is unclear whether the ranking is due to the observable regional labour market environment or due to unobservable regional characteristics. It is, however, of interest to explore this in more detail. For this reason we also compute the resampling based conditional hazard rates for the duration of benefit claim periods conditional on being a sample average individual and residing in the region specific labour market environment (sample average for each region). This is done for five NUTS3 regions which overlap with the four major cities in Britain: Inner London (East and West), Birmingham, Manchester and Glasgow.

Figure 4.5: Estimated conditional hazard rates for several regions.

four major agglomerations (top)



top five performer (bottom)



Apart from Glasgow and Manchester these cities belong to the poorest performers according to the Cox estimates. Moreover, we compute the hazard rates conditional to the top five performers according to Table 4.6. The resulting estimates are presented in Figure 4.5. It is apparent that the observed regional labour market conditions in Greater Manchester (South), Glasgow and Birmingham result in rather similar conditional hazards. The observed characteristics for London and in particular for West-London point to a considerably worse observed labour market environment. Hazard rates for the top performers are considerably higher than for the large cities if we ignore the Western Isles. Moreover, the figure suggests that simultaneous changes in several regional variables can lead to considerable shifts in conditional hazard rates. The shape of the hazard rates looks rather disproportionate and differences are mainly relevant in the interval up to 100 days. These results therefore provide evidence that the region specific environment matters much less for longer durations. Our findings therefore suggest that regional policies may fail to improve employment prospects of the long term unemployed.

4.6 Summary and Conclusion

We create a comprehensive British data set by merging individual claim periods of unemployment benefits with a comprehensive set of regional indicators to capture regional variation in job arrival rates. In our empirical analysis we use this data to investigate the relevance of individual characteristics and local labour market conditions on the length of JSA claim periods. We employ censored quantile regression and apply a resampling method to estimate nonparametric conditional hazard rates.

We find evidence that both individual level variables and the local labour market environment shape the distribution of re-employment times. Although individual level variables turn out to be more important, in particular the local labour demand and supply conditions and structural indicators of a region are also important determinants of the length of claim periods. Our results therefore contrast the results of Collier (2005) who observes regional variables to be more important, while they are often similar to the results of Arntz & Wilke (2009) for Germany. This includes the relative relevance and the sign of the estimated effects. Moreover, we observe that covariate effects are mainly limited to a duration of up to 150 days while they are generally negligible for longer duration. Our results therefore suggests that regional (fiscal) policies are likely to be ineffective for improving employment prospects of long term unemployed. This is an interesting observation which could not be made by employing a proportional hazard model.

From a policy point of view we draw the conclusion that regional labour market conditions and therefore regional (fiscal) policies, targeting job creation, can affect individual labour market outcomes. Our results, however, do not suggest that these are the principal driving forces for re-employment times. Therefore, regional (fiscal) policies seem more to have a supportive role and they cannot substitute for a lack of individual qualities in the job search process. This adds further weight to the already existing support for the targeted individual-level Active Labour Market Policies (ALMP) aimed to improve jobseekers' re-employment prospects, e.g. through sponsored work experience and vocational qualifications. Surprisingly, we observe that large cities such as London and Birmingham provide worse local labour market conditions than rural and even remote regions such as Northern Scotland. This finding is important as many people likely believe the reverse, although the Government is already targeting problematic neighborhoods in these cities.

Our research also leaves some scope for extensions in some respects. From a methodological point of view, the use of censored quantile regression extends standard econometric techniques in several dimensions. However, it also limits our econometric model in several aspects. First, it cannot deal with time varying covariates and thus we only take into account the information at the start of claim periods. Moreover, we cannot take into account multiple spells in our analysis as this is also still to be developed for censored quantile regression. Multiple spells can be exploited for identification purposes (Abbring & Berg 2000; Van den Berg 2001). "Whether the hazard rate of an event depends on a previous event, conditional on a previous event, is an important modeling issue. Second, the form of dependence is of interest. The duration of a previous spell may enter as a covariate in determining the hazard of a later event; the occurrence of a previous event may affect the baseline hazard for a later spell; and, finally, unobserved heterogeneity may show serial dependence. Each of these raises an important modeling issues (Cameron & Trivedi 2005, pp. 656)."

The extent to which multiple spells of unemployment matter, and when they matter, for labour market outcomes is a subject of debate within the literature. The earlier literature on gross worker flows suggested that changes in the size and distribution of inflows into unemployment are the main determinant of the unemployment rate. This suggests that incidence of unemployment matters more for labour market outcomes. Cyclical unemployment is concentrated in groups with low exit probabilities. Thus, the observed procyclicality in average exit probabilities from unemployment may largely be explained by these compositional effects (Darby *et al.* 1986). Recent work has questioned the composition explanation (e.g. Shimer 2012). Moreover, recent literature, e.g. Elsby *et al.* (2009) and Petrongolo & Pissarides (2008) suggests that incidence and duration of unemployment are related to the business cycle. It would thus be fruitful in future work to investigate this further in relation to the van Dijk & Folmer (1999) hypothesis and research questions under test. Inflow rates countercyclical, especially for job losers (layoffs), whereas outflow rates are procyclical. This suggests that high unemployment levels in a recession are driven by longer unemployment durations, rather than higher incidence.

Finally, competing risks allows us to take into account whether a variable affects alternative hazard functions differently, (e.g. McCall 1996, for a seminal contribution). It is common for studies using survival analysis techniques to treat transitions to inactivity as right censored or to drop them completely. In the duration context, treating inactivity as a *censored destination state*, may lead to inconsistent estimates of the parameters determining the transitions of interest as this assumes away unobserved characteristics affecting both transitions of interest and those to censored states (van den Berg & van Ours 1994; van den Berg & Lindeboom 1998). For example, one effect of JSA introduction was to increase transitions onto Incapacity Benefits by 2.5-3% (Petrongolo 2009). It is likely that the composition of these individuals differed substantially from those that moved into employment as a result of JSA introduction.

From a data point of view, we are unable to fully map individuals' employment biographies (their movements in and out of the labour market, wage changes, etc.) due to the lack of merged administrative individual data. The availability of individual data from additional registers would enable us to perform an extension of our analysis. We do not directly address the issue of commuting as the resident population may not be contributing to the productivity of a region. Job density can be used to indirectly control for commuting. We constructed a job density indicator from the regional data, however this variable turned out to be highly correlated with other indicators used in the analysis and for this reason it was not included in our final model. More comprehensive data with information about the workplace would enable us to directly analyse commuting and even intra regional migration.

Chapter 5

Mixed Signals: To what extent does Male Wage Scarring vary with the characteristics of the Local Labour Market in which unemployment was experienced?

5.1 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. This chapter tests 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)? What implications do other important sources of regional variation in previous unemployment experience have in driving wage outcomes?

Individual unemployment experience(s) are hypothesised to increase the likelihood of future unemployment and decreasing future earnings potential. Economic theory provides ambiguous predictions with respect to the question of Wage Scarring. 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 & Portugal (1989) and Houle & 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 & 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 & Storrie 2006) and the US state of Connecticut (Couch & Placzek 2010). Eliason & Storrie (2006) highlight the increased sensitivity of displaced workers' earnings losses to recessionary pressures. Furthermore, Couch & 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]. 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 & Zylberberg 2004). In slack labour markets, the prospects of a successful match are lower as there will be more unem-

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

ployment 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). Job offer arrival rates are also likely to be higher in areas of high (urban) than low (rural) economic activity. van Dijk & 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 the van Dijk & Folmer (1999) hypothesis holds when applied to Great Britain in a dynamic longitudinal context and motivates this study. 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 what implications do other important sources of regional variation in previous unemployment experience have in driving wage outcomes?

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 Appendix E.

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 whilst for multiple spells of unemployment the wage penalty reduces with incidence. 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. These negative implications are long lasting. Weaker evidence is found supporting the main hypothesis in the UK on average, and stronger support for those made redundant in their previous jobs. These results remain robust to specification changes.

The chapter is organised as follows. Selected contributions to the existing literature are summarised in the Appendix, Table 5.16. Section 5.2 describes the data. Section 5.3 describes the methodology in the context of the exisiting literature. Section 5.4 examines descriptive statistics relating to the consequences of job displacement for future wage growth. Section 5.5 presents results from an initial replication of Arulampalam (2001). Section 5.6 extends the basic results in terms of observation period and regional-level effects. Sensitivity checks are briefly detailed in section 5.7, whilst Section 5.8 concludes.

5.2 The Data

Survey data is exploited in order to address the research questions of interest. Administrative data has the advantage of larger sample sizes, and in some cases matched employer-employee data, but this information is not generally collected for the purpose of academic use. This makes identifying the phenomenon on interest difficult. For example, Gregory & Jukes (2001) match the JUVOS to the New Earnings Survey (NES). However, whilst the JUVOS contains daily information, the NES is only collected once a year. The matched data set also restricts the study to comparing registered unemployment as an alternative to employment.

Administrative data is generally limited in its covariate set, with key indicators - reason for leaving job - being non-identifiable. In an attempt to circumvent this issue, studies using administrative data have limited their focus to separations occurring around a firm/plant closure - identifiable with matched employer-employee data - or to firms experiencing large employment changes/ mass layoffs over a certain time period (for example Jacobson, LaLonde, & Sullivan 1993). The argument being that these separations are more likely to be be determined by exogenous demand shocks, unrelated to observed worker ability (Farber 1999). Despite this potential advantage, it is impossible to be certain that separations due to reasons other than layoffs are not captured in this definition. The wider the time period considered, the more likely this is to be the case (Kunze 2002). Relative to Administrative source, Survey data suffers from smaller sample sizes. Despite this, Survey data tends to contain more information about the respondents, including key variables like reason for leaving previous job. However, given its self-reported nature, Survey data is more likely to suffer from problems of measurement error and recall bias.

The high cost of collecting high frequency data means that this information is usually collected retrospectively (Jürges 2007). Comparability is also hampered as overtime and across survey, value labels for key variables may differ considerably making it harder to accurately identify key information (Farber 1999). This is why a rules-based approach to data set construction is adopted in order to minimise these issues (see Apprendix E). The BHPS distinguishes termination of seasonal work/fixed-term contract from other reasons, however the DWS does not make this distinction. This introduces some discretion into the way interviewees may respond, which may make it harder to accurately identify separation types. Despite these potential drawbacks, the depth of information available in survey data makes it an attractive alternative to administrative 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 & 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 & 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 5.8.4 illustrates the structure of the BHPS, whilst full data preparation steps are detailed in Appendix E.

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

[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*

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 Appendix D.

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, see Appendix E for details. This details the rules-based approach adopted to minimise the major sampling issues. Furthermore, the study aims to test the van Dijk & Folmer (1999) hypotheses by matching the individual-level data to the regional context in which these individuals reside.

5.3 Methodology

Ideally our treatment, job displacement, would be randomly assigned (Angrist & 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.

[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.

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. However, since the BHPS does not allow one to isolate redundancies due to plant closures and/or mass layoffs, self-reported redundancy is less likely to be exogenous to individual characteristics. This caveat should be taken into account when interpreting the results. 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.

The following Mincerian Earnings function is estimated:

$$ln(w_{it}) = x'_{it}\beta + (d'_{it}Z_{it})'\gamma + \lambda_t + \alpha_i + \varepsilon_{it} \qquad \forall\ i = 1 \ldots n,\ \forall\ t = 1 \ldots T \tag{5.1}$$

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}).

λ_t = Time dummy.

α_i = Time-invariant individual-specific error component.

ε_i = Idiosyncratic error component.

5.3.1 The Within-Groups estimator

The Within-Groups estimator, $ln(w_{it} - \bar{w_i}) = (x_{it} - \bar{x_i})'\beta + (d_{it} - \bar{d_i})'\gamma + (\lambda_t - \bar{\lambda}) + (\varepsilon_{it} - \bar{\varepsilon_i}) \quad \forall\ i = 1 \ldots n,\ \forall\ t = 1 \ldots T$, 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 & Trivedi 2005). The parameters in specifaction 5.1 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 & 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 & Krueger 1999).

5.3.2 Sample Selection

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 two-step Heckman selection model (Heckman 1979). Given the data structure employed, a cohort followed over time, the Heckman approach seemed a natural way to model initial selection into the sample. This technique follows Arulampalam (2001) and involves two steps. First, a model to explain the probability of an being in the wage equation sub-sample is estimated using a reduced form probit. Second, a correction term is constructed (inverse Mill's ratio) from the probit and used as an additional regressor in the wage equation to correct for the selection. The identifying restriction required to identify the parameters of the wage equation using the selected sample is that the identifying variables (exclusion restrictions) are assumed to impact on the probability of being in the selected sample but are assumed not to influence the wages conditional on being in the sample (Arulampalam 2001). A fundamental challenge is that of finding valid instruments.

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 (see Appendix E).

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 & 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]. Unlike in the National Child Development Survey, TTWA unemployment rate at 16 is unavailable. 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 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]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.

5.4 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 F.5, Appendix Section F). Thus the descriptive analysis mainly focusses on the extended 1991-2001 period.

Table 5.1: 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 5.1 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 5.1 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 recall bias, leading to inconsistencies with the spell length measure used in this analysis.

Summary statistics for the 1991-1997 and 1991-2001 period are presented in

Table 5.2: 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 5.1

Table F.5, Appendix Section F. According with intuition, tables F.5 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

[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.

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 5.2 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 5.3 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.

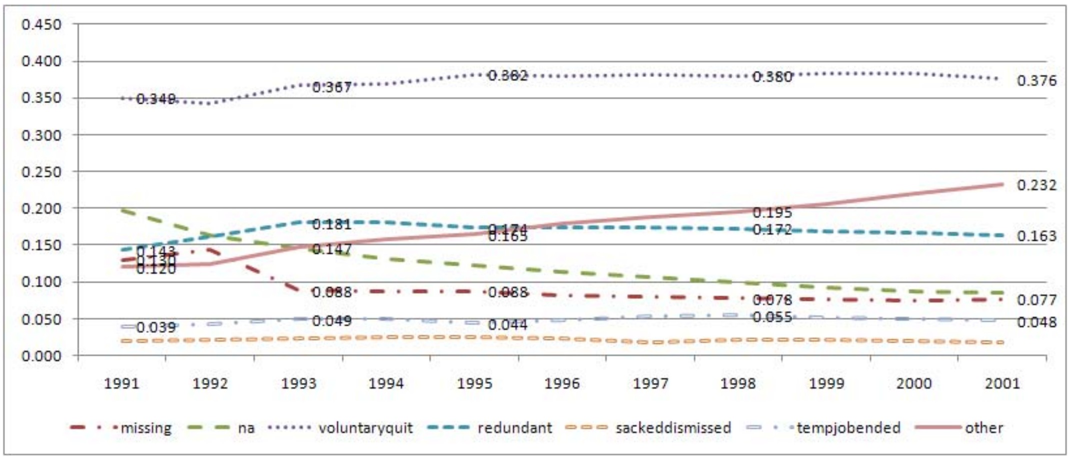
Table 5.3: Reason for leaving previous job by previous status, 1991 - 2001.

	EMP % (Obs.)	**UNEMP** % (Obs.)	**OLF** % (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 5.1

Figure 5.1 illustrates how displacement rates varied over the survey years by displacement type. Figure 5.1'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.

Figure 5.1: Reason for Previous Job Ending (as % of total displacements), 1991 - 2001.



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 5.1 highlights a trend increase in the proportion of people ending their jobs for "other" reasons. These 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

[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.

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.

5.5 Empirical Results

5.5.1 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. The data selection rule is detailed in Section 5.3.2, as well as motivation for controlling for sample selection and identification considerations. 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 this initial selection varies across the years. Current labour market status is also conditioned to exclude self-employment. Individuals reporting themselves in self-employment

[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.

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 5.13, in Appendix, Section 5.8.2. 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 & Jukes (2001), I find evidence to suggest 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. The identifying variables are jointly significant. However, given the limitations of the identifying variables highlighted in Section 5.3, this cannot be interpreted as clear evidence of the absence of sample selection.

5.5.2 1991-1997: Replication of Arulampalam (2001).

Key wage equation results for the 1991-1997 period are summarised in Tables 5.4, specifications 3 & 4 (OLS results are presented for comparison). Model 3/7 estimates the baseline model illustrated in equation 5.1, with $d'_{it}Z_{it}$ capturing whether an individual entered the current employment spell via an unemployment/OLF spell. In Model 4/8 $d'_{it}Z_{it}$ enters as an interaction between previous labour market status and current tenure. This captures how real wages recover whilst on the job, relative to an individual who entered their current employment spell via a job-to-job transition. Current (interview date) labour market status is conditioned to exclude self-employment in the main analysis, however, pre-1.9.90 labour market status is not[9]. Tenure effects are allowed to vary up to 10 years, with the effect of tenure on wage restricted to being constant for job spells longer than this. This permits non-linearities in the impact of tenure on wage growth to be better captured, relative to a more restrictive quadratic specification. Models 3 & 7 restrict the impact of previous labour market status to be constant over time, whereas 4 & 8 allow this to vary up to 4 years after the event, with the impact after 4 years restricted to being constant.

Table 5.4: Log Real Hourly Wage Equations for Male Sub-Sample: Individual-level observed heterogeneity and Fixed Effects, Previous Status unrestricted. Robust Standard Errors in parenthesis.

	1991-1997				1991-2001			
	OLS		FE		OLS		FE	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
constant	0.922***	0.950***	1.356***	1.292***	0.907***	0.933***	1.194***	1.169***
	(0.086)	(0.091)	(0.188)	(0.189)	(0.082)	(0.085)	(0.144)	(0.144)
Tenure in current employment.								
base is <1 year.								
1-2 years	0.021	0.040	0.020	0.040***	0.030*	0.038*	0.020*	0.034***
	(0.021)	(0.025)	(0.013)	(0.015)	(0.017)	(0.020)	(0.011)	(0.012)
2-3 years	0.057***	0.059***	0.019	0.035***	0.061***	0.055***	0.031***	0.034***
	(0.021)	(0.024)	(0.015)	(0.016)	(0.017)	(0.020)	(0.013)	(0.013)
3-4 years	0.065***	0.079***	0.037***	0.059***	0.084***	0.084***	0.056***	0.063***
	(0.021)	(0.024)	(0.017)	(0.018)	(0.018)	(0.021)	(0.014)	(0.014)
4-5 years	0.090***	0.089***	0.055***	0.079***	0.097***	0.092***	0.068***	0.082***
	(0.024)	(0.026)	(0.019)	(0.021)	(0.020)	(0.021)	(0.015)	(0.016)
5-10 years	0.096***	0.097***	0.060***	0.082***	0.115***	0.111***	0.075***	0.088***
	(0.018)	(0.020)	(0.019)	(0.020)	(0.015)	(0.016)	(0.016)	(0.017)
10 years +	0.169***	0.168***	0.123***	0.145***	0.176***	0.172***	0.113***	0.125***
	(0.018)	(0.020)	(0.027)	(0.028)	(0.015)	(0.016)	(0.022)	(0.023)
Previous labour market status.								
Inactivity	-0.108***		-0.115**		-0.106***		-0.100**	
	(0.020)		(0.051)		(0.016)		(0.040)	
Unemployment	-0.095***		-0.084***		-0.093***		-0.098***	
	(0.012)		(0.029)		(0.010)		(0.023)	
Time since interruption (ref. Previous Employment)								
Unemployment.								
< 1 year		-0.083***		-0.050		-0.098***		-0.076***
		(0.031)		(0.033)		(0.027)		(0.027)
1-2 years		-0.133***		-0.107***		-0.116***		-0.115***
		(0.034)		(0.032)		(0.029)		(0.029)
2-3 years		-0.110***		-0.117***		-0.096***		-0.100***
		(0.039)		(0.042)		(0.033)		(0.033)
3-4 years		-0.111***		-0.107**		-0.091***		-0.092***
		(0.041)		(0.043)		(0.035)		(0.034)
4 years +		-0.085***		-0.122***		-0.086***		-0.123***
		(0.016)		(0.042)		(0.012)		(0.035)
Inactivity.								
< 1 year		-0.159**		-0.065		-0.144***		-0.081
		(0.066)		(0.060)		(0.055)		(0.050)
1-2 years		-0.177***		-0.129**		-0.163***		-0.124**
		(0.057)		(0.060)		(0.049)		(0.051)
2-3 years		-0.063		-0.093		-0.064		-0.056

Continued on next page

[9] Job history in the last 12 months is not conditioned to exclude self-employment. This is consistent with the approach adopted by Arulampalam (2001). Wave 1 interviews did not ask this question, and thus self-employment status cannot be easily determined for individuals who did change labour market status between 1990 and 1991. Halpin (1997) uses the overlap between Wave 1 and the retrospective labour market history information collected at Wave 2 in order to determine this information. I do not attempt to ascertain this, grouping previous employment and self-employment instead.

Table 5.4 – continued from previous page

	1991-1997				1991-2001			
	OLS		FE		OLS		FE	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
		(0.055)		(0.067)		(0.049)		(0.056)
3-4 years		-0.177***		-0.179***		-0.142***		-0.115**
		(0.046)		(0.064)		(0.043)		(0.053)
4 years +		-0.081***		-0.174**		-0.089***		-0.116**
		(0.023)		(0.070)		(0.018)		(0.051)
N	7666	7666	7666	7666	10912	10912	10912	10912
LL	-3079	-3074	1414	1422	-4225	-4222	1440	1445
$\bar{R}^2$	0.573	0.573	0.367	0.368	0.604	0.604	0.512	0.512
RMS error	0.363	0.363	0.202	0.202	0.358	0.358	0.213	0.213
AIC	6315.729	6321.938	-2673.422	-2674.406	8623.324	8633.461	-2707.988	-2702.320

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.
Significance levels: ***: 1% **: 5% *: 10%

Empirical results highlight persistent average wage penalties that depend on previous labour market status. These results replicate those of Arulampalam (2001) very closely. Table 5.4 presents fixed effects estimates based on an the unrestricted previous labour market definition. Specification [3] suggests that, relative to a job-to-job transition, previous inactivity carries the largest average wage penalty at 11.5%. Previous unemployment carries a relatively lower penalty of 8.4% into future employment.

Specifications 4 & 8 suggest that, relative to a job-to-job transition, entering one's current job via a spell of unemployment carries an estimated 5.0% average wage penalty in the first year of employment. This initial penalty is insignificant at conventional levels, but increases to a significant 11.7% over the first 3 years of employment, with a long run penalty of 12.2% after 4 years. Previous inactivity carries a higher initial penalty, 6.5%, which rises to a significant 12.9% in the second year, relative to the baseline. This penalty rises to 17.9% during the third year of re-employment. A significant penalty of 17.4% remains in the long run, however, the wage consequences of this category are likely to be less precisely measured due to heterogeneity within this labour market state. Due to small cell size, Arulampalam (2001) grouped previous labour market status into non-employment versus employment categories. This strategy is not pursued as this study is not hampered by this limitation to the same extent.

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 5.16 for the results of selected studies.

5.6 Extensions

5.6.1 1991-2001

Columns 4 & 8, table 5.4, highlight that extending the time-period under consideration to 1991-2001 reduces the economic significance of previous inactivity. Previous inactivity over the 1991-2001 period is estimated to carry a 9.9% wage penalty in the current job. As the interaction terms indicate, this penalty seems to be less precisely measured relative to the large persistent impact of previous unemployment. Coming into employment via a spell of inactivity is estimated to carry an insignificant wage penalty of 8.1% in the first year, relative to a job-to-job transition. This penalty increases to a significant 12.4% in the second year, and the long run penalty of 11.6% which is significant at the 5% level. Initial wage losses of individuals coming into their current job via a spell of unemployment shows no sign of recovery, with an initial penalty of 7.6% in the first year of employment, rising to around 12.3% in the long-run.

The results so far suggest that the wage penalty associated with a spell of unemployment is large and persistent, with no sign of recovery. Although magnitudes may differ, this is consistent with the findings in the existing literature. There is likely to be substantial heterogeneity across separation types. Data limitations have prevented many previous studies from explicitly addressing this issue. In what follows the impact of an interruption by reason for that interruption is considered. Displacements "for cause" are likely to carry different implications into future employment than separations that can be considered as independent of individual characteristics. Unfortunately, the BHPS does not contain enough information to accurately identify the impact of being sacked from a previous job. Furthermore, the UK institutional context means that the reported reason for leaving previous job categories are unlikely to be precise. Temporary contracts are likely to be stepping stones to permanent jobs (Booth *et al.* 2002), and whether someone reports themselves as redundant or sacked/dismissed is likely to be more subjective in the UK than the US (Borland *et al.* 2002). Unlike US DWS-based studies, the BHPS does not allow for the separation of layoffs from plant closures.

5.6.2 Reason for leaving previous job: Redundancy

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. A full analysis of involuntary job displacement is beyond the scope of this study, due to lack of administrative data. However, whether the impact of displacement on wage scarring varies with self-reported reason for leaving previous job is of interest. Focus is limited to impact of self-reported "involuntary" displacement (redundancies), which are likely to be an imperfect proxy for exogenous job separations.

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.

Wage Scarring effect of Incidence and Duration of previous Unemployment/OLF

Contrary to Arulampalam (2001), who finds a positive average impact of 1.8%, specification 1, Table 5.5, 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, grouping them into a non-employment category. 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 for multiple spells of unemployment the wage penalty associated with unemployment reduces with incidence, i.e. the first spell carries the highest penalty (consistent with the finding for non-employment in Arulampalam (2001). 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 5.5). 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 5.5: Log Real Hourly Wage Equations for Male Sub-Sample, Individual-level observed heterogeneity controls, Previous Status unrestricted. Robust Standard errors in parenthesis.

	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.
Significance levels: ***: 1% **: 5% *: 10%

5.6.3 Regional data

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 & 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 & 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 2008a). 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 5.2).

Table 5.6: Average wage penalties by previous labour market characteristics. Robust standard errors in parenthesis ("Reason for leaving previous job" not controlled for).

	Previous Labour Market Characteristics.					
Time Period	**Tight**	**Slack**	**Urban**	**Rural**	**High U**	**Low U**
	Previous Labour Market Status (ref. Employment).♭					
Unemployment						
1991 - 1997	*-0.085****	*-0.094****	*-0.052*	*-0.141****	*-0.084****	*-0.097***
	(0.030)	*(0.038)*	*(0.033)*	*(0.053)*	*(0.030)*	*(0.038)*
1991 - 2001	*-0.098****	*-0.098****	*-0.072****	*-0.141****	*-0.097****	*-0.104****
	(0.024)	*(0.030)*	*(0.025)*	*(0.044)*	*(0.024)*	*(0.030)*
Inactivity						
1991 - 1997	*-0.085*	*-0.102**	*-0.067*	*-0.114*	*-0.098**	*-0.056*
	(0.052)	*(0.057)*	*(0.052)*	*(0.073)*	*(0.051)*	*(0.055)*
1991 - 2001	*-0.078**	*-0.102***	*-0.073*	*-0.089*	*-0.089***	*-0.062*
	(0.043)	*(0.047)*	*(0.044)*	*(0.057)*	*(0.042)*	*(0.044)*
	Previous Labour Market Status (ref. Employment).†					
Unemployment						
1991 - 1997	*-0.079***	*-0.093****	*-0.052*	*-0.140****	*-0.084****	*-0.088****
	(0.034)	*(0.032)*	*(0.033)*	*(0.053)*	*(0.031)*	*(0.033)*
1991 - 2001	*-0.091****	*-0.106****	*-0.072****	*-0.141****	*-0.098****	*-0.098****
	(0.028)	*(0.027)*	*(0.025)*	*(0.044)*	*(0.024)*	*(0.024)*
Inactivity						
1991 - 1997	*-0.085*	*-0.092**	*-0.067*	*-0.115*	*-0.083*	*-0.094**
	(0.054)	*(0.054)*	*(0.052)*	*(0.073)*	*(0.052)*	*(0.053)*
1991 - 2001	*-0.064*	*-0.102***	*-0.073**	*-0.089*	*-0.078**	*-0.084**
	(0.044)	*(0.045)*	*(0.044)*	*(0.057)*	*(0.043)*	*(0.043)*

♭ 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 5.8.3. Significance levels: ***: 1% **: 5% *: 10%
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.

TTWA Labour market tightness

Table 5.6 suggests that the average wage penalty associated with a job interruption is slightly higher in slack local labour markets, however this difference is not statistically significant at the 10% level. 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 (using a pairwise t-test under the assumption of independence). A significant 10.2% wage penalty is associated with inactivity spells experienced in slack labour markets, however this is insignificantly different to the 8.5% penalty mentioned above. This remains robust over the 1991-2001 period, no significant differences in the penalty associated with unemployment or inactivity persist, conditional on labour market tightness.

This result could be confounded by heterogeneity across separation types conditional on labour market tightness. 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 5.6). But the average only tells part of 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 whilst the wage gain becomes marginal (0.1% gain) in tight labour markets the difference in the penalties is insignificant at conventional levels for the 1991-1997 and 1991-2001 periods. 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 possibility that higher levels of industry-specific human capital (more to lose from switching industry) and more disposable income imply that the older workforce are more likely to engage in productive search, i.e. more likely to be targeted in their search efforts, and more likely wait until an appropriate job offer is received than to accept the first job offer that comes along (Lippman & McCall 1976). Productive search suggests a positive relationship between non-employment duration and re-employment wages. However, this could also be due to an inappropriate control group, as wages are generally higher for those over 45 and wage profiles flatter (Kletzer & 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 13.4% gain in the first year (6.7% penalty, insignificantly different to the average effect), a 1.6% gain in the second (13.4%-11.8%), a 1.7% penalty in the third (13.4%-15.7%), and a long-run 1.6% wage penalty (13.4%-15%) relative to the counterfactual. However, all the short-term gains seem to be 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.

Being made redundant and then having a spell of inactivity (not reported) is found to carry an insignificant wage penalty into subsequent employment spells, over and above the average effect in panel 2, regardless of where it was experienced.

Time-varying Regional Heterogeneity Accessibility, a population-density based measure[10] 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 increase 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

[10] England & Wales: Based on the population density of the surrounding area. National Statistics Postcode Directory based. Measured at Output Area (OA) level. Local Authorities with more than 90% of OAs accessible are defined as accessible. Scotland: Based on driving distance to nearest large settlement (>10000 inhabitants). See Appendix D, Section D.5.1 for more information on construction.

Table 5.7: Wage Penalties: Labour Market Tightness†. Robust standard errors in parenthesis.

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%***		
		(0.042)	(0.033)		
Slack		-9.4%*	-10.4%***		
		(0.051)	(0.037)		
Inactivity					
Tight		-3.9%	-5.9%		
		(0.070)	(0.053)		
Slack		-5.8%	-7.4%		
		(0.071)	(0.055)		
Reason for leaving prev. job					
2. Redundancy§		-8.1%* (0.039)	-7.2%** (0.029)	-8.3%* (0.040)	-7.3%** (0.030)
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%**
		(0.065)	(0.055)	(0.065)	(0.054)
	$x \geq 45$	-5.1%	-7.8%	-5.5%	-8.0%
		(0.057)	(0.049)	(0.058)	(0.049)
Slack	ALL	7.6%	6.3%	7.8%	5.8%
		(0.079)	(0.071)	(0.077)	(0.068)
	$x \geq 45$	-3.1%	-3.7%	-2.2%	-3.3%
		(0.074)	(0.067)	(0.076)	(0.064)
Inactivity					
Tight	ALL	-7.5%	3.8%	-10.7%	2.4%
		(0.156)	(0.128)	(0.158)	(0.128)
	$x \geq 45$	12.1%	10.9%	12.4%	11.6%
		(0.227)	(0.219)	(0.221)	(0.215)
Slack	ALL	-16.2%	-5.9%	-5.9%	1.0%
		(0.190)	(0.089)	(0.219)	(0.119)
	$x \geq 45$	47.4%**	23.0%	57.6%**	35.2%*
		(0.219)	(0.165)	(0.237)	(0.190)
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%***	→	-15.0%***
	(0.046)	(0.044)	(0.051)		(0.052)
Slack	-12.4%*	-22.1%**	-7.0%	→	-11.5%**
	(0.075)	(0.093)	(0.170)		(0.056)
			1991 - 2001		
Tight	-7.6%**	-11.4%***	-12.4%***	→	-13.3%***
	(0.035)	(0.036)	(0.041)		(0.042)
Slack	-15.2%**	-18.8%***	-6.3%	→	-10.6%**
	(0.062)	(0.070)	(0.116)		(0.044)

† Tight labour market - Vacancies/Unemployment ratio > 2/3*Median. Significance levels: ***: 1% **: 5% *: 10%
§ 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 insignificant over and above the average impact. Full results available from author on request.

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 5.8: Wage Penalties: URBAN/RURAL†. Robust standard errors in parenthesis.

		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%**		
		(0.044)	(0.033)		
Rural		-19.3%**	-16.5%***		
		(0.075)	(0.062)		
Inactivity					
Urban		-0.1%	-4.0%		
		(0.067)	(0.054)		
Rural		-7.8%	-7.3%		
		(0.086)	(0.064)		
		Reason for leaving prev. job			
2. Redundancy§		-8.1%* (0.039)	-7.2%** (0.029)	-8.3%* (0.040)	-7.3%** (0.030)
		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%
		(0.074)	(0.060)	(0.074)	(0.059)
	$\geq$ 45	-2.4%	-4.3%	-2.2%	-4.0%
		(0.075)	(0.057)	(0.074)	(0.057)
Rural	ALL	21.7%**	16.7%*	21.4%**	17.3%*
		(0.100)	(0.089)	(0.100)	(0.089)
	$\geq$ 45	-9.5%	-12.4%	-9.7%	-13.5%
		(0.088)	(0.085)	(0.089)	(0.085)
Inactivity					
Urban	ALL	-14.3%	0.2%	-15.6%	1.0%
		(0.161)	(0.112)	(0.158)	(0.110)
	$\geq$ 45	38.0%**	27.0%**	34.1%*	27.1%**
		(0.191)	(0.136)	(0.185)	(0.131)
Rural	ALL	-8.3%	-7.9%	-3.9%	-3.4%
		(0.173)	(0.163)	(0.201)	(0.176)
	$\geq$ 45	9.0%	18.8%	7.1%	14.6%
		(0.306)	(0.334)	(0.331)	(0.343)
		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%	→	-9.6%*
	(0.047)	(0.049)	(0.056)		(0.055)
Rural	-18.6%**	-15.5%**	-24.3%***	→	-21.9%***
	(0.085)	(0.079)	(0.093)		(0.085)
			1991 - 2001		
Urban	-4.4%	-11.2%***	-7.7%**	→	-9.6%**
	(0.035)	(0.038)	(0.044)		(0.044)
Rural	-16.7%**	-15.0%**	-16.1%**	→	-20.4%***
	(0.069)	(0.065)	(0.074)		(0.076)

† Urban/Rural - Defined in text. Significance levels: ***: 1% **: 5% *: 10%
§ 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.

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 5.6 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, Chapter 4 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 & 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.

van Dijk & Folmer (1999) hypothesis: Weak support for the van Dijk & Folmer (1999) hypothesis is found for the UK (Table 5.9, Panel 1), on average and stronger support for over 45s made redundant in their previous jobs. However, the average differences in the wage penalties associated with unemployment experienced in high versus low unemployment regions are consistently insignificant at conventional levels (using a pairwise t-test under the assumption of independence). 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 5.6). 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

Table 5.9: WAGE PENALTIES: HIGH UNEMPLOYMENT/LOW UNEMPLOYMENT†. ROBUST STANDARD ERRORS IN PARENTHESIS.

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%***		
		(0.041)	(0.031)		
Low U		-13.1%**	-12.1%***		
		(0.055)	(0.042)		
Inactivity					
High U		-4.9%	-6.7%		
		(0.069)	(0.053)		
Low U		-1.8%	-4.8%		
		(0.072)	(0.054)		
REASON FOR LEAVING PREV. JOB					
2. Redundancy§		-8.1%* (0.039)	-7.2%** (0.029)	-8.3%* (0.040)	-7.3%** (0.030)
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%*
		(0.065)	(0.056)	(0.065)	(0.055)
	$\geq$ 45	-2.2%	-6.9%	-2.1%	-6.5%
		(0.056)	(0.048)	(0.057)	(0.048)
Low U	ALL	19.9%**	13.4%**	19.9%**	13.7%**
		(0.079)	(0.067)	(0.080)	(0.068)
	$\geq$ 45	-16.6%**	-12.0%*	-15.7%**	-11.9%*
		(0.078)	(0.071)	(0.078)	(0.070)
Inactivity					
High U	ALL	-12.2%	-1.5%	-13.3%	0.0%
		(0.133)	(0.098)	(0.135)	(0.099)
	$\geq$ 45	9.8%	13.7%	11.4%	15.1%
		(0.213)	(0.205)	(0.214)	(0.206)
Low U	ALL	22.6%	27.2%*	23.3%	28.8%**
		(0.180)	(0.146)	(0.186)	(0.144)
	$\geq$ 45	-5.4%	-8.6%	-6.9%	-10.3%
		(0.229)	(0.242)	(0.229)	(0.235)
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%**	→	-15.2%***
	(0.042)	(0.044)	(0.053)		(0.051)
Low U	-13.7%	-17.8%**	-16.0%**	→	-16.0%***
	(0.095)	(0.075)	(0.075)		(0.057)
		1991 - 2001			
High U	-8.0%	-12.3%***	-11.3%***	→	-13.5%***
	(0.033)	(0.035)	(0.042)		(0.042)
Low U	-13.2%*	-16.0%***	-10.4%	→	-14.7%***
	(0.072)	(0.057)	(0.065)		(0.047)

† High Unemployment labour market - ILO unemployment rate > 2/3*Median. Significance levels: ***: 1% **: 5% *: 10%
§ 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.

to extensions of the observation period, however, these estimates may be weakened/confounded due to *efficiency wage* arguments (Shapiro & 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 5.9. 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.

Table 5.10: Reason for leaving previous job (as proportion of those 'at risk' (Farber 1999))

Reason for Leaving Previous Job (♣)								
Prev. Unemployment	Redundant	Sacked	Temporary Job	Voluntary Quit	Missing	Other	N/A	Total
Low	0.029	0.004	0.007	0.091	0.010	0.030	0.029	0.200
High	0.133	0.013	0.036	0.350	0.044	0.109	0.114	0.800
Total	1777	192	466	4827	580	1517	1567	10926

♣ - (Proportion of total separations, by level of unemployment)

Incidence and Duration of Separations in previously high versus low unemployment regions: Table 5.10 suggests that, on average, separations are more likely in high unemployment Local Authorities, measured as a proportion of those in employment at survey date (including redundancies). Moreover, the evidence about average durations of previous spells does not suggest clear differences in spell durations across region types (see Table 5.11). If incidence is higher, but durations shorter in high than low unemployment regions (i.e.

high levels of labour market churn) then one would expect the negative earnings consequences of career interruptions to be lower on average from a human capital theory perspective. However, the descriptives suggest that whilst incidence is higher in high unemployment regions, there is no marked difference in average durations over the sample as a whole . This would suggest that the wage consequences of displacement in high unemployment regions would be higher on average. Establishing whether this is true, all else constant, would require further analysis.

Table 5.11: Average length of previous spell, by reason for leaving previous job

	Prev. Unemployment Level	
¶	Low (♯)	High (♯)
Missing	76	85
N/A	112	114
Voluntary Quit	56	51
Redundant	57	55
Sacked	16	19
Temporary Job	8	11
Other	40	36
Total	60	58

¶ - Reason for leaving previous job. ♯ - Local Authority Unemployment Level

The earlier literature on gross worker flows suggested that changes in the size and distribution of inflows into unemployment are the main determinant of the unemployment rate. This suggests that incidence of unemployment matters more for labour market outcomes. Cyclical unemployment is concentrated in groups with low exit probabilities. Thus, the observed procyclicality in average exit probabilities from unemployment may largely be explained by these compositional effects (Darby *et al.* 1986). Recent work has questioned the composition explanation (e.g. Shimer 2012). Moreover, recent literature, e.g. Elsby *et al.* (2009) and Petrongolo & Pissarides (2008) suggests that incidence and duration of unemployment are related to the business cycle. It would thus be fruitful in future work to investigate this further in relation to the van Dijk & Folmer (1999) hypothesis and research questions under test. Inflow rates countercyclical, especially for job losers (layoffs), whereas outflow rates are procyclical. This suggests that high unemployment levels in a recession are driven by longer unemployment durations, rather than higher incidence.

5.7 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 (productive in human capital terms) labour market state[11]. The average effect of an unemployment spell seems to be robust to classifying full-time education as a separate (productive) 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

[11] The impact of unemployment and inactivity is assessed relative to a base group which includes employment *and* full-time education. See appendix G for the sensitivity analysis. Further 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.

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 & Folmer (1999) hypothesis loses strength, with mixed support at best (see Appendix, Table 5.14 & 5.15). 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). Moreover, this definition retains the property of being objective, although 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 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.

5.8 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 & 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. Moreover, whilst incidence of unemployment matters and the significance of duration at conventional levels is not robust to extensions of the observation period, the impact of OLF spells runs mainly through the duration effect. 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 & 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.

Weaker long-run evidence is found supporting the van Dijk & Folmer (1999) hypothesis, on average and stronger support for over 45s made redundant in their previous jobs. This is robust to specification changes. 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. The low unemployment region results may be hard to reconcile with the regional mobility story, however recent work by the Policy Exchange think tank suggests that age discrimination may be an important driver in the wage outcomes of the UK's older workers and may help to explain this observation which is complementary to the main hypothesis under test (Tinsley 2012). 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 & Picchio (2009b) 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.

Appendix

5.8.1 Data Descriptives

Table 5.12: MALE SUB-SAMPLE BY PREVIOUS LABOUR MARKET STATUS.

	1991-1997.			1991-2001.		
PREV_STAT:	EMP.	UNEMP.	OLF	EMP.	NON-EMP.	OLF
	[1]	[2]	[3]	[4]	[5]	[6]
Personal Characteristics						

Continued on next page

Table 5.12 – continued from previous page

	1991-1997.			1991-2001.		
PREV_STAT:	**EMP.**	**UNEMP.**	**OLF**	**EMP.**	**NON-EMP.**	**OLF**
	[1]	[2]	[3]	[4]	[5]	[6]
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$^\sharp$	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

$^\sharp$: 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.

5.8.2 Selection Equations

Table 5.13: 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

Significance levels: ***: 1% **: 5% *: 10% (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.

5.8.3 Alternative Threshold Definition: $\geq$ Median.

5.8.4 Continuous Work-life histories

Figure E.1 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

Table 5.14: Wage Penalties: Labour Market Tightness†. Robust standard errors in parenthesis.

Previous Labour Market Status (ref. Employment).					
		Model A		Model B	
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%**	→ -12.2%**
Slack		-12.6%**	-16.7%**	-15.0%**	→ -16.1%**
		1991 - 2001			
Tight		-6.2%	-9.3%**	-10.2%**	→ -11.3%**
Slack		-11.6%**	-16.3%**	-12.2%**	→ -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. Significance levels: ***: 1% **: 5% *: 10%

§ 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.

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

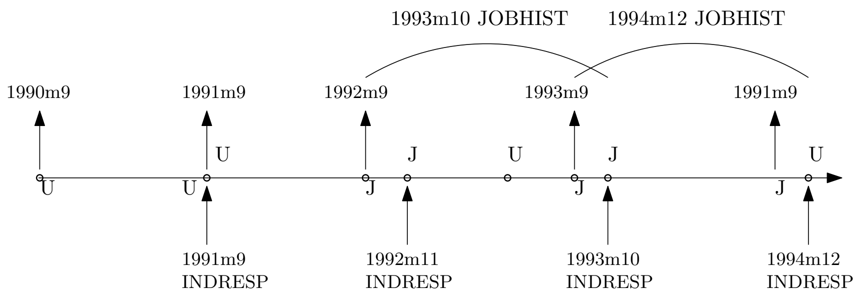
Table 5.15: Wage Penalties: High Unemployment/Low Unemployment†. Robust standard errors in parenthesis.

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 & 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. Significance levels: ***: 1% **: 5% *: 10%
§ 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.

highlighted issues in the literature as well as to aid replication. Appendix E documents and justifies the steps taken.

Figure 5.2: 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 5.16: SUMMARY OF SELECTED JOB DISPLACEMENT STUDIES.

Paper	Data	Sample	Key Findings
Cross-sectional Studies:			*Associated Earnings Penalties:*
Addison & 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 & 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[Δ].
Jacobson *et al.* (1993)	Matched Pennsylvanian Admin. Data	1974-1986. Prime aged: $\geq$6yrs. *continuous* tenure by 1980. Earnings losses 5yrs pre-displacement[♭]. Involuntary displacements[†].	*Penalty:* 40% immediately. 25% 6 yrs. later[Δ]. 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 & 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.

Continued on next page

Table 5.16 – continued from previous page

Paper	Data	Sample	Key Findings
Couch & Placzek (2010)	Matched Connecticut Admin. Data	1993-2004. Prime aged: $\geq$6yrs *continuous* tenure by 1999. Involuntary displacements†.	*Penalty:* 32%-33% immediately, 13%-15% 6yrs. later$^{\triangle}$. Largest earnings losses in highly paid service sector. Mitigated if don't change 4-digit industry.
Hijzen *et al.* (2010)	UK NESPD-IDBR-ARD	Aged 21 - 59; Earnings losses 8yrs. pre-displacement. Involuntary displacements†.	*Penalty:* 18%-35% firm closures; 14%-25% mass layoffs (to 5yrs later)$^{\triangle}$. 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 $\forall$ separation types.
Gregory & 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).

$^{\natural}$ 5-year retrospective window. NB. DWS: Displaced Workers' Survey. Agri/Cons: Agricultural/Construction Sector.
$^{\flat}$ 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). $^{\triangle}$ 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.

Chapter 6

Seeds of Change? Over-Education, Gender & The Persistence of Low Skilled Employment in Local Labour Markets.

6.1 Introduction

Education is not the piling on of learning, information, data, facts, skills, or abilities - that's training or instruction - but is rather making visible what is hidden as a seed.

Sir Thomas More (7 Feb 1478 - 6 July 1535)

The debate about whether formal qualifications reflect true ability has been raging on for centuries, yet the signalling effect of schooling has only been formalised in the last century (Spence 1973). Skill mismatch refers to the mismatch between the skill requirements of the job and the ability of the worker. Lack of adequate measures of skill have lead researchers to use formal qualifications as a proxy. This explains the proliferation of the over-education literature. In this context, over-education could be interpreted as an indicator the imperfect signal of schooling, i.e. overqualified workers have lower innate levels of ability than under-qualified workers. However, neoclassical proponents would argue that this is essentially a short-run phenomenon (Duncan & Hoffman 1981). If this transitory nature holds, then mismatched jobs may be a Stepping Stone to better matches for the overqualified. Moreover, spatial mismatch theories suggest that the Stepping Stone effect would not be the same across a country.

This study asks specific research questions: Does over-education (as a measure of observable skill mismatch) carry worse implications for workers in skilled or less-skilled occupations? How does this vary with gender, the composition of the local labour market and over the business cycle? Sub-regional differences in industrial and skill composition suggest a differential impact of economic downturns within a country. Does this prediction stand when confronted by the data? This study focusses on the UK and is the first study that I am aware of that considers the impact of over-education (and the associated skill mismatch) on the persistence of low-skilled employment. A key advantage of this approach is that, whilst it does not provide direct evidence of job requirements, it gets closer to to a classification of mismatch which takes into account heterogeneity of jobs (approximated by the average skill requirements of a detailed 3-digit occupation (ISCO methodology)). Moreover, in sensitivity tests job matches are further disaggregated by self-reported "job quality" using a method similar to Chevalier & Lindley (2009).

The "hidden brain drain" refers to the negative economic efficiency implications of Skill Mismatch. This phenomenon, key to a Knowledge Economy's prospects of sustainable long-run economic growth, is likely to arise if the existing skills base is not fully exploited and measures are not made to develop future skill potential (Connolly & Gregory 2008). If the long-term economic prospects of the unemployed are to be ensured, then moving them out of unemployment and into high quality employment is key not just for their well being but also for the nation in terms of aggregate welfare gains.

Employment quality can be measured in many dimensions, intrinsic and extrinsic; In this study the dimension of match quality and subsequent employment stability is of primary interest. The existing economic literature takes into account the pay or the stability dimension, but generally not consider both in an integrated framework. Moreover, the impact of occupational change and the associated Skill Mismatch on labour market outcomes is not explicitly controlled for in a dynamic regional context controlling for regional differences in industrial and skill composition. Analyses are primarily limited to pairwise comparisons of changes in occupational status between the current and previous time periods and not the persistence of Skill Mismatch. Due to the inherent difficulty of measuring the phenomenon, focus is mainly consigned to the less-advantaged, however increased occupational downgrading during economic downturns suggests that skilled workers pose a threat to the job stability of the less-skilled (Evans 1999). By jointly modelling both skilled and unskilled labour market transitions, this endogeneity can be taken into account. I explicitly control for the time-varying impact of regional environment. No studies which I am aware of have explicitly considered sub-regional variation in the impact of Mismatch on individuals' labour market flexibility, or the impact of time-varying regional heterogeneity in driving this phenomenon.

If the extend of Skill Mismatch increases during economic downturns, then this could profound implications for matching efficiency during recovery. If skill-mismatch is persistent, in that mismatched jobs do not act as Stepping Stones to better matches, then a situation where skilled vacancies are created that can't be filled with appropriately trained domestic workers is likely to arise. Domestic skill-shortages lead firms to change their hiring practises, making skilled migrant workers more attractive. As already stressed, this is likely to have a strong (sub-)regional dimension. All in all, this phenomenon has the potential of profound implications for unemployment persistence, as well as negative implications for long-term economic policy and public opinion.

Moving disadvantaged groups, e.g. the youth, women, part-time workers, those at the bottom end of the income distribution and the long-term unemployed into regular, stable employment is the main motivation for this research area. Addressing this issue adequately requires directly controlling for a wide range of econometric issues. Many studies addressing the Stepping Stone effect consign themselves to looking at job-to-job transitions, or considering employment and unemployment spells only (for example Stewart 2007). The original aim of this study was to consider unemployment, out of the labour force and self-employment spells as competing states. Computational considerations meant that this was limited to exclude the self-employed, considering high- and low-skilled employment versus non-employment as alternative destination states. Much of the existing literature has adopted a dynamic discrete-choice framework to address this issue (Stewart 2007). Given the dynamic context, the Initial Conditions Problem arises and needs to be accounted for (Hsiao 2003). Since with most labour market data an individual's complete labour market history is not observed, the initial state cannot be treated as exogenous: it is a product of an individual's previous labour market history. This initial state may either be determined by state dependence, or unobserved heterogeneity (Mosthaf *et al.* 2009). Various methods have been proposed to deal with this issue, each with their relative merits. See Section 2.2.4 for a brief discussion. To maximise sample size, ideally initial conditions from the year prior to entering the sample would be controlled for. In this study the initial conditions problem was not controlled for due to difficulties establishing this as individuals were allowed to enter the sample at any point in time over the observation period.

In what follows, due to data limitations in assessing both the skills of the worker (total human capital) and job skill requirements, I focus on over-education and not skill-mismatch. Moreover, I do not use these terms interchangeably. The motivation of this chapter was to study less-skilled employment persistence. Given data limitations the skill requirements of the job were proxied using an average measure of skill requirement at the 3-digit occupational level (International Standard Classification of Occupations). I investigate transitions into less-skilled employment, conditional on overqualification at t-1 in order to get closer to the skill requirements of the actual job. This gives an alternative slant on the issue of overqualification which can be motivated by the skill down/upgrading models presented by Evans (1999) and Léné (2011). I do not study transitions into overqualification, conditional on being in less-skilled employment at t-1. Given the barriers faced to fully implementing a dynamic estimation of the MNL, and sample size restrictions in the BHPS, further dividing the transition matrices into "employment skill"-"over-qualification" blocks would be unlikely to produce precise results if any in the current setup (the dynamic model failed to converge most likely due to small cell size issues). Looking at transitions into overqualification conditional on skill requirements of the previous job would be a more viable alternative. Future implementations could investigate this as an alternative perspective given that the literature on overqualification concentrates on the persistence of the phenomenon.

The paper is organised as follows. Section 6.2 describes and motivates the data construction based on the existing literature. Section 6.3 describes and motivates the empirical methodology. Section 6.4 examines descriptives relating to the impact of state dependence on the probability of transition to a mismatched job. The results from a pooled discrete-choice Multinomial Logit (MNL) are discussed in detail in Section 6.5. Robustness checks are conducted in the sensitivity analysis, Section 6.6, whilst Section 6.7 concludes.

6.2 Data Construction & Motivating This From The Related Literature

The British Household Panel Survery (BHPS) is restricted to exclude the European Community Household Panel (ECHP) sub-sample, as well as proxy respondents. I follow individuals from 1991 to 2008 on an annual basis (survey date as reference point) until the first instance of attrition *or* a key variable becomes missing at the reference point. This approach produces consistent estimates under the assumption that attrition random, and unrelated to $\forall X_i$. This also minimises attrition bias relative to a strategy which restricts the sample to Original Sample Members (OSM) continuously present for all waves. Consistency also requires that observations are not systematically missing. I allow individuals to enter at any point between 1990 and 2008, following using the selection rule above.

Individuals are followed from 16, or from their secondary school leaving age which ever is first, until the first instance of retirement. Most studies in the literature treat each period of employment with an employer as a single job spell. The definition of a job implemented in this study captures promotions as the start of a new job with the same employer. Following much of the literature, the occupation at the start of each job spell is taken to represent that of the spell. Thus occupational change within a firm will be captured. This can be justified since the main interest is in the *complexity/skill-content* of the job, which may change due to promotion or due to 'labour hoarding' during downturns. To the extent that queues for skilled jobs lengthen during downturns, labour market adjustments may manifest themselves in skilled workers moving down the occupational skill-ranking prior to firm exit (Evans 1999). Individuals who were ever self-employed (spells lasting at least a month) are dropped, however those were ever in full-time further education are not, with these spells treated as OLF spells. Previous and current labour market statuses considered are OLF, unemployment, high- and low-skilled employment. Previous labour market status is interacted with whether the individual was mismatch status in the previous period/spell to capture differences across skill categories.

British Household Panel Survey (BHPS) Employment biographies are constructed by merging the survey date and retrospective information collected in the BHPS (see Appendix Section E for details of how this was done). Attention is limited to complete labour market spells which started on or after the 1^{st} of September 1991. In order to minimise attrition bias, I do not restrict the sample to the Original Sample Members (OSM) continuously present over the observation period. I allow the OSM to enter the sample after 1991, but only follow respondents until the first instance of attrition. The BHPS contains time-invariant, spell-varying and time-varying information. Following Upward (1999), I assume that time-varying covariates only collected at the interview date were constant during the preceding year. Given this imputation is not carried out over survey dates, this measurement error is likely to be kept at a minimum. Industry of employment is recorded according to the Standard Industrial Classification 1980 (SIC80) up to wave 12, with the 1992 (SIC92) methodology superseding this from thereon. A detailed (3-digit) concordance scheme was kindly provided by Richard Upward. Unfortunately this scheme is unable to concord roughly 5% of pre-wave twelve SIC80 codes, affecting a fraction of less-skilled occupations in the current analysis. I do not drop these, keeping them in the reference case.

Table 6.1: International Standard Classification of Occupations (ISCO-88).

Skill Level	Description	Major Group
1	Competence associated with general education acquired through compulsory education	(9) Elementary Occupations
2	Requires knowledge as for skill level 1, plus a larger period of work-related training or work experience	(4) Clerks, (5) Service workers & shop & market sales workers (6) Skilled agriculture & fishery workers, (7) Craft & related workers (8) Plant and machine operatives and assemblers.
3	Requires a body of knowledge associated with a period of post-compulsory education but not to degree level	(3) Technicians & associate professionals
4	Requires a degree or an equivalent period of relevant work experience	(1) Legislators, senior officials & managers, (2) Professionals

Definitions: Low-Skilled Occupations: 1, 2; High-Skilled Occupations: 3, 4.
Category 4 includes "Corporate Managers", career progression into which may be independent of formal qualifications (Connolly & Gregory 2008).
Source: (Upward & Wright 2004)

Defining Occupational Skill Groups How to capture the skill requirements of a job through observational data is an issue of controversy. Skill groups are defined according the International Standard Classification of Occupations (ISCO-88). The ISCO-1988 defines skill-levels using both task- and competency-based measures: "Skill levels are linked to the length of time deemed necessary for a person to become fully competent in the performance of tasks associated with a job (Elias *et al.* 1999)". Occupations are classified into 4 skill groups, illustrated in Table 6.1, based on (1) the level of general education and (2) the level of job-specific training required to perform a job (Upward & Wright 2004). Groups 1-2 are classified as low-, whilst 3-4 as high-skilled. Thus the ISCO-88 based measure attempts to closely capture the actual skill *requirements* of a job. The key question, in this context, is the comparability of the occupations in each skill group over time, and whether their composition changes. Do occupations become more or less skilled on average? More importantly, does the ranking of occupations within each skill group change over time due to this?

In order to take this into account the composition of these groups should be allowed to vary over time. Ideally, a time-varying method of classifying occupations by skill *requirements* would also be available. The ISCO-1988 classification has been updated to 2008 rankings, and a concordance between the two methods is available from the International Labour Organisation [1]. I draw on the up-to-date 2008 rankings, *as a robustness check.* As with most studies using other classifications, the ISCO-1988 methodology is a fixed ranking of occupations. Given the time of its release, it is likely that substantial compositional changes may have changed within occupations. These questions suggest the presence of significant cohort effects, as standardisation due technological change results in once skilled occupations becoming unskilled. Moreover, at the same time "skill upgrading" of the existing workforce through the acquisition of further qualifications implies a dynamic context not adequately captured in a static analysis. If individuals do not *retrain* and change occupation, then their once skilled human capital would gradually become less skilled over time. Alternative measures include social prestige indicators (Goldthorpe index) and average earnings.

Using average earnings in this context makes the implicit assumption that high wage jobs are skilled jobs. Connolly & Gregory (2008) use a *fixed* ranking of detailed SOC90 occupations by average wages paid in 2000. Since their study considers a 11 year period, significant changes in the skill composition of occupations are likely to have occurred. This issue is likely to exacerbated in the current study, given that 18 years are considered. Granted, Table A1 in their appendix highlights that a ranking based on average wages, as employed in Manning & Petrongolo (2008), would not perfectly match a ranking

[1] http://www.ilo.org/public/english/bureau/stat/isco/isco08/index.htm

based on average qualifications attained. If technological change favours workers with higher skill levels, then we would expect the two approaches to be better aligned. Evidently there are other forces at play in determining earnings. This could be due to higher levels of *experience*, rather than formal qualifications, tending to characterise pathways into managerial-level jobs (Connolly & Gregory 2008). More recently, the task-composition of occupations has been used to make a distinction between complex and routine jobs using the Dictionary of Occupational Titles (Goos *et al.* 2009).

Defining Over-education in the Literature Overeducation and Skill Mismatch are terms which have been used interchangeably in the literature as they both attempt to capture the same thing: The mismatch between the actual and required skills to undertake the job at hand. The assumed equivalence of the terms is mostly due to data limitations, rather than true equality. At the best, formal education levels are highly correlated with skill, but to argue that overeducation implies underutilisation of skills is fraught with difficulties due to the difficulty of measuring human capital. The main focus of the Overeducation literature is on the impact of "skill underutilisation" on earnings, although persistence has been taken into account. This is spurred by the observation of a decreasing return to education of college graduates (college premium) in the US during the 90s. The essential argument is that this is a cohort effect, due to supply-side factors forcing highly educated workers to take less-skilled jobs, rather than solely demand-led due to Skill Biased Technological Change. In a Human Capital Theory framework, the expected sign of the returns to schooling parameter in a wage equation is positive as it is assumed that schooling raises on-the-job productivity. However there is substantial evidence to suggest that schooling is an imperfect signal of actual ability, and that schooling may be purely a screening device in the recruitment process (Spence 1973). The implicit assumption of the basic Human Capital model is that an individual with low levels of schooling would be unproductive in a highly educated worker's job. This is at odds with the stylized facts, as the proportion of individuals with low formal education levels employed in highly skilled jobs is non-negligible. Proponents of Human Capital Theory would contest that total human capital is what matters, and thus a trade-off exists between formal education and on-the-job experience. Lack of adequate measures that quantify on-the-job training imply that direct tests of this notion are limited. This also makes it difficult to gauge the true extent of total human capital, and thus mismatch. The fundamental question pertains to the causal effect of moving someone with a given level of education from one job to another. Sample size restrictions, coupled with the fact that job changes are relatively infrequent, makes identifying this causal effect difficult using programme evaluation techniques.

Coming from a neoclassical point of view, Duncan & Hoffman (1981) argue that overeducation is only a serious long-run issue if changes in the relative supplies of education do not affect the skill composition of labour *demand*, in other words firms do not adjust their production techniques to cater for changes in the industrial skill base. In essence, their argument is that Skill Mismatch is a short-run phenomenon. The difficulty of measuring *true* skill mismatch has driven the educational mismatch literature. If market failure implies that skill underutilisation is common, then this will have important implications for the economy in the aggregate.

Both subjective and objective measures of overeducation have been commonly employed. The subjective approach takes advantage of survey data with questions relating directly to the minimum (skill/education) requirements of the job. Whilst this is more likely to provide direct evidence of skill mismatch, lack of these questions in the survey design mean that researchers have commonly been restricted to indirect measures. Common objective approaches attempt to determine the educational requirements of the *job* by using job title information and occupational classification systems like the Dictionary of Occupational Titles. One example of an objective approach considers individuals with formal qualifications that are 1 standard deviation above their (detailed) occupation's mean as overeducated. Yet another compares actual educational levels to modal values in (detailed) occupational groups. McGuinness (2006) provides a detailed survey. Objective approaches are generally considered to be inferior to their subjective counterparts. Objective approaches almost invariantly use a time-invariant classification of occupations to calculate mismatch. However, this is a dynamic problem as average educational requirements are likely to evolve over time as the relative supply of highly skilled individuals increases. Moreover, the Reder hypothesis (Reder 1955) highlights that hiring standards are likely to respond due to fluctuations in the business cycle. Despite their inherent limitations, strong arguments in favour of the modal objective approach have been put forward. "[If] a particular occupation contains a higher proportion of overeducated workers, this will raise the occupational average and corresponding cut-off point thus underestimating the true level of overeducation (McGuinness 2006, pp. 396)." In a dynamic context, the fact that the proportion of highly skilled individuals working in an occupation increases due to occupational downgrading does not necessarily make that occupation skilled. However, if these individuals have difficulty moving back into skilled employment then this may lead to employers increasing their hiring standards *once and for all*.

Chevalier (2003) defines overeducation by skill-level, but distinguishes between "apparently" and "genuinely" overeducated if they are satisfied or dissatisfied with their match, respectively. This approach allows for the account of heterogeneity of job match quality, giving clues into the unobserved relationship between actual and skill requirements. Chevalier & Lindley (2009) implement this definition in a study of over-education in the UK graduate labour market. They find profound differences between genuinely and apparently over-educated graduates, whilst little observed differences exist between the latter and well matched graduates. Moreover, being genuinely over-education increases unemployment duration by three months but has no impact of the number of jobs held (Chevalier & Lindley 2009). Chevalier (2003) criticizes objective approaches:

> Defining overeducation as departing from a norm ignores differences in the quality of education, assumes homogeneity of skills of all workers with identical qualification levels, and overestimates the extent of overeducation. An objective measure based on a professional classification of occupations, e.g. DoT, assumes that all jobs within the same title have the same educational requirements. Furthermore, these classifications tend to be out of date. A statistical definition, based on the observed distribution of qualifications by occupation, is sensitive to cohort effects, sensitive to the level of aggregation, and assumes that all jobs with the same title have the same educational requirements. Both approaches also assume that individuals with a given level of education are perfect substitutes. Empirical results are also likely to be sensitive to the definition of overeducation (Chevalier 2003).

A meta analysis by Groot & Maassen van den Brink (2000) highlights that the choice of overeducation definition has a large impact on the incidence of overeducation, but not on the wage penalty associated with overeducation.

Over-education Individuals working in each 3-digit occupation are considered overqualified if their attained highest qualifications a higher than the 3-digit occupational *mode*[2] (or one standard deviation above the mean in the sensitivity analysis).

This approach use a fixed ranking (Q3 2000 QLFS) of SOC92 major (3-digit) occupations by the average highest observed qualifications attained using a 7-point scale index approach similar to (Connolly & Gregory 2008). Following the same methodology used by the LFS to group qualifications, the scale assigns the following values to qualification groups:

- 0 - No qualifications
- 1 - Other qualifications
- 2 - Sub GCSE/O'Level Equivalent
- 3 - GCSE/O'Level Equivalent
- 4 - A'Level or equivalent
- 5 - HND or equivalent
- 6 - Degree level or above

An alternative approach would be to use a time-varying ranking of occupations by mean qualifications, thus allowing the extent to which they are mismatched to vary with their 3-digit occupation's observed skill composition. This approach would be not with its short-comings. An important caveat of the occupational ranking approach is detailed in Elias *et al.* (1999):

> "Simply ranking occupations according to the level of qualifications held by typical job-holders within the occupation groups will produce some undesirable results. For some occupations an increase in the level of qualifications may be an artefact of the dramatic increase in educational qualifications held by younger age cohorts. If the increase in supply of higher qualified individuals left to a

[2]Maximum mode in the case of multi-modal distributions.

> uniform increase in all occupation groups then this would not be a problem. However, as the increase is most marked in the younger age cohorts then occupations with relatively high proportions of young people may appear to qualify for occupation upgrading even if there is no change in the tasks performed. In addition some occupations are known to be popular forms of employment for students such as bar work. This approach of ranking occupations by the qualifications of those who occupy them could, therefore, lead to inappropriate decisions (Elias *et al.* 1999)".

Whilst true, this argument does not take into account demand side interactions, as employers may raise or lower their hiring standards as a response to business cycles and/or increased supply of skilled graduates (Reder 1955). Moreover, using a fixed professional classification of occupations like the ISCO is likely to minimise this particular issue.

Unfortunately the BHPS does not contain a direct measure of over-education, as individuals were not asked this directly in the questionnaire. An indirect measure which compares individual qualifications to the average/mode within an occupation is generally considered inferior to alternative indirect (set by professional bodies, like the Dictionary of Occupational Titles (DOT) in America, and direct measures. It assumes that jobs within an occupation are homogenous and is a product not just of job requirements but also supply and demand (Leuven & Oosterbeek 2011).

Labour Market Tightness Labour market tightness summarises the efficiency of the matching process. A tight labour market is associated with excess demand, as their are more vacancies chasing fewer job seekers. 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 & 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. Following convention, labour market tightness is proxied by the vacancies-to-unemployment ratio. The aggregate Claimant Count is continuously available from NOMIS over the period of interest. Unfortunately a breakdown of the Claimant Count by occupation is not: availability is restricted to the 1996 to 2000 and 2005+ periods only. Furthermore, whilst there is only a one year gap in notified vacancy statistics by country or government office region, at lower levels of aggregation this is two years. This should be taken into consideration when interpreting *Travel-to-Work Area (TTWA)* rates. Labour market tightness is controlled for at the aggregate level in order to capture business cycle effects. Missing values for the country and government office region series (1 year gap) are assumed to be the same as those in the same quarter of the preceding year. In the case of the TTWA series, the first four quarters are imputed using the aforementioned approach. The last four quarters are assumed to be the same as those in the same quarter of the following year. As mentioned in Chapter 4 and 5, Bentley (2005) warns that the vacancies series may be non-comparable pre- and post- this gap in the series due to significant changes in the way vacancies were collected. This caveat aside, figures 6.1 highlights significant variation in the Government Office Region series across the United Kingdom. Despite having the highest levels of economic activity and *resident* occupational skill composition rates, combined with *significantly* higher industrial skill composition (employee-based) than anywhere else in the UK, the London region has the lowest Travel-To-Work-Area vacancies to unemployment ratios over the period. This suggests significant mismatch between the resident claimant count and the vacancies created in this region. Geographical differences in aggregate vacancies to unemployment ratios could also be driven by regional variation in

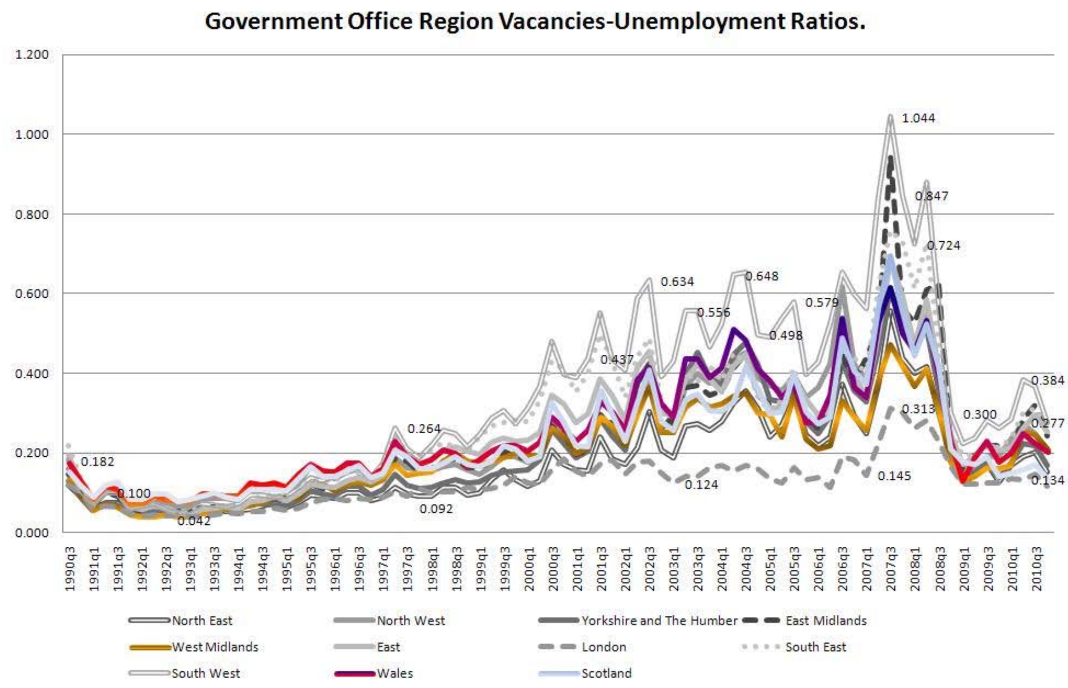


Figure 6.1: Average Vacancies to Unemployment Ratios by Government Office Region.
Source: NOMIS.

the average propensity to use alternative channels to advertise vacancies, which is likely to be related to the industrial mix of the region in question and thus the skill requirements of employers. Employers requiring employees with highly specialised skills are more likely to acquire these through specialist recruitment agencies.

It would be desirable to control for the growth rate of local industry. The Annual Business Inquiry was drawn on to define a measure at the TTWA level of aggregation. However, this indicator captures employment and not productivity growth. Industry-specific growth rates were assigned to individuals based on their TTWA of residence and their self-reported industry. The limitation of this approach is that it assumes that individuals live and work in the same TTWA. This is less likely for skilled workers. Lack of information about TTWA of work limits the usefulness of this indicator. Moreover, time and computational constraints meant that the decision was made to restrict the analysis to local business cycle effects only (approximated by fluctuations in the Vacancies/Unemployment rate).

Local Occupational Skill Composition An Occupational Skill Intensity measure, defined at the local authority level, is drawn from the QLFS. Data limitations imply that only a broad-banded measure can be constructed, with no possible breakdown to ISCO-88 groups. Skill Intensity is defined as the proportion of all employees aged 16 and over, *resident in an area*, and working in the following occupational classifications: Managers & Senior Officials; Professionals; Associated Professionals & Technical; Admin. & Secretarial [3]. Skilled Trades, Personal Service Occupations, Sales and Customer Service Occupations, Process, Plant and Machine Operatives and Elementary Occupations make up the denominator. This categorisation is based on a fixed ranking of occupations based on an index of highest qualifications held by employees in each SOC90 1-digit industry is developed from Census 2001 data (see Appendix, Section I.2).

Changes in weighting methods and discontinuities in data collection strategies imply that the level values of these regional-level series may not be very comparable across time. However, regional-level variables are standardised across regions by month. This captures the evolution of where a Local Area sits in Great Britain relative to the distribution of Local Area values of the covariate in question. Whilst standardisation changes interpretation, this is more likely to better capture the underlying phenomena of interest.

[3]This measure is likely to suffer from measurement error due to heterogeneity of skill-intensities within detailed occupational categories. However, this was the best strategy given the data at my disposal.

6.3 Empirical Strategy

Dual labour market model: Evans (1999) describes a two-sector "dual labour market model" involving a skilled primary sector, in which wages are high, and an unskilled secondary sector where wages are low. Whilst the unskilled sector is competitive and is characterised by market-clearing, the skilled sector is not. Imperfect competition in the skilled sector implies that supply and demand do not equalize. Under these assumptions, an unemployed skilled worker can either que for a skilled job, or accept an unskilled one whilst they continue to search on-the-job for a better match (unskilled workers are not upwardly mobile). Evans (1999) argues that whilst accepting an unskilled job may pay more than being unemployed, this may have negative future ramifications if unskilled jobs are seen by potential employers as a negative signal of future productivity on the job. Although not considered in this study, duration dependence is likely to be important in this context. Human capital specific to skilled jobs will depreciate the longer it takes for a skilled job seeker to find an improved match. The longer a skilled worker works an unskilled job, the more unskilled they become (Khalifa 2010). Furthermore, demand for skilled workers in unskilled jobs may be low if commitment to the job is brought into question. If skilled workers are using unskilled jobs as Stepping Stones to skilled employment, then this may be the case. The effect of this will be to increase the transition rate into non-employment spells.

The intuitive setup is based on the dual labour market model described in Evans (1999). If an individual's highest qualifications deviate from their 3-digit occupation's modal value, this is taken as a sign of Skill Mismatch. Highest qualifications attained are used to proxy individuals' skill levels, which are allowed to vary over time. However, education is an imperfect proxy for productivity on the job (Spence 1973). The more time a "high skilled" worker spends in a low skilled job, the more “low skilled” they become due to depreciation of their under-utilised skilled human capital (Khalifa 2010). Furthermore, skills depreciate whilst in non-employment. The dynamic nature of the problem highlights the importance of duration as well as state dependence in driving the Stepping Stone effect of mismatched employment. A more recent theoretical contribution to the literature, Léné (2011), investigates the extent to which education and experience matter for access to skilled jobs in a general equilibrium framework. This paper is motivated by the recent trend of decreased substitutability of education and experience for the less-skilled in the labour market. Unlike Evans (1999), in this study less-skilled workers are not restricted to job-search in the less-skilled sector and may upgrade into higher-skilled employment if experience allows. Increased supply of highly educated workers limits the less-skilled’s possibility of accumulating experience and thus their career outcomes. Léné (2011) argues that this evidence points to the every increasing need of both education *and* experience to progress into skilled jobs.

The existing literature generally adopts a dynamic multiple discrete choice data structure (competing risks in the limited studies which have assessed this issue in a duration context). However, these studies tend to limit themselves to unemployment and employment, ignoring individuals in inactivity. It is common for studies using survival analysis techniques to treat transitions to inactivity as right censored or to drop them completely. In the duration context, treating inactivity as a *censored destination state*, may lead to inconsistent estimates of the parameters determining the transitions of interest as this assumes away unobserved characteristics affecting both transitions of interest and those to censored states (van den Berg & van Ours 1994; van den Berg & Lindeboom 1998). In discrete-choice Multinomial Logit models (MNL), this restriction would only be valid if this same restriction were present on the contemporaneous individual choice set however, in general restrictions reduce the generalisability of results. Inappropriate restrictions imply bias induced by correlation with unobservables (Omitted Variable Bias). Furthermore, the discrete-choice framework does not take into account duration dependence. Like serially correlated errors, ignoring duration dependence leads to inefficient estimation and incorrectly standard errors (Singer & Willett 2003).

6.3.1 Discrete-Choice Multinomial Logit Model

A pooled dynamic discrete-choice Multinomial Logit (MNL) model (analogous to the standard MNL for cross-section data) is estimated in order to study the impact of state dependence on the probability of downgrading or upgrading. This estimation strategy is motivated in Methodology Section 2.2.4 which highlights ambiguities in the ordering of the labour market states under consideration as a reason for not imposing ordinality on the dependent variable. If we assume that the lag structure of state dependence follows an AR(1)/Markovian process, i.e. All that matters for time t is what happened at t-1, then this model can fully incorporate state dependence, conditional on the labour market status at t-1. However, if time-series correlation extends beyond t-1, then this model will be misspecified and likely biased. The standard model also makes the assumption that the error term is independent of the explanatory variables, an assumption which is unlikely to be valid unless the model is fully specified. The MNL assumes that the alternative-specific error are independent of the errors of other alternatives in the choice set. More over the MNL assumes that these errors are Type-I extreme value distributed. Violation of these assumptions raises concerns about the validity of regression estimates. The standard MNL takes the following form:

$$Pr(y_i = m|x_i) = \frac{\exp(x'\beta_m)}{\sum_{j=1}^{m}\exp(x'\beta_j)} = \frac{\exp(x'\beta_m)}{1+\sum_{j=2}^{m}\exp(x'\beta_j)} \tag{6.1}$$

Equation 6.2 and 6.3 illustrate the assumptions required for identification of the parameters in the standard dynamic MNL. Whilst the MNL is more flexible than the standard Logit model, it places restrictions on the underlying choice structure. For instance, the MNL only captures variables that are constant across alternatives, e.g. race (Cameron & Trivedi 2005). The MNL also suffers from the Independence of Irrelevant Alternatives (IIA) restriction. Since the MNL coefficients are relative to a normalised base category, "discrimination amongst the m alternatives reduces to a series of pairwise comparisons that are unaffected by the characteristics of alternatives other than the pair under consideration (Cameron & Trivedi 2005, p. 593)." In other words, the ratio of probabilities of any two alternatives is independent of the probability of any other outcome. Thus adding an extra alternative to the range of outcomes has no impact on this ratio, an unrealistic feature of the model for describing most real world decision making processes. A natural alternative, if the IIA assumption is problematic, is to jointly model alternatives in order to break this restriction. The basic setup models outcomes as described in equation 6.2.

$$y_{ijt} = x_{it}\beta_j + y_{it-1}\gamma_j + (d'_{it-1}y_{it-1})'\phi_j + \varepsilon_{ijt} \tag{6.2}$$

Thus, the instantaneous probability can be represented as:

$$Pr(y_{ijt}|x_{it}, y_{it-1}) = \frac{exp(x_{it}\beta_j + y_{it-1}\gamma_j + (d'_{it-1}y_{it-1})'\phi_j)}{\sum_{k=1}^{3} exp(x_{it}\beta_k + y_{it-1}\gamma_k + (d'_{it-1}y_{it-1})'\phi_k)} \tag{6.3}$$

Where:

y_{ijt}	$=$	Individual i's labour market status (j) at time t.
j	ϵ	(High Skilled Emp.; Low Skilled Emp.; Non-Emp.)
x_{it}	$=$	Matrix of observed personal, regional characteristics (time-invariant and time-varying) and business cycle effects. I also include a measure of experience due to recent findings of the joint significance of education and experience (Léné 2011).
y_{it-1}	$=$	Previous labour market status (t-1), interacted with previous industry and workplace characteristics.
d_{it-1}	$=$	Categorical variable capturing whether an individual was matched, underqualified or overqualified in previous labour market status.
ε_i	$=$	Idiosyncratic error component.

A pooled MNL is estimated, under the assumption that the error term is independent of the x_{it} conditional on $y_{i,t-1}$. If the model is not fully specified then this assumption is unlikely to hold, and it will be impossible to distinguish between true and spurious state dependence. In order to disentangle true from spurious state dependence, a random individual-alternative-specific intercept term (α_{ij}) can be introduced to control for time-invariant factors influencing an individual's probability of choosing a particular outcome, which varies across alternatives (random taste variation). Since these random effects are allowed to be correlated across alternatives, this strategy has the added advantage of relaxing the Independence of Irrelevant Alternatives assumption, formal tests of which are viewed with caution (Train 2009).

In the main analysis I condition the standard pooled Multinomial Logit estimates on a full set of Mundlak (1978) terms, in order to approximate the fixed effects specification. This specification thus identifies off the time variation in the explanatory variables, and thus these terms cannot be included for time-invariant characteristics like race. Imbens & Wooldridge (2007) show that both in the pooled and the random effects cases, inclusion of Mundlak (1978) terms (time-averages of the time-varying covariates) in a panel regression leads to the fixed effects estimator *in the linear panel case.* This approach has been adopted in much of the empirical literature (e.g. Stewart 2007). The "correlated" random effects approach (combining the Mundlak and random effects approaches), or controls for initial conditions (Wooldridge 2005 approach), are not currently incorporated in the sensitivity analysis due to computation limitations hampering its timely implementation. Thus the results are subject to the IIA restriction. Average Marginal Effects (AMEs) are estimated in order to approximate the Average Partial Effects of interest. Estimated is carried out using the delta method, via standard in-built Stata routines, as a numerical solution exists for the ML function. An estimation strategy for the random effects Multinomial Logit is discussed in Chapter 2. This strategy was not drawn on (possibly) due to small cell size issues implying that the routine did not converge. Estimation was carried out on an Intel PC with 8GB RAM running 4 cores at 2.86GHz each, using 64-bit Stata MP version 11.

Table 6.2: INDIVIDUAL CHARACTERISTICS. CURRENT LABOUR MARKET STATUS: ISCO1988-BASED. MALES *vs.* FEMALES, ANNUAL DISCRETE-CHOICE DATA, 1991 - 2008.

	MALES			FEMALES		
	LSKEMP.	SKEMP.	NONEMP	LSKEMP.	SKEMP.	NONEMP
Variable	Mean (S.D)	Mean (S.D)	Mean (S.D)	Mean (S.D)	Mean (S.D)	Mean (S.D)
Age						
30 - 45	0.381	0.458	0.207	0.386	0.449	0.344
	(0.486)	(0.498)	(0.405)	(0.487)	(0.497)	(0.475)
45 +	0.311	0.351	0.496	0.342	0.338	0.394
	(0.463)	(0.477)	(0.5)	(0.474)	(0.473)	(0.489)
School Type Attended						
Grammar no fee	0.054	0.196	0.069	0.098	0.184	0.099
	(0.225)	(0.397)	(0.254)	(0.298)	(0.388)	(0.299)
Private	0.031	0.09	0.037	0.031	0.086	0.047
	(0.173)	(0.285)	(0.188)	(0.175)	(0.28)	(0.211)
Technical	0.07	0.107	0.115	0.082	0.117	0.103
	(0.255)	(0.309)	(0.319)	(0.275)	(0.321)	(0.304)
Highest Academic Qualifications						
Degree	0.046	0.354	0.061	0.046	0.334	0.056
	(0.21)	(0.478)	(0.239)	(0.209)	(0.472)	(0.229)

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Table 6.2 – continued from previous page

	MALES			FEMALES		
	LSKEMP.	**SKEMP.**	**NONEMP**	**LSKEMP.**	**SKEMP.**	**NONEMP**
Variable	**Mean (S.D)**	**Mean (S.D)**	**Mean (S.D)**	**Mean (S.D)**	**Mean (S.D)**	**Mean (S.D)**
Other Higher	0.301	0.36	0.173	0.221	0.376	0.162
	(0.459)	(0.48)	(0.378)	(0.415)	(0.485)	(0.368)
A Levels	0.148	0.118	0.122	0.14	0.08	0.107
	(0.355)	(0.323)	(0.327)	(0.347)	(0.272)	(0.31)
O Levels	0.23	0.101	0.167	0.28	0.141	0.219
	(0.421)	(0.302)	(0.373)	(0.449)	(0.349)	(0.413)
Vocational Training						
Yes	0.395	0.406	0.288	0.381	0.436	0.269
	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)
Individual Characteristics						
White	0.96	0.956	0.936	0.961	0.96	0.931
	(0.195)	(0.205)	(0.244)	(0.194)	(0.195)	(0.254)
Married/Cohabiting	0.683	0.789	0.555	0.729	0.739	0.685
	(0.465)	(0.408)	(0.497)	(0.444)	(0.439)	(0.464)
Children	0.331	0.382	0.223	0.387	0.357	0.517
	(0.471)	(0.486)	(0.416)	(0.487)	(0.479)	(0.5)
Children X Married/Cohabiting	0.326	0.377	0.212	0.328	0.306	0.389
	(0.469)	(0.485)	(0.408)	(0.47)	(0.461)	(0.487)
Employed Spouse	0.524	0.635	0.217	0.655	0.671	0.472
	(0.499)	(0.482)	(0.412)	(0.475)	(0.47)	(0.499)
Health Limitations	0.087	0.054	0.444	0.091	0.08	0.291
	(0.282)	(0.226)	(0.497)	(0.288)	(0.272)	(0.454)
Disabled	0.011	0.005	0.152	0.006	0.005	0.058
	(0.107)	(0.07)	(0.359)	(0.08)	(0.07)	(0.234)
Housing Tenure						
Owned Outright	0.146	0.142	0.193	0.156	0.136	0.193
	(0.354)	(0.349)	(0.395)	(0.363)	(0.343)	(0.395)
Mortgage	0.637	0.74	0.273	0.618	0.736	0.374
	(0.481)	(0.439)	(0.446)	(0.486)	(0.441)	(0.484)
Council	0.104	0.018	0.345	0.117	0.029	0.271
	(0.305)	(0.132)	(0.475)	(0.321)	(0.169)	(0.444)
Housing Assoc.	0.036	0.012	0.075	0.037	0.016	0.073
	(0.187)	(0.107)	(0.264)	(0.188)	(0.124)	(0.26)
Work-Related Training in last 12 months (+ part-time courses)						
Yes	20.65	20.38	24.813	21.647	19.55	22.97
	(12.869)	(11.031)	(16.377)	(12.718)	(10.853)	(13.705)
Potential Experience						
Pot. Experience	1.588	1.848	0.883	1.629	1.801	1.413
	(2.095)	(2.101)	(1.777)	(2.132)	(2.083)	(2.02)
X 30 - 45	2.322	2.473	4.037	2.514	2.366	3.036
	(3.52)	(3.418)	(4.161)	(3.546)	(3.363)	(3.829)
X 45 +	0.279	0.406	0.052	0.264	0.467	0.058
	(0.449)	(0.491)	(0.222)	(0.441)	(0.499)	(0.233)

Skill groups are defined according the International Standard Classification of Occupations (ISCO-88). The ISCO-1988 defines skill-levels using *both task- and competency-based measures*: "Skill levels are linked to the length of time deemed necessary for a person to become fully competent in the performance of tasks associated with a job (Elias *et al.* 1999)". Occupations are classified into 4 skill groups, illustrated in Table 6.1, based on (1) the level of general education and (2) the level of job-specific training required to perform a job (Upward & Wright 2004). Groups 1-2 are classified as low-, whilst 3-4 as high-skilled. Thus the ISCO-88 based measure attempts to closely capture the *actual* skill *requirements* of a job.

6.4 Descriptive Analysis

Table 6.3: Current Labour Market Status: By Gender, 1991 - 2008.

	Males		**Females**	
jbstat4	Freq.	Percent	Freq	Percent
unskill emp	14,795	48.25	18,727	46.02
skilled emp	11,082	36.14	11,007	27.05
non-emp	4,789	15.62	10,962	26.94
Total	**30,666**	**100**	**40,696**	**100**

NB. Survey Date Reference Point.

In accordance with the literature, gender has a marked impact on current labour market status. Whilst both males and females are more likely to be in less-than more skilled employment at time t, women are much more likely to be in non-employment than men (Table 6.3). Over the 1991-1998 sample, females are in non-employment at survey date 27% of the time whilst for men this is more than 10% lower at 16%. These gender differences are carried through to detailed descriptives, which suggest greater persistence of non-employment for females than males (Table 6.2). However, for women the probability of non-employment does not seem to be related to over-qualification. Married females are on average 69% more likely to be in non-employment at time t than those than are not married. For married males this figure is lower at 56%. Moreover, married females with at least one dependent child in the house are 38.8% more likely than those that are married without children to be in non-employment whilst for males this is 21%. Women with an employed spouse are also 46% more likely to be in non-employment than those without. This figure is 21% for equivalent males.

Summary statistics in Table 6.2 suggest that younger individuals (<30) are more likely to be in low skilled employment or non-employment, whereas prime-aged males and females (30-45) are more likely to be in high skilled employment. Individuals over 45 are more likely to be in non-employment that other age groups, independent of gender. Whilst the majority of individuals in high skilled employment at time t attended Comprehensive (state) high schools, attending a Grammar or Private school increases the probability of being in high skilled employment at time t by more than Comprehensive attendance. In fact, the unconditional probability of being in low-skilled employment at time t is on average higher for those that attended Comprehensive schools. As one would expect, higher formal qualifications increase the probability of being in high skilled employment, but only for qualifications above A Levels. Vocational qualifications also increase this probability, with the largest effect being on female skilled employment probability.

Table 6.4: Regional Characteristics. Current Labour Market Status: ISCO1988-Based. Males *vs.* Females, Annual Discrete-Choice Data, 1991 - 2008.

	MALES			FEMALES		
	LSKEMP.	SKEMP.	NONEMP	LSKEMP.	SKEMP.	NONEMP
Variable	Mean (S.D)	Mean (S.D)	Mean (S.D)	Mean (S.D)	Mean (S.D)	Mean (S.D)
Regional Characteristics						
Urban	0.633	0.604	0.685	0.622	0.642	0.665
	(0.482)	(0.489)	(0.465)	(0.485)	(0.479)	(0.472)
Accessible	0.96	0.974	0.971	0.963	0.969	0.97
	(0.195)	(0.159)	(0.167)	(0.19)	(0.174)	(0.17)
University (in TTWA)	0.728	0.75	0.71	0.725	0.759	0.733
	(0.445)	(0.433)	(0.454)	(0.446)	(0.428)	(0.442)
Skill Intensity	-0.166	0.122	-0.269	-0.09	0.065	-0.167
	(0.9)	(1.0)	(1.0)	(1.0)	(1.0)	(1.0)
Local (TTWA) Business Cycle Effects						
TTWA Labour Market Tightness (V/U)	-0.091	-0.082	-0.197	-0.101	-0.12	-0.178
	(1.0)	(1.0)	(0.9)	(0.9)	(0.9)	(0.9)
TTWA Industry Skill Composition	-0.004	0.185	-0.043	0.046	0.213	0.01
	(1.0)	(1.0)	(1.0)	(1.0)	(1.0)	(1.0)

Skill groups are defined according the International Standard Classification of Occupations (ISCO-88). The ISCO-1988 defines skill-levels using *both task- and competency-based measures*: "Skill levels are linked to the length of time deemed necessary for a person to become fully competent in the performance of tasks associated with a job (Elias *et al.* 1999)". Occupations are classified into 4 skill groups, illustrated in Table 6.1, based on (1) the level of general education and (2) the level of job-specific training required to perform a job (Upward & Wright 2004). Groups 1-2 are classified as low-, whilst 3-4 as high-skilled. Thus the ISCO-88 based measure attempts to closely capture the *actual* skill *requirements* of a job.

Being white, married, being married with dependent children, and having an employed spouse all increase the probability of being in high skilled employment by more than alternative labour market states. The likelihood of non-employment is also sizeably lower for married men than women, regardless of whether they have dependent children in the household. However, health limitations and being disabled both increase the probability of non-employment by the most. Given the wide confidence intervals, these differences are not significant at conventional levels when looking at the unconditional means. It is thus of interest to determine, in addition to the other research questions under address, whether significant gender differences exist when conditional means are considered.

Table 6.5: LABOUR MARKET TRANSITIONS. CURRENT LABOUR MARKET STATUS: ISCO1988-BASED. MALES *vs.* FEMALES, ANNUAL DISCRETE-CHOICE DATA, 1991 - 2008.

	MALES			FEMALES		
	LSKEMP.	**SKEMP.**	**NONEMP**	**LSKEMP.**	**SKEMP.**	**NONEMP**
Variable	**Mean (S.D)**	**Mean (S.D)**	**Mean (S.D)**	**Mean (S.D)**	**Mean (S.D)**	**Mean (S.D)**
Previous Labour Market Status (T-1)						
LowSkilledEmp.xMatched	0.342	0.013	0.093	0.434	0.02	0.082
	(0.474)	(0.112)	(0.291)	(0.496)	(0.139)	(0.275)
Low Skilled Emp. x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	0.047	0.003	0.018	0.071	0.003	0.017
	(0.213)	(0.054)	(0.131)	(0.256)	(0.057)	(0.131)
xBusinessServicesxTIGHT(V/U)	0.078	0.005	0.017	0.132	0.007	0.025
	(0.268)	(0.069)	(0.129)	(0.338)	(0.081)	(0.155)
xPublicServicesxSLACK(V/U)	0.015	0.000	0.005	0.059	0.002	0.011
	(0.122)	(0.019)	(0.072)	(0.236)	(0.044)	(0.103)
xPublicServicesxTIGHT(V/U)	0.031	0.001	0.005	0.1 (0.3)	0.004	0.012
	(0.173)	(0.03)	(0.072)		(0.063)	(0.109)
X Firm Size: 50+	0.2 (0.4)	0.006	0.044	0.197	0.01	0.031
		(0.079)	(0.205)	(0.398)	(0.099)	(0.173)
X Part Time Contract	0.01	0	0.009	0.202	0.005	0.038
	(0.1)	(0.013)	(0.093)	(0.401)	(0.067)	(0.192)
LowSkilledEmp.xOverqualified	0.463	0.032	0.085	0.36	0.033	0.055
	(0.499)	(0.176)	(0.279)	(0.48)	(0.179)	(0.229)
Low Skilled Emp. x Overqualified x "Job Satisfaction"						
x"Genuine Overqual."	0.101	0.006	0.013	0.059	0.005	0.01
	(0.301)	(0.078)	(0.112)	(0.235)	(0.074)	(0.097)
x"Apparent Overqual."	0.362	0.026	0.072	0.302	0.028	0.046
	(0.481)	(0.159)	(0.259)	(0.459)	(0.164)	(0.209)
Low Skilled Emp. x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	0.03	0.005	0.012	0.028	0.003	0.007
	(0.169)	(0.071)	(0.108)	(0.164)	(0.058)	(0.081)
xBusinessServicesxTIGHT(V/U)	0.125	0.010	0.022	0.115	0.011	0.019
	(0.331)	(0.1)	(0.145)	(0.319)	(0.105)	(0.138)
xPublicServicesxSLACK(V/U)	0.023	0.002	0.005	0.037	0.003	0.005
	(0.15)	(0.041)	(0.072)	(0.188)	(0.055)	(0.074)
xPublicServicesxTIGHT(V/U)	0.07	0.004	0.01	0.124	0.01	0.013
	(0.255)	(0.063)	(0.1)	(0.329)	(0.099)	(0.112)
xFirm Size: 50+	0.294	0.019	0.048	0.165	0.017	0.022
	(0.456)	(0.137)	(0.214)	(0.372)	(0.128)	(0.146)
xPart Time Contract	0.023	0.002	0.009	0.145	0.007	0.025
	(0.149)	(0.039)	(0.093)	(0.352)	(0.082)	(0.157)
HighSkilledEmp.xOverqualified	0.007	0.215	0.017	0.005	0.178	0.008
	(0.081)	(0.411)	(0.128)	(0.074)	(0.382)	(0.087)
High Skilled Emp. x Overqualified x "Job Satisfaction"						
x"Genuine Overqual."	0.001	0.03	0.003	0.001	0.022	0.001
	(0.037)	(0.17)	(0.056)	(0.029)	(0.147)	(0.034)
x"Apparent Overqual."	0.005	0.185	0.014	0.005	0.155	0.006
	(0.072)	(0.389)	(0.116)	(0.068)	(0.362)	(0.08)
High Skilled Emp. x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	0.001	0.031	0.004	0.001	0.016	0.001
	(0.033)	(0.174)	(0.064)	(0.029)	(0.127)	(0.03)
xBusinessServicesxTIGHT(V/U)	0.002	0.093	0.005	0.002	0.056	0.003
	(0.044)	(0.29)	(0.071)	(0.046)	(0.229)	(0.053)
xPublicServicesxSLACK(V/U)	0.001	0.01	0.001	0	0.018	0.001
	(0.025)	(0.1)	(0.029)	(0.015)	(0.133)	(0.023)
xPublicServicesxTIGHT(V/U)	0.001	0.033	0.003	0.001	0.073	0.002
	(0.034)	(0.179)	(0.052)	(0.039)	(0.26)	(0.05)
xFirm Size: 50+	0.004	0.132	0.009	0.003	0.099	0.003
	(0.065)	(0.338)	(0.094)	(0.052)	(0.299)	(0.058)
xPart Time Contract	0	0.009	0.001	0.001	0.031	0.003
	(0.016)	(0.094)	(0.035)	(0.027)	(0.173)	(0.053)
NON-EMP.	0.176	0.115	0.752	0.188	0.123	0.821
	(0.381)	(0.319)	(0.432)	(0.391)	(0.328)	(0.383)

BusServ - Business Services; PubServ - Public Services.

Skill groups are defined according the International Standard Classification of Occupations (ISCO-88). The ISCO-1988 defines skill-levels using *both task- and competency-based measures*: "Skill levels are linked to the length of time deemed necessary for a person to become fully competent in the performance of tasks associated with a job (Elias *et al.* 1999)". Occupations are classified into 4 skill groups, illustrated in Table 6.1, based on (1) the level of general education and (2) the level of job-specific training required to perform a job (Upward & Wright 2004). Groups 1-2 are classified as low-, whilst 3-4 as high-skilled. Thus the ISCO-88 based measure attempts to closely capture the *actual* skill *requirements* of a job.

Outright home owners are more likely to be in low-skilled employment or non-employment at time t, whereas mortgaged home owners are more likely to be in high-skilled employment on average. Both council tenants are housing association renters are more likely to be in low-skilled employment or in non-employment at time t, whilst private renters are more likely to be in skilled employment or non-employment.

Living in an Urban Local Authority increases the probability of unemployment, whereas accessibility increases the probability of high-skilled employment or non-employment (Table 6.4). Living in a TTWA with at least one University increases the probability of high-skilled employment for both males and females. Higher Local Authority unemployment rates increase the probability of non-employment, whereas higher skill intensity and industrial skill composition levels increase the probability of high skilled employment. Local Business cycle effects appear to operate in the expected direction, higher levels of which increase the probability of high-skilled employment and decrease the probability of non-employment. However, females seem to be more responsive to the effect of local business cycles on average.

Table 6.6: DISCRETE-CHOICE SPECIFICATION: Labour Market Transitions, 1991-2008.

	MALES - PREVIOUS LABOUR MARKET STATUS					
	Low Skill Emp		High Skill Emp			
jbstat4	**Match**	**Ovqual**	**Match**	**Ovqual**	**Non-Emp**	**Total**
unskill emp	**5,056 (89.6%)**	**6,847 (90.0%)**	189 (2.6%)	98 (3.8%)	2,605 (34.8%)	14,795
skilled emp	142 (25.2%)	356 (4.7%)	**6,927 (94.0%)**	**2,385 (93.1%)**	1,272 (17.0%)	11,082
non-emp	446 (7.9%)	408 (5.4%)	255 (3.5%)	80 (3.1%)	**3,600 (48.1%)**	4,789
Total	5,644	7,611	7,371	2,563	7,477	**30,666**
	FEMALES - PREVIOUS LABOUR MARKET STATUS					
	Low Skill Emp		High Skill Emp			
jbstat4	**Match**	**Ovqual**	**Match**	**Ovqual**	**Non-Emp**	**Total**
unskill emp	**8,135 (87.9%)**	**6,743 (87.4%)**	225 (2.9%)	102 (4.8%)	3,522 (25.4%)	18,727
skilled emp	217 (2.3%)	365 (4.7%)	**7,120 (92.3%)**	**1,955 (91.4%)**	1,350 (9.7%)	11,007
non-emp	903 (9.8%)	607 (7.9%)	367 (4.8%)	83 (3.9%)	**9,002 (64.9%)**	10,962
Total	9,255	7,715	7,712	2,140	13,874	**40,696**

NB. Survey Date Reference Point. % totals may not add up to 100% due to rounding.

Transition matrices in Table 6.6 and 6.15, in the appendix to this chapter, demonstrate the persistent nature of the underlying data by gender[4]. These

[4]Skill groups are defined according the International Standard Classification of Occupations (ISCO-88). The ISCO-1988 defines skill-levels using *both task- and competency-based measures*: "Skill levels are linked to the length of time deemed necessary for a person to become fully competent in the performance of tasks associated with a job (Elias *et al.* 1999)".

results highlight greater persistence of skilled/less-skilled employment persistence for males than females, independent of over-qualification status. However, they also highlight greater skilled/less-skilled employment persistence for well matched females than those that were overqualified at t-1. There is evidence of greater non-employment for females than males. Detailed summary statistics suggest that females are, on average, less-likely to be in skilled or unskilled employment at time t than their male counterparts. They are also more likely to move into non-employment (Table 6.5).

6.5 Empirical Results

6.5.1 Skill Mismatch & the Persistence of Low Skilled Employment

The empirical analysis draws on interview date observations only, in order to investigate the probability of being in a less skilled job at time t conditional on whether one was in a mismatched job at time t-1. I condition the standard pooled Multinomial Logit estimates on a full set of Mundlak (1978) terms, in order to approximate a fixed effects specification (results excluding the Mundlak 1978 terms are presented in the Appendix, Table 6.19). Imbens & Wooldridge (2007) show that both in the pooled and the random effects cases, inclusion of Mundlak (1978) terms (time-averages of the time-varying covariates) in a panel regression leads to the fixed effects estimator in the linear panel case. When this is not the case, i.e. in a non-linear setting, heterogeneity bias is still minimised relative to a pooled MNL without these controls. This approach has been adopted in much of the empirical literature, e.g. Stewart (2007) who employs non-linear approaches, and extends this to the correlated random

Occupations are classified into 4 skill groups, illustrated in Table 6.1, based on (1) the level of general education and (2) the level of job-specific training required to perform a job (Upward & Wright 2004). Groups 1-2 are classified as low-, whilst 3-4 as high-skilled. Thus the ISCO-88 based measure attempts to closely capture the *actual* skill *requirements* of a job.

effects case. Computational constraints meant that the random effects/initial conditions corrections were not applied, a caveat to be taken into account when interpreting the results. However, since the model is well specified the Mundlak (1978) based strategy applied to a pooled MNL should help to minimise this bias.

Individual Characteristics

Relative to reference case, aged 18-29, the probability of low-skilled employment is significantly *lower* for prime age (30-45) females, 6.4%, and significantly *greater* for prime aged males, 5.2%. Skilled employment probability is 5.0% greater for prime aged males and 2.8% more likely for equivalently aged women than the reference group. For females, this also corresponds to the period that likelihood of non-employment is highest, 3.6% higher than the base, whereas for males this is 10.2% lower (the difference being significant at the 1% level). High-skilled employment probability is greatest for both male and female over 45s: 30.9% and 24.0% more likely than for under 30's, respectively. School type attended has a significant impact on labour market status. Whilst selective (non fee paying) Grammar, Private school *and* Technical college attendance significantly reduce probability of being in a low-skilled job for both males and females, only Grammar school and Technical college attendance increase the high-skilled employment transitions for males, relative to the baseline. For females, only Private school attendance significantly increases the likelihood of skilled work. Higher academic qualifications having effects in the expected direction, moreover, a stronger impact on female skilled- and non-employment transitions is evident. However, no significant impact on low-skilled employment probability is found for females. Vocational qualifications have an insignificant impact on labour market status once unobserved heterogeneity is controlled for using the current strategy.

Although race does not significantly predict low-skilled employment transitions, being white significantly increases (decreases) skilled (non-employment) probability by 2%(2.6%) and 1.5%(2.1%) for males and females respectively. However these difference are not statistically significant at conventional levels. The level terms of the interactions can be interpreted as holding the alternative indicator at its base level (e.g. Married/Cohabiting, Children and interaction). Marital status (married or cohabiting), the presence of dependent children in the household, and the interaction between the two, significantly impact on female labour market status only. Being married (without children) decreases low-skilled employment probability and increases non-employment transitions for females, with the gender differences in this effect being significant at the 5% level. For single females, having at least one dependent child in the household significantly decreases both skilled and less-skilled employment probability, significantly increasing non-employment probability by 10.1%. However, only the gender difference in the impact on low-skilled and non-employment transitions is significant. Moreover, females that are married or cohabiting with dependent children are 1.7% less likely to be in skilled jobs and more likely to be in less-skilled work (-2.3% net effect) and 5.5% more likely to be in non-employment.

Table 6.7: State Dependence. Average Marginal Effects (AME). Survey Date as Reference Point - POOLED MULTINOMIAL LOGIT MODEL + Mundlak (1978) Terms, 1991-2008. Time-Const Skill Mismatch Measure (ISCO2008 + Modal-based). TTWA Cluster Robust Standard Errors.

	Male Mundlakd_MNL			Female Mundlak_MNL		
	LSKEMP	SKEMP	NONEMP	LSKEMP	SKEMP	NONEMP
	[1]	[2]	[3]	[1]	[2]	[3]
Age (ref. < 30)						
30 - 45	0.052**	0.050***	-0.102***	-0.064***	0.028**	0.036*
	(0.026)†	(0.017)	(0.031)‡	(0.020)†	(0.013)	(0.020)‡
45 +	0.025	0.309***	-0.334***	0.095**	0.240***	-0.336***
	(0.041)	(0.036)	(0.020)	(0.038)	(0.035)	(0.017)
Type of school attended (ref. Comprehensive, Secondary Modern.)						
Grammar No Fee	-0.037***	0.012**	0.025***	-0.016*	0.005	0.011
	(0.011)	(0.006)	(0.009)	(0.008)	(0.005)	(0.008)
Private	-0.035***	0.005	0.030**	-0.039***	0.015**	0.024**
	(0.011)	(0.006)	(0.012)	(0.012)	(0.006)	(0.011)

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Table 6.7 – continued from previous page

	Male Mundlakd_MNL			Female Mundlak_MNL		
	LSKEMP [1]	SKEMP [2]	NONEMP [3]	LSKEMP [1]	SKEMP [2]	NONEMP [3]
Technical	-0.018**	0.014**	0.004	-0.016***	0.001	0.015**
	(0.008)	(0.006)	(0.009)	(0.006)	(0.006)	(0.008)
Highest Qualification (ref. Below O'Level,None, APPRENTICESHIP)						
Degree	0.024	0.100***	-0.123***	-0.005	0.177***	-0.171***
	(0.022)	(0.022)	(0.019)	(0.025)	(0.028)	(0.023)
Other Higher	-0.024	0.041***	-0.016	-0.023*	0.062***	-0.039***
	(0.015)	(0.014)	(0.018)	(0.013)	(0.011)	(0.016)
A Levels	-0.011	0.029**	-0.018	-0.009	0.046**	-0.037**
	(0.015)	(0.013)	(0.017)	(0.016)	(0.013)	(0.018)
O Levels	0.018	0.013	-0.030*	-0.004	0.031**	-0.027
	(0.016)	(0.014)	(0.018)	(0.014)	(0.013)	(0.017)
Vocational Qualifications.						
Yes	-0.000	-0.004	0.005	0.014	-0.001	-0.013
	(0.035)	(0.024)	(0.043)	(0.030)	(0.018)	(0.035)
Individual Characteristics.						
White	0.006	0.020*	-0.026***	0.006	0.015***	-0.021**
	(0.010)	(0.010)	(0.009)	(0.009)	(0.005)	(0.010)
Married/Cohabiting	-0.009	0.003	0.006	-0.053***	-0.002	0.055***
	(0.010)†	(0.010)	(0.008)†	(0.011)†	(0.008)	(0.010)†
Children	0.005	-0.029	0.024	-0.084***	-0.017***	0.101***
	(0.030)†	(0.027)	(0.025)†	(0.013)†	(0.008)	(0.014)†
ChildrenX Married/Cohab.	-0.010	0.029	-0.019	0.030***	-0.017**	-0.014
	(0.035)	(0.030)	(0.023)	(0.012)	(0.008)	(0.012)
Employed Spouse	0.033***	0.012*	-0.045***	0.052***	0.007	-0.059***
	(0.007)	(0.007)	(0.008)	(0.009)	(0.007)	(0.008)
Health Limits	-0.032***	-0.006	0.038***	-0.035***	-0.022***	0.056***
	(0.007)	(0.007)	(0.006)	(0.009)	(0.005)	(0.008)
Disabled	-0.031*	-0.020	0.051***	-0.018	-0.022*	0.040***
	(0.017)	(0.016)	(0.018)	(0.015)	(0.012)	(0.013)
Housing Tenure (ref. Private Renter)						
Owned Outright	-0.002	-0.014	0.015	0.016	-0.018*	0.002
	(0.011)	(0.010)	(0.012)	(0.011)	(0.009)	(0.012)
Mortgaged Owner	0.022**	0.002	-0.024***	0.028**	-0.012	-0.016
	(0.009)	(0.008)	(0.009)	(0.011)	(0.008)	(0.010)
Council Tenant	0.011	-0.006	-0.005	0.009	-0.018	0.008
	(0.012)	(0.011)	(0.010)	(0.013)	(0.011)	(0.013)
Housing Assocition	0.035**	-0.008	-0.027**	0.009	-0.002	-0.006
	(0.018)	(0.016)	(0.010)	(0.015)	(0.013)	(0.015)
Work Related Training in the last 12 months.						
Yes	0.038***	0.028***	-0.066***	0.066***	0.035***	-0.101***
	(0.006)†	(0.004)	(0.005)‡	(0.006)†	(0.003)	(0.006)‡
Experience.						
Potential Experience (5 years)	-0.004***	0.002	0.002*	-0.010***	0.001	0.009***
	(0.001)†	(0.001)	(0.001)	(0.002)†	(0.001)	(0.001)
X Age Group (ref. < 30)						
X 30 - 45	-0.011	-0.017***	0.028***	0.032***	-0.011**	-0.021***
	(0.009)†	(0.006)	(0.008)‡	(0.009)†	(0.006)	(0.008)‡
X 45 +	-0.023***	-0.049***	0.071***	-0.016*	-0.040***	0.056***
	(0.008)	(0.006)	(0.007)	(0.009)	(0.006)	(0.008)
Mundlak (1978) Terms						√
Year Dummies						√
Government Office Region Fixed Effects						√
N	30666	30666	30666	40696	40696	40696
LL	-12413.9	-12413.9	-12413.9	-18130.6	-18130.6	-18130.6
LL_int	-31219.4	-31219.4	-31219.4	-43306.7	-43306.7	-43306.7
Pseudo R^2	0.602	0.602	0.602	0.581	0.581	0.581
AIC	2.5e+04	2.5e+04	2.5e+04	3.7e+04	3.7e+04	3.7e+04

(*) dy/dx = Marginal Effect. (d) is for discrete change of dummy variable from 0 to 1.
(‡) McFadden's Pseudo R^2: 1 - LL(full)/LL(Intercept Only).
Tests for differences in means (independent samples): ‡_†_§- Difference between equivalent male and female coefficient statistically significant at the 1%_5%_10% level respectively.
Skill groups are defined according the International Standard Classification of Occupations (ISCO-88). The ISCO-1988 defines skill-levels using *both task- and competency-based measures*: "Skill levels are linked to the length of time deemed necessary for a person to become fully competent in the performance of tasks associated with a job (Elias *et al.* 1999)". Occupations are classified into 4 skill groups, illustrated in Table 6.1, based on (1) the level of general education and (2) the level of job-specific training required to perform a job (Upward & Wright 2004). Groups 1-2 are classified as low-, whilst 3-4 as high-skilled. Thus the ISCO-88 based measure attempts to closely capture the *actual* skill *requirements* of a job.
Previous Industry Groupings: Industrial *(Agriculture, Hunting, Forestry & Fishing; Mining & quarrying, Manufacturing, and electricity, gas and water supply; Construction)*; Commercial *(Wholesale & retail trade, repairs, etc.; Transport, Storage & Communications)*; Business Services *(Financial Intermediation; Real estate, renting and business activities)*; Public Services *(Public administration & defence, social security; Health & Social Work; Education; Other)*. NB. Unknown category includes cases assumed missing at random, as well as pre-2002 cases where a concordance between SIC80 and SIC92 could not be established. *5%* of SIC80 codes could not be converted to SIC92 classification. Including these cases in the base category did not change other estimates markedly.
Symmetry of Transitions (IIA): The Hausman test for IIA is not compatible with clustered data. Moreover, formal tests for IIA should be viewed with caution (Train 2009). Alternative modeling methods, that relax the IIA assumption, include the alternative-specific multinomial probit or nested logit models. These alternatives are not pursued in this study and left for future work.
* p<0.10, ** p<0.05, *** p<0.01 (NB. Cluster Robust Standard Errors)

Being married to, or cohabiting with, an employed spouse significantly increases employment probability (both skilled and less-skilled) for males, whilst increasing likelihood of less-skilled employment for females. In the case of both males and females, this scenario reduces the likelihood of non-employment. Both health limitations and disability reduce employment likelihood and increase non-employment, however in both cases skilled employment probability is not significantly impacted for males whilst female less-skilled employment transitions are unaffected by disability status.

Housing tenure has a differential impact for males and females, although these differences are not statistically significant. Female outright home owners are significantly less likely to enter skilled employment and more likely to move into less-skilled work, although this effect is insignificant at conventional levels. Both male and female mortgaged owners are significantly more likely to be take up less-skilled employment, with mortgaged males significantly less likely to move into non-employment than private renters. No significant gender variation in this effect is evident. This may be driven by financial constraints due to mortgaged home ownership increasing the likelihood of taking up "low quality" local employment to keep up mortgage payments, rather than searching wider for a better match (Coulson & Fisher 2009).

Evidence suggests a significantly larger impact on female than male labour market mobility of direct human capital investment in the past 12 months, through work-related and part-time training courses. This increases the probability of low-skilled employment by 3.8% and 6.6% for males and females respectively with the gender difference being significant at the 5% level. Direct human capital investment increases skilled employment probability for both males and females, by 2.8% and 3.5% respectively. However, non-employment probability is reduced by almost 1.5 times as much for females than for males (10.3% and 6.6% respectively) with this difference being significant at the 1%

level.

Potential experience has a larger average impact for females than males. Interacting potential experience with age highlights significant non-linearities in the impact of potential experience accumulation, *relative to under 30s.* For the prime-aged (30-45) category, 5 more years of potential experience has no impact on low-skilled employment probability for men. However, in the case of females low-skilled employment probability is increased by 3.2% more than the reference case for every 5 years gained with the difference being significant at the 5% level. Every 5 years of potential experience accumulated during prime age significantly reduces skilled employment transitions for both males and females, with these effects being indistinguishable at the 10% level. However, 30-45 year old men are 2.8% *more* likely to enter non-employment for every 5 years of potential experience accumulated. Significantly, with this difference being significant at the 1% level, this effect is 2.1% *lower* than the reference case of women for every 5 years of potential experience acquired between 30 and 45. In addition, potential experience accumulation significantly reduces employment probability and significantly increases the likelihood of non-employment for over 45s in general, with this effect being more pronounced (but not significant different) for males. Whilst potential experience can only proxy actual labour market experience, this suggests that whilst in general females are more likely to enter non-employment during prime-age (most likely for child bearing), experience accumulation in the work-force places a constraint on labour market mobility which reduces this incentive. A potential caveat is the fact that there is likely to be more noise in the potential experience indicator for females than males, as females are systematically more likely to take longer career breaks for child rearing purposes than their male counterparts. If years of schooling, including further education, were available then interacting this with actual experience may be a more insightful. The problem with the current measure is that education and experience are negatively correlated. Individuals with higher education levels will tend to have lower levels of actual on-the-job experience. This implies that they will also have lower levels of potential experience, however in general potential experience does not take into account periods of full-time education after entering the job market for the first time. Since this measure is constructed to take into account age left further or secondary education, which ever is greater, this issue should be minimised in most cases.

Time-Varying Regional Characteristics

In general, the significance of time-varying regional heterogeneity is fairly weak. Whilst urbanity does not significantly impact on transition probabilities, for males living in an accessible Local Authority significantly increases skilled employment probability by 5.0%[5]. Having at least one university in the local labour market (TTWA) significantly *decreases* the probability of less-skilled employment and *increases* the probability of skilled employment for females (3.4% and 2.1% respectively). As noted in Section 4.3, the presence of higher education institutions should improve employment prospects for the local population, given the support services needed to run such an institution as well as the influx of young consumers into the local market. However, as pointed out by Arntz & Wilke (2009), the increased availability of a young flexible minimum wage workforce may impact negatively on locals' labour market participation. This result suggest that this negative effect is not working against females' upward career mobility. Skill Intensity[6] is positively related to less-skilled, and negatively related to skilled employment transitions for males. A one stan-

[5] Since these variables are unlikely to have changed over time, this effect is identified off a regional move.

[6] This time-varying indicator is measured at the Local Authority level due to data limitations.

dard deviation increase in local authority Skill Intensity increases(decreases) male less-skilled (skilled) employment probability by 0.6%(0.5%). However, this effect is insignificant at conventional levels for females. In general, labour market transitions are non-responsive to contemporaneous local business cycle effects at conventional levels. Granted, a one standard deviation increase in TTWA labour market tightness decreases male non-employment probability by 0.6%. Local labour market industrial skill composition proxies the availability of skilled jobs in a region. Higher industrial skill composition significantly increases(decreases) male less-skilled employment(non-employment) probability, all else equal. A one standard deviation increase in industrial skill composition significantly increases(decreases) male less-skilled employment(non-employment) probability by 1.3%(2.1%). Out of the time-varying regional covariates, only the effect of industrial skill composition on non-employment probability is significantly different for males than females. For females, no significant effect of industrial skill composition is found.

Table 6.8: State Dependence. Average Marginal Effects (AME). Survey Date as Reference Point - POOLED MULTINOMIAL LOGIT MODEL + Mundlak (1978) Terms, 1991-2008. Time-Const Skill Mismatch Measure (ISCO2008 + Modal-based). TTWA Cluster Robust Standard Errors.

	Male Mundlakd_MNL			Female Mundlak_MNL		
	LSKEMP	SKEMP	NONEMP	LSKEMP	SKEMP	NONEMP
	[1]	[2]	[3]	[1]	[2]	[3]
Time-Varying Regional Characteristics.						
Urban	0.015	-0.002	-0.013	0.021	-0.000	-0.020
	(0.019)	(0.014)	(0.015)	(0.018)	(0.009)	(0.019)
Accessible	-0.014	0.050**	-0.036	0.055	0.019	-0.075
	(0.030)	(0.024)	(0.034)	(0.050)	(0.023)	(0.048)
University (in TTWA)	0.016	0.003	-0.019	-0.034	0.021*	0.013
	(0.018)	(0.015)	(0.020)	(0.021)	(0.012)	(0.020)
Standardised Variables (By month, across TTWA Regions.						
Skill Intensity	0.006	-0.005*	-0.001	-0.000	-0.000	0.001
	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)
Local (TTWA) Business Cycle Effects (Standardised).						
TTWA Labour Market Tightness (V/U)	0.006	0.000	-0.006*	-0.000	-0.000	0.000
	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.003)
TTWA Industrial Skill Composition	0.013**	0.008	-0.021***	0.001	-0.004	0.003
	(0.006)	(0.005)	(0.007)§	(0.007)	(0.005)	(0.006)§
Mundlak (1978) Terms						√
Year Dummies						√
Government Office Region Fixed Effects						√

Continued on next page

Table 6.8 – continued from previous page

	Male Mundlakd_MNL			Female Mundlak_MNL		
	LSKEMP [1]	SKEMP [2]	NONEMP [3]	LSKEMP [1]	SKEMP [2]	NONEMP [3]
N	30666	30666	30666	40696	40696	40696
LL	-12413.9	-12413.9	-12413.9	-18130.6	-18130.6	-18130.6
LL_int	-31219.4	-31219.4	-31219.4	-43306.7	-43306.7	-43306.7
Pseudo R^2	0.602	0.602	0.602	0.581	0.581	0.581
AIC	2.5e+04	2.5e+04	2.5e+04	3.7e+04	3.7e+04	3.7e+04

(*) dy/dx = Marginal Effect. (d) is for discrete change of dummy variable from 0 to 1.
(‡) McFadden's Pseudo R^2: 1 - LL(full)/LL(Intercept Only).
Tests for differences in means (independent samples): ‡_†_§- Difference between equivalent male and female coefficient statistically significant at the 1%_5%_10% level respectively.
Skill groups are defined according the International Standard Classification of Occupations (ISCO-88). The ISCO-1988 defines skill-levels using *both task- and competency-based measures*: "Skill levels are linked to the length of time deemed necessary for a person to become fully competent in the performance of tasks associated with a job (Elias *et al.* 1999)". Occupations are classified into 4 skill groups, illustrated in Table 6.1, based on (1) the level of general education and (2) the level of job-specific training required to perform a job (Upward & Wright 2004). Groups 1-2 are classified as low-, whilst 3-4 as high-skilled. Thus the ISCO-88 based measure attempts to closely capture the *actual* skill *requirements* of a job.
Previous Industry Groupings: Industrial *(Agriculture, Hunting, Forestry & Fishing; Mining & quarrying, Manufacturing, and electricity, gas and water supply; Construction)*; Commercial *(Wholesale & retail trade, repairs, etc.; Transport, Storage & Communications)*; Business Services *(Financial Intermediation; Real estate, renting and business activities)*; Public Services *(Public administration & defence, social security; Health & Social Work; Education; Other)*. NB. Unknown category includes cases assumed missing at random, as well as pre-2002 cases where a concordance between SIC80 and SIC92 could not be established. *5%* of SIC80 codes could not be converted to SIC92 classification. Including these cases in the base category did not change other estimates markedly.
Symmetry of Transitions (IIA): The Hausman test for IIA is not compatible with clustered data. Moreover, formal tests for IIA should be viewed with caution (Train 2009). Alternative modeling methods, that relax the IIA assumption, include the alternative-specific multinomial probit or nested logit models. These alternatives are not pursued in this study and left for future work.
* p<0.10, ** p<0.05, *** p<0.01 (NB. Cluster Robust Standard Errors)

Over-education

Consistent evidence is found to suggest that on average over-qualification, independent of whether experienced in skilled or low-skilled jobs, increases the probability of low-skilled and decreases likelihood of skilled employment relative to being in well-matched skilled work at t-1 (see coefficients in Tables 6.9, 6.13, and 6.14 where the average impact of over-qualification in employment at t-1 is gauged relative to the reference group of those well-matched, high skilled employment)[7].

Tables 6.9 suggests that males in well-matched low-skilled employment at t-1 are estimated to be 78.5% more likely to remain in low-skilled employment at t than the reference case, whereas for females this figure is lower at 68.5%. Well-matched males in low-skilled employment at t-1 are 76.9% less likely to be in skilled employment at time t than those in well-matched skilled jobs at

[7] As highlighted in previous sections, this is subject to the caveat that the average characteristics of 3-digit ISCO occupations are used to proxy job characteristics.

t-1, whilst for females this probability is 68.4% lower, *ceteris paribus* (both of these gender differences are significant at the 1% level). However, the impact on non-employment transitions is insignificant at conventional levels.

Over-qualified males and females in low-skilled employment at t-1 are on average 80.6% and 71.9% more likely to be in low-skilled employment at time t than the reference case respectively. Moreover, this gender difference is significant at the 1% level. Overqualified males in low-skilled employment at t-1 are, on average, 77.8% less likely to be in high-skilled employment than the reference case with this figure lower at 71.3% for females. Relative to the reference case, over-qualification in low-skilled employment at t-1 decreases the likelihood of non-employment by 2.8% more for males. However this effect is insignificant for females and the gender difference is insignificant at conventional levels.

On average, over-qualification in high-skilled employment seems to have a more detrimental effect for females than males. Overqualified females in high-skilled employment are on average 12.1% more likely to be in less-skilled employment at time t than those in well-matched high-skilled employment. This figure is lower at 5.9% for males. Over-qualification in skilled employment at t-1 implies a reduction in male skilled employment probability by 8.4%, relative to well-matched skilled employment. For females, this scenario carries an 12.2% lower probability of remaining in skilled-employment at time t. However, the gender differences in this effect is statistically insignificant. Relative to well-matched skilled jobs, over-qualification in skilled employment at t-1 has an insignificant impact on nonemployment probability for both males and females.

Gender differences in the impact of non-employment are statistically significant at the 1% level. Non-employment carries significantly larger negative consequences for less-skilled employment probability for males than females. Relative to well-matched high-skilled employment, non-employment at t-1 implies a 39.3% and 28.0% higher probability of future low-skilled employment for males and females respectively. However, relative to the reference category, non-employment at t-1 implies a 55.9% and 62.6% higher probability of high-skilled employment at time t for males and females respectively. Compared to well-matched high-skilled employment, non-employment at t-1 implies a 16.6% higher probability of future non-employment for males. However, this probability is significantly higher at 34.6% for females.

The results so far suggest that over-qualification in low-skilled employment has a greater negative effect on "upward" career mobility than over-qualification in skilled jobs on average. Moreover, this negative effect is much larger for males than for females. This average effect is not unexpected as the higher qualifications one has, the less occupations to upgrade into and the more likely downgrading becomes (Evans 1999). However, this cannot be extended to the observation of gender variation in this effect. Average estimates suggest that less-skilled employment is more of a stepping stone into high-skilled employment for females, independent of over-qualification. However, conditional on over-qualification, only women that are overqualified in low-skilled employment are *more* upwardly mobile. This is not the case for overqualified women in high-skilled jobs, who are less likely to remain in skilled employment and more likely to downgrade into less-skilled work than the reference group. Descriptives suggest that on average, non-employment transitions are less-likely for females than males in employment, however, non-employment persistence is much higher. Relative to the baseline, males in non-employment at t-1 are 16.6% more likely to remain in non-employment at time t. This figure is twice as large for females, who are 35.4% more likely to be in non-employment at time t than those in well-matched skilled jobs.

Table 6.9: State Dependence. Average Marginal Effects (AME). Survey Date as Reference Point - POOLED MULTINOMIAL LOGIT MODEL + Mundlak (1978) Terms, 1991-2008. Time-Const Skill Mismatch Measure (ISCO2008 + Modal-based). TTWA Cluster Robust Standard Errors.

	Male Mundlakd_MNL			Female Mundlak_MNL		
	LSKEMP	SKEMP	NONEMP	LSKEMP	SKEMP	NONEMP
	[1]	[2]	[3]	[1]	[2]	[3]
Previous Labour Market Status (T-1) (Ref. SKILLED EMP X MATCHED)						
LowSkilledEmp.xMatched	0.785***	-0.769***	-0.016	0.685***	-0.684***	-0.001
	(0.014)‡	(0.012)‡	(0.011)	(0.021)‡	(0.020)‡	(0.016)
x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	-0.047**	0.060**	-0.013	-0.003	-0.010	0.013
	(0.021)	(0.026)†	(0.012)	(0.015)	(0.012)†	(0.013)
xBusinessServicesxTIGHT(V/U)	-0.038**	0.075***	-0.037***	0.022	0.003	-0.025*
	(0.016)†	(0.017)‡	(0.009)	(0.014)†	(0.010)‡	(0.013)
xPublicServicesxSLACK(V/U)	-0.005	-0.026	0.031	0.028**	-0.031***	0.003
	(0.025)	(0.031)	(0.020)	(0.014)	(0.011)	(0.011)
xPublicServicesxTIGHT(V/U)	0.013	0.021	-0.034**	0.061***	-0.017	-0.044***
	(0.019)	(0.021)	(0.015)	(0.015)	(0.011)	(0.012)
XFirm Size: 50+	0.033***	-0.022**	-0.012	0.027***	0.004	-0.031***
	(0.010)	(0.009)	(0.008)	(0.009)	(0.008)	(0.009)
XPart Time Contract	-0.005	-0.055	0.060**	0.068***	-0.033***	-0.035***
	(0.029)†	(0.037)	(0.027)‡	(0.008)†	(0.008)	(0.008)‡
LowSkilledEmp.xOverqualified	0.806***	-0.778***	-0.028**	0.719***	-0.713***	-0.005
	(0.011)‡	(0.009)‡	(0.011)	(0.019)‡	(0.015)‡	(0.016)
Low Skilled Emp. x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	-0.065***	0.036***	0.030*	-0.019	0.001	0.019
	(0.016)	(0.012)	(0.019)	(0.018)	(0.012)	(0.020)
xBusinessServicesxTIGHT(V/U)	-0.010	0.019**	-0.009	0.024	0.003	-0.028**
	(0.009)	(0.009)	(0.009)	(0.015)	(0.010)	(0.014)
xPublicServicesxSLACK(V/U)	0.001	-0.014	0.013	0.042**	-0.011	-0.031*
	(0.017)	(0.015)	(0.019)	(0.019)	(0.014)	(0.019)
xPublicServicesxTIGHT(V/U)	0.028**	-0.011	-0.017	0.073***	-0.016*	-0.057***
	(0.013)§	(0.011)	(0.011)‡	(0.014)§	(0.009)	(0.012)‡
XFirm Size: 50+	0.016**	-0.006	-0.011	0.015*	0.003	-0.018*
	(0.008)	(0.007)	(0.009)	(0.009)	(0.006)	(0.010)
XPart Time Contract	0.011	0.007	-0.018	0.049***	-0.032***	-0.017*
	(0.016)	(0.013)§	(0.017)	(0.010)	(0.007)§	(0.010)
HighSkilledEmp.xOverqualified	0.059***	-0.084***	0.024	0.121***	-0.122**	0.001
	(0.020)	(0.026)	(0.022)	(0.044)	(0.050)	(0.034)
High Skilled Emp. x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	-0.037	0.004	0.034	-0.006	-0.003	0.009
	(0.029)	(0.016)	(0.027)	(0.058)	(0.017)	(0.059)
xBusinessServicesxTIGHT(V/U)	-0.045*	0.056***	-0.012	-0.012	0.020	-0.008
	(0.026)	(0.018)	(0.020)	(0.041)	(0.019)	(0.040)
xPublicServicesxSLACK(V/U)	0.040	-0.018	-0.022	-0.097	0.064*	0.033
	(0.044)	(0.020)	(0.040)	(0.072)	(0.036)	(0.060)
xPublicServicesxTIGHT(V/U)	-0.014	0.020	-0.006	-0.032	0.029	0.003
	(0.032)	(0.017)	(0.029)	(0.041)	(0.020)	(0.037)
XFirm Size: 50+	0.008	-0.004	-0.004	-0.005	0.001	0.004
	(0.022)	(0.012)	(0.017)	(0.029)	(0.011)	(0.027)
XPart Time Contract	-0.005	-0.003	0.008	-0.057*	0.018	0.039
	(0.052)	(0.027)	(0.043)	(0.034)	(0.014)	(0.032)
NON-EMP	0.393***	-0.559***	0.166***	0.280***	-0.626***	0.346***
	(0.011)‡	(0.012)‡	(0.011)‡	(0.009)‡	(0.011)‡	(0.011)‡
Mundlak (1978) Terms						$\surd$
Year Dummies						$\surd$
Government Office Region Fixed Effects						$\surd$
N	30666	30666	30666	40696	40696	40696
LL	-12413.9	-12413.9	-12413.9	-18130.6	-18130.6	-18130.6
LL_int	-31219.4	-31219.4	-31219.4	-43306.7	-43306.7	-43306.7
Pseudo R^2	0.602	0.602	0.602	0.581	0.581	0.581
AIC	2.5e+04	2.5e+04	2.5e+04	3.7e+04	3.7e+04	3.7e+04

(*) dy/dx = Marginal Effect. (d) is for discrete change of dummy variable from 0 to 1.
(‡) McFadden's Pseudo R^2: 1 - LL(full)/LL(Intercept Only).
Tests for differences in means (independent samples): ‡_†_§- Difference between equivalent male and female coefficient statistically significant at the 1%_5%_10% level respectively.
Skill groups are defined according the International Standard Classification of Occupations (ISCO-88). The ISCO-1988 defines skill-levels using *both task- and competency-based measures*: "Skill levels are linked to the length of time deemed necessary for a person to become fully competent in the performance of tasks associated with a job (Elias *et al.* 1999)". Occupations are classified into 4 skill groups, illustrated in Table 6.1, based on (1) the level of general education and (2) the level of job-specific training required to perform a job (Upward & Wright 2004). Groups 1-2 are classified as low-, whilst 3-4 as high-skilled. Thus the ISCO-88 based measure attempts to closely capture the *actual* skill *requirements* of a job.

Continued on next page

Table 6.9 – continued from previous page

	Male Mundlakd_MNL			Female Mundlak_MNL		
	LSKEMP	SKEMP	NONEMP	LSKEMP	SKEMP	NONEMP
	[1]	[2]	[3]	[1]	[2]	[3]

Previous Industry Groupings: Industrial *(Agriculture, Hunting, Forestry & Fishing; Mining & quarrying, Manufacturing, and electricity, gas and water supply; Construction)*; Commercial *(Wholesale & retail trade, repairs, etc.; Transport, Storage & Communications)*; Business Services *(Financial Intermediation; Real estate, renting and business activities)*; Public Services *(Public administration & defence, social security; Health & Social Work; Education; Other)*. NB. Unknown category includes cases assumed missing at random, as well as pre-2002 cases where a concordance between SIC80 and SIC92 could not be established. *5%* of SIC80 codes could not be converted to SIC92 classification. Including these cases in the base category did not change other estimates markedly.
Symmetry of Transitions (IIA): The Hausman test for IIA is not compatible with clustered data. Moreover, formal tests for IIA should be viewed with caution (Train 2009). Alternative modeling methods, that relax the IIA assumption, include the alternative-specific multinomial probit or nested logit models. These alternatives are not pursued in this study and left for future work.
* p<0.10, ** p<0.05, *** p<0.01 (NB. Cluster Robust Standard Errors)

Previous Firm Characteristics & Local Business Cycle Effects.

Low-Skilled Employment Probability Upward mobility is highest for males in well-matched low-skilled Business Service jobs: Relative to Industrial sector employment, this likelihood is 4.7% lower than the average 78.5% in slack and 3.8% *lower* for those in tight local labour markets at t-1. However, this effect is insignificant for well-matched males in low-skilled Public Service sector jobs.

Upward career mobility is *more limited* for well-matched females in low-skilled Public services. Compared to industrial sector employment, women working in well-matched less-skilled Public Services at t-1 are 2.8% more likely to remain in less-skilled work than the average 68.5% effect when the local labour market is slack and 6.1% higher when it is tight. Notably, the likelihood of low-skilled employment is 2.1% *higher* than the reference case for females in Business Service sector jobs when the local labour market is tight. This suggests gender differences differences in the nature of jobs traditionally undertaken in low-skilled sectors. Léné (2011) points to the ever increasing need for both education and experience to progress into skilled employment. If these jobs have increased in their skill requirements then, all else equal, the probability of upgrading into skilled employment is likely to have decreased. Females have been shown to have a higher propensity to engage in part-time work (Connolly

& Gregory 2008). Human capital accumulation is likely to be lower in part-time work thus further hampering career mobility.

Compared to Industrial sector employment, low-skilled employment probability is 6.5% *lower* than the average 80.6% effect for overqualified male Business Service sector workers in low-skilled employment when the labour market is slack, and 2.8% *higher* for Public Service employees when the labour market is tight. However, relative to the reference case, the probability of less-skilled employment is 2.4% *higher* for overqualified female Business Service sector employees when the local labour market is tight. Moreover, the likelihood of less-skilled employment is 4.2% higher for female Public Service sector workers in slack, and 7.3% higher in tight, local labour markets.

Over-qualification in high-skilled employment increases the average probability of low-skilled employment by 5.9% for males. On average, over-qualification in high-skilled employment seems to have a more detrimental effect for females than males. However, these differences are insignificant at conventional levels. Overqualified females in high-skilled employment are on average 12.1% more likely to be in less-skilled employment at time t than those in well-matched high-skilled jobs. Relative to Industrial sector employment, the probability of less-skilled employment is 4.5% lower for overqualified male Business Service sector employees when the local labour market is tight. However, no significant industry variation in the impact of over-qualification on low-skilled employment probability is found for females.

Working for a large employer (50+ employees) at t-1 significantly increases the average probability (78.5% and 68.5% respectively) of remaining in low-skilled employment for well-matched males and females (by 3.3% and 2.7% respectively). With respect to over-qualification in low-skilled employment, working for a large firm increases the average 80.6% (71.9%) probability of continued less-skilled employment by 1.6% (1.5%) for males (females). However, no significant impact of firm size is found for the overqualified in skilled jobs. Part-time work is an insignificant predictor of low-skilled employment for men, over and above the average effect of state dependence. However, part-time work significantly increases the probability of remaining in less-skilled employment for both well-matched and overqualified women by 6.8% and 4.9% more than the average effect respectively with the gender difference in the former effect being stastically significant at the 5% level. Whilst insignificant, overqualified females in skilled part-time employment at t-1 are estimated to be 5.7% *less* likely to be in less-skilled employment than those is well-matched skilled jobs at t-1.

Skilled Employment Probability Relative to the reference group, when the local labour market is slack (tight), well-matched males in low-skilled employment at t-1 are 6% (7.5%) *more* likely than the average -76.8% effect to be in skilled employment at time t. However, for equivalent females, Public sector employment at t-1 significantly reduces the probability of skilled employment at time t by 3.1% more than the average 68.4% reduction.

Overqualified males in low-skilled employment at t-1 are, on average, 77.8% *less* likely to be in high-skilled employment than the reference case. However, relative to Industrial sector work, Business Service sector employment *increases* high-skilled employment probability by 3.6% and 1.9% more than the average effect when the local labour market is slack and tight respectively. On average, overqualified females are 71.3% less likely than those in well-matched skilled jobs to be in skilled employment at time t. Moreover, when the local labour market is tight over-qualification in low-skilled Public Service sector employment reduces the average skilled employment probability by 1.6% relative to industrial sector employment.

Over-qualification in skilled employment at t-1 implies a reduction in male skilled employment probability by 8.4%, relative to well-matched skilled employment. However, when the local labour market is tight overqualified male Business Service sector employees increase their average skilled employment probability by 5.6%, relative to the baseline. For females, this scenario carries an 12.2% lower probability of remaining in skilled-employment at time t. Overqualified females in skilled Public Service employment are estimated to face a 6.4% higher probability of being in skilled employment when the local labour market is slack.

Relative to the baseline, working for a large employer (50+ employees) at t-1 significantly decreases the average -76.9% probability of skilled employment for well-matched males in low-skilled employment by 2.2%, whilst for females this has an insignificant effect over an above the -68.4% effect of state dependence. However, no significant impact of firm size is found for both overqualified males and females. For women in well-matched less-skilled jobs, part-time work implies a 3.3% reduction in skilled employment probability, over and above the average -68.4% effect. Overqualified females in low-skilled part-time employment at t-1 are estimated to be 3.2% less likely to be in skilled employment than the average 71.3% effect at time t. However, no significant effect of part-time work on skilled employment probability is found for males as well as for overqualified females in high skilled employment.

Non-Employment Probability Well-matched males in low-skilled employment at t-1 are 1.6% less likely to be in non-employment at time t than those in well-matched skilled jobs at t-1, however this average effect is insignificant at conventional levels. Relative to Industrial sector workers, this probability is 3.7% and 3.4% lower in tight local labour markets for Business Service and Public Service sector workers respectively. Although insignificant at the 90% level on average, females in well-matched low-skilled employment are 0.1% less likely than equivalent females in well-matched skilled jobs to be in non-employment at time t. Compared to Industrial sector workers, this probability is 2.5% and 4.4% lower hen the local labour market is tight for female Business Sector and Public Service workers respectively.

Over-qualification in low-skilled employment at t-1 significantly decreases the likelihood of non-employment by 2.8% more than the reference case for males. Little industry variation is found in this effect, however for overqualified males employed in the Business Services sector this probability is 3.0% *higher* than the reference case when the local labour market is slack. Overqualified females are 0.5% less likely than those in well-matched skilled jobs to be in non-employment at time t. This estimate is insignificant at conventional levels. However, when the local labour market is tight, relative to equivalent Industrial sector workers, overqualified females in low-skilled Business Service and Public Service sector jobs are 2.8% and 5.7% less likely than the average effect to be in non-employment at time t. Moreover, overqualified female Public Service sector workers are 3.1% less likely to enter non-employment in slack periods.

Relative to well-matched skilled jobs, over-qualification in skilled employment at t-1 implies an insignificant 2.4% *increase* in male skilled employment probability. Moreover, no significant industry variation is evident. For females, this scenario carries a 0.1% *lower* probability of remaining in skilled-employment at time t. However, this is effect is also insignificant. As in the case of males, industry variation is found to be insignificant at conventional levels.

Relative to the baseline, working for a large employer (50+ employees) at t-1 significantly decreases the average -76.9% probability of non-employment for well-matched females in low-skilled employment by 3.1%, whilst for equivalent males this effect is insignificant. No significant impact of firm size is found for overqualified males over an above the average effect of state dependence. However, for overqualified females in low-skilled employment, working for a large employer lowers non-employment probability by 1.8% (where the average effect is insignificant). Relative to well-matched skilled employment, part-time work *increases* the probability of non-employment by 6.0% for well-matched men in low-skilled employment at t-1. However for women, the same scenario *decreases* the probability of non-employment by 3.1%, *ceteris paribus*. This gender difference is significant at the 1% level. This suggests that, whilst upward career mobility may be on average more limited for females in part-time than full-time work, part-time work seems to be an indicator of loose labour market attachment for males on average. Furthermore, relative to the reference group, over-qualification reduces non-employment probability by 1.7% for females in low-skilled employment (where the average effect is insignificant). However, no significant impact of part-time work on the non-employment transitions of the overqualified in skilled work is found, over and above the impact on non-employment propensities of those in well-matched low-skilled employment at t-1.

6.6 Sensitivity Analysis

The results so far suggest that over-qualification, independent of whether experienced in skilled or low-skilled work, increases the probability of low-skilled and decreases the probability of skilled employment when compared to being in a well-matched skilled job, all else equal. Furthermore, over-qualification is more damaging for career mobility if experienced in low-skilled employment. Low-skilled employment is more of a Stepping Stone to skilled employment for females than males, independent on over-qualification. However, conditional on being overqualified, only women in low-skilled employment are more upwardly mobile than men.

Over-qualification in high-skilled employment carries greater negative career mobility implications for females than their male counterparts. Important variation is evident, both in terms of previous industry and firm characteristics. Moreover, the effect of being over-qualified is not invariant to the business cycle. However, the results so far could be sensitive to various definitional assumptions. The result that the overqualified in high-skilled employment have poorer upward career prospects than their well matched counterparts is unexpected. Taken in the context of existing work looking at the returns to over-education, this suggests that the negative implications of over-qualification could be further reaching. Sicherman (1991) finds that, all else constant, overeducated workers get higher wages than their coworkers but lower wages than workers with similar levels of schooling working in well-matched jobs. Moreover, Dolton & Silles (2008) find that UK graduates that were overeducated in their first jobs earn less that those which were not. Thus over-education is a potential signal of education quality. The potential sensitivity of empirical results to the definition of over-qualification has been highlighted in the literature. Moreover, it would be of interest to gauge whether results are sensitive to another key assumption: the definition of a skilled job.

6.6.1 Definition of a Skilled Job: ISCO-2008

I draw on the updated ISCO-2008 classification of occupations, in order to assess the importance of changes in skill requirements of the 20 year period 1988 - 2008. Significant technological advances and industrial compositional changes since the 1980's (e.g. De-industrialisation and the growing prominence of a two-tier service sector), accompanied by a general up-skilling of the workforce, suggests that the very nature of skill mismatch is likely to have changed accordingly. In extensions both the sensitivity of the Skill Mismatch definition *and* sensitivity of results to the definition of skilled job are assessed as both are ISCO2008 rather than ISCO1988 based in this section.

Table 6.10: ISCO1988 VS. ISCO2008, 1991-2008, Survey Date Reference Point.

MALES:			ISCO2008			
ISCO1988	**Unknown**	**Skill-level 1**	**Skill-level 2**	**Skill-level 3**	**Skill-level 4**	**Total**
Unknown, Invalid	5,341	0	0	0	0	5,341
Skill-level 1	0	1,697	255	0	0	1,952
Skill-level 2	0	218	12,047	70	22	12,357
Skill-level 3	0	0	2,405	2,969	157	5,531
Skill-level 4	0	0	0	439	8,446	8,885
Total	**5,341**	**1,915**	**14,707**	**3,478**	**8,625**	**34,066**
FEMALES:			ISCO2008			
ISCO1988	**Unknown**	**Skill-level 1**	**Skill-level 2**	**Skill-level 3**	**Skill-level 4**	**Total**
Unknown, Invalid	12,300	0	0	0	0	12,300
Skill-level 1	0	2,184	351	0	0	2,535
Skill-level 2	0	210	13,270	18	0	13,498
Skill-level 3	0	0	4,666	4,872	17	9,555
Skill-level 4	0	0	0	313	6,931	7,244
Total	**12,300**	**2,394**	**18,287**	**5,203**	**6,948**	**45,132**

NB. Survey Date Reference Point.

In addition to being a significant update of the ISCO88 classification, the new methodology attempts to address some underlying concerns relating to the previous approach. These include the definition of managers versus sole traders, the distinction between professional and associate professional occupations and the separation of supervisory occupations (Source: www.iser.essex.ac.uk). Table 6.10 show the concordance between the two approaches for the sample under consideration.

Table 6.11: Current Labour Market Status: By Gender, 1991 - 2008.

	Males		**Females**	
Job Status (§)	Freq.	Percent	Freq.	Percent
unskill emp	12,725	41.5	14,481	35.58
skilled emp	13,152	42.89	15,253	37.48
non-emp	4,789	15.62	10,962	26.94
Total	**30,666**	**100**	**40,696**	**100**

§ NB. Definition of a skilled job ISCO2008 based. Survey Date Reference Point.

Table 6.10 shows that most of the movement in terms of broad occupational skill has been upwards ("upskilling") as one would expect from the Technological Change literature. This change is most notable between the second and third skill levels. Moreover, this has impacted on females within the sample more than males (in both absolute and relative terms). Whereas 39% of employed individuals in the sample were in skilled, and 61% in unskilled employment according to the ISCO1988 methodology, this figure is roughly 50% in both cases under the ISCO2008 approach.

Table 6.12: DISCRETE-CHOICE SPECIFICATION: Labour Market Transitions, 1991-2008.

	MALES - PREVIOUS LABOUR MARKET STATUS					
	Low Skill Emp		High Skill Emp			
Job Status (ISCO2008§)	**Match**	**Ovqual**	**Match**	**Ovqual**	**Non-Emp**	**Total**
unskill emp	**4,235 (87.9%)**	**5,837 (88.8%)**	210 (2.6%)	144 (4.0%)	2,299 (30.7%)	12,725
skilled emp	186 (3.8%)	376 (5.7%)	**7,683 (93.8%)**	**3,329 (92.5%)**	1,578 (21.1%)	13,152
non-emp	399 (8.3%)	362 (5.5%)	302 (3.7%)	126 (3.5%)	**3,600 (48.1%)**	4,789
Total	4,820	6,575	8,195	3,599	7,477	**30,666**
	FEMALES - PREVIOUS LABOUR MARKET STATUS					
	Low Skill Emp		High Skill Emp			
Job Status (ISCO2008§)	**Match**	**Ovqual**	**Match**	**Ovqual**	**Non-Emp**	**Total**
unskill emp	**6,018 (85.8%)**	**5,201 (85.4%)**	254 (2.6%)	154 (4.1%)	2,854 (20.6%)	14,481
skilled emp	254 (3.6%)	382 (6.3%)	**9,171 (92.1%)**	**3,428 (91.1%)**	2,018 (14.5%)	15,253
non-emp	742 (10.6%)	510 (8.4%)	528 (5.3%)	180 (4.8%)	**9,002 (64.9%)**	10,962
Total	7,014	6,093	9,953	3,762	13,874	**40,696**

§ NB. Definition of a skilled job ISCO2008 based. Survey Date Reference Point.

Comparing table 6.11 and 6.3 in the Descriptives section, suggests that women are on average 10% more likely to be in skilled occupations under the updated ISCO-2008 methodology. Transition matrices are presented in Table 6.16. Table 6.13 suggests that, whilst on average upward career mobility for women has increased when the ISCO-88 and ISCO-08 methods are contrasted, this is not true for all job matches. The following section discusses the results in more detail. Similar, yet larger, difference in the two approaches are when the marginal effects are evaluated at their sample means (not reported). The AMEs are drawn on due to their closer approximation to Average Partial Effects (APEs), and for contrasting between the male and female sub-samples. See Bartus (2005) for an argument in favour of the AMEs over MEMs.

Table 6.13: State Dependence, Previous Industry Characteristics & Business Cycle Effects. Marginal Effects. Survey Date as Reference Point - POOLED MULTINOMIAL LOGIT MODEL + Mundlak (1978) Terms, 1991-2008. Skill Mismatch Measure (Modal-based). TTWA Cluster Robust Standard Errors.

	Males			Females		
	LSKEMP [1]	SKEMP [2]	NONEMP [3]	LSKEMP [1]	SKEMP [2]	NONEMP [3]
AMEs: ISCO-1988 based. - SPECIFICATION USED IN THE MAIN ANALYSIS.						
Previous Labour Market Status (T-1) (Ref. SKILLED EMP X MATCHED)						
LowSkilledEmp.xMatched	0.785***	-0.769***	-0.016	0.685***	-0.684***	-0.001
	(0.014)‡	(0.012)‡	(0.011)	(0.021)‡	(0.020)‡	(0.016)
x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	-0.047**	0.060**	-0.013	-0.003	-0.010	0.013
	(0.021)	(0.026)†	(0.012)	(0.015)	(0.012)†	(0.013)
xBusinessServicesxTIGHT(V/U)	-0.038**	0.075***	-0.037***	0.022	0.003	-0.025*
	(0.016)†	(0.017)‡	(0.009)	(0.014)†	(0.010)‡	(0.013)
xPublicServicesxSLACK(V/U)	-0.005	-0.026	0.031	0.028**	-0.031***	0.003
	(0.025)	(0.031)	(0.020)	(0.014)	(0.011)	(0.011)
xPublicServicesxTIGHT(V/U)	0.013	0.021	-0.034**	0.061***	-0.017	-0.044***
	(0.019)	(0.021)	(0.015)	(0.015)	(0.011)	(0.012)
XFirm Size: 50+	0.033***	-0.022**	-0.012	0.027***	0.004	-0.031***
	(0.010)	(0.009)	(0.008)	(0.009)	(0.008)	(0.009)
XPart Time Contract	-0.005	-0.055	0.060**	0.068***	-0.033***	-0.035***
	(0.029)†	(0.037)	(0.027)‡	(0.008)†	(0.008)	(0.008)‡
LowSkilledEmp.xOverqualified	0.806***	-0.778***	-0.028**	0.719***	-0.713***	-0.005
	(0.011)‡	(0.009)‡	(0.011)	(0.019)‡	(0.015)‡	(0.016)
Low Skilled Emp. x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	-0.065***	0.036***	0.030	-0.019	0.001	0.019
	(0.016)	(0.012)	(0.019)	(0.018)	(0.012)	(0.020)
xBusinessServicesxTIGHT(V/U)	-0.010	0.019**	-0.009	0.024	0.003	-0.028**
	(0.009)	(0.009)	(0.009)	(0.015)	(0.010)	(0.014)
xPublicServicesxSLACK(V/U)	0.001	-0.014	0.013	0.042**	-0.011	-0.031*
	(0.017)	(0.015)	(0.019)	(0.019)	(0.014)	(0.019)
xPublicServicesxTIGHT(V/U)	0.028**	-0.011	-0.017	0.073***	-0.016*	-0.057***
	(0.013)§	(0.011)	(0.011)‡	(0.014)§	(0.009)	(0.012)‡
XFirm Size: 50+	0.016**	-0.006	-0.011	0.015*	0.003	-0.018*
	(0.008)	(0.007)	(0.009)	(0.009)	(0.006)	(0.010)
XPart Time Contract	0.011	0.007	-0.018	0.049***	-0.032***	-0.017
	(0.016)	(0.013)§	(0.017)	(0.010)	(0.007)§	(0.010)
HighSkilledEmp.xOverqualified	0.059***	-0.084***	0.024	0.121***	-0.122**	0.001
	(0.020)	(0.026)	(0.022)	(0.044)	(0.050)	(0.034)
High Skilled Emp. x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	-0.037	0.004	0.034	-0.006	-0.003	0.009
	(0.029)	(0.016)	(0.027)	(0.058)	(0.017)	(0.059)
xBusinessServicesxTIGHT(V/U)	-0.045*	0.056***	-0.012	-0.012	0.020	-0.008
	(0.026)	(0.018)	(0.020)	(0.041)	(0.019)	(0.040)
xPublicServicesxSLACK(V/U)	0.040	-0.018	-0.022	-0.097	0.064*	0.033
	(0.044)	(0.020)	(0.040)	(0.072)	(0.036)	(0.060)
xPublicServicesxTIGHT(V/U)	-0.014	0.020	-0.006	-0.032	0.029	0.003
	(0.032)	(0.017)	(0.029)	(0.041)	(0.020)	(0.037)
XFirm Size: 50+	0.008	-0.004	-0.004	-0.005	0.001	0.004
	(0.022)	(0.012)	(0.017)	(0.029)	(0.011)	(0.027)
XPart Time Contract	-0.005	-0.003	0.008	-0.057*	0.018	0.039
	(0.052)	(0.027)	(0.043)	(0.034)	(0.014)	(0.032)
NON-EMP	0.393***	-0.559***	0.166***	0.280***	-0.626***	0.346***
	(0.011)‡	(0.012)‡	(0.011)‡	(0.009)‡	(0.011)‡	(0.011)‡
AMEs: ISCO-2008 based. - SPECIFICATION USED IN THE SENSITIVITY ANALYSIS.						
Previous Labour Market Status (T-1) (Ref. SKILLED EMP X MATCHED)						
LowSkilledEmp.xMatched	0.764***	-0.769***	0.005	0.657***	-0.703***	0.046***
	(0.014)‡	(0.012)‡	(0.010)	(0.021)‡	(0.022)‡	(0.016)
Low Skilled Emp. x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	-0.036**	0.046**	-0.010	0.024	-0.032*	0.008
	(0.015)	(0.021)	(0.012)	(0.017)	(0.017)	(0.017)
xBusinessServicesxTIGHT(V/U)	-0.023*	0.058***	-0.035***	0.039***	-0.010	-0.029**
	(0.013)	(0.013)†	(0.010)	(0.014)	(0.013)†	(0.015)
xPublicServicesxSLACK(V/U)	-0.008	-0.037	0.045*	0.061***	-0.066***	0.005
	(0.025)§	(0.032)	(0.024)	(0.016)§	(0.016)	(0.014)
xPublicServicesxTIGHT(V/U)	0.049**	-0.014	-0.036**	0.089**	-0.041***	-0.048***
	(0.020)	(0.022)	(0.017)	(0.016)	(0.015)	(0.014)
XFirm Size: 50+	0.024***	-0.007	-0.017**	0.012	0.023***	-0.035***
	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)	(0.009)
XPart Time Contract	0.021	-0.090*	0.069**	0.048***	-0.019**	-0.028***
	(0.032)	(0.048)	(0.031)†	(0.007)	(0.008)	(0.008)†
LowSkilledEmp.xOverqualified	0.795***	-0.780***	-0.015	0.710***	-0.767***	0.057**
	(0.012)‡	(0.010)‡	(0.011)†	(0.027)‡	(0.017)‡	(0.023)†
Low Skilled Emp. x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	-0.055***	0.021*	0.034*	-0.039*	0.036	0.003
	(0.017)	(0.012)	(0.018)	(0.022)	(0.024)	(0.031)

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Table 6.13 – continued from previous page

	Males			Females		
	LSKEMP [1]	SKEMP [2]	NONEMP [3]	LSKEMP [1]	SKEMP [2]	NONEMP [3]
xBusinessServicesxTIGHT(V/U)	-0.008	0.010	-0.002	0.021	0.014	-0.036*
	(0.009)	(0.009)	(0.009)	(0.019)	(0.017)	(0.020)
xPublicServicesxSLACK(V/U)	0.016	-0.050**	0.034	0.063*	-0.005	-0.058*
	(0.020)	(0.022)	(0.023)	(0.033)	(0.023)	(0.033)
xPublicServicesxTIGHT(V/U)	0.028**	-0.018	-0.010	0.067***	-0.010	-0.058***
	(0.012)	(0.011)	(0.011)	(0.021)	(0.019)	(0.021)
XFirm Size: 50+	0.011	-0.006	-0.005	0.016	0.017*	-0.033**
	(0.009)	(0.009)	(0.010)	(0.012)	(0.010)	(0.013)
XPart Time Contract	0.003	0.010	-0.014	0.047***	-0.016	-0.032**
	(0.014)§	(0.015)	(0.015)	(0.011)§	(0.011)	(0.013)
HighSkilledEmp.xOverqualified	0.052***	-0.070***	0.019	0.109***	-0.158***	0.049
	(0.015)	(0.019)	(0.015)	(0.034)	(0.041)	(0.035)
High Skilled Emp. x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	-0.063**	0.003	0.060**	0.010	-0.006	-0.004
	(0.028)	(0.020)	(0.026)	(0.045)	(0.018)	(0.045)
xBusinessServicesxTIGHT(V/U)	-0.067***	0.061***	0.006	-0.006	0.019	-0.013
	(0.024)	(0.018)	(0.017)	(0.032)	(0.020)	(0.035)
xPublicServicesxSLACK(V/U)	0.035	-0.009	-0.026	-0.064	0.018	0.046
	(0.041)	(0.023)	(0.036)	(0.043)	(0.032)	(0.048)
xPublicServicesxTIGHT(V/U)	-0.016	0.024	-0.008	-0.067*	0.063**	0.004
	(0.029)	(0.016)	(0.025)	(0.034)	(0.027)	(0.034)
XFirm Size: 50+	0.008	0.008	-0.016	0.009	0.002	-0.011
	(0.020)	(0.012)	(0.014)	(0.022)	(0.010)	(0.022)
XPart Time Contract	-0.012	0.043	-0.030	-0.024	0.032*	-0.009
	(0.052)	(0.048)	(0.047)	(0.034)	(0.017)	(0.025)
NON-EMP	0.354***	-0.538***	0.184***	0.240***	-0.618***	0.378***
	(0.011)‡	(0.011)‡	(0.011)‡	(0.007)‡	(0.011)‡	(0.011)‡
N	30666	30666	30666	40696	40696	40696

The Marginal Effect is for discrete change of dummy variable from 0 to 1. In the categorical variable case, this is measured relative to the baseline, holding all other options at zero.
Tests for differences in means (independent samples): ‡_†_§- Difference between equivalent male and female coefficient statistically significant at the 1%_5%_10% level respectively.
Skill groups are defined according the International Standard Classification of Occupations (ISCO-1988/ISCO-2008). The ISCO defines skill-levels using *both task- and competency-based measures*: "Skill levels are linked to the length of time deemed necessary for a person to become fully competent in the performance of tasks associated with a job (Elias *et al.* 1999)". Occupations are classified into 4 skill groups, illustrated in Table 6.1, based on (1) the level of general education and (2) the level of job-specific training required to perform a job (Upward & Wright 2004). Groups 1-2 are classified as low-, whilst 3-4 as high-skilled. Thus the ISCO-88 based measure attempts to closely capture the *actual* skill *requirements* of a job.
Previous Industry Groupings: Industrial *(Agriculture, Hunting, Forestry & Fishing; Mining & quarrying, Manufacturing, and electricity, gas and water supply; Construction)*; Commercial *(Wholesale & retail trade, repairs, etc.; Transport, Storage & Communications)*; Business Services *(Financial Intermediation; Real estate, renting and business activities)*; Public Services *(Public administration & defence, social security; Health & Social Work; Education; Other)*. NB. Unknown category includes cases assumed missing at random, as well as pre-2002 cases where a concordance between SIC80 and SIC92 could not be established. *5%* of SIC80 codes could not be converted to SIC92 classification. Including these cases in the base category did not change other estimates markedly.
Independent Variables: Model includes controls for individual characteristics, housing tenure, educational attainment, school type attended, work-related training, experience (plus interaction with age), time-varying regional characteristics, and Mundlak (1978) terms to control for unobserved heterogeneity.
Symmetry of Transitions (IIA): The Hausman test for IIA is not compatible with clustered data. Moreover, formal tests for IIA should be viewed with caution (Train 2009). Alternative modeling methods, that relax the IIA assumption, include the alternative-specific multinomial probit or nested logit models. These alternatives are not pursued in this study and left for future work.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$ (NB. Cluster Robust Standard Errors)

ISCO-2008 & Over-education

The general story, with respect to the average effect of over-education, remains consistent with that in the main analysis (see Table 6.13). Over-qualification in low-skilled employment has more of a negative effect on upward career mobility than if experienced in high-skilled employment. Furthermore, this negative effect is larger for males than for females. However, whilst over-qualification was estimated to be worse for overqualified females in high-skilled employment, this impact is estimated to be close to that of males when the ISCO-2008 based methodology is used. The persistence of non-employment is estimated to be stronger for both males and females, relative to the reference case. Significant changes in the skill-composition of jobs over the 1988 to 2008 are likely to have increased the career mobility costs associated with over-education. The ISCO-2008 based results suggest that over-qualification increases the probability of low-skilled employment and reduces the likelihood of skilled work by *more* than that suggested in Table 6.9. For males the ISCO-2008 update leads us to the conclusion that over the 20 year period between 1988 and 2008, industry variation in the impact of over-qualification on career mobility has *decreased* for males working in the Business Services sector. With respect to females, the equivalent impact *increased* for Public Service sector workers. This is true in both tight and slack labour markets. However, this is also true for the well-matched which suggests that career mobility may have increased (decreased) between 1988 and 2008 for females (males) in general.

Estimates in Table 6.13 suggest that firm size has decreased in importance for males labour market transitions, possibly due to the increased prevalence of small and medium size enterprises in the UK Business Service sector as defined herein. However, females in well matched low-skilled employment, working for larger employers increases the probability of skilled employment by more as a result of the job definition change. ISCO-2008 based estimates further imply that firm size has decreased in importance as a predictor of career mobility for overqualified women in low-skilled jobs. Whilst part-time work has negative implications for career mobility, see Connolly & Gregory (2008) for the case of female part-time work, the move to ISCO-2008 based estimates suggests that under the updated skilled job definition female part-time workers in low-skilled jobs actually fared slightly better in terms of upward career mobility, independent of over-qualification status. However, for males in well-matched low-skilled jobs, part-time work has a more detrimental effect on the probability of skilled employment under the updated skilled job definition, whilst significantly increasing non-employment transitions.

6.6.2 State Dependence & The Definition of Over-education

The bulk of the literature focussing on the average impact of over-education incidence (state dependence) on the persistence of over-education addresses the sensitivity of results to definitional assumptions, without any attempt to address heterogeneity in the type of job held. Whilst a direct measure of over-education may come closer to this, it still assumes homogeneity in the impact of over-qualification in high- and low-skilled jobs on future career mobility. In this study I do not make this assumption, and in this section I assess sensitivity to definitional choices whilst relaxing this restriction. In order to test the sensitivity of results to the skill mismatch definition, I redefine this using the main competing objective measure of mismatch commonly pursued in the literature. An individual is considered mismatched if their highest formal qualifications are greater than *one standard deviation more than the mean* qualification in their 3-digit occupational category. This produces estimates, see Table 6.14, very similar to those estimated in the main analysis (see appendix for full results). It seems that the commonly mentioned sensitivity to skill mismatch definition is not as big an issue when heterogeneity in job skill is taken into account. However, this ignores job quality differences that may still exist even within occupation groups which a direct measure of skill mismatch is more likely to capture (Chevalier 2003).

I also assess **heterogeneity within the group of overqualified** in an attempt identify *quality of a job match.* I divide the overeducated into "apparently" and "genuinely" overeducated sub-groups based on whether respondents were satisfied or dissatisfied with their actual job itself respectively. This approach follows the methodology of Chevalier (2003) and Chevalier & Lindley

(2009), a study based on an Institute of Employment Research (Warwick University) postal survey. The survey used in the two studies asked interviewees directly whether they considered their qualifications to be ideal or inappropriate for their job. Thus this allows for a time-varying measure of over-qualification, which will capture any added qualification which may improve a match over time. Since the job satisfaction question in the BHPS does not directly ask about appropriateness of qualifications for the current job, I use self-reported satisfaction the actual job itself at the beginning of a job spell to reflect satisfaction with a job match, rather than using a time-varying measure which is more likely to be affected by other non-job match related factors like how well they get on with coworkers. I also do not use an average job satisfaction measure, as this again is affected by unrelated job security and pay-related factors. During economic downturns, when jobs are scarce, an individual may report that she is satisfied with a job match just because they are happy to get a job rather than because of their satisfaction with the job itself. However, I would argue that this issue is likely to be minimised since this study controls for quarterly local business cycle effects, as well as yearly time dummies to capture longer-term evolution of the overall economy.

Table 6.14: State Dependence. Marginal Effects. Survey Date as Reference Point - POOLED MULTINOMIAL LOGIT MODEL + Mundlak (1978) Terms, 1991-2008. Robustness to Skill Mismatch Measure (Modal vs. 1 Std. Deviation > Mean). TTWA Cluster Robust Standard Errors.

	Males			Females		
	LSKEMP [1]	SKEMP [2]	NONEMP [3]	LSKEMP [1]	SKEMP [2]	NONEMP [3]
ISCO-1988 based.						
AME - Skill Mismatch: > Mode.						
Labour Market Status T-1 (Ref. SKILLED EMP X MATCHED)						
LSKEMPxMATCH	0.787***	-0.754***	-0.032***	0.771***	-0.731***	-0.041***
	(0.011)	(0.010)	(0.008)	(0.010)	(0.010)	(0.009)
LSKEMPxOVQUAL	0.814***	-0.776***	-0.039***	0.790***	-0.749***	-0.041***
	(0.007)†	(0.007)§	(0.008)	(0.007)†	(0.009)§	(0.009)
HSKEMPxOVQUAL	0.036***	-0.047***	0.011	0.052***	-0.044***	-0.008
	(0.008)	(0.010)	(0.010)	(0.010)	(0.015)	(0.014)
NON-EMP	0.396***	-0.567***	0.171***	0.256***	-0.640***	0.384***
	(0.009)‡	(0.011)‡	(0.010)‡	(0.007)‡	(0.010)‡	(0.010)‡

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Table 6.14 – continued from previous page

	Males			Females		
	LSKEMP	**SKEMP**	**NONEMP**	**LSKEMP**	**SKEMP**	**NONEMP**
	[1]	**[2]**	**[3]**	**[1]**	**[2]**	**[3]**
AME - Skill Mismatch: > Mode + Job Match Satisfaction.						
Labour Market Status T-1 (Ref. SKILLED EMP X MATCHED)						
LSKEMPxMATCH	0.785***	-0.753***	-0.032***	0.770***	-0.728***	-0.042***
	(0.011)	(0.010)	(0.008)	(0.010)	(0.010)	(0.009)
LSKEMPxOVQUAL						
X“Genuinely Overeducation”	0.833***	-0.780***	-0.053***	0.784***	-0.747***	-0.037***
	(0.013)§	(0.009)§	(0.010)	(0.014)§	(0.010)§	(0.014)
X“Apparently Overeducation”	0.808***	-0.773***	-0.034***	0.791***	-0.748***	-0.043***
	(0.008)	(0.008)	(0.009)	(0.007)	(0.009)	(0.009)
HSKEMPxOVQUAL						
X“Genuinely Overeducation”	0.069***	-0.110***	0.041	0.059***	-0.052*	-0.007
	(0.022)	(0.030)	(0.029)	(0.027)	(0.028)	(0.026)
X“Apparently Overeducation”	0.031***	-0.038***	0.007	0.052***	-0.043***	-0.009
	(0.009)	(0.011)	(0.010)	(0.011)	(0.016)	(0.016)
NON-EMP	0.399***	-0.566***	0.168***	0.256***	-0.637***	0.381***
	(0.009)‡	(0.011)‡	(0.010)‡	(0.007)‡	(0.010)‡	(0.010)‡
AME - Skill Mismatch: 1 Std. Deviation > Mean.						
Labour Market Status T-1 (Ref. SKILLED EMP X MATCHED)						
LSKEMPxMATCH	0.787***	-0.755***	-0.032***	0.763***	-0.725***	-0.038***
	(0.009)	(0.009)	(0.008)	(0.009)	(0.010)	(0.008)
LSKEMPxOVQUAL	0.814***	-0.772***	-0.041***	0.793***	-0.754***	-0.040***
	(0.008)	(0.007)	(0.008)	(0.007)	(0.009)	(0.009)
HSKEMPxOVQUAL	0.064***	-0.080***	0.016	0.096***	-0.121***	0.024
	(0.013)	(0.016)	(0.015)	(0.016)	(0.019)	(0.016)
NON-EMP	0.396***	-0.563***	0.167***	0.253***	-0.638***	0.385***
	(0.009)‡	(0.012)‡	(0.010)‡	(0.007)‡	(0.010)‡	(0.010)‡
ISCO-2008 based.						
AME - Skill Mismatch: > Mode.						
Labour Market Status T-1 (Ref. SKILLED EMP X MATCHED)						
LSKEMPxMATCH	0.778***	-0.770***	-0.008	0.771***	-0.765***	-0.005
	(0.011)	(0.009)	(0.007)	(0.011)	(0.008)	(0.009)
LSKEMPxOVQUAL	0.807***	-0.791***	-0.015**	0.788***	-0.779***	-0.009
	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.009)
HSKEMPxOVQUAL	0.029***	-0.032***	0.003	0.032***	-0.039***	0.008
	(0.005)	(0.009)	(0.008)	(0.006)	(0.011)	(0.010)
NON-EMP	0.361***	-0.551***	0.190***	0.216***	-0.630***	0.414***
	(0.009)‡	(0.010)‡	(0.010)‡	(0.006)‡	(0.010)‡	(0.009)‡
AME - Skill Mismatch: > Mode + Job Match Satisfaction.						
Labour Market Status T-1 (Ref. SKILLED EMP X MATCHED)						
LSKEMPxMATCH	0.778***	-0.770***	-0.008	0.771***	-0.765***	-0.005
	(0.011)	(0.009)	(0.007)	(0.011)	(0.008)	(0.009)
LSKEMPxOVQUAL						
X“Genuinely Overeducation”	0.828***	-0.793***	-0.035**	0.772***	-0.772***	-0.001
	(0.015)§	(0.010)	(0.010)	(0.017)§	(0.010)	(0.016)
X“Apparently Overeducation”	0.801***	-0.791***	-0.011	0.791***	-0.780***	-0.010
	(0.008)	(0.007)	(0.008)	(0.007)	(0.007)	(0.009)
HSKEMPxOVQUAL						
X“Genuinely Overeducation”	0.048**	-0.063***	0.015	0.055***	-0.058***	0.004
	(0.012)	(0.019)	(0.016)	(0.016)	(0.021)	(0.022)
X“Apparently Overeducation”	0.024**	-0.024**	-0.000	0.027**	-0.036***	0.009
	(0.005)	(0.010)	(0.010)	(0.006)	(0.013)	(0.010)
NON-EMP	0.361***	-0.551***	0.190***	0.216***	-0.630***	0.414***
	(0.009)‡	(0.010)‡	(0.010)‡	(0.006)‡	(0.010)‡	(0.009)‡
AME - Skill Mismatch: 1 Std. Deviation > Mean.						
Labour Market Status T-1 (Ref. SKILLED EMP X MATCHED)						
LSKEMPxMATCH	0.776***	-0.770***	-0.006	0.761***	-0.756***	-0.005
	(0.009)	(0.008)	(0.007)	(0.009)	(0.008)	(0.008)
LSKEMPxOVQUAL	0.811***	-0.793***	-0.019***	0.794***	-0.786***	-0.008
	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)	(0.009)
HSKEMPxOVQUAL	0.041***	-0.052***	0.010	0.053***	-0.076***	0.023**
	(0.008)	(0.013)	(0.011)	(0.009)	(0.013)	(0.010)
NON-EMP	0.357***	-0.549***	0.191***	0.213***	-0.628***	0.414***
	(0.009)‡	(0.011)‡	(0.010)‡	(0.006)‡	(0.009)‡	(0.009)‡

The Marginal Effect is for discrete change of dummy variable from 0 to 1. In the categorical variable case, this is measured relative to the baseline, holding all other options at zero.
Tests for differences in means (independent samples): ‡_†_§- Difference between equivalent male and female coefficient statistically significant at the 1%_5%_10% level respectively.
Skill groups are defined according the International Standard Classification of Occupations (ISCO-1988/ISCO-2008). The ISCO defines skill-levels using *both task- and competency-based measures*: "Skill levels are linked to the length of time deemed necessary for a person to become fully competent in the performance of tasks associated with a job (Elias *et al.* 1999)". Occupations are classified into 4 skill groups, illustrated in Table 6.1, based on (1) the level of general education and (2) the level of job-specific training required to perform a job (Upward & Wright 2004). Groups 1-2 are classified as low-, whilst 3-4 as high-skilled. Thus the ISCO-88 based measure attempts to closely capture the *actual* skill *requirements* of a job.
Previous Industry Groupings: Industrial *(Agriculture, Hunting, Forestry & Fishing; Mining & quarrying, Manufacturing, and electricity, gas and water supply; Construction)*; Commercial *(Wholesale & retail trade, repairs, etc.; Transport, Storage & Communications)*; Business Services *(Financial Intermediation; Real estate, renting and business activities)*; Public Services *(Public administration & defence, social security; Health & Social Work; Education; Other)*. NB. Unknown category includes cases assumed missing at random, as well as pre-2002 cases where a concordance between SIC80 and SIC92 could not be established. *5%* of SIC80 codes could not be converted to SIC92 classification. Including these cases in the base category did not change other estimates markedly.
Independent Variables: Model includes controls for individual characteristics, housing tenure, educational attainment, school type attended, work-related training, experience (plus interaction with age), time-varying regional characteristics, and Mundlak (1978) terms to control for unobserved heterogeneity.

Continued on next page

Table 6.14 – continued from previous page

	Males			Females		
	LSKEMP	SKEMP	NONEMP	LSKEMP	SKEMP	NONEMP
	[1]	[2]	[3]	[1]	[2]	[3]

Symmetry of Transitions (IIA): The Hausman test for IIA is not compatible with clustered data. Moreover, formal tests for IIA should be viewed with caution (Train 2009). Alternative modeling methods, that relax the IIA assumption, include the alternative-specific multinomial probit or nested logit models. These alternatives are not pursued in this study and left for future work.
* p<0.10, ** p<0.05, *** p<0.01 (NB. Cluster Robust Standard Errors)

On average, Table 6.14 highlights strong robustness of estimates across specifications. This contrasts with results in Groot & Maassen van den Brink (2000) which suggests that whilst the effect of over-qualification on earnings seems invariant to definitional choice, choice of how to classify someone as overeducated has a large impact on incidence of over-qualification. Consistent with Chevalier (2003), important differences in job quality exist within occupational groups when a direct measure of satisfaction with a job match is employed. Being overqualified carries more damaging implications for upward career mobility for "genuinely overqualified" men. This result carries over to the case of females in high skilled employment. However, "genuinely overqualified" females are more upwardly mobile than those that are "apparently overqualified" in both the ISCO1988 and ISCO2008 cases. This result, and that for the *apparently overqualified*, is still consistent with the main story.

6.7 Summary and Conclusions

Over-qualification, independent of whether experienced in skilled or low-skilled work, increases the probability of low-skilled and decreases the probability of skilled employment when compared to being in a well-matched skilled job, all else equal. Furthermore, over-qualification is more damaging for career mobility if experienced in low-skilled employment. Low-skilled employment is more of a Stepping Stone to skilled employment for females than males, independent on over-qualification. However, conditional on being overqualified, only women in low-skilled employment are more upwardly mobile than men. Over-qualification in high-skilled employment carries greater negative career mobility implications for females than their male counterparts. Important variation is evident, both in terms of previous industry and firm characteristics. Whilst upward career mobility may be on average more limited for females in part-time than full-time work, part-time work seems to be an indicator of loose labour market attachment for males on average. Moreover, the effect of being over-qualified is not invariant to the business cycle. Contrasting results from a 1988 and 2008 based classification of occupational skill, estimates suggest that upward career mobility may have increased (decreased) between 1988 and 2008 for overqualified females (males) in general. Moreover, with regards to state-dependence, this story is robust to the definition of over-qualification. Whilst desirable, a decomposition by previous industry and firm characteristics based on the Chevalier (2003) methodology was impossible to due small cell size constraints.

Whilst the results are compelling, it is important to determine whether the Independence of Irrelevant Alternatives assumption, analogous to the proportionality assumption in the duration model context, is not driving the results as this assumption is likely to not be appropriate in the setting considered. Given the dynamic nature of the models presented in Evans (1999) and Léné (2011), it is difficult to argue that the choice of less-skilled employment reduces both the probabilities of skilled and non-employment proportionally. For skilled individuals downgrading into less-skilled employment, this temporary situation may act as a Stepping Stone to future skilled employment during skilled job shortages/economic contractions thus increasing, and not decreasing, skilled employment probability. For others downgrading, this may turn out to be a permanent solution. The more skilled workers downgrade into less-skilled occupations the more likely that less-skilled individuals in less-skilled employment will be squeezed into non-employment. Moreover, the traditional route into skilled employment for the less-skilled is via experience accumulation in less-skilled positions (although Léné (2011) provides evidence to suggest that this route is becoming less likely). Since labour market states are likely to be correlated over time periods greater than t and t-1, this would violate the conditional independence (across time periods) assumption of the pooled MNL. Techniques can be exploited to control for the endogeneity of Initial Conditions, and thus fully capture serial correlation over time when incorporated with Mundlak (1978) terms (Wooldridge 2005)[8]. However, alternative labour market states would still be assumed independent. Haan & Uhlendorff (2006) propose a method of jointly modelling labour market states using a bivariate normal distribution (correlated random effects) which breaks the IIA/proportionality restriction of the standard MNL allowing for more flexible characterisations of state dependence (see Section 2 for more information). Since the booster samples, which over-sample low socioeconomic group status individuals, are included in the analysis, it could be argued that this exacerbates the Initial Conditions problem. The biggest obstacle to applying these methods is time, as they can be computationally intensive especially when solving higher dimensional integrals given current computing limitations (Train 2009). Steps are being made to address these issues, which will be incorporated into a discussion paper version of this chapter. However, it would be impractical to estimate all versions of the model given that this process can take 3/4 weeks using the current computing setup.

Ideally a more appropriate definition of Skill Mismatch could be employed, in order to better capture *direct* job skill requirements. There is a wide gap in

[8]Given the discrete choice setting, the inclusion of Mundlak (1978) terms on their own without controlling for endogeneity of initial conditions is likely to raise identification issues, given the endogeneity of Initial Conditions problem, an subject for investigation in a discussion paper version of this chapter. Moreover, identification considerations would be aided by an explicit formulation of the decision making process under consideration.

the skill mismatch on the investigation of the impact of measurement error on coefficient estimates. A limited number of papers in the Over-education literature have explicitly addressed this issue, e.g. Chevalier, 2003. The main focus of this literature has been on the identification of "required skills" using both subjective and objective measures for contrast. Subjective measures have been championed as giving direct evidence of skill requirements, however they are almost exclusively from the employees viewpoint and thus likely to suffer from substantial measurement error. Whilst the subjective approaches are more likely to capture the time-varying nature of this notion, almost exclusively objective studies have used time-invariant measures to classify occupations. One notable criticism of the subjective approach is the nature of individuals references. See for example, Bago d'Uva *et al.* (2009) for an exposition of the vignettes approach to identification in survey data which attempts to control the comparison groups which survey respondents use when answering subjective questions. If individuals are comparing the requirements when they were hired to current requirements, then this will capture the fact that "upgrading of skill requirements" due to technological change has resulted in occupations that were once less-skilled becoming more skilled, thus raising current requirements. However, it will not capture accumulation of on-the-job training which contributes to skill formation. If individuals are comparing their education levels at the time of hire to requirements at that time, then this will essentially be time-invariant. Subjective methods aside, I am unaware of any studies using time-varying objective measures of skill mismatch. The current study uses a time-invariant classification based the mid-point of the observation period, however the educational composition of the labour force has changed markedly between 1991 and 2008. I have experimented with this in a shorter panel, and intend to incorporate this into a working paper version of this chapter. Chapter 7, interesting follow on questions, highlights other considerations if policy recommendations are to be drawn from this work.

Appendix

6.7.1 Transition Matrices

Definition of a Skilled Job: ISCO-1988 + Over-qualification: $\geq$ 3-digit occupation's modal qualification level.

Table 6.15: DISCRETE-CHOICE SPECIFICATION: Labour Market Transitions, 1991-2008.

	MALES - PREVIOUS LABOUR MARKET STATUS					
	Low Skill Emp		High Skill Emp			
jbstat4	**Match**	**Ovqual**	**Match**	**Ovqual**	**Non-Emp**	**Total**
unskill emp	5,056	6,847	189	98	2,605	14,795
skilled emp	142	356	6,927	2,385	1,272	11,082
non-emp	446	408	255	80	3,600	4,789
Total	5,644	7,611	7,371	2,563	7,477	30,666
	FEMALES - PREVIOUS LABOUR MARKET STATUS					
	Low Skill Emp		High Skill Emp			
jbstat4	**Match**	**Ovqual**	**Match**	**Ovqual**	**Non-Emp**	**Total**
unskill emp	8,135	6,743	225	102	3,522	18,727
skilled emp	217	365	7,120	1,955	1,350	11,007
non-emp	903	607	367	83	9,002	10,962
Total	9,255	7,715	7,712	2,140	13,874	40,696

NB. Survey Date Reference Point.
Over-qualification: $\geq$ 3-digit occupation's modal qualification level.

Definition of a Skilled Job: ISCO-2008 + Over-qualification: $\geq$ 3-digit occupation's modal qualification level.

Table 6.16: DISCRETE-CHOICE SPECIFICATION: Labour Market Transitions, 1991-2008.

	MALES - PREVIOUS LABOUR MARKET STATUS					
	Low Skill Emp		High Skill Emp			
jbstat4	**Match**	**Ovqual**	**Match**	**Ovqual**	**Non-Emp**	**Total**
unskill emp	4,235	5,837	210	144	2,299	12,725
skilled emp	186	376	7,683	3,329	1,578	13,152
non-emp	399	362	302	126	3,600	4,789
Total	4,820	6,575	8,195	3,599	7,477	30,666
	FEMALES - PREVIOUS LABOUR MARKET STATUS					
	Low Skill Emp		High Skill Emp			
jbstat4	**Match**	**Ovqual**	**Match**	**Ovqual**	**Non-Emp**	**Total**
unskill emp	6,018	5,201	254	154	2,854	14,481
skilled emp	254	382	9,171	3,428	2,018	15,253
non-emp	742	510	528	180	9,002	10,962
Total	7,014	6,093	9,953	3,762	13,874	40,696

NB. Survey Date Reference Point.
Over-qualification: $\geq$ 3-digit occupation's modal qualification level.

Table 6.17: MALE DISCRETE-CHOICE SPECIFICATION: Labour Market Transitions, 1991-2008, Survey Date Reference Point.

	MALES - PREVIOUS LABOUR MARKET STATUS					
	Low Skill Emp		High Skill Emp			
jbstat4	**Match**	**Ovqual**	**Match**	**Ovqual**	**Non-Emp**	**Total**
unskill emp	7,110	4,793	229	58	2,605	14,795
skilled emp	210	288	7,657	1,655	1,272	11,082
non-emp	580	274	290	45	3,600	4,789
Total	7,900	5,355	8,176	1,758	7,477	30,666
	FEMALES - PREVIOUS LABOUR MARKET STATUS					
	Low Skill Emp		High Skill Emp			
jbstat4	**Match**	**Ovqual**	**Match**	**Ovqual**	**Non-Emp**	**Total**
unskill emp	10,318	4,560	259	68	3,522	18,727
skilled emp	310	272	7,706	1,369	1,350	11,007
non-emp	1,102	408	392	58	9,002	10,962
Total	11,730	5,240	8,357	1,495	13,874	40,696

Over-qualification: 1 Standard Deviation $\geq$ 3-digit occupation's mean qualification level.

Table 6.18: MALE DISCRETE-CHOICE SPECIFICATION: Labour Market Transitions, 1991-2008, Survey Date Reference Point.

	MALES - PREVIOUS LABOUR MARKET STATUS					
	Low Skill Emp		High Skill Emp			
jbstat4	**Match**	**Ovqual**	**Match**	**Ovqual**	**Non-Emp**	**Total**
unskill emp	7,110	4,793	229	58	2,605	14,795
skilled emp	210	288	7,657	1,655	1,272	11,082
non-emp	580	274	290	45	3,600	4,789
Total	7,900	5,355	8,176	1,758	7,477	30,666
	FEMALES - PREVIOUS LABOUR MARKET STATUS					
	Low Skill Emp		High Skill Emp			
jbstat4	**Match**	**Ovqual**	**Match**	**Ovqual**	**Non-Emp**	**Total**
unskill emp	7,681	3,538	303	105	2,854	14,481
skilled emp	366	270	10,243	2,356	2,018	15,253
non-emp	909	343	585	123	9,002	10,962
Total	8,956	4,151	11,131	2,584	13,874	40,696

Over-qualification: 1 Standard Deviation $\geq$ 3-digit occupation's mean qualification level.

Definition of a Skilled Job: ISCO-1988 + Over-qualification: 1 Standard Deviation $\geq$ 3-digit occupation's mean qualification level.

Definition of a Skilled Job: ISCO-2008 + Over-qualification: 1 Standard Deviation $\geq$ 3-digit occupation's mean qualification level.

6.7.2 Average Marginal Effects (AMEs): Multinomial Logit, no Mundlak (1978) terms.

Table 6.19: State Dependence. Average Marginal Effects (AME). Survey Date as Reference Point - POOLED MULTINOMIAL LOGIT MODEL **NO Mundlak (1978) Terms**, 1991-2008. Time-Const Skill Mismatch Measure (ISCO1988 + Modal-based). TTWA Cluster Robust Standard Errors.

	Male Mundlakd_MNL			Female Mundlak_MNL		
	LSKEMP	SKEMP	NONEMP	LSKEMP	SKEMP	NONEMP
	[1]	[2]	[3]	[1]	[2]	[3]

Continued on next page

Table 6.19 – continued from previous page

	Male Mundlakd_MNL			Female Mundlak_MNL		
	LSKEMP [1]	SKEMP [2]	NONEMP [3]	LSKEMP [1]	SKEMP [2]	NONEMP [3]
Age (ref. < 25)						
30 - 45	0.024	0.044**	-0.069***	-0.035*	0.023**	0.012
	(0.018)	(0.012)	(0.022)	(0.018)	(0.009)	(0.020)
45 +	0.043	0.130***	-0.173***	0.092***	0.112***	-0.204***
	(0.029)	(0.026)	(0.020)	(0.024)	(0.023)	(0.019)
Type of school attended (ref. Comprehensive, Secondary Modern.)						
Grammar No Fee	-0.042***	0.014**	0.028***	-0.022***	0.004	0.017**
	(0.011)	(0.006)	(0.009)	(0.008)	(0.004)	(0.008)
Private	-0.040***	0.008	0.032***	-0.046***	0.014**	0.032***
	(0.012)	(0.006)	(0.013)	(0.013)	(0.006)	(0.012)
Technical	-0.021**	0.013*	0.009	-0.017**	0.003	0.014*
	(0.010)	(0.007)	(0.012)	(0.006)	(0.006)	(0.008)
Highest Qualification (ref. Below O'Level,None, APPRENTICESHIP)						
Degree	-0.135***	0.155***	-0.020**	-0.115***	0.143***	-0.028***
	(0.013)	(0.008)	(0.010)	(0.010)	(0.009)	(0.010)
Other Higher	-0.048***	0.076***	-0.029***	-0.051***	0.072***	-0.021**
	(0.009)	(0.006)	(0.008)	(0.008)	(0.006)	(0.009)
A Levels	-0.041***	0.058***	-0.017**	-0.024***	0.037***	-0.013
	(0.008)	(0.006)	(0.008)	(0.008)	(0.007)	(0.009)
O Levels	-0.004	0.029***	-0.025***	0.006	0.022***	-0.029***
	(0.008)	(0.006)	(0.008)	(0.007)	(0.005)	(0.007)
Vocational Qualifications.						
Yes	0.009*	-0.002	-0.007	0.010**	0.009***	-0.019***
	(0.005)	(0.004)	(0.005)	(0.005)	(0.003)	(0.005)
Individual Characteristics.						
White	0.008	0.019*	-0.027***	0.014*	0.017***	-0.031***
	(0.009)	(0.010)	(0.010)	(0.008)	(0.005)	(0.010)
Married/Cohabiting	-0.012*	0.021***	-0.008	-0.051***	0.002	0.049***
	(0.007)	(0.006)	(0.007)	(0.008)	(0.005)	(0.008)
Children	-0.050**	0.010	0.041	-0.099***	-0.021***	0.120***
	(0.022)	(0.026)	(0.028)	(0.009)	(0.006)	(0.010)
ChildrenX Married/Cohab.	0.040*	-0.014	-0.026	0.015	-0.019***	0.004
	(0.023)	(0.024)	(0.024)	(0.010)	(0.006)	(0.010)
Employed Spouse	0.044***	0.017***	-0.061***	0.067***	0.006	-0.073***
	(0.006)	(0.005)	(0.006)	(0.008)	(0.005)	(0.009)
Health Limits	-0.088***	-0.039***	0.127***	-0.082***	-0.033***	0.115***
	(0.009)	(0.006)	(0.009)	(0.007)	(0.005)	(0.007)
Disabled	-0.050***	-0.041***	0.091***	-0.081***	-0.034**	0.115***
	(0.018)	(0.013)	(0.018)	(0.018)	(0.016)	(0.016)
Housing Tenure (ref. Private Renter)						
Owned Outright	0.023**	-0.006	-0.017*	-0.000	-0.014*	0.014
	(0.009)	(0.005)	(0.009)	(0.008)	(0.008)	(0.010)
Mortgage	0.040***	0.006	-0.046***	0.031***	0.010	-0.041***
	(0.007)	(0.007)	(0.008)	(0.007)	(0.006)	(0.008)
Council	-0.008	-0.062***	0.071***	-0.001	-0.035***	0.036***
	(0.010)	(0.010)	(0.012)	(0.008)	(0.007)	(0.011)
Housing Assoc.	0.015	-0.049***	0.034***	-0.003	-0.024**	0.027**
	(0.012)	(0.012)	(0.011)	(0.011)	(0.010)	(0.012)
Work Related Training in the last 12 months.						
Yes	0.049***	0.045***	-0.094***	0.084***	0.060***	-0.144***
	(0.005)	(0.003)	(0.005)	(0.005)	(0.003)	(0.006)
Experience.						
Pot. Experience	0.002**	-0.000	-0.002**	0.001	-0.000	-0.000
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)
X 30 - 45	-0.009**	-0.003	0.013**	0.006	-0.001	-0.005
	(0.005)	(0.003)	(0.005)	(0.005)	(0.002)	(0.005)
X 45 +	-0.019***	-0.014***	0.033***	-0.024***	-0.014***	0.038***
	(0.005)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)
Regional Characteristics.						
Urban	-0.006	-0.007	0.013**	-0.004	0.001	0.003
	(0.005)	(0.004)	(0.006)	(0.005)	(0.003)	(0.006)
Accessible	-0.020	0.010	0.010	-0.014**	0.000	0.014**
	(0.013)	(0.009)	(0.014)	(0.007)	(0.006)	(0.007)
University (in TTWA)	0.014**	-0.002	-0.011*	-0.003	-0.001	0.004
	(0.006)	(0.004)	(0.006)	(0.006)	(0.005)	(0.007)
Skill Intensity	-0.000	0.003	-0.003	-0.002	-0.001	0.003
	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Local Business Cycle Effects.						
Labour Market Tightness (V/U)	0.007***	0.001	-0.008***	0.005*	-0.000	-0.004*
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Industrial Skill Composition	0.001	0.003	-0.004	0.003	0.005**	-0.008***
	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Previous Labour Market Status (T-1)						
LowSkilledEmp.xMatched	0.796***	-0.791***	-0.005	0.700***	-0.720***	0.020
	(0.013)	(0.011)	(0.010)	(0.021)	(0.019)	(0.016)
x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	-0.048**	0.062**	-0.014	0.000	-0.006	0.006
	(0.022)	(0.027)	(0.011)	(0.016)	(0.013)	(0.013)
xBusinessServicesxTIGHT(V/U)	-0.038**	0.078***	-0.040***	0.024*	0.005	-0.029**
	(0.016)	(0.017)	(0.010)	(0.014)	(0.010)	(0.014)
xPublicServicesxSLACK(V/U)	0.001	-0.020	0.019	0.040***	-0.021*	-0.018
	(0.026)	(0.032)	(0.020)	(0.015)	(0.012)	(0.012)
xPublicServicesxTIGHT(V/U)	0.017	0.021	-0.038**	0.069***	-0.012	-0.057***
	(0.019)	(0.021)	(0.016)	(0.016)	(0.012)	(0.013)
XFirm Size: 50+	0.036***	-0.020**	-0.016**	0.026***	0.007	-0.033***
	(0.010)	(0.009)	(0.008)	(0.009)	(0.008)	(0.010)
XPart Time Contract	-0.013	-0.060	0.073***	0.067***	-0.036***	-0.032***

Continued on next page

Table 6.19 – continued from previous page

	Male Mundlakd_MNL			Female Mundlak_MNL		
	LSKEMP	SKEMP	NONEMP	LSKEMP	SKEMP	NONEMP
	[1]	[2]	[3]	[1]	[2]	[3]
	(0.028)	(0.039)	(0.028)	(0.008)	(0.009)	(0.009)
LowSkilledEmp.xOverqualified	0.822***	-0.799***	-0.023***	0.737***	-0.746***	0.008
	(0.010)	(0.008)	(0.009)	(0.018)	(0.015)	(0.016)
Low Skilled Emp. x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	-0.072***	0.034***	0.038*	-0.021	0.001	0.020
	(0.017)	(0.012)	(0.020)	(0.019)	(0.012)	(0.023)
xBusinessServicesxTIGHT(V/U)	-0.015	0.021**	-0.007	0.027*	0.004	-0.031**
	(0.009)	(0.009)	(0.009)	(0.015)	(0.010)	(0.014)
xPublicServicesxSLACK(V/U)	-0.005	-0.010	0.015	0.043**	-0.008	-0.036*
	(0.018)	(0.015)	(0.020)	(0.020)	(0.014)	(0.021)
xPublicServicesxTIGHT(V/U)	0.026**	-0.006	-0.020*	0.078***	-0.013	-0.065***
	(0.013)	(0.011)	(0.012)	(0.014)	(0.009)	(0.012)
XFirm Size: 50+	0.014*	-0.006	-0.008	0.016*	0.004	-0.020**
	(0.008)	(0.007)	(0.009)	(0.009)	(0.007)	(0.010)
XPart Time Contract	0.004	0.000	-0.004	0.044***	-0.037***	-0.007
	(0.017)	(0.014)	(0.019)	(0.010)	(0.007)	(0.011)
HighSkilledEmp.xOverqualified	0.058***	-0.082***	0.024	0.129***	-0.143***	0.014
	(0.020)	(0.026)	(0.023)	(0.044)	(0.052)	(0.033)
High Skilled Emp. x Previous Industry (ref. Commercial/Industrial) x Local (TTWA) Business Cycle Effects						
xBusinessServicesxSLACK(V/U)	-0.041	-0.000	0.041	-0.007	0.000	0.007
	(0.031)	(0.016)	(0.029)	(0.060)	(0.018)	(0.061)
xBusinessServicesxTIGHT(V/U)	-0.044	0.056***	-0.012	-0.013	0.021	-0.008
	(0.027)	(0.019)	(0.022)	(0.040)	(0.020)	(0.038)
xPublicServicesxSLACK(V/U)	0.048	-0.016	-0.032	-0.097	0.076*	0.021
	(0.046)	(0.020)	(0.041)	(0.075)	(0.040)	(0.067)
xPublicServicesxTIGHT(V/U)	-0.017	0.019	-0.001	-0.029	0.037	-0.008
	(0.034)	(0.017)	(0.034)	(0.040)	(0.023)	(0.036)
XFirm Size: 50+	0.009	-0.002	-0.007	-0.001	0.006	-0.005
	(0.023)	(0.012)	(0.018)	(0.029)	(0.011)	(0.027)
XPart Time Contract	-0.003	-0.005	0.008	-0.075**	0.007	0.068**
	(0.054)	(0.027)	(0.046)	(0.033)	(0.013)	(0.032)
NON-EMP	0.385***	-0.588***	0.202***	0.274***	-0.666***	0.391***
	(0.012)	(0.011)	(0.012)	(0.009)	(0.011)	(0.011)
Mundlak (1978) Terms						✓
Year Dummies						✓
Government Office Region Fixed Effects						✓
N	30666	30666	30666	40696	40696	40696
LL	-12172.5	-12172.5	-12172.5	-18674.2	-18674.2	-18674.2
LL_int	-30955.5	-30955.5	-30955.5	-43306.7	-43306.7	-43306.7
Pseudo R^2	0.607	0.607	0.607	0.569	0.569	0.569
AIC	2.5e+04	2.5e+04	2.5e+04	3.8e+04	3.8e+04	3.8e+04

(*) dy/dx = Marginal Effect. (d) is for discrete change of dummy variable from 0 to 1.
(‡) McFadden's Pseudo R^2: 1 - LL(full)/LL(Intercept Only).
Skill groups are defined according the International Standard Classification of Occupations (ISCO-2008). The ISCO defines skill-levels using *both task- and competency-based measures*: "Skill levels are linked to the length of time deemed necessary for a person to become fully competent in the performance of tasks associated with a job (Elias *et al.* 1999)". Occupations are classified into 4 skill groups, illustrated in Table 6.1, based on (1) the level of general education and (2) the level of job-specific training required to perform a job (Upward & Wright 2004). Groups 1-2 are classified as low-, whilst 3-4 as high-skilled. Thus the ISCO-88 based measure attempts to closely capture the *actual* skill *requirements* of a job.
Previous Industry Groupings: Industrial *(Agriculture, Hunting, Forestry & Fishing; Mining & quarrying, Manufacturing, and electricity, gas and water supply; Construction)*; Commercial *(Wholesale & retail trade, repairs, etc.; Transport, Storage & Communications)*; Business Services *(Financial Intermediation; Real estate, renting and business activities)*; Public Services *(Public administration & defence, social security; Health & Social Work; Education; Other)*. NB. Unknown category includes cases assumed missing at random, as well as pre-2002 cases where a concordance between SIC80 and SIC92 could not be established. *5%* of SIC80 codes could not be converted to SIC92 classification. Including these cases in the base category did not change other estimates markedly.
Symmetry of Transitions (IIA): The Hausman test for IIA is not compatible with clustered data. Moreover, formal tests for IIA should be viewed with caution (Train 2009). Alternative modeling methods, that relax the IIA assumption, include the alternative-specific multinomial probit or nested logit models. These alternatives are not pursued in this study and left for future work.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$ (NB. Cluster Robust Standard Errors)

Chapter 7

Conclusion

This thesis makes a number of contributions to the literature, raising some interesting questions and possibilities for future research. Given the current economic climate, as a package it is both timely and relevant in terms of renewed academic interest in unemployment and its consequences, notably from the US, and from a political point of view.

Thesis contributions Limited evidence on the importance of regional context over time implies that gauging the relative importance of the individual versus the regional level is crucial in order to gain insights into the effectiveness of government policy. The empirical results in Chapter 4 suggest that, whilst regional labour market conditions and therefore regional (fiscal) policies can affect individual labour market outcomes, expansionary regional fiscal policy (job creation) seem more to have a supportive role and they cannot substitute for a lack of individual-level qualities in the job search process.

Furthermore the evidence suggests that regional development programmes, targeting job creation, cannot substitute for targeted individual-level Active Labour Market Policies (ALMP) aimed to improve individuals' re-employment prospects in the long-run (≥ 6 months), e.g. through sponsored work experience and vocational qualifications. Moreover, since the increasing risk labour market detachment implies that re-employment prospects decrease with unemployment duration, only ALMPs seem to be effective in targeting the prospects of the long-term unemployed.

This is a new insight, that could not be reached using standard techniques. Conventional wisdom suggests that regional development policies encouraging job creation can alleviate the problem of regional unemployment. However, these results imply that this is only part of the solution, and will only help those with the highest re-employment probabilities. Given that in the current context ALMP like New Deal only kick in after 6 months of a claimant spell, these results point to the potential benefits of early intervention to improve individuals' re-employment prospects.

UK studies, Brown & Sessions (1997), Kalwij (2001), Collier (2005) and Kalwij (2010) all find that regional variation in job offer arrival rates is the main driver of average unemployment experiences. However, Chapter 4 finds that regional characteristics are less important than individual effects, and that regional heterogeneity is insignificant in the long-run. Kalwij (2004) finds less of a role for regional heterogeneity. Incidence, rather than duration of unemployment is found to be most important for young male career outcomes. Since the standard duration approaches, including those adopted in Kalwij (2004), average out the time-varying effect of covariates on the dependent variable, our results suggest more of a role for regional heterogeneity at least in the short-run. Although we were unable to test this, and it would be of interest to explore further in future work, Kalwij (2004) suggests that regional heterogeneity may be more important for unemployment incidence for young males.

Surprisingly, we observe that large cities such as London and Birmingham provide worse local labour market conditions than rural and even remote regions such as Northern Scotland. This finding is important as many people likely believe the reverse, although the Government is already targeting problematic neighborhoods in these cities.

A unique feature of this analysis is the use of rich individual-level claimant count data which has not been fully explored so far. Moreover, we exploit the regional variation in the data in order to link individuals to the regional context in which they reside. We exploit flexible Censored Quantile Regression techniques, as well as drawing on Quantile Decomposition techniques in order to isolate the "true"[1] regional effect (Machado *et al.* 2006).

Another contribution of this thesis is to address important sources of (regional) variation in Wage Scarring. Seminal UK research by Arulampalam (2001) concludes that the first spell of non-employment carries the highest penalty. Considering unemployment and inactivity separately, no reduction in the penalty associated with incidence of inactivity is found whilst for multiple spells of unemployment the wage penalty reduces significantly with incidence with the first spell carrying the highest penalty. Moreover, whilst incidence of unemployment matters and the significance of duration at conventional levels is not robust to extensions of the observation period, the impact of OLF spells runs mainly through the duration effect. 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. These negative implications are long lasting.

In chapter 5, a rigorous assessment of the van Dijk & Folmer (1999) hypothesis - that unemployment experienced in high unemployment regions is seen as

[1]Given that we do not directly control for individual unobserved heterogeneity, regional rankings based on the CQR results could be criticised. However, CQR is robust to the distribution of unobserved heterogeneity in that regions will retain their relative importance in the results. Rank invariance carries over to the estimates in Table 4.5.

more of a characteristic of that region and less of a negative productivity signal- is conducted and its long-term implications assessed for the UK.

This contribution is unique to Wage Scarring and to the UK. Moreover, the main hypothesis under test has only been tested in the short-run, and for the Netherlands (van Dijk & Folmer 1999) and Italy (Lippi & Ordine 2002). Both studies find evidence supporting this hypothesis, however, whilst the effect points in the right direction, I do not find strong support for the (van Dijk & Folmer 1999) hypothesis for the UK on average as the differences in these penalties across high and low unemployment regions are not statistically different. Granted I do find stronger support amongst over 45s made redundant in their previous jobs. These results remain robust to specification changes. I develop Continuous Work-Life Histories, using a novel approach extending that of Upward (1999) and taking into account concerns within the literature. The novelty of this approach is that labour market history since leaving full-time education is incorporated, allowing for a direct measure of experience to be used in the analysis. This allows for the regional location at the time of displacement, whilst searching for a job, and at re-employment to be directly controlled for. Since actual and potential experience are more likely to diverge for women, assessing the questions in this chapter in the context of females' Wage Scarring outcomes is of direct interest.

Another contribution of this thesis is to investigate whether low-skilled jobs are a Stepping Stone to better matches for the overqualified. Over-qualification is an important phenomenon in the labour market, both in terms of future earnings potential but also in terms of career mobility. This affects all spectrums of the earnings and skills distribution, impacting on the prospects of both school leavers, university graduates and even PhDs.

This is a contentious area, let alone the measurement of over-education. Omitted Variable Bias and Measurement error are key issues. This requires a rigorous approach to evaluating the impact of over-qualification that has to date been distinctly lacking in the literature, bar one or two examples (Leuven & Oosterbeek 2011). By differentiating between the broad skill requirements of a job (using the ISCO methodology to distinguish between the task composition of fine 3-digit occupation classifications), this assessment of the career mobility consequences of over-education gets closer to controlling for the differing educational requirements across jobs. Furthermore, controlling for job match satisfaction in robustness checks, following as similar an approach to Chevalier (2003) as possible with the BHPS, leads to results consistent with the main analysis.

Over-qualification, independent of whether experienced in skilled or low-skilled work, increases the probability of low-skilled and decreases the probability of skilled employment when compared to being in a well-matched skilled job, all else equal. Furthermore, over-qualification is more damaging for career mobility if experienced in low-skilled employment. Low-skilled employment is more of a Stepping Stone to skilled employment for females than males, independent on over-qualification. However, conditional on being overqualified, only women in low-skilled employment are more upwardly mobile than men. Over-qualification in high-skilled employment carries greater negative career mobility implications for females than their male counterparts. Important variation is evident, both in terms of previous industry and firm characteristics. Whilst upward career mobility may be on average more limited for females in part-time than full-time work, part-time work seems to be an indicator of loose labour market attachment for males on average. Moreover, the effect of being over-qualified is not invariant to the business cycle. Contrasting results from a 1988 and 2008 based classification of occupational skill, estimates suggest that upward career mobility may have increased (decreased) between 1988 and 2008 for overqualified females (males) in general. Moreover, with regards to state-dependence, this story is robust to the definition of over-qualification.

Potential criticisms Drawing on a youth sample from the Joint Unemployment and Vacancies Operating System (JUVOS), Kalwij (2004) find evidence to suggest that regional characteristics impact more on the probability of unemployment (unemployment incidence) than on the probability of re-employment (unemployment duration). Given that the study does not use linked employer-employee data it assumes that gaps in employment history, when individuals are off the claimant register, are periods of employment. This assumption implies that the study's results are potentially seriously biased. Using linked New Earnings Survey (NES)-JUVOS data would allow one to construct full employment biographies. Although not the initial question of this chapter, this would allow for a better answer to the question of wether region affects the incidence or duration of unemployment most and be a welcome complement to the thesis as a whole.

The search and matching literature stresses the importance of labour market tightness for re-employment probabilities. Moreover, studies have shown a role for both the stock of vacancies and jobseekers (Petrongolo 2001). As noted in chapter 4, incorporating this measure would mean making some strong assumptions due to a 2 year gap in the vacancies series at lower levels of aggregation. This is why we use unemployment to proxy supply and demand factors instead. Rather than using the Cox PH model, a discrete-time duration model with Heckman & Singer (1984) distributed unobserved heterogeneity could be employed. Moreover, Multiple Spells and Competing Risks are of importance. The advantage of controlling for multiple spells is that identification in multiple spell context achieved under much weaker assumptions than in the single spell case (Honore 1993). Individuals moving onto Income Support and Incapacity Benefits are likely to have very different observed and unobserved characteristics to those finding re-employment, which is of direct interest for policy makers. Incorporating more realism into econometric methods for duration analysis strengthens policy recommendations. However, more flexible techniques are not without their limitations. Although identification under competing risks has only been proved under independent risks Van den Berg (2001), this imposes restrictions on the nature of the error structure between competing risks.

Since the aim is to control for labour market history since leaving full-time education, the Chapter 5 could be extended by allowing new labour market entrants to enter the sample. However, since actual and potential experience are more closely aligned for males, an alternative approach could be to use potential instead of actual experience and allow individuals to enter the sample at any point in time and model selection as a dynamic problem (e.g. Wooldridge 1995).

In Chapter 5 the appropriateness of the un-testable exclusion restrictions, and those generally used to support Identification of the Heckman LIML 2-step estimator, was called into question. Given the likely identification issues, more robust modern techniques like IV could be employed which would not rely on un-testable assumptions for identification. An important caveat is that the results testing the van Dijk & Folmer (1999) are based on ILO unemployment data at the Local Authority and not TTWA level of aggregation, where TTWA's approximate self-contained labour markets. Local Authorities cannot be considered self-contained and thus their use would call for alternative econometric techniques which take into account this spatial autocorrelation, e.g. Spatial Econometrics.

In Chapter 6, the transition probabilities associated with the over-qualification in high-skilled employment category are likely to be measured imprecisely due to small cell size issues. It would thus be instructive if large scale administrative panel data could be used to get a more accurate picture for this population sub-group. Moreover, this would allow for decomposition by previous industry and firm characteristics based on the Chevalier (2003) methodology.

Whilst the results are compelling, it is important to determine whether the Independence of Irrelevant Alternatives assumption, analogous to the proportionality assumption in the duration model context, is not driving the results as this assumption is likely to not be appropriate in the setting considered. Given the dynamic nature of the models presented in Evans (1999) and Léné (2011), it is difficult to argue that the choice of less-skilled employment reduces both the probabilities of skilled and non-employment proportionally.

For skilled individuals downgrading into less-skilled employment, this temporary situation may act as a Stepping Stone to future skilled employment during skilled job shortages/economic contractions thus increasing, and not decreasing, skilled employment probability. For others downgrading, this may turn out to be a permanent solution. The more skilled workers downgrade into less-skilled occupations the more likely that less-skilled individuals in less-skilled employment will be squeezed into non-employment. Moreover, the traditional route into skilled employment for the less-skilled is via experience accumulation in less-skilled positions (although Léné (2011) provides evidence to suggest that this route is becoming less likely).

Since labour market states are likely to be correlated over time periods greater than t and t-1, this would violate the conditional independence (across time periods) assumption of the pooled MNL. Techniques can be exploited to control for the endogeneity of Initial Conditions, and thus fully capture serial correlation over time when incorporated with Mundlak (1978) terms (Wooldridge 2005)[2]. However, alternative labour market states would still be

[2]Given the discrete choice setting, the inclusion of Mundlak (1978) terms on their own without controlling for endogeneity of initial conditions is likely to raise identification issues, given the endogeneity of Initial Conditions problem, an subject for investigation in a discus-

assumed independent. Haan & Uhlendorff (2006) propose a method of jointly modelling labour market states using a bivariate normal distribution (correlated random effects) which breaks the IIA/proportionality restriction of the standard MNL allowing for more flexible characterisations of state dependence (see Section 2 for more information).

Since the booster samples, which over-sample low socioeconomic group status individuals, are included in the analysis, it could be argued that this exacerbates the Initial Conditions problem. The biggest obstacle to applying these methods is time, as they can be computationally intensive especially when solving higher dimensional integrals given current computing limitations (Train 2009). Steps are being made to address these issues, which will be incorporated into a discussion paper version of this chapter. However, it would be impractical to estimate all versions of the model given that this process can take 3/4 weeks using the current computing setup.

Ideally a more appropriate definition of Skill Mismatch could be employed, in order to better capture *direct* job skill requirements. There is a wide gap in the skill mismatch on the investigation of the impact of measurement error on coefficient estimates. A limited number of papers in the Over-education literature have explicitly addressed this issue, e.g. Chevalier (2003). The main focus of this literature has been on the identification of "required skills" using both subjective and objective measures for contrast. Subjective measures have been championed as giving direct evidence of skill requirements, however they are almost exclusively from the employees viewpoint and thus likely to suffer from substantial measurement error.

Whilst the subjective approaches are more likely to capture the time-varying nature of this notion, almost exclusively objective studies have used time-

sion paper version of this chapter. Moreover, identification considerations would be aided by an explicit formulation of the decision making process under consideration.

invariant measures to classify occupations. One notable criticism of the subjective approach is the nature of individuals references. See for example, Bago d'Uva *et al.* (2009) for an exposition of the vignettes approach to identification in survey data which attempts to control the comparison groups which survey respondents use when answering subjective questions. If individuals are comparing the requirements when they were hired to current requirements, then this will capture the fact that "upgrading of skill requirements" due to technological change has resulted in occupations that were once less-skilled becoming more skilled, thus raising current requirements. However, it will not capture accumulation of on-the-job training which contributes to skill formation. If individuals are comparing their education levels at the time of hire to requirements at that time, then this will essentially be time-invariant. Subjective methods aside, I am unaware of any studies using time-varying objective measures of skill mismatch. The current study uses a time-invariant classification based the mid-point of the observation period, however the educational composition of the labour force has changed markedly between 1991 and 2008. I have experimented with a time-varying objective measure on a shorter panel, and intend to incorporate this into a working paper version of this chapter. However, this would be subject to the criticisms raised by Evans in Chapter 6, Section 6.2.

Interesting follow on questions In future work I would like to extend Chapter 4, employing flexible discrete-time duration models to address related questions. Men and women could be analyzed separately, as unobserved factors impacting on male and female labour market transitions are likely to differ. Moreover, the difference between the ILO unemployment and Claimant Count series is more likely to be larger for females than males (anonymous referee pointed this out). Multiple spells, time-varying covariates, and competing risks would also add further realism. Potential questions of interest include the combined impact of jobcenter closures (data at hand), regional labour markets, and the distance to a jobcenter (data at hand). It would also be interesting to assess how the impact of regional location has changed over time. Has living in certain areas got better or worse for re-employment prospects?

Recent research suggests that that unobserved factors not adequately captured in the model may not be biasing results towards not finding strong support for the van Dijk & Folmer (1999) hypothesis. Looking at the the psychological cost of being unemployed and whether this is lower if there is more unemployment around, using Swiss and German data Oesch & Lipps (2011) find that becoming unemployed hurts as much when regional unemployment is high as when it is low. Moreover, the impact on well-being does not diminish over time, nor do repeated episodes of unemployment improve wellbeing. Granted, this result may not hold for the UK. Thus alternative empirical strategies are likely to lend credence to the results herein.

In terms of Chapter 5, difference-in-difference approaches could be adopted to assess the impact of the introduction of JSA on unemployment/wage scarring. It would be of interest to assess whether the cuts the eligibility to non-means tested unemployment benefits from 12 to 6 months increased the initial unemployment penalty, and whether this impacted on subsequent wage growth. Petrongolo (2009) conducts a similar study finding that while tighter search requirements were successful in moving individuals off unemployment benefits, they were not successful in moving them onto long-lasting or better jobs. Establishing whether significant regional variation in this effect exists would complement the initial research questions. Recent literature, e.g. Elsby *et al.* (2009) and Petrongolo & Pissarides (2008) suggests that incidence and duration are related to the business cycle, and it would be fruitful in future work to investigate this further in relation to the van Dijk & Folmer (1999) hypothesis and research questions under test. Inflow rates countercyclical, especially for job losers (layoffs), whereas outflow rates are procyclical. This suggests that high unemployment levels in a recession are driven by longer unemployment durations, rather than higher incidence.

In addition to the econometric and measurement issues highlighted in the conclusion to Chapter 6, the following highlights other considerations if policy recommendations are to be drawn from this work. Determining, beyond reasonable doubt, whether less-skilled employment is more persistent than non-employment will aid policy makers when deciding whether to concentrate resources on getting people into work or into a good match. Moreover, an alternative approach to quantify the scarring effects is through wage equations.

As noted, the result that the overqualified in high-skilled employment have poorer upward career prospects than their well matched counterparts is unexpected. Taken in the context of existing work looking at the returns to over-education, this suggests that the negative implications of over-qualification could be further reaching. Sicherman (1991) finds that, all else constant, overeducated workers get higher wages than their coworkers but lower wages than workers with similar levels of schooling working in well-matched jobs. Moreover, Dolton & Silles (2008) find that UK graduates that were overeducated in their first jobs earn less that those which were not. This suggests that over-education is a negative productivity signal that the reflects educational quality not adequately captured in studies investigating the average returns to attained levels of education. Establishing the extent to which workers who transition into low and high skilled jobs suffer a wage penalty relative to workers who were previously matched in these jobs and the relative magnitudes of these penalties. Dealing with potential endogeneity issues through alternative methods, e.g. Arellano-Bond and/or GMM, could also strengthen conclusions.

I see the final chapter as a potential platform for a wide range of future research initiatives. These research questions include investigating the impact on unemployment duration: is there a threshold duration above which previous over-qualification becomes an issue? Moreover, is there a threshold job duration above which over-qualification starts to deplete skilled human capital? These questions would require the adoption of a flexible discrete-time duration approach, as mentioned in the Methodology section. What is the impact of over-qualification in one's previous job on accepted wages, and what consequences does this have for future wage growth? Is there a threshold length of time in low-skilled employment/unemployment for graduates, above which over-qualification becomes a serious issue? In general, there is a 2 years threshold above which consideration for graduate schemes is very difficult. Moreover, is there regional variation in the impact of over-education on the graduate labour market. If liquidity constraints (Chetty 2008), in this case due to mortgaged home ownership, mean that unemployed mortgaged home owners are more likely to accept the first job offer in order to keep up mortgage payments then do shocks to local (TTWA) house prices (Nationwide time series data at hand) result in a higher probability of over-education (in less-skilled employment) and what consequences does this have for future earnings trajectories? What impact does self-employment have on career mobility, once the type of job engaged in when self-employed is taken into account? Does human capital depreciate whilst self-employed? Does it matter whether self-employment was skilled or unskilled? Is the signal of previous self-employment enough to signal high productivity to potential employers?

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Appendix A

Chapter 4: Summary Statistics

Table A.1: Description & summary statistics of individual-level covariates, 1999-2005

Variable	Mean	SD
Calendar time		
Years (ref. 1999):		
y2000	.158	.365
y2001	.156	.363
y2002	.137	.344
y2003	.139	.346
y2004	.125	.331
y2005	.121	.326
Quarters (ref.q1):		
q2	.231	.422
q3	.244	.429
q4	.263	.440
Socio Demographics:		
Age<25	.493	.500
Age>55	.012	.110
Female	.223	.416
Occupation (ref. unkown):		
Elementary	.374	.484
Manufacturing	.071	.257
Trade & Services	.364	.481
Technical	.048	.213
Senior & Professional	.057	.231
Work History variables:		
Active Labour Market Participation	.146	.354
Long-Term Unemployment	.357	.479
Incapacity Benefits	.059	.237
Income Support	.014	.117
Number of obs = 187,032, 39.1% censored.		
Min/Median/Max duration in days: 1/60/2,899.		

Source: Joint Unemployment and Vacancies Operating System (JUVOS) 5% cohort.

Appendix B

Chapter1: Sensitivity Analysis

The multiple spell data structure of the JUVOS raises concerns about the validity of conclusions made, treating each observation as a single independent spell. To see whether this concern makes a difference to the results, I draw a random observation for each individual in the data and rerun the basic results. Table B.1 compares the basic model across alternative parametric and non-parametric baseline hazards. The results indicate general robustness to this restriction. Where there are differences, e.g. to the accessibility indicator, this does not change the general story.

Table B.1: Estimated hazard ratios of the Exponential♯, Gompertz♯ & Cox PH (vs. original results) models.

	Exponential (SE)	Gompertz (SE)	Cox PH (SE)	(SE)
	exp1A5	gomp1A5	cox1A5	Cox†
	b/p	b/p	b/p	
Socio-demographics				
age<25	0.971	0.903	0.900	0.894
	(0.021)	(0.000)	(0.000)	(0.000)
age>56	0.687	0.725	0.726	0.847
	(0.000)	(0.000)	(0.000)	(0.000)
female	1.084	1.069	1.067	1.033
	(0.000)	(0.000)	(0.000)	(0.000)
Occupation(ref:Elementary)				
Manufacturing	1.256	1.234	1.231	1.157
	(0.000)	(0.000)	(0.000)	(0.000)
Trade, services	1.309	1.263	1.260	1.168
	(0.000)	(0.000)	(0.000)	(0.000)
Technical	1.266	1.244	1.238	1.129
	(0.000)	(0.000)	(0.000)	(0.000)
Senior, professional	1.478	1.415	1.405	1.288
	(0.000)	(0.000)	(0.000)	(0.000)
Unknown	1.844	1.679	1.672	1.348
	(0.000)	(0.000)	(0.000)	(0.000)
Previous Work History				

Continued on next page

Table B.1 – continued from previous page

	Exponential	Gompertz	Cox PH	
	(SE)	(SE)	(SE)	(SE)
Active Labour Market Programme	0.700	0.743	0.746	0.802
	(0.000)	(0.000)	(0.000)	(0.000)
Long-Term Unemployment	0.518	0.583	0.587	0.654
	(0.000)	(0.000)	(0.000)	(0.000)
Incapacity Benefits	0.873	0.872	0.873	0.955
	(0.000)	(0.000)	(0.000)	(0.000)
Income Support	0.855	0.852	0.854	0.943
	(0.002)	(0.001)	(0.001)	(0.027)
Calendar time(ref: 1999q1)				
y2000	1.016	1.002	1.002	1.018
	(0.392)	(0.891)	(0.888)	(0.072)
y2001	1.067	1.039	1.037	1.029
	(0.001)	(0.021)	(0.027)	(0.006)
y2002	1.063	1.037	1.035	0.985
	(0.004)	(0.043)	(0.049)	(0.161)
y2003	0.986	0.963	0.962	0.935
	(0.506)	(0.030)	(0.024)	(0.000)
y2004	0.944	0.926	0.924	0.889
	(0.006)	(0.000)	(0.000)	(0.000)
y2005	0.874	0.857	0.860	0.754
	(0.000)	(0.000)	(0.000)	(0.000)
Quarter(ref: q1)				
q2	0.957	0.956	0.957	0.991
	(0.004)	(0.001)	(0.001)	(0.272)
q3	0.952	0.956	0.958	0.971
	(0.001)	(0.001)	(0.001)	(0.001)
q4	0.916	0.913	0.914	0.902
	(0.000)	(0.000)	(0.000)	(0.000)
Regional variables				
Urban	0.933	0.948	0.947	0.947
	(0.000)	(0.001)	(0.001)	(0.000)
Accessible	0.929	0.929	0.931	0.987
	(0.054)	(0.021)	(0.023)	(0.499)
University Present	0.918	0.919	0.919	0.919
	(0.000)	(0.000)	(0.000)	(0.000)
Skill Intensity	1.000	1.004	1.004	1.000
	(0.966)	(0.588)	(0.577)	(0.936)
GDPPH	1.028	1.051	1.046	0.972
	(0.699)	(0.410)	(0.449)	(0.471)
ILO unemployment rate	0.983	0.982	0.983	0.980
	(0.012)	(0.003)	(0.003)	(0.000)
Change in GDPPH	0.993	0.992	0.992	1.003
	(0.388)	(0.232)	(0.252)	(0.546)
Change in ILO unemployment rate	0.992	0.991	0.991	1.000
	(0.282)	(0.175)	(0.184)	(0.957)
Flow of Unemployed/ Resident Population	0.956	0.966	0.967	0.976
	(0.000)	(0.001)	(0.001)	(0.000)
New Small Business Startups/ Resident Population	0.979	0.976	0.977	0.978
	(0.096)	(0.029)	(0.035)	(0.003)
18-24 New Deal Starters	1.021	1.020	1.020	1.012
	(0.006)	(0.003)	(0.004)	(0.005)
constant	0.003	0.003		
	(0.000)	(0.000)		
gamma				
constant		0.998		
		(0.000)		
N	92008	92008	92008	187,032
LL	-113328	-111313	-513183	
AIC	2.3e+05	2.2e+05	1.0e+06	

Significance levels: ***: 1% **: 5% *: 10%
Note: for regional dummy results see Figure 4.1(Cox model B only)
† Original specification.

A limited covariate set at the individual-level raises the concern that individual-level unobserved heterogeneity could be driving the results. Despite its flexibility of a semi-parametric baseline hazard, penalized likelihood of the partial likelihood function in the Cox PH model implies a "curse of dimensionality" as this estimation strategy penalizes the researcher for loss of degrees of freedom. This drawback implies that the Cox model cannot accommodate an individual-level frailty parameter, a limitation not faced in parametric specifications.

Table B.2: Estimated hazard ratios of the Exponential$^{\sharp}$, Gompertz$^{\sharp}$ & Cox PH models.

	Exponential Model (SE)	Gompertz Model (SE)	Cox Model (SE)
constant	0.011	0.010	
	(0.000)	(0.000)	
Socio-demographics			
age<25	0.848	0.878	0.894
	(0.000)	(0.000)	(0.000)
age>56	0.847	0.845	0.847
	(0.000)	(0.000)	(0.000)
female	1.044	1.037	1.033
	(0.000)	(0.000)	(0.000)
Occupation(ref:Elementary)			
Manufacturing	1.209	1.175	1.157
	(0.000)	(0.000)	(0.000)
Trade, services	1.214	1.185	1.168
	(0.000)	(0.000)	(0.000)
Technical	1.156	1.141	1.129
	(0.000)	(0.000)	(0.000)
Senior, professional	1.372	1.321	1.288
	(0.000)	(0.000)	(0.000)
Unknown	1.436	1.380	1.348
	(0.000)	(0.000)	(0.000)
Previous Work History			
Active Labour Market Programme	0.761	0.787	0.802
	(0.000)	(0.000)	(0.000)
Long-Term Unemployment	0.589	0.630	0.654
	(0.000)	(0.000)	(0.000)
Incapacity Benefits	0.951	0.952	0.955
	(0.003)	(0.001)	(0.000)
Income Support 0.933	0.939	0.943	
	(0.039)	(0.027)	(0.027)
Calendar time(ref: 1999q1)			
y2000	1.014	1.017	1.018
	(0.259)	(0.120)	(0.072)
y2001	1.021	1.028	1.029
	(0.113)	(0.014)	(0.006)
y2002	0.971	0.981	0.985
	(0.034)	(0.107)	(0.161)
y2003	0.907	0.926	0.935
	(0.000)	(0.000)	(0.000)
y2004	0.857	0.880	0.889
	(0.000)	(0.000)	(0.000)
y2005	0.692	0.732	0.754
	(0.000)	(0.000)	(0.000)
Quarter(ref: q1)			
q2	0.995	0.992	0.991
	(0.598)	(0.379)	(0.272)
q3	0.972	0.970	0.971
	(0.007)	(0.001)	(0.001)
q4	0.862	0.888	0.902
	(0.000)	(0.000)	(0.000)
Regional variables			
Urban	0.932	0.943	0.947
	(0.000)	(0.000)	(0.000)
Accessible	0.998	0.989	0.987
	(0.919)	(0.605)	(0.499)
University Present	0.899	0.911	0.919
	(0.000)	(0.000)	(0.000)
Skill Intensity	1.003	1.001	1.000

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Table B.2 – continued from previous page

	Exponential Model (SE)	Gompertz Model (SE)	Cox Model (SE)
	(0.567)	(0.809)	(0.936)
GDPPH	0.957	0.971	0.972
	(0.367)	(0.487)	(0.471)
ILO unemployment rate	0.974	0.977	0.980
	(0.000)	(0.000)	(0.000)
Change in GDPPH	1.004	1.003	1.003
	(0.447)	(0.573)	(0.546)
Change in ILO unemployment rate	1.000	1.000	1.000
	(0.957)	(0.944)	(0.957)
Flow of Unemployed/ Resident Population	0.969	0.974	0.976
	(0.000)	(0.000)	(0.000)
New Small Business Startups/ Resident Population	0.968	0.975	0.978
	(0.001)	(0.001)	(0.003)
18-24 New Deal Starters	1.014	1.013	1.012
	(0.009)	(0.005)	(0.005)
ln_the			
constant	0.487	0.145	
	(0.000)	(0.000)	
gamma			
constant		0.998	
		(0.000)	
NUTS3 fixed effects	✓	✓	✓
LL	-249553	-249140	-1311963
Number of obs = 187,032			

Significance levels: ***: 1% **: 5% *: 10%
Note: for regional dummy results see Figure 4.1(Cox model B only)
♯ Gamma distributed unobserved heterogeneity.

Maintaining a competing risks data structure, under independent risks, I estimate parametric models with unobserved heterogeneity controls at the individual-level as a robustness check. Tables B.2 & B.3 present Exponential and Gompertz models with Gamma and Inverse Gaussian distributed heterogeneity respectively[1]. The results highlight that the flexible Cox Proportional Hazards specification captures individual heterogeneity very well. Not directly controlling for individual heterogeneity implies that the baseline hazard will be confounded by both state dependence and individual heterogeneity. The industry standard in the Statistics literature is to compare the Cox estimates to those of the Censored Quantile Regression model (Portnoy, 2003), hence this strategy is followed in the main text given the robustness of this specification. These results reflect the conclusions reached by Cameron & Trivedi (2005) and

[1]The Weibull model was estimated, however this failed to converge. Furthermore, the Piecewise Constant Exponential model with a baseline hazard constant for 1 month failed to converge. The assumption that hazards are constant over each month seems a bit arbitrary, with no theoretical foundation especially given that the underlying data is in daily format. I present Piecewise Constant hazards in Table B.4, where the hazard has been assumed to be constant for 60 day intervals. Reducing this interval increases the data size exponentially. A hazard constant over each day fails to converge and introduces the limitation of computational power, even on a system with a quad core processor and 16GB of RAM.

others in the literature, that with single spell data the specification of unobserved heterogeneity is not so important given a flexible baseline hazard. One would expect sensitivity of results across the parametric specifications, however, no evidence of this is found. It may be that results are more sensitive in a specification which takes into account time-varying covariates however this possibility is left for future research.

Table B.3: Estimated hazard ratios of the Exponential♯, Gompertz♯ & Cox PH models.

	Exponential Model (SE)	Gompertz Model (SE)	Cox Model (SE)
constant	0.012	0.010	
	(0.000)	(0.000)	
Socio-demographics			
age<25	0.867	0.878	0.894
	(0.000)	(0.000)	(0.000)
age>56	0.829	0.845	0.847
	(0.000)	(0.000)	(0.000)
female	1.042	1.037	1.033
	(0.000)	(0.000)	(0.000)
Occupation(ref:Elementary)			
Manufacturing	1.202	1.175	1.157
	(0.000)	(0.000)	(0.000)
Trade, services	1.213	1.185	1.168
	(0.000)	(0.000)	(0.000)
Technical	1.155	1.141	1.129
	(0.000)	(0.000)	(0.000)
Senior, professional	1.369	1.321	1.288
	(0.000)	(0.000)	(0.000)
Unknown	1.447	1.380	1.348
	(0.000)	(0.000)	(0.000)
Previous Work History			
Active Labour Market Programme	0.759	0.787	0.802
	(0.000)	(0.000)	(0.000)
Long-Term Unemployment	0.585	0.630	0.654
	(0.000)	(0.000)	(0.000)
Incapacity Benefits	0.952	0.952	0.955
	(0.003)	(0.001)	(0.000)
Income Support	0.935	0.939	0.943
	(0.043)	(0.027)	(0.027)
Calendar time(ref: 1999q1)			
y2000	1.019	1.017	1.018
	(0.142)	(0.120)	(0.072)
y2001	1.025	1.028	1.029
	(0.056)	(0.014)	(0.006)
y2002	0.976	0.981	0.985
	(0.078)	(0.107)	(0.161)
y2003	0.914	0.926	0.935
	(0.000)	(0.000)	(0.000)
y2004	0.861	0.880	0.889
	(0.000)	(0.000)	(0.000)
y2005	0.699	0.732	0.754
	(0.000)	(0.000)	(0.000)
Quarter(ref: q1)			
q2	0.992	0.992	0.991
	(0.427)	(0.379)	(0.272)
q3	0.969	0.970	0.971
	(0.002)	(0.001)	(0.001)
q4	0.871	0.888	0.902
	(0.000)	(0.000)	(0.000)
Regional variables			
Urban	0.932	0.943	0.947
	(0.000)	(0.000)	(0.000)
Accessible	0.993	0.989	0.987
	(0.782)	(0.605)	(0.499)
University Present	0.901	0.911	0.919
	(0.000)	(0.000)	(0.000)
Skill Intensity	1.002	1.001	1.000
	(0.745)	(0.809)	(0.936)
GDPPH	0.953	0.971	0.972
	(0.326)	(0.487)	(0.471)

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Table B.3 – continued from previous page

	Exponential Model (SE)	Gompertz Model (SE)	Cox Model (SE)
ILO unemployment rate	0.974	0.977	0.980
	(0.000)	(0.000)	(0.000)
Change in GDPPH	1.005	1.003	1.003
	(0.404)	(0.573)	(0.546)
Change in ILO unemployment rate	1.000	1.000	1.000
	(0.974)	(0.944)	(0.957)
Flow of Unemployed/ Resident Population	0.969	0.974	0.976
	(0.000)	(0.000)	(0.000)
New Small Business Startups/ Resident Population	0.970	0.975	0.978
	(0.001)	(0.001)	(0.003)
18-24 New Deal Starters	1.013	1.013	1.012
	(0.011)	(0.005)	(0.005)
ln_the			
constant	0.673	0.145	
	(0.000)	(0.000)	
gamma			
constant		0.998	
		(0.000)	
NUTS3 fixed effects	✓	✓	✓
LL	-249935	-249140	-1311963
Number of obs = 187,032			

Significance levels: ***: 1% **: 5% *: 10%
Note: for regional dummy results see Figure 4.1(Cox model B only)
♯ Inverse-Gaussian distributed unobserved heterogeneity.

Table B.4: Estimated hazard ratios of the Piecewise Constant Exponential Hazard model♯.

	[1] (SE)	[2] (SE)	[3] (SE)
Socio-demographics			
age<25	0.901	0.884	0.887
	(0.000)	(0.000)	(0.000)
age>56	0.712	0.695	0.696
	(0.000)	(0.000)	(0.000)
female	0.991	0.989	0.989
	(0.356)	(0.290)	(0.300)
Occupation(ref:Elementary)			
Manufacturing	1.191	1.212	1.209
	(0.000)	(0.000)	(0.000)
Trade, services	1.166	1.178	1.177
	(0.000)	(0.000)	(0.000)
Technical	1.116	1.121	1.120
	(0.000)	(0.000)	(0.000)
Senior, professional	1.215	1.227	1.227
	(0.000)	(0.000)	(0.000)
Unknown	1.250	1.256	1.257
	(0.000)	(0.000)	(0.000)
Previous Work History			
Active Labour Market Programme	0.840	0.832	0.833
	(0.000)	(0.000)	(0.000)
Long-Term Unemployment	0.645	0.620	0.623
	(0.000)	(0.000)	(0.000)
Incapacity Benefits	0.964	0.966	0.966
	(0.084)	(0.142)	(0.131)
Income Support	0.925	0.917	0.919
	(0.059)	(0.056)	(0.057)
Calendar time(ref: 1999q1)			
y2000	1.026	1.025	1.025
	(0.086)	(0.131)	(0.121)
y2001	1.041	1.038	1.038
	(0.008)	(0.027)	(0.022)
y2002	0.998	0.990	0.991
	(0.915)	(0.565)	(0.623)
y2003	0.914	0.898	0.900
	(0.000)	(0.000)	(0.000)
y2004	0.876	0.857	0.860
	(0.000)	(0.000)	(0.000)
y2005	0.766	0.737	0.741
	(0.000)	(0.000)	(0.000)
Quarter(ref: q1)			
q2	0.987	0.988	0.988
	(0.300)	(0.362)	(0.344)
q3	0.987	0.986	0.986
	(0.278)	(0.290)	(0.279)
q4	0.923	0.913	0.915
	(0.000)	(0.000)	(0.000)
Regional variables			

Continued on next page

Table B.4 – continued from previous page

	[1] (SE)	[2] (SE)	[3] (SE)
Urban	0.940	0.933	0.933
	(0.000)	(0.000)	(0.000)
Accessible	0.937	0.934	0.934
	(0.026)	(0.033)	(0.032)
University Present	0.925	0.918	0.918
	(0.000)	(0.000)	(0.000)
Skill Intensity	1.006	1.008	1.007
	(0.355)	(0.314)	(0.321)
GDPPH	1.003	1.005	1.004
	(0.955)	(0.930)	(0.942)
ILO unemployment rate	0.981	0.978	0.978
	(0.000)	(0.000)	(0.000)
Change in GDPPH	1.001	1.000	1.000
	(0.907)	(0.974)	(0.952)
Change in ILO unemployment rate	0.997	0.996	0.996
	(0.576)	(0.603)	(0.599)
Flow of Unemployed/ Resident Population	0.972	0.971	0.971
	(0.003)	(0.004)	(0.004)
New Small Business Startups/ Resident Population	0.978	0.974	0.975
	(0.022)	(0.015)	(0.016)
18-24 New Deal Starters	1.013	1.013	1.013
	(0.053)	(0.066)	(0.064)
ln_the			
constant		0.242	0.246
		(0.000)	(0.000)
N	236789	236789	236789
F			
LL	-130964	-130908	-130913
AIC	2.6e+05	2.6e+05	2.6e+05

Number of obs = 236,789

Significance levels: ***: 1% **: 5% *: 10%
All specifications include regional dummies. Data expanded so that the hazard rate is constant over each 60 day interval. Data size highlights that a large proportion of spells in the data lasted less than two months.
♯ [1] No unobserved heterogeneity controls; [2] Gamma [3] Inverse-Gaussian distributed unobserved heterogeneity.

Appendix C

Chapter 4: Linking the Individual & Regional Levels

This appendix briefly describes how the link between the individual and regional data was established. For more details and a full description of the regional data see the Appendix, Section D.

Overview of Process

Main data sources included the JUVOS, National Statistics Postcode Directory (NSPD), NOMIS and the Local Area Quarterly Labour Force Survey (available from the UK Data Archive). The linked data set matching the individual- and regional-level data to the UK geography is conditioned on the start of claimant spells. In order to match the continuous individual-level data to the regional information, individual spells were matched to the regional information pertaining to the month in which they started. Merging the two data sources was a non trivial exercise which involved several technical difficulties due a lack of a one-to-one link between regional entities. Due to censorship of the full postcode information in the individual-level JUVOS data[1], this introduced an *overlapping regions problem*, removing the one-to-one link between the individual- and regional-levels. In addition a one-to-one match between local authorities and

[1]Only postcode district information is available in the JUVOS

NUTS3 regions does not exist for Scotland.

In an attempt to overcome this problem, postcode districts were matched to full postcode information using the National Statistics Postcode Directory (NSPD). Merging schemes were defined in order to create a one-to-one link between the different regional classifications. Although more complicated methods are available, e.g. map-based area interpolation (see Arntz & Wilke 2007), a simple average weighting method was employed that assigns a postcode district to the local authority in which it most falls based on the full postcode information. This link was established for all regional definitions of interest resulting in a one-to-one link between the postcode district, local authority and NUTS3 levels of aggregation.

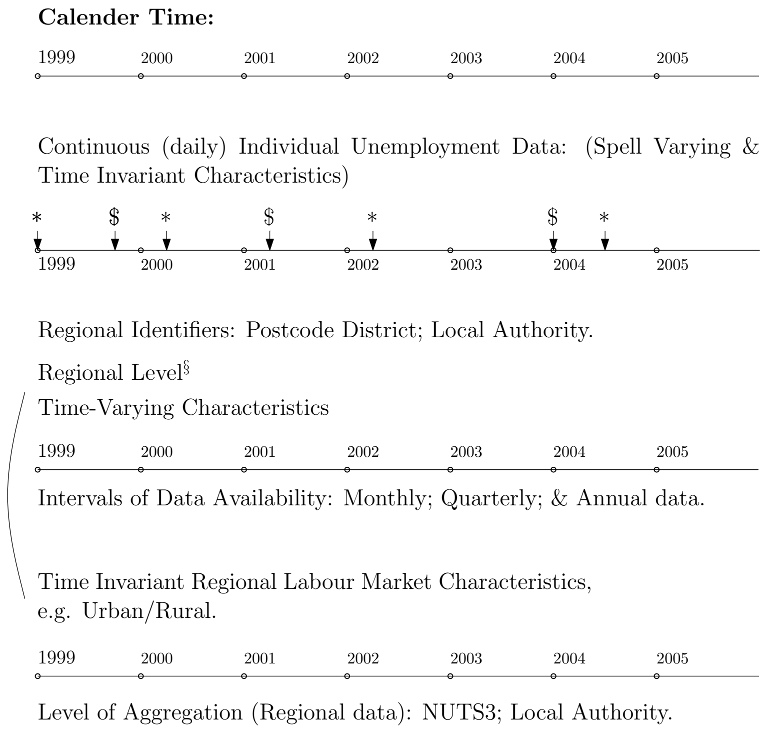
Regional Identifier

Our aim is to exploit the regional variation in the JUVOS in order to create a link between individual-level and the economic and institutional environment in which claimants reside. To this end, we identified the following geographical information in the JUVOS:

- Self-reported residential postcode data (censored to the postcode district level).
- Unemployment Benefit Office (UBO) codes.

Given the self-reported nature of the first option, we were faced with data quality issues were present with postcode information missing or wrongly imputed at times. In order to improve the quality of this indicator, we used the following imputation strategy: *Replace the current postcode with the self-reported postcode during the relevant claimant's previous unemployment spell (assuming*

Figure C.1: Structure of the data:

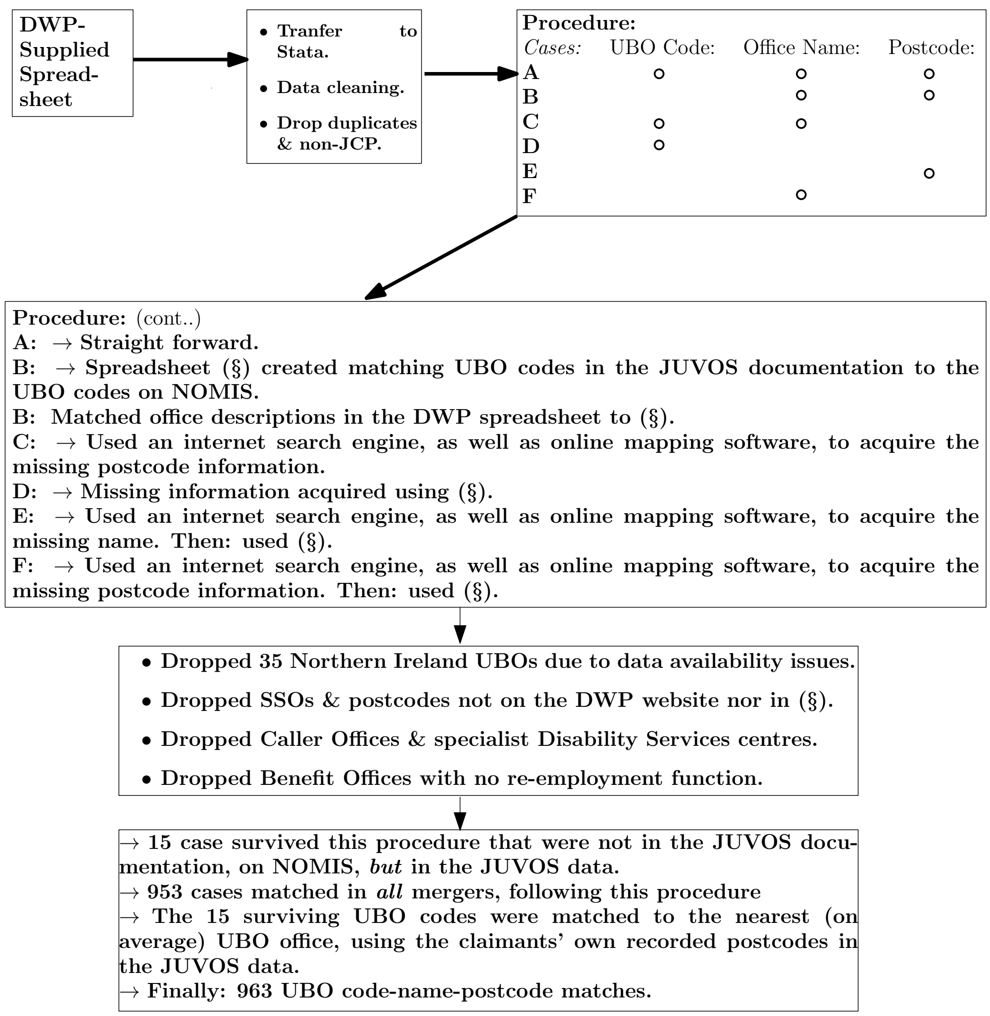


* = Individual characteristics collected at the start of each unemployment spell (assuming constant during spell).
$ = Exit destinations (restricted to Employment vs. Non-Employment).
§= Regional data merged to individual data by *month* of claimant spell commencement.

that the individual did not move during the intervening period). This strategy was implemented in 2.8% of the cases. In order to maintain some regional variation we only replaced this self-reported variable with the UBO postcode district when this variable was missing and no information could be obtained from previous spells (implemented in 2% of cases). Each observation in the JUVOS is reported by Unemployment Benefit Office, which allows us to complete this assignment in a relatively straightforward manner. The UBO postcodes were derived using the following procedure.

A spreadsheet containing detailed (but incomplete) information about benefit office locations was sourced from the Department of Work & Pensions (DWP). In order to prepare this information for use we followed the procedure outlined in Figure 2.

Figure C.2: Procedure for construction of Unemployment Benefit Office indicator:



· SSO = Social Security Office
· (§) = Spreadsheet matching UBO codes provided in the JUVOS documentation to the codes published on NOMIS.
· The JUVOS dataset is conditioned to include post 01/01/1996 data only.

The first step was to clean the data in the supplied spreadsheet, dropping certain entries and duplicates as well as checking whether the supplied information matched that available from the Department of Work & Pensions (DWP) online system[2]. Cases with missing UBO codes were noted, and where neces-

[2]Available at: http://www.jobcentreplus.gov.uk/JCP/Aboutus/Ouroffices/Search/LocalOfficeSearch.aspx

sary postcode information was ammended using internet search engines, Job Centre web pages, as well as the aforementioned DWP online search system. In some cases, all that was missing was the relevant postcode. However this problem was easily overcome by following the above procedure.

Jobcentres and jobcentre plus with the same postcode were assigned to the same UBO code, i.e. The Jobcentre was dropped. The spreadsheet provided by the DWP contained Social Security Office (SSO) locations. Since these offices are exclusively for the receipt of benefits and have no job related function, we decide to drop this information from the data. Specialist Disability Services centres were also dropped. After conditioning on post 01/01/1996 data, there were 963 Unemployment Benefit Office code-location matches in the data.

Appendix D

Constructing a Regional Level Dataset

D.1 Introduction

Cameron & Trivedi (2005) highlight the importance of controlling for unobserved heterogeneity at lower levels of aggregation when attempting to identify causal relationships in applied econometrics. "[It is important to control for confounding factors which arise] when the individual contributions of different regressors..to the variation in the variable of interest cannot be separated (see Cameron & Trivedi 2005, pg.8)." A common approach widely used is to control for these confounding factors using fixed effects. This approach essentially parameterizes the nature of unobserved heterogeneity to be a shift parameter. This parametrisation may itself drive the results, if incorrectly specified. Since fixed effects removes any time varying macroeconomic effects, changes in local labour demand conditions are not controlled for in models which explicitly take into account the regional context via regional dummies only[1].

There is a growing popularity of this approach to modelling economic interactions, and the recent availability of geographical products tailored for use in

[1]Control for time varying factors is possible by interacting the fixed effect with time dummies, however this approach is quite restrictive.

the field of Economics has fueled this. However, the level of geographical detail at which many of these studies are conducted - using full postcode information - means that much the data used is not publicly available. Publicly available economic data tends to be more commonly available in grouped form, at the Local Authority level of aggregation and higher. The individual administrative data that is publicly available in the UK, e.g. the Joint Unemployment & Vacancies Operating System, contains residential location information that is censored to the postcode district level of aggregation. These restrictions introduce numerous issues with linking publicly available data at various levels of aggregation, due to the fact that these sub-regional classifications are not necessarily contiguous[2]. This issue is not faced when using full postcode information.

The strategies implemented in this paper develop a one-to-one link between the postcode district- & higher levels of aggregation, allowing researchers to link publicly available datasets together from varying sources. This approach also provides added explanatory power, as it allows one to highlights the regional disparities driving the overall regional effect. Ultimately, the goal of this project is to provide new insights for Regional Policy.

The database covers the period 1995 to 2007, with variable availability varying depending on source (see the appendix in Ball 2009 for more information). Coverage is restricted to Great Britain (excluding Northern Ireland). Regional identifiers available include: Local Authority; Travel-to-Work Area; and Nomenclature of Territorial Units for Statistics (NUTS) level 3 (NUTS3) levels of aggregation. The dataset includes information pertaining to socio-demographic and institutional features, regional labour market performance, as well as supply and demand conditions. An example of the application of

[2]Regional boundaries may also change over time, an issue which we do not deal with in this study.

this resource is provided in chapter 5, whilst appendix C explains how the individual-level data was linked to the regional-level data set. Regional-level indicators were sourced from various providers with some key variables being self-constructed. Given the complexity of this procedure, this article describes this in detail.

D.2 Level of Aggregation of Interest & Main Data Sources.

The aggregation levels of interest to us are:

- Local Authority
- Travel to work areas (TTWA)
- Nomenclature of Territorial Units for Statistics (NUTS) level 3

The first question faced is at which level of aggregation the variables of interest are most relevant. Since the aim is to capture regional characteristics one is presented with the challenge of defining self-contained regional labour markets (Petrongolo 2001). TTWA's are the closest approximation, however there is a systematic lack of data at this level of aggregation. Thus data is collected at the lowest level of aggregation (local authority and NUTS3). This provides the flexibility to redefine geographical areas, aggregating up to the level of interest.

D.3 Linking the Regional levels

The Local Authority Unitary Authority (LAUA) classification represents the lowest aggregation level of interest in the data. After encountering many issues with the merging process, like regions disappearing, the procedure was revised as follows.

Postcode Grid The starting point was to create a grid containing observations for every Local Authority (LAUA) in Great Britain for every possible time period (year-quarter-month). This was initially constructed over the period 1995 to 2007. This complete grid was then used as a *blueprint* onto which all the other data was merged.

D.3.1 National Statistics Postcode Directory.

The map between the postcode- and the regional-level is established in the NSPD. This provides a complete mapping of the UK geographies, from the full postcode to the national level. The advantage of using postcode districts instead of full postcode information is that there is relatively less variation in postcode district classifications over the observation period[3]. Non-geographical postcode data in the NSPD was dropped. These relate to postboxes and are used by direct mailing companies for re-routing mail (NSPD 2007).

An indicator was created, highlighting whether a postcode was live or terminated during the observation window. Over the period 01/01/1996 to 31/12/2005, roughly 20% of the full postcodes in the NSPD were terminated (see Table D.1). I did not drop terminated postcodes, as they are relevant for the merging scheme during the periods in which they were live. The full postcode coverage of the NSPD is detailed in Table D.2.

Table D.1: Number of live and terminated full postcode observations in the NSPD after conditioning on 01/01/1996 and dropping Northern Ireland.

live	Freq.	Percent	Cum.
terminated	427,383	19.98	19.98
live	1,711,536	80.02	100.00
Total	2,138,919	100.00	

[3] Looking at the NSPD full postcode data, there are some postcodes that were introduced after the beginning of 1996 and were subsequently terminated before November 2007.

Table D.2: Distribution of observations in the NSPD, by country.

live	Freq.	Percent	Cum.
ctry	Freq.	Percent	Cum.
Channel Islands	6,498	0.30	0.30
England	1,841,028	84.03	84.33
Isle of Man	5,485	0.25	84.58
Northern Ireland	51,979	2.37	86.95
Scotland	167,804	7.66	94.61
Wales	118,104	5.39	100.00
Total	2,190,898	100.00	

D.3.2 Overlapping Regions Problem

I am restricted to using postcode districts as regional identifiers, due to the ONS's censoring of full postcode information in most publicly available data sets. This introduces an *Overlapping Regions* issue, removing the one-to-one link between the postcode district and regional-levels, as postcode districts may fall into more than one Local Authority and one Local Authority may contain more than one postcode district[4]. This issue is not present at the full postcode level. There is also a lack of concordance between the Local Authority and level 1 Local Administrative Units (LAU1) ,former NUTS4, regional classifications in Scotland. In order to get around these issues, merging schemes are developed which allowed me to define a one-to-one link between postcode districts and the higher levels of aggregation of interest. This scheme also established a link across regions. The procedure is detailed below.

D.3.3 Distribution of observations:

In order to establish a one-to-one link between the relevant levels of aggregation, the first piece of information we wanted was to know was how many *unique* regions that postcode district falls into[5]. As a first step, a variable indicating how many *unique* postcode districts fall into each Local Authority/ NUTS3/

[4]This issue is present for the other levels of aggregation of interest as well. See Figure **??** for an illustration of the overlapping regions problem.

[5]See Table D.23

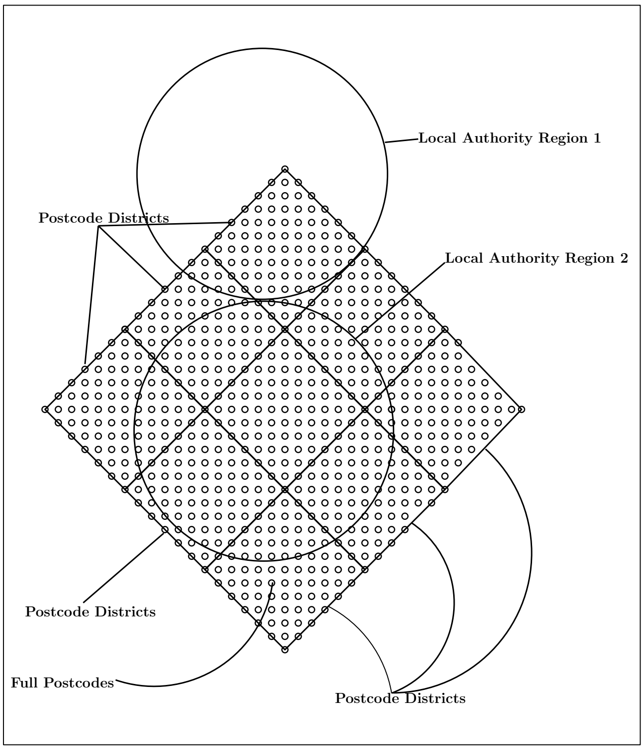


Figure D.1: *The Overlapping Regions problem.* - Squares represent postcode districts, and dots represent full postcodes.

LAU1 was generated in the NSPD. Since a many-to-many link exists between the different levels of aggregation, this identifier was generated by first collapsing the data of interest by the "postcode district-higher level of aggregation" link, generating the identifier, and then merging this information to the NSPD.

Table D.23 illustrates that most postcode districts in Great Britain fall into 2 or 3 higher aggregation levels. This pattern is the same when full postcodes are taken into account. Table D.24 illustrates the distribution of full postcodes falling into a postcode district that falls into T higher aggregation levels of interest. Most full postcodes in Great Britain seem to fall into postcode districts that fall into 1 to 4 higher aggregation levels. This is true at the local authority level and remains the case when aggregating up to the NUTS3 level.

An indicator, Uniq1, was generated to indicate the number of unique postcode districts falling into a higher aggregation level. The average Local Authority in Great Britain overlaps roughly 18 postcode districts. In case of level 1

Local Administrative Units (former NUTS4) this figure is roughly 17, whereas, on average, NUTS3 regions overlap 46 postcode districts. See Table D.3 for summary statistics of this indicator.

Table D.3: Distribution of UniqN: The number of unique postcode districts falling into higher aggregation levels of interest:

Variable	Obs	Mean	Std. Dev.	Min	Max
Uniq1	5366	18.018	13.295	2	87
Uniq3	5382	16.588	9.342	2	57
Uniq2	4131	46.339	24.465	3	109

Uniq1 # of unique postcode districts falling into Local Authorities.
Uniq2 # of unique postcode districts falling into NUTS3 regions.
Uniq3 # of unique postcode districts falling into level 1 Local Administrative Units (former NUTS4).

D.3.4 Merging Schemes

Two merging schemes were developed:

Scheme 1:

Higher levels of aggregation are ranked in terms of the number of postcode districts falling into them. Assign the postcode district in question to the area (local authority; NUTS3/4[lau1]) in which it falls that has **the least** number of postcode districts falling into it. The idea behind this is that areas with less postcode districts falling into them may contain a greater proportion of the district in question. Random assignment is implemented, in the event of a tie.

Scheme 2:

Assign the postcode district in question to the area (local authority; NUTS3/4[lau1]) in which it **falls the most**, based on the full postcode information in the NSPD. This algorithm creates a ranking of higher aggregation levels, in terms of the number of full postcodes within a postcode district that fall into each region. A postcode district is assigned to the area which ranks the highest on this scale. Random assignment is implemented, in the event of a tie.

Table D.4: Merging Scheme 1: Assignment of level 1 Local Administrative Units (LAU: former NUTS4):

pcd2	LAU	LAU Area	Uniq3	Ctry	Assigned LAU	Assigned LAU Area
AB25	UKM1001	Aberdeen City	17	179	UKM1001	Aberdeen City
AB3	UKM1002	Aberdeenshire	32	179	UKM1002	Aberdeenshire
AB30	UKM1002	Aberdeenshire	32	179	UKM2101	Angus
AB30	UKM2101	Angus	14	179	UKM2101	Angus
AB31	UKM1001	Aberdeen City	17	179	UKM1001	Aberdeen City
AB31	UKM1002	Aberdeenshire	32	179	UKM1001	Aberdeen City
AB32	UKM1002	Aberdeenshire	32	179	UKM1002	Aberdeenshire
AB33	UKM1002	Aberdeenshire	32	179	UKM1002	Aberdeenshire
AB34	UKM1002	Aberdeenshire	32	179	UKM1002	Aberdeenshire
AB35	UKM1002	Aberdeenshire	32	179	UKM1002	Aberdeenshire
AB36	UKM1002	Aberdeenshire	32	179	UKM1002	Aberdeenshire
AB37	UKM4202	Badenoch & Strathspey	9	179	UKM4203	West Moray
AB37	UKM4203	West Moray	7	179	UKM4203	West Moray
AB38	UKM1003	North East Moray	11	179	UKM4203	West Moray
AB38	UKM4203	West Moray	7	179	UKM4203	West Moray
AB39	UKM1002	Aberdeenshire	32	179	UKM1002	Aberdeenshire
AB4	UKM1003	North East Moray	11	179	UKM1003	North East Moray
AB41	UKM1002	Aberdeenshire	32	179	UKM1002	Aberdeenshire
AB42	UKM1002	Aberdeenshire	32	179	UKM1002	Aberdeenshire

Merging Scheme 1: Table D.4 illustrates an example of the assignment of postcode districts to the Local Administrative Units Level 1 (LAU1) level of aggregation. In addition to the lack of a one-to-one link between the postcode district and the local authority level, the other problem faced was establishing a link between the Local Authority level and the NUTS3 level of aggregation. A one-to-one link between the local authority level and level 1 Local Administrative Units (former NUTS4) exists in the case of England & Wales, however this is not the case for Scotland. By establishing a one-to-one link between postcode districts and higher aggregation levels, this also establishes a one-to-one link across regional classifications.

Looking at Table D.4, postcode district AB31 falls both into Aberdeen City and Aberdeenshire level 1 Local Administrative Units. However, 32 postcode districts fall into Aberdeenshire, whereas only 17 fall into Aberdeen City. Based on this information, merging scheme 1 assigns AB31 to Aberdeen City. Due to the first merging scheme no being based on full postcode information, this scheme has a bias towards assigning postcode districts at the boundary of the Local Authority/level 1 Local Administrative Unit(former NUTS4)/NUTS3 region to the smallest region in which it falls, regardless of the actual proportion of the postcode district that actually falls into that region. In the case of Local Authorities, larger regions with more postcode districts falling into them will tend to lose postcode districts on their boundaries to smaller neighbouring Local Authorities.

Table D.23 illustrate that the large majority of Local Authorities in Great Britain are a mixture of postcode districts falling into one to three unique Local Authorities with a significant proportion falling into just one (55%). This is also the case with other aggregation levels of interest, rising to 71% in the case of level 1 Local Administrative Units. This suggests this merging scheme may be vary in accuracy across these aggregation levels, given that the extent of regional overlap differs.

Merging Scheme 2: Given the problem of *Overlapping Regions*, the second merging scheme developed aims to assign postcode districts to the higher aggregation level into which they mostly fall. Table D.5 illustrates a simplified version of how merging scheme 2 operates. The first question to be addressed would be into which Local Authority does postcode district NG9 mostly fall.

Step 1 generates uniQ1 for each postcode district, an indicator for the number of unique full postcodes falling into each Local Authority that said postcode district falls into. Then, for each postcode district, Step two sorts these Lo-

Table D.5: Merging Scheme 2: Example of assignment of postcode districts to Local Authorities:

Full Postcode	Local Authority (LAUA)	Postcode District	uniQ1	VAR1	Assigned LAUA
NG9 1BB	00QA	NG9	3	3	00QA
NG9 2BC	00QA	NG9	3	3	00QA
NG9 1SG	00QB	NG9	1	3	00QA
NG9 2CD	00QA	NG9	3	3	00QA

uniQ1: # of full postcodes in postcode district NG9 that fall into Local Authority 00QA.

cal Authorities by this newly generated indicator, thus ranking them in terms of the number of full postcodes falling into them. This approach is a simple weighting scheme which is based on the premise that full postcodes are evenly dispersed across a postcode district, i.e. giving equal weight to each full postcode and not taking its population density into account. This allows us to make the further assumption that if more full postcodes within a postcode district fall into Local Authority A rather than B, then the postcode district in question mostly falls into former Local Authority. Steps 4 & 5 are based on this assumption. One issue with this scheme is that full postcode which overlap Local Authorities will be treated as falling into both Local Authorities making full postcode-based boundaries fuzzy.

D.4 Regional Identifiers In The Data

D.4.1 Levels of Aggregation in the regional data:

Travel-to-Work Areas

The goal is to attempt to capture exogenous variation between regional entities. Given this aim, Unitary Authority & Local Authority Districts could not be considered self-contained labour markets due to the impact of inward & outward commuting (Office for National Statistics 2008b) . Using residence-based denominators, e.g. ILO unemployment counts as a proportion of the residence-based (mid-year) working-age population, & local job density estimates, is likely to paint a more accurate picture of the local labour market (Office for National Statistics 2008b). However, the use of work-based denominators will bias downward estimates in an area with net in-commuting. The opposite is true in the analogous case (Office for National Statistics 2008b). To highlight this issue, consider calculation of the ILO unemployment rate for region j (U_j):

$$U_j = \frac{\sum_i^n U_{ij}}{\sum_i^n E_{ij} + \sum_i^n U_{ij}} \qquad \text{where i = number of individuals residing in region j.}$$

The unemployed residing in an area are likely to have very different characteristics to those working in the same area, especially at longer unemployment durations[6]. The degree of this mismatch is likely to increase due the impact of commuting. in the case of net inward commuting, this will inflate the figure for the number of employees in an area, causing the overall statistic to be under-estimated (Thomas 1997; 1998; 2005). This statistic is only suitable for larger areas, in which the impact of commuting is reduced to a minimum. Travel-to-Work Areas (TTWAs) were introduced as areas which approximate self-contained labour markets, however, they are not without their problems.

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 2008a). The resulting pattern is that, although the definitive minimum working population in a TTWA is 3,500, many are much larger - indeed, the whole of London and surrounding area forms one TTWA.

A trend-reduction in the number of TTWAs can be observed, as the trend in more and longer distance commuting increases: in 1991 there were 314 TTWAs

[6]The distinction between structural versus frictional unemployment is likely to be important in this case.

and in 1981, 334. As TTWAs become larger, on the one hand, they become more representative of self-contained labour markets, however, they also become increasingly inappropriate as units for the measurement of unemployment as unemployment is a local phenomenon and large area statistics tend to give a distort view of the unemployment problem by smoothing out concentrations (Thomas 1997). Despite these drawbacks, we include regional identifiers at this level of aggregation as this measure provides the closest approximation available to self-contained labour markets (Petrongolo 2001).

The link between the Local Authority level and Travel-To-Work Area level of aggregation is established in the National Statistics Postcode Directory. Again, this is not a 1-to-1 link. This link was established using information from GeoConvert (UKBORDERS). This information tells us the proportion of a local authority that falls into a TTWA. Using this information, merging scheme 2 was implemented in order to establish a one-to-one link.

Alternative indicators in the data include NUTS3 regions as well as the 1999 Unitary Authorities and Local Authority Districts (UALAD99) classification of regions.

D.5 The Regional Data.

D.5.1 Self-constructed variables:

University Indicator:

Information on Higher Education institution locations was sourced from the Higher Education Statistics Authority (HESA)[7]. Using this information institutional data was matched to the relevant postcode districts. Unfortunately, the HESA only hold data on the location of Higher Education Institutions' administrative centres, rather than the location of campuses. See Figure D.2 for the distribution of 167 Administrative Centres across Great Britain.

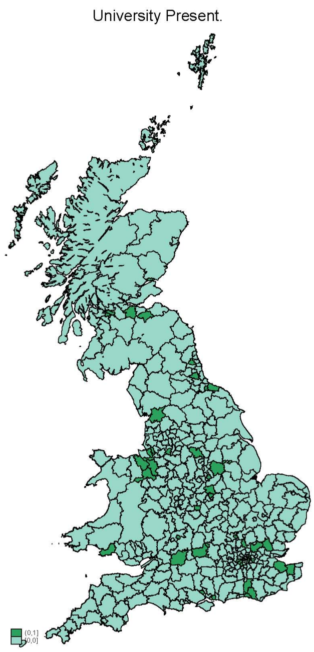
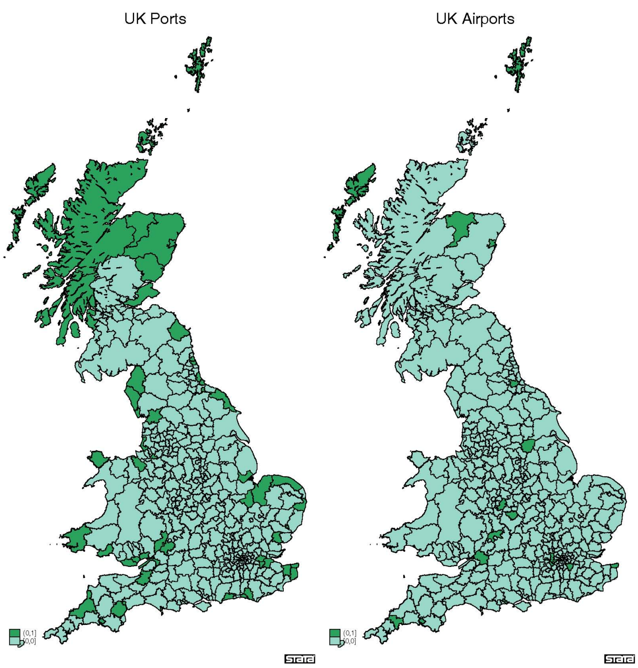


Figure D.2: University Present?. *Created using 'spmaps' (Pisati 2007).*

[7]Information on the location of the 167 institutions in the UK is available at http://www.hesa.ac.uk/index.php/component/option,com_heicontacts/Itemid87/

Ports/Airports Indicator:

Data for this indicator was sourced from the port directory available from the Association of Port Health Authorities website (Association of Port Health Authorities 2007). Three versions of this indicator were developed: two separate port and airport indicators, as well as an indicator grouping ports and airports, at the Local Authority & NUTS3 level of aggregation. Three Royal Navy Ports were dropped from the data, however Great British ports for commercial use were retained. The distribution of ports and airports in Great Britain is shown in Figures D.3a & D.3b. A list of UK airports can be found from the Royal Aeronautical Society's website (Royal Aeronautical Society 2005). This list was used to check for consistency of the existing data.



(a) Port in Local Authority? (b) Airport in Local Authority?

Figure D.3: GB Ports & Airports. *Created using 'spmaps' (Pisati 2007).*

Urban/Rural Indicator:

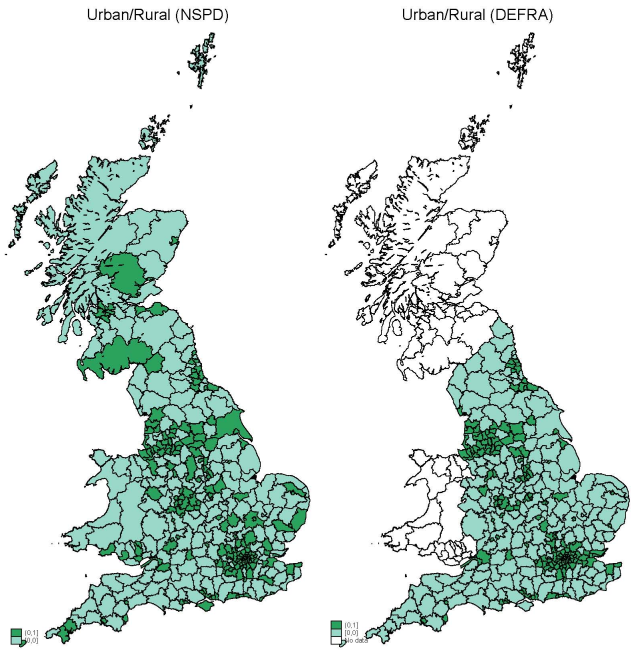
Two versions of this indicator were initially sourced: One from the National Statistics Postcode Directory (NSPD) and one from the Department of Environment, Food & Rural Affairs (DEFRA). A third measure was constructed, which combined these two measures.

1: NSPD version: For England and Wales this population density-based indicator was sourced from the 21st of July 2004 release of the National Statistics Rural & Urban Classification of Output Areas (NSPD 2007), and thus not valid for higher levels of aggregation which may include a mixture of rural & urban output areas based on the definitions used. See Table D.20 Column 1 for a breakdown of this output-based classification for England & Wales. For Scotland, areas with $<$ 3000 inhabitants are defined as rural (NSPD 2007). The distribution of this variable at the Local Authority level is shown in Figure D.4a. The DEFRA methodology as the benchmark against which other Local Authority-level Urban/Rural definitions should be measured.

2: DEFRA version: The DEFRA Rural-Urban indicator[8] was introduced in 2005 and covers the England local authority geography only. See Table D.19 Column 2 for a breakdown of this output-based classification. The distribution of this variable is shown in Figure D.4b.

3: Constructed Measure: The additional constructed measure joins the 2 approaches, using the DEFRA methodology for England. This implies some

[8] For more information see http://www.defra.gov.uk/rural/ruralstats/rural-definition.htm



(a) NSPD (b) DEFRA

Figure D.4: How Urban is a Local Authority? *Created using 'spmaps' (Pisati 2007).*

measurement error for Scotland & Wales, an issue acknowledged and an issue for which robustness checks were constructed using alternative indicators[9].

URBAN/RURAL INDICATOR ISSUES

Table D.6 presents the overall UK statistics from the different approaches. We define 'howurban' as the sum of urban output areas as a proportion of the total number of output areas in a Local Authority. The Output Area-based Urban/Rural measure, from the NSPD, paints a distorted picture of how urban the UK geography is. This is more evident when broken down by country.

The Urban/Rural classification based on the DEFRA methodology, defines 60% of local authorities in England as Urban, whereas the NSPD-based measure - based on output area classifications, defines 77% of local authorities in England as Urban (see Figure D.4 for the distribution of these variables).

[9]Similar strategies can be easily implemented using the threshold-based indicators developed in the subsequent section as a substitute for the NSPD-base measure.

Table D.6: Comparison between the different approaches: how urban; NSPD & DEFRA Urban/Rural classifications for Scotland, England & Wales.

Variable	Obs	Mean	Std. Dev.	Min	Max
howurban	2363433	.7646	.261	0	1
Urban(NSPD)	2363433	.740	.439	0	1
Urban(DEFRA)	2020682	.601	.490	0	1

howurban: sum urban output areas/number output areas
Urban (NSPD): Statistic calculated at Output Area level
Urban (DEFRA): Statistic calculated at Local Authority level

Table D.7: Comparison of how urban Local Authorities in Scotland, England & Wales are.

Variable	Obs	Mean	Std. Dev.	Min	Max
England					
howurban	2020682	.787	.250	0	1
Scotland					
howurban	210984	.642	.299	0	.995
Wales					
howurban	131767	.640	.279	.112	.972

howurban: sum urban output areas/number output areas

As well documented on the DEFRA website (DEFRA 2007), local authorities borders may encapsulate a mixture of urban and rural output areas. Thus, aggregating this data to the local authority level presents us with an issue.

DEFRA developed their methodology due to these concerns, however, it only covers England. Table D.20 highlights the differences in the methodologies. The statistics in table D.6 show that, on average over Great Britain, 76.5% of output areas falling into a local authority are classified as Urban. This figure varies markedly across Great Britain. When we break this down by country, this figure is 78.7% in England, 64.2% for Scotland & 64.0% for Wales.

Table D.8: Comparison of how urban Local Authorities in Scotland, England & Wales are.

Variable	Obs	Mean	Std. Dev.	Min	Max
England					
Urban	2020682	.770	.421	0	1
UrbanDefra	2020682	.601	.490	0	1
Scotland					
Urban	210984	.530	.499	0	1
Wales					
Urban	131767	.612	.487	0	1

howurban: sum urban output areas/number output areas

On average, the output area-based NSPD methodology classifies 74% of output areas in Great Britain as urban. As stated, the DEFRA local authority-based measure classifies 60% of English Local Authorities as urban. This aggregate figure masks the variation in this NSPD-based indicator across Great Britain. The statistics in Table D.8 demonstrate that the highest proportion of Urban areas lie in England (77%) with 61% of output areas classified as Urban in Wales. This number is as low as 53% in Scotland. This is the benchmark against which more aggregated statistics should be measured, when considering Great Britain as a whole.

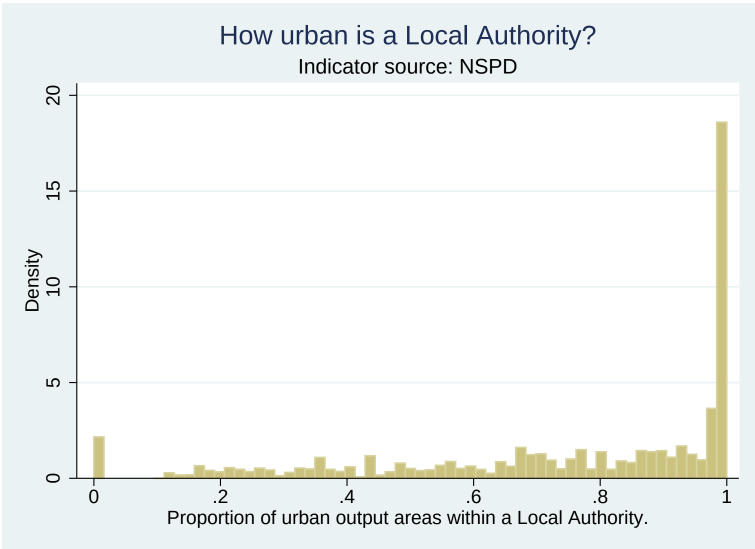


Figure D.5: How Urban is a Local Authority?

Figure D.5 illustrates the overall distribution of the 'howurban' variable detailed in Table D.7. This distribution is skewed to the right at 1, with a long left tail and a relatively isolated mass point at zero. Based on this information, the idea was to look at the proportion of urban Output Areas falling into a Local Authority in order to decide how to classify a Local Authority (Urban/Rural). This information is captured by the 'howurban' variable. The same idea was implemented in the case of Accessibility. Since we already have a reliable indicator for England at the Local Authority-level (DEFRA methodology), we only need to conduct this exercise for Scotland and Wales. We develop thresholds above which a Local Authority is classified as Urban. These thresholds are defined in Tables D.9 & D.10.

Table D.9: Local Authority Urban/Rural Indicator: Scotland & Wales

Variable	Obs	Mean	Std. Dev.	Min	Max
Scotland & Wales					
>50%	342751	0.671	0.470	0	1
>60%	342751	0.643	0.479	0	1
>70%	342751	0.507	0.500	0	1
>80%	342751	0.415	0.493	0	1
>90%	342751	0.235	0.424	0	1

Proportion of Output Areas in Local Authority that are NSPD Urban

Table D.10: Local Authority Urban/Rural Indicator: Scotland & Wales

Variable	Obs	Mean	Std. Dev.	Min	Max
Scotland:					
>50%	210984	0.667	0.471	0	1
>60%	210984	0.643	0.479	0	1
>70%	210984	0.487	0.500	0	1
>80%	210984	0.441	0.497	0	1
>90%	210984	0.302	0.459	0	1
Wales:					
>50%	131767	0.677	0.468	0	1
>60%	131767	0.641	0.480	0	1
>70%	131767	0.537	0.499	0	1
>80%	131767	0.373	0.484	0	1
>90%	131767	0.128	0.334	0	1

Proportion of Output Areas in Local Authority that are NSPD Urban

Remoteness/Accessibility:

England & Wales In this case the population density of the surrounding area was used as a measure of whether a local authority was accessible or remote in the case of England and Wales[10]. See Table D.21 for how this indicator was constructed.

Scotland Driving distance to the nearest large settlement (>10000 inhabitants) is used as a proxy in the case of Scotland. Areas more than 30 minutes driving distance from an urban centre of >10000 residents were classified as rural. See Table D.21 for how this indicator was constructed.

[10]The NSPD defines this indicator at the Output area level of aggregation, which suggests that aggregation issues could be present when aggregating to Local Authority level. The accessibility of Output Areas is based on their surrounding geography, "whether the wider surrounding area of a given output area is sparsely populated or less sparsely populated (NSPD 2007, pp.17)".

Accessibility Indicator Issues

It was felt that the Accessibility indicator did not give enough variation to accord with intuition about the UK geography. Alternative measures of this indicator were adopted, using an approach similar to that used for the Urban/Rural indicator. The sum of accessible Output Areas as a proportion of the number of Output Areas (OAs) within a Local Authority (LA) was used as a measure of how accessible Great British Local Authorities are. This measure was based on the National Statistics Postcode Directory data. The distribution of this indicator is detailed in Table D.11.

Table D.11: How Accessible? (% Urban Output Areas *within a Local Authority*: Great Britain

Variable	**Obs**	**Mean**	**Std. Dev.**	**Min**	**Max**
England.					
How Accessible?	2020682	0.983	0.080	0.034	1
Scotland.					
How Accessible?	210984	0.858	0.213	0	0.998
Wales.					
How Accessible?	131767	0.860	0.255	0.190	1

NB. How Accessible?: $\sum$(Accessible Output Areas/# Output Areas).

The summary statistics in Table D.11 raise concerns about our original definition of Accessibility. On the one hand this definition may be erroneous, whilst on the other, it may be that Local Authorities across Great Britain are truly very Accessible. More variation in this measure would be more desirable, thus alternative definitions were constructed.

Table D.12: Thresholds Accessibility Criterion: Great Britain

Variable	Obs	Mean	Std. Dev.	Min	Max
>50%	2430314	0.951	0.216	0	1
>60%	2430314	0.947	0.223	0	1
>70%	2430314	0.935	0.247	0	1
>80%	2430314	0.905	0.293	0	1
>90%	2430314	0.891	0.312	0	1

Alternative definitions of Accessibility were constructed using differing accessibility criteria. Five thresholds were initially established: >50%; >60%; >70%; >80%; and >90%. Summary statistics for these thresholds are detailed for Great Britain in Table D.12, as well as by country in Table D.13. Table

D.12 highlights the lack of large variation in this statistic at the aggregate level. Whilst there is not a lot of variation in these summary statistics for England (only 3.7% difference between the >50% & >90% criterion), they varied markedly in the case of Scotland & Wales. There is a 27.4% difference between the >50% & >90% criterion in Scotland, whereas the difference between the >50% & >90% criterion in Wales is 9.9%.

Table D.13: Thresholds for Accessibility Criterion: Country-level

Variable	Obs	Mean	Std. Dev.	Min	Max
England:					
>50%	2020682	0.992	0.090	0	1
>60%	2020682	0.990	0.097	0	1
>70%	2020682	0.982	0.133	0	1
>80%	2020682	0.965	0.183	0	1
>90%	2020682	0.955	0.207	0	1
Scotland:					
>50%	210984	0.921	0.270	0	1
>60%	210984	0.921	0.270	0	1
>70%	210984	0.855	0.352	0	1
>80%	210984	0.712	0.453	0	1
>90%	210984	0.647	0.478	0	1
Wales:					
>50%	131767	0.852	0.355	0	1
>60%	131767	0.810	0.392	0	1
>70%	131767	0.810	0.392	0	1
>80%	131767	0.753	0.431	0	1
>90%	131767	0.753	0.431	0	1

Stricter criterion were also implemented, using the following thresholds: >95%; >96%; >97%; >98%; and >99% (see Table D.14). Again, for England the summary statistics did not vary much when conditioned on these tougher hurdles. There is only a 2.3% difference between the >95% & >99% measures, with 92.3% of at least 99% of the Output Areas falling into English Local Authorities being classified by the NSPD as Accessible according to our measure. Furthermore,this measure did not vary much for Wales (A 3.1% difference between the >95% & >99% measures). This measure seems quite stable at high thresholds for England & Wales, suggesting that Local Authorities classified as urban at high moments of the distribution of these indicators possess similar characteristics in terms of the number of urban OAs falling into them.

There is not a lot of variation at the top of the distribution in England. In the case of Scotland, we see a very large variation in this indicator when using these strict accessibility criterion. There is a 60.7% difference between the

>95% & >99% measure. The difference between the >95% & >96% thresholds is 0%, whilst the difference between the >96% & >97% thresholds is 4.6%. These differences increase exponentially. The difference between the >97% & >98% thresholds is 17.4%, whilst the difference between the >98% & >99% thresholds is 29.5%! These observations suggest that the overall distribution in Figure D.5 is mostly driven by England which accords with intuition.

Table D.14: Strict Accessibility Criterion: Country-level

Variable	Obs	Mean	Std. Dev.	Min	Max
England:					
>95%	2020682	0.946	0.226	0	1
>96%	2020682	0.936	0.244	0	1
>97%	2020682	0.931	0.254	0	1
>98%	2020682	0.929	0.256	0	1
>99%	2020682	0.923	0.266	0	1
Scotland:					
>95%	210984	0.608	0.488	0 1	
>96%	210984	0.608	0.488	0	1
>97%	210984	0.562	0.496	0	1
>98%	210984	0.388	0.487	0	1
>99%	210984	0.093	0.290	0	1
Wales:					
>95%	131767	0.717	0.450	0	1
>96%	131767	0.717	0.450	0	1
>97%	131767	0.686	0.464	0	1
>98%	131767	0.686	0.464	0	1
>99%	131767	0.686	0.464	0	1

D.5.2 Local Area Quarterly Labour Force Survey (QLFS)

Background.

The Local Area Quarterly Labour Force Survey dates back to 1992q1, and includes roughly 100 variables covering the following subjects: employment by age group; employees; self-employed; economic activity; employment by industrial sector; ethnic minority economic activity; persons in full-time education; qualifications; job-related training (Government 2008). For confidentiality reasons, local area data available on the UK Data Archive website at the Local Authority Unitary Authority (LAUA) level of aggregation has been suppressed by the Office of National Statistics (ONS) (Government 2008). This restricted one to the Local Area Quarterly Labour Force Survey as a widely available source of information.

The local area data available via the UK Data Archive's standard end user license is formatted according to the Local Area District (LAD) classification. Since the existing dataset has been constructed according to the Local Authority & Unitary Authority (LAUA) classification, the first challenge was to develop a concordance between the LAD and LAUA methodologies. This was not very obvious given the lack of clear documentation, or a concordance table. The LFS estimates for LADs are based on 1981 boundaries, implying that boundary changes since 1991 will not be accounted for in the data (Labour Force Survey 2006).

The quarterly Labour Force Survey (LFS) is a representative survey based on some 60,000 households (Government 2008), with a single LFS quarter representing roughly 150,000 individuals. However, when interest is in small population sub-groups, or smaller areas, the quarterly LFS is fairly limited as a source of reliable estimates given the small sample sizes (Labour Force Survey 2003). The LFS documentation suggests that an average of a larger sample over a longer period will improve the accuracy of estimates as well as smoothing out seasonal variation (Labour Force Survey 2003).

Given the small sample size at lower levels of aggregation, the LFS adopts the following rules:

- the base population for each area is rounded to the nearest thousand; and
- any proportion based on an estimate of less than 10,000 is suppressed(Labour Force Survey 2003).

These rules imply that the data pertaining to the City of London & Isle of Scilly Local Authorities are generally suppressed - censored at zero - in the published data. In the case of the City of London, many sample sizes are considered too small to provide reliable estimates; In the case of the Isle of Scilly, this geography is not sampled due to its remote location and small population. Furthermore, since the LFS is assumed a representative sample, individual responses are weighted to reflect the distributions of the relevant aggregate statistics. If less than 2 individuals replied to the survey in an area, this information is considered disclosive and dropped for confidentiality reasons (Government Statistical Service 1999). This is likely to be an issue the less disaggregate the level of analysis.

The quarterly LFS is a survey, and thus subject to issues like *Non-Response* which affect survey accuracy. Given the rotating nature of the QLFS, following individuals for 5 quarters, it is also subject to sampling variability implying that comparability over time is affected. Since interest lies in broad band regional characteristics, these issues are less of a concern.

Concordance between Local Area Data Classifications.

During the major Local Government reorganisation during the 1990s, single-tier unitary authorities were established in urban areas, with responsibility for all areas of local government. The existing 2-tier system of counties and non-metropolitan districts, established in the 70s, remained for the rest of the country. The result is a mixture of single-tier and two-tier administrative structures at the local level. This phase of restructuring occurred between 1995 and 1998 (see National Statistics 2004). In April 1996 the counties of Avon, Cleveland, and Humberside, their districts, and the district of York City were abolished, and 13 unitary authorities were created in their place. In 1997 13 further unitary authorities were established, and 19 in 1998, making a total of 46 unitary authorities in England, in addition to the existing London and Metropolitan boroughs, which already had unitary powers. For full details of these changes, see Office for National Statistics (1999).

Given these changes in local area classifications, more than one concordance system was needed to link the regional identifiers over the time period of interest. The concordance system developed is detailed in Ball & Wilke (2009).

One system was established for the 1996q1-1998q4 data and another for the 1999q1+ data. After 1999q1, 13 extra unitary authorities were introduced into the data implying a discontinuity with respects to these regional classifications. Where possible, these were matched to previous years by district name[11].

The match between the post-1998 local area geography and the LAUA classification is an improvement over the pre-1999 geography. The matching scheme developed (see Ball & Wilke 2009) was applied to the UK Data Archive Local Area LFS, period: 1996q1 - 2006q1. This resulted in roughly 408 Local Authority matches, which varied across the years. Before 1998q4 there were 378 Local Authority matches, as it seems that the newly introduced Unitary Authorities were not accounted for in the pre-1999 waves of the quarterly survey.

Harmonization of the Waves

Small variable name changes over the waves of the Local Area QLFS led to the implementation of a standard system for variable labelling. Furthermore, there were cases in which regional names or codes were missing. Given the concordance system developed, this was a simple case of imputing the missing value. In other instances, districts wrongly coded. For example, in 1998q1 Norwich and North Norfolk we assigned each other's district codes instead of their own. This was a fairly arbitrary task, given the information in our concordance tables. In order to minimise problems when it came to matching the waves, a template of all possible local area regions was merged to all waves of interest.

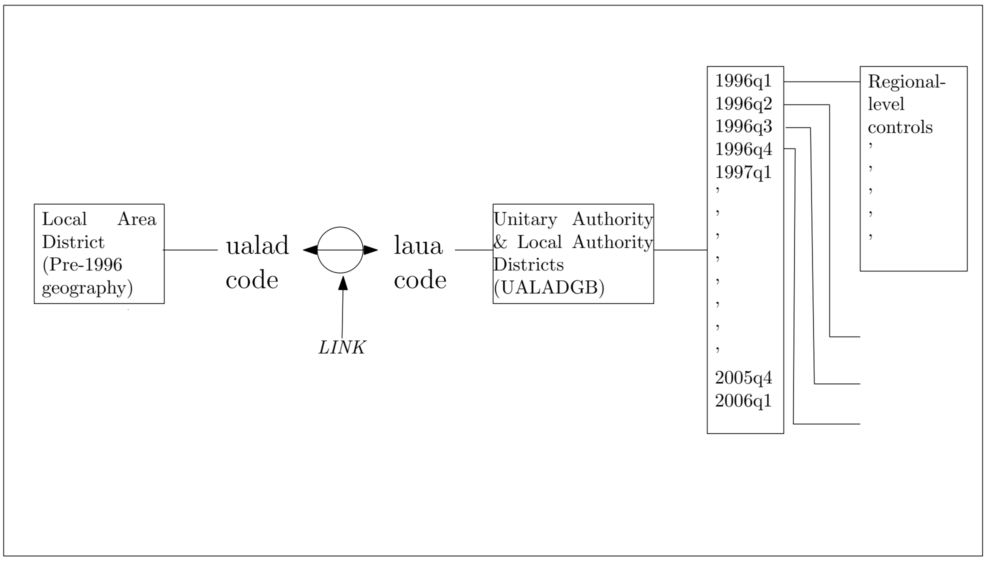
The pre-1999 waves include occupational information according to the 1990 Standard Occupational Classification, whereas the post-1998 occupation data is only available according to the 2000 methodology (SOC2000). Given an in-

[11] Matching by district name would imply some inaccuracies in this procedure pertaining to Local Authority Districts that were split to form a Unitary Authority and a Local Authority.

herent lack of comparability(Beerten *et al.* 2001), an attempt to match the two methodologies is not advised as the composition of 1-digit occupational groups has changed and detailed occupational data is unavailable. These compositional changes are within broadband occupational groups. However, a weak concordance is developed between the SOC90 and SOC2000 for the purposes of linking the pre- and post-1999q1 waves (see Table D.22). Regardless, this issue does not affect the analysis in Ball & Wilke (2009) given the time period under observation: 1999q1 - 2005q4.

New variables introduced over period: Figure D.6 illustrates the data structure aimed for. In this form, the raw Local Area QLFS presents one with 122 unique variables over the period of interest, 5 labels and 107 unique left hand side variables.

Figure D.6: Data Structure of the QLFS.



The number of variables did not change between 1996q1 & 1998q4, and there was no large changes in the magnitudes of the variables over these quarters. Since no labels and codes were provided between 1996q1 & 1997q4, it was assumed that the variable labels didn't change over this period. In order to ease the merging process, it was desirable for all periods to have the same formatting, layout, variables & variable names.

Table D.15: New variables

Time period	# Vars.	New Regressors Introduced
1996q1 - 1998q4	106	
1999q1 - 2001q1	107	ea18trt4 "pers. econ. active aged 18-retirement with nvq 4 or above" ea18trt3 "pers. econ. active aged 18-retirement with nvq 3 or above"
2001q2 - 2002q2	108	ea18trt "persons economically active aged 18-retirement" **NB.** alempuo "all in employment working in unskilled occupations", systematically missing from 2002q2 onwards.
2002q3	109	
2004q4	107	
2005q1 - 2006q1	109	

The waves from 1996q1 to 1998q4 contained 102 variables: 2 labels (uk; ualad) and 104 regressors. The Local Authority (LAUA) was matched in using the concordance table developed (see Ball & Wilke 2009 for details). An additional column was also added, indicating the quarter in which the wave occurred. Columns were added for the following variables, not included in these waves: ea18trt4 (persons economically active aged 18 to retirement, with NVQ level 3+) and ea18trt (persons economically active aged 18 to retirement, with NVQ level 4+) which were both introduced in 1999q1; and ea18trt (persons economically active aged 18 to retirement) which was introduced in 2001q2. In total this gave 112 variables.

The waves from 1999q1 to 2001q1 contained 107 variables: 1 label (uk) and 106 regressors. The Local Area District (pre-1996 geography) codes, and Local Authority (LAUA) codes and area names, were matched in using the developed concordance scheme. An additional column was also added, indicating the quarter in which the wave occurred. An additional Column was added for the following variable, not included in these waves: ea18trt (persons economically active aged 18-retirement, with NVQ level 4+) which were both introduced in 1999q1; and ea18trt (persons economically active aged 18-retirement) which was introduced in 2001q2. In total this gave 112 variables.

The waves from 2001q2 to 2002q2 contained 108 variables: 1 label (uk) and 107 regressors. The 'person33' variable was renamed to 'ea18trt' as they had the same definition. The Local Area District (pre-1996 geography) codes, and Local Authority (LAUA) codes and area names, were matched in using the concordance scheme developed (see Ball & Wilke 2009 for more detais). An additional column was also added, indicating the quarter in which the wave occurred. In total this gave 112 variables.

The 2002q4 wave contained 107 variables: 1 label (uk) and 106 regressors. The Local Area District (pre-1996 geography) codes, and Local Authority (LAUA) codes and area names, were matched in using the concordance scheme developed (see Ball & Wilke 2009 for more detais). An additional column was also added, indicating the quarter in which the wave occurred. The variable indicating the number of workers in the 'Unskilled' occupational category, 'alempuo', was missing from the dataset for this wave. This variable was added with values set to missing. Since this variable was present but contained missing values from 2001q2 to 2006q1, this increased confidence that this strategy was appropriate for the 2002q4 wave. In total this gave 112 variables.

2002q3 and the waves from 2003q1 to 2006q1 contained 109 variables: 2 labels (uk; code/ualad) and 107 regressors. The Local Area District (pre-1996 geography) codes, and Local Authority (LAUA) codes and area names, were matched in using the aforementioned concordance scheme. An additional column was also added, indicating the quarter in which the wave occurred. In total this gave 112 variables.

Matching waves

Identifiers used:

- **uk** - Local Area Districts (pre-1996 geography).

- **ualad** - Local Area District codes (pre-1996 geography).
- **area** - Local Authority Areas (UKLADGB).
- **laua** - Local Authority Unitary Authority codes.
- **quarter** - Quarter in which the wave occured.

Given this common imposed underlying structure, it was a simple case of converting the 1996q1 to 2006q1 waves from wide to long format, and then stacking the datasets on top of each other using the 'append' command in Stata 10.0.

Variable Selection

Table D.16 indicates the quality of the variables utilised. This indicator is calculated as the total number of missing observations as a fraction of the total number of observations. Breakdowns of the ILO unemployment rate by age are of some concern, as their quality is quite low. When compared to the NOMIS versions of these variables, available from the quarterly labour force survey (4 quarter averages), these statistics are relatively favourable. Before these variables were used, an imputation strategy was implemented that replaced missing values with the values in the preceding quarter. This approach is valid if one assumes that these observations are randomly missing. However, it is hard to justify this approach in the cases where the % of missing values was high (Greater than 5%: (see Cameron & Trivedi 2005, chap. 26)).

The variables in Table D.16 are relevant as base variables for the construction of other indicators. The indicators constructed include: The fraction of New Deal Starters in the *eligible* population. This indicator was constructed for targets of the 18-24 as well as the 25-49 programmes. Two definitions of the numerator were used for this variable:

Table D.16: Quality of Variables in the Local Area QLFS (1995q1-2006q1)

	1999q1-2005q4		1995q1-2006q1	
Variable	Obs	% Missing	Obs	% Missing
ethmin	16368	0.222	11424	0.209
pworkage	16368	0.003	11424	0.003
resph16	16368	0.003	11424	0.003
alemmani	16368	0.006	11424	0.006
inemp16	16368	0.003	11424	0.003
alemanso	16368	0.004	11424	0.004
alemproo	16368	0.008	11424	0.007
alemptoc	16368	0.006	11424	0.005
alemasoc	16368	0.006	11424	0.006
alemstoc	16368	0.006	11424	0.006
ilo16t19	16368	0.325	11424	0.336
ilo20t24	16368	0.429	11424	0.461
ilo25t34	16368	0.323	11424	0.362
ilo35t49	16368	0.25	11424	0.28
ilou16	16368	0.035	11424	0.042

- 18-24 ILO unemployed population (residence-based).
- 18-24 Claimant Count (Claiming for $\succeq$ 6 months).

Using definition 1 is likely to bias downwards results as not all ILO unemployed are eligible. Eligibility requires receipt of Jobseeker's Allowance (JSA) for 6 months (McVicar & Podivinsky 2003). Furthermore, the Claimant Count-based denominator is relatively more attractive given that it is not affected by missing values and the low quality of the ILO-based alternative.

Lack of an average years of schooling indicator led to the use Skill Intensity as a proxy. This occupation-based indicator was defined as the fraction of people in the working population working in the following occupations: Managers & Senior Officials; Professionals; Associated Professionals & Technical; Admin. & Secretarial; & Skilled Trades.

Other indicators developed from the Local Area QLFS include: manufacturing industry employement as a proportion of total employment; the fraction of the working population with qualifications at NVQ level 3 and over; the fraction of the working population with qualifications at NVQ level 4 and over; and the fraction of Ethnic Minorities in the total population (aged 16+).

The initial motive for using the Local Area QLFS was to capture ILO unemployment. Four rates were constructed: the fraction of ILO unemployed in the total population (mid-year estimate from NOMIS); the fraction of total working-age population (mid-year estimate from NOMIS); the fraction of all aged 16+ (QLFS Local Area data); and the fraction of all working-age population (QLFS Local Area data).

Imputation Strategy

Imputation makes sense if it is reasonable to assume that the missing observations are missing at random. However, it is hard to justify this approach in the cases where the % of missing values was high (Greater than 5%: (see Cameron & Trivedi 2005, chap. 26)).

Table D.17 highlights the underlying data problems for the City of London local authority. This extract suggests that variables 'ilo16t19' is systematically 0, possibly due to low number of respondents, and can be assumed to be zero. However, the pattern of missing values for the other variables in Table D.17 suggest a case of missing values.

Table D.17: Imputation issues: Local Area Quarterly Labour Force Survey

Code	Area	month	resph16	inemp16	pworkage	ilo16t19	ilo20t24
00AA	City of London	2004q3	9763	5894	6586	0	0
00AA	City of London	2004q4	7151	3423	4115	0	0
00AA	City of London	2005q1	9486	3697	6543	0	697
00AA	City of London	2005q2	11809	5493	8113	0	875
00AA	City of London	2005q3	9329	4576	5377	0	0
00AA	City of London	2005q4	11066	4877	6430	0	0
00AA	City of London	2006q1	8838	3960	5477	0	0

In Table D.17 variable 'ilo20t24' seems to be missing for 2004q3 to 2004q4 & 2005q3 to 2006q1 in City of London Local Authority. However, a change in magnitude from zero doesn't seem very realistic given its value of 875 in 2005q2. This is also the case with the variables in Table D.18 for Rochdale Local Authority. It is well documented that data for the City of London and Isle of Scilly are affected by small sample sizes. This implies that censoring of the data for these sub-regions for confidentiality reasons will be common.

However, it is hard to see how this is the case in the above illustrated cases. The pattern is the same throughout the dataset. This issue is not well documented, since the documentation provided refers to the quarterly labour force survey and the annual local area QLFS which are both going to have larger sample sizes and thus higher thresholds (in terms of number of individual responses required to avoid data censorship).

Communications with the LFS helpdesk, as well as ONS, have so far come to the conclusion that the "0" values in the LAQLFS are actually zero. However, this is hard to believe in some cases and thus the issue is still being pursued. One example of an issue variable would be ethnic minority counts in Local Authorities. It may be that the high level of entries coded as zero (and subsequently treated as missing under the initial methodology) are truly zero, given relatively the thin spread of ethnic minorities across the UK. The current implementation of the dataset treats these zero values as missing and imputes accordingly (replacing missing values with the value in the preceding period).

Table D.18: Imputation issues: Local Area Quarterly Labour Force Survey

Code	Area	month	resph16	inemp16	pworkage	ilo16t19	ilo20t24
00BQ	Rochdale	1997q4	168279	97545	133421	415	1475
00BQ	Rochdale	1998q1	172178	100473	137632	843	0
00BQ	Rochdale	1998q2	168914	94965	135611	2777	1460
00BQ	Rochdale	1998q3	173521	103829	141716	950	1463
00BQ	Rochdale	1998q4	167991	102250	133746	480	0
00BQ	Rochdale	1999q1	164732	99468	130745	849	485
00BQ	Rochdale	1999q2	161366	91266	125380	1196	481

Linking the Local Area Quarterly Labour Force Survey

The matched Local Area QLFS waves were matched to the existing Local Authority-level regional dataset, on a monthly basis. In order to achieve this, the matched waves were merged with a grid containing all possible Local Authorities on a yearly, quarterly, and monthly basis. This merger resulted in quarterly LFS waves being repeated for the relevant months within the 4 month interval.

D.5.3 Data construction and definitions

Table D.19: NSPD Urban/Rural classification for Scotland.

No.	**NSPD Area Classification**	Definition
1	Large Urban Area:	Population > 125,000
2	Other Urban Area:	Population 10,000-125,000
3	Accessible Small Town:	Population 3,000-10,000, <= 30 minutes drive to settlement of 10,000+
4	Remote Small Town:	Population 3,000-10,000, 30-60 minutes drive to settlement of 10,000+
5	Very Remote Small Town:	Population 3,000-10,000, > 60 minutes drive to settlement of 10,000+
6	Accessible Rural:	Population < 3,000, <= 30 minutes drive to settlement of 10,000+
7	Remote Rural:	Population < 3,000, 30-60 minutes drive to settlement of 10,000+
8	Very Remote Rural:	Population < 3,000, > 60 minutes drive to settlement of 10,000+

Classification: 1,2 = Urban; 3-8 = Rural;
Source: National Statistics Postcode Directory (NSPD)

Table D.20: Comparison between the NSPD and DEFRA Urban/Rural classifications for England.

No.	**NSPD Classification: (England/Wales)**	No.	**DEFRA Classification (England)**
1	Urban (Sparse) population > 10,000	1	Major Urban: population > 100,000 or 50% of population in urban areas with population > 750,000.
2	Urban (Less Sparse) > 10,000	2	Large Urban: population > 500,000 or 50% of population in one of 17 urban areas with population between 250,000 & 750,000.
3	Town (Less Sparse)	3	Other Urban: population < 37,000 or < 26% of population in rural settlements & larger market towns.
4	Town (Sparse)	4	Significant Rural: population > 37,000 or > 26% of population in rural settlements & larger market towns.
5	Village (Less Sparse)	5	Rural-50: population ≥ 50% but < 80% of population in rural settlements & larger market towns.
6	Village (Sparse)	6	Rural-80: population ≥ 80% of population in rural settlements & larger market towns.
7	Dispersed: hamlets & isolated dwellings (Less Sparse)		
8	Dispersed: hamlets & isolated dwellings (Sparse)		

NSPD Classification: 1,2 = Urban; 3,4,5,6,7,8 = Rural
DEFRA Classification: 1,2,3 = Urban; 4,5,6 = Rural;
Source: Department of Environment, Food and Rural Affairs (DEFRA), www.defra.org.uk
National Statistics Postcode Directory (NSPD)

Table D.21: Comparison between the England/Wales & Scottish Accessibility indicators.

No.	**England & Wales**	No.	**Scotland**
1	Urban (Sparse) population > 10,000	1	Large Urban Area: Population > 125,000
2	Urban (Less Sparse) > 10,000	2	Other Urban Area: Population 10,000-125,000
3	Town (Less Sparse)	3	Accessible Small Town: Population 3,000-10,000, <= 30 minutes drive to settlement of 10,000+
4	Town (Sparse)	4	Remote Small Town: Population 3,000-10,000, 30-60 minutes drive to settlement of 10,000+
5	Village (Less Sparse)	5	Very Remote Small Town: Population 3,000-10,000, > 60 minutes drive to settlement of 10,000+
6	Village (Sparse)	6	Accessible Rural: Population < 3,000, <= 30 minutes drive to settlement of 10,000+
7	Dispersed: hamlets & isolated dwellings (Less Sparse)	7	Remote Rural: Population < 3,000, 30-60 minutes drive to settlement of 10,000+
8	Dispersed: hamlets & isolated dwellings (Sparse)	8	Very Remote Rural: Population < 3,000, > 60 minutes drive to settlement of 10,000+

England/Wales Classification: 1,2,3,5,7 = accessible; 4,6,8 = remote
Scotland Classification: 1,2,3,6 = accessible; 4,5,7,8 = remote
Source: National Statistics Postcode Directory (NSPD)

Table D.22: Weak Concordance Between SOC90 and SOC2000

1998q4-		**1999q1+**	
alemanad	all in employment working as managers & administrators	alemanso	All in emp. working as managers & senior officials
alemproo	all in employment working in professional occupations	alemproo	All in employment working in professional occupations
alemptoc	all in employment working in assoc. prof. & tech. occup.	alemptoc	All in employment working assoc. prof. & technical occs.
alemcloc	all in employment working in clerical occupations	alemasoc	All in emp. working in admin and secretarial occupations
alemcroc	all in employment working in craft related occup.	alemstoc	All in employment working in skilled trades occups.
alemppo	all in employment working in personal & protective occup.	alempso	All in employment working in personal service occups.
alemseoc	all in employment working in selling occup.	alemsoc	All in employment working in sales customer serv occs
alempmo	all in employment working as plant & machine operators	alempmo	All in employment working as plant & machine operators
alempoo	all in employment working in other occupations	alempoo	All in employment working in other occupations
alemuno	all in employment working in unskilled occupations	alempuo	All in Employment working in Unskilled Occupations

Table D.23: The number of unique regions that a postcode district falls into:

reg1	Freq.	Percent	Cum.
1	2,953	55.03	55.03
2	1,596	29.74	84.77
3	627	11.68	96.46
4	155	2.89	99.35
5	30	0.56	99.91
6	4	0.07	99.98
7	1	0.02	100.00
Total	5,366	100.00	
reg2	Freq.	Percent	Cum.
1	2,953	71.48	71.48
2	993	24.04	95.52
3	173	4.19	99.71
4	11	0.27	99.98
5	1	0.02	100.00
Total	4,131	100.00	
reg3	Freq.	Percent	Cum.
1	2,953	54.87	54.87
2	1,606	29.84	84.71
3	632	11.74	96.45
4	156	2.90	99.35
5	30	0.56	99.91
6	4	0.07	99.98
7	1	0.02	100.00

reg1 # of unique Local Authorities that a postcode district falls into.

reg2 # of unique NUTS3 regions that a postcode district falls into.

reg3 # of unique level 1 Local Administrative Units (former NUTS4) that a postcode district falls into.

Format- uniqNT where:
N == 1,2,3 (identifying the case above)
T == Number of interest.
Reg#: the number of unique regions that the postcode district falls into.

Table D.24: Distribution of postcodes that fall into a postcode district that falls into T higher aggregation levels of interest:

uniq1	Freq.	Percent	Cum.
1	753,579	35.23	35.23
2	773,898	36.18	71.41
3	434,552	20.32	91.73
4	140,759	6.58	98.31
5	29,870	1.40	99.71
6	4,624	0.22	99.92
7	1,637	0.08	100.00
Total	2,138,919	100.00	
uniq2	Freq.	Percent	Cum.
1	1,276,065	59.66	59.66
2	703,105	32.87	92.53
3	146,493	6.85	99.38
4	12,569	0.59	99.97
5	687	0.03	100.00
Total	2,138,919	100.00	
uniq3	Freq.	Percent	Cum.
1	750,715	35.10	35.10
2	774,167	36.19	71.29
3	436,530	20.41	91.70
4	141,376	6.61	98.31
5	29,870	1.40	99.71
6	4,624	0.22	99.92
7	1,637	0.08	100.00
Total	2,138,919	100.00	

uniq1 # of full postcodes that fall into a postcode district that falls into T Local Authorities.

uniq2 # of full postcodes that fall into a postcode district that falls into T NUTS3 regions.

uniq3 # of full postcodes that fall into a postcode district that falls into T level 1 Local Administrative Units (former NUTS4).

Format- uniqNT where:
N == 1,2,3 (identifying the case above)
T == Number of interest.

Appendix E

Chapter 5: Constructing Continuous Work-Life Histories. An Alternative Approach

The current release of the BHPS covers 18 waves of the survey, from 1991/1992 to 2008/2009. Socio-Economic data available at the individual & household level. The BHPS provides an annual nationally representative sample of 5000+ household and over 10000 individual-level observations per wave (BHPS User Guide, v.3). 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.

Each wave contains a household identifier and an individual identifier, identifying the position of the individual in question within the household. These two identifiers link information at the individual and household level within each wave, however, they cannot be used to link information across waves. This is due to the fact that household composition changes over time, and an individual's position within the household is prone to change as well (BHPS User Guide, v.3). A unique personal identifier (pid) is also supplied, which can be used for linking information across waves. In order to achieve this, the cross-wave link file is provided. This file contains the response status and identifiers for all known sample members in each wave. In addition time invariant information can be matched in by pid using a cross-wave data file. Retrospective continuous labour market history, from first exit from full time education, is collected at waves two and job history at wave three[1]. I construct the data set by stacking all the relevant work-life information on top of each other in chronological order, from when the individual first left full time education. Full data preparation steps are detailed in subsequent sections. Systematic issues, e.g. recall bias, as well as survey design changes, are explicitly taken into account.

Before conditioning the sample, there were 3516 males directly interviewed in 1991 who were between 16 and 58. After dropping problematic cases (missing wage information) this figure dropped to 3,444. The sample is restricted to individuals continuously present in the BHPS for at least two waves since 1991, that gave full interviews at each wave (excluding proxy respondents & telephone interviews). In addition to minimising non-random 'sample attrition bias', this ensures that observed earnings losses are purely due to wage and hour changes (Ruhm 1991). One aim of this study is to control for individuals' full work-life histories since leaving full-time education. Respondents not present at wave 2 will lack of both retrospective job and labour market status information. Furthermore, individuals present at wave 2 but not 3 will lack information about the reason for leaving previous job if their current labour market status at wave 1 lasted more than 12 months (Halpin 1997). If the sample is restricted to include only individuals that were continuously present for over the full observation window of interest, 1991 to 1997, then this results in a

[1]Where 'job' is defined as a continuous spell of employment, ignoring intra-firm movements, and job characteristics refer to the start of each spell.

sample of 1656 males. Whilst this simplifies the calculation of previous labour market status, and ensures that all these individuals were interviewed at Wave 2 & Wave 3[2], it severely limits sample size. This also assumes that attrition is random and not systematically related with X_{it}. It is also debateable whether the Original Sample Members (OSM) are a random sample of the population, given that responding to survey is not a legal requirement. Out of the 3,444 men directly interviewed at Wave 1, 2,140 were continuously present in the sample for at least 2 waves. This figure drops to 1,842 when those ever in self-employment is exclude, making up the sub-sample in the wage analysis. One way to test the impact of this conditioning is to allow individuals to enter and exit the sample, provided that they give full interview responses for at least two consecutive waves and are part of the OSM, taking first differences to control for time-invariant ability bias. If individuals are allowed free entry and exit from the sample without the OSM restriction (the majority of OSM contribute to the BLIFEMST file (see Halpin 1997, Table 3)), then pre-1.9.1990 information will not exist for those that were not interviewed at the key data collection dates (1992,1993) without further restrictions. Furthermore, the calculation of previous labour market status is complicated due to sample attrition.

In the main analysis, current labour force status is restricted to exclude self-employment. This common approach is done due to difficulties in recording hours of work for self-employed individuals (Arulampalam 2001), the majority of which are unable to provide their usual hours of work in the BHPS data (restricting the analysis to exclude anyone ever reporting themselves as self-employed at survey interview implies a sample of 1850 individuals at wave 1). Labour market status during the past 12 months is not conditioned to exclude

[2]They will all have detailed restrospective information pertaining to the pre-1.9.1990 period, thus allowing for the control of industry experience.

self-employment as this not possible for Wave 1 due to lack of information. However from Wave 2 this information is available in the job history information due to a change of labelling for the relevant variable[3]. To avoid issues regarding the retirement decision, individuals in the sample are restricted to be between the ages of 16 and 58 at the time of the first interview[4]. Individuals are then followed until they turn 65, the standard retirement age. The BHPS interview field work starts on the first of September, usually taking until December, but with a cut off of around April. Attempts to trace house movers and convert initial refusals mean that interview may appear in the data as having taken place in the following year, however in most cases this is within the first 3 months (BHPS User Manual, v.3). There are a few cases where there were still interviews taking place after April, but before September. The later the interview takes place the greater the potential for overlap of the multiple data sources in the BHPS (Paull 2002). Data preparation steps may be more important in these cases, especially if inconsistencies arise between survey date and retrospective information.

The labour market history (JOBHIST) file includes job history since the first of September in the previous year. In order to follow individuals' labour market histories, current and job history status codes are used. In the individual response file (INDRESP) this refers to labour market status at the time of interview, which is not fixed[5]. New job history entries are not requested for individuals that have not changed job since the first of September of the previous year (BHPS User Guide: p.52). Time-invariant information is imputed for

[3]Arulampalam (2001) explicitly states that in her paper: "...previous labour market status of employment includes self-employment (Arulampalam 2001, pp. F593)."

[4]Arulampalam (2001) restricts this to be between 16 & 58 in the published paper, but between 16 & 55 in the working paper.

[5]Interviews usually take place between September and December, but could still be open until April of the following year (ref. BHPS Manual).

spells in which this was missing, as well as job-related information that is only asked at the beginning of an employment spell (ignoring intra-firm movements, given how the job definition used).

Individuals are asked about their complete labour market histories since they first left full-time education. There are 2 files relevant for the first seven waves, data for which was collected at the second and third waves respectively. These files hold retrospective work-life history information including labour market status and spell beginning and end dates. Dates are recorded to the nearest month, however, season codes were used in the case where an individual could not recall the precise month (BHPS Manual, pp. 62). I adopt the same convention as that used by the BHPS: winter is coded January, spring as April, summer as July, and autumn as October. In addition, if an individual supplied the year that they started/ended a spell, but couldn't remember the month, then we assumed that this was July. This rule is relevant for the pre.1.9.90 lifetime history data only, where recall issues are more likely to arise.

In the lifetime history data, if start and end month are missing (coded 'don't know') and the year is known, the length of the labour market spell is imputed by the data providers to the nearest year and entered in daily format[6]. Length is set to missing in cases where the spell started and ended in the same year and the start and/or end months are entered as seasonal codes or aren't supplied. See Table E.1 for an example of an individual that supplied pre-1.9.90 lifetime history data but could not remember the start or end months of any spells except the penultimate record. In these cases spell length (cjsten) is imputed according to the procedure detailed above. The second and forth

[6]If day is unknown this is treated as 1, and in the case of month this is treated as July (See BHPS User Manual for further details).

records supplied by this individual illustrate the point raised about imputation of the length of labour market spell. Since the start and end date are unknown, the first spell is assumed to last 16 years (365.25*16=5844).

Table E.1: Example of individual that could not remember start and end month in pre-1.9.90 data.

wave	jbstat	jhstat	spell start month	interview month	spell end month	start month	start year	end month	end year	Spell length
0	Emp					d/k	72	d/k	88	5844
0	Emp					d/k	88	d/k	88	missing
0	Emp					d/k	88	d/k	89	365
0	Emp					d/k	89	d/k	89	missing
0	Emp					d/k	89	d/k	90	365
0	Emp		1990m2			feb	90	n/a	n/a	1339
1	Emp	Same emp.	1989m2	1991m9	1991m3	feb		march		
1	Emp		1991m3	1991m9		march				211
2	Emp		1991m3	1992m10		march				584
3	Emp		1990m2	1993m10		feb				1349

Months in the pre-1.9.90 data entered as d/k (don't know) are imputed as July. Spells with zero duration are assumed to have lasted at least one month. Raw data illustrated. Spell length unavailable in JOBHIST records.

Whereas the retrospective data collected at the third wave refers exclusively to continuous employment spells with separate employers, the information collected at the second wave is more extensive and covers more labour market states. Granted, as stated in the reference manual, more effort has been made to ensure that the data collected at the third wave is consistent with the information in the main dataset. It should be noted, however, that the definition of a job spell is slightly different to that in the main dataset. In the third wave retrospective data, an employment spell refers to a continuous spell with an individual employer, excluding intra-employer job movements. This contrasts with the main data, where a a new spell is created when an individual gets a promotion with the same employer (BHPS Manual, pp. 68). Furthermore, employment spells collected at wave two refer to continuous spells in separate labour market state. This implies that inter-employer job-to-job transitions will not be captured if using this retrospective data source.

E.0.4 Treatment of Promotions (intra-firm movements)

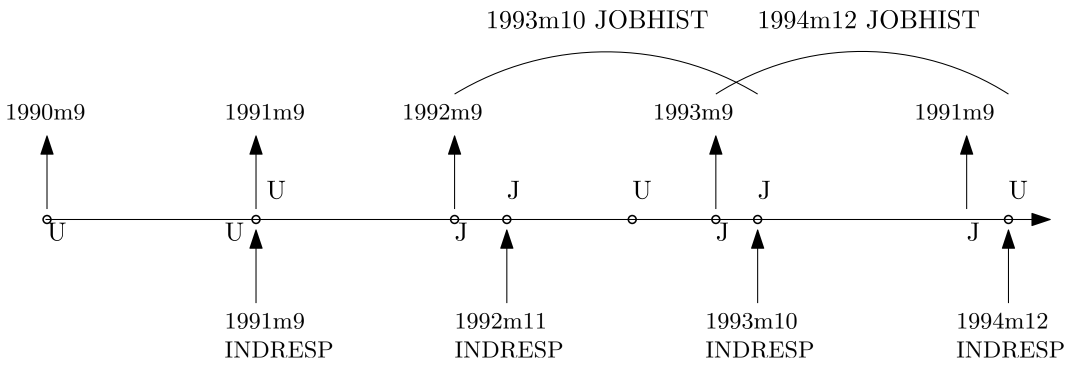
The definition of a job in this study is taken to be a continuous spell with a separate employer. This definition is consistent with that used in the bulk of the existing literature (Farber 1999). If promotions are not treated as the start of a new labour market spell, then the raw 'cjsten' measure provided in the BHPS cannot be used without some alterations (Maré 2006). In calculating this measure, the creators of the BHPS treated promotions as the start of a new labour market spell in the post-1.9.90 data. If occupation-specific human capital is of interest, a more accurate measure may take into account intra-firm movements. In the context of firm-specific human capital, promotions should be not be treated as the beginning of a labour market spell[7].

E.0.5 Overlapping Labour Market History Information

Individuals' labour market histories are prone to overlap due to the timing of interviews in the BHPS varying over the period of the survey. In order to deal with this issue, the start and end dates of the spells can be used. The JOB-HIST data refers to changes in labour market status that occurred since the first of September in the previous year. The simplest approach would be to assume that spells that started or ended before the previous interview month were reported in the previous interview. However, dealing with this issue accurately is not as simple as this due to the potential overlapping of information

[7]Inconsistencies in the promotions measure are identified in the data. This can be expected given its self-reported nature. If an individual reports a promotion and this is to a different employer, then we do not consider this a promotion in the usual sense. These moves are relabelled as moves to a better job.

Figure E.1: 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).

in the JOBHIST file and the INDRESP file. Figure E.1 illustrates this issue. JOBHIST data is 'always' collected from the first of September of the previous year, whereas the interview date is not set. Simply dropping JOBHIST information about spells starting before the previous interview date would result in the loss of information about the exit reason if this spell was employment and it ended within the last 12 months. Table E.2 provides more detail relevant to this point, as the JOBHIST data collected includes job changes that would be lost if this information was simply dropped without copying over the information to the other records pertaining to that labour market spell. Furthermore, rules need to be adopted in the event of inconsistencies between the interview date and retrospective data sources.

If an individual reports that they were in a non-employment labour market status they are asked what date that spell began in order to derive the 'cjsten'. Due to changes in the sequence of questioning as the BHPS has developed,

Table E.2: **Raw data pertaining to Original Sample Member (OSM).**

wave	**jbstat**	**jhstat**	**jhstpy**	**spell start month**	**interview month**	**spell end month**	**same spell?**
1	OLF	OLF	N/A	1990m6		1991m2	
1	Emp.			1991m2	1991m10		
2	Emp.			1990m2	1992m10		<
3	Emp.	Diff. emp.	Better Job	1990m2		1993m6	<
3	Emp.			1993m6	1993m10		<<
4	Emp.	Diff. emp.	Dismissed/ Sacked	1993m7		1994m8	<<
4	Emp.			1994m8	1994m11		
5	Emp.			1994m7	1995m11		
6	Emp.			1993m9	1996m9		<<<
7	Emp.	Diff. emp.	College/ Uni.	1993m9		1996m9	<<<
7	Emp.	Diff. emp.	College/ Uni.	1996m9		1997m2	
7	Emp.	Diff. emp.	College/ Uni.	1997m2		1997m9	
7	Emp.			1997m9	1997m10		

internal inconsistencies may arise in the underlying data. An individual may report themselves as being self-employed or unemployed, when their current (self-defined/subjective) labour market status is initially requested. However, due to the branching nature of the BHPS, the way an individual responds to initial questioning affects whether variables collected later in the survey are available, e.g. job and workplace characteristics. This information can be used to determine whether the self-defined labour market status is accurate [8]. The longitudinal nature of the BHPS implies that if an individual could not remember the start date of an ongoing spell, they may recall it at a later date if spell status changes. Table E.3 illustrates an example of an individual that could not remember the start date of the 'out of the labour force' spell reported at Wave 1, but remembered this information at the next interview date. In this case one would copy the start date of the spell to the previous incidences of that spell.

[8] See the BHPS user manual for more detail on this feature of the data.

Table E.3: **Extract of the raw data, pertaining to sample member.**

wave	**jbstat**	**jhstat**	**jhstpy**	**spell start month**	**interview month**	**spell end month**	**Recall issue**
1	OLF			?	1991m10		<<
2	OLF	OLF	N/A	1974m4	1992m11	1992m6	<<
2	U	U	N/A	1992m6	1992m11	1992m7	
2	E	Diff. emp.	Temp. Job	1992m7	1992m11	1992m8	
2	U	U	N/A	1992m8	1992m11	1992m10	
2	E	Diff. emp.	Temp. Job	1992m10	1992m11	1992m11	
2	U			1992m11	1992m11		

The use of interview date observations implies that short spells of employment will not be captured in the data. This is likely to be of significance for frequent job movers in the post-1.9.90 data. Table E.4 illustrates an example of an individual in the raw data who will not be in the Wage equation as they reported themselves in Employment only once in the INDRESP file, although they were actually in employment over the sample period five times. A positive real wage is only observed for individuals who report themselves in employment at current interview date (INDRESP file). For employment spells in the JOBHIST file, a positive real wage is unobserved, unless one imputes 'usual hours worked' using the interview date records.

E.0.6 Pre 1.9.1990 Lifetime History Data

Before attempting to use the retrospective pre-1.9.1990 labour market history data collected in the BHPS, an important technicality needs to be taken into account. Labour market status history is collected from all individuals at wave 2, provided they are not still in full-time education[9]. Although all individu-

[9]As Mare (2006) highlights, this condition does not seem to be consistently applied by interviewers.

Table E.4: Survey date labour market status underreports intra-survey date dynamics.

wave	**jbstat**	**jhstpy**	**Start month**	**Interview month**	**End month**	**Source:**	**Positive Wage Observed?**
1	OLF			1991m10		INDRESP	
2	OLF	N/A	1974m4		1992m6	JOBHIST	
2	Unemployed	N/A	1992m6		1992m7	JOBHIST	
2	Employed	Temp. Job	1992m7		1992m8	JOBHIST	
2	Unemployed	N/A	1992m8		1992m10	JOBHIST	
2	Employed	Temp. Job	1992m10		1992m11	JOBHIST	
2	Unemployed		1992m11	1992m11		INDRESP	
3	Employed	Temp. Job	1992m7		1992m10	JOBHIST	
3	Employed	Temp. Job	1992m10		1992m11	JOBHIST	
3	Unemployed		1992m11	1993m11		INDRESP	
4	Unemployed			1994m10		INDRESP	
5	OLF	N/A	1989m10		1995m8	JOBHIST	
5	Unemployed		1995m7	1996m1		INDRESP	
6	OLF	N/A	1993m9		1995m10	JOBHIST	
6	Unemployed		1995m11	1996m9		INDRESP	
7	Unemployed	N/A	1995m12		1996m12	JOBHIST	
7	Employed		1996m12	1997m9		INDRESP	<<<<

als present at Wave 2 contribute a BLIFEMST record, only those present at both Waves 2 & 3 contribute a CLIFEJOB record (ref. Halpin (1997) 'Unified BHPS work-life histories', pp. 20). This contribution is further conditioned on whether the interviewee has had a previous job lasting for at least 1 month before the interview date employment spell, given that they are in employment at wave 3 (Maré 2006). Those in their first job since leaving full-time education do not contribute to the CLIFEJOB file. Thus the data collected at Wave 3 cannot be considered an alternative overlapping source of information on retrospective labour market spells. If one drops retrospective employment information collected at Wave 2, this results in the loss of all retrospective employment information if an individual was only interviewed at Wave 1 & 2. This will also be the case if an individual was interviewed at wave 2 & 3, was in their first job since leaving full-time education, and they spent more than a month in non-employment prior to the first job.

One potential strategy, taking into account whether an individual is in their first job, is to assume that retrospective employment information is duplicated if an individual was continuously present in the survey for at least 3 Waves. Given this criteria, employment information collected at Wave 2 could be dropped. If this condition is not met, the retrospective employment information collected at Wave 2 could be used. This artifact, although not explicitly documented in Arulampalam (2001), may explain why the author only controls for previous full-time experience (given that she states that she uses both the BLIFEMST and CLIFEJOB data sources). However, this strategy is not implemented in this study given that roughly half of the CLIFEJOB spells conflict with the BLIFEMST records. The retrospective information collected at Wave 2 does not allow one to control for pre-1.9.1990 industry-specific experience as the Wave 3 CLIFEJOB data allows. Furthermore, "reason for leaving previous job" will be missing for individuals that did not contribute to the CLIFEJOB file. However, it is likely that the industry-specific information in the retrospective CLIFEJOB file will be inconsistent with that collected in the current BHPS as it refers to the start of a spell with a single employer and ignores subsequent intra-firm movements. The current BHPS data can refer to separate job spells with the same employer, so information may change if an individual changes job or workplace with the same employer.

The BLIFEMST data: This captures all continuous labour market spells since leaving full-time education, that lasted more than one month. Labour market spells ongoing at interview date, as well as spells that began on or after

1.9.90 are dropped as an initial step. I do not drop ongoing spells that started before 1.9.90 as in this case they are relevant for the rule-based approach.

The CLIFEJOB data: In theory "employment status spell number" permits one to link the BLIFEMST data with that collected at wave 3 as it points to the relevant spell in the sequence of labour market states collected at wave 2[10]. However, this indicator will be coded zero in some instances, cases where the two data sources cannot be accurately linked. At the beginning of the wave 3 interview respondents that contributed to the BLIFEMST data are given a chance to confirm the accuracy of the lifetime history data collected at wave 2 (Maré 2006)[11]. If it is deemed accurate then the BLIFEMST file is used as a basis for collecting the CLIFEJOB data. In this case the two files are easily linkable. If the BLIFEMST data is deemed inaccurate then the CLIFEJOB data is collected, but "employment status spell number" is coded zero. In this case inconsistencies between the wave 2 & 3 lifetime history datasets are likely to arise. Some of these inconsistencies may be impossible to resolve without further assumptions. Lifetime job history spells that began after 1.9.90 are dropped as an initial step. Furthermore, entries that refer to the present job reported at wave 3 are dropped regardless of whether they started before or after 1.9.90.

E.0.7 Rules-based Approach

In constructing the dataset, a *rules-based* approach is adopted to ensure consistency of the data. Three labour market states considered are Employment,

[10]In cases where "employment status spell number" is available, the BLIFEMST record captures a continuous periods in employment (potentially with seperate employers) whereas the CLIFEJOB data details continuous job spells with separate employers.

[11]Due to the dataset construction, all individuals present at wave 3 will have contibuted to the wave 2 data.

Unemployment & Out of the Labour Force (OLF). The OECD definition of the 'currently inactive population' is used to classify labour market states as 'Out of the Labour Force'[12]. This definition implies that it is possible for an individual to experience two consecutive OLF spells if the underlying labour market state is different. I do not merge these consecutive OLF spells, but take this into account when constructing indicators of interest.

Interviews that are recorded as having taken place before September in any year as assumed to have taken place in the subsequent year but are still treated as coming from the reported wave, e.g. Wave 1 interviews started in September 1991, so interviews in that wave reported as having taken place before September are assumed to have taken place in 1992. This implies that the gap between interview dates will vary since interview date is taken as the reference point[13]. Furthermore, if interview month is recorded as missing in the INDRESP file, I assume it took place in September.

The first step is to put all the spells in chronological order. Since a globally consistent job spell number is provided in the current BHPS, this is a straightforward task in the post-1.9.1990 data. If one wants to combine the Wave 2 and 3 retrospective lifetime history files, this is not as straightforward. Inconsistencies exist between the two pre-1.9.1990 data sources due to their retrospective nature and different collection points (see Section E.0.6). Spell start and/or end month is missing for a significant proportion of labour market spells due to interviewees not being able to recall this information at all. In

[12]The OECD classifies the following individuals as inactive: attending an educational institution; performing household duties; retiring on pension or capital income; other reasons, including disability or impairment (OECD 2004).

[13]Borland *et al.* (2002) use the first of September as their fixed reference point, however this information is collected retrospectively and likely to suffer from issues detailed in this section.

general, this phenomenon is more common the further back into time an individual has to remember, more notably for frequent job changers Paull (2002).

Various methods exist to deal with pre 1.9.90 information (See Maré (2006) for a detailed survey). If one is just interested in previous labour market states then the BLIFEMST can suffice (although the Wave 2 file may be subject to more error than the Wave 3 information). Since in principle the CLIFEJOB data details job held within each continuous employment spell in the BLIFEMST file, job data can be merged in where possible. In general, an identifier consistent with the sequence of spells in the BLIFEMST file is available for roughly half of the CLIFEJOB (clejsfn). This is only possible for individuals present at Wave 2 & 3 and who weren't in their first employment spell at wave 3 and weren't still in full-time education at wave 2, prerequisites for contribution to the CLIFEJOB file. The approach adopted to establish this link by combining the BLIFEMST AND CLIFEJOB files and then applying Rule X (see Figure E.2). Halpin (1997) created a file merging month-by-month job and labour market status records from the CLIFEJOB and BLIFEMST files using a set of rules common in some aspects to the approach adopted herein. Once the data is in chronological order, the following rules are adopted in order to harmonise the data and address inconsistencies.

Lifetime history data: If a respondent indicates that they can't remember spell start and/or end month, this is imputed as July in both lifetime history files. Given the chronological ordering, spells are also assumed to start in the same month that the previous spell ended. This operation is initially only conducted on the BLIFEMST file. Once the CLIFEJOB file has been merged in, Rule X is adopted to deal with any inconsistencies. Once Rule X has been applied, any chronological spells from the CLIFEJOB file are assumed to have began when the last one ended.

Rules were adopted for dealing with retrospective lifetime history data that overlaps with the current BHPS. Before implementing these rules the following "mantra" was decided upon: **Trust current information over retrospective data, and trust retrospective data from the JOBHIST file over that collected from the Wave 2/3 lifetime history files.** This assumption is essentially equivalent to the common approach in the literature of giving preference to information reported closest to the event of interest (Halpin 1997; Upward 1999; Paull 2002; Dustmann & Pereira 2008, see), however, treatment of the overlapping information varies.

Upward (1999) drops individuals who recorded negative spell lengths. In this study these spells are not dropped as the rules-based approach takes deals with this issue by assuming that these spells began when the last spell ended[14]. All these cases in the data arise when an individual reported a spell that started and ended in the same year and the start and/or end date was a season code. Inconsistencies in the season coding mean that if a spell ended in November it could end up in the data as January of that same year (winter season code is assigned to January by the BHPS). Dealing with this issue is not a straightforward task. Maré (2006) details various approaches taken in the literature. Application of the assumption that the spell started when the last one ended to the merged work-life histories file seems to deal with most of these cases. See Maré (2006) Table 10 for alternative approaches taken to deal with this issue. Work-life history information is dropped if start or end dates are coded as

[14]For the few of cases where this approach did not remove this issue, spell length was set to missing.

missing. This may lead to an underestimation of the impact of experience, especially in cases where we drop significant pre-1990 employment periods, however impact of this assumption is assumed small *(how many cases affected?)*.

The key assumptions distinguishing the different approaches:

1. Treatment of intra-firm movements.
2. Defining previous labour market status.
3. Time period: 1991-1997; 1991-200(1);

Assumptions affecting all approaches:

1. Sample selection - “Continuously present” definition.
2. Job status definition.
3. Manipulation of pre-1.9.90 data: - e.g. Dropping all spells that ended before 1.1.1980, as well as those that began after 1.9.90 when the main BHPS starts collecting data.

The BHPS contains various data sources, collected at differing points and covering different periods. Merging these different data sources results in overlap and inconsistencies, which are dealt with using a rules-based approach.

The treatment of intra-firm movements depends on the phenomenon one aims to capture. Neal (1995) is an example of a seminal paper addressing the importance of industry-specific human capital in establishing the relationship between pre-displacement tenure and post-displacement earnings. Parent (1999; 2000) addresses returns to tenure in the context of industry-specific human capital. Gibbons & Waldman (2004) investigate task-specific human capital, a novel approach viz-a-viz the existing literature. With regards to firm-specific human capital, continuous spells with separate employers are of interest (Farber 1999). Given the "job spell" definition used in this study, continuous spells with separate employers are of interest. However, if the interest is in occupation-specific human capital, then each job held should be taken into consideration (Sullivan 2008).

Previous labour market status is impacted by the treatment of intra-firm movements. How one categorises an individual as 'continuously present' is likely to be key, alternative definitions of which are implemented as a robustness check. Using an alternative strategy which follows Original Sample Members (OSM) until they leave the sample, provides a larger sample size than the methodology, implemented in some studies like Halpin (1997) and Dustmann & Pereira (2008), which essentially only includes individuals that are continuously present over the observation window.

A summary of the rules-based approach (main rules) is given below:

RULE 1: - Drop JOBHIST spells that ended on or before the first of September of the previous year.

RULE 2: - If the spell in the INDRESP/JOBHIST file started before the last spell in the lifetime history files, drop the lifetime history record.

RULE 3: - If last spell in lifetime history file stared before the first spell in the INDRESP/JOBHIST file and ended after it, but job status is the same, assume they are the same spell. Drop the lifetime history entry. This rule is only implemented for non-employment spells.

MODIFIED RULE 3: - If last spell in lifetime history file stared before the first spell in the INDRESP/JOBHIST file and ended after or when it ENDED, but job status is the same, assume they are the same spell. Drop the lifetime history entry. This rule is only implemented in the case of employment spells originating from the CLIFEJOB file.

RULE 4: - If the last spell in the lifetime history file started before the first spell in the INDRESP/JOBHIST file, keep & assume that it ended when the first entry in the INDRESP/JOBHIST file began[15]

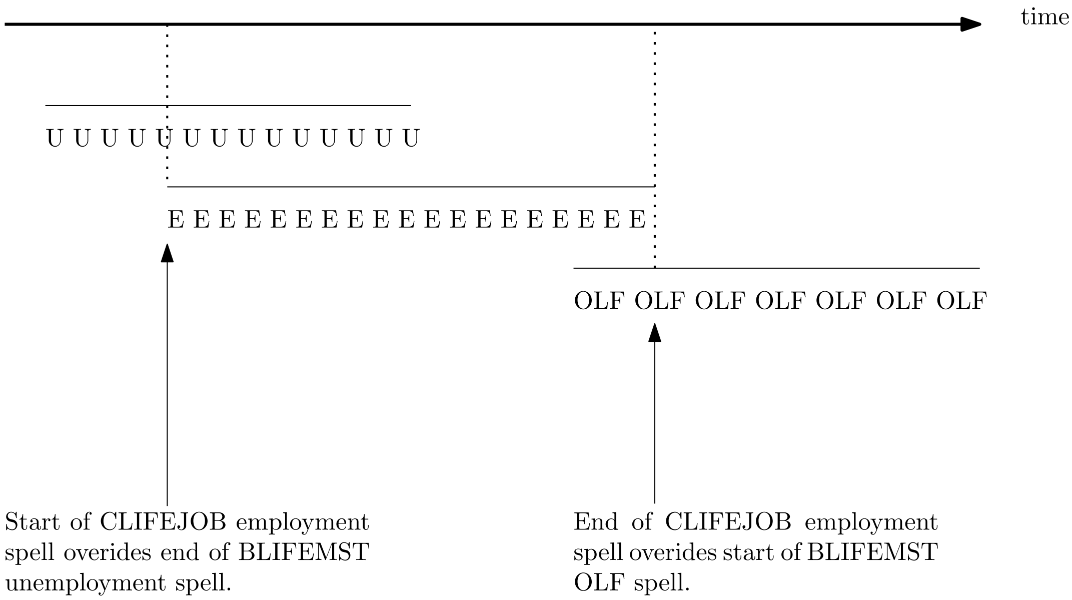
RULE 5: - Rule 3 from Upward (1999). This rule states that if a spell from the JOBHIST record starts before the previous interview date, it should be the same spell as recorded in last year's INDRESP record. If the status of this spell is the same as the status in the previous wave, copy across the history status value and jhstpy and then drop the information from JOBHIST. The values of history status and jhstpy should be copied across to the first occurence of that spell (Upward 1999).

E.0.8 Application of Rules-Based Approach to pre-1.9.90 Retrospective Data.

The CLIFEJOB and BLIFEMST files are combined to form continuous job histories since leaving full-time education. Inconsistencies between the CLIFEJOB and BLIFEMST files arise due to their retrospective nature, and the fact that they were collected a year apart. The approach adopted to establish this link was by overlaying CLIFEJOB file on the BLIFEMST, linking the job information to employment spells where possible, overriding the BLIFEMST information with that from the CLIFEJOB file. The data was then sorted into

[15]This is essentially Rule 2 in Upward (1999).

Figure E.2: Illustration of Rule X (that CLIFEJOB dominates BLIFEMST) applied to fictitious data set combining pre-1.9.90 lifetime labour market history data.



This is essentially a brute force approach. More sophisticated approaches have been adopted in the literature on work-life histories (see Mare,2006 for a survey of these contributions). The BLIFEMST & CLIFEJOB files are combined, retaining the information in the CLIFEJOB file in the case of matches and inconsistencies.

chronological order using the start, end year, and spell sequencing information which was always available. Rule X (see Figure E.2) was then applied to the resulting data set. Halpin (1997) created a file merging month-by-month job and labour market status records from the CLIFEJOB and BLIFEMST files using a set of rules common in some aspects to the approach adopted herein. Once the data is in chronological order, the following rules are adopted in order to harmonise the data and address inconsistencies.

Cases in the lifetime history which started and ended before school leaving age are dropped (40 person year observations), keeping those that started before and ended after this point[16]. Two formulations of the lifetime history are

[16]If the first spell in the lifetime history file was employment and this started on or after

drawn on, one which includes all information since the individual in question left full-time education, the other which which truncates lifetime history spell end date at 1.1.1980 (keeping spells which started before and ended after this date). The motive for conditioning the lifetime history data to exclude labour market spells that ended before the first of January 1980 is the likelihood of higher incidence of recall bias for earlier spells[17]. Spells which began before this date, and ended after it, are included in the analysis when this formulation is adopted. Spell length for spells starting and ending in the same year, where start and/or end month is recorded as a season code, is treated as missing. Where start date could not be imputed from adjacent spells, employment spells are dropped if an individual failed to report the month/year a spell began[18]. Furthermore, if month is missing in the pre-1.9.90 data then this is assumed to be July.

After encountering numerous difficulties, inconsistencies between the CLIFE-JOB and BLIFEMST data are minimised by basing the pre-1.9.90 data on the latter file. This is a similar approach to that adopted by Halpin (1997) when merging these two data sources. Maré (2006) relies on the dating sequence in the BLIFEMST file when merging the two retrospective data sources (step i, pp. 77). Since the BLIFEMST file contains a chronological record of continuous labour market spells since first leaving full-time education, missing date information can be easily imputed using information from adjacent spells[19]. Halpin

further education leaving age (feend), replace school leaving age with further education leaving age (in practise I generate a new indicator). This rule is also applied to post-1.9.90 data. If the first spell in lifetime history was employment the length of the last spell is assumed to have been the 12*(age that spell began - 5) where 5 is the assumed school starting age. This affects 1801 person-year observations, the majority of which are in the pre-1.9.90 data.

[17] The degree of imputation required in the construction of retrospective spells starting and ending in earlier time periods increases the further into the past one looks. This information is used to gauge previous labour market status and reason for leaving previous job.

[18] This step is only carried out on the combined job and employment status history files. This affects 45 out of the 15980 spells in the combined file.

[19] In order for our rules to work, spell start date is required in the lifetime history file. The

(1997) implements a rule, applied to the JOBHIST records, that overrides the start date with the end date of the previous spell. If an individual indicates that the labour market information BLIFEMST record is correct[20] then assuming that individuals have not reported this information erroneously, their employment spells are split into separate jobs using the CLIFEJOB data. If inconsistencies still arise then they are dealt with using Rule X, detailed below.

Rule X: In the case of inconsistencies between the BLIFEMST and CLIFEJOB file, these inconsistencies are dealt with by the assumption that the CLIFEJOB spell start and end dates override the BLIFEMST entries. This overlap will be with non-employment spells, and will have the effect of reducing the reported period in non-employment pre-1.9.90. See Figure E.2 for an illustration of this rule.

E.0.9 Application of Rules-Based Approach to Current Data.

Figure E.1 illustrates the extent of overlap in the current BHPS. The job history (JOBHIST) file, only collected if job status changed in the last ˜12 months, contains retrospective data from the first of September of the previous year. The individual response file (INDRESP) contains a snapshot of what the individual was doing at the time of interview. Since the JOBHIST file is collected retrospectively, inconsistencies can arise between this and the information collected at interview date. An example is what (Jürges 2007) refers to as a "false

majority of cases with missing start or end date information are due to individuals not being able to recall the month of a spell starting. Consistent with our general approach missing months are assumed to be July. 45 person-year observations are dropped where spell start date could not be imputed from the adjacent spell information or using the BHPS-supplied spell length indicator. These were cases where spell start year was missing as well as month. Consistent with BHPS methodology, records which started and ended in a month entered as a season code have spell lengths coded as missing.

[20]49% of cases where highlighted as being correct in the full CLIFEJOB file (Maré 2006). This figure is % amongst the Original Sample Members (OSM) used in this analysis.

negative" result: where an individual fails to report themselves as being in unemployment at a particular date when asked about it retrospectively, although they reported themselves in that state at the date of occurrence Jürges (2007). A rules-based approach is adopted in order to deal with this and other systematic issues in the data.

1. The first step is to drop JOBHIST spells that ended on or before the first of September of the previous year. These spells are assumed to already be in the data. See Table E.5 for an example of spells that this rule would drop. In this case, there are two spells reported at wave 6 which this individual has already been reported at wave 5. These spells are dropped according to Rule 1.

Table E.5: An individual Rule 1 will affect.

wave	jbstat	jhstat	jhstpy	Start month	Interview month	End month	Jobhist start month
5	Emp.	Diff. emp.	Other	1991m9		1995m9	1994m9
5	Unemp.	Unemp.	N/A	1995m9		1995m9	1994m9
5	Emp.			1995m9	1995m11		1994m9
6	Emp.	Diff. emp.	Other	1991m9		1995m9	1995m9
6	Unemp.	Unemp.	N/A	1995m9		1995m9	1995m9
6	Emp.	Same emp.	Better Job	1995m9		1995m10	1995m9
6	Emp.	Same emp.	Better Job	1995m10		1996m5	1995m9
6	Emp.			1996m5	1996m10		1995m9

Rule 1: Drop JOBHIST spells that ended on or before the first of September of the previous year.

Rules implemented to deal with pre-1.9.90 retrospective data collected at wave 2 (BLIFEMST) and wave 3 (CLIFEJOB):

2. If the first spell in INDRESP/JOBHIST file started before last

spell in life time hist. files (CLIFEJOB/BLIFEMST), drop lifehist.

This rule is the same as the rule implemented by Halpin to join the BLIFEMST lifetime history to the post-1.9.90 BHPS data (see Halpin 1997, pp.14). See Table E.6 for an example of why I implement this rule. The first spell in the INDRESP file refers to a spell of employment. Since current labour market status is trusted over retrospective information, the start date of the spell at wave 1 1983m2 is assumed to be correct. Thus the last entry in the pre-1.9.90 data is dropped. In this case inconsistencies between the pre- and post-1.9.90 data are evident, however due to our *mantra* these differences are assumed to be attributable to recall bias.

Table E.6: An individual that Rule 2 will affect.

wave	jbstat	jhstat	jhstpy	Start month	Interview month	End month	cjsten
0	Emp.		Missing	1980m10		1984m1	1187
0	Unemp.			1984m1		1990m7	2374
1	Emp.			1983m2	1991m10		3174
2	Emp.	Diff. emp.	Redundant	1990m7		1992m5	
2	Unemp.			1992m5	1992m9		130

Rule 2: If the first spell in the INDRESP/JOBHIST file started before the last spell in the lifetime history files, drop the lifetime history record.

Table E.7 is an example of an individual who reported themselves as being in employment in the last spell of the pre-1.9.90 data. In this case, the spell is duplicated later in the pre-1.9.90 data when the employment spell ended at wave 3. Rule 2 would drop the last record in the lifetime history.

Retrospective data is dropped, if it overlaps exactly with the first wave information in terms of labour market status and start month. This is also done in cases where the pre-1.9.90 data overlapped with wave 1 data in terms of end month and labour market status, but start month missing at wave 1. In these

Table E.7: An individual that Rule 2 will affect.

wave	jbstat	jhstat	jhstpy	Start month	Interview month	End month	cjsten
0	Unemployed			1988m8		1989m1	152
0	Employed		Dismissed/ Sacked	1989m1		1993m8	1674
1	Employed			1988m10	1991m10		1123
2	Employed			1991m8	1992m10		455
3	Employed	Diff. emp.	Dismissed/ Sacked	1989m1		1993m8	
3	Unemployed			1993m9	1993m10		51

Rule 2: If the first spell in the INDRESP/JOBHIST file started before the last spell in the lifetime history files, drop the lifetime history record.

case, start month, and reason for leaving job (in the case of CLIFEJOB spells) is copied over from the historic record to the wave 1 spell and the lifetime history spell is dropped.

3a. RULE 3. If the last spell in lifetime history file stared before the first spell in the INDRESP/JOBHIST file and ended after it began, but job status is the same, assume they are the same spell. Drop the lifetime history entry.

Table E.8 illustrates an example of this rule in action. This Individual started working with their employer in January 1970 and retired in December 1992[21]. The pre-1.9.90 record comes from the CLIFEJOB file. The first entry at Wave 1 refers to a job with the same employer that started in 1981m6, with previous jobs with the employer not captured in the current BHPS. Since the wave 3 pre-1.9.90 employment spell data refers to continuous spells with separate employers, any intra-firm movements before the job held at wave 1 will not be captured in the BHPS data using this approach. In this case it seems appropriate to assume that this individual experienced an intra-firm movement in June 1981. If intra-firm movements are being taken into account, then the reason

[21] Slight recall error is evident in this example, as the individual reported the end date as January in the current BHPS but December 1992 in the retrospective file.

for the previous spell ending at wave 1 would be coded as missing.

Table E.8: Application to employment spells: An individual that Rule 3 would affect if implemented indiscriminately.

wave	**jbstat**	**jhstat**	**jhstpy**	**start month**	**Interview month**	**End month**	**cjsten**
0	Employed		Retirement	1970m1		1992m12	8370
1	Employed			1981m6	1991m9		3761
2	Employed			1988m10	1992m10		1484
3	Employed	Same emp.	Retirement	1988m10	1993m9	1993m1	
3	Self emp.			1993m1	1993m9		256

Rule 3: If last spell in lifetime history file stared before the first spell in the INDRESP/JOBHIST file and ended after it, but job status is the same, assume they are the same spell. Drop the lifetime history entry. This rule is only implemented for non-employment spells.

Rule 3 cannot be implemented indiscriminately: if individuals entered a spell end dates erroneously, so that there is *marginal overlap* with the JOBHIST/INDRESP file, this rule will assume that the lifetime history entry is the same spell even if they are actually different. Furthermore, the source of the pre-1.9.90 data needs to be taken into account, given that the two retrospective lifetime history files capture different phenomena. Rule 3 is not implemented in the case of pre-1.9.90 employment spells, regardless of their source. This decision was made due to the problem of *marginal overlap* caused by recall bias with respect to start and end dates. A modified version of Rule 3 is implemented in the case of pre-1.9.90 employment spells. An example of *marginal overlap,* due to recall error is highlighted in Table E.9. If rule 3 were indiscriminately implemented in the case of employment spells, this would drop over 900 person-year observations. This issue is further exemplified in Figure E.3.

APPLICATION OF RULE 3 TO NON-EMPLOYMENT SPELLS:

Relative to the treatment of spells of employment, application of this rule to non-employment spells seems fairly uncontroversial. In the example presented in Table E.10, the last spell in the pre-1.9.90 data and the first spell at wave 1

Table E.9: *Marginal overlap* due to recall bias: An individual that Rule 3 would affect if implemented indiscriminately.

wave	**jbstat**	**jhstat**	**jhstpy**	**Start month**	**Interview month**	**End month**	**cjsten**
0	Employed		Promoted	1982m6		1988m5	2161
0	Employed		Better Job	1988m5		1989m11	547
1	Employed	Diff. emp.	Better Job	1989m5	1991m11	1990m11	
1	Self emp.			1990m11	1991m11		367
2	Self emp.				1992m11		1100

Rule 3: If last spell in lifetime history file stared before the first spell in the INDRESP/JOBHIST file and ended after it, but job status is the same, assume they are the same spell. Drop the lifetime history entry. This rule is only implemented for non-employment spells.

are assumed to be the same spell.

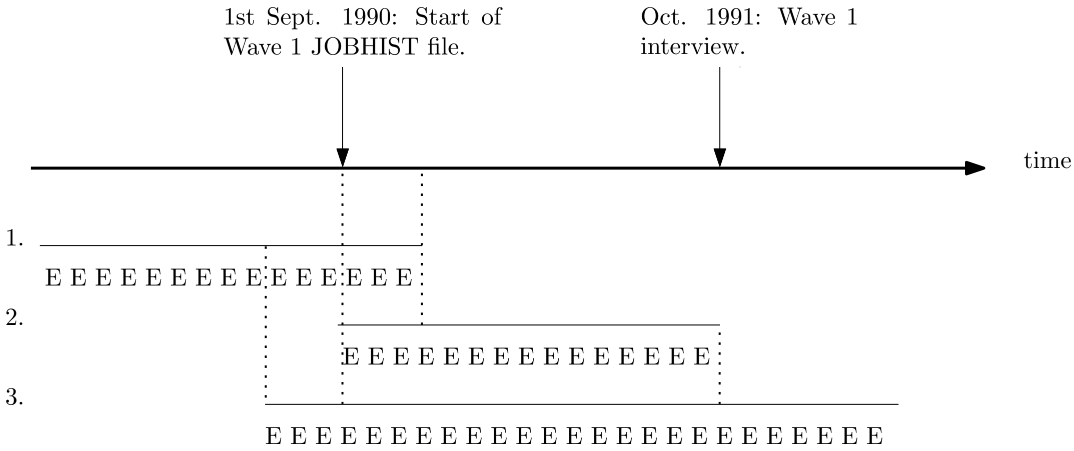
Table E.10: Application to non-employment spells: An individual Rule 3 would affect

wave	**jbstat**	**jhstat**	**jhstpy**	**Start month**	**Interview month**	**End month**	**cjsten**
0	Emp.		Temp. Job	1989m7		1990m7	365
0	Unemp.			1990m7		1991m4	273
1	Unemp.	Unemp.	N/A	1990m8	1991m11	1991m2	
1	Emp.			1991m2	1991m11		291
2	Emp.	Diff. emp.	Redundant	1991m4	1992m11	1992m4	
2	Emp.	Diff. emp.	Redundant	1992m4	1992m11	1992m9	
2	Unemp.			1992m9	1992m11		85

Rule 3: If last spell in lifetime history file stared before the first spell in the INDRESP/JOBHIST file and ended after it, but job status is the same, assume they are the same spell. Drop the lifetime history entry. This rule is only implemented for non-employment spells.

Figure E.3 highlights how overlapping information from the pre- and post-1.9.90 BHPS are dealt with. Given the **mantra** it is easier to deal with spell 3 and 2. These two data sources are treated as referring to the same spell of employment. In this case, the start month of employment spell 3 seems to have been reported with some error (see modified rule 5). However, if one were presented with spell 1 and 2, it would be harder to tell whether these refer to the same spell. Since spell 1 originates from the retrospective lifetime history file, this overlap is assumed to be recall error with respect to spell end dates. Spell

Figure E.3: Illustration of issue of marginal overlap in the data, *viz.-a-viz.* the pre-1.9.90 and first wave data.



Spell 1 and 3 originate from the pre-1.9.90 files (CLIFEJOB), whereas spell 2 is from the wave 1 INDRESP record. Where wave 1 spell start date was missing, this was imputed using ‘cjsten’. In 28 cases, cjsten was missing so these spells were assumed to have started on 1.9.1990.

1 is assumed to have ended when spell 2 began, given that we give priority to information recorded closer to the date of occurrence.

3b. MODIFIED RULE 3: If last spell in lifetime history file stared before the first spell in the INDRESP/JOBHIST file and ended after or when it ENDED, but job status is the same, assume they are the same spell. Drop the lifetime history entry. This rule is only implemented in the case of employment spells originating from the CLIFEJOB file.

Table E.11 is an example of this rule in action. The last entry in the pre-1.9.90 data is sourced from the CLIFEJOB file. In this case one can be sure that the first entry at wave 1 is a job spell within the continuous employer spell recorded in the lifetime history file. It is certain that the first employment spell at wave 1 is part of the continuous employment spell recorded in the last entry at wave 0.

Table E.11: Modified Rule 3: An individual this rule would affect.

wave	jbstat	jhstat	jhstpy	Start month	Interview month	End month	cjsten
0	Emp.		Health	1984m1		1986m7	821
0	Emp.		Redundant	1987m7		1992m9	1887
1	Emp.	Diff. emp.	Better Job	1987m8	1991m9	1990m9	
1	Emp.	Diff. emp.	Health	1990m9	1991m9	1990m11	
1	Unemp.	Unemp.	N/A	1990m11	1991m9	1991m8	
1	Emp.	Diff. emp.	Better Job	1991m8	1991m9	1991m9	
1	Emp.			1991m9	1991m9		4

Modified Rule 3: If last spell in lifetime history file stared before the first spell in the INDRESP/JOBHIST file and ended after or when it ENDED, but job status is the same, assume they are the same spell. Drop the lifetime history entry. This rule is only implemented in the case of employment spells originating from the CLIFEJOB file.

A further modified version of rule 3 is applied to spells reported at wave 1 with no beginning date supplied, and cjsten missing so beginning data could not be imputed.

3c. If no beginning date supplied at wave 1 but the previous spell at wave 0 is present, job status is the same, and the wave 0 record spell ended at or after the wave 1 interview date, then these records are assumed to be the same spell.

Table E.12 provides an example of this rule applied to a continuous employment spell. Wave 1 interview date is used in this instance as start date is missing and cannot be imputed.

3d. This further modified rule is also implemented in the case of spells recorded at wave 1 with no end date, i.e. ongoing at interview date. In these cases interview date is also used instead of spell end date. Again, there the issue of *marginal overlap* due to recall bias is encountered. The example in Table E.13

Table E.12: Modified Rule 3c: An individual this rule would affect.

wave	jbstat	jhstat	jhstpy	start month	Interview month	end month	cjsten
0	Emp.		Redundant	1983m3		1983m5	60
0	Emp.		N/A	1983m5		1993m9	3804
1	Emp.				1991m10		
2	Emp.			1983m5	1992m9		3438
3	Emp.			1991m5	1993m10		885

Alternative Modified Rule 3c: If no beginning date supplied at wave 1 but the previous spell at wave 0 is present, job status is the same, and the wave 0 record spell ended at or after the wave 1 interview date, then these records are assumed to be the same spell.

highlights this issue[22]. There is no impact of rule on non-employment spells, where this rule is less controversial.

Table E.13: Modified Rule 3d: An individual this rule would affect.

wave	jbstat	jhstat	jhstpy	Start month	Interview month	End month	cjsten
0	Emp.		Better Job	1980m7		1983m7	1004
0	Emp.		Better Job	1983m4		1989m2	2130
1	Emp.			1989m1	1991m10		1001
2	Emp.			1989m2	1992m9		1312

Alternative Modified Rule 3d: If no end date supplied at wave 1 but the previous spell at wave 0 is present, job status is the same, and the wave 0 record spell ended at or after the wave 1 interview date, then these records are assumed to be the same spell.

4. If last spell in pre-1.9.90 file started before the first spell in the INDRESP/JOBHIST file, keep the lifetime history entry and assume that it ended when first entry in INDRESP/JOBHIST file began. This rule is implemented since we trust the JOBHIST information over the lifetime history, and the interview date information (INDRESP) over JOBHIST. Employment spells originating from the BLIFEMST file that overlap with the first wave of the BHPS will all be truncated at the first wave start date[23]. This affects Original Sample Members

[22] Whilst the two retrospective pre-1.9.90 employment spells are both taken from the CLIFEJOB file, there is a mismatch between the end date of the first spell and start date of the following spell. This mismatch is caused because the end date of the first spell is entered as a season code (summer) and not a specific date. In the application of Rule X, the last spell in the pre-1.9.90 data would be assumed to have began when the previous record ended. This rule is only implemented after spells are in chronological order, in the case of adjacent spells from the CLIFEJOB file.

[23] It is important to note whether these spells refer to the first employment spell at wave

that contributed to the survey at wave 2, but were not continuously present for at least 3 waves since 1991.

If an individual was continuously present in the survey for at least 3 waves, but their last employment spell in the pre-1.9.90 data came from the BLIFEMST file and this was ongoing at wave 2 interview date, I assume that this is part of the same job spell reported at wave 3 interview. I copy over the start date to the first spell at wave 1, dropping the pre 1.9.90 job spell. This accounts for the fact that individuals who were in their first jobs at wave 3 interview are not asked for the job history. This affect 198 person-year observations.

Duplicate entries also dropped from the lifetime history data, if they overlap in terms of start and end date as well as job status. CLIFEJOB data is trusted over BLIFEMST data, in the case of overlap.

5. Application of modification of Upward(1999) rule 3.

This rule states that if a spell from the JOBHIST record starts before the previous interview date, it should be the same spell as recorded in last year's INDRESP record. If the status of this spell is the same as the status in the previous wave, copy across the history status value, reason for leaving job, and then drop the information from JOBHIST. The values of history status and jhstpy should be copied across to the first occurrence of that spell (Upward 1999). This rule is implemented in this study using the following procedure (NB. This rule is not implemented for first wave, given the previous rules).

5a. If the labour market status reported in the individual response file started

3 interview date. If so then the last BLIFEMST employment record can be assumed to be part of the same continuous employment spell the individual was in at wave 3, in which case no CLIFEJOB information will be collected.

before the last record in the job history, then assume that this is due to recall bias and drop JOBHIST entry.

5b. If a JOBHIST spell started before previous interview month \& job status is different, assume that this individual forgot what they were doing. Drop this information (assumed recall bias).

This rule controls for what Jürges (2007) refers to as a "false negative" result: where an individual fails to report themselves as being in unemployment at a particular date when asked about it retrospectively, although they reported themselves in that state at the date of occurrence (Jürges 2007). The example in Table E.14 illustrates how this rule deals with this inconsistency in the data. The temporary job recorded in the wave 3 job history file will be dropped under the assumption that current labour market status is always correct, although in this case there is only marginal overlap of the reported spells.

Table E.14: Application of Rule 5a: An individual this rule would affect.

wave	**jbstat**	**jhstat**	**jhstpy**	**Start month**	**Interview month**	**End month**	**Previous interview month**
1	Self emp.			1985m1	1991m10		1990m9
2	Self emp.				1992m9		1991m10
3	Employed	Diff. emp.	Temp. Job	1992m8	1993m9	1993m3	1992m9
3	Unemployed			1993m3	1993m9		1992m9

Rule 5b. If a JOBHIST spell started before previous interview month \& job status is different, assume that this individual forgot what they were doing. Drop this information (assumed recall bias).

Table E.15 illustrate this rule working as per our priors. The employment spell reported in the wave 4 job history seems to be a duplicate of the employment spell recorded at wave 3. There seems to be recall error with respects to the end date of this spell. By this rule, the highlighted record will be dropped.

Table E.15: Application of Rule 5b: An individual this rule would affect.

wave	jbstat	jhstat	jhstpy	Start month	Interview month	End month	Previous interview month
3	Employed	Diff. emp.	Temp. Job	1991m6	1993m9	1993m1	1992m10
3	Employed	Diff. emp.	Temp. Job	1993m1	1993m9	1993m8	1992m10
3	Unemployed			1993m9	1993m9		1992m10
4	Employed	Diff. emp.	Temp. Job	1993m1	1994m10	1994m1	1993m9
4	Unemployed	Unemployed	N/A	1994m1	1994m10	1994m4	1993m9
4	Employed			1994m4	1994m10		1993m9

Rule 5b. If a JOBHIST spell started before previous interview month \& job status is different, assume that this individual forgot what they were doing. Drop this information (assumed recall bias).

5c. If a JOBHIST spell ended before the previous interview month, assume this is a duplicate entry. Drop the job history record. See Table E.16 for a case that this rule would affect.

Table E.16: Application of Rule 5c: An individual this rule would affect.

wave	jbstat	jhstat	jhstpy	spell start month	interview month	spell end month	previous interview month
4	Employed			1994m9	1994m11		1993m10
5	Employed	Same emp.	Promoted		1995m11	1995m10	1994m11
5	Employed			1995m10	1995m11		1994m11
6	Employed	Same emp.	Promoted		1996m11	1995m10	1995m11
6	Employed			1995m10	1996m11		1995m11

Rule 5c. If a JOBHIST spell ended before the previous interview month, assume this is a duplicate entry. Drop the job history record.

5d. If JOBHIST job status is same as reported in the INDRESP file, and spell started before previous interview month, assume that the two records are from same spell. Copy reason for leaving labour

market state back to the first instance of that state. Table E.17 is example of a case that this rule would affect. Since these rules are implemented incrementally, these cases are affected by the implementation of previous rules.

Table E.17: Application of Rule 5d: An individual this rule would affect.

wave	jbstat	jhstat	jhstpy	Start month	Interview month	End month	Previous interview month
3	Employed	Diff. emp.	Dismissed/ Sacked	1987m9	1993m10	1993m1	1992m12
3	Unemployed			1993m1	1993m10		1992m12
4	Unemployed	Unemployed	N/A	1993m1	1994m10	1994m2	1993m10
4	Employed	Diff. emp.	Temp. Job	1994m2	1994m10	1994m4	1993m10
4	Unemployed			1994m4	1994m10		1993m10
5	Unemployed	Unemployed	N/A	1994m8	1995m10	1994m11	1994m10

Rule 5d. If JOBHIST job status is same as reported in the INDRESP file, and spell started before previous interview month, assume that the two records are from same spell. Copy reason for leaving labour market state back to the first instance of that state.

5e. If the job status of the first record in job history file is different to that which was recorded at the last INDRESP interview, and the record began after previous interview month, then keep this as a new spell and assume that it started the month of the previous interview. See table E.18 for an illustration of a case that this rule will affect.

Table E.18: Application of Rule 5e: An individual this rule would affect.

wave	jbstat	jhstat	jhstpy	Start month	Interview month	End month	Previous interview month
3	Self emp.			1993m9	1993m10	1993m10	1992m10
4	Employed	Diff. emp.	Temp. Job	1994m1	1995m1	1994m7	1993m10
4	Self emp.		N/A	1994m8	1995m1	1995m1	1993m10
5	Employed	Diff. emp.	Temp. Job	1995m2	1996m1	1995m6	1995m1

Rule 5e. If the job status of the first record in job history file is different to that which was recorded at the last INDRESP interview, and the record began after previous interview month, then keep this as a new spell and assume that it started the month of the previous interview.

Rule 5e is also implemented in cases where job status changed from one INDRESP interview to another but, after the implementation of the preceding rules, we have conflicting - or missing - information about when this job change occurred. This could be the result of the choice of job status definition, or our rules, decomposing what was reported as a continuous employment spell into two separate labour market experiences. It may also be the case that an individual reported themselves as being in a different labour market state from one wave to the next, but they did not provide any information in the JOBHIST file for whatever reason. Whatever the case, in this event the end date is assumed to be the same month that the interview occurred[24]. This imputation is likely to exacerbate what is termed in the literature as the "seam problem" (Halpin 1997; Paull 2002), "the tendency for reported changes in status to bunch in the period immediately after an interview (Paull 2002, pp. 3)". An example of an individual affected by this rule is presented in Table E.19. End dates at wave 4 and 5 have been already been imputed in this instance.

Table E.19: Application of Rule 5e: An individual this rule would affect.

wave	**jbstat**	**jhstat**	**jhstpy**	**Start month**	**Interview month**	**End month**	**Previous interview month**
3	Employed			1992m2	1993m10		1992m10
4	Employed			1992m2	1994m11	1994m11	1993m10
5	Self emp.		N/A		1995m10	1995m10	1994m11
6	Employed			1992m2	1996m9		1995m10

Rule 5e. If the job status of the first record in job history file is different to that which was recorded at the last INDRESP interview, and the record began after previous interview month, then keep this as a new spell and assume that it started the month of the previous interview.

Table E.20 is an example of an individual with no job history entries, but three

[24] This assumption was made to reduce the margin for error. It could be assumed that the change happened in the following month, however, given that the underlying data is in daily format this assumption would increase the average margin for error to two months instead of one.

labour market status changes in the data. In order to gauge when each spell began, the previous spell is assumed to have ended the month of the previous interview. In the case of employment spells, 'reason for leaving job' is set to missing at the seam. For non-employment spells this is set to 'not applicable'.

Table E.20: Application of Rule 5e: An individual this rule would affect.

wave	jbstat	jhstat	jhstpy	Start month	Interview month	End month	Previous interview month
3	OLF		N/A		1993m10	1993m10	1992m10
4	Unemployed		N/A		1994m9	1994m9	1993m10
5	OLF				1995m10		1994m9
6	OLF		N/A		1996m11	1996m11	1995m10
7	Unemployed				1997m9		1996m11

Rule 5e. If the job status of the first record in job history file is different to that which was recorded at the last INDRESP interview, and the record began after previous interview month, then keep this as a new spell and assume that it started the month of the previous interview.

For subsequent spells in the JOBHIST file, if start and end dates are missing they are imputed using the chronological ordering of the data, i.e. assuming that a spell started when the previous spell ended. Spell start date information is copied over to subsequent records of the same spell, either taking into account intra-firm movements or ignoring them. If labour market status is different, the last spell in the JOBHIST file is assumed to have ended in the same month as the spell recorded in the INDRESP file started[25]. In the initial implementation we ignore intra-firm movements due to our definition of what a job is. 'Reason for leaving job' (jhstpy) is copied over to the first instance of that labour market state. Table E.21 presents an example of an individual in the data after the rules-based approach has been implemented.

[25] There are a few cases where job status changed but start date of subsequent spell in the INDRESP file is missing. To make this rule operational, spell start date is imputed using the length of current spell (cjsten) indicator provided by the BHPS. As stated previously, this assumes that missing day was 1 and missing month was July. Cases where spell start year is missing are assumed to have begun when the last spell in the JOBHIST file ended.

Table E.21: An example of an individual in the data after the rules-based approach has been implemented.

wave	jbstat	jhstat	jhstpycurr	Start month	Interview month	End month	Previous interview month
0	Employed		Promoted	1987m3		1988m2	
1	Employed	Diff. emp.	Temp. Job	1988m2	1991m12	1991m2	1990m9
1	Employed	Diff. emp.	Redundant	1991m2	1991m12	1991m6	1990m9
1	Employed	Diff. emp.	Better Job	1991m6	1991m12	1991m8	1990m9
1	Employed		Redundant	1991m8	1991m12		1990m9
2	Employed		Redundant	1991m8	1992m10		1991m12
3	Employed	Diff. emp.	Redundant	1991m8	1993m10	1993m9	1992m10
3	Self emp.		N/A	1993m9	1993m10	1993m10	1992m10
4	Employed	Diff. emp.	Temp. Job	1993m10	1995m1	1994m8	1993m10
4	Self emp.		N/A	1994m8	1995m1	1995m1	1993m10
5	Employed	Diff. emp.	Temp. Job	1995m1	1996m1	1995m6	1995m1
5	Employed	Diff. emp.	Temp. Job	1995m6	1996m1	1995m10	1995m1
5	Employed	Diff. emp.	Temp. Job	1995m10	1996m1	1995m12	1995m1
5	Self emp.			1995m12	1996m1		1995m1
6	Self emp.			1995m12	1996m9		1996m1
7	Self emp.			1995m12	1997m10		1996m9

Individuals who still have not left full-time education for the first time are dropped from the sample. There are 96 individuals at wave 1 who provided full interviews and had not left full-time education for the first time. The sample is also conditioned to exclude individuals who have not left full-time education permanently. Intra-firm movements are dropped, reason for leaving job as well as spell end dates are copied to the first occurrence of the spell in question. The data is collapsed leaving just one observation per spell. This collapsed data is used to construct spell-varying indicators, e.g. Previous labour market status and job characteristics only asked at the beginning of a job spell. The resulting file is merged with the pre-collapsed information, by spell sequence.

E.1 Linking in other information.

Time-varying regional-level information is matched in by household identifier. This identifier allows Local Authority and Travel-to-Work Area (TTWA) of residence to be identified. A one-to-one link between these geographical entities does not exist. See chapter D for how a one-to-one link between the Local Authority and TTWA levels was established. A similar approach to Upward (1999) was employed to link in both time-varying, spell-varying and time-invariant individual-level information from the BHPS. Chapter 5 can be consulted for further detail.

Appendix F

Chapter 5: Data Descriptives

Table F.1: COMPARISON OF SAMPLE INCLUDING WITH REDUCED SAMPLE EXCLUDING SELF-EMPLOYED (PAIRWISE T-TESTS FOR EQUALITY OF MEANS)

	Cont. Pres. $\geq$ 2 Waves				Cont. Pres. $\geq$ 2 Waves(§)			
	All	**Excl. †**	**t-test**	**t_prob**	**All**	**Excl. †**	**t-test**	**t_prob**
Personal Characteristics								
Age < 25	0.214	0.213	0.108	0.914	0.189	0.181	0.666	0.505
Age 25 - 29	0.152	0.155	-0.303	0.762	0.15	0.155	-0.36	0.719
Age 30 - 34	0.152	0.154	-0.155	0.877	0.157	0.16	-0.236	0.813
Age 35 - 39	0.126	0.124	0.183	0.854	0.13	0.131	-0.053	0.958
Age 40 - 44	0.131	0.129	0.175	0.861	0.137	0.136	0.12	0.904
Age > 45	0.225	0.225	0.003	0.998	0.236	0.238	-0.156	0.876
white	0.964	0.963	0.163	0.87	0.967	0.966	0.141	0.888
married1	0.682	0.682	0.037	0.971	0.704	0.709	-0.288	0.773
spouseempl d	0.509	0.514	-0.262	0.794	0.541	0.546	-0.325	0.745
children	0.371	0.365	0.41	0.682	0.375	0.373	0.085	0.933
children*married	0.365	0.359	0.392	0.695	0.369	0.368	0.061	0.951
School Type Attended								
Grammar School	0.139	0.135	0.366	0.714	0.14	0.14	-0.016	0.987
Private School	0.06	0.057	0.423	0.672	0.058	0.057	0.182	0.856
Technical	0.079	0.073	0.723	0.469	0.071	0.066	0.521	0.602
Highest Qualification								
Degree	0.12	0.122	-0.171	0.864	0.123	0.126	-0.336	0.737
Other Higher	0.22	0.214	0.398	0.691	0.227	0.223	0.328	0.743
A' Levels	0.158	0.154	0.355	0.723	0.151	0.149	0.189	0.85
O' Levels	0.223	0.226	-0.229	0.819	0.223	0.222	0.068	0.945
Apprenticeship	0.03	0.029	0.187	0.852	0.03	0.029	0.25	0.802
Other	0.074	0.077	-0.363	0.716	0.073	0.073	-0.029	0.977
Housing Tenure								
Owned	0.102	0.104	-0.168	0.867	0.106	0.105	0.073	0.942
Mortgage	0.681	0.683	-0.171	0.864	0.706	0.707	-0.028	0.978
Council tenant	0.103	0.106	-0.247	0.805	0.091	0.093	-0.25	0.802
Housing Association	0.018	0.017	0.348	0.728	0.017	0.017	-0.037	0.971
Disabled	0.012	0.012	0.005	0.996	0.01	0.01	0.071	0.943
Health limits type of work	0.064	0.062	0.207	0.836	0.058	0.06	-0.21	0.833
TTWA Unemployment Rate	0.093	0.093	0.109	0.913	0.092	0.092	0.127	0.899
Total	2140	1731			1842	1589		

§ Excluding missing real wage observations.
† - 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.

Table F.2: COMPARISON OF SAMPLE INCLUDING WITH REDUCED SAMPLE EXCLUDING SELF-EMPLOYED & PROBLEMATIC REGIONS (PAIRWISE T-TESTS FOR EQUALITY OF MEANS)

	Cont. Pres. ≥ 2 Waves				Cont. Pres. ≥ 2 Waves(§)			
	All	**Excl. †**	**t-test**	**t_prob**	**All**	**Excl. †**	**t-test**	**t_prob**
Personal Characteristics								
Age < 25	0.213	0.211	0.142	0.887	0.181	0.176	0.34	0.734
Age 25 - 29	0.155	0.154	0.078	0.937	0.155	0.154	0.041	0.967
Age 30 - 34	0.154	0.155	-0.11	0.913	0.16	0.16	-0.04	0.968
Age 35 - 39	0.124	0.12	0.359	0.719	0.131	0.127	0.341	0.733
Age 40 - 44	0.129	0.132	-0.209	0.834	0.136	0.14	-0.29	0.772
Age > 45	0.225	0.228	-0.223	0.823	0.238	0.243	-0.338	0.736
white	0.963	0.96	0.439	0.661	0.966	0.964	0.303	0.762
married1	0.682	0.679	0.185	0.853	0.709	0.708	0.06	0.952
spouseempl d	0.514	0.509	0.229	0.819	0.546	0.545	0.073	0.942
children	0.365	0.364	0.093	0.926	0.373	0.373	0.011	0.991
children*married	0.359	0.356	0.166	0.869	0.368	0.367	0.073	0.942
School Type Attended								
Grammar School	0.135	0.142	-0.531	0.595	0.14	0.147	-0.582	0.56
Private School	0.057	0.058	-0.112	0.911	0.057	0.058	-0.194	0.846
Technical	0.073	0.074	-0.171	0.864	0.066	0.067	-0.074	0.941
Highest Qualification								
Degree	0.122	0.127	-0.433	0.665	0.126	0.131	-0.378	0.705
Other Higher	0.214	0.212	0.164	0.87	0.223	0.221	0.113	0.91
A' Levels	0.154	0.152	0.207	0.836	0.149	0.148	0.078	0.938
O' Levels	0.226	0.227	-0.014	0.989	0.222	0.222	0.023	0.982
Apprenticeship	0.029	0.026	0.501	0.616	0.029	0.025	0.598	0.55
Other	0.077	0.078	-0.05	0.96	0.073	0.072	0.089	0.929
Housing Tenure								
Owned	0.104	0.108	-0.37	0.711	0.105	0.111	-0.467	0.641
Mortgage	0.683	0.685	-0.09	0.929	0.707	0.708	-0.096	0.924
Council tenant	0.106	0.097	0.824	0.41	0.093	0.084	0.896	0.37
Housing Association	0.017	0.018	-0.17	0.865	0.017	0.018	-0.135	0.893
Disabled	0.012	0.014	-0.467	0.641	0.01	0.012	-0.372	0.71
Health limits type of work	0.062	0.068	-0.641	0.521	0.06	0.065	-0.601	0.548
TTWA Unemployment Rate	0.093	0.093	-0.031	0.975	0.092	0.092	-0.145	0.885
Total	1731	1425			1589	1303		

§ Excluding missing real wage observations.
† - 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.

Table F.3: Current Employer Tenure, by previous labour market status. 1991-1997,Unrestricted (full sample).

	Previous Status (unrestricted)			
Employer Tenure	**Employed**	**Unemployed**	**OLF**	**Total**
<1 year	631	348	95	1074
1-2 years	519	241	92	852
2-3 years	462	166	86	714
3-4 years	423	124	73	620
4-5 years	357	104	68	529
5-10 years	1448	347	261	2056
>10 years	2032	376	880	3288

Previous labour market states: Employment/Self-Employment; Unemployment; OLF (Out of the Labour Force).

Table F.4: Current Employer Tenure, by previous labour market status: 1991-1997, Only last 5 years matters (full sample).

	Previous Status (unrestricted)			
Employer Tenure	**Employed**	**Unemployed**	**OLF**	**Total**
$¡$1 year	631	348	95	1074
1-2 years	519	241	92	852
2-3 years	462	166	86	714
3-4 years	423	124	73	620
4-5 years	357	104	68	529
5-10 years	2041	9	6	2056
$¿$10 years	3288	0	0	3288

Previous labour market states: Employment/Self-Employment; Unemployment; OLF (Out of the Labour Force).

Table F.5: Male sub-sample by previous labour market status, 1991-2001.

	Unrestricted.				Restricted.			
PREV_STAT:	EMP.		NON-EMP.		EMP.		NON-EMP.	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Personal Characteristics								
Age < 25	0.06	0.05	0.17	0.16	0.06	0.05	0.33	0.32
Age 25 - 29	0.11	0.11	0.15	0.14	0.12	0.12	0.16	0.15
Age 30 - 34	0.15	0.15	0.15	0.15	0.16	0.16	0.10	0.10
Age 35 - 39	0.16	0.16	0.14	0.14	0.16	0.16	0.10	0.10
Age 40 - 44	0.15	0.15	0.12	0.12	0.15	0.15	0.08	0.08
Age > 45	0.37	0.37	0.26	0.28	0.35	0.35	0.24	0.24
white	0.97	0.97	0.97	0.97	0.97	0.97	0.96	0.96
married1	0.80	0.80	0.67	0.68	0.79	0.79	0.53	0.55
spouseempl d	0.62	0.62	0.49	0.51	0.61	0.61	0.39	0.41
children	0.39	0.38	0.37	0.37	0.41	0.40	0.26	0.26
School Type Attended								
Grammar School	0.14	0.15	0.14	0.14	0.15	0.15	0.11	0.11
Private School	0.06	0.06	0.06	0.06	0.06	0.06	0.05	0.04
Technical	0.07	0.08	0.07	0.07	0.07	0.07	0.10	0.10
Highest Qualification								
Degree	0.16	0.16	0.17	0.17	0.16	0.16	0.17	0.17
Other Higher	0.32	0.32	0.25	0.25	0.31	0.30	0.24	0.24
A' Levels	0.12	0.12	0.18	0.17	0.13	0.13	0.19	0.17
O' Levels	0.17	0.17	0.19	0.20	0.18	0.18	0.19	0.19
Other	0.06	0.06	0.07	0.06	0.06	0.06	0.07	0.07
Apprenticeship	0.03	0.03	0.01	0.01	0.02	0.02	0.02	0.02
Housing Tenure								
Owned	0.13	0.13	0.13	0.14	0.13	0.13	0.17	0.17
Mortgage	0.72	0.73	0.67	0.68	0.73	0.73	0.56	0.57
Council tenant	0.06	0.05	0.09	0.08	0.06	0.05	0.12	0.11
Housing Association	0.02	0.02	0.03	0.03	0.02	0.02	0.04	0.05
Health limits type of work	0.06	0.07	0.07	0.08	0.06	0.07	0.09	0.10
Disabled	0.01	0.01	0.02	0.02	0.01	0.01	0.02	0.03
Workplace Characteristics								
Public Sector	0.03	0.03	0.02	0.02	0.03	0.03	0.01	0.01
Public Services	0.20	0.19	0.21	0.23	0.22	0.21	0.15	0.16
Charity	0.02	0.02	0.02	0.02	0.02	0.02	0.03	0.02
Other Sector	0.02	0.02	0.01	0.02	0.02	0.02	0.01	0.01
Sector Missing	0.00	0.00	0.01	0.00	0.00	0.00	0.01	0.01
Workplace Size								
50 - 99	0.15	0.15	0.12	0.13	0.14	0.14	0.13	0.13
100 - 199	0.13	0.13	0.11	0.11	0.13	0.12	0.10	0.11
> 200	0.37	0.37	0.37	0.38	0.38	0.39	0.27	0.26
Workplace Union Presence	0.55	0.55	0.53	0.53	0.58	0.58	0.34	0.35
Union Member	0.38	0.39	0.36	0.35	0.42	0.41	0.15	0.15
Current job is part-time	0.02	0.01	0.08	0.07	0.02	0.01	0.15	0.15
Contract								
Current job is temporary	0.04	0.04	0.13	0.11	0.04	0.04	0.24	0.22
Occupation								
Skilled Non-Manual	0.29	0.29	0.27	0.27	0.29	0.29	0.25	0.26
Unskilled Manual	0.14	0.14	0.18	0.19	0.14	0.14	0.25	0.25
Non-manual	0.26	0.25	0.31	0.30	0.27	0.27	0.30	0.30
Professional/ Managerial	0.28	0.29	0.21	0.21	0.27	0.28	0.15	0.15
Cumulative Employment Experience	271.54	273.41	218.66	222.62	267.35	269.45	175.51	175.91
Current Spell Length (months)	111.39	110.45	121.39	124.93	131.08	131.94	22.81	23.05
Industry								
Energy & Water Supplies	0.04	0.04	0.04	0.04	0.04	0.04	0.01	0.01
Extraction of Metals, etc. Manufacture of Metals	0.05	0.05	0.05	0.04	0.05	0.05	0.03	0.02
Metal goods, engineering & Vehicles	0.16	0.16	0.15	0.16	0.16	0.17	0.12	0.12
Other Manufacturing	0.12	0.12	0.12	0.12	0.11	0.12	0.14	0.14
Construction	0.04	0.05	0.04	0.04	0.04	0.05	0.04	0.04
Distribution, Hotels & Catering, Repairs	0.13	0.13	0.17	0.15	0.12	0.12	0.24	0.21
Transport & Communications	0.10	0.10	0.06	0.07	0.09	0.09	0.06	0.07
Banking, Finance, etc.	0.12	0.13	0.11	0.12	0.12	0.12	0.12	0.13
Other Services	0.22	0.21	0.24	0.25	0.23	0.23	0.22	0.23
Deflated Real Wage (jbhrs)	10.69	10.72	9.38	9.37	10.76	10.76	7.19	7.21
New Deflated Real Wage 1 (jbhrs + jbotpd)	10.07	10.13	8.81	8.82	10.14	10.17	6.72	6.74
New Deflated Real Wage 2 (jbhrs + 1.5*jbotpd)	9.87	10.14	8.64	8.70	9.93	10.14	6.61	6.72
Usual hours worked	39.78	39.84	37.47	37.52	39.51	39.55	35.96	36.03
Usual paid overtime hours	3.22	3.17	2.76	2.74	3.15	3.09	2.56	2.58
TTWA Unemployment Rate	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0.09
Total	9350	7185	4877	3947	12095	9460	2141	1672

Unrestricted version considers full labour market history since leaving full-time education. Restricted version only considers disruptions which occured in the last 5 years of labour market history when constructing previous labour market history. Specification [2], [4], [6] & [8] are from the sample used in the Wage analysis which excludes the problematic regions defined in Table **??**.

Table F.6: Reason for leaving previous job by previous status, 1991-1997.

Reason for leaving previous job	Prev. Status Unrestricted				Prev. Status Restricted			
	Prev. EMP		Prev. NON-EMP		Prev. EMP		Prev. NON-EMP	
	Obs.	%	Obs.	%	Obs.	%	Obs.	%
Redundancy	797	12.30	735	21.0	1,032	12.4	501	30.36
Sacked/ Dismissed	78	1.20	93	2.66	105	1.25	66	4.0
Temporary Job Ended	170	2.62	202	5.78	225	2.70	147	8.91
Voluntary Quit	4,245	6.54	256	7.32	4,383	52.54	118	7.15
Missing	714	10.99	200	5.72	736	8.82	178	10.79
Other Reason	490	7.55	420	1.20	664	7.94	246	14.91
Not Applicable	0	0.00	1,591	45.50	1,197	14.35	394	23.88
Total	6494		3497		8342		1650	

Sample selection: Individuals never in self-employment at interview date.

Table F.7: Reason for leaving previous job, 1991-2001.

Year	missing	na	voluntary quit	redundant	sacked/ dis-missed	temp. job ended	other	Total
1991	0.130	0.197	0.349	0.143	0.020	0.039	0.120	2147
1992	0.144	0.163	0.342	0.162	0.022	0.043	0.125	2147
1993	0.088	0.146	0.367	0.181	0.022	0.049	0.147	1878
1994	0.088	0.131	0.369	0.180	0.025	0.049	0.157	1741
1995	0.088	0.123	0.382	0.174	0.025	0.044	0.165	1608
1996	0.082	0.114	0.380	0.174	0.023	0.048	0.179	1549
1997	0.080	0.106	0.382	0.174	0.018	0.053	0.188	1493
1998	0.078	0.099	0.380	0.172	0.021	0.055	0.195	1428
1999	0.077	0.092	0.383	0.169	0.021	0.052	0.207	1374
2000	0.074	0.087	0.383	0.167	0.020	0.050	0.220	1327
2001	0.077	0.086	0.376	0.163	0.018	0.048	0.232	1256
Total	1707	2297	6637	3022	387	853	3045	17948

Figures are percentages of Wave, not reason, total.

Appendix G

Chapter 5: Sensitivity Analysis

G.1 F/T (Higher) Education as a separate labour market state.

As a sensitivity check, rather than treating full-time education as “Out of the Labour Force”, I treat full-time (higher) education as a productive investment in human capital. Thus the impact of unemployment and inactivity is assessed relative to a base group which includes employment *and* full-time education.

Table G.1: Log Real Hourly Wage Equations for Male Sub-Sample, 1991-1997: Individual-level observed heterogeneity, F/T (Higher) Education as a separate labour market state, Previous Status unrestricted.

	1991-1997		1991-2001	
	[1]	[2]	[3]	[4]
constant	1.155**	1.113**	1.129**	1.104**
	(0.150)	(0.149)	(0.122)	(0.122)
Tenure in current employment.				
base is <1 year.				
1-2 years	0.019	0.042**	0.021*	0.036**
	(0.013)	(0.015)	(0.011)	(0.012)
2-3 years	0.020	0.035**	0.033**	0.036**
	(0.015)	(0.016)	(0.013)	(0.013)
3-4 years	0.037**	0.063**	0.058**	0.069**
	(0.017)	(0.018)	(0.014)	(0.014)
4-5 years	0.055**	0.077**	0.071**	0.085**
	(0.019)	(0.021)	(0.015)	(0.016)
5-10 years	0.059**	0.080**	0.078**	0.092**
	(0.019)	(0.021)	(0.016)	(0.017)
10 years +	0.122**	0.141**	0.116**	0.129**
	(0.027)	(0.028)	(0.022)	(0.023)
Previous labour market status.				
Inactivity	-0.059		-0.059	
	(0.048)		(0.040)	
Unemployment	-0.077**		-0.093**	
	(0.028)		(0.023)	
Time since interruption (ref. Previous Employment)				
Unemployment.				

Continued on next page

Table G.1 – continued from previous page

	1991-1997		1991-2001	
	[1]	[2]	[3]	[4]
< 1 year		-0.041		-0.069**
		(0.033)		(0.027)
1-2 years		-0.100**		-0.109**
		(0.032)		(0.028)
2-3 years		-0.104**		-0.094**
		(0.041)		(0.033)
3-4 years		-0.098**		-0.090**
		(0.043)		(0.034)
4 years +		-0.106**		-0.114**
		(0.040)		(0.034)
Inactivity.				
< 1 year		0.013		-0.021
		(0.059)		(0.051)
1-2 years		-0.092*		-0.088*
		(0.054)		(0.048)
2-3 years		-0.004		0.010
		(0.070)		(0.061)
3-4 years		-0.169**		-0.113**
		(0.062)		(0.055)
4 years +		-0.127*		-0.088
		(0.077)		(0.056)
N	7666	7666	10912	10912
LL	1403	1417	1438	1446
$\bar{R}^2$	0.365	0.367	0.511	0.512
RMS error	0.203	0.202	0.213	0.213
AIC	-2652.290	-2664.199	-2703.298	-2704.725

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

The average effect of an unemployment spell seems to be robust to classifying full-time education as a separate (productive) 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.

Table G.2: Log Real Hourly Wage Equations for Male Sub-Sample: Individual-level observed heterogeneity, F/T (Higher) Education as a separate labour market state, Previous Status unrestricted.

	1991-1997			1991-2001		
	[1]	[2]	[3]	[4]	[5]	[6]
Reason for leaving job§.						
Redundant	-0.062			-0.064**		

Continued on next page

Table G.2 – continued from previous page

	1991-1997			1991-2001		
	[1]	[2]	[3]	[4]	[5]	[6]
	(0.039)			(0.029)		
Previous Status (ref. Previous Employment.						
Unemployment	-0.085**	-0.051	-0.124**	-0.091**	-0.077**	-0.134**
	(0.041)	(0.033)	(0.035)	(0.031)	(0.028)	(0.030)
Inactivity	0.013	-0.032	-0.050	-0.025	0.024	-0.077*
	(0.066)	(0.088)	(0.052)	(0.049)	(0.071)	(0.045)
Reason for leaving job by previous labour market status (ref. previous employment/no interruption).						
prev_unemp_redundant1	0.086			0.072		
	(0.066)			(0.054)		
prev_unemp_redundant_45p1	-0.064			-0.076		
	(0.055)			(0.048)		
prev_olf_redundant1	-0.196			-0.055		
	(0.140)			(0.102)		
prev_olf_redundant_45p1	0.231			0.186		
	(0.222)			(0.214)		
Length of previous interruption (ref. < 6 months.						
prev_unemp_duration_6to12mnth1a		-0.094*			-0.062	
		(0.049)			(0.039)	
prev_unemp_duration_12p1a		-0.025			-0.020	
		(0.073)			(0.053)	
prev_olf_duration_6to12mnth1a		-0.077			-0.194**	
		(0.116)			(0.092)	
prev_olf_duration_12p1a		-0.014			-0.077	
		(0.116)			(0.092)	
Number of previous unemployment spells (>1).						
Unemployment						
oneplus_ue_spell			-0.070			-0.056
			(0.060)			(0.047)
prev_unemp_oneplus_ue1			0.099*			0.091**
			(0.057)			(0.044)
prev_unemp_oneplus_olf1			0.132			0.070
			(0.093)			(0.081)
Out of the Labour Force						
oneplus_olf_spell			0.088			0.075
			(0.105)			(0.081)
prev_olf_oneplus_ue1			0.003			0.039
			(0.151)			(0.102)
prev_olf_oneplus_olf1			-0.050			0.042
			(0.129)			(0.100)
N	7666	7666	7666	10912	10912	10912
LL	1427	1409	1416	1462	1450	1454
$\bar{R}^2$	0.368	0.366	0.367	0.513	0.512	0.512
RMS error	0.202	0.202	0.202	0.213	0.213	0.213
AIC	-2.7e+03	-2.7e+03	-2.7e+03	-2.7e+03	-2.7e+03	-2.7e+03

Sample selection: Individuals never in self-employment at interview date. Number of person-years: 16011 (combined sample); 8751 (females); 7440 (males). 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.
See Section **??** for the full results.
* p<0.10, ** p<0.05, *** p<0.01

Self-Employment

The general robustness of the results in the main analysis is further reflected when the self-employed are included in the analysis and this treated as a separate previous labour market state. However, the penalty associated with previous self-employment is only temporary in nature. Moreover, the general story is invariant to restricting the sample to the Original Sample Members (under the assumption that attrition is random) although the penalty associated with previous inactivity is less robust.

Table G.3: Log Real Hourly Wage Equations for Male Sub-Sample, Self-Employment, unrestricted previous status interacted with Local Authority Unemployment level.

	1991-1997		1991-2001	
	[1] (SE)	[2] (SE)	[3] (SE)	[4] (SE)
constant	0.956**	0.916**	0.961**	0.948**
	(0.172)	(0.174)	(0.115)	(0.115)
Tenure in current employment. base is <1 year.				
1-2 years	0.013	0.032**	0.015	0.026**
	(0.012)	(0.014)	(0.010)	(0.011)
2-3 years	0.014	0.020	0.026**	0.025**
	(0.014)	(0.015)	(0.012)	(0.013)
3-4 years	0.039**	0.055**	0.058**	0.065**
	(0.015)	(0.018)	(0.012)	(0.014)
4-5 years	0.052**	0.061**	0.069**	0.070**
	(0.017)	(0.019)	(0.014)	(0.015)
5-10 years	0.055**	0.064**	0.081**	0.083**
	(0.017)	(0.019)	(0.015)	(0.017)
10 years +	0.109**	0.118**	0.109**	0.112**
	(0.025)	(0.026)	(0.020)	(0.021)
Previous labour market status (ref. Employment/ Full-time education).				
Inactivity	-0.077*		-0.088**	
	(0.045)		(0.035)	
Unemployment	-0.096**		-0.100**	
	(0.026)		(0.021)	
Self-Employment	-0.065		0.008	
	(0.045)		(0.032)	
Time since interruption (ref. Previous Employment/ Full-time education				
Unemployment.				
< 1 year		-0.073**		-0.085**
		(0.030)		(0.024)
1-2 years		-0.122**		-0.115**
		(0.030)		(0.026)
2-3 years		-0.116**		-0.105**
		(0.038)		(0.029)
3-4 years		-0.109**		-0.100**
		(0.039)		(0.030)
4 years +		-0.108**		-0.119**
		(0.037)		(0.031)
Inactivity.				
< 1 year		-0.008		-0.066
		(0.056)		(0.047)
1-2 years		-0.119**		-0.115**
		(0.054)		(0.044)
2-3 years		-0.054		-0.036
		(0.068)		(0.052)
3-4 years		-0.205**		-0.159**
		(0.060)		(0.051)
4 years +		-0.128*		-0.100**
		(0.068)		(0.049)
Self-Employment.				
< 1 year		-0.191**		-0.121**
		(0.068)		(0.059)
1-2 years		-0.069		-0.004
		(0.047)		(0.036)
2-3 years		0.013		0.025
		(0.050)		(0.042)
3-4 years		-0.015		0.006
		(0.048)		(0.041)
4 years +		0.012		0.108**
		(0.062)		(0.053)
N	9245	9245	13295	13295
LL	1525	1547	1345	1367
$\bar{R}^2$	0.356	0.358	0.500	0.501
RMS error	0.206	0.206	0.219	0.219
AIC	-2894.480	-2913.032	-2516.168	-2535.829

† Tight labour market - Vacancies/Unemployment ratio > Median.
Sample selection: Individuals never in self-employment at interview date. Number of person-years: 16011 (combined sample); 8751 (females); 7440 (males). 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.
Significance levels: ***: 1% **: 5% *: 10%
Note: for regional dummy results see Figure 4.1(Cox model B only)
See Section **??** for the full results.

Table G.4: Log Real Hourly Wage Equations for Male Sub-Sample, Self-Employment, unrestricted previous status interacted with Local Authority Unemployment level.

	1991-1997			1991-2001		
	[1] (SE)	[2] (SE)	[3] (SE)	[4] (SE)	[5] (SE)	[6] (SE)
Reason for leaving job§.						
redundant	-0.052			-0.051**		
	(0.036)			(0.026)		
Previous labour market status (ref. Employment/ Full-time education).						
Unemployment						
prev_u1	-0.104**	-0.080**	-0.141**	-0.088**	-0.088**	-0.146**
	(0.035)	(0.031)	(0.032)	(0.026)	(0.025)	(0.027)
Inactivity						
prev_inactivity1	0.026	-0.083	-0.047	-0.034	-0.022	-0.067*
	(0.063)	(0.095)	(0.048)	(0.043)	(0.065)	(0.040)
Self-Employment						
prev_semp1	-0.079	-0.163	-0.055	-0.002	-0.019	0.005
	(0.057)	(0.117)	(0.051)	(0.044)	(0.083)	(0.038)
Previous labour market status by reason for leaving previous job (ref. Employment/ Full-time education).						
Unemployment						
Redundant						
prev_unemp_redundant1	0.069			0.042		
	(0.060)			(0.046)		
prev_unemp_redundant_45p1	-0.057			-0.080*		
	(0.051)			(0.045)		
Inactivity						
Redundant						
prev_olf_redundant1	-0.322*			-0.136		
	(0.172)			(0.110)		
prev_olf_redundant_45p1	0.309			0.289*		
	(0.213)			(0.165)		
Self-Employment						
Redundant						
prev_semp_redundant1	-0.034			0.044		
	(0.135)			(0.103)		
prev_semp_redundant_45p1	-0.097			-0.098		
	(0.138)			(0.120)		
Length of previous interruption (ref. < 6 months.						
prev_unemp_duration_6to12mnth1a		-0.096**			-0.073**	
		(0.045)			(0.035)	
prev_unemp_duration_12p1a		0.030			0.020	
		(0.064)			(0.048)	
prev_olf_duration_6to12mnth1a		-0.096			-0.191**	
		(0.124)			(0.083)	
prev_olf_duration_12p1a		0.071			-0.034	
		(0.116)			(0.079)	
prev_semp_duration_6to12mnth1a		0.192			0.073	
		(0.136)			(0.098)	
prev_semp_duration_12p1a		0.097			0.021	
		(0.124)			(0.090)	
Number of previous unemployment spells (>1).						
Unemployment						
oneplus_ue_spell			-0.110**			-0.076*
			(0.055)			(0.043)
prev_unemp_oneplus_ue1			0.101*			0.092**
			(0.054)			(0.041)
prev_olf_oneplus_ue1			-0.216			-0.033
			(0.202)			(0.097)
prev_semp_oneplus_ue1			0.054			0.054
			(0.105)			(0.072)
N	9245	9245	9245	13295	13295	13295
LL	1569	1541	1550	1385	1365	1365
$\bar{R}^2$	0.361	0.358	0.359	0.503	0.501	0.501
RMS error	0.205	0.206	0.206	0.219	0.219	0.219
AIC	-3.0e+03	-2.9e+03	-2.9e+03	-2.6e+03	-2.5e+03	-2.5e+03

† Tight labour market - Vacancies/Unemployment ratio > Median.
Sample selection: Individuals never in self-employment at interview date. Number of person-years: 16011 (combined sample); 8751 (females); 7440 (males). 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.
Significance levels: ***: 1% **: 5% *: 10%
Note: for regional dummy results see Figure 4.1(Cox model B only)
See Section ?? for the full results.

Continuously Present Definition

Table G.5: Log Real Hourly Wage Equations for Male Sub-Sample, Continuously Present (random attrition), unrestricted previous status interacted with Local Authority Unemployment level.

	1991-1997		1991-2001	
	[1] (SE)	[2] (SE)	[3] (SE)	[4] (SE)
constant	1.265**	1.205**	1.236**	1.207**
	(0.129)	(0.131)	(0.119)	(0.120)
Tenure in current employment. base is <1 year.				
1-2 years	0.024**	0.042**	0.026**	0.036**
	(0.012)	(0.014)	(0.010)	(0.011)
2-3 years	0.021	0.035**	0.045**	0.050**
	(0.014)	(0.016)	(0.012)	(0.014)
3-4 years	0.038**	0.055**	0.056**	0.060**
	(0.016)	(0.019)	(0.013)	(0.015)
4-5 years	0.051**	0.072**	0.070**	0.082**
	(0.018)	(0.020)	(0.015)	(0.016)
5-10 years	0.058**	0.077**	0.076**	0.087**
	(0.018)	(0.020)	(0.016)	(0.017)
10 years +	0.112**	0.130**	0.107**	0.119**
	(0.026)	(0.027)	(0.022)	(0.023)
Previous labour market status (ref. Employment/ Full-time education).				
prev_inactivity1	-0.118**		-0.077*	
	(0.048)		(0.039)	
prev_u1	-0.100**		-0.094**	
	(0.027)		(0.022)	
Time since interruption (ref. Previous Employment Unemployment				
< 1 year		-0.069**		-0.076**
		(0.029)		(0.024)
1-2 years		-0.118**		-0.105**
		(0.031)		(0.028)
2-3 years		-0.126**		-0.102**
		(0.039)		(0.030)
3-4 years		-0.117**		-0.083**
		(0.041)		(0.033)
4 years +		-0.131**		-0.113**
		(0.040)		(0.035)
Inactivity				
< 1 year		-0.067		-0.054
		(0.058)		(0.051)
1-2 years		-0.137**		-0.089**
		(0.055)		(0.045)
2-3 years		-0.094		-0.040
		(0.064)		(0.057)
3-4 years		-0.166**		-0.087*
		(0.059)		(0.049)
4 years +		-0.174**		-0.104**
		(0.059)		(0.048)
N	6703	6703	8983	8983
LL	2262	2270	2374	2378
$\bar{R}^2$	0.421	0.422	0.579	0.579
RMS error	0.174	0.174	0.187	0.187
AIC	-4369.596	-4369.464	-4575.624	-4568.140

† Tight labour market - Vacancies/Unemployment ratio > Median.
Sample selection: Individuals never in self-employment at interview date. Number of person-years: 16011 (combined sample); 8751 (females); 7440 (males). 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.
Significance levels: ***: 1% **: 5% *: 10%
Note: for regional dummy results see Figure 4.1(Cox model B only)
See Section **??** for the full results.

Table G.6: Log Real Hourly Wage Equations for Male Sub-Sample, Continuously Present (random attrition), unrestricted previous status interacted with Local Authority Unemployment level.

	1991-1997			**1991-2001**		
	[1] (SE)	[2] (SE)	[3] (SE)	[4] (SE)	[5] (SE)	[6] (SE)
Reason for leaving job§.						
redundant	-0.078**			-0.063**		
	(0.036)			(0.030)		
Previous labour market status (ref. Employment/ Full-time education).						
Unemployment						
prev_u1	-0.130**	-0.079**	-0.163**	-0.086**	-0.084**	-0.132**
	(0.041)	(0.033)	(0.034)	(0.032)	(0.027)	(0.030)
Inactivity						
prev_inactivity1	-0.098	-0.073	-0.143**	-0.071	0.045	-0.118**
	(0.062)	(0.089)	(0.054)	(0.048)	(0.066)	(0.047)
Previous labour market status by reason for leaving previous job (ref. Employment/ Full-time education).						
Unemployment						
Redundant						
prev_unemp_redundant1	0.133**			0.044		
	(0.063)			(0.055)		
prev_unemp_redundant_45p1	-0.046			-0.043		
	(0.052)			(0.045)		
Inactivity						
Redundant						
prev_olf_redundant1	-0.068			0.021		
	(0.132)			(0.108)		
prev_olf_redundant_45p1	0.184			0.101		
	(0.233)			(0.244)		
Length of previous interruption (ref. < 6 months.						
prev_unemp_duration_6to12mnth1a		-0.071			-0.018	
		(0.046)			(0.038)	
prev_unemp_duration_12p1a		-0.034			-0.045	
		(0.064)			(0.052)	
prev_olf_duration_6to12mnth1a		-0.062			-0.230**	
		(0.116)			(0.090)	
prev_olf_duration_12p1a		-0.066			-0.148*	
		(0.113)			(0.080)	
Number of previous unemployment spells (>1).						
Unemployment						
oneplus_ue_spell			-0.103*			-0.074
			(0.054)			(0.048)
prev_unemp_oneplus_ue1			0.134**			0.094**
			(0.053)			(0.045)
prev_olf_oneplus_ue1			0.148			0.071
			(0.104)			(0.093)
N	6703	6703	6703	8983	8983	8983
LL	2282	2266	2283	2390	2391	2388
$\bar{R}^2$	0.424	0.422	0.425	0.580	0.580	0.580
RMS error	0.173	0.174	0.173	0.186	0.186	0.186
AIC	-4.4e+03	-4.4e+03	-4.4e+03	-4.6e+03	-4.6e+03	-4.6e+03

† Tight labour market - Vacancies/Unemployment ratio > Median.
Sample selection: Individuals never in self-employment at interview date. Number of person-years: 16011 (combined sample); 8751 (females); 7440 (males). 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.
Significance levels: ***: 1% **: 5% *: 10%
Note: for regional dummy results see Figure 4.1(Cox model B only)
See Section **??** for the full results.

G.2 Labour market history considered

The existing literature differs in the labour market history considered when constructing key indicators of interest. In this section I test the robustness of the main results to the assumption that only the last five years of labour market history matter for current wage profiles (I do not treat full-time education as a separate state in this analysis).

Table G.7: Current Employer Tenure, by previous labour market status: Only last 5 years matters (§).

	Previous Status (unrestricted)			
Employer Tenure	Employed	Unemployed	OLF	Total
<1 year	549	295	79	923
1-2 years	449	200	86	735
2-3 years	395	140	75	610
3-4 years	357	104	62	523
4-5 years	290	88	58	436
5-10 years	1681	8	5	1694
>10 years	2745	0	0	2745

Previous labour market states: Employment/Self-Employment; Unemployment; OLF (Out of the Labour Force).
§ - 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.

In the restricted approach only the last five years was considered when constructing the previous labour market status variable as well as key indicators. If a previous non-employment spell occurred more than five years ago, it is ignored and previous status is set to employment. The rationale for this is that, if faced with retrospective information for the last five years only, as is the case with the Displaced Workers' Survey in the US, then this strategy would seem appropriate as it makes the analysis more comparable to studies face with this restriction. Another way to look at it would be a test for the hypothesis that spells which happened more than five years ago do not matter for current wage growth. A common approach in the literature is to restrict attention to labour market history over the last 5/6 years. The indicator used in Table G.7 closely reflects the pattern of cjsten-based tenure indicator used in Arulampalam (2001), Table 2. Differences can be attributed to Arulampalam (2001) using the self-reported spell length variable ("cjsten" - reported in days) instead of a direct measure of length of time in the current labour market state. The self-reported indicator is likely to suffer from substantial measurement error due to recall bias. However, based on model performance (AIC criterion) I adopt the indicator in Table 5.1 in the main analysis.

Table G.8: Log Real Hourly Wage Equations for Male Sub-Sample: Individual-level observed heterogeneity, Previous Status last 5 years only.

	1991-1997		1991-2001	
	[1]	[2]	[3]	[4]
constant	1.156**	1.141**	1.131**	1.133**
	(0.151)	(0.153)	(0.121)	(0.122)
Tenure in current employment. base is <1 year.				
1-2 years	0.020	0.036**	0.022**	0.032**
	(0.013)	(0.015)	(0.011)	(0.012)
2-3 years	0.022	0.029*	0.034**	0.031**
	(0.015)	(0.016)	(0.013)	(0.013)
3-4 years	0.039**	0.049**	0.060**	0.057**
	(0.017)	(0.018)	(0.014)	(0.014)
4-5 years	0.058**	0.065**	0.073**	0.073**
	(0.019)	(0.020)	(0.015)	(0.016)
5-10 years	0.049**	0.054**	0.064**	0.065**
	(0.019)	(0.020)	(0.016)	(0.016)
10 years +	0.114**	0.119**	0.105**	0.105**
	(0.028)	(0.028)	(0.022)	(0.022)
Previous labour market status.				
Inactivity	-0.056		-0.056*	
	(0.037)		(0.033)	
Unemployment	-0.055**		-0.063**	
	(0.024)		(0.019)	
Time since interruption (ref. Previous Employment Unemployment.				
<1 year		-0.035		-0.059**
		(0.031)		(0.025)
1-2 years		-0.079**		-0.087**
		(0.029)		(0.026)
2-3 years		-0.074**		-0.063**
		(0.037)		(0.029)
3-4 years		-0.055		-0.046*
		(0.036)		(0.027)
4 years +		-0.055		-0.058*
		(0.045)		(0.033)
Inactivity.				
<1 year		-0.037		-0.058
		(0.058)		(0.049)
1-2 years		-0.082		-0.089*
		(0.053)		(0.047)
2-3 years		-0.017		-0.008
		(0.055)		(0.049)
3-4 years		-0.080*		-0.054
		(0.042)		(0.040)
4 years +		-0.066		-0.060
		(0.053)		(0.051)
N	7666	7666	10912	10912
LL	1398	1402	1422	1426
$\bar{R}^2$	0.364	0.364	0.510	0.510
RMS error	0.203	0.203	0.213	0.213
AIC	-2642.862	-2634.511	-2671.591	-2663.862

Sample selection: Individuals never in self-employment at interview date. Number of person-years: 16011 (combined sample); 8751 (females); 7440 (males). 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.
See Section **??** for the full results.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

G.2.1 1991-1997 vs. 1992-2001

Table G.8 restricts previous labour market status to the last five years. This specification investigates whether this restriction has a sizable impact on the existing results. The Akaike Information Criterion (AIC) suggest that this specification does not perform as well as a specification which considers previous labour market status since leaving full-time education.

Whereas the specification which considered all information since leaving full-time education found the average wage penalty associated with a previous spell out of the labour force to be 11.6%, the impact of coming into the current employment spell via an OLF spell that occurred within the last five years is estimated to be substantially lower at 5.6%. However, for the 1991 to 1997 period, the impact of previous inactivity is no longer economically significant at conventional levels. In comparison, experiencing a spell of unemployment in the last five years carries forward to a 5.5% wage penalty in the subsequent job, significant at the 5% level. Specification [2], Table G.8 shows how wages recover on the current job. The long-term wage penalty due to previous unemployment is not robust to the retrospective window considered. In this specification, coming into the current employment spell via a spell of unemployment carries an insignificant wage penalty of 3.5% in the first year of employment. This penalty increases to 7.9% in the second year and 7.4% in the third year, both significant at the 5% level. However after 4 years the 5.5% long-run penalty that remains, relative to individuals who came into their current employment spell via a direct job-to-job transition, is poorly measured. Furthermore, the OLF category is imprecisely measured which is likely due to heterogeneity within this labour market state.

One would expect that disruptions that happened further in the past would have less of an impact on current incomes than those that happened recently. However, this result could be explained due to it being measured relative to the baseline group of individuals that *never* experienced an interruption over the period of investigation. If the wage penalty due to displacement is only temporary then one could expect fast catchup of wages of the displaced and baseline groups. However, if the penalty is persistent then the gap in earnings would remain and could possibly even increase over time.

Table G.9: Log Real Hourly Wage Equations for Male Sub-Sample: Individual-level observed heterogeneity, Previous Status last 5 years only.

	1991-1997			1991-2001		
	[1]	[2]	[3]	[4]	[5]	[6]
Reason for leaving job§.						
Redundant	-0.017			-0.037**		
	(0.023)			(0.019)		
Previous Status (ref. Previous Employment.						
Unemployment	-0.069*	-0.043	-0.073**	-0.061**	-0.052**	-0.079**
	(0.036)	(0.029)	(0.025)	(0.026)	(0.023)	(0.020)
Inactivity	-0.017	-0.074	-0.061	-0.024	0.028	-0.065
	(0.042)	(0.075)	(0.046)	(0.038)	(0.068)	(0.043)
Reason for leaving job by previous labour market status (ref. previous employment/no interruption).						
prev_unemp_redundant3	0.051			0.036		
	(0.053)			(0.044)		
prev_unemp_redundant_45p3	-0.030			-0.049		
	(0.053)			(0.046)		
prev_olf_redundant3	-0.209			-0.080		
	(0.128)			(0.089)		
prev_olf_redundant_45p3	0.242			0.164		
	(0.233)			(0.169)		
Length of previous interruption (ref. < 6 months.						
prev_unemp_duration_6to12mnth3a		-0.071			-0.052	
		(0.044)			(0.035)	
prev_unemp_duration_12p3a		0.022			-0.006	
		(0.058)			(0.044)	
prev_olf_duration_6to12mnth3a		0.020			-0.135	
		(0.107)			(0.092)	
prev_olf_duration_12p3a		0.027			-0.104	
		(0.093)			(0.082)	
Number of previous unemployment spells (>1).						
Unemployment						
oneplus_ue_spell			-0.016			-0.021
			(0.048)			(0.039)
prev_unemp_oneplus_ue3			0.060			0.085*
			(0.057)			(0.046)
prev_unemp_oneplus_olf3			0.234**			0.090
			(0.105)			(0.090)
Out of the Labour Force						
oneplus_olf_spell			0.086			0.103
			(0.100)			(0.074)
prev_olf_oneplus_ue3			-0.163			0.037
			(0.106)			(0.102)
prev_olf_oneplus_olf3			0.047			-0.007
			(0.073)			(0.066)
N	7666	7666	7666	10912	10912	10912
LL	1415	1402	1413	1444	1430	1437
$\bar{R}^2$	0.366	0.365	0.366	0.511	0.510	0.511
RMS error	0.202	0.203	0.202	0.213	0.213	0.213
AIC	-2656.406	-2642.975	-2660.522	-2.7e+03	-2.7e+03	-2.7e+03

Sample selection: Individuals never in self-employment at interview date. Number of person-years: 16011 (combined sample); 8751 (females); 7440 (males). 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.
See Section **??** for the full results.
* p<0.10, ** p<0.05, *** p<0.01

The general story seems fairly robust even to previous labour market status definition once reason for leaving previous job is controlled for, although the magnitude of the coefficients is reduced (Table G.9). Furthermore, whilst the long-run penalty is insignificant at conventional levels in the 1991-1997 sample, once the time period is extended there is no strong evidence of recovery of earnings in the case of unemployment (Table G.8).

Appendix H

Chapter 5: Variable Definitions & Construction.

H.0.2 Key Variable Definitions

In order to ensure precision in the variable construction, the sequence of questioning in the survey needs to be taken into account. The branching nature of the BHPS means that the way an interviewee responds to questions earlier in the survey will affect the availability of information collected later on. Following the questionnaire, the interviewer initially establishes whether the interviewee would define themselves as employed or self-employed. In the next stage, the interviewer asks for the interviewee's "current status", which appears in the individual response file. The interviewer then checks to see if reported status is the same as at the last interview, if not then their job history is requested for the last 12 months. A serious attempt to record a complete and accurate picture of this, with pay slips referred to when necessary (see BHPS Questionnaires for more information).

Real Wage Calculated by dividing an individual's gross monthly pay (including overtime, bonuses, etc. before tax deductions, national insurance or pensions contributions, union dues, etc.) by 4.33 times the number of hours usually worked plus 1.5 times usual paid overtime hours (under the assumption that overtime is paid at 1.5 times the usual hourly rate). Overtime payment is only available in the BHPS from 1999. Inline with Arulampalam (2001), this series is deflated using the Retail Price Index (RPI) and reported in 1991 prices. Since spell start and end dates are recorded to the nearest month, the September version of the monthly RPI - available from the Office for National Statistics - is used. The version used includes mortgage payments as well as indirect taxes in the RPI series. Non-positive values of usual hours worked are coded as missing. Non-positive values of paid overtime hours are coded as 0 in the calculation of wages in cases where both paid (jbotpd) and usual (jbot) overtime hours worked were coded as not applicable. All other non-positive cases are coded as missing. Extreme values for normal weekly hours, and for normal paid overtime hours. These are truncated at 90 hours for normal hours, and 30 paid overtime hours. Real wages less than 0.5 and more than 100 pounds per hour were dropped as outliers.

Current (INDRESP file) Labour Market Status The literature has highlighted the importance of reporting error with regards to labour market status, which can generate spurious transitions between labour market states (Poterba & Summers 1986). With this in mind, 'current labour market status' is conditioned on the sequence of questions in the BHPS survey. Due to the branching nature of the BHPS, internal inconsistencies may arise in the data (see Taylor *et al.* 2010, vol. 3, pp. A3-16). An individual may initially report themselves to be unemployed, but later on it transpires that they actually have a job and they provide this information. This inconsistency is likely to arise due to the survey design, as current labour market status refers to what an individual considers to be their main activity. For example, an interviewee may be registered as unemployed and thus consider their main activity to be unemployment, even though they have a job (Jobseeker's Allowance rules permit individuals to work less than 16 hours a week but still claim unemployment benefits). In order to deal with this, two variables "jbhas" (whether an individual did paid work in the last week) and "jboff" (whether an individual did not work last week, but has a job) were used.

For reference, the first question in the current employment section asks the following question: "Can I just check, did you do any paid work last week - that is in the seven days ending last Sunday - either as an employee or self employed?" This is stored as "jbhas". If the individual currently has a job, then they are asked about the type of employment contract that they have (permanent, seasonal/temp, fixed period). If the individual does not currently have a job, then they are asked: Even though you weren't working did you have a job that you were away from last week? This is stored as "jboff". See BHPS questionnaires for more detail.

Previous Labour Market Status For spells ongoing at the first wave interview which lasted more than a year, previous labour market status is not collected at Wave 1. This information is calculated using the job history information in the JOBHIST file as well as the pre-1.9.1990 retrospective data collected from first leaving full-time education at Waves 2 & 3. Two versions of previous labour market status are developed, one which does not restrict how far back into the past it looks, i.e. since leaving full-time education, and another that restricts this to up to 5 years in the past. Kunze (2002) allows the effect of previous labour market status to vary up to 6 years into the past, although the study is based on administrative event history information from tax records of the German IABS sub-sample. Due to lack of retrospective data, (Gregory & Jukes 2001) restrict the impact of unemployment to to have an effect on wages up to two years prior to the event. Studies based on the DWS are restricted to 5/6 years of retrospective information (Farber 1999). Thus job displacements occurring more than 5/6 years years ago will not be captured in the DWS, with these individuals being assigned to the control group of non-displaced workers. This distinction is likely to be of significance for individuals with long employment spells due to a good initial match, if promotions are not considered as the start of a new spell. One would expect previous non-employment to be less significant the longer is job tenure, and consequently when using a previous labour market status indicator which does not restrict how far into the past one looks. The assumption that only the last the last 5 years matters is the implicit assumption that studies using surveys which only provide limited retrospective data, like the DWS, are making. These studies are also limited by lack of a direct measure of General Experience. Previous status since leaving full-time education is used in the main analysis, given its superiority based on statistical grounds (information criterion).

Leave Marker This indicator groups reasons for ending *previous* job ('jhstpy'[job-1]). This indicator defines the following groups: voluntary quit (move to a better job); redundant; sacked/dismissed; temp. job ended; other (retirement; health; baby; children/homecare; care other; other). I was unable to identify how Arulampalam (2001) defines separation types when generating Table 5 in her study. McLaughlin (1991) defines 'quits' as changes to better or different jobs; 'layoffs' as dismissals and redundancies; and 'other reasons' to include termination of contract, bad health, retirement, pregnancy, family care, national service and full time education. The definition of the 'other' group is consistent with McLaughlin's (1991) methodology but excludes termination of contract as this can be identified in the BHPS (McLaughlin 1991). Following Arulampalam (2001) I create separate groups for individuals that were 'made redundant', 'sacked/dismissed', 'temporary job ended' and those with 'missing' reasons. In this study's case, since we include individuals without wave 3 job history, the incidence of missing values for this indicator is not negligible.

General Human Capital/ Job Market Experience Different methods have been used in the literature to proxy job market experience. In the Mincerian earnings function, experience is taken to represent *cumulative* tenure, or in the original formulation: *years since leaving school* (Chiswick 2003). However, some studies have proxied experience with age [experience + schooling + 5/6], controlling for years of schooling (for example Addison & Portugal 1989; Jacobson *et al.* 1993; Houle & van Audenrode 1995) or potential experience: age - [years of schooling - school starting age (5/6)]/ full-time education leaving age (Gangl 2006). This approach is usually taken due to underlying data limitations implying a lack of information about full employment biographies (Gangl 2006). Others have used cumulative employment tenure, subtracting non-employment tenure from total tenure, years of employment since 18 (Antonji & Shakotko, 1987) or summing up all employment spells post full-time education (Dustmann & Pereira 2008). Arulumpalam (2001) uses cumulative full-time employment tenure. Using full-time employment tenure imposes the assumption that employees enter part-time jobs on flat wage-tenure profiles, a testable assumption. (Gregory & Jukes 2001) proxy general experience by subtracting months of claimant unemployment in the (UK Joint Unemployment & Vancancies Operating System, JUVOS) from years of employment (from the National Earnings Survey, NES). To control for endogeneity Dustmann & Meghir (2005) adopt a control function approach to estimating experience, the residuals of which are used as controls for experience in a standard wage equation formulation. As highlighted in section 2.2.3, the way one defines experience is likely to have an impact on the results, given the increased sensitivity to omitted variable bias and measurement error.

Information on the length of time spent in each labour market state is required in order to calculate this indicator directly. "Length of time in current labour market spell" (cjsten) is collected at each INDRESP interview. An additional indicator is developed using the start, end and interview dates in order to calculate the spell length (once spells are in chronological order). Both indicators could be used in this analysis; however there is reason to believe that the spell length indicator is likely to be more accurate than the BHPS supplied cjsten measure. Table H.1, which refers to an individual in the raw unconditional data, highlights the potential for recall error in the survey data. The reported starting date of the wave 1 employment spell varies across the waves although the individual was continuously employed, in the same spell, until wave 5. At wave 2 the spell start date seems to be erroneously reported, whilst at wave 5 the start date reported in the JOBHIST entry suggests that the individual subsequently ignored the two months that they spent in unemployment from 1982m7 to 1982m9. This pattern is an empirical regularity in the data, due to the prevalence of recall error in the BHPS (Maré 2006), and thus the use of the raw cjsten measure to calculate experience indicators is likely to produce biased estimates of the impact of general experience on wages in the illustrated case. This is likely to be further exacerbated if a spell ends between interview dates as only the spell length recorded at interview is captured by the indicator. Furthermore, cjsten refers to the current labour market spell at interview. This implies that short spells experienced over the previous 12 months will not be captured when using cjsten as a basis for constructing the experience indicator. The self-constructed spell length indicator is more likely to better capture these short-run dynamics.

The self-defined spell length indicator is chosen in favour of the BHPS-supplied measure as is likely to be more accurate due to strategies implemented

Table H.1: Data Inconsistency Example, one individual, raw unconditional data

wave	jbstat	jhstat	jhstpy	Spell Start Month	Interview Month	Spell End Month	cjsten
0	Emp		Redundant	1959m11		1981m11	3168
0	Emp		Redundant	1981m11		1982m7	96
0	Unemp			1982m7		1982m9	60
1	Emp			1982m9	1991m11		3358
2	Emp			1990m5	1992m9		869
3	Emp			1982m10	1993m9		4011
4	Emp			1982m9	1994m9		4393
5		Emp/ Self-Emp	Retired	1981m9		1995m7	
5	Retired			1995m7	1995m9		60
6	Retired				1996m9		423
7	Retired				1997m9		789

to minimise recall error. General employment experience is calculated by cumulating length of time in employment since first leaving full time education using the self-constructed spell length indicator. General employment experience is calculated by cumulating length of time in employment since first leaving full time education using the self-constructed spell length indicator[1]. Employment spells in the wave 0 data are the easiest to deal with, as each record refers to separate spells with the a different employer (wave 3 data) or length of time in continuous employment (wave 2 data). Potential experience (age - school leaving age, where school leaving age is taken to be age first left full-time education) is used as a robustness check, as well as a test for the appropriateness of this indicator as a proxy for General Human Capital[2]

Specific Human Capital/ Current Employer Tenure Length of time with current employer (tenure) is used to proxy specific human capital. Tenure is endogenous as it can be viewed as a sequence of non-quit decisions (Farber 1999). Furthermore, the returns to tenure can be interpreted as the returns to both

[1]Although (Arulampalam 2001) states that a BHPS-supplied indicator of spell length was used in the study, "[the] tenure variable used is the current employer tenure as recorded at the interview (Arulampalam 2001, pp. F593)", Professor Arulampalam's SPSS code does not include steps for the construction of the general experience, indicating that this was supplied pre-constructed by the Essex Institute for Social & Economic Research (ISER). The advantage of the self-constructed measure is a lower incidence of missing values.

[2]This indicator has been widely used in the literature, however mostly due to limitations in the underlying data implying a lack of full employment biographies.

match and firm-specific human capital. No attempt is made to differentiate between these potential sources.

H.0.3 Urban/Rural & Accessibility Indicators

The methodology used to classify these indicators is consistent with that used in chapter 4, detailed in appendix chap:p1LinkedDataset. For further details, please see these sections of the thesis.

H.0.4 Unemployment/Vacancies ratio

The Unemployment to Vacancies ratio is constructed using Notified vacancies (inflow) and the Claimant Count for the period 1991 - 2001. Outlier observations (>99 percentile) are dropped in the construction of this indicator. Recording of vacancy postings was suspended from May 2001 onwards by the Office of National Statistics (ONS) under guidance of the Department of Work and Pensions (DWP). This suspension in the series was due to significant distortions in the series introduced by changes in vacancy notification methodology, e.g. Substantial process changes that took place with the introduction of telephone call centre and later internet filing of vacancies also need taking into account (Bentley 2005). This reporting was resumed in May 2002, however, changes in vacancy notification and handling methods hamper comparability of this series over time. Although (Bentley 2005) states that the historic series (pre-suspension) is available on NOMIS (www.nomisweb.co.uk), there is a two year gap in headline statistics and a one year gap if one aggregates occupation/industry-level data.

Appendix I

Chapter 6: Further Descriptives.

I.1 Further Descriptives

Table I.1: Industrial Skill Composition (Census 2001 - based).

Highest Qualifications Level (Census 2001 - based)		
No qualifications:	No academic, vocational or professional qualifications.	
Level 1:	1+ 'O' levels/CSE/GCSE (any grade), NVQ level 1, Foundation GNVQ.	
Level 2:	5+ 'O' levels, 5+ CSEs (grade 1), 5+ GCSEs (grade A - C), School Certificate, 1+ 'A' levels/'AS' levels, NVQ level 2, Intermediate GNVQ or equivalents.	
Level 3:	2+ 'A' levels, 4+ 'AS' levels, Higher School Certificate, NVQ level 3, Advanced GNVQ or equivalents.	
Level 4/5:	First degree, Higher Degree, NVQ levels 4 - 5, HNC, HND, Qualified Teacher Status, Qualified Medical Doctor, Qualified Dentist, Qualified Nurse, Midwife, Health Visitor or equivalents.	
Other qualifications/level unknown:	Other qualifications (e.g. City and Guilds, RSA/OCR, BTEC/Edexcel), Other Professional Qualifications.	
SIC92 Sections		**Qualifictation Index**†
England		
F.	Construction:	0.169432
A, B.	Agriculture, hunting, forestry and fishing :	0.188137
G.	Wholesale and retail trade, repairs :	0.239862
H.	Hotels and restaurants :	0.260218
I.	Transport, storage and communications :	0.298577
C, D, E.	Mining and quarrying, manufacturing, and electricity, gas and water supply :	0.318857
O, P, Q.	Other¶ :	0.562674
L.	Public administration and defence, social security¶ :	0.668772
J.	Financial intermediation¶ :	0.777015
N.	Health and social work¶ :	0.868222
K.	Real estate, renting and business activities¶ :	0.935312
M.	Education¶ :	1.683318
Wales		
F.	Construction :	0.167373
A, B.	Agriculture, hunting, forestry and fishing :	0.171054
G.	Wholesale and retail trade, repairs :	0.192753
I.	Transport, storage and communications :	0.203045
H.	Hotels and restaurants :	0.217952
C, D, E.	Mining and quarrying, manufacturing, and electricity, gas and water supply :	0.263324
O, P, Q.	Other¶ :	0.48415
J.	Financial intermediation¶ :	0.547211
L.	Public administration and defence, social security¶ :	0.644476
K.	Real estate, renting and business activities¶ :	0.675296
N.	Health and social work¶ :	0.792586
M.	Education¶ :	1.836414

NB. Scotland values assumed to be the same as those for Wales.
† Qualifications index used to rank industries (employment counts, all people): (3,4/5)/(No Quals,1,2,Other/Unknown).
¶ Skilled Occupations. Defining "Other" category as unskilled had no impact on rankings.

Table I.2: Broad-banded Occupational Skill Intensity (Census 2001 - based).

Highest Qualifications Level (Census 2001 - based)		
SOC90 1-digit groups		**Qualifictation Index**†
England		
2.	Professional Occupations :	4.864
3.	Associate Professional and Technical Occupations :	1.172
1.	Managers and Senior Officials :	0.724
	ALL PEOPLE :	0.394
4.	Administrative and Secretarial Occupations :	0.390
6.	Personal Service Occupations :	0.284
7.	Sales and Customer Service Occupations :	0.264
	Never worked or occupation not coded :	0.167
9.	Elementary Occupations :	0.141
5.	Skilled Trades Occupations :	0.137
8.	Process, Plant and Machine Operatives :	0.092
Wales		
2.	Professional Occupations :	5.042
3.	Associate Professional and Technical Occupations :	1.126
1.	Managers and Senior Officials :	0.542
4.	Administrative and Secretarial Occupations :	0.381
	ALL PEOPLE :	0.325
7.	Sales and Customer Service Occupations :	0.242
6.	Personal Service Occupations :	0.238
	Never worked or occupation not coded :	0.151
5.	Skilled Trades Occupations :	0.142
9.	Elementary Occupations :	0.134
8.	Process, Plant and Machine Operatives :	0.083

NB. Scotland values assumed to be the same as those for Wales.
† Qualifications index used to rank occupations (employment counts, all people): (3,4/5)/(No Quals,1,2,Other/Unknown).
NB. Group definitions the same as in Table I.1.

Table I.3: Current Labour Market Status: ISCO2008-Based. Males *vs.* Females, Annual Discrete-Choice Data, 1991 - 2008.

	Males			Females		
	LSKEMP	SKEMP	NON-EMP	LSKEMP	SKEMP	NON-EMP
Variable	Mean (S.D)	Mean (S.D)	Mean (S.D)	Mean (S.D)	Mean (S.D)	Mean (S.D)
Age						
30 - 45	0.375 (0.484)	0.452 (0.498)	0.207 (0.405)	0.391 (0.488)	0.426 (0.495)	0.344 (0.475)
45 +	0.316 (0.465)	0.34 (0.474)	0.496 (0.5)	0.334 (0.472)	0.346 (0.476)	0.394 (0.489)
School Type Attended						
Grammar No Fee	0.053 (0.225)	0.174 (0.379)	0.069 (0.254)	0.084 (0.277)	0.174 (0.379)	0.099 (0.299)
Private	0.029 (0.168)	0.082 (0.275)	0.037 (0.188)	0.028 (0.166)	0.073 (0.261)	0.047 (0.211)
Technical	0.068 (0.251)	0.103 (0.305)	0.115 (0.319)	0.086 (0.28)	0.104 (0.305)	0.103 (0.304)
Highest Academic Qualifications						
Degree	0.044 (0.205)	0.308 (0.462)	0.061 (0.239)	0.041 (0.199)	0.258 (0.438)	0.056 (0.229)
Other Higher	0.302 (0.459)	0.35 (0.477)	0.173 (0.378)	0.223 (0.416)	0.331 (0.471)	0.162 (0.368)
A Levels	0.15 (0.357)	0.121 (0.327)	0.122 (0.327)	0.141 (0.348)	0.097 (0.296)	0.107 (0.31)
O Levels	0.222 (0.416)	0.129 (0.335)	0.167 (0.373)	0.269 (0.444)	0.191 (0.393)	0.219 (0.413)
Vocational Training						
Yes	0.397 (0.489)	0.402 (0.49)	0.288 (0.453)	0.333 (0.471)	0.466 (0.499)	0.269 (0.444)
Individual Characteristics						
White	0.962 (0.191)	0.955 (0.208)	0.936 (0.244)	0.961 (0.194)	0.961 (0.194)	0.931 (0.254)
MarriedXCohabiting	0.685 (0.464)	0.771 (0.42)	0.555 (0.497)	0.724 (0.447)	0.742 (0.438)	0.685 (0.464)
Children	0.326 (0.469)	0.379 (0.485)	0.223 (0.416)	0.403 (0.491)	0.35 (0.477)	0.517 (0.5)
ChildrenXMarriedXCohab	0.322 (0.467)	0.373 (0.484)	0.212 (0.408)	0.341 (0.474)	0.3 (0.458)	0.389 (0.487)
Employed Spouse	0.523 (0.499)	0.618 (0.486)	0.217 (0.412)	0.646 (0.478)	0.674 (0.469)	0.472 (0.499)
Health Limits	0.086 (0.281)	0.06 (0.237)	0.444 (0.497)	0.093 (0.29)	0.082 (0.274)	0.291 (0.454)
Disabled	0.012 (0.107)	0.006 (0.076)	0.152 (0.359)	0.006 (0.079)	0.005 (0.073)	0.058 (0.234)
Housing Tenure						
Owned Outright	0.149 (0.356)	0.14 (0.347)	0.193 (0.395)	0.152 (0.359)	0.146 (0.354)	0.193 (0.395)
Mortgage	0.626 (0.484)	0.734 (0.442)	0.273 (0.446)	0.598 (0.49)	0.722 (0.448)	0.374 (0.484)
Council	0.107 (0.309)	0.028 (0.165)	0.345 (0.475)	0.137 (0.344)	0.034 (0.182)	0.271 (0.444)
Housing Assoc.	0.037 (0.189)	0.015 (0.121)	0.075 (0.264)	0.042 (0.199)	0.017 (0.129)	0.073 (0.26)
Work-Related Training in last 12 months (+ part-time courses)						
Yes	0.278 (0.448)	0.387 (0.487)	0.052 (0.222)	0.262 (0.44)	0.413 (0.492)	0.058 (0.233)
Potential Experience						
Pot. Experience	20.8 (13.0)	20.3 (11.2)	24.8 (16.4)	21.5 (12.8)	20.2 (11.4)	23.0 (13.7)
X 30 - 45	1.564 (2.091)	1.831 (2.103)	0.883 (1.777)	1.655 (2.142)	1.728 (2.089)	1.413 (2.02)
X 45 +	2.363 (3.539)	2.409 (3.416)	4.037 (4.161)	2.464 (3.535)	2.455 (3.427)	3.036 (3.829)
Regional Characteristics						
Urban	0.627 (0.484)	0.614 (0.487)	0.685 (0.465)	0.62 (0.485)	0.639 (0.48)	0.665 (0.472)
Accessible	0.958 (0.201)	0.974 (0.158)	0.971 (0.167)	0.959 (0.198)	0.97 (0.17)	0.97 (0.17)
University (in TTWA)	0.729 (0.444)	0.745 (0.436)	0.71 (0.454)	0.723 (0.448)	0.752 (0.432)	0.733 (0.442)
Skill Intensity	-0.169 (0.904)	0.08 (0.951)	-0.269 (0.932)	-0.134 (0.916)	0.063 (0.942)	-0.167 (0.934)
Local Business Cycle Effects						
Labour Market Tightness (V/U)	-0.086 (0.92)	-0.088 (0.919)	-0.197 (0.823)	-0.123 (0.859)	-0.094 (0.907)	-0.178 (0.843)
Industrial Skill Composition	-0.009 (0.949)	0.16 (1.001)	-0.043 (0.955)	0.023 (0.975)	0.188 (0.993)	0.01 (0.967)
Previous Labour Market Status						
LSKEMPxMATCH	0.333 (0.471)	0.014 (0.118)	0.083 (0.276)	0.416 (0.493)	0.017 (0.128)	0.068 (0.251)
x Previous Industry (ref. Commercial/Industrial)						
XBusServ XLowV/U	0.044 (0.206)	0.003 (0.053)	0.016 (0.126)	0.067 (0.249)	0.003 (0.052)	0.014 (0.116)
XBusServ XHighV/U	0.072 (0.259)	0.022 (0.146)	0.017 (0.129)	0.136 (0.342)	0.038 (0.191)	0.025 (0.155)
XPubServ XLowV/U	0.017 (0.129)	0.001 (0.026)	0.005 (0.071)	0.063 (0.243)	0.002 (0.039)	0.01 (0.099)
XPubServ XHighV/U	0.034 (0.181)	0.001 (0.03)	0.004 (0.064)	0.106 (0.308)	0.003 (0.058)	0.011 (0.103)
XFirm Size: 50+	0.182 (0.386)	0.008 (0.086)	0.037 (0.19)	0.177 (0.382)	0.008 (0.091)	0.024 (0.152)
XPart Time Contract	0.011 (0.106)	0 (0.012)	0.008 (0.09)	0.212 (0.408)	0.005 (0.072)	0.034 (0.18)
LSKEMPxOVQUAL	0.459 (0.498)	0.029 (0.167)	0.076 (0.264)	0.359 (0.48)	0.025 (0.156)	0.047 (0.211)
LSKEMPxOVQUALxJobSatisfaction						
X"Genuinely Overqual."	0.098 (0.298)	0.006 (0.075)	0.01 (0.102)	0.056 (0.231)	0.005 (0.067)	0.008 (0.09)
X"Apparently Overqual."	0.361 (0.48)	0.023 (0.15)	0.065 (0.247)	0.303 (0.459)	0.021 (0.142)	0.038 (0.192)
x Previous Industry (ref. Commercial/Industrial)						
XBusServ XLowV/U	0.03 (0.17)	0.004 (0.062)	0.011 (0.105)	0.025 (0.157)	0.003 (0.054)	0.006 (0.076)
XBusServ XHighV/U	0.128 (0.334)	0.009 (0.094)	0.02 (0.139)	0.117 (0.321)	0.009 (0.095)	0.017 (0.127)
XPubServ XLowV/U	0.025 (0.155)	0.001 (0.035)	0.005 (0.071)	0.041 (0.199)	0.002 (0.044)	0.005 (0.068)
XPubServ XHighV/U	0.071 (0.257)	0.004 (0.062)	0.009 (0.094)	0.129 (0.335)	0.007 (0.085)	0.011 (0.104)
XFirm Size: 50+	0.285 (0.451)	0.017 (0.129)	0.043 (0.202)	0.153 (0.36)	0.012 (0.111)	0.017 (0.13)
XPart Time Contract	0.023 (0.149)	0.002 (0.039)	0.008 (0.09)	0.156 (0.363)	0.007 (0.084)	0.022 (0.148)
HSKEMPxOVQUAL	0.011 (0.106)	0.253 (0.435)	0.026 (0.16)	0.011 (0.103)	0.225 (0.417)	0.016 (0.127)
HSKEMPxOVQUALxJobSatisfaction						
X"Genuinely Overqual."	0.003 (0.051)	0.042 (0.201)	0.005 (0.073)	0.002 (0.047)	0.033 (0.178)	0.003 (0.051)
X"Apparently Overqual."	0.009 (0.093)	0.211 (0.408)	0.021 (0.143)	0.008 (0.091)	0.192 (0.394)	0.014 (0.117)
x Previous Industry (ref. Commercial/Industrial)						
XBusServ XLowV/U	0.001 (0.032)	0.031 (0.175)	0.005 (0.071)	0.002 (0.042)	0.021 (0.143)	0.002 (0.042)
XBusServ XHighV/U	0.003 (0.05)	0.095 (0.293)	0.007 (0.081)	0.004 (0.064)	0.067 (0.251)	0.006 (0.075)
XPubServ XLowV/U	0.001 (0.025)	0.011 (0.103)	0.001 (0.032)	0 (0.014)	0.019 (0.137)	0.001 (0.037)
XPubServ XHighV/U	0.002 (0.04)	0.037 (0.19)	0.004 (0.061)	0.002 (0.043)	0.082 (0.274)	0.004 (0.065)
XFirm Size: 50+	0.007 (0.086)	0.163 (0.369)	0.014 (0.119)	0.006 (0.075)	0.127 (0.333)	0.008 (0.089)
XPart Time Contract	0 (0.018)	0.011 (0.104)	0.002 (0.043)	0.002 (0.047)	0.049 (0.216)	0.006 (0.075)
NON-EMP	0.181 (0.385)	0.12 (0.325)	0.752 (0.432)	0.197 (0.398)	0.132 (0.339)	0.821 (0.383)