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

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17th September 2013

Abstract

A recent experimental programme for unemployed welfare recipients in the UK found that temporary earnings supplements combined with post-employment services led to increased employment rates. This paper examines whether these overall impacts are due to employment entry or employment retention effects. Findings from a multivariate mixed proportional hazards model suggest that entry effects dominated initially but that longer-term impacts were primarily due to increased retention. This retention effect persisted beyond the operational period of the programme and was evident even after controlling for the effect of employment experience.

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

JEL codes: C31, C41, J64, J68

*This work was supported by the Economic and Social Research Council (grant number ES/J003581/1). The usual disclaimer applies.

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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 conditioned on work. Typically, such programmes pay workers a cash amount for each month of employment. Some programmes condition the payments on full-time employment. Others combine them with enhanced pre- and post-employment services intended to help recipients obtain and retain jobs.

This paper considers the case of a recently-completed experimental programme for out-of-work welfare recipients in the UK; the UK Employment Retention and Advancement programme (ERA). ERA was designed to encourage longer-term self-sufficiency by providing temporary support to individuals who entered full-time employment with a view to helping them remain in work. It represented a departure from labour market policy in the UK which focused almost exclusively on job entry rather than employment retention. This paper attempts to identify whether ERA increased employment retention or whether the observed overall impacts were due instead to increased employment entry. The distinction is important since employment retention has associated with it a number of longer-term benefits, such as increased employment stability, skill acquisition, earnings growth, career advancement, etc. 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 oft-cited 'low-pay no-pay' cycle, thereby improving upward mobility in the labour market.

Many programmes tested in recent years have utilised random assignment on samples of welfare recipients in various locations throughout North America and Europe. The evaluations of these programmes have exploited the experimental data to obtain unbiased estimates of programme effects on a variety of employment outcomes, using the full sample of treatment and control group members. Well-designed random assignment studies produce unbiased estimates of the overall effect on employment. Using experimental data to examine programme effects on the rates of entering and leaving employment, however, can be problematic. This is because obtaining such effects requires separate analysis of spells of employment and non-employment and the experimental design does not guarantee that treatment-control comparisons within these subgroups provide unbiased estimates. The problem arises because randomisation does not ensure that the treatment incidence is independent of unobserved characteristics in subsequent employment and non-employment spells. For example, when the programme is successful, it helps those not working at baseline to become employed and these individuals may have characteristics that are different from those in the control group who become employed for reasons other than the experimental programme. 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 attempt to overcome these difficulties by adopting the methodology used in Ham and LaLonde (1996) and Eberwein et al. (1997) to estimate the effects of ERA on transitions into and out of employment. To preview the results, we find that, during the period of ERA eligibility, rates of exit 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. That is, while initial job entry rates were increased,

where those jobs did not last, the likelihood of re-entry was unaffected. Similarly, there was no effect on employment retention during the eligibility period. Once eligibility for ERA had expired, there was no longer an effect on employment entry rates but there was a significant improvement in employment retention. The analysis goes further and attempts to capture the extent to which employment experience acquired post-randomisation influenced subsequent outcomes. The findings show the importance of employment experience to transitions into and out of employment. However, the key ERA impacts described above remain. It therefore appears that ERA increased employment retention and did this both through its indirect effect on employment experience and through a direct effect. The precise nature of this direct effect cannot be inferred from the analysis in this paper but one possibility is that the training incentives provided by ERA had the effect of improving skills and employability.

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 discusses the results in the context of other relevant evidence and the changed welfare system in the UK.

2 Findings from previous programmes for welfare recipients

Much of the available experimental evidence on the effect of temporary in-work support on employment originates from evaluations carried out in North America. Previous experimental 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 (overall or in a specific job). 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.[1]

Several studies have shown that provision of temporary earnings supplements can promote employment among low-wage workers. Rigorous evaluations, using random assignment

[1]Such interventions are distinct from more traditional policies in the sense that they aim explicitly to support employment retention as opposed to employment entry.

experiments, of several programmes, including 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 economic outcomes (Michalopoulos, 2005). This research shows that individuals offered these supplements were more likely to work, earned more, and had more income than those in a control group. While all of these programmes produced positive effects on employment while the supplements remained payable (the operational period), these effects subsequently faded soon before or after the earnings supplements ended.

More recently, programmes targeting out-of-work welfare recipients have combined temporary earnings supplements with a variety of employment-related services aimed at helping the recipients find and retain jobs. The services provided to these persons ranged from simple job-search assistance to more extensive services prior to and after jobs were found. SSP Plus, an experimental programme providing earnings supplements and limited employment services to single-parent families on welfare in Canada found sustained effects that exceeded those from a programme that provided earnings supplements alone (Robins et al., 2008). The Texas ERA programme, which combined a temporary earnings supplement with both pre- and post-employment services, produced long-term increases in earnings in one site (Corpus Christi), but in another site (Fort Worth), the pattern of effects was more typical of a traditional incentive programme in which effects faded shortly after the programme period (Hendra et al., 2010).

A model of ERA very similar to that in Texas was later trialled in the UK. Full details are provided in the next section but for now we note that, for long-term unemployed people, the combination of temporary earnings supplements and employment-related services provided by UK ERA resulted in positive employment impacts that lasted beyond the period of programme operation (Hendra et al., 2011).

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 but, over time, the retention effects faded while the employment entry effects persisted. For the Fort Worth site, there were smaller effects overall and less evidence of impacts that lasted much beyond the programme operation period.

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

The impact of UK ERA (hereafter, "ERA") was evaluated through a random assignment demonstration. It was trialled for three groups: 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. In this paper, we consider the last group.[2]

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 (approximately $100 per week at the exchange rate that prevailed at the time). 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. The purpose of the New Deal was to boost job search effort and to provide a range of practical assistance to encourage employment entry. It involved three stages. The first ('Gateway') stage typically lasted up to four months and mostly involved caseworkers providing increased support and encouragement with job search. The second stage was called the 'Intensive Activity Period' and usually lasted 13 weeks, but could be extended to 26 weeks or even longer (up to a year). It was this stage of the New Deal that offered the most substantial support; individuals participated in full-time activities such as education, training or work experience. Those still not in work after this stage entered the 'Follow-Through'. This was the third and final stage of the New Deal and involved a period of further job search assistance for between 6 and 13 weeks, intended to build on the experience gained in the second stage.

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. Earnings supplements were conditioned on working 30 or more hours per week for a sustained period. Specifically, a supplement of £400 ($700 in 2004) became payable if an individual could demonstrate that they had worked full-time for at least 13 weeks within a 17-week period. To give some idea of the generosity of this supplement, it equates to a rate of slightly more than £1 per hour for those working just enough (30 hours per week for 13 weeks) to qualify for it. This compares to an average hourly wage of £6.40 for those individuals who were in work one year after entering the New Deal (Dorsett et al., 2007).[3] This supplement was not taxable and was in addition to other in-work benefits that might be payable. Eligible workers could receive up to six payments of the earnings supplement in the first 33 months following randomisation, after which point eligibility to both the supplement and the post-employment services ended. In addition, ERA increased incentives to train. Those working at least 16 hours per week could qualify for tuition payments of up to £1,000 ($1,750) for approved courses and received an additional payment for £8 ($14) for every hour of training completed, up to a maximum of £1,000.

Figure 1 shows the budget constraint facing a minimum wage worker without children in 2004 and how the introduction of ERA affected work incentives. The black line shows pre-tax

[2]Hendra et al. (2011) provide evaluation results for all three groups. There was no evidence of sustained impacts on employment or earnings for either of the two single parent groups.

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

earnings, which is simply the number of hours worked multiplied by the national minimum wage. This amount excludes welfare benefits including Working Tax Credit (WTC), an in-work payment analogous to the U.S. Earned Income Tax Credit.[4] The light grey area shows net income under the tax and transfer system without ERA. Up to 26 hours a week, this individual's net income exceeds gross earnings but, beyond that point, the individual is mostly a net contributor. The exception is when working 30 hours, at which point WTC becomes payable. This creates a step in the budget constraint. 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. WTC entitlement tapers away as hours increase further until it disappears entirely at 36 hours per week. Beyond this point, each additional hour increases net income by close to £4.

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.[5] 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. As before, beyond 36 hours, net weekly income increases by close to £4 for each additional hour worked. 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.

The situation for better-paid workers is somewhat different. Receiving a wage as little as 20 per cent above the minimum wage would rule the worker out from any WTC entitlement. In the absence of ERA, the budget constraint for such a worker is smoother than for a minimum wage worker since there is no step at 30 hours. As before, ERA rewards the move from 29 to 30 hours with a payment worth £24 per week. However, whereas the ERA supplement *strengthened* the pre-existing incentive for a minimum wage worker to work at least 30 hours a week, for better-paid workers ERA *introduced* an incentive to work at that level.

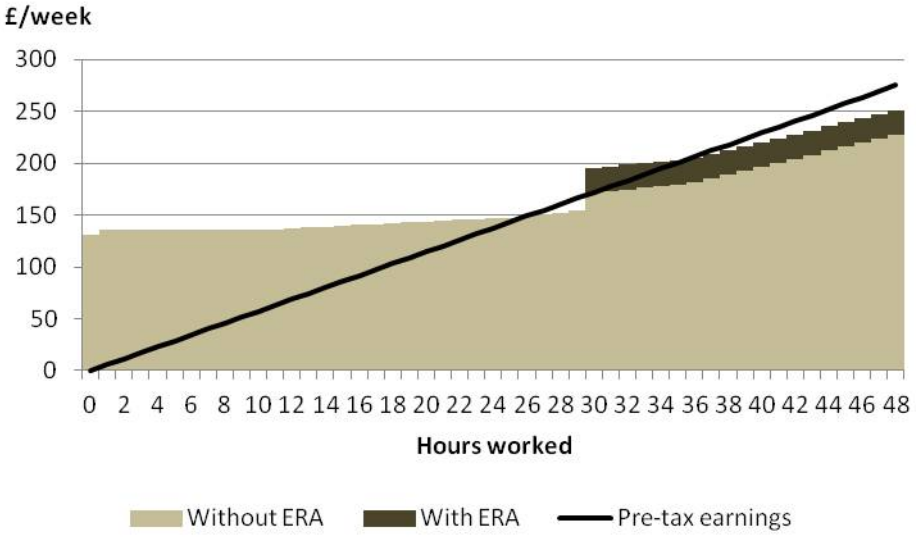
Under ERA, caseworkers in the public employment service received training intended to promote *sustained* employment. This was a departure from existing practice that had focused instead on encouraging individuals to enter work. The out-of-work support provided by ERA caseworkers was designed to encourage individuals to take into account issues such as the likely longevity of employment, prospects for advancement and so on in their job search. The in-work support could take various forms. Retention-focused support was designed to help individuals remain in work by, for instance, assisting with in-work benefit claims, childcare arrangements, transport problems etc. Advancement-focused support tended to focus on identifying appropriate education or training opportunities, determining career goals, increasing working hours and getting a better job. ERA also introduced an Emergency Discretion Fund (EDF) – a small fund (£300 per individual) that caseworkers could use to meet the costs of minor emergencies that threatened individuals' continued employment.

It is helpful to consider what the expected effects of this package of temporary in-work

[4]There are of course important differences between WTC and EITC, both with regard to generosity and other payment arrangements (for instance, WTC is payable weekly or monthly while EITC is payable annually). See Blundell and Hoynes (2004) for a comparison of in-work benefits in the UK and the US.

[5]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. Figure 1 relates to this same 17-week period, and converts the ERA earnings supplement to an average weekly amount.

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.

support might be. The existence of earnings supplements allows job seekers to accept lower-paying jobs than they would otherwise. A simple search model predicts that such a reduction in the reservation wage would increase the probability of accepting a job offer and thereby speed individuals' entry into work. Under ERA, earnings supplements were only payable for those in full-time work. Consequently, we would expect to see an increase in the hazard of unemployment exit to full-time work. Although the available data do not record hours of work, survey data collected one year post-randomisation suggest the individuals considered tend to either work full-time or not at all,[6] so it is reasonable to expect that this increased movement into full-time work will show up in the data as a movement into employment. The earnings supplements should also increase employment retention as individuals become less likely than they would otherwise be to choose to leave full-time employment. The caseworker support provided under ERA is designed to reinforce these incentives and also to help workers to stay in employment should their job be threatened.

Once the operational period (that is, the period during which ERA provides support) ends, those individuals who acquired additional employment experience as a result of ERA but who nevertheless find themselves out of work may be better placed to find a new job; their period of employment may have increased their skills and could also act as a positive signal to employers. Furthermore, they may 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. Lastly, it should be borne in mind that there are costs associated with moving from welfare into employment. Some of these are monetary costs while others are more psychological in nature. For those who have spent a long time out of work and

[6] Dorsett et al. (2007) show that approximately 70 per cent of those employed one year post-randomisation worked 30 hours or more.

whose financial arrangements have developed around welfare receipt, the barriers to entering employment can be high. If ERA had been successful in encouraging employment during the operational period, it is plausible to argue that it may have served also to reduce individuals' psychological barriers to employment. If so, we would expect to see a long-term effect on employment entry. Similar arguments would suggest that that ERA might have positive effects on employment retention in the longer term. Increased skills, experience and qualifications are likely to make workers more valuable to their employers. In turn, this should be reflected in higher wages which will widen the gap between the income they can achieve in work and what they can expect on welfare.

4 The evaluation of ERA

Intake to the experiment began in October 2003 and ran until April 2005, although 97 per cent of the intake had joined by the end of 2004. Randomisation was at the point of entering the New Deal. Randomising at this point means that the experiment could deliver robust estimates of the overall effect of ERA but could not directly address the question of whether employment retention was increased. To get closer to this, an alternative might have been to randomise at the point of job entry. While attractive in some regards, this option was dismissed for a number of reasons. First, evidence emerging from the US seemed to suggest that it was optimal to encourage individuals to begin thinking about retention and advancement *before* job entry. Second, such a design would have been artificial since, in any implemented programme, the individual would know about the existence of the post-employment support while still on the New Deal, so the conditions of the experiment would never be replicated in practice. Third, while randomizing at the point of job entry may have provided a robust estimate of the impact of ERA on the duration of the first employment spell, consideration of retention in subsequent employment spells would encounter the same problems of non-random selection that arise when randomizing at New Deal entry.

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.[7] In fact, qualitative results reported in Hendra et al. (2011) suggest that ERA did relatively little to alter the nature of out-of-work support provided by caseworkers. The key difference when out of work was that those in the treatment group knew that they would become eligible for in-work support should they find full-time employment. Consequently, ERA altered work incentives both for those in work and those out of work. The in-work caseworker support available to the treatment group, however, was significantly different from that available to the control group; three quarters of those eligible for ERA had contact with caseworkers while in work compared to only 30 per cent for the control group.

The evaluation of ERA used data on clients' characteristics collected as part of the random assignment process and employment outcomes were taken from administrative tax records for a five-year follow-up period. This information was used to create a series of

[7] 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.

monthly indicators showing whether employment was the main 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.

Table 1 summarises a number of characteristics of the sample. This is shown separately for the treatment and control groups in order to provide some indication of the extent to which random assignment was successful in creating two similar-looking groups. For all the characteristics reported, the two groups appear to resemble each other very 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.[8]

Table 1: 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	0.14
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

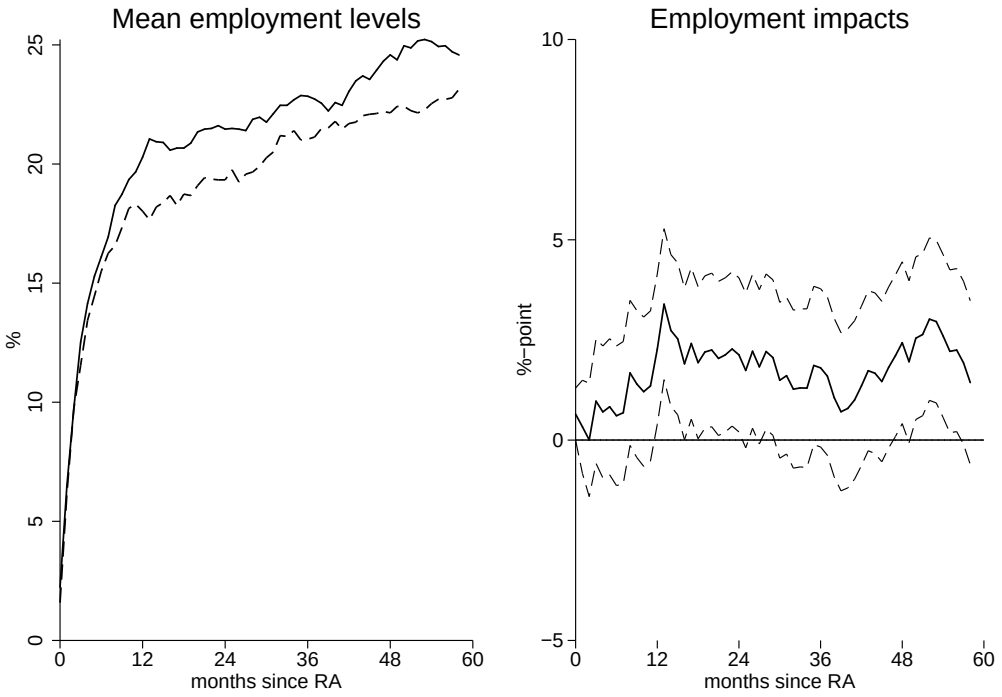
Figure 2 summarises the overall impacts on employment. The graph on the left charts the mean levels of employment for the treatment and control groups. There was little difference for much of the first year following random assignment but then the groups diverged and remained separated for the remainder of the observation period. The graph on the right shows the difference between the treatment and control groups. Due to random assignment, these differences provide an estimate of the impact of ERA on employment. Twelve months after randomisation, a small but significant effect appeared. This was quite stable (at about 2 percentage points) until month 30 when it lost statistical significance. However, after the four-year mark, the effect regained statistical significance and remained significant until close to the final observed months.

There are two points to highlight about the employment effect. First, although small, the 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 close to

[8]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.

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 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.

Figure 2: Employment levels and the overall impact of ERA



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.

5 Econometric approach

The effects presented in Figure 2 cannot tell us how ERA affected the length of employment spells among those who became employed, which is the key measure of employment retention. To address this question, we follow a similar methodology to Ham and LaLonde (1996) and Eberwein et al. (1997). The analysis allows the effect of ERA on hazard rates of employment entry and exit to be estimated so the extent to which the programme was successful in achieving the aim of increasing employment retention can be judged. In addition, persistence of treatment effects beyond ERA's operational period is also considered since this provides an insight into whether the programme might have lasting impacts and thereby increase self-sufficiency in the longer term.

We also consider the effect of post-randomisation employment experience on transitions into and out of employment. The main motivation behind this is to explore the extent to which any direct effects of ERA on employment might lead to persistent indirect effects mediated through increased employment experience. This might arise, for instance, if employers interpret employment experience as a signal of productivity or a proxy for skills acquired on-the-job.

As noted already, within subgroups defined on the basis of a post-random assignment outcome, there is no guarantee of statistical equivalence between treatment and control group members. Consequently, treatment-control contrasts within such subgroups cannot be assumed to have a causal interpretation. For example, if ERA had the effect of inducing less motivated individuals to enter employment, treatment-control group differences in the length of employment spells conditional on entering work would be biased because less motivated control group members would be absent from the employed subgroup. We follow the methodology pursued in other studies (for example, Dolton and O'Neill, 1996) to address this complication. This involves simultaneously estimating two duration processes – the time to enter employment and the time to exit employment. Each duration process is specified to include an unobserved heterogeneity term that can influence its hazard rate. The unobserved heterogeneity term of the employment entry hazard is allowed to be correlated with the unobserved heterogeneity term of the employment exit hazard and in this way controls for selection into subgroups on the basis of unobserved characteristics. Following Heckman and Singer (1984a), the unobserved heterogeneity joint distribution is approximated by a specified number of discrete mass points. Such an approach has become dominant in the literature and is appealing as it avoids arbitrary distributional restrictions.

We assume a mixed proportional hazards (MPH) specification. Identification of such models has been considered in a number of papers (for a survey, see Van den Berg, 2001). We argue that the MPH restriction is not the sole source of identification. Observing multiple spells for the same individual helps since it allows restrictions to be imposed across spells. Furthermore, in line with Gaure et al. (2007), Cockx and Picchio (2012) and Brinch (2011), the time-varying covariates included in the model provide an additional source of identification so that the proportionality assumption is not essential for identification. Most identification results relate to continuous time processes. Brinch (2011) considers identification of the single risk MPH model in the case of discrete data. 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.

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 (Eberwein et al., 1997; Kalwij, 2004) by adopting the solution suggested by Heckman and Singer (1984b), treating these initial spells separately from 'fresh' spells beginning after randomisation. This results in three possible states: initial non-employment (u_0), fresh non-employment (u) and fresh employment (e) and the econometric model allows three types of transition: $\{u_o \rightarrow e, e \rightarrow u, u \rightarrow e\}$.

Employment status is observed on a monthly basis. We follow Van den Berg and Van der

Klaauw (2001) and write the MPH hazard rate[9] from state j for individual i as:

$$\theta_{ij}(t|x_{ij}(\tau+t),\upsilon_{ij}) = \exp\left[\gamma_j(t) + x_{ij}(\tau+t)\beta_j\right]\upsilon_{ij}$$

for a spell that started at calendar time τ, where t measures the time since the start of the spell (see, for example, Cockx and Picchio, 2012). Duration dependence is captured by the baseline hazards, $\gamma_j(t)$, 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 $x_{ij}(\tau+t)\beta_j$. The specification allows for unobserved heterogeneity, υ_{ij}, where the subscript indicates that this may affect different types of transitions differently. Individual unobserved heterogeneity is assumed to be fixed across spells; this is not required for identification but is imposed to ease computation (which can be difficult for these types of models). Table 2 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 less 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.[10]

Table 2: 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	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

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

$$L_i^s(\upsilon_{ij}) = \left(1-\exp\left(-\theta_{ij}(d_i)\right)\right)^{y_{i,j}^s}\prod_{r=1}^{d_i-y_{i,j}^s}\exp\left(-\theta_{ij}(r)\right)$$

where $y_{i,j}^s$ 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). For compactness, the conditioning of the hazards

[9] 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.

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

on $x_{ij}(\tau+t)$ and υ_{ij} is left implicit. 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: $\{\upsilon_{u0e}, \upsilon_{eu}, \upsilon_{ue}\}$. The joint distribution is discretely approximated by M support points, where support point m is assumed to have probability p_m. The number of support 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:

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

6 Results

Within the econometric framework set out in section 5, four models are estimated. Model (a) estimates the overall effect of ERA on all hazards, taking no account of unobserved heterogeneity. Model (b) also captures the overall effect of ERA but now takes account of unobserved heterogeneity. The joint unobserved heterogeneity distribution is approximated using $M = 3$ points of support.[11] Model (c) also controls for unobserved heterogeneity but differs from model (b) in that it distinguishes the effect of ERA during its operational period from the effect of ERA after the operational period had completed. Lastly, model (d) is the same as model (c) but is parameterised to include an additional variable; the number of months of post-randomisation employment experience an individual has accumulated at the start of each new spell. The econometric framework allows this to have a causal interpretation. The experience term will reflect ERA's influence on previous spells. By including it, model d) provides an insight into whether ERA had an effect over and above that resulting from any increase in employment experience.

In addition, comparing the results of models c) and d) provides an informal check of whether the influence of unobserved heterogeneity has been adequately captured by the model. Should it not be adequately captured, one would expect that including post-randomisation employment experience as an additional regressor would alter the estimated effect of ERA. The intuition behind this is that post-randomisation employment experience is itself the product of a number of factors including unobserved heterogeneity. Therefore if unobserved heterogeneity had not been adequately controlled for through the specification of the model, adding the experience term as an additional regressor in the model would be expected to change the estimated impact of ERA.

The estimated effects of ERA under each of these model specifications are shown in Table 3. The coefficients are transformed so they show the proportionate impact on hazard rates

[11] 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. Including additional points of support resulted in convergence difficulties.

of employment entry (columns 1 and 3) and exit (columns 2). Full estimation results are given in Appendix Tables 5 - 8.

The results for Model (a) suggest that the only effect of ERA was to increase by 7.8 per cent the hazard of exit from the initial non-employment spell. There was no evidence of an effect on employment retention (column 2) nor on subsequent employment entry (column 3). As emphasised above, the process of differential sorting means that these estimates of programme effect are likely to be biased. The results of model (b) control for this by taking account of unobserved heterogeneity. Again, the largest effect is seen for the initial employment spell. However, while still short of statistical significance at the conventional level, the retention effect (column 2) is now more definite (in fact, the p-value for the transformed coefficient is 0.104).

Distinguishing between the operational period and the post-operational period (model c), a different impression is formed. The increase in the hazard of exit from the initial non-employment spell is seen to apply only in the operational period. This effect is large; an increase of 17.6 per cent. Once eligibility to ERA ended, this effect completely disappeared. This model also reveals an impact on employment retention. This is not seen during the operational period but emerges after that, reducing the hazard of exit from employment by 12.5 per cent.

Model (d) shows that this effect remains even after taking account of the separate effects of accumulated employment experience on hazard rates. Since ERA has been shown to increase the hazard of exit from the initial non-employment spell, it is plausible that the resulting increase in employment experience may itself help individuals find a job more quickly when out of work and remain longer in work.[12] Columns 2 and 3 confirm this to be the case. However, even after controlling for this, a significant effect of ERA on employment retention remains. It seems therefore that it is not just through increased employment experience that those in the treatment group are better able to remain in work but that the support available under ERA has been successful through some other channel in making workers more resilient in employment. Furthermore, in line with the previous discussion, the robustness of the estimated ERA effect to the inclusion of post-randomisation employment as an additional regressor provides an informal indication that the model controls adequately for the influence of unobserved heterogeneity.

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 a random sample of 1,000 individuals from our estimation sample. Next, we take 1,000 draws from a multivariate normal distribution with means corresponding to the estimated coefficients reported in Appendix Table 7 and variance given by the associated variance-covariance matrix. For each individual in our sample, 1,000 post-randomisation labour market trajectories are simulated, one for each

[12] Frijters et al. (2009) provide evidence that longer periods of employment experience increase transitions into work, while longer non-employment experience reduces transitions into work.

Table 3: Estimated proportionate effects of ERA on hazard of exit, by type of spell

	(1) Initial non-employment	(2) fresh employment	(3) fresh non-employment
Model (a)			
ERA	0.078**	-0.028	0.020
	(0.037)	(0.030)	(0.044)
Model (b)			
ERA	0.111*	-0.061	0.021
	(0.057)	(0.038)	(0.045)
Model (c)			
ERA, operational period	0.176***	-0.009	-0.016
	(0.065)	(0.045)	(0.055)
ERA, post-operational period	0.001	-0.125***	0.052
	(0.074)	(0.043)	(0.056)
Model (d)			
ERA, operational period	0.179***	0.021	0.036
	(0.064)	(0.044)	(0.060)
ERA, post-operational period	-0.006	-0.108**	-0.016
	(0.072)	(0.042)	(0.053)
Employment experience		-0.008**	0.017***
		(0.003)	(0.003)

Notes: Standard errors in parentheses. Asterisks indicate level of statistical significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Full estimation results are given in Appendix Tables 5 - 8. The results presented here are given as a transformation, $f(x) = \exp(x) - 1$, of the estimated coefficients in order to show the proportionate effect of ERA on the hazard rate in question. Results also control for the length of the spell (baseline hazard), age, sex, education, partnership status, whether the individual lived with dependent children, the ratio of unemployed per vacancy (nationally) and the deviation of local claimant unemployment from the national rate.

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.

We use model (c) as the preferred specification for simulation. To re-cap, model (c) controls for unobserved heterogeneity and allows the effect of ERA during its operational period to differ from its effect in the post-operational period. An alternative would be to use model (d) which in addition allows employment experience post-randomisation to play a role. However, because the level of post-employment experience is itself affected by ERA, choosing model (d) as the basis for simulation would result in these indirect effects of ERA being excluded from the simulated impact. For the purposes of understanding the relative contribution of employment entry and employment retention to the overall ERA effect, model (c) is the appropriate choice.

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, which is similar to the raw treatment-control differences shown in Figure 2. Also similar is that these impacts are on the margins of statistical significance (as indicated by the 95 per cent confidence intervals). The main difference between the simulated impacts compared to those shown in Figure 2 is that the model predicts the impacts to emerge more quickly. With the raw data, no significant effect was found for the first year, whereas the simulated impacts are statistically significant from the start.

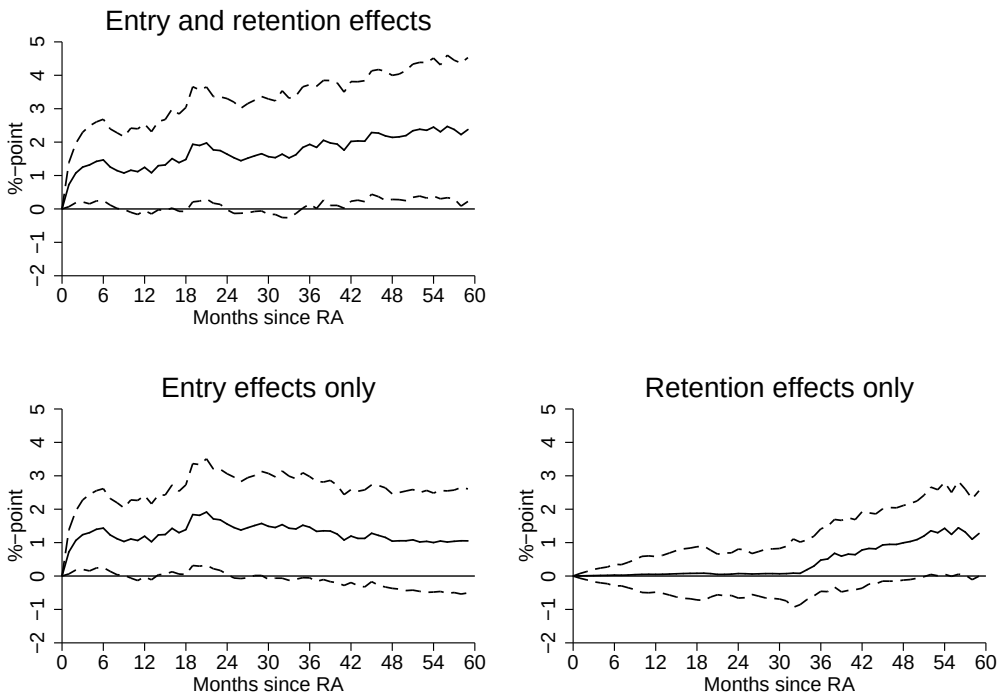
The two graphs at the bottom of Figure 3 show the simulated impacts when allowing only employment entry effects (bottom left) or only employment retention effects (bottom right). From this, it is clear that the overall impacts in the top graph are driven almost entirely by employment entry effects during the operational period. From about the 20-month mark, the contribution of these entry effects gently decline and, over time, become less significant. This reflects the fact evident from Table 3 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 begins in the post-operational period and grows over time to match the contribution of the entry effect. From the 50-month point onwards, retention effects alone are sufficient to result in significant employment gains. Again, the pattern of results reflects the findings reported in Table 3 that ERA reduced the hazard of employment exit in the post-operational period but not in the operational period.

6.1 Sensitivity analysis

The results presented above define ERA's operational period as being the first 33 months since randomisation and the post-operational period as being all later months. As already discussed, this threshold was chosen on the grounds that eligibility for ERA expired for everyone after this point. In practice, however, individuals varied in when their eligibility ended. At the extreme, eligibility could end after 24 months. This would be the case for individuals who received six payments of the earnings supplement within the first 24 months following randomisation.[13] For others receiving all six payments, their eligibility would end

[13]Hendra et al. (2011) report that 35 per cent of individuals in the treatment group received at least one payment of the earnings supplement and, of these, 20 per cent received all six. This implies that 7 per

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



at some point between 24 and 33 months post randomisation. So, while eligibility ends 33 months after randomisation for those receiving fewer than six payments, it can end sooner for others.

To test the sensitivity of the results to how the operational and post-operational periods are defined, an additional model was estimated that allowed the impact of ERA to vary over time in a more flexible way. Specifically, model (c) was changed so that, instead of the effect varying before and after the 33-month point, ERA was now allowed to have a different effect every quarter.[14] The estimation results are summarised in Table 4 and shown in full in Appendix Table 9. These are helpful in providing a more detailed insight into the evolution of the ERA effect. Unsurprisingly, the quarterly estimates display a certain amount of variation but some broad patterns are evident. Looking first at exits from the initial non-employment spell, the significant positive effects are concentrated in the first 6 quarters following randomisation. It appears that the support available under ERA induced an increased exit from the initial non-employment spell but that those individuals who benefited from this did so in the first four or five quarters. While the results presented earlier found a large significant effect in the first 33 months, these new results show that this was driven by even larger impacts in the first five quarters, with little impact beyond that point. Turning to the fresh spells, with the exception of quarter 6, there are no significant impacts

cent of the treatment group received the maximum number of payments. Unfortunately, the time taken by individuals to receive all six is not recorded.

[14] Quarter 1 covers month 0 (the month of randomisation), month 1 and month 2.

on employment exits until quarter 11. Beyond that point, the effects are consistently negative (indicating increased employment retention) and often statistically significant. Hence, there is evidence that the retention effect is sustained through much of the observation period. Lastly, the results reveal a more nuanced impression of the effect of ERA on the hazard of exiting fresh non-employment spells. The results presented earlier suggest no effect in the first 33 months, nor in the months after that. The results in the third column of Table 4 suggest ERA impacts in quarters 6, 13, 15 and 16 post-randomisation. The quarter 15 and quarter 16 results provide some suggestion of a positive longer-term effect on employment entry but this is offset to some extent by the negative effect in quarter 13.

Table 4: Sensitivity analysis - allowing the effects of ERA to vary by quarter since randomisation

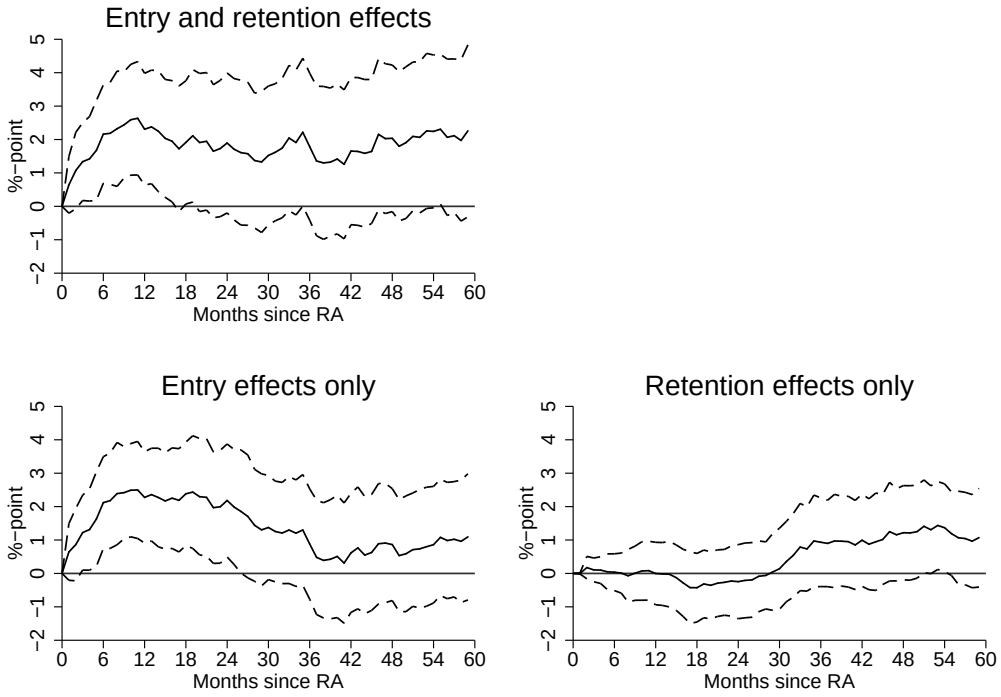
	(1)	(2)	(3)
ERA effect	Initial non-employment	fresh employment	fresh non-employment
Quarter 1	0.157*	-0.168	0.577
Quarter 2	0.280**	0.027	0.047
Quarter 3	0.504***	0.014	0.113
Quarter 4	0.331**	-0.078	0.007
Quarter 5	0.271*	0.030	0.017
Quarter 6	0.239	0.207*	-0.246**
Quarter 7	0.175	-0.004	0.117
Quarter 8	0.051	-0.006	-0.100
Quarter 9	-0.186	0.047	0.100
Quarter 10	-0.002	-0.115	-0.070
Quarter 11	0.060	-0.183**	-0.024
Quarter 12	-0.045	-0.168*	-0.042
Quarter 13	-0.335***	-0.060	-0.231**
Quarter 14	-0.001	-0.191**	-0.019
Quarter 15	-0.248*	-0.103	0.264*
Quarter 16	0.044	-0.159*	0.307**
Quarter 17	0.032	-0.175**	0.054
Quarter 18	-0.093	-0.171*	0.205
Quarter 19	0.441*	-0.014	-0.104
Quarter 20	0.206	-0.134	-0.047

Notes: Asterisks indicate level of statistical significance: * p<0.10, ** p<0.05, *** p<0.01. For compactness, standard errors are not shown but full estimation results are given in Appendix Table 9. The results presented here are given as a transformation, $f(x) = \exp(x) - 1$, of the estimated coefficients in order to show the proportionate effect of ERA on the hazard rate in question. Results also control for the length of the spell (baseline hazard), age, sex, education, partnership status, whether the individual lives with dependent children, the ratio of unemployed per vacancy (nationally) and the deviation of local claimant unemployment from the national rate.

Figure 4 uses these more flexible results to repeat the simulation exercise presented above. The inclusion of numerous regressors that are not statistically significant reduces the precision of the simulated impacts somewhat relative to those seen in Figure 3. However, the

same broad pattern of results is visible. Initially, it is employment entry effects that dominate but this declines (a little more markedly than before), and from about the 33-month point, retention effects become more important. The fact that this more flexible specification gives results comparable to those provided by model (c) suggests that assuming the impacts of ERA to be the same over the first 33 months and then over the remaining months is not unduly restrictive, although it does mask some suggestions of a post-operational impact on the hazard of exiting fresh non-employment spells.

Figure 4: Simulating the overall ERA effect, the effect on employment entry and the effect on employment retention, allowing the ERA effect to vary by quarter since randomisation



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). The detailed cost-benefit analysis in Hendra et al. (2011) shows that making ERA available to the long-term unemployed delivers substantial net benefits, both at the level of the individual and government. Over a five year period, individuals benefited (primarily through increased earnings) by about £550 while the Government benefited by £350 per eligible individual (the combination of reduced transfer payments and increased taxes more than compensating

for ERA-related costs). Over ten years, the projected benefits are greater still; £725 for individuals and £1,800 for the Government.

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. Beyond this point, significant positive effects on employment retention were seen. On this basis, ERA was successful in achieving its aim of not only increasing employment but, more specifically, increasing employment retention. While the effect of ERA was small, this must be seen against the low employment rates among the control group (always less than 25 per cent).

Increasing employment retention has a number of desirable consequences, including employment stability, skill acquisition, earnings growth and, potentially, career advancement. The fact this this has been possible among a 'hard-to-help' group such as the long-term unemployed is particularly encouraging. It suggests that the combination of improved incentives and caseworker support is successful in helping individuals survive the transition into employment and may therefore be an effective policy to break the 'low-pay no-pay' cycle. It is revealing to note that the longer-term effects of ERA cannot be accounted for simply by the fact that those helped into employment by ERA acquired valuable experience that helped them remain longer in work and, when unemployed, helped them to find a new job more quickly. Despite this effect being shown to be important, controlling for it does little to alter the estimated effect of ERA. This suggests that, as well as having a long-term effect by increasing individuals' stock of employment experience, ERA improved workers' resilience in another way. Of course, part of the aim of ERA was to strengthen human capital and it attempted to achieve this by incentivizing training. While speculative, one possibility is that individuals' improved skills increase their value to employers and perhaps help them to advance to a position of greater security.

When considering the policy relevance of these results, attention must be given to the fact that changes to both the welfare system and active labour market policy in Britain have been announced since the time of this study. In October 2013, Universal Credit will begin to replace Jobseeker's Allowance and five other major benefits with a single welfare payment. The range of active labour market programmes in Britain has also been streamlined. The Work Programme was introduced in June 2011 and replaces most other schemes, including New Deal 25 Plus. A key feature of the Work Programme is that support is provided, or at least coordinated, largely by private sector companies who are paid according to the outcomes they achieve. The size of the payments varies according to customer characteristics; those thought more difficult to place in work attract higher payments. However, for all customer groups, the structure of payments is designed to reward sustained employment.

Providers are free to choose the methods they feel are most likely to achieve the required results. Since the Work Programme identifies long-term unemployed people over the age of 25 as one of its specified customer groups, the demonstrated effectiveness of ERA for this group should be of real interest, particularly since this group represents by far the largest number of Work Programme participants. However, there are two obstacles to providers making available an ERA-type programme. First, the Work Programme is structured around a two-

year period of working with customers. Significant impacts from ERA were visible only after a year and longer-term benefits accumulated only beyond this point. However, since the costs of ERA fell most heavily in the early years (the operational period), the prospect of impacts in the longer-term is not helpful to providers, who must make a profit within a two-year accounting period. Second, while ERA comfortably passes a cost-benefit test, much of the benefit is driven by increased taxes and reduced transfer payments. These do not accrue to providers and consequently do not feature in their decision-making. In conclusion, while ERA has been shown to be effective in increasing employment and employment retention, under current arrangements there is little incentive for the private companies who deliver back to work services in Britain to introduce a similar type of support.

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Appendix

Table 5: Model a - estimated effects of ERA on hazard of exit, by type of spell (not allowing for unobserved heterogeneity)

	Type of spell: Initial non-employment	Employment	Non-employment
ERA effect	0.075**	-0.028	0.020
	(0.034)	(0.031)	(0.043)
Baseline hazard:			
- months 1-12	1.574***		
	(0.075)		
- months 13-24	1.098***		
	(0.056)		
- month 1		1.397***	0.895***
		(0.045)	(0.080)
- month 2		1.284***	0.948***
		(0.049)	(0.080)
- month 3		1.044***	0.844***
		(0.057)	(0.086)
- months 4-6		0.825***	0.710***
		(0.044)	(0.064)
- months 7-12			0.580***
			(0.058)
Age:			
- 30-39	-0.280***	-0.088**	-0.080
	(0.047)	(0.041)	(0.056)
- 40-49	-0.419***	-0.181***	-0.087
	(0.051)	(0.045)	(0.062)
- 50-59	-0.630***	-0.362***	-0.361***
	(0.060)	(0.057)	(0.083)
Female	0.071	-0.255***	0.037
	(0.044)	(0.042)	(0.057)
Partnered	0.157***	-0.117**	0.008
	(0.052)	(0.047)	(0.068)
Dependent children	0.101*	-0.165***	0.129*
	(0.060)	(0.055)	(0.078)
Highest educational qualification:			
- secondary school	0.277***	-0.063	0.207***
	(0.042)	(0.038)	(0.054)

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- post-secondary school	0.365***	-0.183***	0.345***
	(0.051)	(0.048)	(0.066)
- some other qualification	0.297***	-0.069	0.295***
	(0.055)	(0.051)	(0.069)
Number of unemployed per vacancy	-0.592***	-0.051**	-0.152***
	(0.045)	(0.022)	(0.029)
Unemployment rate, local deviation	-0.242***	-0.069*	0.061
	(0.041)	(0.040)	(0.057)
Constant	-2.749***	-2.947***	-3.595***
	(0.126)	(0.081)	(0.111)
Log-likelihood		-42,337.349	
N		6,742	

Table 6: Model b - estimated effects of ERA on hazard of exit, by type of spell (allowing for unobserved heterogeneity)

	Type of spell: Initial non-employment	Employment	Non-employment
ERA effect	0.105**	-0.063	0.021
	(0.051)	(0.040)	(0.044)
Baseline hazard:			
- months 1-12	1.149***		
	(0.132)		
- months 13-24	0.561***		
	(0.077)		
- month 1		0.979***	0.835***
		(0.057)	(0.086)
- month 2		0.897***	0.893***
		(0.059)	(0.086)
- month 3		0.684***	0.793***
		(0.064)	(0.091)
- months 4-6		0.513***	0.664***
		(0.050)	(0.068)
- months 7-12			0.545***
			(0.061)
Age:			
- 30-39	-0.351***	-0.099*	-0.065
	(0.068)	(0.053)	(0.059)
- 40-49	-0.532***	-0.209***	-0.067
	(0.077)	(0.059)	(0.066)
- 50-59	-0.848***	-0.404***	-0.331***
	(0.090)	(0.076)	(0.090)
Female	0.089	-0.291***	0.048
	(0.064)	(0.052)	(0.059)
Partnered	0.300***	-0.153**	-0.001
	(0.075)	(0.061)	(0.070)
Dependent children	0.129	-0.200***	0.138*
	(0.086)	(0.069)	(0.079)
Highest educational qualification:			
- secondary school	0.395***	-0.073	0.215***
	(0.063)	(0.050)	(0.056)
- post-secondary school	0.459***	-0.214***	0.359***
	(0.079)	(0.061)	(0.069)

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- some other qualification	0.461***	-0.116*	0.311***
	(0.082)	(0.065)	(0.073)
Number of unemployed per vacancy	-0.105**	-0.052**	-0.162***
	(0.041)	(0.024)	(0.031)
Unemployment rate, local deviation	-0.386***	-0.061	0.060
	(0.058)	(0.049)	(0.059)
Constant	-4.537***	-2.519***	-3.552***
	(0.150)	(0.127)	(0.151)
Unobserved heterogeneity			
Mass point 2	3.024***	0.530***	-0.157
	(0.099)	(0.123)	(0.142)
Mass point 3	2.441***	-1.427***	0.566**
	(0.140)	(0.158)	(0.252)
Prob(unobserved heterogeneity group 1)	0.748***		
	(0.013)		
Prob(unobserved heterogeneity group 2)	0.136***		
	(0.013)		
Prob(unobserved heterogeneity group 3)	0.117***		
	(0.016)		
Log-likelihood		-41,938.641	
N		6,742	

Table 7: Model c - estimated effects of ERA on hazard of exit, by type of spell (allowing for unobserved heterogeneity; distinguishing between operational and post-operational periods)

	Type of spell:		
	Initial non-employment	Employment	Non-employment
ERA effect, operational period	0.162***	-0.009	-0.016
	(0.055)	(0.045)	(0.056)
ERA effect, post-operational period	0.001	-0.134***	0.051
	(0.074)	(0.049)	(0.053)
Baseline hazard:			
- months 1-12	1.138***		
	(0.131)		
- months 13-24	0.524***		
	(0.077)		
- month 1		0.975***	0.867***
		(0.057)	(0.087)
- month 2		0.892***	0.923***
		(0.059)	(0.086)
- month 3		0.680***	0.822***
		(0.064)	(0.091)
- months 4-6		0.508***	0.691***
		(0.050)	(0.069)
- months 7-12			0.567***
			(0.062)
Age:			
- 30-39	-0.352***	-0.096*	-0.073
	(0.067)	(0.053)	(0.058)
- 40-49	-0.532***	-0.203***	-0.076
	(0.075)	(0.059)	(0.066)
- 50-59	-0.848***	-0.398***	-0.347***
	(0.089)	(0.076)	(0.089)
Female	0.087	-0.293***	0.046
	(0.063)	(0.052)	(0.058)
Partnered	0.292***	-0.156***	0.002
	(0.074)	(0.060)	(0.069)
Dependent children	0.129	-0.199***	0.137*
	(0.084)	(0.068)	(0.079)
Highest educational qualification:			
- secondary school	0.390***	-0.075	0.216***

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	(0.062)	(0.050)	(0.056)
- post-secondary school	0.465***	-0.214***	0.359***
	(0.078)	(0.061)	(0.069)
- some other qualification	0.462***	-0.116*	0.311***
	(0.081)	(0.065)	(0.072)
Number of unemployed per vacancy	-0.097**	-0.041*	-0.163***
	(0.042)	(0.024)	(0.031)
Unemployment rate, local deviation	-0.381***	-0.061	0.057
	(0.058)	(0.048)	(0.059)
Constant	-4.528***	-2.541***	-3.587***
	(0.149)	(0.125)	(0.146)
Unobserved heterogeneity			
Mass point 2	3.023***	0.519***	-0.084
	(0.102)	(0.125)	(0.138)
Mass point 3	2.429***	-1.425***	0.516*
	(0.143)	(0.164)	(0.277)
Prob(unobserved heterogeneity group 1)	0.756***		
	(0.014)		
Prob(unobserved heterogeneity group 2)	0.127***		
	(0.013)		
Prob(unobserved heterogeneity group 3)	0.116***		
	(0.016)		
Log-likelihood		-41,936.17	
N		6,742	

Table 8: Model d - estimated effects of ERA on hazard of exit, by type of spell (allowing for unobserved heterogeneity; distinguishing between operational and post-operational periods; allowing for the effect of post-randomisation employment experience)

	Type of spell: Initial non-employment	Employment	Non-employment
ERA effect, operational period	0.165***	0.021	0.035
	(0.054)	(0.043)	(0.058)
ERA effect, post-operational period	-0.006	-0.115**	-0.016
	(0.073)	(0.048)	(0.054)
Post-randomisation employment (months)		-0.008***	0.017***
		(0.003)	(0.003)
Baseline hazard:			
- months 1-12	1.193***		
	(0.135)		
- months 13-24	0.529***		
	(0.076)		
- month 1		1.048***	0.719***
		(0.059)	(0.090)
- month 2		0.966***	0.781***
		(0.061)	(0.090)
- month 3		0.752***	0.685***
		(0.066)	(0.095)
- months 4-6		0.575***	0.568***
		(0.052)	(0.073)
- months 7-12			0.471***
			(0.065)
Age:			
- 30-39	-0.349***	-0.091*	-0.075
	(0.067)	(0.049)	(0.058)
- 40-49	-0.524***	-0.196***	-0.073
	(0.074)	(0.055)	(0.066)
- 50-59	-0.836***	-0.378***	-0.367***
	(0.088)	(0.071)	(0.088)
Female	0.091	-0.283***	-0.003
	(0.062)	(0.050)	(0.060)
Partnered	0.274***	-0.119**	-0.031
	(0.073)	(0.057)	(0.070)
Dependent children	0.129	-0.212***	0.101

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	(0.083)	(0.065)	(0.080)
Highest educational qualification:			
- secondary school	0.380***	-0.077	0.190***
	(0.061)	(0.047)	(0.056)
- post-secondary school	0.450***	-0.208***	0.313***
	(0.077)	(0.057)	(0.069)
- some other qualification	0.438***	-0.102*	0.265***
	(0.080)	(0.061)	(0.072)
Number of unemployed per vacancy	-0.099**	-0.045*	-0.216***
	(0.041)	(0.025)	(0.032)
Unemployment rate, local deviation	-0.369***	-0.069	0.083
	(0.057)	(0.047)	(0.059)
Constant			
Unobserved heterogeneity			
Mass point 2	2.941***	0.672***	0.047
	(0.134)	(0.097)	(0.109)
Mass point 3	2.639***	-1.000***	-0.907***
	(0.118)	(0.137)	(0.220)
Prob(unobserved heterogeneity group 1)	0.768***		
	(0.013)		
Prob(unobserved heterogeneity group 2)	0.100***		
	(0.013)		
Prob(unobserved heterogeneity group 3)	0.132***		
	(0.015)		
Log-likelihood		-41,921.764	
N		6,742	

Table 9: Sensitivity analysis - estimated effects of ERA on hazard of exit, by type of spell (allowing for unobserved heterogeneity; allowing for the effect to vary by quarter since randomisation)

	Type of spell:		
	Initial non-employment	Employment	Non-employment
ERA, quarter 1	0.146*	-0.183	0.456
	(0.081)	(0.136)	(0.583)
ERA, quarter 2	0.246***	0.027	0.046
	(0.088)	(0.093)	(0.257)
ERA, quarter 3	0.408***	0.014	0.107
	(0.096)	(0.094)	(0.175)
ERA, quarter 4	0.286***	-0.082	0.007
	(0.108)	(0.100)	(0.160)
ERA, quarter 5	0.240**	0.029	0.017
	(0.116)	(0.096)	(0.148)
ERA, quarter 6	0.214*	0.188**	-0.282*
	(0.123)	(0.095)	(0.157)
ERA, quarter 7	0.161	-0.004	0.111
	(0.130)	(0.103)	(0.129)
ERA, quarter 8	0.050	-0.006	-0.105
	(0.142)	(0.101)	(0.139)
ERA, quarter 9	-0.206	0.046	0.095
	(0.163)	(0.101)	(0.124)
ERA, quarter 10	-0.002	-0.122	-0.072
	(0.152)	(0.108)	(0.133)
ERA, quarter 11	0.058	-0.203*	-0.025
	(0.151)	(0.111)	(0.129)
ERA, quarter 12	-0.047	-0.184*	-0.043
	(0.160)	(0.109)	(0.129)
ERA, quarter 13	-0.407**	-0.062	-0.263*
	(0.191)	(0.104)	(0.140)
ERA, quarter 14	-0.001	-0.212*	-0.019
	(0.161)	(0.113)	(0.123)
ERA, quarter 15	-0.285	-0.109	0.234**
	(0.185)	(0.103)	(0.110)
ERA, quarter 16	0.043	-0.173	0.268**
	(0.163)	(0.105)	(0.109)
ERA, quarter 17	0.031	-0.193*	0.052
	(0.170)	(0.106)	(0.122)

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ERA, quarter 18	-0.098	-0.188*	0.187
	(0.192)	(0.109)	(0.122)
ERA, quarter 19	0.365**	-0.014	-0.109
	(0.174)	(0.110)	(0.148)
ERA, quarter 20	0.188	-0.144	-0.048
	(0.242)	(0.145)	(0.180)
Baseline hazard:			
- months 1-12	1.190***		
	(0.123)		
- months 13-24	0.574***		
	(0.080)		
- month 1		1.000***	0.860***
		(0.058)	(0.090)
- month 2		0.911***	0.918***
		(0.060)	(0.089)
- month 3		0.691***	0.819***
		(0.065)	(0.093)
- months 4-6		0.513***	0.691***
		(0.052)	(0.071)
- months 7-12			0.574***
			(0.063)
Age:			
- 30-39	-0.351***	-0.092*	-0.071
	(0.066)	(0.052)	(0.058)
- 40-49	-0.531***	-0.192***	-0.076
	(0.074)	(0.058)	(0.066)
- 50-59	-0.842***	-0.382***	-0.346***
	(0.088)	(0.075)	(0.089)
Female	0.084	-0.293***	0.044
	(0.062)	(0.052)	(0.058)
Partnered	0.276***	-0.168***	0.002
	(0.073)	(0.060)	(0.069)
Dependent children	0.127	-0.199***	0.141*
	(0.083)	(0.067)	(0.079)
Highest educational qualification:			
- secondary school	0.383***	-0.079	0.217***
	(0.061)	(0.049)	(0.056)
- post-secondary school	0.472***	-0.215***	0.361***
	(0.076)	(0.060)	(0.069)
- some other qualification	0.455***	-0.120*	0.311***
	(0.079)	(0.064)	(0.072)
Number of unemployed per vacancy	-0.145***	-0.050*	-0.151***
	(0.049)	(0.029)	(0.036)

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Unemployment rate, local deviation	-0.369***	-0.061	0.056
	(0.058)	(0.048)	(0.059)
Constant	-4.353***	-2.478***	-3.629***
	(0.162)	(0.150)	(0.152)
Unobserved heterogeneity			
Mass point 2	3.111***	0.412**	-0.080
	(0.134)	(0.182)	(0.139)
Mass point 3	2.228***	-1.559***	0.597*
	(0.233)	(0.200)	(0.305)
Prob(unobserved heterogeneity group 1)	0.770***		
	(0.018)		
Prob(unobserved heterogeneity group 2)	0.116***		
	(0.013)		
Prob(unobserved heterogeneity group 3)	0.114***		
	(0.019)		
Log-likelihood		-41,899.483	
N		6,742	