WORKING PAPER SERIES

NO. 74

UNEMPLOYMENT DURATION BEFORE AND AFTER NEW DEAL

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ECONOMIC RESEARCH CENTRE

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July 2002

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We gratefully acknowledge financial support and permission to reproduce figures and tables from the Northern Ireland Department for Employment and Learning (DEL) and access to data granted by Statistics Research Branch at the Northern Ireland Department of Enterprise, Trade and Investment (DETI). In particular we would like to thank Fiona Hepper, Sean Donnelly and Wendy McMeekan at DETI, Eoin McFadden and Dave Rogers at DEL, Chris Allen at NIERC for excellent research assistance and Stephen Jenkins at the University of Essex for advice on programming. The research is part of NIERC's Human Resources and Economic Development research programme supported by DETI and the Department of Finance and Personnel (DFP). The views expressed are those of the authors.

Abstract

A major active labour market policy for unemployed young people – the New Deal for 18-24 Year Olds or New Deal for Young People (ND18-24/NDYP) – was introduced throughout the UK in 1998. We examine its effects on unemployment duration by estimating hazard functions for unemployment outflows before and after its introduction. We add value to existing ND18-24 evaluations in the following ways. First, we examine previously unused administrative data for Northern Ireland. Second, we examine ND18-24 effects at *all* unemployment durations. Third, we estimate separately by gender. Fourth, exits to employment, education and training and other benefits are identified separately. Our results suggest that since ND18-24's introduction, young people are 25-50% less likely to experience year-long unemployment spells, with increased probabilities for all types of exit. ND18-24 is intended, however, to largely eradicate long term youth unemployment. We ask why this has not been the case in Northern Ireland.

1. Introduction

This paper examines the effects of the New Deal for Young People (ND18-24) in Northern Ireland (NI)[1] – a major active labour market policy for unemployed young people aged 18-24 – on outflows from unemployment and duration of unemployment spells for the target age group. The policy, aimed at eradicating long-term youth unemployment by helping young people into work, was introduced across the UK in April 1998.[2]

Our data are a 20% random sample of all those aged between 16 and 30 years old experiencing spells of claimant count unemployment in NI beginning between January 1995 and July 2001. These data are taken from the NI computerised unemployment, or *Job Seekers Allowance* (JSA) register. This study is the first to examine NI administrative data and provides the first detailed quantitative evaluation of New Deal in NI.

The paper evaluates the effects of ND18-24 by estimating hazard functions – showing the probability of exits from unemployment after different durations – before and after its introduction. This approach allows New Deal effects to be estimated across disaggregated and unrestricted unemployment durations in contrast to existing studies. We also add value by presenting separate hazard functions for exits to employment, education and training and other benefits, and by estimating the effects of New Deal on exits to employment separately for males and females. After controlling for other relevant factors, and using 25-29 year olds as a comparison group to control for general economic and labour market trends, the difference between the pre and post ND18-24 hazard functions gives a measure of the effects of ND18-24 on unemployment outflows for the target age group. Corresponding survival functions show 'before and after' chances of experiencing unemployment spells of different durations.

[1] ND18-24 is called the New Deal for Young People (NDYP) in GB, but the policies are the same. Therefore the terms ND18-24 and NDYP are interchangeable, although we stick with ND18-24 for NI and NDYP for GB.
[2] Some areas in GB introduced NDYP in January 1998 to act as pilots. There were no such pilot areas in NI.

Our motivation is to inform policy makers, in the UK and beyond, of the effect ND18-24 is having on outflows from youth unemployment, and the resulting implications for long-term unemployment amongst young people. Large numbers of young people are directly affected by ND18-24 (e.g. 650,000 had participated in NDYP in GB by May 2001) and large sums of public money are involved (e.g. £1.5bn has been allocated in GB for 1997/98-2001/02). There has also been considerable recent interest in active labour market policies in many other OECD countries (see, e.g. Van Reenen, 2001, for a discussion).

The following section details ND18-24, presents brief summary statistics for ND18-24 in NI, and reviews existing evaluations of ND18-24/NDYP. Section 3 sets out our model in the context of the search theory and empirical duration analysis of unemployment literatures. Section 4 introduces the data. Section 5 presents and discusses our empirical results and Section 6 concludes.

2. The New Deal for 18-24 Year Olds

Following the introduction of ND18-24, a young person unemployed and claiming unemployment benefits (JSA) for six months *must* report for an interview with a personal advisor or benefits can be withdrawn. There follows a period of individually tailored guidance and support, particularly in job search, called *Gateway*, which is intended to last up to four months.[3] If at the end of that time the young person is still unemployed, a compulsory *option* must be taken up (or again, benefits can be withdrawn). These options include full time education or training (FTET), subsidised employment placements, voluntary or environmental work (ETF). The subsidised employment option pays a wage, as can voluntary or environmental work (or they can attract a small supplement in addition to the continuation of benefits). Options usually last for up to six months with FTET usually lasting for up to one year. Young people on a New Deal option are counted as having left the unemployment register. The most common in NI is the voluntary sector option, followed by FTET, subsidised employment and ETF. Females are more likely than males to choose the FTET option

[3] In practice, Gateway sometimes lasts considerably longer than four months. 20% of all Gateway episodes in NI fall into this category, with some episodes apparently lasting a year or more (Source: McVicar and Podivinsky, 2002).

and are very unlikely to choose the ETF option. If, after completing a New Deal option, a young person is still without a job, they enter a *follow-through* stage (and go back on the unemployment register), with three months of further guidance and assistance in job search. At any time, a young person may leave ND18-24 to take up an unsubsidised job. Figure 1 shows a timeline for a ND18-24 episode.[4]

Some young people are eligible to join ND18-24 early, i.e. before they have been unemployed for six months, if they choose to do so. These include those who would otherwise be unemployed for six months but for short breaks in claiming JSA (amounting to not more than 28 days); those with a health condition or disability; those needing help with basic skills; those whose first language is not English; lone parents; and returners who have been out of the labour market for two years or more. Young people who leave unemployment at the Gateway stage but return to claimant unemployment within 13 weeks automatically re-enter Gateway.

Since its introduction in NI, there have been 35,671 ND18-24 episodes covering 29,396 people, some of whom have gone through ND18-24 more than once.[5] There have been 4,876 episodes with early entry (before six months of unemployment). There have been 2,938 episodes in follow-through. By gender, these figures are split roughly two thirds male and one third female, reflecting relative unemployment rates. Numbers in ND18-24 have been evenly spread over the last four years, with the exception of higher numbers in 1998 as the ‘stock’ of existing young long-term unemployed entered the program.

Around half of all the ND18-24 episodes in NI have ended with the young person finding work. Of these, three quarters are described as having ‘found sustained employment’, i.e. employment that lasted for at least three months.

Our use of 25-29 year olds as a comparison group is potentially complicated by a related policy, New Deal 25+ (ND25+), introduced in June 1998 in NI and GB. Those

[4] An ‘episode’ ends when an individual has left a ND18-24 activity and not returned to another ND18-24 activity within 13 weeks.

[5] 23,906 young people have had one ND18-24 episode, 4,689 have had two and 775 three or more episodes (Source: McVicar and Podivinsky, 2002).

aged 25 or over that have been in receipt of JSA benefits for 18 months or more are required to participate. As for ND18-24 job seekers are assigned a personal advisor during an initial Gateway period where assistance in job search is offered. Those remaining unemployed after Gateway enter either subsidised employment, FTET or a 13 week Intensive Activity Period (IAP) providing work experience placements. An enhanced ND25+ was introduced in April 2001, offering a longer Gateway period and a longer Preparation for Employment Programme (PEP) to replace IAP (NI Assembly Oral Question AQO/334/01). Since its introduction, there have been a total of 29,392 ND25+ episodes in NI, 25,198 of which have been males.[6]

Prior to the introduction of ND18-24 and ND25+, the most significant policy change with direct implications for unemployment over the latter half of the 1990s was the replacement of the existing unemployment benefit system by JSA in October 1996. This was intended to tighten the relationship between claiming unemployment-related benefits and actively searching for work through regular interviews and monitoring of job search progress for all claimants. Benefit sanctions were possible for those not showing evidence of sufficient job search effort.

Prior to this study, there has been little quantitative analysis of the effects of ND18-24 on young people in NI. Some survey-based analysis of New Dealers' characteristics is presented in DEL (2001). McVicar and Podivinsky (2001) provide exploratory (also survey-based) analysis of joblessness duration before and after ND18-24 as part of a more general study, and find some evidence of positive ND18-24 effects.

There has been more analysis of NDYP in GB. In particular, studies by the Institute of Fiscal Studies and the National Institute of Economic and Social Research are presented in papers by Van Reenen (2001), Blundell et al. (2001) and Riley and Young (2001a, b).

Van Reenen (2001) uses claimant count data up to the end of 1999 to examine the effect of NDYP on the employment chances of young men in GB who have spent at least six months on JSA. He analyses the JUVOS longitudinal database of a random

[6] Source: McVicar and Podivinsky (2002).

5% of all individuals who have ever claimed JSA. He uses both pilot areas where NDYP was introduced three months earlier than in other areas, and 25-30 year olds who have been unemployed for at least six months as comparison groups in order to identify NDYP effects. Because of a large increase in the participation rate of 18 year-olds in full time education, he analyses only 19-24 year olds.[7]

Van Reenen makes two critical assumptions. First, that the 19-24 year old age group and the 25-29 year old age group react to macroeconomic trends in the same way. Second, that there are no 'substitution' effects where 19-24 year old NDYP participants take jobs that would otherwise have been taken by 25-30 year olds. He then compares the difference in the outflow rates between the 19-24 year old and the relevant comparison group after NDYP began compared to the difference in outflow rates before NDYP started. The focus is on the effect by the end of the tenth month on JSA of outflows from JSA, conditional on individuals having completing six months of unemployment.

Given these assumptions, Van Reenen estimates that NDYP has increased the chances of young men getting jobs by around 20%, based upon a comparison between 19-24 year olds and 25-30 year olds after the nation-wide start of NDYP. This increased chance of getting a job includes subsidised jobs that are part of NDYP itself (the subsidised employment option). A comparison based upon the difference between pilot and non-pilot areas suggests an even greater effect (of around 40%) in obtaining jobs. These raw differences are also corrected for possible composition effects by including a set of extra controls for marital status, sought occupation, region, number of previous unemployment spells and the proportion of time spent unemployed in the previous two years. Allowing for these extra controls produces very similar overall effects. In addition, his analysis of the pilot areas suggests no evidence of 'substitution effects'. In an analysis of pilot versus non-pilot areas, the estimated effects for women are somewhat smaller than for men, but are imprecise because of the small proportion of women in the sample.

[7] In NI, the rate of participation in full time education for 18 year olds has been relatively stable over the last five years.

Blundell et al. (2001) use the same JUVOS longitudinal data and a similar approach to that of Van Reenen (2001) but focus on the effect of NDYP on outflows to employment only during the Gateway stage of NDYP for males and females. As with Van Reenen (2001), they use as a comparison group either eligible individuals in non-pilot areas or ineligible individuals (on age grounds) after the nation-wide NDYP introduction. They take care to ensure that pilot and non-pilot areas are matched as closely as possible with respect to labour market characteristics.

Based upon the pilot period of the programme, Blundell et al. (2001) find that the NDYP has increased the employment chances for young men (19-24 year olds) by around 20%. This effect is relatively robust to the particular choice of comparison group, the precise method of estimation, and the presence of other control variables. However they find no significant NDYP effect on employment chances for young women, partly because differential trends in GB for the 19-24 age group and the 25-30 age group make identification of such effects problematical.[8] They also find a 'program introduction effect' where the first three months of NDYP boosted employment chances for the 'stock' of young people that had been unemployed for well over six months. However, they do not consider the longer-term effects of NDYP, due to data constraints. Similarly to Van Reenen (2001), they find no evidence of significant 'substitution' effects.

Riley and Young (2001a) analyse New Deal effects using data on individuals from the New Deal Evaluation Database, matched by National Insurance numbers to the Benefits Agency's database. As with Van Reenen (2001) and Blundell et al. (2001), NDYP effects are identified by using older age groups and pilot areas as comparison groups. They estimate differences in outflow rates for 18-24 year olds both before and after the introduction of NDYP, and compare these with outflow rates for 25-29 year olds and 30-49 year olds. Unemployment duration is aggregated to three-monthly groups. They also estimate time series models for different age and duration groups, including an NDYP dummy variable and various measures of aggregate effects.

[8] The labour market trends for the two age groups in NI over the last five years are broadly similar.

With all these specifications they find NDYP to have reduced measured unemployment for the target group by raising employment, but also through shifts into an aggregate category they call ‘non-work’. Outflow rates to employment for those 18-24 year olds unemployed for six months or more are estimated to have increased by between 5% and 15%. This is confirmed by their evaluation of NDYP effects by comparing youth employment with estimated ‘counterfactual’ unemployment. They find mixed evidence on the effects of NDYP on outflows to employment for short-term (less than six months) unemployed 18-24 year olds.

Riley and Young (2001b) explore whether NDYP has had indirect effects on youth unemployment through equilibrium wage levels, finding no significant evidence of such effects.

3. Hazard Functions and Duration Dependence in Unemployment

Search theory suggests that the probability of a ‘match’ between a job seeker and a job vacancy at a particular point of time is the product of the probability of getting an offer and the probability of accepting that offer (see, e.g. Mortensen, 1987). These probabilities in turn will depend on factors such as search intensity, individual and local labour market characteristics.

If reservation wages fall with unemployment duration then job offers will be accepted more readily the longer a person remains unemployed. We might therefore observe positive duration dependence where the probability of a young person leaving unemployment for a job at any particular time increases with time spent unemployed (see, e.g. Mortensen, 1977). If, however, the unemployed lose motivation for job search the longer they have been unemployed, this might reduce the probability of getting a job offer, implying negative duration dependence (see, e.g. Layard et al., 1991). Negative duration dependence might also be caused by depreciation of human capital during an unemployment spell (e.g. Phelps, 1972), or because employers see unemployment as a negative productivity signal and are reluctant to hire previously unemployed workers (e.g. Blanchard and Diamond, 1994). Given this theoretical ambiguity, the nature of duration dependence is an interesting empirical question.

Duration dependence is reflected in the slope of the hazard function, with downward (upward) sloping hazard functions corresponding to negative (positive) duration dependence. Empirical evidence on the shape of hazard functions for unemployment outflows has been mixed, both across and within countries. Reviews of this literature are provided by Devine and Kiefer (1991) and Machin and Manning (1999).

Studies examining duration dependence in unemployment specifically for young people are not common, but internationally Heckman and Borjas (1980), Lynch (1989), Korpi (1995), and Russell and O'Connell (2001) between them find evidence of no duration dependence, negative duration dependence and non-monotonic duration dependence. McVicar and Podivinsky (2001) find evidence of generally downward sloping hazard functions for young people in NI.

One factor likely to be contributing to this empirical ambiguity is the near observational equivalence of duration dependence and unobserved heterogeneity. People may be long-term unemployed because they have poor job prospects rather than people having poor job prospects because they are long-term unemployed. Failure to consider unobserved heterogeneity can lead to a false conclusion of negative duration dependence (Heckman and Borjas, 1980).

There are different ways of approaching this problem of unobserved heterogeneity, including specifying a 'random effects' type error term for each individual, either parametrically or non-parametrically. Narendranathan and Stewart (1993) argue, however, that there is no reason to believe the resulting distortion from introducing such specifications for unobserved heterogeneity is any less severe than any distortion that would arise from ignoring the unobserved heterogeneity in the first place. An alternative may be to adopt a flexible specification for the hazard function (Arulampalam and Stewart, 1995). Boheim and Taylor (2000) argue that given a sufficiently flexible specification for the hazard function it becomes unnecessary to model unobserved heterogeneity. Many recent studies adopt a flexible specification for the hazard function based on a model first introduced by Prentice and Gloeckler (1978) (e.g. Arulampalam and Stewart, 1995; Narendranathan and Stewart, 1993; Bratberg and Vaage, 2000; Portugal and Addison, 2000; Boheim and Taylor, 2000; McVicar and Podivinsky, 2001). This is the route we follow here.

The hazard rate (probability of exit, denoted h_{ijt}) for individual i in time interval t to exit unemployment to destination j is given by Equation 1, where $\theta_j(t)$ is a function relating the hazard rate with the duration of the unemployment spell. The PG approach assumes there is a specific parameter that is constant over each period identified, i.e. the function $\theta_j(t)$ is piecewise constant. X_{it} is a set of observed covariates and β_j measures the relationship between each covariate and the hazard rate.

$$h_{ijt}(X_{it}) = 1 - \exp\{ -\exp[X_{it}'\beta_j + \theta_j(t)]\} \qquad (1)$$

Because we identify several destinations after leaving unemployment (employment, education and training or other benefits) we adopt the competing risks Prentice-Gloeckler (PG) model (see, e.g. Boheim and Taylor, 2000). This model treats exits to different destinations as independent, i.e. the probability of exit to one destination is assumed not to depend on the probability of exit to another destination. This allows us to analyse ND18-24 effects on *identified* exits independently of exits to *unidentified* destinations (e.g. failure to sign on).[9]

A key problem in estimating ND18-24 effects is that of constructing a suitable counterfactual. Our methodology follows that of Van Reenen (2001) and relies on the same two critical assumptions: (i) that economic trends affect the 18-24 and 25-29 age groups in the same way and (ii) that there are no substitution effects between age groups as a result of ND18-24. The first assumption can be defended by an examination of unemployment rates over time for the two age groups, which have followed broadly similar paths in NI over the 1990s. Assumption (ii) is more a matter of faith, although no evidence has yet been presented to suggest such inter-age group ND18-24 substitution effects do exist to any significant degree. If such effects do exist, and ND18-24 has led to the younger age group taking jobs that would have been taken by the older age group, then our estimated ND18-24 effects on the 18-24 age group may be biased upwards.[10]

[9] Exits to unidentified destinations include in order of importance (largest first) failure to sign on and destination unknown. We also include the small number of exits because of emigration, claim withdrawal, court/prison and death in this category.

One further assumption (assumption (iii)) is implicit in our methodology, namely that unemployed young people before the introduction of ND18-24 have similar characteristics as unemployed young people after the introduction of ND18-24. We compare observed characteristics for unemployed 18-24 year olds pre and post ND18-24 in the following Section and find very little contrast. Nevertheless, if unemployed young people following the introduction of ND18-24 are in some *unobserved* way less employable than their counterparts before its introduction, we may be *under-estimating* the true effects of ND18-24 on the hazard rate.

4. The Data

Our data are computerised register records for all those unemployed and claiming JSA benefits (claimant count) in NI aged between 16 and 30 on entry to unemployment. All spells on the register since January 1995 are recorded and the last point of observation is July 2001.[11] The data therefore span 335 weeks, and are longitudinal in that we can track individuals who return to the register through time. Estimating the PG model is highly computer-intensive.[12] The analysis is therefore conducted on a random 20% sample of this data, containing 86,965 unemployment spells.

To set up the PG model, we need to specify the duration intervals for which interval-specific parameters giving the shape of the baseline hazard function are to be estimated. Although we can pinpoint the duration of an unemployment spell to the nearest day, some degree of aggregation is necessary for two reasons. First, aggregating the duration intervals significantly cuts down on the computing power needed to estimate the model. Second, the interval-specific hazard parameters can only be identified for those duration intervals in which exits occur, and can be imprecise where only few exits occur (Jenkins, 1997). We group unemployment durations as follows: fortnightly for the first year, five groups of eight weeks followed by one group of twelve weeks for the second year, and one residual group for longer durations. This allows a considerably more disaggregated examination of ND18-24

[10] We also assume no equilibrium wage effects of ND18-24, as supported by Riley and Young (2001b).
[11] Spells starting before January 1995, where coverage of the data is not universal, are excluded.
[12] Several rows of data are required for each spell of unemployment, with the number of rows proportional to the length of the spell.

effects at different durations than has been the case with previous GB studies. The corresponding distribution of spell durations is shown in Figure 2.

We can identify the number of unemployment spells that end in particular types of exits for our sample by age group, gender, and separately pre and post ND18-24. This is shown in Table 1. Perhaps the most striking pattern for males is the increase in the proportion of exits from the *unidentified* category (e.g. failure to sign on) following New Deal. This is true of both age groups but is more dramatic for the target age group of ND18-24. Relative to the older age group, exits to employment, education and training and other benefits appear little changed following the introduction of ND18-24 as proportions of *identified* exits. For females there is no such pattern of increased unidentified exits following ND18-24. Exits to other benefits appear to have increased for the target age group relative to the older age group. As for males, proportions of exits to employment and education and training appear to have changed little following the introduction of ND18-24 relative to the older age group. Of course, Table 1 treats all exits as alike (i.e. regardless of duration) so is unlikely to tell the whole story.

Existing literature – both theoretical and empirical – suggests a set of generally observable factors that may influence unemployment duration and should therefore enter Equation (1). Many of these factors are recorded in the JSA register data and are included in our model as covariates. To this we add a JSA binary dummy variable which takes the value 'one' for all spells or parts of spells from October 1996 onwards (the date of the introduction of JSA) and 'zero' otherwise. We also add a socio-economic indicator – the Multiple Deprivation Index (MDI) score – for the electoral ward in which sample members live (see NISRA, 2001, for more details). Sample means and standard deviations are given in Table 2. Note the (observed) similarity between young people entering the unemployment register post ND18-24 and those on the unemployment register pre ND18-24.[13]

[13] The list of factors observed in the data is not exhaustive. Information on other potentially significant factors, such as qualifications held, is unavailable, and information on benefits received during unemployment spells is unfortunately very patchy in the database.

Those unemployment spells not completed by July 2001 (the last point at which we observe the data) are treated as right-censored. When estimating pre ND18-24 hazard functions, we also treat unemployment spells that start before April 1998 but do not end before April 1998 as right-censored. Such spells account for less than 5% of the total number of spells.

5. Results and Discussion

Meyer (1990) sets out an extension to the PG model that allows for (parametric) unobserved heterogeneity.[14] Given the size of the data set and the nature of the estimation process for such models, however, it is time consuming: it can take between four and eight days for a single estimation to run on a fast PC. PG models with unobserved heterogeneity have therefore not been estimated in all cases, but for a representative subset of the various models (five models in all). In each case where unobserved heterogeneity is specified, it is found to be a highly insignificant addition to the standard PG model. This is a common finding when estimating PG models (see, e.g. Boheim and Taylor, 2000; Carling et al., 1996) and lies behind Boheim and Taylor's assertion that it may be unnecessary to specify unobserved heterogeneity given a sufficiently flexible specification of the baseline hazard function. We report only those models that do not incorporate unobserved heterogeneity.

First consider exits to employment for males. Figure 3 presents hazard functions pre and post ND18-24 for the 18-24 age group, and Figure 4 for the 25-29 age group. The hazard functions are for an 'average' unemployed male in the specified age group (covariates are set at sample means). There is clear negative duration dependence for both age groups, consistent with survey based evidence for young people in NI presented in McVicar and Podivinsky (2001).

Figure 3 is immediately striking – the hazard functions pre and post ND18-24 are almost identical. In other words, for 18-24 year old males, the probability of exit from unemployment into employment at all durations of unemployment is unchanged following the introduction of ND18-24.

[14] Baker and Melino (2000) caution against using the Heckman-Singer non-parametric (mass point) specification for unobserved heterogeneity when duration dependence is not estimated parametrically.

Although there is no apparent difference in the hazard functions pre and post ND18-24 for this group, we must also examine the hazard functions pre and post ND18-24 for the comparison group of 25-29 year old males before we can conclude anything about the actual effects of ND18-24. For this older age group, there is a contrast between the pre and post ND18-24 hazard functions, albeit a small one. Post ND18-24 hazard rates are lower over the first three months or so of unemployment than pre ND18-24 hazard rates. In other words, an average member of our comparison group is less likely to get a job during the first three months of unemployment following the introduction of ND18-24 than previously.

If we accept the assumptions discussed in Section 3 and there is some background factor having a small downward effect on the hazard rate for 25-29 year olds, then the fact that this is not shown for 18-24 year olds in Figure 3 is suggestive of a small positive effect of ND18-24 on the hazard rate for the younger age group. In other words, ND18-24 may have maintained the hazard rate for 18-24 year olds at its previous level despite slightly 'worse' conditions.

It is straightforward to quantify the ND18-24 effect by using the 25-29 age group hazard rates to calculate a counterfactual hazard function for the 18-24 age group had ND18-24 not been introduced. The counterfactual suggests ND18-24 has increased the hazard rate for 18-24 year old males at 1-2 weeks from 0.44 to 0.49, at 3-4 weeks from 0.21 to 0.25, at 13-14 weeks from 0.05 to 0.06, and so on. Beyond 13-14 weeks duration, the hazard rates are little changed by ND18-24. Although these period-by-period effects of ND18-24 on the hazard rate are small, they have a larger cumulative effect on the probability of long-term unemployment. Pre ND18-24, the chance of an 18-24 year old male entrant to unemployment not having found a job three months later is 0.30 compared to a post ND18-24 figure of 0.23. In other words, just from increased chances of employment, ND18-24 has reduced the chances of young males being unemployed for a continuous spell of three months or more by a factor of between 20% and 25%. This ratio persists through to longer durations.

In terms of magnitude these estimates are broadly consistent with those suggested by previous studies for GB (i.e. Van Reenen, 2001; Blundell et al., 2001; Riley and Young, 2001a). That most of the 'action' occurs during the first three months of unemployment, however, is perhaps more surprising. Information from the GB studies on NDYP effects in the first three months of unemployment is somewhat patchy. Van Reenen (2001) and Blundell et al. (2001) do not examine effects during the first six months of unemployment. Riley and Young (2001) do examine effects in 0-3 months and 3-6 months aggregate duration groups, but find a mixture of insignificant, positive and negative NDYP effects depending on the version of their model estimated.[15] A potential explanation for this effect at short unemployment durations is that early entrants to NDYP (e.g. lone parents, those returning directly to Gateway) and those in follow-through are exiting to employment faster than would otherwise have been the case. There may also be 'anticipation' or 'avoidance' effects where behaviour is changed by the expectation of being called for interview.

A related question is why we find no evidence of any significant ND18-24 effect on exits to employment for males after six months unemployment, in contrast to the earlier GB studies. Is there more 'locking in' during Gateway in NI than in GB?[16] Alternatively, is the fact that our model is estimating longer-term ND18-24 effects and not effects on the existing 'stock' of long-term unemployed during the introduction of ND18-24 driving this contrast? In other words, by ignoring 'programme introduction effects' and having a longer run of post-ND18-24 data are we picking up a 'mature programme effect' not found in previous studies? These are intriguing questions for further research.

Table 3 presents the estimated coefficients for the covariates in the PG models corresponding to the hazard functions in Figure 3. Significant negative (positive) coefficients indicate a negative (positive) effect on the hazard rate. Although it is straightforward to see the direction of the marginal effect of each covariate on the hazard rate, it is not necessarily straightforward to calculate their *magnitudes*, especially for binary dummy variables. We derive an approximation of magnitudes by calculating the hazard rate for different values of the covariates around their sample means.

[15] Given Riley and Young (2001a) use three-month duration groups and observe data quarterly, they also fail to fully account for exits from unemployment during the first three months of spells.
[16] Calmfors et al. (2001) presents evidence that 'locking in' can reduce outflows from ALMPs.

First consider the pre ND18-24 coefficients. Older young people have higher hazard rates than younger young people. For example, a 23 year-old is around 30% more likely to get a job within three months after joining the unemployment register than an 18 year-old, other things being equal. Those describing themselves as seeking skilled manual employment have marginally higher hazard rates than others. Number of previous unemployment spells has a small effect – having experienced an additional unemployment spell increases the hazard rate by around 1%. Although we expect those with more history of unemployment to have lower hazard rates, this covariate merely counts the number of spells and does not take into account the length of such spells. Therefore it may be reflecting *fluidity* rather than previous unemployment experience: young people with experience of several previous unemployment spells are likely to be in and out of short unemployment spells with comparative regularity. Young people living in electoral wards with a high MDI (more deprived) score have lower hazard rates for exit to employment. The hazard rate for a young unemployed male from Wallace Park (the least deprived ward) is a factor of 30-40% higher than the hazard rate for a young unemployed male from Crumlin (the most deprived ward), other things being equal.

The introduction of JSA in October 1996 coincides with significantly increased hazard rates for exits to employment for 18-24 year old unemployed males. Because of the nature of the dummy variable (it can change value during an unemployment spell) the interpretation of the estimated coefficient is less straightforward than for the other covariates. It is also likely that calculating hazard rates for the extreme cases where JSA=1 or JSA=0 (note that there are no *non-extreme* cases), using the estimated coefficients presented in Table 3, gives what is likely to be a fairly inaccurate approximation of the magnitude of the JSA effect. Indeed, calculated in this way, the hazard rate appears to be around four times higher post JSA than pre JSA. We interpret this figure with extreme caution. Using survey data, McVicar and Podivinsky (2001) estimated hazard functions to be around a third higher following the introduction of JSA, other things being equal. This is likely to be closer to the true magnitude of the JSA effect on the hazard rate.

The coefficients on the covariates for the post ND18-24 sample are broadly similar to those for the pre ND18-24 sample. There is a difference in the sign of the coefficient for number of previous unemployment spells, however, which in this case is negative, although the magnitude of the effect is still small. Where other coefficients change in size between the pre and post ND18-24 samples, these changes tend to be similarly observed for the 25-29 age group.

Now consider exits to employment for females. Figure 5 presents hazard functions pre and post ND18-24 for the 18-24 age group, and Figure 6 for the 25-29 age group. As for males all hazard functions indicate negative duration dependence.

Figure 5 shows a considerable contrast between the pre and post ND18-24 hazard functions for exits to employment for the female 18-24 age group. Following ND18-24, hazard rates are higher by a factor of around 25% at all but a few longer unemployment durations. The corresponding pattern for the comparison group, is one of very similar hazard functions pre and post ND18-24 – there is just a slight increase in the hazard rate post ND18-24 for some durations. At first glance then, if we accept the assumptions set out in Section 3, ND18-24 appears to have had a significant positive effect on increasing exits to employment for females in the 18-24 age group, once again particularly at shorter durations.

To quantify this ND18-24 effect we calculate the counterfactual as before. The chances of an 'average' female in the 18-24 age group not getting a job within the first three months of unemployment are 0.15 post ND18-24 compared to 0.27 under the counterfactual. Similar figures for six months and twelve months respectively are 0.13 and 0.08 post ND18-24 compared to 0.20 and 0.15 under the counterfactual. This corresponds to a ND18-24 effect that almost halves the chances of females in the 18-24 age group experiencing long term unemployment, just through increased exits to employment.

That females may benefit more from ALMPs than males is not inconsistent with conventional wisdom (see, e.g. Van Reenen, 2001). It may be that unemployed female 18-24 year olds are more 'employable' than unemployed male 18-24 year olds, for whatever reason, and therefore more likely to benefit from policy intervention. Observed characteristics in the data lend some support to this argument. For example, females are more likely to be seeking managerial, professional or related employment, live in less deprived areas on average and have less previous experience of unemployment than their male counterparts. Another explanation may lie in the increased proportion of male exits from the register post ND18-24 that are unidentified, i.e. failure to sign on or unknown destination. It is likely that some of these exits are to employment. We cannot therefore rule out the possibility that a stronger ND18-24 effect for males lies hidden in this category, although these unidentified exits begin to increase earlier than the introduction of ND18-24 (around the introduction of JSA). Survey data will be needed to examine this issue in more depth.

Again, there is little in the way of existing evidence for comparative purposes. Van Reenen (2001) does not consider national NDYP effects for females, and comes up against the problem of small sample sizes in his analysis by gender in pilot areas. Blundell et al. (2001) also do not obtain satisfactory estimates of female effects, partly due to differential trends between the target and comparison female groups in GB over their sample period. Unemployment rates for both age groups in NI have followed broadly similar downward trends over the period of our study, however, and our sample size is large. Riley and Young (2001a) do not report estimates separately by gender.

Table 3 presents the estimated coefficients for the covariates corresponding to the hazard functions in Figure 5. The effects of the covariates on the hazard rate pre ND18-24 are broadly consistent across both genders. Two differences stand out, however. Females describing themselves as seeking managerial, professional or related employment have higher hazard rates than those seeking other types of work. Two contrasting effects are likely to be at work here – unemployed females seeking higher status employment are likely to wait longer before accepting job offers and those seeking higher status employment are likely to be better qualified than those seeking other types of work. The positive sign on the coefficient for this covariate suggests the latter effect outweighs the former, i.e. that ‘seeking managerial work’ is primarily acting as a proxy for qualification level for females. The second contrast between the genders is in the effects of the ‘living with partner’ covariate. For males this is insignificant, but for females strongly significant and negative. Three months after entering unemployment, around 31% of females in this group living with a partner will not have found work, compared to around 22% of those not living with a partner.

As for males, the covariates appear to have broadly similar effects on the hazard rate pre and post ND18-24. There are three cases where coefficients change in size (not matched by similar changes for the 25-29 age group). First, the negative effects of MDI appear to be stronger post ND18-24 than pre ND18-24, suggesting ND18-24 may be acting to increase the relative advantage of females living in less deprived areas. Second, the negative effects on the hazard rate of living with a partner are stronger post ND18-24, which might suggest ND18-24 has had slightly more impact on single females. Finally, post ND18-24 there is no significant effect of seeking managerial, professional or related employment – perhaps the relative advantage of higher qualifications is diminished, or the relative wait for a high status job has lengthened.

Because exits to education and training and exits to other benefits are less common than exits to employment (see Table 2), we estimate hazard functions for both genders together. Figures 7 and 8 give pre and post ND18-24 hazard functions for exits to education and training.

Figure 7 shows an interesting contrast between the pre and post ND18-24 hazard functions – the post ND18-24 hazard function has pivoted giving lower hazard rates in early months of unemployment and higher hazard rates in later months of unemployment. Figure 8 shows no such pattern for the comparison group of 25-29 year olds. In fact, Figure 8 shows a lower hazard rate at all durations post ND18-24 than pre ND18-24. At first glance therefore, there appears to be a positive ND18-24 effect on the hazard rate for exits to education and training, at least for those individuals that have been unemployed for longer durations.

To quantify the apparent ND18-24 effect we again use the 25-29 age group to calculate the counterfactual. Over the first three months of unemployment the actual post ND18-24 hazard function for 18-24 year olds is sometimes above and sometimes below the counterfactual indicating little overall effect of ND18-24. From three months on, however, the actual post ND18-24 hazard function begins to draw away from the counterfactual, being on average between twice and three times higher. The gap increases further as unemployment duration increases, particularly beyond nine months duration. Although the estimated effects of ND18-24 on the hazard function are very strong, it must be remembered that the actual probabilities of exits to education and training in all but the first few months of unemployment are very small.

Table 4 presents the estimated coefficients for the covariates corresponding to the hazard functions in Figure 7. First consider the coefficients for the pre ND18-24 model. Age on entry to unemployment is (unsurprisingly) negatively related to the hazard rate for exits to education and training. An unemployed 23 year-old is approximately half as likely to enter education and training as an unemployed 18 year-old at all unemployment durations, for example. Those who describe themselves as seeking managerial, professional or related employment have higher hazard rates for exits to education and training. Those seeking skilled manual employment are less likely to exit unemployment to education and training. Experience of previous unemployment spells reduces the hazard rate for exits to education and training, perhaps reflecting a labour market entry effect, where those that have been in the labour market for some time are the least likely to return to education or training. The JSA dummy is again positive and significant – hazard rates for exits to education and training are higher following the introduction of JSA by a factor of around 80%.

The relationship between age, seeking skilled manual employment and living with a partner and the hazard rate are of similar sign post ND18-24. The age effect has weakened, however, following the introduction of ND18-24 (this is not shared by the 25-29 comparison group), suggesting ND18-24 has increased exits to education and training for the older end of the 18-24 age range to a greater extent than the younger end of the age range (perhaps because of the FTET option on ND18-24). The effect of seeking managerial, professional or related employment is also noticeably weaker post ND18-24, which may suggest those with less qualifications have been encouraged to enter education and training by ND18-24 to a greater extent than those with good qualifications. Other contrasts in estimated marginal effects are shared by those for the comparison group.

Figures 9 and 10 give pre and post ND18-24 hazard functions for exits to other benefits. Figure 9 shows a post ND18-24 hazard function everywhere higher than that for pre ND18-24. Figure 10 shows a similar pattern, although not as strong. At first glance, therefore, it appears ND18-24 has increased exits to other benefits for the 18-24 age group. To quantify the ND18-24 effect, we again calculate the counterfactual. Over the first six months of unemployment, there is very little overall ND18-24 effect on the hazard rate. From six months on, however, the actual post ND18-24 hazard rate for 18-24 year olds begins to move away from the counterfactual, being on average higher by a factor of 50-60%. The suggestion here is that on eligibility for ND18-24 after six months of unemployment, an increased number of young people in the 18-24 age group move off JSA benefits onto alternative forms of benefit for which participation in ND18-24 is not compulsory. The effect is clear, but it must be borne in mind that the actual probabilities of exit to other benefits are small both pre and post ND18-24.

To the best of our knowledge, this study is the first to estimate ND18-24 effects specifically on outflows to education and training and to other benefits. Riley and Young (2001a), however, do find increased exits to a catchall category of 'non-employment' following the introduction of NDYP. In this respect, our results for NI are broadly consistent with theirs for GB.

Table 4 presents the estimated coefficients for the covariates corresponding to the hazard functions in Figure 9. First consider the coefficients for the pre ND18-24 model. Age on entry to unemployment is negatively related to the hazard rate for exits to other benefits for 18-24 year olds, other things being equal. An 18-24 year old living with a partner has a hazard function for exits to other benefits around twice as high as that of a similar young person not living with a partner. This is likely to be a reflection of the nature of the benefit system and how cohabiting or married jobless are treated with regards to unemployment benefits. There is an interesting gender effect shown by Table 4 – males are less likely than females to exit to other benefits. The dummy for seeking managerial, professional or related employment has a strong negative effect on the hazard rate for exits to other benefits. The MDI score of the ward in which an unemployed young person lives is positively related to the hazard rate, so young people from more deprived areas are more likely to exit unemployment to other benefits. Hazard rates for a resident of Crumlin (most deprived ward) are on average two and a half times those for a resident of Wallace Park (least deprived ward), other things being equal.

The JSA dummy again has a large positive coefficient suggesting hazard rates for exits to other benefits are considerably higher following the introduction of JSA. The argument that JSA may have led to jobless people previously counted as unemployed moving on to alternative benefits (and therefore off the unemployment register) is supported, even for this young age group, by our evidence (see, e.g. Beatty and Fothergill, 1999).

The estimated coefficients for the post ND18-24 sample are broadly similar to those for the pre ND18-24 sample. Where there are differences between pre and post ND18-24 coefficients, they are generally also reflected in the 25-29 age group pre and post ND18-24 models.

6. Summary and Conclusions

Following the introduction of ND18-24 in NI in April 1998, hazard rates for exits to *identified destinations* (employment, education and training and other benefits) have increased for unemployed 18-24 year olds, in some cases substantially, relative to those for the best available comparison group, i.e. unemployed 25-29 year olds. This suggests ND18-24 itself has caused much of the increase.

We find a clear gender contrast in ND18-24 effects on exits to employment. ND18-24 has increased the hazard rate for young males by considerably less than the corresponding increase for females. Van Reenen (2001) remarks that active labour market policy is often thought ineffective for males, although his study concludes that this is not true in the case of NDYP. Neither is it true in our study, although it does appear to be *less* effective for males than for females.

Neither have the effects of ND18-24 been uniform across different durations of unemployment. Previous studies for GB have either not examined NDYP effects at shorter unemployment durations, or have come up against data constraints in trying to do so. We find much of the increase in the hazard rate for exits to employment occurs over the early months of unemployment, perhaps because of early entrants or anticipation or avoidance effects before individuals are called for ND18-24 interviews. The increase in the hazard rate for exits to education and training occurs at longer durations, perhaps related partly to young people taking up the FTET option on ND18-24, again in some cases as early entrants. The hazard rate for exits to other benefits has increased, as a result of ND18-24, by a factor of 50% at unemployment durations beyond six months. This appears to be a direct result of entry or anticipated entry to ND18-24. To the best of our knowledge, ours is the first study of ND18-24 to disaggregate non-employment exits.

As a result of increased exits to employment, education and training and other benefits, the chance of an average unemployed 18-24 year old male remaining in unemployment for a continuous three month spell following ND18-24 is lower by a factor of around 20%. The chances of the same male remaining in unemployment for a continuous six (twelve) month spell are 20-25% (25-30%) lower following ND18-24. Before the introduction of ND18-24, around *one in nine* 18-24 year old males entering unemployment would not have found a job, an education or training place or moved on to other benefits twelve months later. After the introduction of ND18-24, the corresponding figure is under *one in twelve.*

The chance of an average unemployed 18-24 year old female remaining in unemployment for a continuous three month spell following ND18-24 is lower by a factor of around 40%. The chances of the same female remaining in unemployment for a continuous six (twelve) month spell are 40-45% (45-50%) lower following ND18-24. Before the introduction of ND18-24, around *one in eleven* 18-24 year old females entering unemployment would not have found a job, an education or training place or moved on to other benefits 12 months later. After the introduction of ND18-24, the corresponding figure is around *one in twenty.*

Clearly ND18-24 has had a significant effect to reduce the chances of young people experiencing long-term unemployment in NI. But why are these effects not stronger? If ND18-24 was being implemented according to the ‘rules’ there should be almost no young people in the target age group that experience unemployment durations of a year or more. In other words, the introduction of ND18-24 should have reduced the chances of year-long unemployment spells by close to 100% rather than 25-50%. Increased unidentified exits, some of which may be to employment, for males following the introduction of ND18-24 may provide a partial explanation. It is also possible, however, that some of these exits represent young people that stop signing on to avoid ND18-24 interviews. Survey data, or joined up administrative data across government departments will help answer this question. Given that no such increase in unidentified exits occurs for females, however, this is unlikely to provide the full explanation. Two other factors may contribute to why these figures are not closer to 100%. Firstly, ND18-24 in NI has *not* been implemented strictly according to the guidelines. In particular, Gateway often extends beyond four months. Secondly, a small but significant number of long-term unemployed young people in the target group have still not been invited for ND18-24 interview, for whatever reason (McVicar and Podivinsky, 2002). Policy makers need to address these issues as a matter of priority if ND18-24 is to have its maximum potential effect in NI.

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Fig 1: ND18-24 Timeline

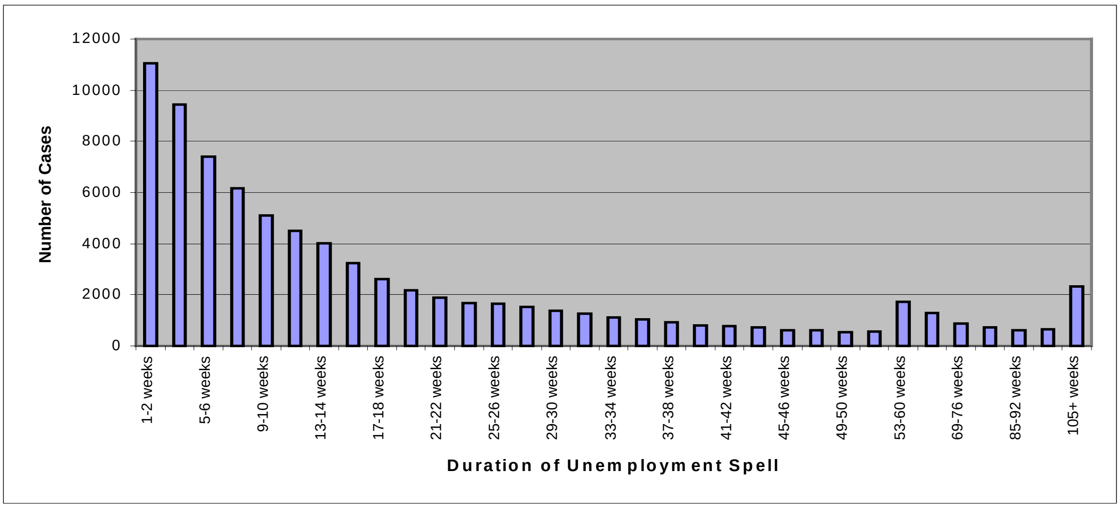
Unemployed - usually for six months → Gateway – usually up to four months → New Deal Options – up to 12 months:

- Subsidised employment
- Education and training
- Voluntary work
- Environmental Taskforce

→ Follow-through – usually up to three months

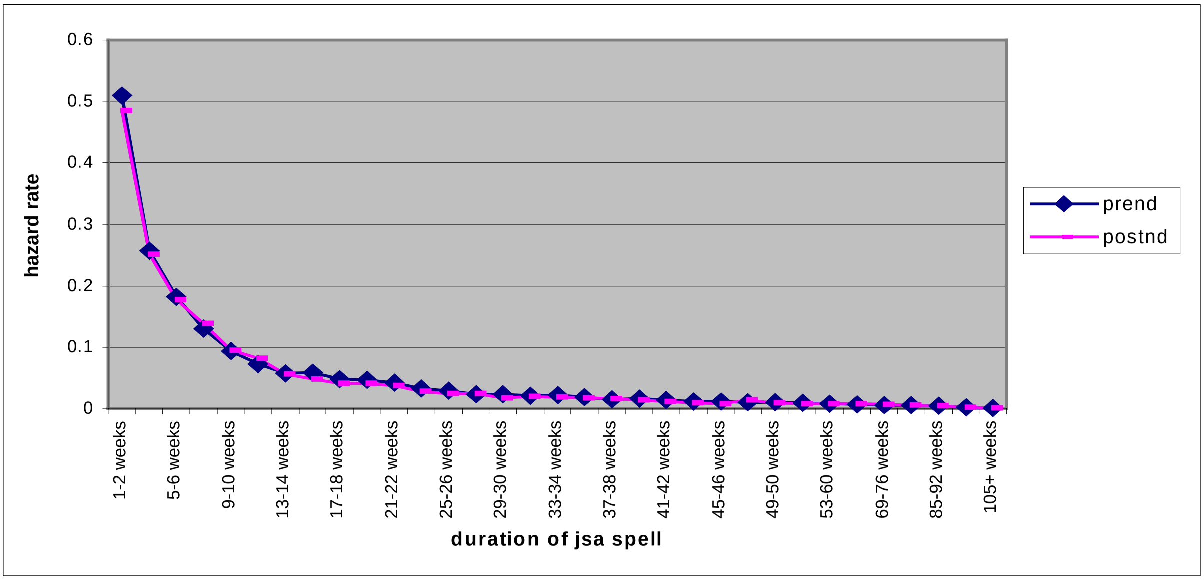
Source: McVicar and Podivinsky (2002).

Fig 2: Frequency Distribution of Spell Duration



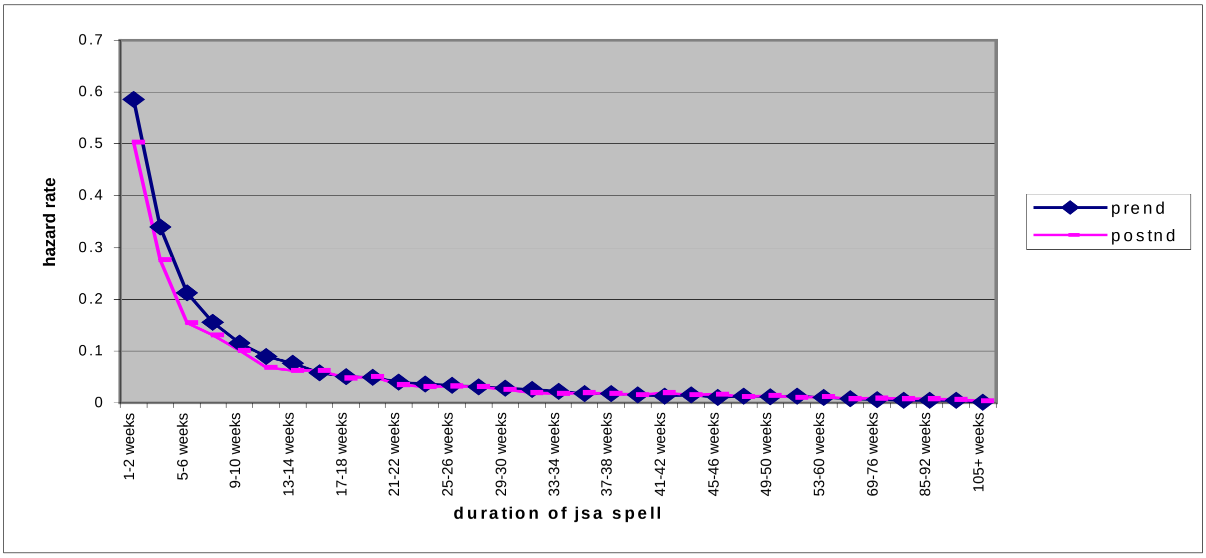
Source: McVicar and Podivinsky (2002).

Fig 3: Hazard Functions for Exits to Employment, Males, 18-24, Pre/Post ND18-24



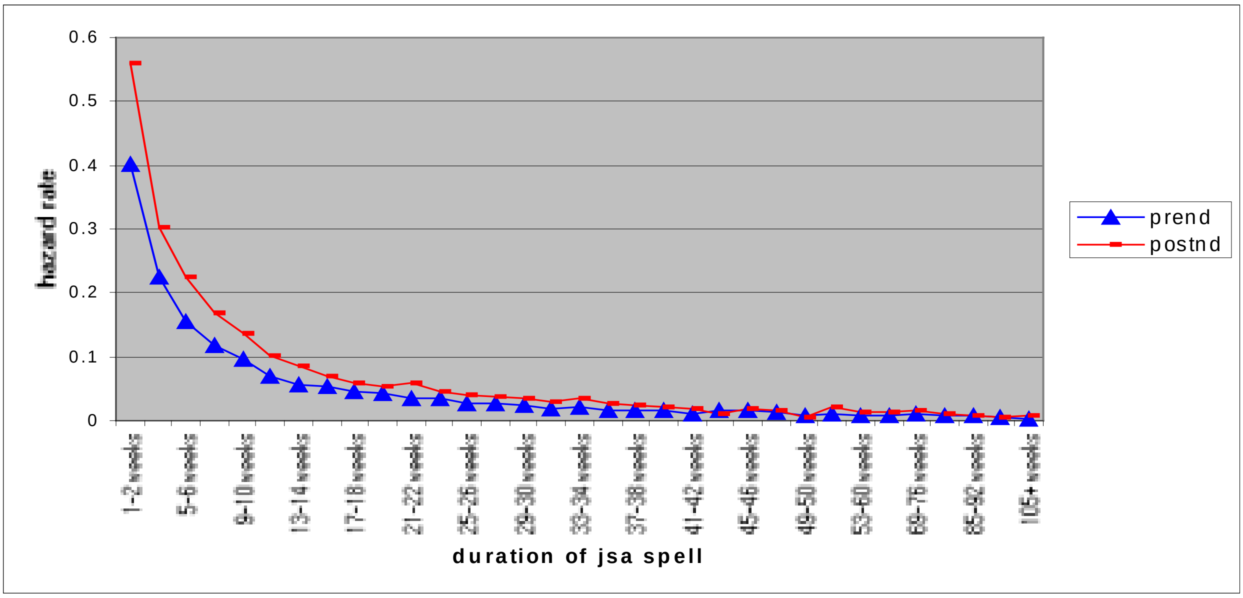
Source: McVicar and Podivinsky (2002).

Fig 4: Hazard Functions for Exits to Employment, Males, 25-29, Pre/Post ND18-24



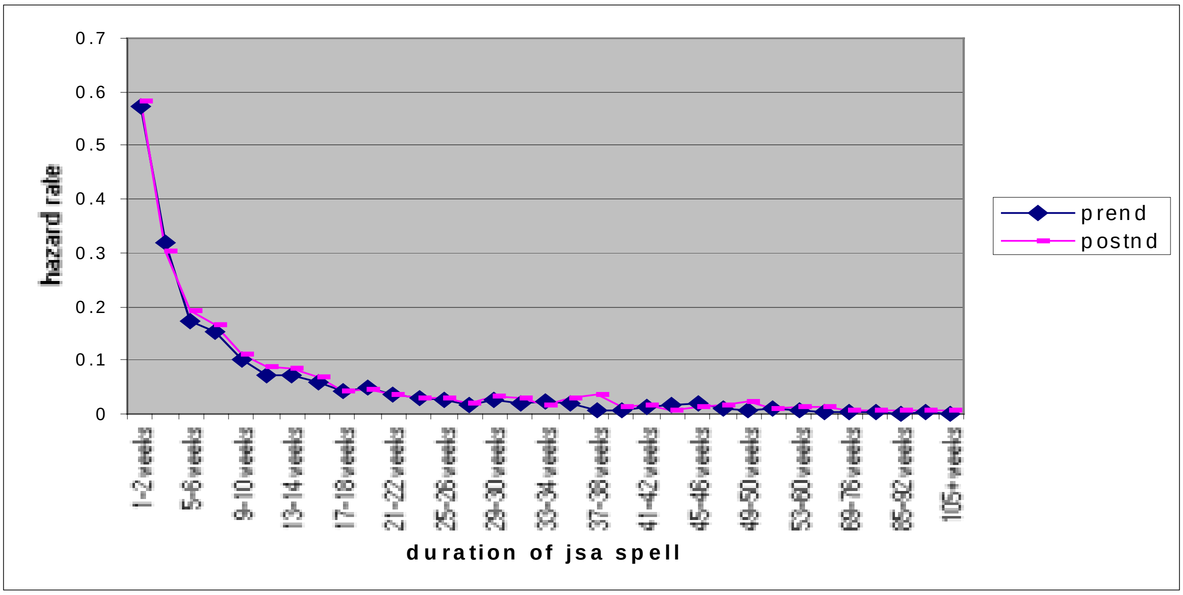
Source: McVicar and Podivinsky (2002).

Fig 5: Hazard Functions for Exits to Employment, Females, 18-24, Pre/Post ND18-24



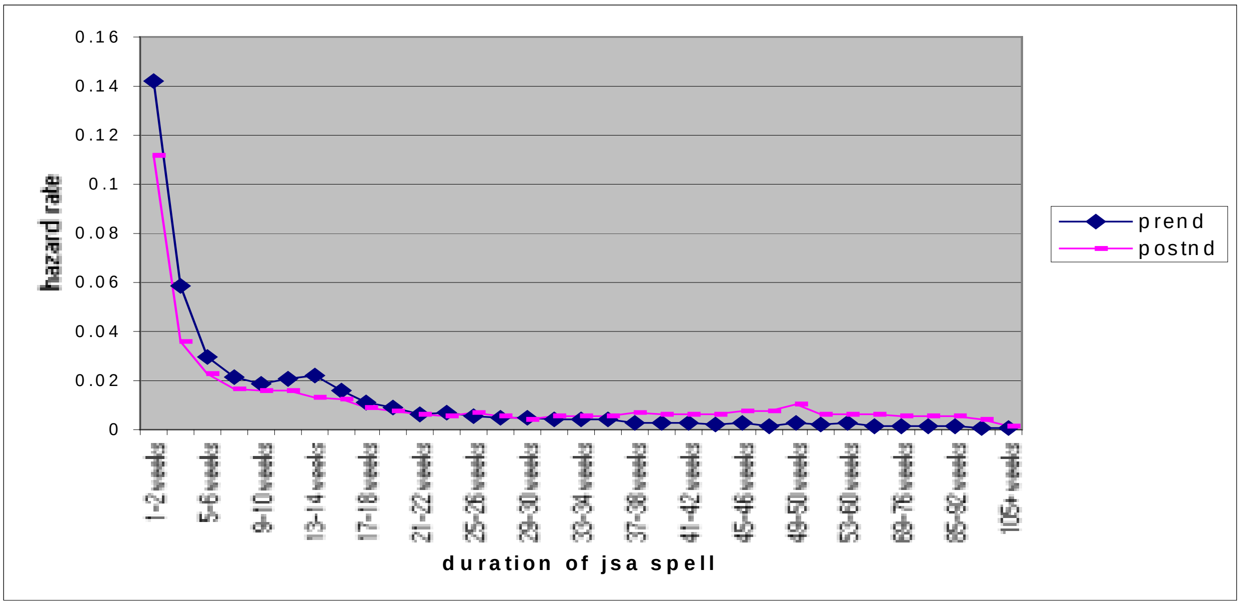
Source: McVicar and Podivinsky (2002).

Fig 6: Hazard Functions for Exits to Employment, Females, 25-29, Pre/Post ND18-24



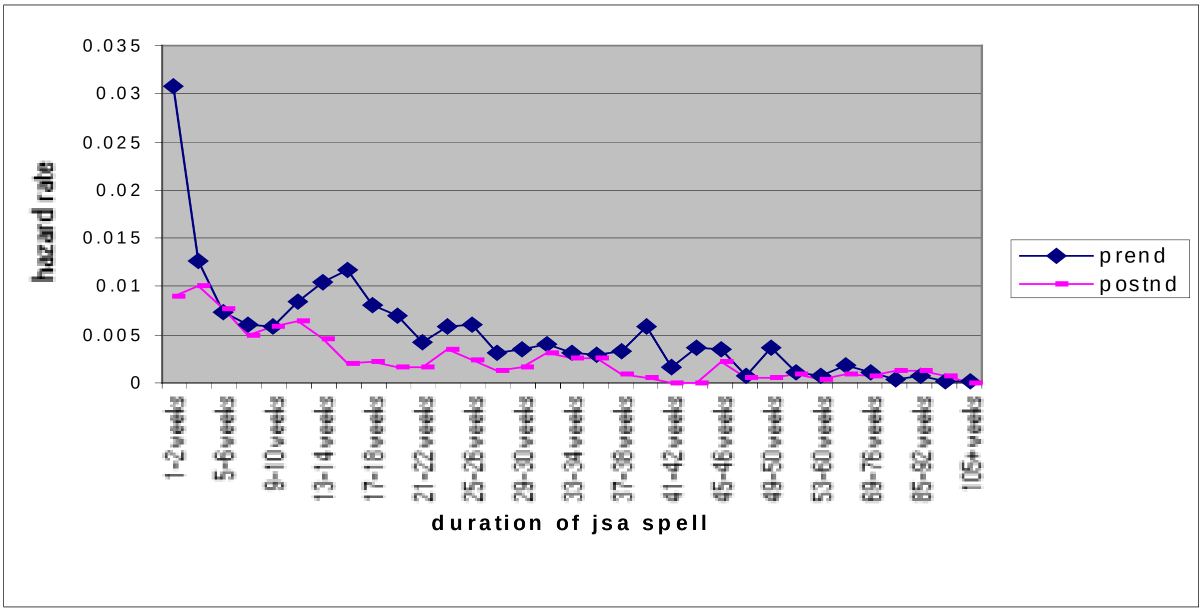
Source: McVicar and Podivinsky (2002).

Fig 7: Hazard Functions for Exits to Education and Training, Males and Females, 18-24, Pre/Post ND18-24



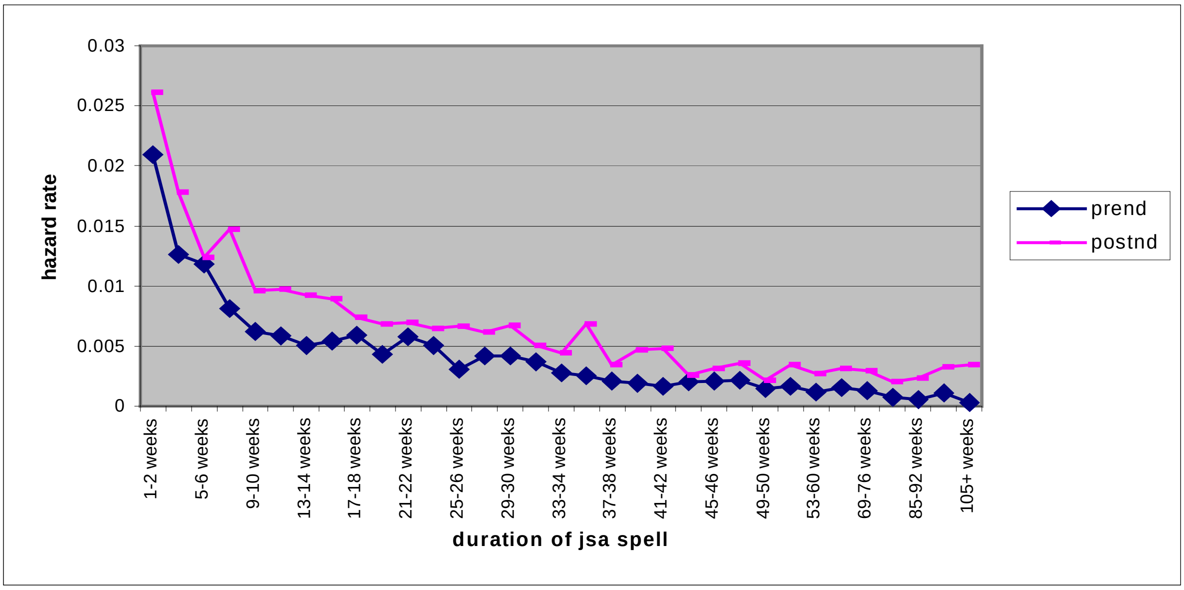
Source: McVicar and Podivinsky (2002).

Fig 8: Hazard Functions for Exits to Education and Training, Males and Females, 25-29, Pre/Post ND18-24



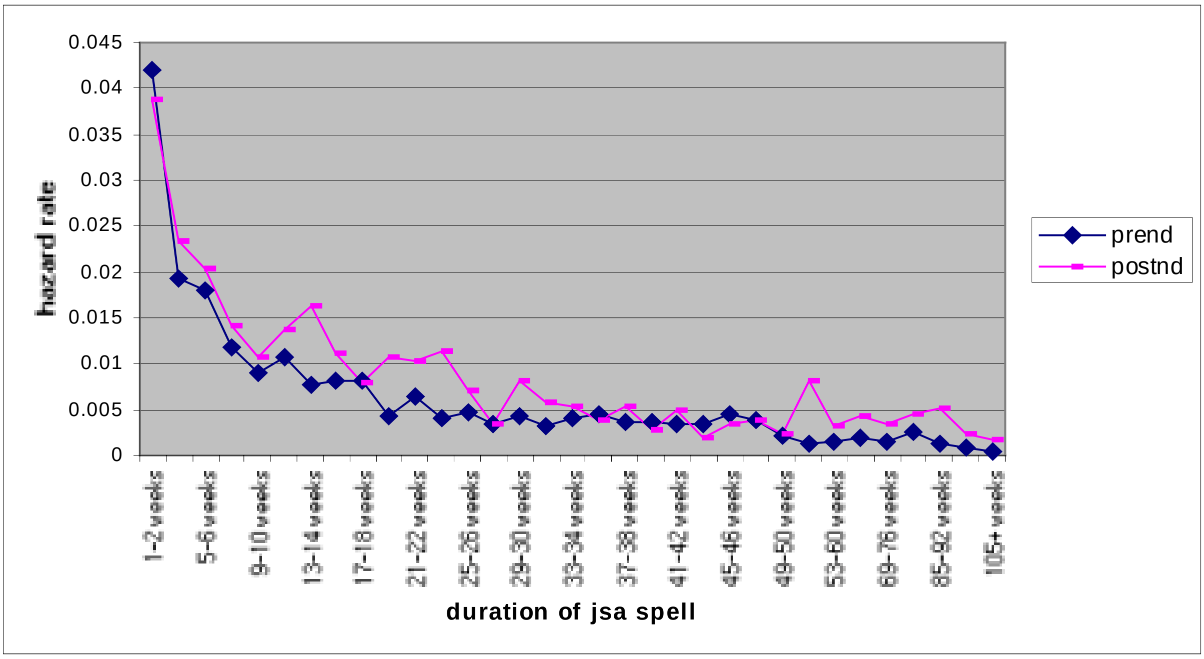
Source: McVicar and Podivinsky (2002).

Fig 9: Hazard Functions for Exits to Other Benefits, Males and Females, 18-24, Pre/Post ND18-24



Source: McVicar and Podivinsky (2002).

Fig 10: Hazard Functions for Exits to Other Benefits, Males and Females, 25-29, Pre/Post ND18-24



Source: McVicar and Podivinsky (2002).

Table 1: Number of Unemployment Spells Ending in Exits to Particular Destinations, by Age Group, Gender, Pre and Post ND18-24

	Full Sample	Male 18-24 Pre ND18-24	Male 18-24 Post ND18-24	Male 25-29 Pre ND18-24	Male 25-29 post ND18-24	Female 18-24 Pre ND18-24	Female 18-24 Post ND18-24	Female 25-29 Pre ND18-24	Female 25-29 Post ND18-24
Employment	40,756 (47%)	6,589 (50%)	5,363 (44%)	3,267 (57%)	2,533 (52%)	4,509 (49%)	3,753 (49%)	1,604 (54%)	1,447 (58%)
Education/ Training	7,893 (9%)	1,846 (14%)	1,339 (11%)	339 (6%)	182 (4%)	1,516 (16%)	947 (12%)	146 (5%)	89 (4%)
Other benefits	9,504 (11%)	542 (4%)	664 (5%)	379 (7%)	445 (9%)	545 (6%)	551 (7%)	265 (9%)	166 (7%)
Total identified exits	58,153 (67%)	8,977 (68%)	7,366 (60%)	3,985 (70%)	3,160 (65%)	6,570 (71%)	5,251 (69%)	2,015 (68%)	1,702 (68%)
Total unidentified exits	28,812 (33%)	4,145 (32%)	4,850 (40%)	1,716 (30%)	1,728 (35%)	2,668 (29%)	2,385 (31%)	936 (32%)	808 (32%)

Source: DETI. Note: Figures in parentheses give proportion of total exits for each group.

Table 2: Covariate Sample Means and Standard Deviations

	Full Sample	Males 18-24 pre ND18-24	Males 18-24 Post ND18-24	Males 25-29 Pre ND18-24	Males 25-29 Post ND18-24	Females 18-24 Pre ND18-24	Females 18-24 Post ND18-24	Females 25-29 Pre ND18-24	Females 25-29 Post ND18-24
Age on entry	22.3 (3.26)	20.6 (1.91)	20.5 (1.89)	26.8 (1.42)	26.8 (1.41)	20.5 (1.92)	20.5 (1.91)	26.7 (1.41)	26.7 (1.41)
Manag/Prof job sought	.175 (.380)	.133 (.340)	.123 (.328)	.161 (.368)	.167 (.373)	.228 (.420)	.198 (.398)	.342 (.475)	.362 (.481)
Skilled man job sought	.136 (.343)	.203 (.402)	.171 (.377)	.236 (.425)	.203 (.403)	.018 (.133)	.010 (.100)	.027 (.163)	.021 (.144)
No. of spells of unemp	2.21 (1.81)	2.00 (1.36)	2.83 (2.17)	2.24 (1.57)	3.93 (2.72)	1.67 (.996)	2.07 (1.43)	2.10 (1.49)	3.37 (2.61)
Live with partner	.077 (.266)	.020 (.141)	.015 (.120)	.172 (.378)	.120 (.325)	.042 (.200)	.027 (.163)	.331 (.471)	.257 (.437)
MDI	25.7 (17.6)	25.8 (17.8)	27.6 (18.7)	26.2 (17.5)	26.6 (17.7)	23.5 (16.6)	24.5 (16.5)	21.7 (15.6)	21.7 (15.3)
No. of spells^2	8.17 (19.8)	5.84 (11.9)	12.72 (26.6)	7.47 (16.1)	22.9 (39.4)	3.77 (5.99)	6.33 (11.0)	6.62 (11.2)	18.2 (32.4)
No. of obs.	86,965	13,122	12,216	5,701	4,888	9,238	7,636	2,951	2,510

Source: McVicar and Podivinsky (2002).

Table 3: Estimated Effects of Covariates on Hazard Rate, Pre and Post ND18-24, 18-24 Age Group, Exits to Employment

	Coefficient Pre ND18-24 (Males)	Coefficient Post ND18-24 (Males)	Coefficient Pre ND18-24 (Females)	Coefficient Post ND18-24 (Females)
Age on entry	.048* (6.76)	.038* (4.53)	.062* (7.00)	.065* (6.24)
Managerial job sought	-.037 (-.94)	.015 (.36)	.084* (2.23)	-.013 (-.31)
Skilled manual job sought	.125* (4.03)	.052 (1.40)	.030 (.26)	-.171 (-.95)
No. of unemployed spells	.051* (2.26)	-.042* (-2.76)	.037 (1.00)	-.019 (-.62)
Live with partner	.063 (.75)	-.028 (-.24)	-.255* (-3.05)	-.383* (-3.24)
MDI	-.006* (-7.51)	-.005* (-6.39)	-.003* (-3.44)	-.007* (-6.53)
Number of spells^2	-.005 (-1.81)	.001 (.84)	0.00 (.07)	.002 (.55)
JSA	2.14* (48.1)		2.08* (37.61)	
Time trend	-.114* (-66.6)	-.019* (-15.2)	-.108* (-52.44)	-.019* (-12.74)
Constant	-1.03* (-6.59)	-1.11* (-5.62)	-1.26* (-6.56)	-1.49* (-6.18)
Log likelihood	-21,252	-17,493	-13,791	-10,930
χ^2 statistic	17,601#	6,784#	11,073#	4,946#

Source: McVicar and Podivinsky (2002). Notes: The χ^2 statistic is a test of the explanatory power of the model compared to an intercept-only model. Rejection of the intercept-only model is denoted by #. Covariates that have statistically significant effects on the hazard rate at the 5% level are denoted *. T-ratios are given in parentheses. JSA is dropped in the post ND18-24 sample since it takes the value one at all times. We do not report estimates of $\theta_j(t)$ due to space constraints.

Table 4: Estimated Effects of Covariates on Hazard Rate, Pre and Post ND18-24, Males and Females, 18-24, Exits to Education and Training and Other Benefits

	Exit to Ed/Training		**Exit to Other Benefits**	
	Coefficient Pre ND18-24	Coefficient Post ND18-24	Coefficient Pre ND18-24	Coefficient Post ND18-24
Age on entry	-.169* (-16.1)	-.091* (-6.59)	-.042* (-2.37)	-.063* (-3.35)
Managerial job sought	.200* (4.01)	-.011 (-.17)	-.960* (-7.23)	-.803* (-6.26)
Skilled manual job sought	-.399* (-6.07)	-.267* (-3.35)	.157 (1.70)	.025 (.37)
No. of unemployed spells	-.235* (-4.90)	-.033 (-1.16)	.197* (2.99)	.181* (4.36)
Male	-.024 (-.65)	-.095* (-2.08)	-.648* (-9.74)	-.536* (-8.50)
Live with partner	-.879* (-4.90)	-.795* (-3.33)	.821* (6.19)	.457* (2.54)
MDI	0.00 (-.40)	.004* (3.42)	.014* (8.90)	.016* (10.5)
Number of spells^2	.004 (.53)	0.00 (.17)	-.014 (-1.52)	-.010* (-2.40)
JSA	.635* (10.07)		2.85* (26.1)	
Time trend	-.070* (-33.53)	-.019* (-9.66)	-.142* (-30.54)	-.013* (-4.84)
Constant	3.20* (14.92)	1.50* (5.12)	-2.01* (-5.21)	-2.062* (-4.82)
Log likelihood	-14,201	-10,250	-6,138	-6,667
χ^2 statistic	9,510#	2,735#	2,362#	658#

Source: McVicar and Podivinsky (2002). Notes: The χ^2 statistic is a test of the explanatory power of the model compared to an intercept-only model (i.e. not including any covariates). Rejection of the intercept-only model is denoted by #. Covariates that have statistically significant effects on the hazard rate at the 5% level are denoted *. T-ratios are given in parentheses. JSA is dropped in the post ND18-24 sample since it takes the value one at all times. We do not report estimates of $\theta_j(t)$ due to space constraints.