The effect of temporary in-work support on employment retention: evidence from a field experiment

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Abstract: A recent experimental programme for unemployed welfare recipients in the UK found that temporary earnings supplements combined with post-employment services led to a sustained rise in employment. This paper examines whether this was due to increases in employment entry or to reductions in employment exit. Using a hazard rate model, we find a significant effect on initial employment entry but not on subsequent transitions. The results also show that the length of a completed unemployment spell has a negative effect on the hazard of exit from the next unemployment spell. While the direct effect of the programme is to shorten the initial unemployment spell, an indirect effect arises due to this lagged duration dependence.

Keywords: Employment retention, earnings supplements, treatment effects, duration model, unobserved heterogeneity.

JEL codes: C31, C41, J64, J68

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1. Introduction

In recent years, many social programmes have attempted to encourage out-of-work welfare recipients to seek and retain employment through the use of time-limited earnings supplements. This paper evaluates one such programme recently trialled in the UK; the UK Employment Retention and Advancement (ERA) programme (Hendra at al., 2011). ERA offered temporary financial support and employment services to individuals moving from welfare into full-time work. It was structured in a way that rewarded *sustained* employment and, as such, represented a departure from labour market policy in the UK which had until then focused on job entry rather than employment retention. Because it was evaluated as a randomised control trial, ERA's effectiveness could be robustly assessed, and it was shown to significantly increase employment among the long-term unemployed.

This paper attempts to identify whether ERA did in fact increase employment retention or whether the overall impacts were due instead to increased employment entry. The distinction is important since employment retention can have a number of longer-term benefits, such as increased employment stability, skill acquisition, earnings growth and career advancement. If ERA can increase employment retention, this would suggest that programmes supporting individuals in the early months of new employment (when the risk of job loss is highest) might have the potential to break the `low-pay no-pay' cycle, thereby improving upward mobility in the labour market.

Although random assignment of ERA eligibility allows unbiased estimates of overall effects, using experimental data to examine programme effects on the rates of entering and leaving employment is more complicated. The difficulty arises because randomisation does not ensure that treatment incidence is independent of unobserved characteristics in employment and non-employment spells that begin post-randomisation. Consequently, treatment-control comparisons among those individuals who have become employed since the programme began cannot be viewed as providing causal estimates of impact.

In this paper, we use hazard rate models to gain an insight into the relative effects of ERA on employment entry and employment retention. We allow unobserved heterogeneity to (separately) influence entry and exit hazards. By allowing these unobserved influences to be correlated, we aim to control for dynamic selection into and out of employment and thereby achieve estimates of the impact of ERA on both processes that can be regarded as causal. We deal with the complication surrounding initial spells by adopting the methodology used in Ham and LaLonde (1996) and Eberwein et al. (1997), specifying a separate process for initial and subsequent non-employment spells. To preview the results, we find that, during the period of ERA eligibility, exit rates from those non-employment spells that were ongoing at the time of randomisation increased but there was no such effect for non-employment spells that began after randomisation, nor was there an effect on employment retention. Post-eligibility, there were no significant effects of ERA on either employment entry or retention. It seems therefore that the higher employment rates seen among the treatment group are due to ERA shortening the initial nonemployment spell. Lagged duration dependence (longer spells out of work reducing the hazard of exit from the next workless spell), reinforces and prolongs this effect.

The remainder of the paper has the following structure. Section 2 summarises the evidence from previous random assignment evaluations of temporary earnings supplements. Section 3 describes the main features of UK ERA and sets it within the context of the welfare system that existed at the time in the UK. It also describes the expected effects on employment entry and retention. Section 4 describes the experiment and shows the overall effect of ERA on employment. The econometric model is presented in section 5 and estimation results are given in section 6. Section 7 concludes.

2. Experimental evidence from previous programmes for welfare recipients

Much of the available experimental evidence originates from evaluations carried out in North America. Previous programmes targeting out-of-work welfare recipients have provided earnings supplements to encourage employment (Martinson and Hamilton, 2011; Gennetian et al., 2005; Huston et al., 2003; Michalopoulos, 2002). In some cases, the supplements were designed to encourage work by providing a cash reward if a job was found. Some programmes also offered incentives to promote employment retention by tying receipt of supplements to the achievement of designated milestones, such as 90 days of continuous employment. Still other programmes offered incentives to encourage full-time employment, with receipt contingent upon working a certain number of hours in a given time period (Hendra et al., 2010).

The intuition behind temporary earnings supplements is that the transition from benefits into work is often difficult and the risk of employment exit is particularly high in the period immediately following employment entry. By providing financial support for a fixed period of time, the intention is to help individuals complete the transition successfully and, with time, become established workers. This should increase long-term employment and earnings. Such interventions are distinct from more traditional policies in the sense that they aim explicitly to support employment retention as opposed to employment entry.

Several studies have shown that provision of temporary earnings supplements can promote employment among low-wage workers. The Minnesota Family Investment Program (MFIP), The New Hope Project and the Canadian Self-Sufficiency Project (SSP), are remarkably consistent in demonstrating positive effects on employment, earnings and income (Michalopoulos, 2005). One year after randomisation, MFIP increased employment by 14 percentage points (relative to an employment rate of 34 per cent among the control group). For SSP, the impact was also 14 percentage points after a year (relative to 31 per cent employment among the control group). For New Hope, the increase was 11 percentage points, although this time relative to an employment rate of 63 per cent among the control group. In all three cases, impacts subsequently faded and ceased to be statistically significant once the earnings supplements ended.

Later programmes combined temporary earnings supplements with a variety of employment-related services aimed at helping those eligible to find and retain jobs. SSP Plus, a programme for single-parent welfare recipients in Canada, found sustained effects that exceeded those from regular SSP that provided earnings supplements alone (Robins et al., 2008). The additional impact relative to regular SSP was sizeable, with an increased employment rate averaging nearly 7 percentage points 36-52 months after randomisation. The Texas ERA programme, combined a temporary earnings supplement with both pre- and post-employment services. In Corpus Christi, the employment rate was increased over the four years post-randomisation by an average of 3.7 percentage points (compared to an average employment rate of 48 per cent among the control group). However, in Fort Worth, the pattern of effects was more typical of a traditional incentive programme in which effects faded shortly after the programme period. The impact on employment peaked in the second year after randomisation (an increase of over 6 percentage points in the proportion employed at some point in year 2, against a control group employment rate of 63 per cent) but was not statistically significant in later years, nor was the overall effect across all four years post-randomisation statistically significant (Hendra et al., 2010).

While the ability of these interventions to increase employment has been demonstrated, precisely how the effects arose is less clear. As already noted, knowing whether they were due to effects on employment entry or to effects on employment retention is important and findings in either direction potentially could provide guidance for policy-makers in allocating funds to run the programmes. A very small number of studies distinguish between these two effects. Card and Hyslop (2005), for example, attribute the overall effect found in the Canadian SSP evaluation primarily to faster exits from welfare, with only one-quarter due to reduced rates of welfare re-entry (i.e. employment retention). Dorsett et al. (2013) provide mixed evidence for Texas. In the Corpus Christi site, short-term effects were estimated to be due to both employment retention and employment entry; the employment entry hazard rate increased by 14 per cent, while the employment exit hazard rate reduced by 18 per cent. Once the operational period had finished, the employment entry effect remained but the retention effect was no longer statistically significant. For the Fort Worth site, the only (marginally) significant effect was on employment retention during the operational period; a reduction in the employment exit hazard of 12 per cent.

3. The welfare system in the UK and the expected effects of ERA

UK ERA (hereafter, ERA) was trialled for three groups: out of work single parents on welfare, low-wage single parents in part-time work, and long-term unemployed welfare recipients entering the New Deal 25 Plus active labour market programme ("New Deal"). Hendra et al. (2011) provide evaluation results for all three groups. The employment impacts are summarised in Table 1. There was no evidence of sustained impacts on employment for either of the two single parent groups but more lasting positive impacts were found for the long-term unemployed group.

<Table 1>

It is important to emphasise, therefore, that the results presented in this paper are specific to the population of long-term unemployed and should not be taken to suggest that ERA increased earnings and employment more generally. We focus on the long-term unemployed because the interest in this paper is to understand whether the long-term impacts of ERA were due to employment entry or employment retention effects (or, indeed, both). Since the single parent groups did not show long-term impacts, they do not satisfy the pre-requisite for exploring this research question.

We note also that, compared to the early North American programmes summarised in the previous section, the employment impacts for the UK long-term unemployed group are smaller but (excepting SSP Plus) longer-lived. They are in fact very similar to the effects of Texas ERA in Corpus Christi – across the four years post-randomisation, ERA increased employment in the UK and Corpus Christi by respective averages of 7.9 and 7.7 per cent – but more sustained than the effects in Fort Worth. In drawing comparisons, it should be borne in mind that the nature and context of the experiments generating these results differ in fundamental ways. For instance, there are differences in the interventions trialled (Texas ERA is the most similar to UK ERA), the populations targeted by the intervention (single mothers in North America, whereas this paper considers long-term unemployed), the labour market conditions at the time of each trial and the welfare regimes of different countries.

3.1 Welfare and labour market programmes for the unemployed

To provide context for the evaluation and also to allow an appreciation of the support offered to the control group in the study, we briefly describe the relevant aspects of the welfare system in the UK and the key features of the New Deal. At the time of the study, the main welfare benefit for the long-term unemployed was Jobseeker's Allowance (JSA), a means-tested payment that, in 2004, stood at £55.65 per week. Individuals remaining out of work could continue receiving support for an essentially indefinite period so long as they continued to actively search for work. Those over the age of 25 who had been claiming JSA for 18 of the last 21 months had to participate in the New Deal as a condition of their ongoing eligibility. New Deal participants received intensified and tailored support to encourage them to find work (Dorsett et al., 2013, provide a detailed description).

3.2 What ERA offered

Against this backdrop, the ERA evaluation tested the extent to which the availability of earnings supplements and caseworker support (including post-employment services) could encourage individuals to work full-time and thereby achieve both self-sufficiency and advancement.

Under ERA, a supplement of £400 became payable if an individual worked full-time for at least 13 weeks within a 17-week period. For those working just enough to qualify, this equates to a rate of slightly more than £1 per hour and compares to an average hourly wage of £6.40 among those in work one year after entering the New Deal (Dorsett et al., 2007).[1] This supplement was not taxable and was in addition to other in-work benefits that might be payable. Eligible workers

[1] This is higher than the minimum wage, which stood at £4.50 in October 2003 and increased to £4.85 in October 2004.

could receive up to six payments in the first 33 months following randomisation. Beyond 33 months eligibility ended, regardless of the number of payments received.

Figure 1 shows the budget constraint facing a minimum wage worker without children in 2004. The black line shows pre-tax earnings. The light grey area shows net income under the tax and transfer system without ERA. There is a step in the budget constraint at 30 hours, the point at which Working Tax Credit (WTC), an in-work payment with some resemblance to the U.S. Earned Income Tax Credit, becomes payable.[2] Whereas the overall gains to an extra hour worked are modest below 30 hours, moving from 29 to 30 hours brings a gain in excess of £17 per week.

The effect of ERA on the budget constraint is shown by the dark grey area in Figure 1. Here, the ERA earnings supplement of £400 has been converted into a weekly equivalent of roughly £24.[3] Clearly, ERA strengthens the WTC incentive to work 30 or more hours a week: moving from 29 to 30 hours now brings an increase in net income of nearly £41. Thus, the expected effect of ERA for a minimum wage worker is to increase full-time employment, at least during the period of eligibility for the earnings supplement.[4]

<Figure 1>

Under ERA, caseworkers in the public employment service encouraged individuals to take into account issues such as the likely longevity of employment, prospects for advancement and so on

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[2] See Blundell and Hoynes (2004) for a comparison of in-work benefits in the UK and the US.
[3] This is calculated as £400/17=£23.53. As discussed, the earnings supplement is payable if, within a 17-week period, at least 13 weeks are spent in full-time work.
[4] Better-paid workers may not be entitled to WTC but will still qualify for the £24 ERA supplement (if working just enough to qualify).

in their job search. For those in work, caseworkers encouraged retention by, for instance, assisting with in-work benefit claims, childcare arrangements or transport problems. They could meet the costs of minor emergencies that threatened individuals' employment. They also encouraged training (assistance with fees and financial incentives were made available) and helped individuals determine career goals, increase working hours and get better jobs. All support under ERA came to an end 33 months after randomisation.

3.3 Expected effects of ERA

ERA reduces the reservation wage, so a simple search model would predict an increase in employment entry. Since earnings supplements were conditioned on full-time work, we would expect to see an increase in the hazard of unemployment exit to full-time work, at least while the supplements remained payable.

A more nuanced model would need to take account of the fact that the supplements only become payable if an individual remains sufficiently long in employment. In response, job seekers may take greater care to find a good match and so take longer in their job search. In this case, despite incentives being higher, exits into employment could be initially reduced.

The effect of earnings supplements on retention is also ambiguous. The earnings supplement may induce jobseekers to accept lower quality jobs but equally, as described above, it may result in individuals findings better matches. In any event, those in work have a strong incentive to remain employed for at least as long as required to receive a supplement. The caseworker support provided under ERA is designed to reinforce this incentive and also to help workers to stay in employment should their job be threatened.

Once the operational period ends, individuals who acquired additional employment experience as a result of ERA may continue to benefit (through improved skills, for example). Equally, by interrupting their period of non-employment, the negative effects of prolonged worklessness may be reduced. They may also have responded to ERA's incentives to invest in their human capital while in work and so be able to demonstrate higher qualification levels than previously. One might expect, therefore, that the longer-term effects will be to increase employment entry among those out of work and reduce employment exit among those in work.[5]

4. The evaluation of ERA

Intake to the experiment began in October 2003 and ran until April 2005. Randomisation was at the individual level and carried out at the point of entering the New Deal.[6] Those randomised to the treatment group became eligible for ERA in addition to the support usually available under the New Deal. Those in the control group on the other hand received the usual New Deal support. Hence, the treatment being tested was the effect of eligibility for ERA.

A consequence of this is that there is no drop out from the experiment. Individuals could choose not to take advantage of the support available under ERA but did not lose their eligibility to do so. Put another way, those in the treatment group may have been affected by ERA even where

[5] The design of the trial does not allow us to distinguish the effect of the earnings supplement from the effect of the other support available under ERA. Dorsett and Robins (2013) examine this question for one of the other ERA groups (single parents on welfare) but this relies on survey data, which is unavailable for the long-term unemployed group considered in this paper.

[6] It is conceivable that some individuals may have altered their behaviour in the knowledge that they might become eligible for ERA. Somewhat mitigating such concerns is the fact that those entering the New Deal were long-term unemployed with low hazard rates of employment entry. Nonetheless, it remains possible that some individuals did reduce their job search in anticipation of the possibility of becoming eligible for ERA. This would not affect the internal validity of the estimated ERA effects but could affect external validity (see Chowdry and Sianesi, 2011, for a detailed examination of external validity).

though they did not receive any additional payments. For example, for some individuals it may have influenced the length of their initial nonemployment spell but still not resulted in a sufficiently long employment spell to qualify for an earnings supplement.

It is informative, though, to know what proportion of those satisfying the criteria to receive an earnings supplement actually did so. During the eligibility period, 32.9 per cent of the treatment group had been employed for four (or more) consecutive months. Meanwhile, Hendra et al. (2011) report that 34.8% received at least one payment of the earnings supplement. In principle, the proportion receiving the bonus should not exceed the proportion eligible. However, allowing for some incomparability arising from the different data sources for used for these estimates,[7] the figures are very close, suggesting that those who had satisfied the conditions for receipt of the payment were likely to receive it.

The evaluation of ERA used data on clients' characteristics collected as part of the random assignment process and employment outcomes taken from administrative tax records for a five-year follow-up period. This information was used to create a series of monthly indicators showing employment status within each month. An advantage of using administrative data rather than survey data is that there is no problem of non-response. Instead, we have full information for all individuals involved in the experiment.

A limitation of the available data is that they do not record hours worked. Survey data collected one year post-randomisation suggest individuals worked either work full-time (30 or more hours)

[7] Information on receipt of the earnings supplement originated from administrative data held within the Department for Work and Pensions.

or not at all; fewer than 10 per cent worked part-time.[8] Consequently, for this population, employment observed in the administrative data will typically capture full-time employment.

Table 2 summarises a number of characteristics of the sample, shown separately for the treatment and control groups. For all characteristics, the two groups resemble each other closely. One point to note is the proportion of individuals entering the New Deal with fewer than 18 of the previous 21 months spent unemployed. While such individuals did not meet the criteria for compulsory New Deal entry, admission rules allowed some individuals facing particular labour market disadvantage to enter the programme early.[9]

<Table 2>

Figure 2 summarises the overall impacts on employment. There was little difference between the treatment and control groups for much of the first year following random assignment but then the groups diverged (left chart). This impact is shown in the right chart. It was quite stable (at about 2 percentage points) until month 30 when it lost statistical significance. It regained statistical significance after year four and this persisted until close to the final observed months.

<Figure 2>

Although small, these impacts are meaningful in size when compared to control group employment levels. In fact, the average effect of ERA after the first year was to increase employment by nearly 10 per cent. Second, the results provide evidence of positive impacts beyond the period during which ERA was operational. As noted above, eligibility ended after 33

[8] This compares to a rate of 15 per cent in the working age population as a whole in 2005.
[9] Across the country as a whole, roughly a quarter of New Deal 25 Plus starters were early entrants over the period October 2003 to April 2005.

months. It is intriguing that the impacts appeared to be declining somewhat after a peak at the end of the first year but that this downward trend reversed in later months, after ERA eligibility had ended. The econometric model estimated later is careful to allow the effect of ERA during the operational period to vary from that in the post-operational period.

As a preliminary to the duration analysis that forms the focus of this paper, column 1 of Table 3 presents experimental estimates of the impact of ERA on the duration of the initial non-employment spell. More formally, writing the length of time post-randomisation as D, the outcome considered in column 1 is $Pr(d \leq D)$, where d is the realised post-randomisation duration of the initial non-employment spell. Each row in the table relates to a specific value of D. The results show no significant impact in the year following random assignment but, when considering longer durations, those in the treatment group are more likely to have found employment than those in the control group. The fact that this impact remains fairly stable (between 2.4 and 3.1 percentage points) regardless of the length of time considered suggests that there was little additional impact on the initial non-employment spell after roughly the first year and a half.

Columns 2-3 decompose the impact reported in column (1) into the impact on two joint probabilities: respectively, $\Pr(d \leq D, r \geq R|T)$ and $\Pr(d \leq D, r < R|T)$,where r (for retention) is the duration of the first employment spell. We select a value of $R = 4$, since this approximates the length of time an individual must remain in continuous employment in order to qualify for an earnings supplement under ERA. The results in column 2 show, at shorter durations post-randomisation, a significant positive impact on the probability of having exited initial non-employment and remained in work for at least 4 months. The fact that this fades at longer durations suggests that ERA may be speeding up initial entry into sustained jobs but that those in the control group 'catch up' after a while. Seen against the results in column 1, it seems that ERA might increase (initial) employment entry more than employment retention. The results in column 3 suggest little impact on the probability of leaving the initial non-employment spell to employment of less than four months.

While intriguing, these experimental estimates cannot directly address the question of how impacts arise. In the next section, we introduce the econometric model used to unravel the impacts on employment entry and retention.

< Table 3 >

5. Econometric approach

5.1 The econometric model

The effects presented in Figure 2 cannot tell us how ERA affected the length of employment spells among those who found work, which is the key measure of employment retention. Treatment-control contrasts within the subgroup of those who find employment will not represent causal impact estimates if ERA influences selection into that subgroup. We follow other studies (for example, Dolton and O'Neill, 2002), and simultaneously model the hazard rates of employment entry and exit. We specify each of these to include an unobserved heterogeneity term and allow these terms to be correlated. In this way, we attempt to control for non-random selection into employment so that treatment-control comparisons of outcomes can be viewed as causal impacts. However, in doing this, we depart from the experimental design and our results depend on how well the underlying assumptions (discussed in sub-section 5.2) are met.

Employment status is observed on a monthly basis. 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.[10]

A complication arises from the fact that initial spells are only observed conditional on lasting sufficiently long to qualify for New Deal entry. This causes an initial conditions problem (Heckman, 1981). To address this, we follow other studies (Ham and LaLonde, 1996; Eberwein et al., 1997; Kalwij, 2004) by adopting the solution suggested by Heckman and Singer (1984a), treating these initial spells separately from 'fresh' spells beginning after randomisation. This results in three possible states: initial non-employment (u_0), fresh employment (e) and fresh non-employment (u) and the econometric model therefore allows three types of transition: $\{u_0 \rightarrow e,\ e \rightarrow u,\ u \rightarrow e\}$.

Formally, the hazard rate from the initial non-employment state can be written:

$$\theta_{u_0}\left(t|T, \mathbf{x}, v_{u_0}\right) = \exp\left(\gamma_{u_0}(t) + \delta_{u_o}^1 T \cdot 1(\tau \leq 33) + \delta_{u_o}^0 T \cdot 1(\tau > 33) + \beta_{u_0}' \mathbf{x}\right) v_{u_0}.$$

Here, t is the duration of the spell, T is a dummy variable identifying the treatment group, unobserved heterogeneity is represented by v_{u_0} and τ represents the number of months elapsed since randomisation.[11] Duration dependence is captured by the baseline hazards, $\gamma_{v_0}(t)$, where these are specified to have a flexible piecewise constant form. The effects of other observed time-varying and fixed influences are captured by the term $\beta_{u_0}' \mathbf{x}$.. We allow the treatment effect during the operational period, $\delta_{u_o}^1$, to differ from that in the post operational period, $\delta_{u_o}^0$.

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[10] In fact, when modelling transitions between three states, it is natural to refer to transition intensities rather than hazard rates. Since the analysis in this paper does not allow for competing risks, the more familiar hazard rate terminology is used.

[11] As noted previously, ERA eligibility lasted 33 months.

The hazard rates for fresh spells are similar apart from the fact that they include the lagged duration dependence terms for employment and non-employment spells completed since randomisation:

$$\theta_j(t|T,\mathbf{x},v_j) = \exp\Big(\gamma_j(t) + \delta_j^1 T \cdot 1(\tau \leq 33) + \delta_j^0 T \cdot 1(\tau > 33) + \beta_j'\mathbf{x} + \phi_j \ln\big(t_e^{lag}+1\big) + \psi_j \ln\big(t_u^{lag}+1\big)\Big) v_j$$

for $j = e, u$. Here, t_e^{lag} denotes the length of the previous employment spell completed post-randomisation (and so equals zero for the first fresh employment spell) and t_u^{lag} denotes the length of the previous non-employment spell completed post-randomisation. Unobserved heterogeneity, v_j, is assumed fixed across spells. Although perhaps restrictive, this is a standard assumption in the empirical literature and helps with model identification (see section 5.2).

Table 4 describes the spell structure of the estimation dataset. Respondents were observed for five years post-randomisation. In total, we have information on more than 6,700 individuals and 16,000 spells. Slightly fewer than half of all individuals (3,214) never left the non-employment spell underway at the time of randomisation. For the remainder of the sample, nearly 1,000 experienced two or more fresh employment spells and two or more fresh non-employment spells. Hence, for a sizeable proportion of the sample, multiple spells of both types are observed.[12]

<Table 4>

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

[12] For employment spells alone, there are a further 454 individuals with at least two spells.

$$L_i^s\left(\upsilon_{ij}\right) = \left(1 - \exp\left(-\theta_j\left(d_i^s|\upsilon_j\right)\right)\right)^{y_{ij}^s} \prod_{r=1}^{d_i - y_{ij}^s} \exp\left(-\theta_j\left(r|\upsilon_j\right)\right)$$

where y_{ij}^s is a dummy variable taking the value 1 if individual *i*'s spell *s* that began in state *j* resulted in an exit (zero otherwise).[13] We write the product of individual *i*'s S_i spells as $L_i(\mathbf{v}_i)$ where $\mathbf{v}_i$ collects the unobserved heterogeneity terms associated with all transition types for individual *i*. To obtain the unconditional likelihood, the unobserved heterogeneity terms must be integrated out. With three possible transition types, the unobserved heterogeneity distribution is also of dimension three. We follow Heckman and Singer (1984b) and discretely approximate the unobserved heterogeneity joint distribution by *M* mass points, υ^m, $m = 1, 2,...,M$ where $\upsilon^m = \{\upsilon_{u_0e}, \upsilon_{eu}, \upsilon_{ue}\}$. The probability attached to υ^m is specified as $p^m = \exp(\lambda^m)/\sum_{g=1}^{M}\exp(\lambda^g)$, where $\lambda^1 = 0$. The number of mass points, *M*, is 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:

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

5.2 Identification

The fundamental identification problem with models of this type arises from the fact that a hazard rate that is observed to fall with spell length could be attributable either to *genuine* negative duration dependence (whereby spell length causally reduces hazard rates) or

[13] Conditioning on treatment, observed characteristics and lagged duration dependence is left implicit.

weeding/sorting effects (whereby individuals with the highest hazards exit in the early stages of their spell, leaving behind other individuals, who dominate observed hazards at longer spell durations). Elbers and Ridder (1982) prove identification of both the unobserved heterogeneity (or, 'mixing') distribution and the baseline hazard in single-spell models under assumptions about the mixing distribution, so long as there is variation in regressors. Honoré (1993) shows that identification is achieved without the need for regressor variation or an assumption about the mixing distribution when multiple spells are observed. However, identification of lagged duration dependence relies again on regressor variation that is independent of unobserved heterogeneity. This requirement for independence can be relaxed to some extent by introducing the additional assumption that $\upsilon_j = \nu_j \exp(\mathbf{x}\mu_j)$, for $j = u_0, e, u$. The drawback of this is that the coefficients of $\mathbf{x}$ no longer have a structural interpretation. However, the other coefficients are still identified (Cockx et al., 2013).

Were we interested in lagged duration dependence purely across spells of the same type (sharing a common baseline hazard and unobserved heterogeneity), Fritjers (2002) shows that identification is achieved without the need for regressors. This does not help in the model considered in this paper, where we allow for lagged duration dependence across spells of the same type as well as across spells of different types. However, Picchio (2012) shows that, where a sufficient number of spells are observed, identification is achieved without the need for regressors or fixed baseline hazards. Since within our sample, 21 per cent of individuals are observed to have four or more spells (Table 4), we have grounds to view the conditions for the Picchio (2012) result to hold as being met.

The proportionality assumption remains important, despite having no justification from economic theory. 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 number of unemployed per vacancy (measured at the national level) and local unemployment rate relative to the national rate. 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.[14] This provides another source of identification and thereby reduces reliance on the assumption of proportionality. There may be concern that the unemployment-related series are not exogenous since they partly reflect transitions among the experimental sample. In fact, the long-term unemployed that form the focus of this study contribute rather little to the movements in these overall unemployment rate series. This is because, numerically, they form only a minority of all unemployed (over the full evaluation period, those unemployed for 18 months or longer accounted for seven percent of all Jobseeker's Allowance claims) but also because they have much lower transition rates than individuals who have more recently become unemployed.

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.

6. Results

[14] Cockx and Picchio (2013) include regional GPD, district unemployment rates and calendar quarter dummies and argue in a similar way that this provides another source of identification.

Table 5 presents the estimation results. The model controls for age, sex, education, partnership status, whether the individual lives with dependent children, the ratio of unemployed per vacancy (nationally), the deviation of local claimant unemployment from the national rate, year and calendar quarter. Duration dependence is captured through piecewise constant baseline hazards. This allows exit rates to vary flexibly over the duration of the spell.[15] The joint unobserved heterogeneity distribution is approximated using $M = 3$ points of support.[16]

The results of most interest are those showing the effect of ERA. The model labelled 'Unrestricted' shows that, during the operational period, ERA increased the hazard of exit from the initial non-employment spell (column 1) by nearly 10 per cent.[17] Consistent with an increase in employment retention, the sign of the coefficient in column 2 is negative, indicating a reduction in the hazard of employment exit. However, this is not statistically significant.[18] For non-employment spells after the initial one (column 3), the estimated impact on the hazard of exit is close to zero. This is perhaps surprising given the significant impact on initial spells. One potential explanation is that it reflects impact heterogeneity, with the effect on the subgroup of individuals whose initial employment entry is unsustained being smaller than that for the rest of

[15] The piecewise segmentation was carried out to ensure sufficient observed exits within each segment. As a result of this, the definition of the baseline hazards differs across the three transitions types.

[16] We were able to estimate the model with up to three points of support. Following (Gaure et al., 2007), it is common to use the Akaike Information Criterion (AIC) to help inform the choice of *M*. Of the three specifications, $M = 3$ minimised the AIC and was therefore adopted as the preferred specification.

[17] Calculated as exp(0.092)-1=0.0957.

[18] As an alternative specification, we included in the employment hazard equation a dummy variable taking the value 1 when the spell duration was in excess of three months. The purpose of doing this was to examine whether the minimum employment duration criterion for receipt of an earnings supplement might influence employment exits. The results (not presented) showed this variable to not be statistically significant. Furthermore, the estimated coefficients for the other regressors were almost identical to the specification excluding that variable.

the eligible population. For instance, if the main effect of ERA was to increase job entry among those capable of sustained employment, such individuals would tend not to feature among the subgroup experiencing fresh spells of non-employment, which would be largely made up of individuals with behaviour relatively unaffected by ERA. Lastly, we see that once eligibility to ERA ended, there were no statistically significant effects on any of the hazard rates.

The finding that employment entry effects dominate employment retention effects is consistent with the case of SSP in Canada (Card and Hyslop, 2005) and, in the longer-term, Texas ERA in Corpus Christi (Dorsett et al., 2013). The finding of no retention effect, though, differs from the Canadian and Texan cases. As noted already, there are numerous differences between these experiments that could account for the variation in estimated effects. For instance, movements into and out of work among the control group were more common in Texas than the UK.[19] This suggests that an inability to keep a job may be more of an issue in Texas than in the UK (where job entry was more relevant). If so, there could be more scope for interventions to improve retention in Texas than in the UK.

Differences in the design of the interventions may also have influenced their effectiveness. Insights from behavioural economics (for a recent survey, see Babcock et al., 2012) suggest that individuals tend to respond more to smaller, high-frequency incentives than they do to larger, less frequent incentives. They also respond more to simpler rules than they do more complicated ones. In Texas, earnings supplements were received monthly, where an individual had worked full-time throughout that month. In the UK, the supplements were payable if, over a 17-week

[19] In each of the four years following randomisation, an average of nearly half of those in the Texas control groups worked at some point, compared to just one-third in the UK control group.

period, an individual had worked full-time for at least 13 weeks. Hence, with regard to both frequency and simplicity, the design of ERA in Texas was preferable to that of ERA in the UK.

The other key finding relates to the effect of earlier spells. There are no significant effects on employment exits (column 2) but the results show that the length of the previous nonemployment spell has a significant negative effect on employment entry (column 3). This finding is consistent with the literature on the scarring effects of unemployment, and the idea that prolonged worklessness can reduce the probability of employment entry for reasons such as employer discrimination, reduced morale, skills deterioration and so on. The results suggest that, by reducing the length of the initial nonemployment spell, ERA has an ongoing indirect effect on employment entry.

<Table 5>

To make this more concrete, simulation methods can be used to show the effect of ERA as predicted by the model. Re-simulating with the effects on employment retention suppressed allows the employment entry effect to be seen. Similarly, suppressing instead the employment entry effects allows the retention effect to be seen. Comparing the three simulations provides an insight into the relative importance of employment entry and employment retention effects to the overall employment impact.

The simulation approach is as follows. We use our full sample of 6,742 individuals. We take 1,000 draws from a multivariate normal distribution with means corresponding to the estimated coefficients reported in Table 5 and variance given by the associated variance-covariance matrix. We use the model labelled "Restricted" – this is similar to the model discussed above except that, in order to avoid excessive noise in the simulations, the treatment effects and lagged duration dependence terms that are close to zero (and imprecisely estimated) have been suppressed and the two treatment effects for employment exits have been combined into a single treatment effect.[20] For each individual in our sample, 1,000 post-randomisation labour market trajectories are simulated, one for each draw from the estimated coefficient distribution. These labour market histories cover a period of 60 months post-randomisation. In each month, hazard rates are calculated and exits from the current state are determined on the basis of a lottery, with the probability of exit equal to the appropriate hazard rate.

The results are shown in Figure 3. The graph at the top shows the simulated overall impact. This reaches a stable level of between 1.5 and 2 percentage points. For the purpose of comparison and to provide some impression of model fit, the top graph in Figure 3 also shows the unadjusted experimental impacts on employment (identical to those depicted in Figure 2). We see that the simulated impacts show a smoother time-path than the experimental impacts, reflecting the greater structure assumed for the model. However, the experimental impacts lie almost entirely within the 95 per cent confidence intervals of the simulated results, indicating that the differences are not statistically significant. Furthermore, it is reasonable to point out that the experimental impacts are themselves estimates.[21] There are two consequences of this. First, when we take into account the variation around the experimental point estimates, the statistical significance of any differences from the simulated impacts further reduces. Second, given this variation, it is reasonable to judge the performance of the model by how well it captures broad

[20] Statistical tests comfortably accepted these restrictions. Furthermore, the estimates for the remaining coefficients were hardly affected when imposing these restrictions.
[21] To avoid cluttering the graph, the confidence intervals around the experimental impacts are not shown in Figure 3 but are shown in Figure 2.

trends rather than the extent to which it follows month-on-month fluctuations. On this basis, it seems justified to view the simulated impacts as providing a reasonably good fit.

The two graphs at the bottom of Figure 3 show the simulated impacts when allowing only employment entry effects (bottom left) or only employment retention effects (bottom right). From this, it is clear that the overall impacts in the top graph are driven primarily by employment entry effects during the operational period. Significant impacts persist beyond this point. These are due in part to the entry effects themselves but are bolstered by the indirect effect operating through the lagged duration dependence term in the hazard of exiting a fresh nonemployment spell. Over time, the contribution of these entry effects slowly declines. This reflects the fact that ERA increased exit hazards for initial non-employment spells but not for fresh non-employment spells. From the bottom right graph, we see that the contribution of retention effects is positive throughout but fails to attain statistical significance. This is in line with the results reported in Table 5.

<Figure 3>

7. Conclusion

In recent years, a small number of experimental programmes have tested the use of temporary earnings supplements together with employment services to encourage self-sufficiency among welfare recipients. The evaluation of ERA in the UK represents an important contribution to this small evidence base, confirming the potential to achieve sustained impacts for the long-term unemployed (while showing little effect for lone parents on welfare).

This paper has attempted to look beyond the overall impacts to understand the extent to which they are due to increased employment entry and the extent to which they are due to increased employment retention. Data from the random assignment evaluation of ERA have been used to estimate an econometric model of employment and non-employment durations. Significant positive effects of ERA on exit from the initial non-employment spell were found during the period ERA was operational. This direct effect is compounded through lagged duration dependence. However, the effect on retention, while positive, was not statistically significant.

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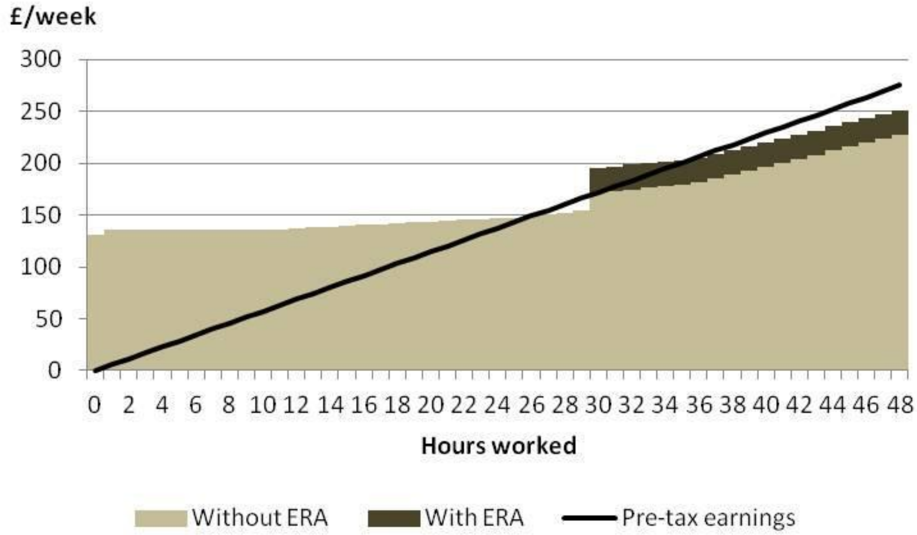
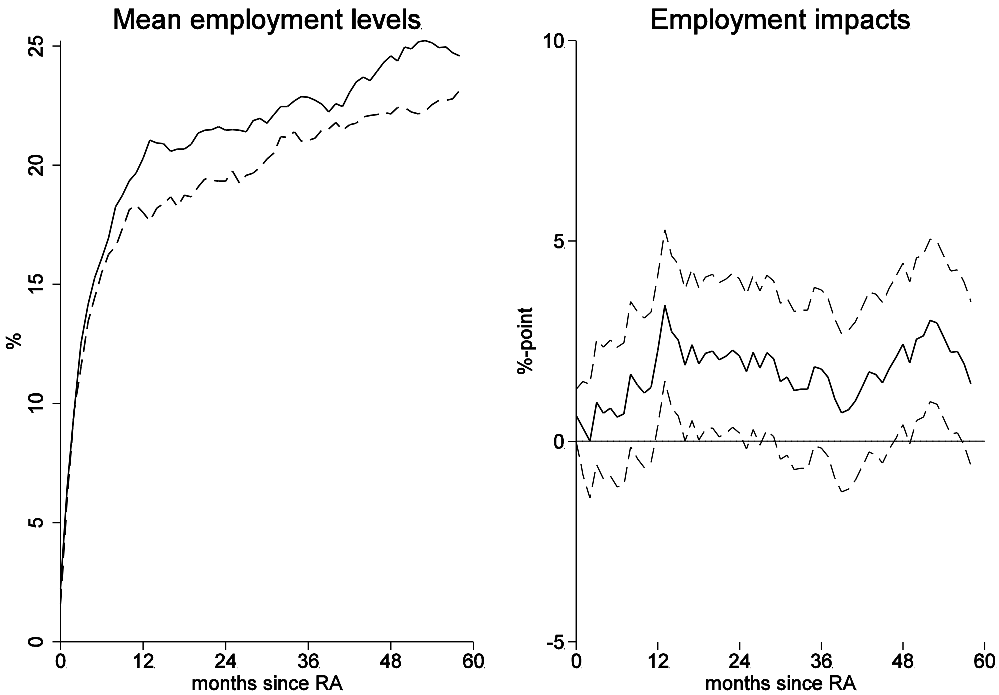
Figure 1: Budget constraint with and without ERA, 2004



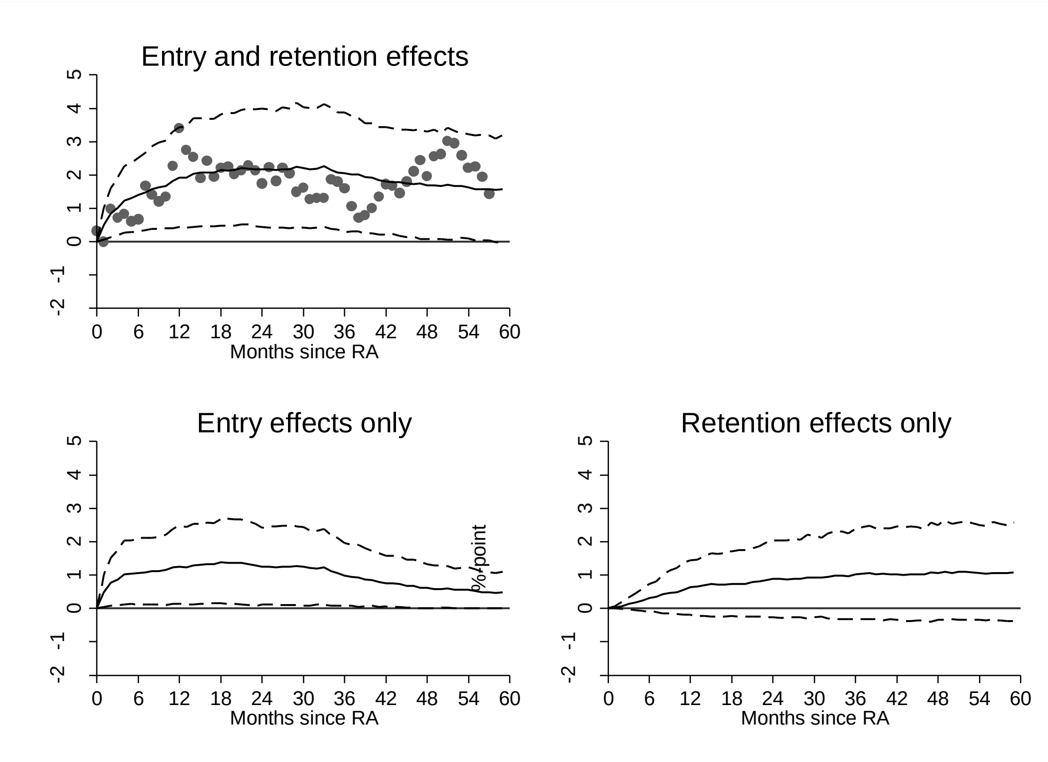
Notes: The chart is shown for a single adult without children who, when working, is paid at the national minimum wage and who lives in rented accommodation costing £60 per week.

Figure 2: Employment levels and the overall impact of ERA on employment



Notes: The chart on the left shows mean levels of employment for the control group (dashed line) and treatment group (solid line) by month since random assignment. The chart on the right shows the overall impact of ERA on employment, together with 95 per cent confidence intervals.

Figure 3: Simulating the effect on employment: the overall ERA effect, the employment entry effect and the employment retention effect



Note: Simulated impacts (solid line) are shown with 95 per cent confidence intervals (dashed lines). Top graph also plots experimental employment impacts from Figure 2.

Table 1: Summary of the ERA evaluation results: impacts on probability of working, by year since randomisation

Single parents not working at randomisation

Probability of working in:	Treatment	Control	Impact		P-value
Year 1	57.1	56.5	0.6		0.618
Year 2	57.8	55.6	2.2	*	0.066
Year 3	53.7	53.8	-0.2		0.895
Year 4	53.2	54	-0.8		0.507
Year 5	52.9	53.9	-1		0.42

Single parents working part-time at randomisation

Probability of working in:	Treatment	Control	Impact		P-value
Year 1	77	76.6	0.4		0.78
Year 2	74.1	73.4	0.8		0.649
Year 3	71.2	69.9	1.3		0.454
Year 4	71.3	70.1	1.2		0.47
Year 5	68.6	68.2	0.3		0.849

Long-term unemployed at randomisation

Probability of working in:	Treatment	Control	Impact		P-value
Year 1	37.3	35.4	1.9	*	0.098
Year 2	36.3	32.7	3.6	***	0.001
Year 3	34.6	32.5	2.1	*	0.057
Year 4	35	32.1	2.9	***	0.009
Year 5	32.8	30.9	1.9	*	0.094

Note: Significance levels: * = 10 per cent; ** = 5 per cent; *** = 1 per cent. The table summarises results in tables 4.1, 4.2 and 6.2 of Hendra et al. (2011).

Table 2: Selected sample characteristics at randomisation

	Treatment group	Control group
Female	0.19	0.19
Age	40	40
(std. dev.)	(9.22)	(9.28)
Has partner	0.23	0.19
Has children	0.15	0.15
Highest qualification:		
- none	0.37	0.37
- secondary school qualification (or equivalent)	0.35	0.34
- post-secondary school qualification (or equivalent)	0.15	0.16
- other	0.13	0.13
Unemployment in the previous 21 months:		
- less than 18	0.17	0.18
- 18 or more	0.83	0.82
N	3,401	3,341

Table 3: Experimental estimates of the effect of ERA on the duration of the initial non-employment spell and the joint distribution of initial non-employment and employment spells.

m=month since random assignment	(1) Within m months had entered initial employment spell.	(2) Within m months had entered initial employment spell, which lasted 4 months or longer	(3) Within m months had entered initial employment spell, which lasted less than 4 months
6	0.001	0.009	-0.008
	(0.009)	(0.007)	(0.007)
12	0.008	0.016**	-0.008
	(0.011)	(0.008)	(0.009)
18	0.024**	0.020**	0.004
	(0.011)	(0.008)	(0.010)
24	0.031***	0.016*	0.014
	(0.012)	(0.008)	(0.010)
30	0.028**	0.013	0.015
	(0.012)	(0.008)	(0.011)
36	0.029**	0.008	0.021*
	(0.012)	(0.008)	(0.011)
42	0.030**	0.013*	0.017
	(0.012)	(0.008)	(0.011)
48	0.030**	0.013	0.017
	(0.012)	(0.008)	(0.011)
54	0.025**	0.008	0.017
	(0.012)	(0.008)	(0.012)

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$ (standard errors in parentheses). Estimates control for the following characteristics (all measured at time of randomisation): age, sex, education, partnership status, whether the individual lived with dependent children and region.

Table 4: Summary spell statistics

Number of individuals	6,742
Number of spells	16,348
Spells per person (mean)	2.42
Mix of spells:	
- initial non-employment only	3,214
- 1 fresh employment	914
- 1 fresh employment, 1 fresh non-employment	1,180
- 2 fresh employment, 1 fresh non-employment	454
- 2 fresh employment, 2 fresh non-employment	481
- 3+ fresh employment or non-employment	499

Table 5: Estimated effects of ERA on hazard of exit, by type of spell

	Unrestricted			Restricted		
	(1)	(2)	(3)	(4)	(5)	(6)
	Initial non-emp	emp	non-emp	Initial non-emp	emp	non-emp
ERA effect, operational period	0.091**	-0.058	0.007	0.092**		
	(0.043)	(0.048)	(0.065)	(0.042)	-0.060	
ERA effect, post-operational period	0.005	-0.059	0.008		(0.041)	
	(0.075)	(0.057)	(0.059)			
Lagged (log) duration dependence:						
- prior employment spell (months)		0.010	-0.009			
		(0.039)	(0.042)			
- prior non-employment spell (months)		-0.010	-0.157***			-0.157***
		(0.026)	(0.026)			(0.025)
Baseline hazard:						
- month 1		1.081***	0.675***		1.082***	0.682***
		(0.086)	(0.111)		(0.075)	(0.108)
- month 2		1.009***	0.746***		1.009***	0.752***
		(0.083)	(0.109)		(0.075)	(0.106)
- month 3		0.810***	0.658***		0.809***	0.662***
		(0.084)	(0.111)		(0.078)	(0.108)
- months 1-3	1.079***			1.079***		
	(0.222)			(0.221)		
- months 4-6	1.536***	0.658***	0.539***	1.535***	0.656***	0.542***
	(0.139)	(0.070)	(0.091)	(0.139)	(0.066)	(0.088)
- months 7-9	1.013***			1.014***		
	(0.155)			(0.154)		
- months 10-12	0.975***			0.976***		
	(0.146)			(0.146)		
- months 7-12		0.398***	0.454***		0.395***	0.455***
		(0.063)	(0.078)		(0.061)	(0.076)
- months 13-18	0.828***			0.827***		
	(0.112)			(0.111)		

- months 19-24	0.600***			0.600***		
	(0.075)			(0.074)		
Female	0.073	-0.292***	0.042	0.073	-0.290***	0.038
	(0.049)	(0.054)	(0.064)	(0.048)	(0.053)	(0.062)
Partnered	0.169***	-0.149**	-0.025	0.170***	-0.150**	-0.028
	(0.058)	(0.063)	(0.075)	(0.058)	(0.062)	(0.074)
Dependent children	0.120*	-0.216***	0.145*	0.120*	-0.215***	0.143*
	(0.066)	(0.071)	(0.085)	(0.066)	(0.071)	(0.084)
Highest educational qualification:						
- NVQ 1-3	0.305***	-0.077	0.210***	0.305***	-0.074	0.206***
	(0.048)	(0.051)	(0.060)	(0.048)	(0.051)	(0.059)
- NVQ 4-5	0.399***	-0.241***	0.343***	0.399***	-0.237***	0.337***
	(0.058)	(0.063)	(0.073)	(0.058)	(0.062)	(0.072)
- some other qualification	0.331***	-0.119*	0.293***	0.330***	-0.119*	0.289***
	(0.063)	(0.067)	(0.077)	(0.063)	(0.066)	(0.076)
Age:						
- 30-39	-0.296***	-0.105*	-0.031	-0.296***	-0.104*	-0.031
	(0.053)	(0.056)	(0.062)	(0.053)	(0.056)	(0.062)
- 40-49	-0.442***	-0.204***	-0.005	-0.442***	-0.203***	-0.006
	(0.058)	(0.063)	(0.070)	(0.058)	(0.062)	(0.069)
- 50-59	-0.675***	-0.405***	-0.245***	-0.674***	-0.404***	-0.247***
	(0.069)	(0.077)	(0.092)	(0.069)	(0.077)	(0.091)
Calendar quarter:						
- January-March	0.160***	-0.091*	-0.017	0.160***	-0.093*	-0.016
	(0.060)	(0.054)	(0.076)	(0.060)	(0.053)	(0.075)
- April-June	0.352***	-0.076	0.223***	0.352***	-0.077	0.224***
	(0.052)	(0.048)	(0.067)	(0.052)	(0.047)	(0.066)
- July-September	0.236***	-0.088**	0.166***	0.236***	-0.089**	0.167***
	(0.050)	(0.045)	(0.064)	(0.050)	(0.044)	(0.064)
Year						
- 2005	-0.547***	-0.145**	-0.036	-0.549***	-0.140**	-0.038
	(0.063)	(0.065)	(0.132)	(0.063)	(0.062)	(0.131)

- 2006	-0.895***	-0.139	-0.007	-0.898***	-0.132*	-0.012
	(0.097)	(0.088)	(0.147)	(0.096)	(0.078)	(0.142)
- 2007	-1.171***	-0.280***	-0.050	-1.171***	-0.272***	-0.055
	(0.099)	(0.095)	(0.150)	(0.094)	(0.075)	(0.139)
- 2008	-0.874***	-0.195*	0.045	-0.873***	-0.186**	0.038
	(0.116)	(0.114)	(0.166)	(0.110)	(0.089)	(0.151)
- 2009	-0.861***	-0.269	0.359	-0.862***	-0.258	0.350
	(0.323)	(0.235)	(0.314)	(0.319)	(0.217)	(0.300)
Number of unemployed per vacancy	-0.117	0.000	-0.295***	-0.116	-0.001	-0.294***
	(0.094)	(0.061)	(0.079)	(0.093)	(0.061)	(0.079)
Unemployment rate, local deviation	-0.281***	-0.059	0.107*	-0.281***	-0.060	0.107*
	(0.044)	(0.049)	(0.062)	(0.044)	(0.049)	(0.061)
Constant	-3.659***	-2.079***	-2.813***	-3.666***	-2.119***	-2.793***
	(0.253)	(0.220)	(0.253)	(0.251)	(0.203)	(0.248)
Unobserved heterogeneity:						
- $\ln(\upsilon^2)$	2.370***	-0.662***	-0.976**	2.357***	-0.625***	-1.031**
	(0.236)	(0.248)	(0.455)	(0.227)	(0.218)	(0.415)
- $\ln(\upsilon^3)$	0.061	-1.447***	0.689***	0.076	-1.473***	0.634***
	(0.256)	(0.135)	(0.187)	(0.251)	(0.121)	(0.173)
Probability masses (logistic transforms):						
- λ^2	-2.034***			-2.045***		
	(0.253)			(0.242)		
- λ^3	-0.334			-0.371		
	(0.448)			(0.382)		
Resulting probabilities:						
- Prob (group1)	0.541			0.550		
- Prob (group2)	0.071			0.071		
- Prob (group3)	0.388			0.379		
Log likelihood	-41,745.362			-41,745.583		
Number of individuals	6,742			6,742		

Notes: * p<0.10, ** p<0.05, *** p<0.01 (standard errors in parentheses).