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YOUNG PEOPLE'S LABOUR MARKET TRANSITIONS: THE ROLE OF EARLY EXPERIENCES

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Young people's labour market transitions: the role of early experiences

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Abstract

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Keywords: young people; labour market transitions; duration dependence; state dependence; unobserved heterogeneity

JEL codes: C33, C41, J64, J68

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1. Introduction

The youth labour market has a number of features that set it apart from the labour market for prime-age workers. It is characterised by high transition rates between jobs as individuals engage in 'job-shopping' (Miller 1984) and movements into and out of employment are also common. Human capital investments feature prominently as young people often remain in full-time education beyond the age at which they can legally work or, alternatively, return to full-time education after a period in work. Those entering work will often participate in training as a means of gaining additional skills and thereby increasing their future earnings prospects.

In many countries, the youth labour market has been affected particularly badly by the global economic downturn. The UK is no exception (Gregg and Wadsworth 2010). However, in addition to cyclical fluctuations, it also shows signs of structural problems; its youth labour market started to deteriorate in 2004, considerably pre-dating the onset of the recession in 2008 (Goujard et al. 2010). At the time of writing, the latest official statistics (for the three months to April 2014) put the number of young unemployed at 853,000. This is a marked reduction from its peak of over one million roughly two years earlier but still represents an unemployment rate of 18.5 per cent, far in excess of the 4.7 per cent for those aged 25 or over. Consequently, efforts to improve the fortunes of young workers remain a policy priority.

Supply-side labour market policies often rely on the fact that individuals' early labour market experiences shape their subsequent outcomes in important ways. Kalwij (2004) finds that, in the UK, young men's transitions both into and out of claimant unemployment show negative duration dependence. In the case of unemployment exits, there are a number of possible explanations for this: skill deterioration (Pissarides 1992), employer discrimination (Blanchard and Diamond 1994, Lockwood 1991, Vishwanath 1989) or social networks as a source of employment becoming exhausted (Calvo-Armengol and Jackson 2004). Conversely, negative duration dependence in employment exits may be a result of individuals acquiring valued skills on the job or, alternatively, of firms' tendency to organise layoffs on a 'first in, last out' principle (Kiefer and Neumann 1989).

There may also be effects that persist across spells. Successive UK studies point to the scarring effects of unemployment (Arulampalam 2001, Arulampalam et al. 2000, Stewart

2007) and compelling evidence exists to suggest that unemployment when young can have negative impacts on adult outcomes including unemployment (Burgess et al. 2003, Gregg 2001) and wages (Gregg and Tominey 2005).

What is less understood is how these longer-term outcomes arise. If there is a negative effect of youth unemployment on adult outcomes it is likely that there will also be a negative effect on youth outcomes. This is of interest in its own right but also provides an insight into how the effects on adult outcomes evolve. This, in turn, allows for more informed policy responses to be identified. Interventions that can act to replace adverse experiences with more positive ones have the potential to bring about long-term improvements in young people's employment prospects and thereby to positively influence adult outcomes. To be effective in doing this, policymakers must judge how early into an adverse spell they should intervene and how long any such intervention should last.

It is here that this paper aims to contribute. We estimate a model of young people's transitions between four potential states: employment, unemployment, education and a residual category made up of people who are neither studying nor economically active, which we refer to in this paper as NEA (Neither in Education nor Active). We allow transitions to be influenced by duration, lagged duration and occurrence dependence, thereby capturing within-spell and cross-spell dynamics. We specify the econometric model to take account of unobserved heterogeneity and argue on the grounds of existing identification results that the duration, lagged duration and occurrence dependence effects have a causal interpretation.

The analysis updates and extends existing results in the empirical literature. The distinction between being unemployed and being out of the labour force (OLF) was explored by Flinn and Heckman (1983). They reject the hypothesis that the two states are behaviourally indistinguishable. Their analysis was based on 122 white US males leaving high school with a diploma in 1969 and not returning within the period over which outcomes are observed (30 months). Our analysis updates these results for the UK, using a much larger number of individuals, observed for up to nine years. Furthermore, we regard education as a separate state in the model. This permits individuals to return to study but also for the decision to leave school initially to be endogenous. More recently, Bonnal et al. (1997), Jones and Riddell (2006) and Doiron and Gørgens (2008) have estimated transition models that treat OLF as a separate state. Our model is more granular in this regard; treating education as a separate state, the NEA group represents individuals who are both economically and educationally inactive. This is a high priority group in the UK.

Together, the unemployed and NEA groups represent the more familiar NEET (Not in Employment, Education or Training) group but we maintain a distinction in the belief that the two states are very different not only in terms of their composition but also in terms of how the experience of each state affects subsequent labour market outcomes. We re-visit the scarring literature, examining the extent to which different labour market experiences influence later transition probabilities. A number of studies have examined this for young people (for example, Bonnal et al. 1997; Bratberg and Nilsen 2000; Mroz and Savage 2006; Cockx and Picchio 2012, 2013) but for the UK the most relevant evidence is Kalwij (2004) who considers transitions into and out of claimant unemployment. Our paper therefore represents an addition to the evidence base for the UK, building on Kalwij (2004) by using survey data that allow a larger number of states to be considered.

Methodologically, our paper is closest to Doiron and Gørgens (2008), who study transitions between employment, unemployment and OLF among Australian youth between 1989 and 1994. The main difference in our paper is that, as already mentioned, we consider four states rather than three and, unlike Doiron and Gørgens (2008), treat the school-leaving decision as endogenous. In addition, we exclude cumulative experience terms (total time spent in employment, for example) from the regressor set since we are not aware of any formal identification results that allow them to be interpreted causally. As a further refinement, we take seriously the issue of sample attrition in our data and model this simultaneously with the labour market transitions.

As a way of illustrating the type of insights permitted by the model, we simulate the effects of a series of hypothetical work experience programme that differ with regard to duration and eligibility criteria. We present results showing how the estimated treatment effect changes as these features of the intervention are varied. This provides some clues for the design of similar programmes. We consider in more detail the idealised case of a 12-month work experience programme, compulsory on reaching 12 months unemployment and show the impact on all states.

The paper proceeds as follows. Section 2 describes the data used. The econometric model is presented in section 3 which also states the key research questions. The estimation results are given in section 4, with simulations in section 5. Section 6 concludes.

2. Data

The analysis is based on the British Household Panel Survey (BHPS), a nationally representative annual longitudinal survey that ran from 1991 until 2008. All eighteen waves of data are used, covering the period up to 2008. The design of the BHPS is such that children within sampled households become eligible for full (adult) interviews once they reach the age of 16 and, thereafter, are interviewed annually. We restrict our attention to those observed to turn 16 at any time between 1991 and 2008. To retain the focus on the youth labour market, we right-censor observations at the point of reaching age 25.

The estimation dataset uses monthly labour market and education histories that run from the month before individuals are legally allowed to leave school (in this first month, all individuals are in education).[1] An individual's status in each month is defined according to his or her own self-reported main activity, selected from a list of ten available choices. These activities are then grouped into the four states under consideration: Employment, Unemployment, NEA and Education.[2]

All transitions are allowed to vary with age, sex, ethnicity (white/ non-white), qualification level at age 16, household income at age 16 and whether the individual is a mother. Season and year dummies are also included. Local labour market effects are captured using the local claimant unemployment rate, expressed as a deviation from the national average. This deviation form was chosen in order to control for changes over time in the rules governing receipt of out of work benefits. The intuition is that while institutional changes reduce the usefulness of the unemployment benefit series as a measure of the business cycle, cross-sectional variations remain important as a means of capturing geographic variations in the

[1] Maré (2006) and Paull (2002) provide detailed accounts of how to construct monthly labour market histories from the BHPS.

[2] The original ten states are: 1) self-employed 2) employed 3) unemployed 4) retired 5) maternity leave 6) family carer 7) full-time student 8) long-term sick/disabled 9) Government training scheme 10) other. Our groupings are: Employed = 1, 2 or 5; Unemployed = 3 or 9; NEA = 4, 6, 8 or 10; Education = 7. As (Paull 2002) notes, where individuals are engaged in more than one activity in a month, there will be an element of subjectivity in the choice of activity they report. Note that the education category is made up of full-time students and excludes individuals on training programmes. We ignore breaks in education spells of up to 2 months in order to ensure that school holidays are not recorded as NEA.

strength of the labour market. Effects of the business cycle are instead captured using estimates of monthly (national) GDP (Mitchell et al. 2005). As discussed below, such time-varying exogenous variables are important since they help with model identification. In addition, the transitions out of education are allowed to vary with parental education level, whether the individual lived in a workless household at age 16 and – reflecting the finding from Crawford et al. (2010) that those who are the youngest in their academic year tend to perform less well than their older peers – variables showing the individual's birth month.

Selected descriptive statistics for the sample when first observed are given in Table 1. We note that, at this point, everyone in the sample is in their final month of compulsory schooling. Academic years in the UK begin in September. Pupils must remain in school until June in the academic year in which they turn 16. In practice, this means that most children are 16 by the time they are able to leave school. Reflecting this, Table 1 shows that the mean age was slightly below 16 during the last month of compulsory schooling. Another point worth highlighting in Table 1 is that one third of the sample have 8 or more high-grade GCSEs.[3] This is a level of qualification commensurate with continuing comfortably into higher education. At the other extreme, 11 per cent have no qualifications and 13 per cent have only a CSE.[4] Parental qualifications are shown differently, converted to NVQ "equivalents".[5] On this basis, 19 per cent of children have parents with no qualifications.

Table 1 also describes the spell structure of the resulting estimation dataset. In total, we have information on nearly 3,500 individuals and more than 11,000 separate spells. This represents a mean of 3.38 spells per person, with a median of 3. On average, respondents were observed for more than 50 months.

<TABLE 1>

Table 2 shows there are comparable numbers of employment and education spells, roughly half that number of unemployment spells and fewer still NEA spells. Employment spells have the longest mean duration (20.1 months), followed by education (17.7 months). Unemployment spells are the shortest (5.7 months on average), while NEA spells are considerably longer, averaging 11.8 months. Consistent with this, unemployment spells have

[3] General Certificate of Secondary Education, the standard age 16 qualifcation.
[4] Certificate of Secondary Education, a low level qualification.
[5] National Vocational Qualifications. Converting all qualifications to NVQ equivalents allows academic and vocational qualifications to be considered on the same scale.

the highest exit rates; half leave unemployment within 3 months. This compares to median durations of 5, 11 and 13 months for NEA, employment and education, respectively.

<TABLE 2>

These summary descriptives provide an early indication of the importance of distinguishing between unemployment and NEA. This impression is reinforced by Figure 1 which illustrates the extent to which the rate of exits from an initial state changes over the course of the spell. The graphs on the leading diagonal are the empirical (Kaplan-Meier) survival curves for employment, unemployment, NEA and education spells, respectively. These show negative duration dependence in exits from all states. This is most marked in the case of unemployment and NEA spells. Duration dependence appears weakest in education exits although there is a substantial number of exits in the first month. This partly reflects those individuals who leave school at the earliest opportunity. However, the fact that education spells are defined to include both school and subsequent education following a break in study reduces the extent to which school-leavers alone determine the shape of the survival curve.

The survival curves reveal important gender differences in NEA exits. Long-term NEA spells are shown to be more common among females than males (for whom exit patterns resemble those seen for unemployment spells). This perhaps reflects the high incidence of parenthood among young NEA women; their childcare responsibilities may prevent economic activity or participation in education. Employment is the only other state that exhibits gender differences in exits. This is only evident at longer durations, where exits among female workers occur more commonly than among male workers. This may be capturing women leaving the labour force to bring up children, as they progress into their twenties.[6]

Since exits can be to one of three alternative states, it is helpful to examine how transition rates to a specific destination alter over the course of the spell. The off-diagonal charts in Figure 1 plot the cumulative incidence curves, that is, the probability of having exited the initial state (in the row) to a specific destination (in the column) as the spell lengthens.[7]

[6] These patterns are discussed in more detail in Dorsett and Lucchino (forthcoming).
[7] The calculation and properties of cumulative incidence curves are described in Coviello and Boggess (2004).

These charts provide additional insight into the nature of transitions. Exits from employment to unemployment and education are concentrated in the earliest months of a spell while exits from employment to NEA occur steadily over time but at a slow rate. For the unemployed, by far the most common exit destination is employment, with a heavy concentration in the earlier months. The inference appears to be that those who do not find work relatively quickly are unlikely to do so at all. Fewer NEA spells end with a move into employment although, again, there is a concentration towards the start of the spell. Unemployment and NEA appear qualitatively distinct, with moves between the two tending to be rare. Education is a somewhat more important destination for NEA spells, particularly for females. Lastly, exits from education into any of the three other states do not appear to be so heavily concentrated in the early months of the spell.

<FIGURE 1>

The number of transitions between states is summarised in Table 3. Again, the distinction between unemployment and NEA status is evident; 55 per cent of individuals become unemployed on exiting employment but only 15 per cent become NEA. The importance of allowing for returns to education is apparent; this accounts for 29 per cent of observed employment exits. The majority (80 per cent) of unemployment exits are to employment. NEA spells, on the other hand, end with a job in only 50 per cent of cases; 30 per cent will instead return to education. For those in education, it is most common for them to be economically active after they complete their studies. In 65 per cent of cases, they will find work and in a further 26 per cent of cases they will be looking for work (i.e. unemployed).

<TABLE 3>

3. Empirical approach and key research questions

3.1 The econometric model

We estimate a multi-state model to examine transitions between four mutually exclusive and exhaustive activity states: work (w), unemployment (u), NEA (n) and education (e). We write the transition intensity from origin state *j* to destination state *k* as θ_{jk}. There are twelve possible types of transition: $\{w \rightarrow u, w \rightarrow n, w \rightarrow e, u \rightarrow w, u \rightarrow n, u \rightarrow e, n \rightarrow w, n \rightarrow u, n \rightarrow e, e \rightarrow w, e \rightarrow u, e \rightarrow n\}$.

The econometric challenge is to control for unobserved heterogeneity. As first shown by Lancaster (1979), not to do so will result in duration dependence being over-estimated. This arises due to sorting effects whereby individuals with an unobserved characteristic that increases exit rates leave the origin state earlier than those without that characteristic so that observed exit rates decline with the length of spell. By controlling for unobserved heterogeneity we aim to identify genuine duration dependence whereby remaining in a state for a longer period of time in itself reduces the rate of exit. The consideration of prior experience raises similar issues: unobserved characteristics may influence past outcomes in a similar way to how they influence later outcomes (Heckman and Borjas 1980). For instance, individuals who are highly motivated might be expected to have consistently different outcomes from individuals who are not. If we wish to argue that estimates of the impact of prior experience on subsequent transitions can be given a causal interpretation, the econometric model must control for correlation across transition types in unobserved characteristics. We attempt to capture the effect of prior experience by including three types of variable: variables identifying the individual's state immediately prior to their current state; the duration of the individual's previous spell in the state they currently occupy (lagged duration dependence); and the number of times the individual has experienced their current state (occurrence dependence).

As already noted, all individuals in our sample are first observed in their last month of compulsory education. In the following month, they are legally allowed to leave school and three types of transition become possible: beginning work, becoming unemployed or entering a NEA spell. Alternatively, they may remain in education. Similarly, in each subsequent month, individuals face the competing risks of entering one of the other states or, alternatively, remaining in their current state.

An important advantage of our approach is that including education as one of the states of interest avoids the need to impose restrictions that could affect the representativeness of the results. Doiron and Gørgens (2008), for example, model transitions *after* leaving school and therefore treat the age at which individuals leave school as exogenous. We model transitions from education – including the first transition – and so the endogeneity of individuals' decisions about when to leave school (or, indeed, later education) is addressed. Similarly, modelling transitions into education avoids the need to exclude from the estimation sample those individuals observed to return to education following a spell in an alternative state

(Doiron and Gørgens 2008) or to treat such transitions as a form of censoring (Cockx and Picchio 2012).[8]

We follow Van den Berg and Van der Klaauw (2001) and specify the discrete-time process as having an underlying continuous-time mixed proportional hazard (MPH) form. Integrating the continuous-time outcomes over the observation intervals of calendar months, the transition intensity from j to k $(j \neq k)$ can be written with a mixed proportional hazard (MPH) form:

$$\theta_{jk}\left(t|\mathbf{x}, v_{jk}, a_{lag}, t_{lag}, count_j\right) = h_{jk}(t)\phi_{jk}(\mathbf{x})\psi_{jk}\left(a_{lag}, t_{lag}, count_j\right)v_{jk.}$$

Here, a_{lag} is the activity immediately preceding the current spell, t_{lag} is the length of that previous spell and $count_j$ is the number of prior spells in state j. $h_{jk}(t)$ is the baseline hazard. In order to minimise the extent to which this is subject to functional form assumptions, we specify this as piecewise constant,

$$h_{jk}(t) = \exp\left(\sum_{b=1}^{B_{jk}} \gamma_{jk}^{b} 1(\tau_{b-1} \leq t < \tau_{b-1})\right)$$

where B_{jk} is the number of segments and $\tau_0 = 0$. For each transition type, the segments were chosen in order to ensure that a sufficient number of transitions were observed within each one. Due to variation in the numbers experiencing each transition type, this means that the segmentation of the baseline hazard is coarser for some transition types than for others. The systematic part of the hazard is specified as

$$\phi_{jk}(\mathbf{x}) = \exp\left(\mathbf{x}\beta_{jk}\right).$$

Here, $\mathbf{x}$ includes both fixed and time-varying observed characteristics.

The lagged dependencies are specified:

[8] Ideally, our model would distinguish between different types of education (school, university, etc). Our ability to do this is limited by the data and the fact that introducing an additional state would increase the number of transition types from 12 to 20, making the model considerably more complex. Instead, we include dummy age variables in the model that are intended to distinguish between secondary education, university education and any additional study taking place after the usual age of completing tertiary education.

$$\psi_{jk}(a_{lag}, t_{lag}, count_j)$$
$$= \exp\left(\sum_{p\neq j} \alpha_{jk}^{p} 1(a_{lag} = p) + \sum_{q\neq j} \lambda_{jk}^{q} 1(a_{lag} = q)\ln(t_{lag}) + \sum_{r=1,2+} \pi_{jk}^{r} 1(count = r)\right).$$

The third term on the right hand side allows for the hazard to shift (proportionately) according to whether it is the first spell of its type or the second. There is no shift for subsequent spells. This allows for early spells to differ from later spells and is in recognition of the high degree of initial movement among young people, which later settles to a position of more stability.

Lastly, the specification allows for unobserved heterogeneity, $\upsilon_{jk,}$ where the subscript indicates that this may affect different types of transitions differently. Individual unobserved heterogeneity is assumed fixed across spells of each type.

The contribution to the likelihood of individual *i*'s spell *s* of *d* months with origin state *j* is

$$L_i^s(\mathbf{v}_{ij}) = \prod_{(j,k)\in\mathcal{J}} \left[\left(1 - \exp\left(\sum_{(j,k)} \theta_{jk}(d)\right)\right) \frac{\theta_{jk}(d)}{\sum_{(j,k)} \theta_{jk}(d)}\right]^{y_{i,(j,k)}^s} \times \prod_{r=1}^{d-\sum_k y_{i,(j,k)\in\mathcal{J}}^s} \exp\left(-\sum_{(j,k)\in\mathcal{J}} \theta_{jk}(r)\right)$$

where $\mathcal{J} = \{(w,u),(w,n),(w,e)\}$ if $j = w$, $\mathcal{J} = \{(u,w),(u,n),(u,e)\}$ if $j = u$, $\mathcal{J} = \{(n,w),(n,u),(n,e)\}$ if $j = n$, $\mathcal{J} = \{(e,w),(e,u),(e,n)\}$ if $j = e$, $y_{i,(j,k)}^s$ is a dummy variable taking the value 1 if individual *i*'s spell *s* that began in state *j* resulted in a transition to state *k* (zero otherwise) and $\mathbf{v}_{ij}$ collects the unobserved heterogeneity terms associated with the transition from state *j* to state *k* for individual *i*. For compactness, the conditioning of the transition intensities on $x_{jk}(\tau + t)$ and υ_{jk} is left implicit.

An important feature of our estimation sample is that it is subject to substantial attrition; 46 per cent of individuals drop out of the sample before age 25. This complicates the analysis since, if the influences on attrition also affect transitions between labour market states, estimating the transition model on those individuals who remain in the sample may yield biased results. To address this, we model sample attrition as a function of observed and unobserved characteristics, which are allowed to correlate with unobserved influences on labour market transitions. This approach is in the spirit of van den Berg and Lindeboom (1998).

The attrition hazard rate is the probability of not responding to the survey at wave *w*, conditional on responding in all prior interview waves. We again assume a MPH form:

$$\theta_a(w|z,\nu) = \exp(\varphi(w) + \delta' Z)\nu$$

where $w \in \{2, \dots, W\}$ is the interview wave, duration dependence is captured by $\varphi(w)$ and the effect of background characteristics when first observed are captured by $\delta' Z$. Unobserved heterogeneity is represented by ν.

The contribution of the survey participation duration for individual *i* who remained in the sample for *W* interview waves is

$$L_i^a(\nu_i) = \left(1 - \exp\left(-\theta_a(W)\right)\right)^{q_i} \prod_{l=1}^{W-q_i} \exp\left(-\theta_a(l)\right)$$

where q_i is a dummy variable taking the value 1 if individual *i* dropped out of the survey before reaching age 25, zero otherwise.[9]

Writing the product of each of individual *i*'s S_i labour market spells as $L_i(\mathbf{v}_i)$, the overall likelihood contribution for individual *i*, conditional on unobserved heterogeneity, is:

$$L_i(\mathbf{v}_i, \nu_i) = L_i(\mathbf{v}_i) L_i^a(\nu_i)$$

The twelve possible transition types in the multi-state model together with the attrition duration give an unobserved heterogeneity distribution of dimension thirteen. We follow Heckman and Singer (1984) and discretely approximate the unobserved heterogeneity joint distribution by *M* mass points, $\upsilon^m, m = 1, 2, \dots, M$, where

[9] Those individuals not observed up to age 25 simply because they had not reached this age by 2008 are treated as independently right-censored.

$\upsilon^m = \{\upsilon_{wu}, \upsilon_{wn}, \upsilon_{we,}\upsilon_{uw}, \upsilon_{un}, \upsilon_{ue,}\upsilon_{nw}, \upsilon_{nu}, \upsilon_{ne,}\upsilon_{ew}, \upsilon_{eu}, \upsilon_{en}, \nu\}$. The probability attached to υ^m is specified as $p^m = \exp(\lambda^m)/\sum_{g=1}^{M} \exp(\lambda^g)$, $m = 1, \ldots, M$, where $\lambda^1 = 0$. The number of mass points, *M*, is unknown a priori but chosen on the basis of specification tests.

Denoting by L_i^m the likelihood contribution associated with mass point *m* for individual *i*, the unconditional likelihood function across the full sample of *N* individuals is:

$$L = \prod_{i=1}^{N} \sum_{m=1}^{M} p^m L_i^m.$$

3.2 Identification

Horny and Picchio (2010) show that, under the MPH assumption, both the unobserved heterogeneity distribution and the structural parameters of the model – including the lagged dependences – are non-parametrically identified. To further assist identification, we restrict the specification of hazards to be similar across multiple spells of the same type, other than allowing for a proportionate shift to capture occurrence dependence. Brinch (2007) proves that exogenous variation in covariates over time and across individuals is sufficient for identification, without the need for proportionality. We include in our model calendar year and quarter dummies, the local unemployment rate relative to the national rate and GDP at the national level. These series vary month on month and, due to differences between individuals in when they entered ERA and the fact that we observe multiple spells of differing durations, there is variation in these covariates across individuals at the same point in their spell. This provides another source of identification and thereby reduces reliance on the assumption of proportionality. In the attrition equation, we include another exogenous variable: whether the survey interview has changed since the previous interview.

Lastly, we note that most identification results relate to continuous time processes. Gaure et al. (2007) provide extensive Monte Carlo evidence that the parameters of the underlying continuous time model can be recovered using discrete data, so long as the likelihood function reflects the discrete nature of the available data.

3.3 Key research questions

The analysis is structured around two key research questions. The first concerns whether it is necessary to distinguish between unemployment and NEA. We explore this by comparing the estimated transition intensity into employment for an individual who has been unemployed for a particular length of time with that of an individual who has been out of work for the same length of time, but who was initially NEA rather than unemployed. We use a similar approach to consider transition intensities from NEA into employment,

The second key research question is how prior experience affects subsequent outcomes. This is difficult to grasp from the estimated coefficients of the econometric model so, in order to better understand the overall effects of different types of experience, we use simulation techniques to show how intervening in an unemployment spell can affect subsequent outcomes. This intervention is intended to mimic important features of conventional work experience schemes and so may provide some guidance for the design of labour market policy for young people.

4. Results

This section presents the key results. The full estimation results are given in the Appendix, along with a discussion of the other findings.

Unobserved heterogeneity is represented by $M = 4$ points of support.[10] Essentially, the model approximates the distribution of unobserved heterogeneity by assuming individuals fall into four groups, with members of each group identical with regard to unobserved characteristics. The size of each group is shown at the end of Table A.1. Group 1 accounts for roughly 29 per cent of the sample; group 2 for 16 per cent, group 3 for 38 per cent and group 4 for 18 per cent.

Before presenting the results of most interest, we provide some indication of the fit of the model. Figure 2 shows the changing activity profile of males from the month after reaching school-leaving age up to age 24. The actual proportion accounted for by each state is shown

[10] Estimation began with a model without unobserved heterogeneity. The coefficient estimates from this were used as starting values to estimate a model with $M = 2$ points of support, with the unobserved heterogeneity starting values generated as random numbers. The $M = 2$ results were in turn used as starting values in a $M = 3$ model, although again using random numbers for the unobserved heterogeneity starting values. This process was repeated for the $M = 4$ case and attempted for the $M = 5$, but this last specification did not converge. Of the four specifications for which results were achieved, the $M = 4$ case minimised the Akarike Information Criterion and on that basis was chosen as the preferred model.

with a solid line. Alongside this is a dashed line showing the simulated proportion in each state.

The simulation approach is as follows. We begin by constructing a representative male, defined to have the mean characteristics of all males at the time of their last month of compulsory schooling.[11] Next, we take 1,000 draws from a multivariate normal distribution with means corresponding to the estimated coefficients reported in Table A.1 and variance given by the associated variance-covariance matrix. For each draw, a labour market trajectory is simulated. The first month for which outcomes are simulated is the month of reaching school-leaving age (this is the first month for which individuals can be in a state other than education).[12] In each subsequent month, transition intensities are re-calculated and used to simulate exits from the current state to an alternative state. The endogenous time-varying covariates (such as the state occupied prior to the current state) are updated consistent with simulated outcomes. Since we wish to compare simulated proportions in each state to the actual proportions, we need to take account of the fact that these actual proportions are based on data that is subject to attrition. To allow a fair comparison, the simulated proportions also use the model results to reduce sample size over time in line with simulated sample attrition.[13]

The overall impression from Figure 2 is that the simulated proportions are, for the most part, quite similar to the observed levels. The biggest divergence arises at the time of higher education participation, roughly during the third to fifth years following school-leaving age. During this period simulated employment is elevated in comparison to actual levels while simulated education participation is lower. Elsewhere, simulated and actual employment and education levels compare very well. With regard to the other outcomes – unemployment and NEA – the simulated and actual levels are also very similar throughout. In summary, with the caveat around higher education, the model appears to successfully capture the changing profile of the youth labour market over an extended (nine year period).

[11] We consider males only since the main reason for economic inactivity among females – motherhood – cannot be simulated by the model. This complicates the simulation of female outcomes over the longer term for reasons unconnected to the fit of the model.
[12] This is achieved by dividing the unit interval into 4 sub-intervals according to the transition intensity for each possible destination from the current state (education, initially). The size of each sub-interval reflects the probability of exiting at that point to a particular destination (note that one of the destinations is the origin state, so "exits" to that state correspond to remaining in the origin state). We then generate a uniformly-distributed random number and decide the next state on the basis of which sub-interval this falls within.
[13] In fact, simulations that that take no account of attrition are very similar, confirming the findings from other studies (e.g. Cappellari and Jenkins 2004) that attrition often has little effect on eventual results.

<FIGURE 2>

Figure 3 presents the baseline transition intensities between all states as spell length increases. As described earlier, the piecewise specification of the baseline intensity was specific to the transition type. This is evident from the charts; the employment to unemployment intensity exhibits more changes over time than the NEA to education intensity, for instance. Ideally, these intervals would be as narrow as possible in order to avoid restricting the pattern of exits. However, the ability to do this depends on the available data and pattern of transitions. Having fewer, wider intervals clearly limits how fully duration dependence can be captured. To the extent that it was possible, the baseline intensities were allowed to vary freely in the early stages of a spell, before slowly settling to their long-term rates.

Since the model controls for unobserved heterogeneity, these estimates have a causal interpretation. The results show negative duration dependence in the employment to unemployment transition intensity and also, beyond the first month, in the unemployment to employment transition intensity. This is consistent with Kalwij (2004) who finds negative duration dependence in young men's transitions into and out of unemployment in the UK. However, Figure 3 provides additional insight. While transitions from employment to all other destinations show negative duration dependence, transitions from unemployment to destinations other than employment do not. This is consistent with Doiron and Gørgens' (2008) finding that constant baseline intensity cannot be rejected for youth transitions from unemployment to inactivity in Australia. The Kalwij (2004) finding of negative duration dependence in unemployment exits may reflect the fact that fact that the hazard rate is dominated by transitions into employment due to them being more common than transitions to other states.

For transitions from NEA, there is no comparable evidence in the literature. The small number of available observations meant that the piecewise specification of the baseline intensity could not be as granular as when considering other transitions. Despite this, Figure 3 is suggestive of negative duration dependence to all destinations. Transitions out of education, on the other hand, are more complicated. After peaking at the very start of the spell, transitions to all destinations show little sign of duration dependence. If anything, there is positive duration dependence over the medium- to longer-term, presumably as individuals' courses complete.

<FIGURE 3>

Table 4 presents the estimated lagged dependences. The upper panel shows estimates without taking account of unobserved heterogeneity. This is included simply to illustrate how controlling for unobserved heterogeneity (see results in lower panel) alters the conclusions we would otherwise reach about the nature of cross-spell dynamics. For instance, the upper panel results suggest that transitions from employment to unemployment are increased where the individual was unemployed directly before finding work or where there had been two or more previous spells of unemployment. The lower panel, however, finds no such relationships, suggesting that the upper panel results reflect sorting rather than capture causal relationships.

The estimates in the lower panel suggest that prior unemployment experience has no significant effect on the intensity of transitions from employment to unemployment. However, the length of time spent unemployed prior to working does reduce the intensity of transitions from employment into education; a 10 per cent increase in this lagged duration reduces the transition intensity by 4.2 per cent. The only other significant effect on transitions from employment is the length of time previously in education. Here, a 10 per cent increase reduces the intensity of transitions into unemployment by 1.1 per cent. This last finding is consistent with higher-skilled individuals achieving better matches. Acquiring more skills through education may enable individuals to enter higher quality jobs for which firing costs may also be higher. Consequently, employers take greater care at the recruitment stage to ensure a good match. The fact that the duration of job search does not appear to affect the rate of employment exit suggests that for this age group it may preferable to move into work quickly rather than to hold out for a better job offer. One thing that is not visible in this model is on-the-job search. Cockx and Picchio (2012) find a negative impact of prior unemployment duration on job-to-job transitions but not on employment exits. This implies that longer job search achieves a better job match but that those in poor matches continue to search on the job and, through this, manage to stay in work.

Turning to exits from unemployment, Table 4 shows that the length of a preceding NEA spell significantly reduces the transition intensity into work. A 10 per cent increase in this lagged duration reduces the intensity by 3.9 per cent. There is also evidence of occurrence dependence. Having previously experienced unemployment at some point reduces the intensity of a transition into work by 22.6 per cent; having multiple prior unemployment spells reduces it by 32.9 per cent (and also reduces the transition intensity into education). These results have a human capital interpretation; skills may deteriorate during a period out of work and education, and this is picked up in the model by both the number of prior unemployment spells and the duration of the preceding NEA spell. It is also consistent with signalling. In this case, employers use applicants' labour market history as a screening device and may construe multiple unemployment spells or length periods out of work as an indicator of low productivity.

With NEA spells, having been unemployed immediately beforehand reduces by 69 per cent the transition intensity back into unemployment while the duration of a preceding unemployment spell has a positive effect; a 10 per cent increase in this lagged duration appears to increase the transition intensity by 4.0 per cent. This combination of results is difficult to interpret without knowing the reason for moving from unemployment to NEA. One possibility is disengagement from the labour market. However, this kind of discouraged worker reasoning is not consistent with the estimated positive lagged duration dependence. While puzzling, both these results are only marginally significant so we should be cautious about over-interpreting them. Lastly, there is also marginally significant evidence that having had multiple prior NEA spells substantially increases the transition intensity into education.

With regard to transitions from education, having been unemployed immediately beforehand reduces the intensity of transitions into NEA by 76 per cent (again, only marginally significant). More definite is the effect of employment experience. A 10 per cent increase in the length of the preceding employment spell reduces the intensity of transitions into unemployment and NEA by 2.9 per cent and 5.1 per cent, respectively. A similar proportionate increase in the length of the preceding unemployment spell, on the other hand, increases the intensity of transitions into NEA by 8.0 per cent.

<TABLE 4>

It is appropriate at this point to consider how these results relate to other empirical evidence. Most comparable is Kalwij (2004) who considers young people in the UK. He finds a significant effect of prior unemployment on exits from unemployment but not on returns to unemployment. This is consistent with our findings. His finding of negative lagged duration dependence in unemployment exits cannot be compared with our results due to differences in model parameterisation. However, his finding that unemployment re-entry (loosely, employment exit) is not affected by the duration of the previous spell of unemployment is again consistent with the findings summarised in Table 4. Böheim and Taylor (2002) provide additional results for the UK, this time for adults of all ages. They find that the incidence of unemployment inflicts a penalty on subsequent job tenure but, similar to our results, the duration of previous unemployment has no effect. Tatsiramos (2009) also finds no significant effect of the length of prior unemployment spell on the rate of employment exit in the UK. However, using the European Community Household Panel, he highlights important differences across countries. Such variation is also evident in single country studies of young people. In Belgium, Cockx and Picchio (2013) find that lagged unemployment duration reduces the employment exit hazard (for men but not for women), Bratberg and Nilsen (2000) show a similar finding for Norway while Doiron and Gørgens (2008) find no such effect in the case of Australia.

Fewer studies examine the effect of employment experience. Böheim and Taylor (2002) provide marginally significant evidence that having more prior employment experience reduces the rate at which men return to unemployment but is not significant for women. Our results find no lagged effects of employment, except in transitions from education. Given the weakness of the Böheim and Taylor (2002) finding for men, we do not view our results to be in stark contradiction. Empirical evidence from other countries on the role of employment experience on young people's transitions is limited. For Belgium, Cockx and Picchio (2013) find marginally significant evidence of negative occurrence dependence in the employment exit hazard for young men. For young women they find strong evidence of negative lagged duration dependence in employment exits. For Australia, Doiron and Gørgens (2008), considering males and females together, find that the number of prior employment spells increases the intensity of transitions from unemployment into employment but that no other lagged dependences are significant.

Overall, the results in this paper appear broadly consistent with the results from other UK studies but there is considerable variation across countries in empirical findings; not surprising perhaps, given the fundamental institutional, cultural and economic differences across countries.

With regard to our first key research question, Table 5 shows the importance of distinguishing between unemployment and NEA when considering transitions into employment. The upper panel presents results for transitions from unemployment into employment. Each estimate is the proportionate increase or decrease in the transition intensity due to an individual's unemployment starting with an NEA spell of a specified length. The logic behind the approach is that if there is no behavioural difference between unemployment and NEA, then there should be no effect on the transition intensity since, in both cases, the overall length of time out of work is the same.

The results in Table 5 suggest that unemployment and NEA are qualitatively different, although not always. The upper panel suggests that unemployed people who have been out of work for up to a year do not have a significantly different transition rate if they spent the first 6 months or twelve months of their workless spell NEA rather than unemployed. However, when longer workless spells are considered, the negative effect of NEA becomes apparent. The results confirm those of Flinn and Heckman (1983), but go further by suggesting that it is prolonged NEA spells that are particularly damaging. NEA spells of 6 months do not significantly affect the probability of finding work.

The bottom panel of Table 5 follows the same format as the upper panel but focuses now on transitions from NEA. The evidence of a significant difference arising from being initially unemployed rather than NEA is much weaker. There is some suggestion of initial short spells of unemployment reducing the probability of finding work. This is surprising if we view unemployment as being closer to the labour market than NEA. Although speculative, one interpretation might be that this captures discouraged workers, who having been unsuccessful in their job search become more disengaged than other NEA individuals.

<TABLE 5>

5. Simulating the effect of a hypothetical active labour market programme

To understand how the effects reported in the previous section interact to shape future outcomes requires the use of simulation methods. In this section, we explore the combined effects of duration, lagged duration and occurrence dependence by simulating a hypothetical active labour market programme (ALMP). This addresses our second key research question of how prior experience affects subsequent outcomes.

5.1 Characteristics of the ALMP

Our ALMP is designed to have some of the characteristics of work experience schemes in the UK and elsewhere. It is targeted at the unemployed, reflecting the reality that there is unlikely to be a policy lever available to require NEA individuals to participate. With unemployed people, the threat of benefit withdrawal is the main instrument to incentivise participation.

Participation in the scheme is compulsory after a specified period of time unemployed. On entering the ALMP, the individual continues to be regarded for the purposes of simulation as unemployed. Exits can occur with the same probability as in the case where there is no ALMP. This parallels the common situation whereby individuals exit their ALMP before it has completed.

The intervention affects transitions from the point of ALMP exit onwards. By this point, individuals who have completed the full ALMP are regarded as having acquired a number of months of work experience equal to the duration of the ALMP. We implicitly treat this work experience as exerting a similar effect on subsequent transitions as actual (that is, unsupported) employment experience. Informally, we might regard this as a kind of idealised work experience programme that is indistinguishable from unsupported employment.

How the experience acquired through the ALMP influences subsequent transitions can be seen by considering its effect on the variables in the model. For an individual completing the ALMP, the relevant variables are updated as follows

- the duration of the unemployment spell is reset to 1
- the preceding state is recoded as "employed"
- the length of the preceding employment spell is set to the duration of the ALMP.

Individuals who exit the ALMP before completion by entering employment have their preceding state set to "unemployed" and the length of their prior unemployment is set to its length at the point of ALMP entry. In this case, the ALMP period is implicitly regarded as a suspension of the unemployment spell. Alternatively, those who exit early by beginning a NEA or education spell have their preceding state set to "employed" and the length of their prior employment set to the length of time they participated in the ALMP. This regards the ALMP is a suspension of unemployment that provides employment experience.

Whether this represents a reasonable approximation to a real-life ALMP is of course debatable. The balance attempted through the approach adopted here is to allow the ALMP to provide experience that is a substitute for actual employment experience without going so far as to treat the work experience scheme as open-ended employment.

5.2 How treatment on the treated effects differ with programme parameters

The first part of the simulation exercise explores how the effectiveness of the ALMP varies according to the point in the unemployment spell at which it becomes compulsory (the 'onset') and its duration. First, the baseline (no programme) labour market history is simulated. Second, an alternative labour market history, with the ALMP, is simulated. Comparing outcomes in the months following programme entry with the corresponding months in the baseline case provides an estimate of the impact of the programme.

The simulation approach is similar to that used to assess the fit of the model (Section 4) except that now both males and females are considered. Furthermore, the model controls for the effect of attrition rather than imposing simulated attrition on the estimates as before. A range of hypothetical ALMPs are considered. These vary in two dimensions: onset and duration. Since participation in those with longer onsets is uncommon, we simulate 10,000 rather than 1,000 histories for each ALMP considered. This is to allow a sufficient number of simulated participants to be observed. We concentrate for now on the effect of participation on employment (additional outcomes are considered later).

Manipulating the onset and duration of this hypothetical work experience programme changes the estimated impact. Figure 4a shows the result of varying the onset of the programme from 1 month of unemployment (corresponding to the case of immediate entry) to 24 months of unemployment. Likewise, the duration is allowed to vary from 1 to 12 months. Each cell of Figure 2 corresponds to a unique combination of onset and duration and the entry in each cell gives the percentage point impact on employment 12 months after onset. Cells are shaded according to their estimated impact size in order to visualise the results more readily.

A first comment to make is that the pattern shown in Figure 4a is very mixed and would have been difficult to predict from the coefficient estimates reported in Table A.1. This demonstrates the value of simulation as a means of seeing how a range of different effects can combine in a non-linear way to give an overall impact. However, it is important to note that the impact estimates in each row relate to a different group of individuals. For instance, the impact estimates in the top row relate only to individuals who remain unemployed for a period of two years. This is a sub-group of those who experience any unemployment (and whose estimated impacts are reported in the bottom row).

With this caveat in mind, Figure 4a shows (reading from bottom to top) that treatment on the treated effects associated with interventions targeting individuals with longer unemployment spells are greater than those for interventions targeting individuals with shorter unemployment spells. Reading from left to right across Figure 2 shows that shorter duration interventions often have greater effects than longer ones. Taking both dimensions together we see that, while increasing onset and reducing duration generally increases impacts, this is not always the case and the pattern mapped out by these different combinations is more variegated.

<FIGURE 4A>

It is perhaps surprising to find that shorter duration interventions are more effective, particularly for interventions requiring a longer qualifying unemployment spell. Part of the explanation for this is that treating the unemployment spell as starting afresh after completing the ALMP can make a large difference, since the intensity of transitions into employment is much higher in the early stages of an unemployment spell. This raises concerns about how reasonable it is to reset the clock on unemployment in this case, since it is unlikely that a short intervention could be expected to overcome the negative effect of a long unemployment spell. With longer interventions resetting the clock in this way is more defensible, particularly if the period of work experience has served to improve motivation and increase employability.

Figure 4b presents similar results to Figure 4a but considers a longer-term outcome; employment 24 months after entering the ALMP. This shows a different pattern. Impacts are still greater for interventions with a longer qualifying period (onset), but with this longer-term outcome, the duration of the intervention appears positively related to its effectiveness.

<FIGURE 4B>

5.2 The detailed effects of a work experience programme

In this sub-section, we consider in more detail the effect of a (hypothetical) work experience programme that is compulsory after being unemployed for 12 months and that lasts for 12 months. This choice of onset and duration was informed by the results summarised in Figure 4b which suggest that this combination may be expected to have reasonably large 24-month employment impacts relative to many of the others considered. Clearly, some combinations appear to deliver larger impacts, but the choice also reflects a desire to focus on a programme with onset and duration values in the range typical of actual programmes in the UK.[14] Furthermore, the longer-term impacts are often associated with programmes requiring a longer spell of unemployment. The proportion of individuals qualifying for such spells is smaller than the proportion qualifying on the basis of being unemployed for a year.[15]

The results in this section simulate histories for the full sample, from the point of first being observed onwards. This is different from the simulation approach adopted so far but is appropriate since we are interested in the statistical significance of the impacts and want to allow for uncertainty in the estimated parameters but also variation in characteristics across the population. The simulations use 1,000 draws from the distribution of estimated parameters.

Figure 5 shows the estimated impact on monthly employment, unemployment, NEA and education levels in the five years following ALMP entry. The programme considered here is predicted to significantly increase participants' probability of employment during the period 16-27 months after entry. The size of this effect peaks at 4.2 percentage points after five years. This positive impact is mirrored by a reduction in the probability of unemployment during the same period, with the maximum reduction in any month being 3.8 percentage points. The impact on NEA probability is smaller (just below 1 percentage point at its peak) and never attains statistical significance at the conventional level. Similarly, there is no effect on education.

[14] Since June 2011, ALMPs in the UK have been streamlined into a single " Work Programme", for which participation is compulsory after 9 months unemployment (for young people). Prior to this, the "Flexible New Deal" was compulsory at 12 months unemployment. Prior to that, entry to the "New Deal for Young People" was compulsory after 6 months unemployment and participation in the active elements (including work experience schemes) began some four months later.

[15] The simulation results suggest that approximately 13 per cent of the sample will experience a 12-month spell of unemployment.

There are two more general points suggested by Figure 5. First, the fact that there are impacts on employment and unemployment but not on NEA or education reflects the distinction between economic activity and economic activity. Had the intervention significantly affected the probability of being NEA, the conclusion might have been that that status is similar in some ways to conventional unemployment. Instead, the results further reinforce the impression that unemployment and NEA are behaviourally distinct states. An alternative way of viewing this is that an intervention of the type considered here may help unemployed young people find work without having any side effects on NEA or education levels.

The second general point is that the impacts on employment and unemployment do not last. It is possible that the high level of movement between states means that young people have an above-average chance of achieving sustained employment without external encouragement. In this case, a work experience scheme of the type considered here may accelerate the process but individuals in the counterfactual no-ALMP world will nevertheless catch up. Alternatively, the intervention may provide a temporary boost to participants who, with time, fall back in line with what their trajectory would have been anyway. It is not possible empirically to distinguish between these two explanations.

<FIGURE 5>

6. Conclusion

This paper has examined labour market transitions among young people in the UK from the point of school-leaving age onwards. Using an econometric model of transitions between four states – employment, unemployment, NEA and education – that allows for the influence of unobserved heterogeneity and non-ignorable attrition, we have been able to identify the effect of prior spells on current transitions.

We make a number of contributions. First, despite much research activity around the issue of the youth labour market, there is little detailed econometric analysis of their transitions between states. Our results update the existing evidence but also extend it in important ways. We look beyond transitions into and out of employment by allowing young people to occupy four potential states. The importance of doing this is demonstrated by the fact that the influences on exit rates vary according to the destination state. Formal testing showed a significant difference between unemployment and NEA status. The policy focus on NEETs (essentially, the combination of unemployment and NEA) may miss important differences. Equally, treating NEA and education as a single state ignores important differences between these two statuses.

Second, we provide rich evidence on the effect of experience on subsequent outcomes. This is clearly important for young people, who all begin with the same (zero) stock of experience and, over time, can acquire employment experience which enhances their long-term attractiveness to employers or unemployment/NEA experience which may weaken their ability to compete for jobs in future. This complements the existing literature on scarring, which typically relates adult outcomes to experiences when younger. By considering month-on-month transitions, our results provide an insight into how these longer-term effects evolve. Our results also extend the scarring literature by considering not only the effects of youth unemployment but also the effects of employment, of NEA and of education. Furthermore, we provide updated evidence on duration dependence and allow this to vary depending on destination.

Third, as a demonstration of the insights permitted by the model, we use the estimation results to simulate young peoples' labour market histories and show the potential effects of a range of hypothetical compulsory work experience programmes. We show how the characteristics of such programmes have important consequences for effectiveness. In this way, the results may provide some guidance for the design of such interventions. We consider a fairly typical 12-month programme for those unemployed for a year. This is simulated to result in a temporary employment increase and unemployment reduction, with no side-effects on NEA or education

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Appendix

The full estimation results are presented in Table A.1. Below, we highlight particular findings which, although not the primary focus of this paper, are of interest in their own right.

Sex

The results show significant differences between males and females. We capture gender differences through the inclusion of a female dummy and a further dummy that identifies mothers. This is in recognition of the fact that it is child care that mostly differentiates female from male labour supply. Ideally, we would estimate the model separately for men and women. However, the sample is too small to permit this. The results show that young women without children behave similarly to young men in their transitions between employment and unemployment. In all states, the transition intensity into NEA is higher for women than men. Where women have children, their transitions to NEA are increased further still, and transitions into anything other than NEA are reduced. Clearly, the situation facing mothers with young children is fundamentally different from that facing other young unemployed people. In other words, young motherhood is a major factor when considering economic inactivity among those who have left education.

Age

Transition intensities from employment to unemployment or education decline with age. Conversely, transition intensities from unemployment to education increase with age, almost monotonically. This may be reflect, in part, the effect of accumulated experience that has not been included in the model (due to a lack of formal identification results). It may also, however, reflect other factors such as improved job search technique, wider networks, less age-based discrimination from employers and so on.

Qualifications at age 16

Information on qualifications at age 16 is included as a proxy for academic ability. There is a clear gradient with regard to transitions from employment to unemployment and education. Transition intensities into unemployment are lower where qualifications at 16 are higher. Transition intensities into education are higher where qualifications at 16 are higher. Considering transitions out of unemployment into employment, there is no evidence of a gradient but having no qualifications significantly reduces the intensity. Attainment at age 16 is known to be an important predictor of staying on in education and this is confirmed in our results.

Variables included only in the transitions from education

Crawford et al. (2010) have shown that those who are the youngest in their academic year tend to perform less well than their older peers. We include in the equations covering exits from education dummy variables showing whether the individual's birth month was between September and December or January and April (the reference category being May to August – the youngest children in the academic year). Those born between September and December were found to move more quickly from education into employment. We also allow transitions from education to vary with parental education level and whether the individual lived in a workless household at age 16. The results show that young people are slower to leave education the more qualified their parents are. Parental employment (at age 16) is associated with a reduced transition intensity from education to unemployment and NEA, but an increased rate of entering employment.

Household income at age 16

Information on household income (immediately prior to school-leaving age) is included. This is captured through variables showing the income quartile of the household at that time. As well as proxying other unobserved background characteristics, household income is relevant since liquidity constraints are likely to play an important role in transitions out of, and into, non-compulsory education. The results show that individuals from the highest income quartile have a significantly lower intensity than lower income individuals of transition from education to unemployment. They also have a higher transition intensity from employment into education.

The business cycle and the local labour market environment

With regard to the economically active population, the transition intensity from unemployment into employment is higher when the economy is strong. Individuals living in areas with high relative levels of unemployment tend to move more quickly from work to unemployment but more slowly in the opposite direction. An intriguing result is that transition intensities into NEA from both employment and unemployment increase when the economy is growing. This perhaps suggests that NEA should not automatically be viewed as the result of disadvantaged circumstances but may sometimes be the result of choice. Higher relative unemployment is associated with reduced transitions from education into employment and increased transitions from education into unemployment. Furthermore, both workers and NEA individuals are more likely to return to education in areas that have relatively high levels of unemployment.

Sample attrition

Preliminary tests suggested it was sufficient to allow the attrition baseline hazard to vary freely over the first seven interview waves but to be fixed beyond that. As often found, attrition occurs most frequently in the earlier waves. Attrition is also lower among females and those with partners. There is some regional variation, with those in the reference area (London) more likely to drop out than those in other areas. The results show that the attrition hazard is increased where there is a change of interviewer.

Table A.1: Full estimation results showing transitions between work (w), unemployment (u), NEA (n) and education (e) and the attrition hazard.

	w→u	w→n	w→e	u→w	u→n	u→e	n→w	n→u	n→e	e→w	e→u	u→n	Attrit.
Baseline hazard:													
- month 1	1.395		1.106	0.164						0.795	0.417	1.469	
	(0.152)		(0.172)	(0.145)						(0.131)	(0.204)	(0.237)	
- month 2	1.240		1.016	0.316						-0.689			
	(0.156)		(0.179)	(0.144)						(0.158)			
- month 3	1.187		1.248	0.629						-0.412			
	(0.159)		(0.177)	(0.142)						(0.150)			
- months 1-3		0.728				-0.051	0.136	0.274	0.356				
		(0.187)				(0.177)	(0.186)	(0.228)	(0.188)				
- months 2-3											-0.638		
											(0.216)		
- months 1-6					-0.471								
					(0.198)								
- months 4-6	1.056		0.946	0.609			0.450			-0.427	-0.547		
	(0.141)		(0.170)	(0.131)			(0.189)			(0.131)	(0.211)		
- months 2-12												-0.245	
												(0.203)	
- months 4-12		0.373											
		(0.158)											
- months 7-12	0.822		0.906	0.297						-0.640	-0.991		
	(0.131)		(0.136)	(0.129)						(0.114)	(0.184)		
- months 13-18	0.496									-0.482			
	(0.141)									(0.107)			
- months 13-24											-0.560		
											(0.147)		
- months 19-24	0.497									-0.384			
	(0.148)									(0.099)			

Baseline hazard (attrition):													
- *wave 1*													-0.209
													(0.251)
- *wave 2*													0.141
													(0.214)
- *wave 3*													0.365
													(0.187)
- *wave 4*													0.107
													(0.179)
- *wave 5*													0.274
													(0.173)
- *wave 6*													0.125
													(0.178)
- *wave 7*													0.559
													(0.170)
- *wave 8*													0.615
													(0.176)
Preceding spell													
- *Employed*				0.223	-0.122	0.213	0.479	0.447	-0.363	-0.194	0.510	0.045	
				(0.166)	(0.448)	(0.403)	(0.426)	(0.573)	(0.506)	(0.339)	(0.472)	(0.541)	
- *Unemployed*	0.165	-0.253	-0.095				-0.525	-1.167	-0.759	0.527	0.166	-1.405	
	(0.193)	(0.362)	(0.325)				(0.572)	(0.637)	(0.698)	(0.415)	(0.545)	(0.781)	
- *NEA*	0.021	0.130	0.009	0.484	-0.786	-1.222							
	(0.370)	(0.415)	(0.498)	(0.346)	(0.670)	(1.061)							
Lagged duration dependence													
Length of previous spell, by type (log months):													
- *Employed*				0.042	-0.063	-0.171	-0.156	-0.262	-0.102	-0.043	-0.309	-0.551	

				(0.049)	(0.148)	(0.161)	(0.097)	(0.180)	(0.178)	(0.074)	(0.132)	(0.217)
- Unemployed	0.112	0.123	-0.453				-0.090	0.411	-0.346	-0.229	0.191	0.808
	(0.076)	(0.160)	(0.188)				(0.250)	(0.219)	(0.352)	(0.190)	(0.217)	(0.346)
- NEA	-0.209	0.102	-0.198	-0.422	0.245	0.560				-0.166	-0.022	-0.133
	(0.198)	(0.153)	(0.304)	(0.173)	(0.231)	(0.466)				(0.185)	(0.248)	(0.198)
- Education	-0.115	-0.071	0.065	0.030	-0.173	0.074	0.004	-0.146	-0.089			
	(0.056)	(0.093)	(0.068)	(0.056)	(0.152)	(0.127)	(0.128)	(0.179)	(0.139)			
Occurrence dependence:												
Number of prior employment spells												
- 1	-0.054	-0.019	0.118									
	(0.098)	(0.156)	(0.128)									
- 2 or more	0.053	0.054	0.173									
	(0.152)	(0.214)	(0.220)									
Number of prior unemployment spells												
- 1				-0.256	-0.202	-0.235						
				(0.082)	(0.231)	(0.219)						
- 2 or more				-0.399	0.128	-1.195						
				(0.112)	(0.291)	(0.371)						
Number of prior NEA spells												
- 1							0.040	0.401	-0.137			
							(0.186)	(0.249)	(0.299)			
- 2 or more							0.212	-0.827	1.021			
							(0.322)	(0.749)	(0.573)			
Number of prior education spells												
- 1										0.227	0.413	0.605
										(0.297)	(0.409)	(0.379)

- 2 or more										0.046	0.067	-0.075	
										(0.323)	(0.464)	(0.486)	
Female	-0.031	0.568	0.196	0.061	1.164	0.309	-0.027	-0.125	-0.276	-0.045	-0.256	0.356	-0.234
	(0.070)	(0.130)	(0.083)	(0.065)	(0.190)	(0.152)	(0.153)	(0.209)	(0.166)	(0.051)	(0.071)	(0.118)	(0.069)
Children													-0.218
													(0.288)
Female with children	0.061	1.705	-0.653	-0.852	0.837	-0.595	-1.536	-1.617	-1.259	-1.230	-0.177	1.898	-0.474
	(0.230)	(0.196)	(0.532)	(0.211)	(0.273)	(0.582)	(0.211)	(0.284)	(0.295)	(0.446)	(0.447)	(0.309)	(0.334)
Age:													
-17	-0.166	0.083	-0.302	0.115	-0.434	-0.029	0.693	-0.316	-0.321	0.218	-0.248	-0.437	
	(0.125)	(0.328)	(0.173)	(0.113)	(0.331)	(0.261)	(0.331)	(0.455)	(0.340)	(0.096)	(0.142)	(0.232)	
-18	-0.130	0.367	-0.302	0.352	-0.030	-0.719	0.399	0.146	-0.665	0.494	-0.373	0.437	
	(0.145)	(0.332)	(0.198)	(0.131)	(0.361)	(0.334)	(0.366)	(0.452)	(0.384)	(0.128)	(0.189)	(0.256)	
-19	-0.241	0.274	-0.432	0.625	0.399	-0.316	0.684	0.366	0.018	-0.008	-0.858	-0.100	
	(0.166)	(0.352)	(0.229)	(0.149)	(0.397)	(0.377)	(0.385)	(0.463)	(0.392)	(0.161)	(0.238)	(0.306)	
-20	-0.395	0.080	-0.495	0.700	0.023	-0.712	0.814	0.185	-1.684	0.072	-0.783	-0.471	
	(0.187)	(0.371)	(0.250)	(0.165)	(0.467)	(0.462)	(0.415)	(0.527)	(0.595)	(0.178)	(0.261)	(0.373)	
-21	-0.387	-0.197	-1.267	0.695	0.162	-0.800	0.645	0.349	-1.494	0.920	-0.331	0.612	
	(0.201)	(0.390)	(0.279)	(0.176)	(0.483)	(0.487)	(0.424)	(0.521)	(0.528)	(0.187)	(0.278)	(0.343)	
-22	-0.581	-0.108	-1.934	0.822	-0.496	-0.977	1.287	0.019	-1.254	1.435	0.221	0.935	
	(0.215)	(0.391)	(0.320)	(0.183)	(0.567)	(0.534)	(0.423)	(0.584)	(0.519)	(0.212)	(0.307)	(0.381)	
-23	-0.692	-0.454	-2.221	0.970	0.226	-0.647	1.640	-0.135	-1.498	1.735	0.117	0.475	
	(0.233)	(0.416)	(0.369)	(0.204)	(0.549)	(0.615)	(0.458)	(0.652)	(0.615)	(0.250)	(0.389)	(0.518)	
-24	-0.975	-0.310	-3.076	0.545	-0.717	-0.693	1.005	-1.181	-2.854	1.494	0.638	0.176	
	(0.270)	(0.428)	(0.532)	(0.240)	(0.696)	(0.726)	(0.506)	(0.896)	(1.106)	(0.320)	(0.450)	(0.821)	
Month of birth:													
- September-December										0.152	0.099	0.009	
										(0.062)	(0.088)	(0.138)	
- January-April										0.047	0.078	0.059	
										(0.061)	(0.086)	(0.138)	

Non-white	-0.029	0.155	0.723	-0.089	-0.621	0.314	-0.555	-0.172	-0.195	-0.860	-0.454	-0.377	0.415
	(0.190)	(0.370)	(0.191)	(0.161)	(0.527)	(0.305)	(0.441)	(0.496)	(0.359)	(0.143)	(0.154)	(0.273)	(0.142)
Partnered													-0.351
													(0.127)
Qualification at age 16:													
- 5-7 A-C GCSEs	0.240	-0.143	-0.623	0.036	-0.855	-0.177	0.581	0.285	0.076	0.277	0.355	-0.281	-0.098
	(0.127)	(0.181)	(0.127)	(0.118)	(0.400)	(0.266)	(0.232)	(0.421)	(0.242)	(0.083)	(0.128)	(0.199)	(0.118)
- < 5 A-C GCSEs	0.166	-0.476	-0.970	0.100	-0.660	-0.388	0.252	0.612	-0.559	0.761	0.793	-0.014	0.102
	(0.106)	(0.162)	(0.113)	(0.102)	(0.280)	(0.220)	(0.202)	(0.320)	(0.218)	(0.074)	(0.105)	(0.156)	(0.100)
- CSE	0.528	-0.560	-1.342	-0.034	0.067	-0.497	-0.214	0.601	-0.645	1.082	1.328	0.645	0.370
	(0.122)	(0.200)	(0.171)	(0.113)	(0.259)	(0.254)	(0.233)	(0.320)	(0.269)	(0.096)	(0.125)	(0.196)	(0.111)
- none	0.662	0.257	-1.046	-0.416	-0.079	-0.965	-0.922	0.364	-1.416	0.536	1.161	0.432	0.283
	(0.133)	(0.193)	(0.180)	(0.121)	(0.272)	(0.291)	(0.253)	(0.318)	(0.324)	(0.105)	(0.128)	(0.198)	(0.122)
Household income quartile, age 16													
- quartile 1 (lowest)	0.141	0.046	-0.463	-0.252	-0.327	-0.508	-0.588	-0.424	-0.467	0.095	0.498	0.035	0.244
	(0.112)	(0.199)	(0.130)	(0.112)	(0.286)	(0.274)	(0.227)	(0.320)	(0.260)	(0.091)	(0.144)	(0.202)	(0.110)
- quartile 2	-0.259	0.031	-0.393	-0.107	0.054	-0.087	-0.405	-0.761	-0.458	0.139	0.430	0.056	-0.037
	(0.112)	(0.187)	(0.119)	(0.110)	(0.287)	(0.266)	(0.212)	(0.326)	(0.254)	(0.083)	(0.138)	(0.184)	(0.109)
- quartile 3	-0.175	0.068	-0.335	0.058	-0.256	0.107	-0.049	-0.719	-0.165	0.090	0.360	0.031	0.068
	(0.111)	(0.190)	(0.118)	(0.111)	(0.323)	(0.268)	(0.211)	(0.348)	(0.247)	(0.081)	(0.138)	(0.179)	(0.107)
Season:													
- January - March	-0.256	-0.237	-1.296	-0.148	-1.254	-1.812	-0.480	-0.205	-0.525	-0.718	-0.080	-0.488	
	(0.090)	(0.180)	(0.207)	(0.082)	(0.319)	(0.444)	(0.202)	(0.326)	(0.422)	(0.102)	(0.170)	(0.279)	
- April - May	-0.216	-0.102	-1.915	-0.135	-1.339	-1.533	-0.432	-0.102	-2.816	0.454	1.381	1.227	
	(0.089)	(0.174)	(0.269)	(0.083)	(0.323)	(0.394)	(0.195)	(0.317)	(1.034)	(0.071)	(0.125)	(0.186)	
- July - September	0.294	0.618	1.319	0.353	0.536	1.144	0.522	0.968	1.799	0.963	0.858	0.789	
	(0.074)	(0.144)	(0.105)	(0.071)	(0.188)	(0.189)	(0.157)	(0.250)	(0.271)	(0.065)	(0.128)	(0.198)	
Local unemployment rate (deviation)	0.089	-0.044	0.069	-0.122	0.008	0.041	-0.048	0.029	0.114	-0.063	0.056	0.013	

	(0.022)	(0.045)	(0.029)	(0.022)	(0.054)	(0.049)	(0.047)	(0.063)	(0.055)	(0.017)	(0.022)	(0.040)
Deviation from trend monthly GDP	-0.047	0.331	0.017	0.107	0.354	-0.029	-0.031	0.056	-0.132	0.071	0.011	0.135
	(0.051)	(0.105)	(0.112)	(0.052)	(0.185)	(0.189)	(0.091)	(0.152)	(0.242)	(0.046)	(0.073)	(0.113)
Year												
-1991	0.847	-1.078	1.614	0.887	-18.419	0.881	1.916	2.036	-8.908	0.119	0.047	-1.326
	(0.443)	(1.155)	(0.831)	(0.414)	(6057)	(1.462)	(1.245)	(1.216)	(249.0)	(0.347)	(0.507)	(0.887)
-1992	1.311	-1.223	-0.169	0.673	-1.338	-0.104	1.766	0.230	0.799	-0.202	-0.119	-1.075
	(0.306)	(1.078)	(0.828)	(0.298)	(1.289)	(1.119)	(0.727)	(1.223)	(1.616)	(0.267)	(0.361)	(0.625)
-1993	0.408	-1.798	0.108	0.369	-0.165	-0.322	0.392	-16.256	0.524	-0.500	-0.200	-1.141
	(0.293)	(1.055)	(0.672)	(0.271)	(0.882)	(1.030)	(0.679)	(2483)	(1.539)	(0.242)	(0.318)	(0.553)
-1994	0.731	-1.171	-0.024	0.407	-0.348	0.213	0.558	0.888	0.434	-0.254	-0.073	-1.529
	(0.279)	(0.624)	(0.707)	(0.268)	(0.894)	(1.095)	(0.640)	(0.796)	(1.660)	(0.247)	(0.339)	(0.613)
-1995	0.574	-0.268	0.651	0.510	0.025	0.013	0.488	0.216	1.257	-0.335	-0.410	-1.805
	(0.263)	(0.444)	(0.611)	(0.255)	(0.834)	(1.039)	(0.587)	(0.798)	(1.361)	(0.235)	(0.337)	(0.632)
-1996	0.619	-0.833	0.587	0.768	-0.307	0.588	0.308	-0.272	1.728	-0.066	-0.274	-0.775
	(0.241)	(0.474)	(0.556)	(0.237)	(0.814)	(0.933)	(0.500)	(0.755)	(1.165)	(0.210)	(0.304)	(0.452)
-1997	0.096	-0.697	0.462	0.736	-0.103	0.887	0.986	0.584	1.404	0.066	-0.324	-1.693
	(0.249)	(0.416)	(0.564)	(0.243)	(0.814)	(0.948)	(0.453)	(0.664)	(1.278)	(0.207)	(0.304)	(0.546)
-1998	0.248	-0.358	0.300	0.508	0.012	0.855	-0.329	-0.097	1.688	0.159	-0.122	-1.413
	(0.256)	(0.398)	(0.609)	(0.253)	(0.833)	(1.011)	(0.522)	(0.752)	(1.315)	(0.217)	(0.315)	(0.519)
-1999	0.249	-0.636	0.674	0.594	-0.389	0.512	0.165	-0.227	1.446	0.192	-0.128	-0.572
	(0.263)	(0.419)	(0.631)	(0.259)	(0.880)	(1.078)	(0.457)	(0.738)	(1.347)	(0.224)	(0.321)	(0.462)
-2000	0.198	-0.720	0.618	0.536	-0.556	0.133	0.202	-0.305	1.850	0.036	-0.181	-0.395
	(0.297)	(0.469)	(0.706)	(0.289)	(0.981)	(1.215)	(0.517)	(0.839)	(1.509)	(0.259)	(0.385)	(0.553)
-2001	0.140	-0.639	0.789	0.244	-1.141	0.559	0.307	-0.469	1.499	0.312	-0.108	-0.571
	(0.297)	(0.463)	(0.702)	(0.292)	(1.003)	(1.204)	(0.502)	(0.839)	(1.529)	(0.253)	(0.372)	(0.541)
-2002	0.092	-0.973	0.510	0.371	-0.114	0.337	-0.144	-0.045	1.561	0.244	-0.206	-0.446
	(0.283)	(0.452)	(0.671)	(0.276)	(0.919)	(1.155)	(0.500)	(0.772)	(1.443)	(0.235)	(0.337)	(0.484)
-2003	0.228	-0.766	0.898	0.433	-0.687	1.118	-0.051	-0.188	1.981	0.074	-0.123	-0.776

	(0.297)	(0.474)	(0.722)	(0.289)	(1.009)	(1.217)	(0.517)	(0.822)	(1.519)	(0.259)	(0.375)	(0.553)	
-2004	0.129	-0.925	0.771	0.388	-0.211	0.126	-0.009	-1.002	1.819	-0.070	-0.458	-0.526	
	(0.286)	(0.455)	(0.659)	(0.282)	(0.917)	(1.145)	(0.493)	(0.835)	(1.415)	(0.245)	(0.370)	(0.522)	
-2005	0.383	-0.941	0.663	0.295	-0.363	-0.182	-0.246	-0.057	1.521	-0.055	-0.180	-0.478	
	(0.248)	(0.408)	(0.604)	(0.247)	(0.840)	(1.048)	(0.450)	(0.686)	(1.283)	(0.215)	(0.307)	(0.438)	
-2006	0.168	-0.596	0.553	0.159	-0.634	-0.430	-0.308	-0.533	1.625	-0.391	0.153	-0.350	
	(0.234)	(0.367)	(0.541)	(0.226)	(0.762)	(0.930)	(0.420)	(0.666)	(1.134)	(0.203)	(0.283)	(0.405)	
-2007	0.065	-0.919	0.233	0.148	-0.707	0.606	-0.177	-0.196	1.362	-0.078	-0.015	-0.570	
	(0.255)	(0.412)	(0.642)	(0.247)	(0.873)	(1.074)	(0.444)	(0.718)	(1.361)	(0.219)	(0.309)	(0.448)	
Parent's highest qualification:													
- *NVQ1*										-0.110	-0.367	0.065	
										(0.081)	(0.099)	(0.182)	
- *NVQ2*										-0.123	-0.307	-0.244	
										(0.104)	(0.135)	(0.268)	
- *NVQ3*										-0.366	-0.600	-0.174	
										(0.096)	(0.130)	(0.226)	
- *NVQ4*										-0.598	-0.899	-0.069	
										(0.090)	(0.122)	(0.196)	
At age 16, parent working										0.178	-0.297	-0.385	
										(0.080)	(0.094)	(0.163)	
Interview changed since last wave													0.320
													(0.075)
Region													
- *North East*													-0.382
													(0.247)
- *North West*													-0.615
													(0.182)

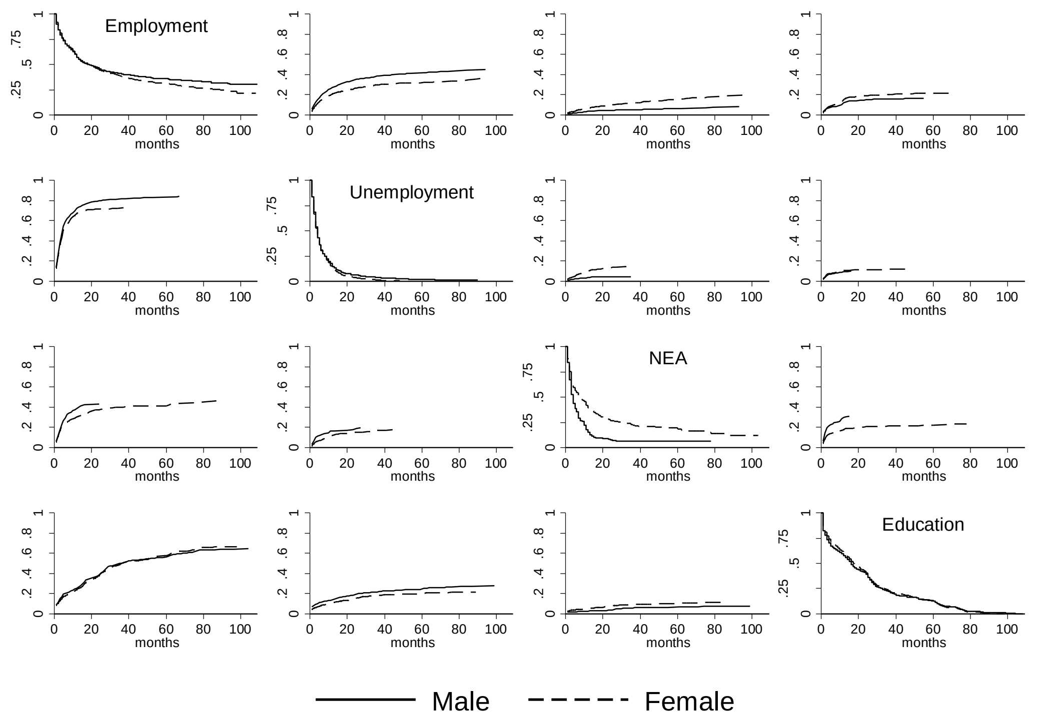
- Yorkshire and Humberside													-0.149
													(0.178)
- East Midlands													-0.149
													(0.178)
- West Midlands													-0.068
													(0.169)
- East of England													-0.632
													(0.199)
- South East													-0.225
													(0.162)
- South West													-0.291
													(0.187)
- Wales													-0.157
													(0.157)
- Scotland													0.295
													(0.155)
- Northern Ireland													0.280
													(0.168)
Constant	-4.830	-5.258	-4.182	-3.753	-3.428	-3.976	-4.416	-3.748	-3.479	-5.113	-5.147	-5.782	-2.976
	(0.401)	(0.676)	(0.735)	(0.331)	(0.961)	(1.143)	(0.714)	(1.061)	(1.404)	(0.302)	(0.436)	(0.606)	(0.234)
Unobserved heterogeneity													
Log of mass points													
- $\ln(\upsilon^2)$	-0.324	-1.746	-1.418	0.157	-0.368	-47.537	3.111	1.207	-11.181	1.154	0.800	-1.056	2.598
	(0.287)	(1.978)	(0.528)	(0.245)	(0.439)	(0.000)	(0.741)	(0.884)	(301.822)	(0.191)	(0.236)	(0.908)	(0.259)
- $\ln(\upsilon^3)$	-0.808	-0.406	-0.698	1.145	-0.342	0.052	1.502	0.115	-0.607	1.362	0.315	0.386	-0.014
	(0.196)	(0.374)	(0.286)	(0.149)	(0.436)	(0.481)	(0.425)	(0.788)	(0.278)	(0.173)	(0.252)	(0.336)	(0.196)
- $\ln(\upsilon^4)$	0.775	0.406	-0.465	1.098	0.406	1.090	1.592	0.580	-0.488	0.597	0.901	0.429	0.680
	(0.196)	(0.344)	(0.358)	(0.130)	(0.353)	(0.305)	(0.439)	(0.529)	(0.325)	(0.183)	(0.248)	(0.354)	(0.211)

Probability masses (logistic transforms):	
- λ^2	-0.629
	(0.297)
- λ^3	0.254
	(0.337)
- λ^3	-0.507
	(0.310)
Resulting probabilities:	
- Prob (group1)	0.292
- Prob (group2)	0.156
- Prob (group3)	0.376
- Prob (group4)	0.176
Log likelihood	-39,686.059
Number of individuals	3,487

The reference individual is a 15 or 16 year old single white male living in London without children, born between May and August, with 8 or more A_C grade GCSEs at age 16. Household income at age 16 of the reference individual is in the highest quartile. Parents are assumed to have no qualifications and not to be working when the reference individual was 16. The survey interviewer remains unchanged between successive waves for the reference individual.

FIGURES

Figure 1: Survival curves and cumulative incidence curves, by sex



Notes: the charts on the leading diagonal show the Kaplan-Meier survival curves for the named origin states. Off-diagonal charts show cumulative incidence curves; the probability of having exited the initial state (in the row) to a specific destination (in the column) as the spell lengthens.

Figure 2: Actual (solid line) and simulated (dashed line) economic status, by month since reaching school-leaving age

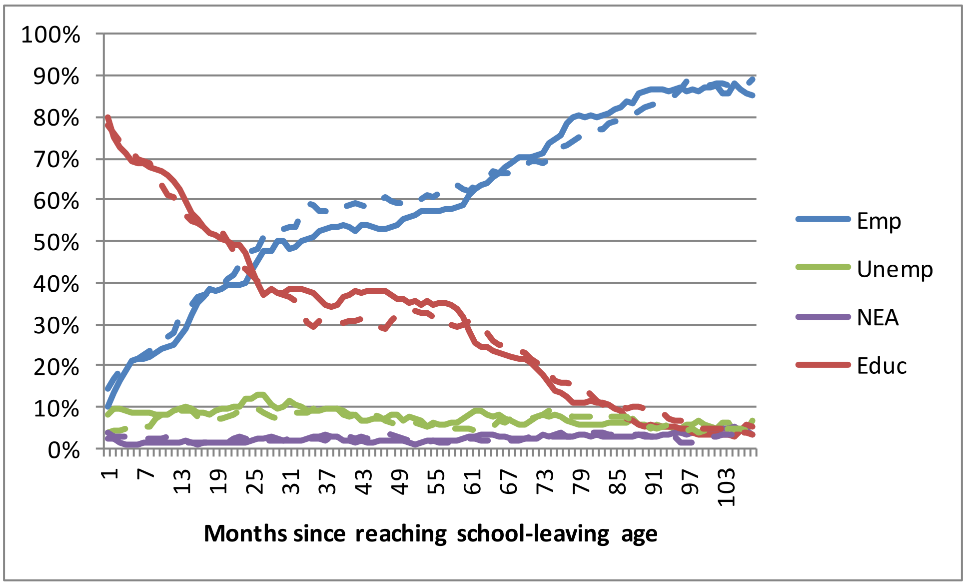


Figure 3: Transition intensities over time

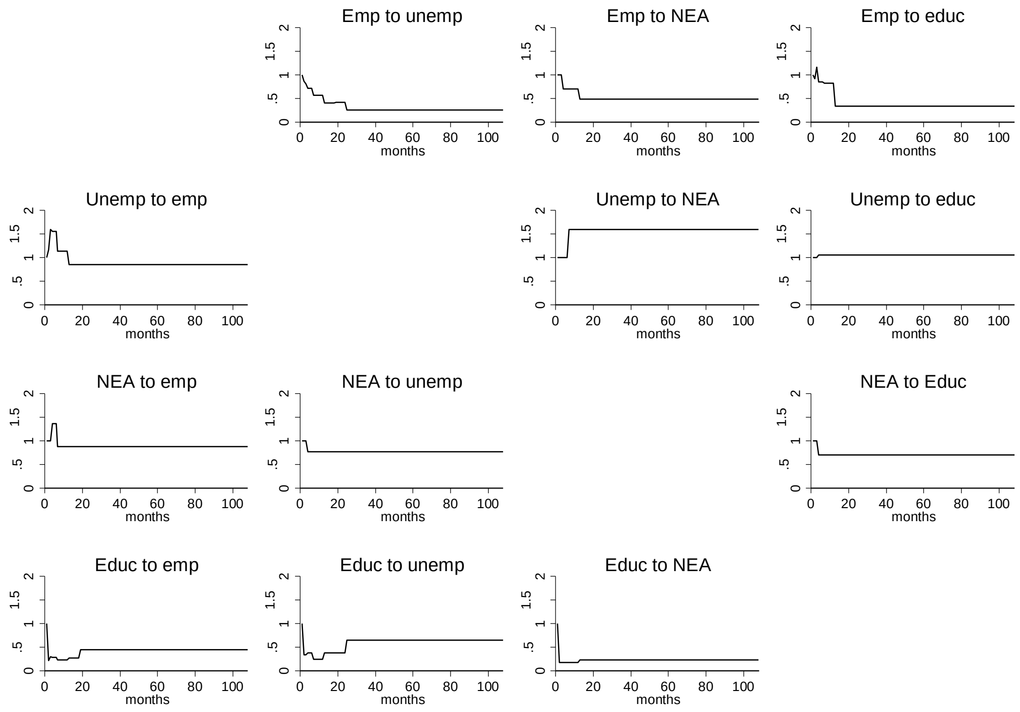
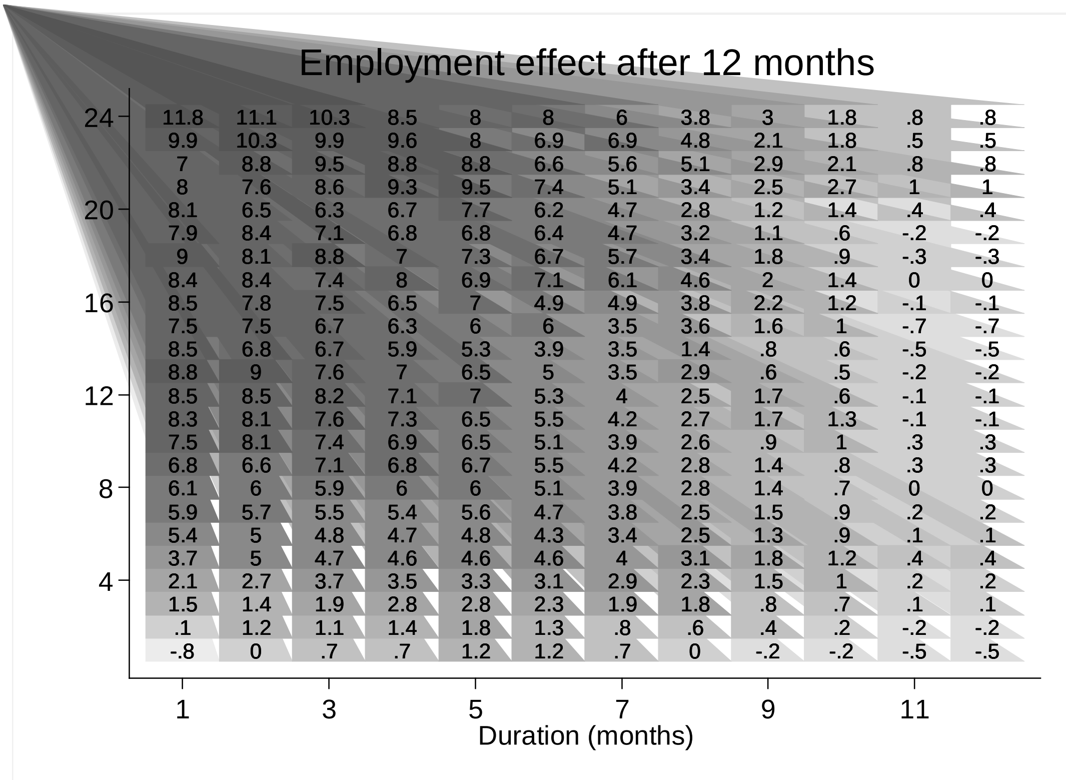
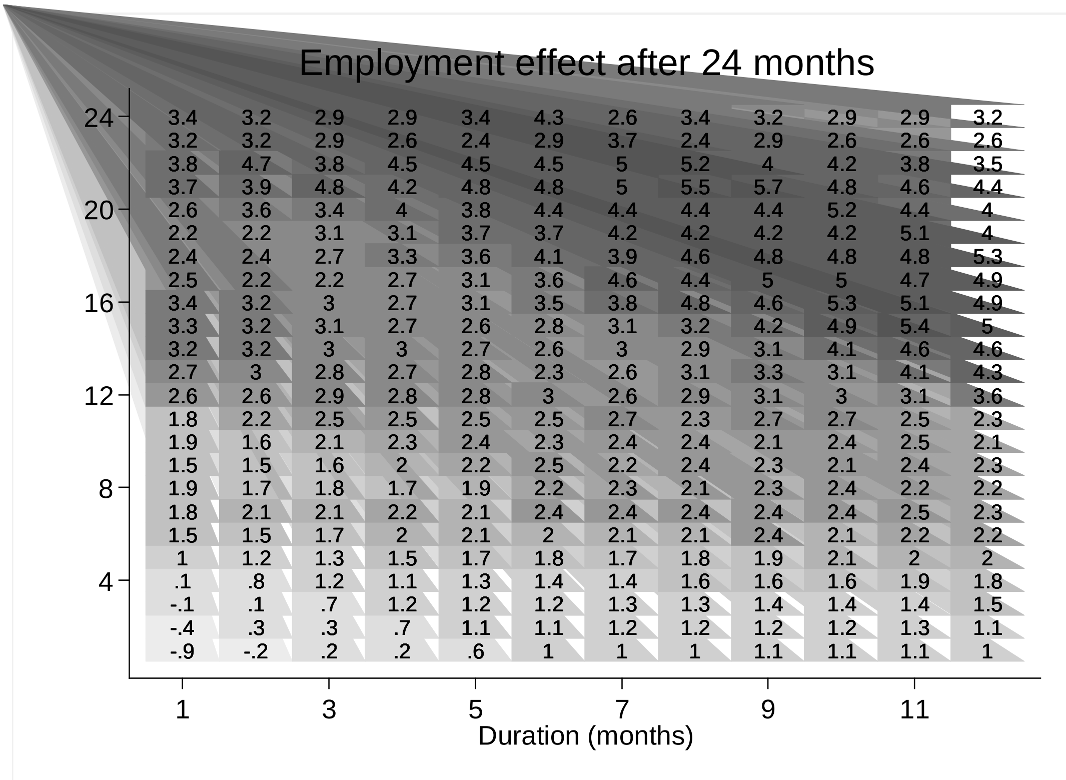


Figure 4a: Simulating the ATET on month 12 employment for work experience ALMP with different combinations of onset and duration



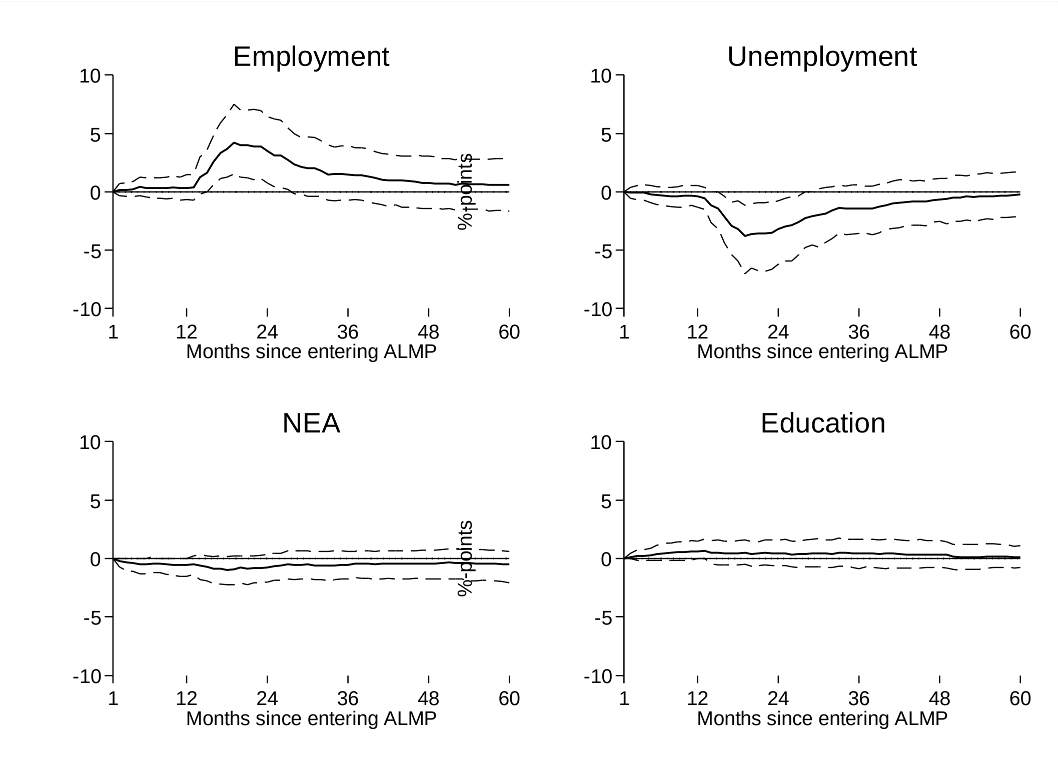
Note: cell entries show the percentage point increase in employment 12 months after starting the work experience scheme. Simulations are based on 10,000 draws for an individual with average characteristics.

Figure 4b: Simulating the ATET on month 24 employment for work experience ALMP with different combinations of onset and duration



Note: cell entries show the percentage point increase in employment 24 months after starting the work experience scheme. Simulations are based on 10,000 draws for an individual with average characteristics.

Figure 5: Simulating the effect of a 12 month work experience ALMP, beginning after 12 months unemployment



Notes: simulations based on 1,000 draws. Dashed lines show 95% confidence intervals

TABLES

Table 1: Summary statistics

Characteristics at age 16	*mean*
Age	15.81
Female	0.52
Non-white	0.05
Qualifications:	
- 8 or more A-C grade GCSEs	0.33
- 5-7 A-C grade GCSEs	0.13
- 1-4 A-C grade GCSEs	0.30
- CSE	0.13
- None	0.11
Living in working household	0.80
Parents' highest qualification:	
- NVQ level 4	0.28
- NVQ level 3	0.15
- NVQ level 2	0.09
- NVQ level 1	0.29
- None	0.19
Number of individuals	3,474
Spell characteristics	
Number of spells	11,753
Number of spells per person	
- 1	801
- 2	921
- 3	471
- 4	451
- 5	251
- 6	215
- 7	108
- 8	90
- 9	54
- 10+	112
- mean	3.38
- median	3
Number of months observed	
- mean	53.53
- median	51

Table 2: Spell length (in months) by origin state

	Employment	Unemployment	NEA	Education
Mean spell length	20.1	5.7	11.8	17.7
Deciles				
- 10	1	1	1	1
- 20	3	1	2	2
- 30	4	2	3	4
- 40	8	3	3	8
- 50	11	3	5	13
- 60	14	4	8	16
- 70	23	6	11	24
- 80	34	8	16	28
- 90	57	13	33	43
N	4104	2258	848	4543

Table 3: Flows between states

	Destination:				
Origin:	Employment	Unemployment	NEA	Education	N
Employment	-	1,241	340	659	2,240
Unemployment	1,553	-	172	217	1,942
NEA	320	132	-	193	645
Education	2,231	885	336	-	3,452
N	4,104	2,258	848	1,069	8,279

Table 4: Lagged dependences in transition intensities between work (w), unemployment (u), NEA (n) and education (e)

	w→u	w→n	w→e	u→w	u→n	u→e	n→w	n→u	n→e	e→w	e→u	e→n
No unobserved heterogeneity												
Preceding spell:												
- *Employed*				0.214	-0.015	0.431	0.447	0.504	-0.354	0.004	0.504	0.08
- *Unemployed*	0.533***	-0.015	-0.015				-0.144	-1.019*	-0.821	0.776**	0.561	-1.131
- *NEA*	0.283	0.303	0.102	0.625**	-0.84	-1.183						
Lagged duration dependence (log months):												
- *Employed*				0.069*	-0.108	-0.212	-0.056	-0.272*	-0.152	0.034	-0.3**	-0.531**
- *Unemployed*	0.074	0.095	-0.384**				-0.312	0.363*	-0.285	-0.377**	0.041	0.695**
- *NEA*	-0.199	0.097	-0.196	-0.506***	0.259	0.579				-0.141	-0.036	-0.11
- *Education*	-0.075	-0.035	0.117*	-0.007	-0.163	0.107	-0.045	-0.163	-0.074			
Occurrence dependence (number of prior spells of the same type as the origin state):												
- *1*	-0.075	-0.039	0.052	-0.219***	-0.131	-0.055	0.131	0.451*	-0.141	0.103	0.411	0.624*
- *2or more*	0.278***	0.165	0.075	-0.194**	0.3	-0.742**	0.535*	-0.68	0.89	0.051	0.147	-0.004
With unobserved heterogeneity												
Preceding spell:												
- *Employed*				0.223	-0.122	0.213	0.479	0.447	-0.363	-0.194	0.51	0.045
- *Unemployed*	0.165	-0.253	-0.095				-0.525	-1.167*	-0.759	0.527	0.166	-1.405*
- *NEA*	0.021	0.13	0.009	0.484	-0.786	-1.222						
Lagged duration dependence (ln months):												
- *Employed*				0.042	-0.063	-0.171	-0.156	-0.262	-0.102	-0.043	-0.309**	-0.551**
- *Unemployed*	0.112	0.123	-0.453**				-0.09	0.411*	-0.346	-0.229	0.191	0.808**
- *NEA*	-0.209	0.102	-0.198	-0.422**	0.245	0.56				-0.166	-0.022	-0.133
- *Education*	-0.115**	-0.071	0.065	0.03	-0.173	0.074	0.004	-0.146	-0.089			
Occurrence dependence (number of prior spells of the same type as the origin state):												
- *1*	-0.054	-0.019	0.118	-0.256***	-0.202	-0.235	0.04	0.401	-0.137	0.227	0.413	0.605
- *2 or more*	0.053	0.054	0.173	-0.399***	0.128	-1.195***	0.212	-0.827	1.021*	0.046	0.067	-0.075

* p<0.10, ** p<0.05, *** p<0.01.

Table 5: Testing whether the distinction between unemployment and NEA is empirically important in transition intensities between unemployment and employment

		length of preceding NEA spell (months)					
		6	12	18	24	30	36
length of unemployment spell (months)	12	0.026	-0.400				
		(0.245)	(0.287)				
	24	-0.272	-0.564**	-0.438	-0.693*		
		(0.222)	(0.271)	(0.332)	(0.365)		
	36	-0.272	-0.564**	-0.735**	-0.857**	-0.857**	-0.864**
		(0.222)	(0.271)	(0.317)	(0.355)	(0.355)	(0.420)

		length of preceding unemployment spell (months)					
		6	12	18	24	30	36
length of NEA spell (months)	12	-0.686*	-0.613				
		(0.404)	(0.472)				
	24	-0.686*	-0.749	-0.786	-0.675		
		(0.404)	(0.455)	(0.510)	(0.564)		
	36	-0.686*	-0.749	-0.786	-0.811	-0.811	-0.712
		(0.404)	(0.455)	(0.510)	(0.558)	(0.558)	(0.633)

Notes: * p<0.10, ** p<0.05, *** p<0.01l standard errors in parentheses. The table shows the case of the reference individual entering his first spell of worklessness from education. At specified points (after 12, 24 or 36 months out of work), the following month's transition intensity into employment of those who were unemployed (upper panel) or NEA (lower panel) throughout their workless spell is compared to that of an otherwise similar individual whose workless spell began with a NEA (upper panel) or unemployment (lower panel) spell of a length indicated by the column headings. Exponentiating estimates and subtracting 1 gives proportionate increases/decreases in the transition intensity due to the intervening spell.