Does the Impact of Welfare to Work Depend on the State of the Labour Market? The Case of the UK New Deal for Young People

Duncan McVicar

School of Management and Economics

Queen's University Belfast

University Road

Belfast BT7 1NN

d.mcvicar@qub.ac.uk

Tel: 44 (0)28 9097 3297

and

Jan M. Podivinsky

Economics Division

University of Southampton

Southampton SO17 1BJ

J.M.Podivinsky@soton.ac.uk

Tel: 44 (0)23 8059 2542

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Abstract

There is much debate, but surprisingly little evidence, concerning the impact of primarily supply side Welfare to Work programmes in labour markets characterised by weak labour demand. The usual argument is that we might expect Welfare to Work measures to have greater impacts in tight labour markets than in slack ones because more (and perhaps better) job vacancies exist. On the other hand, the added value of such programmes may be lower in tighter labour markets. There may also be heterogeneous programme impacts if the characteristics of the unemployed differ across labour markets. In this paper we explore whether a compulsory Welfare to Work programme for unemployed young people introduced in 1998 – the UK New Deal for Young People – has had differential impacts on the probability of unemployment exits in different local labour markets. Our results show this to be the case, with the programme impact on the hazard rate for exits from unemployment increasing with the local unemployment rate. Disaggregating exits by destinations, however, shows that there exists a negative (positive) relationship between local unemployment rates and the size of the programme impact on the probability of exit to employment (inactivity).

1. Introduction

The starting point for this paper is the view – let's call it the supply side view – that long term unemployment results from a lack (or deterioration) of human capital, reservation wages that are too high e.g. as a result of welfare benefits, and/or insufficient job search on the part of the unemployed (e.g. Layard et al., 1991; Phelps, 1972; Blanchard and Diamond, 1994). In response to this policy makers in many countries have introduced Welfare to Work (WTW) programmes usually consisting of the introduction or reform of some or all of the following measures: job search assistance, job search monitoring, education and/or training placements, subsidised employment placements in the private sector, work placements in the public or voluntary sectors and benefits sanctions for non-compliance (see e.g. Martin, 2000; Blank, 2002; Blundell, 2001; Carling and Richardson, 2004).[1]

There is an argument, however, that WTW measures might have greater impacts where (or when) labour markets are tight than where they are slack because more and/or better vacancies exist (e.g. Turok and Webster, 1998; Bloom et al., 2001; Sunley et al., 2005).[2] Although this argument is not normally made in the context of any particular model, it can be shown to be consistent with relatively simple job search models. In what follows we will concentrate on the impact of WTW on the reduced form hazard rate for exiting unemployment, generally written as $\theta = \lambda(1 - F(w^*))$, where λ is the job offer arrival rate, F is the wage offer cumulative distribution function and w^* is the reservation wage. For example, Cahuc

[1] Here we will use the term WTW to include Active Labour Market Programmes (ALMPs) and restrict our attention to progammes aimed at the unemployed.
[2] Proponents of this argument in the UK have recently called for stronger demand side policies, e.g. capital grants to firms in lagging areas, alongside WTW programmes (e.g. Turok and Webster, 1998; Sunley et al., 2005).

and Zylberberg (2004, p122-123) present a model where the job offer arrival rate is the product of a parameter denoting the state of the labour market and a concave function of (endogenous) search effort.[3] For a given reservation wage, a WTW programme increasing search effort, e.g. because of a minimum search requirement, would have an impact on the hazard rate proportional to the state of the labour market.[4] If the parameter for the state of the labour market entered the model with power greater than one, then the WTW impact on the hazard could increase more than proportionally with the state of the labour market.

A more attractive explanation is possible if, like Manning (2005), we allow unemployed workers to cease claiming unemployment benefits as a response to a strict WTW search effort requirement. In Manning's model there is a point at which the search effort requirement becomes so high that unemployed workers are better off by not meeting the requirement, avoiding the associated search costs, and sacrificing their unemployment benefits. These unemployed workers continue to search at a new optimal level, higher than the previous level, but lower than the WTW required threshold. Intuitively, WTW will also increase the attractiveness of other alternative states, e.g. inactivity or education, in the same way. Given that the marginal return to search is increasing in the tightness of the labour market (e.g. see Cahuc and Zylberberg, 2004, pp123), exiting registered unemployment to such destinations will be more likely in slack labour markets than in tight labour markets because the unemployed are forced further from their desired level of search effort. In other words, a WTW programme with strict search requirements might increase the hazard

[3] The model is a simplified version of Mortensen's (1986) model with endogenous search effort but no on the job search. Gorter and Kalb (1996) present a similar model.

[4] In Cahuc and Zylberberg's model, $\lambda = \alpha\lambda(e)$ where $\alpha > 1$ denotes the state of the labour market. Of course the WTW programme might also affect the reservation wage with ambiguous sign depending on the sign of the marginal return to search effort.

rate for exits from registered unemployment to inactivity, or more generally to destinations *other than employment*, to a greater degree in slack labour markets. Because the proportion of exits that are to each destination must sum to one, there is a negative dependence between the state of the labour market and the impact of the WTW programme on the hazard rate for exits to employment. We think of this as a discouraged worker effect of WTW.

An alternative argument suggests the relationship between labour demand and the impact of WTW might be negative because the added value of WTW programmes will be smaller in tight labour markets due to higher deadweight loss (e.g. Gueron and Pauly, 1991). In the context of Cahuc and Zylberberg's (2004) search model, a WTW programme imposing a minimum search requirement might have a greater impact in slack labour markets because the optimal level of search effort is lower than in tight labour markets, i.e. the increase in search effort associated with a minimum search requirement will be smaller the tighter the labour market.[5] In short, it is relatively straightforward to think of models where there might be demand side implications for the impact of WTW programmes, which, when taken together, are of uncertain sign. This, coupled with its direct policy relevance, makes the issue of such differential impacts of WTW programmes an interesting one from an empirical perspective.

Surprisingly, however, there is little existing empirical evidence on the possible dependence of WTW impacts on the prevailing labour market conditions. Review papers (e.g. Heckman et al., 1999; Martin, 2000; Bloom and Micholopoulos, 2001;

[5] Another argument – although of less interest empirically where individual heterogeneity is controlled for – is that those remaining (long term) unemployed in tight labour markets may be characterised by severe employability problems and therefore harder to place in employment than the long term unemployed in other labour markets (Bloom et al., 2001; Sunley et al., 2005).

Blank, 2002) report no evidence on the issue, although measures of prevailing labour market conditions (usually unemployment rates) are commonly *controlled for* (independent of programme effects) in WTW programme evaluations.[6] These reviews do discuss heterogeneous impacts of WTW programmes, however, just not explicitly with respect to labour market conditions. For example, Heckman et al. (1999) discuss the likelihood that programme impacts may be heterogeneous across individuals because of differences in their personal characteristics, e.g. in terms of skills (see Bonnal et al., 1997, for a particular application). Both Blank (2002) and Heckman et al. (1999) discuss the possible dependency of programme impacts on the actual implementation of the programme, citing evidence of heterogeneous impacts for the same programme in different sites. Blank (2002) does speculate that a relationship between WTW impacts and unemployment rates might help explain why, in general, some studies find larger impacts than others, but she cites no formal evidence of such a relationship.[7] Neither does a search of more recent literature throw up anything any more explicit.

Bloom et al. (2001) is one review, however, that does explicitly address the issue. The paper reviews evaluations of WTW programmes operating in 59 sites across the US, and finds programme impacts on future earnings to depend positively on the extent to which programmes emphasise employment, positively on the degree of personalised attention provided, and negatively on the size of adviser caseload (i.e. what might be thought of as implementation differences). More interestingly given our focus here, WTW programmes are also shown to have a bigger impact on the future earnings of

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[6] In other words unemployment rates often enter alongside, but not interactively with, programme dummies on the right hand side of regression equations for programme impacts.

[7] Blank argues, however, that interaction between multiple policy changes (WTW) and a booming economy in the mid 1990s US contributed to unexpectedly large declines in welfare caseloads (see also Blank, 2000).

participants in areas of low unemployment relative to areas of high unemployment, other things being equal.[8] They argue that this has important implications for the performance of WTW programmes during economic downturns. Bloom et al. (2001) also cite a small number of earlier US studies indicating differential impacts of WTW programmes by labour market conditions, including Gueron and Pauly (1991), Newman and Lennon (1995), Friedlander and Burtless (1995) and Jensen and Chitose (1997), but the body of evidence from these earlier studies is small.

This paper examines the particular case of the UK New Deal for Young People (NDYP) – a mandatory WTW programme for young people aged between 18 and 24 years old that have been unemployed and claiming unemployment benefit (called Jobseeker's Allowance, or JSA) for 6 months[9] – in search of the suggested relationships between programme impact and the state of local labour markets as measured by local unemployment rates. Existing evaluations of the NDYP have found a significant boost both to the hazard rate for exits from unemployment to employment and to the hazard rate for exits to inactivity (e.g. Blundell et al., 2001, 2003; Riley and Young, 2001a, 2001b; McVicar and Podivinsky, 2003a, 2003b; de Giorgi, 2005). As for the literature more generally, however, these studies have generally not looked for differential impacts according to the state of local labour markets, despite a belief among many of those analysing the NDYP that such a relationship might exist (e.g. Turok and Webster, 1998; Blundell et al., 2003; McVicar and Podivinsky, 2003b; White, 2004; Sunley et al., 2005).

[8] They find a 1 percentage point increase in the county level unemployment rate reduces the programme impact on future earnings by an average of $94 per year.
[9] Unlike in the US, eligibility for unemployment benefits in the UK does not end after 6 months for most claimants. Rather, benefits remain at the same level but are subject to means testing.

Here we follow the spirit of some of these earlier NDYP studies, and studies of other WTW programmes (e.g. Dolton and O'Neill, 1996; Jensen et al., 2003; Bolvig et al., 2003; Carling and Richardson, 2004; Rosholm and Svarer, 2004; Carling and Larsson, 2005), by estimating competing risks mixed proportional hazard (MPH) models for exits from unemployment to separately identified destinations – employment, inactivity, and 'unknown' – in addition to a MPH model for all unemployment exits aggregated across destinations. Estimated hazard rates are conditional upon observed covariates, including an NDYP participation dummy and local (Travel to Work Area (TTWA)) unemployment rates, along with unobserved heterogeneity, variously specified. We capture the relationship between programme impact and local unemployment rates by including an interactive term (NDYP*Unemployment Rate) in the observed covariates. The NDYP was not introduced experimentally, but, given certain assumptions, the programme impact can be identified by using 25-29 year olds (not covered by the programme) to construct the counterfactual. In other words we treat the introduction of the NDYP as a WTW natural experiment.

The rest of this paper is set out as follows. Section 2 presents more detail on the NDYP and findings from existing evaluations as to its impacts. Section 3 summarises our data and our approach to estimation of the MPH models. Section 4 presents and discusses the estimation results and Section 5 concludes.

2. The UK New Deal for Young People

Following the introduction of NDYP, a young person (aged between 18 and 24 years) that has been unemployed and claiming JSA for six months must report for an interview with a personal advisor or face benefit sanctions. There follows a period of individually tailored job search assistance and monitoring called *Gateway,* which is intended to last up to four months (but has been known to last up to six months or more in some cases). If at the end of that time the young person is still unemployed, a compulsory *option* must be taken up. These include full time education or training courses, subsidised employment placements, placements in the voluntary sector or on the Environmental Taskforce (essentially low level public sector work experience). Options usually last for up to six months (twelve months for education and training). Young people on an option are counted as having left the unemployment register; although they still receive benefits with a small supplement (those on subsidised employment placements cease to receive benefits).[10] Most participants leave NDYP before reaching the option stage.[11] If, after completing an 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 assistance and monitoring in job search.

<Figure 1 around here>

Figure 1 summarises possible paths through an NDYP episode. At all times during participation in the programme young people are encouraged to take up unsubsidised jobs. Between its introduction in April 1998 and October 2001 (roughly the 'policy on' period over which we observe our sample), 600,000 young people participated in

[10] Here the definition of unemployment becomes fuzzy. Although young people on an option are not counted as being on the unemployment register, and we treat them as no longer unemployed here, they can still be thought of as being unemployed in the sense that they are still receiving transfers equivalent to benefits, and are still encouraged to job search. This caveat is probably least relevant to subsidised employment in the private sector.

[11] To March 2005, of 1220100 total leavers from NDYP, 805370 (two thirds) left before reaching the option stage (Source: http://www.dwp.gov.uk/asd/ndyp.asp).

the programme, some more than once (National Audit Office, 2002). By March 2005 this number has doubled. NDYP is primarily a supply side policy in the sense that the focus is on job search (during Gateway), skills (education and training option) and work experience (voluntary and environmental task force options). Around one fifth of all option placements (i.e. around one fifteenth of all participants) are in subsidised jobs, however, so the NDYP can be thought of as having a demand side element (stimulating labour demand for unemployed 18-24 year olds)[12] along with the supply side elements of search assistance and monitoring, work experience, and training.[13]

There is a considerable international literature on the impacts of WTW programmes, much of it from the US where a substantial (often experimental) evaluation literature has built up. Friedlander et al. (1997), Heckman et al. (1999), Bloom and Michalopoulos (2001), and Blank (2002) provide recent reviews. There are also review papers for other countries, e.g. for Sweden see Sianesi (2004), Carling and Richardson (2004), for UK see Van Reenen (2003). Martin (2000) provides an international (OECD) review. These reviews tend to suggest that job search assistance coupled with monitoring and sanctions has been an effective tool to raise employment rates and future earnings, with more mixed evidence on the impacts of training and work placements. As already discussed, however, the possibility of heterogeneous programme impacts caused by spatial or temporal variations in labour market conditions has tended to be overlooked in this literature.

[12] The literature on job subsidies tends to suggest large deadweight losses and displacement, however, which implies net labour demand effects may be small (see e.g. Katz, 1996).

[13] Those on the subsidised employment option are required to attend one day per week classroom training for which the employer receives a one off payment.

NDYP itself has also been the subject of a number of evaluations as cited in the previous section. Most (but not all) studies have taken a partial equilibrium approach using the age restrictions of the policy to construct the counterfactual and therefore identify the treatment effects. Between them these existing evaluations suggest (i) that NDYP has increased outflow probabilities from unemployment for programme participants; (ii) that these outflows are primarily outflows to education or training or to employment; and (iii) that NDYP has had little effect on outflow probabilities for the target age group before entry to the programme, i.e. pre-participation. This last point can be interpreted as indicating a lack of selection into the programme, which might have arisen despite its mandatory nature because participation can be avoided by leaving unemployment before 6 months (e.g. Blundell et al., 2001).

In common with the WTW/ALMP literature more generally, these evaluations of the NDYP have tended to overlook the possibility of heterogeneous treatment effects caused by variations in the economic environment. The exceptions (McVicar and Podivinsky, 2003b, White, 2004, Sunley et al., 2005) *suggest* or *argue* for the presence of spatial differences in treatment effects linked to labour demand, without providing explicit evidence of such a relationship. For example, on the eve of the introduction of the NDYP, Turok and Webster (1998) predicted that the programme would have differential impacts in areas of high and low unemployment. Sunley et al. (2005) provide some descriptive evidence in support of this prediction by showing exit rates to employment for NDYP participants to be negatively correlated with local unemployment rates. Their analysis is based on a simple count of exits, however, with no attempt to construct a counterfactual to get at the net impact, or value added, of the programme. White (2004) finds heterogeneous NDYP impacts on unemployment exit rates according to the precise nature of the implementation of the programme in different areas (with greater 'work focus' leading to more positive impacts like Bloom et al. (2001)), and speculates that economic environments might matter. McVicar and Podivinsky (2003b) find the impact of NDYP on exits to employment (inactivity) to differ across 12 UK regions, in a manner weakly negatively (positively) correlated with regional unemployment rates.

Dorsett (2006), although he doesn't estimate treatment effects of NDYP (there is no control group), does compare labour market outcomes for participants taking different *routes* through NDYP, e.g. different options. He finds that labour market outcomes, e.g. in terms of job entry, are on average better for participants that stay longer on Gateway than for those entering options (with the exception of the subsidized employment option). He also finds that NDYP participants in high unemployment areas are more likely to enter options rather than stay on Gateway. Putting the two together suggests that labour market outcomes for participants might be better in low unemployment areas than in high unemployment areas. Again, although this does not provide explicit evidence of differential treatment effects by local labour market conditions, it is consistent with such differential effects.

3. Empirical Model and Data

Our (administrative) data are a random 5% sample of all male JSA claimants from 1996-2001 across the UK – taken from the JUVOS database[14] – and covering all spells in unemployment starting during that time. We restrict our attention to those

[14] Office for National Statistics, JUVOS Cohort: Longitudinal Database of the Claimant Unemployed since 1982, 12th Edition. Colchester, Essex: UK Data Archive, September 2005. SN: 3721.

aged 18-29 years[15], and also drop all repeat spells in unemployment once an individual has already been a NDYP participant, i.e. we only consider NDYP 'first timers'. This gives us information on 229,522 unemployment spells across 97,876 young men. The data include information on start date and end date of spell, destination on leaving unemployment, age, gender, occupation sought (a potential proxy for offer wage distribution or for qualifications), marital status, and – crucially – on the location of the benefit office at which the claimant is registered. They are also partly longitudinal in the sense that we can track multiple spells for each individual. In fact, the JUVOS data run back as far as 1982 (although destination information has only been collected since 1996) allowing us to build up a long run picture of unemployment spells during and prior to the 1996-2001 period, where applicable.

The JUVOS data do not include information on whether individual claimants are participating in NDYP or not. Given the mandatory nature of the programme, however, we can *assume* that all JSA claimants in the relevant age group become NDYP participants on day one of their seventh month of unemployment. Exceptions to this are single parents that are eligible to enter NDYP early (i.e. before being unemployed for six months) if they choose to do so, and those re-entering NDYP (follow-through) after an option or re-entering the programme for the second time following an unsuccessful follow-through and a subsequent spell of unemployment outside the programme.[16] By excluding females, and by dropping repeat spells following NDYP participation, we should omit almost all of these 'special' cases. In our model, participation in NDYP is captured by a time varying binary dummy taking

[15] Under 18s are not eligible for JSA in the UK and therefore do not feature in the data.
[16] Although such cases are theoretically possible given the 'rules' of NDYP, in practice claimants may remain on follow-through beyond the three month deadline.

the value 1 for those aged 24 years or under for that part of any unemployment spell beyond six months of duration and since April 1998, and 0 otherwise.

In common with much of the WTW evaluation literature, our measure of the state of the labour market is the local unemployment rate. Using the postal (zip) code of the benefit office at which a JSA claim is made we can map claimants to Travel to Work Areas (TTWAs) for which monthly claimant count unemployment rate data are available.[17] We therefore take the TTWA unemployment rate in the start month of an unemployment spell to be our measure of the state of the local labour market.[18] By interacting the NDYP dummy with the local unemployment rate, we can directly test whether the data support our hypothesis of a relationship between economic environment and the impact of the NDYP. The estimated sign of the coefficient on this interactive term indicates the direction of any relationship.

Again, in common with much of the empirical unemployment duration literature, we take a reduced form approach to estimation. The particular specification we adopt is the mixed proportional hazards (MPH) model (see van den Berg, 2001), as given below:

$$\theta_k(t_j \mid \alpha) = \alpha\theta_{0k}(t)\exp(X_j\beta_k), \tag{1}$$

[17] We assume people claim in their local offices, which is usually the case but doesn't *have* to be the case.

[18] Given that these unemployment rates are fairly stable over time (at least month by month), the added realism of including time varying unemployment rates is not worth the added complication – and the huge increase in the size of the data file – of doing so.

for individuals j=1,..,N, for exit type k, where $\alpha \sim d(1,\phi)$ captures the multiplicative effect of unobserved heterogeneity on the hazard rate, where $\theta_{0k}(t)$ denotes the baselines hazard, and where X_j are a set of observed characteristics, including local unemployment rate, the policy dummy and their interaction. Exits to employment, inactivity and 'unknown' are identified separately, assuming *independent* competing risks (conditional on X_j).[19] We treat our (daily) data as continuous.[20] The JUVOS data contain no information on skills (beyond occupation sought), so this is likely to be an important component of the unobserved heterogeneity term.

The main advantages of the MPH model are that (i) it has been widely applied in previous research (therefore our results can be more readily compared with existing findings and the properties of the model are well understood) and (ii) the parsimonious manner in which it combines those parts of the overall hazard explained by (observed and unobserved) covariates and a baseline hazard capturing the relationship between the hazard rate and the duration of the unemployment spell (van den Berg, 2001). Van den Berg (2001) sets out a number of disadvantages of the model, however, including (i) that the assumption of proportionality (i.e. the baseline hazard determines the shape of the hazard function and the observed and unobserved covariates determine its position) doesn't follow in general from job search models; (ii) that the β_k s are not structural parameters from the point of view of the theory; and (iii) that estimates might be very sensitive to functional form assumptions, particularly in the case of single spell data. Reduced form alternatives to the MPH model are no

[19] The assumption of independence implies that, for each type of exit, exits to all other destinations can be treated as right censored, and also that unobserved heterogeneity terms are uncorrelated across exit destinations.
[20] The average length of spell is 109 days, so a continuous treatment is more appropriate than a discrete treatment.

better in these respects, however, and may even be worse (van den Berg, 2001). To examine the sensitivity of our results we do go on to explore one such alternative – the Accelerated Failure Time (AFT) model – but our focus is on the MPH results.

In order to identify the treatment effect of the NDYP we estimate the MPH models for 25-29 year olds (not eligible for NDYP participation on grounds of age) in addition to 18-24 year olds over a 5 year period spanning the introduction of NDYP. In using the older age group as controls we follow a number of earlier evaluations of NDYP (e.g. Blundell et al., 2001; Riley and Young, 2001a; McVicar and Podivinsky, 2003a, b). The validity of this approach in correctly identifying the treatment effect is discussed elsewhere (e.g. Blundell et al., 2001). Briefly, two necessary assumptions for the 25-29 year old age group to be a valid control group – that the two age groups had been following similar trends prior to the introduction of the NDYP and that there are no significant inter age group substitution effects – have been shown to be supported by the data, at least for males. Heckman et al. (1999) presents a general discussion of identification in the context of WTW evaluations, and one of their conclusions is that there is a place for simple, well specified, non experimental studies alongside the more usual (at least in the US) experiments.

Our framework does not allow us to identify whether there are different NDYP effects at different durations of unemployment. Our participation dummy is intended to capture the effects of participation in the Gateway stage (first time around) of the NDYP (those leaving Gateway to enter NDYP options are counted as leaving the JSA register so are included in the observed outflow from unemployment). We have therefore assumed that the treatment effect of the NDYP does not vary according to the number of weeks in which an individual has participated in the programme.[21] We have also explicitly assumed there is no pre-participation NDYP effect at less than six months duration of unemployment. Such effects are feasible, e.g. if JSA claimants in the eligible age group cease to claim in the month prior to NDYP entry in order to avoid the programme. This could be because they receive a letter summoning them to participate or simply because they know such a letter is imminent (see Blundell et al., 2001). This has been interpreted as a 'threat' effect, and such effects have been found to be significant in other contexts by some studies (e.g. Black et al., 2003; Jensen et al., 2003). Indeed, Heckman et al. (1999) argue that many evaluations are actually measuring a treatment effect that implicitly includes this threat effect rather than a programme participation effect, *per se*. In the particular case of NDYP, however, earlier evaluations that have searched for such effects have found no evidence that they exist on any significant scale (see e.g. Blundell et al., 2001; McVicar and Podivinsky, 2003a, b; de Giorgi, 2005). The fact that NDYP is a mandatory programme for long term unemployed 18-24 year olds therefore implies we can proceed to estimate our MPH models without worrying about selection bias, since there is no evidence of self-selection into the programme.[22]

Although we can argue *a priori* that there is no self-selection into the programme, by dropping repeat spells in NDYP we may introduce an age related selective element into our sample. Consider the following example. We might expect to observe two unemployment spells for someone aged 20 in 1996 with a moderate tendency to be unemployed, e.g. one as a 22 year old including a treatment period and one as a 25

[21] Some earlier evaluations of the NDYP do allow for variation in treatment effect over the course of programme participation (see e.g. McVicar and Podvinsky, 2003a, b, Riley and Young, 2001a). But this moves us away from an MPH framework.
[22] This mandatory nature of the programme, coupled with evidence that suggests a lack of pre-participation effects, is an attractive programme characteristic from an evaluation point of view.

year old in the control group. By dropping all spells subsequent to an NDYP spell, however, we drop the control group spell in this case. On the plus side this means that there is no one in our control group that has been treated in the past.[23] On the down side, by dropping these spells, we increase the number of single spell observations in the sample and we may under-represent people with a high unemployment tendency in the post introduction of NDYP control group. Also, to the extent that this is age related, we may not consistently estimate the impact of age on the hazard rate. More importantly, the possibility that the average control group baseline hazard will be higher than it would be if the group was truly representative of all 25-29 year old claimants, may lead us to underestimate the 'true' NDYP treatment effect. Our belief is that this bias will be smaller than it would be were we to leave these spells in.

In general, multiple spells data are helpful in terms of identification of the MPH model, essentially by allowing unobserved heterogeneity to be treated like a 'fixed effect' in conventional panel data (van den Berg, 2001). Here we have the in-between case of mixed single spell and multiple spell data, although most sample members are observed more than once. Van den Berg's (2001) third criticism of MPH models – that estimates might be very sensitive to functional form assumptions, particularly for single spell data – is therefore less of a concern here than it would be for wholly single spell data. Nevertheless we test the sensitivity (of the estimated covariate parameters, particularly the policy and interactive ones) to different assumptions about the form of the baseline hazard.

[23] If such spells were included in the control group, and there are long run effects of treatment, we would be underestimating the treatment effect because our control group would also have been treated.

We make the assumption that the local unemployment rate is exogenous to our model (i.e. it is not jointly determined with the hazard rate), for which arguments might be that our age group make up only a small part of overall adult unemployment and that the unemployment rate is measured at start of spell. In any case, we seek to place this paper in a literature that generally adopts the MPH framework and usually includes unemployment rate as an (assumed exogenous) control.

Finally, consider measurement error. First, the recording of exit destinations in the administrative data from which the JUVOS sample are drawn is far from perfect. The main problem is the large proportion of spells that end due to a failure to sign on by the claimant, in other words an unknown destination. Around a third of all spells in our data end for this reason. It is generally believed that around half of these spells are in fact exits to employment (e.g. National Audit Office, 2002). Without better data, we are stuck with this problem, so it should be borne in mind in interpretation of the competing risks hazards in the following section. Second, as mentioned above, we infer NDYP participation from the programme rules rather than *observing* participation. Therefore our policy dummy may involve error, which if non-random might lead to inconsistent parameter estimates.[24] There are other potential concerns with measurement error, e.g. how well do local unemployment rates measure the state of the labour market, but these are the main ones.

4. Results and Discussion

[24] For example, we might imagine implementation delays in high unemployment areas because of insufficient staff to deal with case load.

Table 1 presents results from estimation of the MPH model for all exits (i.e. we do not separately identify exits by destination) first without unobserved heterogeneity (PH) and second with unobserved heterogeneity (MPH). We assume a Weibull baseline hazard and gamma distributed unobserved heterogeneity. Results are presented in the form of hazard ratios, with numbers greater than one indicating a positive proportional impact on the hazard and numbers less than one indicating the opposite. The Weibull baseline hazard appears downward sloping where unobserved heterogeneity is ignored ($p<1$) but is gently upward sloping where unobserved heterogeneity is modelled ($p>1$). A likelihood ratio test shows the unobserved heterogeneity term to be significant, however, so column three of Table 1 is our preferred specification.[25] (Notice that the estimated hazard ratios are in any case very similar in the PH and MPH versions of the model.) Most of the controls have significant impacts on the hazard rate in the expected directions, e.g. hazard rates are lower in high unemployment areas, for older workers, for those with a longer previous history of unemployment[26], and for those in London and the South East[27] and Northern Ireland.

<Table 1 around here>

[25] As is commonly found in studies of unemployment duration, ignoring unobserved heterogeneity leads to spurious negative duration dependence (see Heckman et al., 1999).

[26] We treat historical unemployment durations as exogenous throughout the analysis. To check the sensitivity of our results to this assumption we re-estimate the MPH model for exits from unemployment replacing the accumulated duration of previous unemployment spells covariate with a predicted value for the individual's number of spells of unemployment per year averaged over all years since the individual was first observed. This predicted value is derived from a simple OLS regression (in logs) on individual characteristics and TTWA unemployment rates taken from 1991 Census data (significantly and positively related to an individual's number of unemployment spells per year). The results from this alternative MPH model are very close to those reported in Table 1, suggesting robustness.

[27] This is the omitted region. Our intuition for the lower hazards in the South East overall is that London, which includes many highly deprived areas, dominates the rest of the South East.

The estimates presented in Table 1 suggest that participation in NDYP increases the hazard rate for exit from unemployment by 24%. The hazard ratio for the interactive term suggests there is a small, but statistically significant, additional impact of NDYP on the hazard rate which is *positively* related to the local unemployment rate, with a 1 percentage point increase in the unemployment rate increasing the NDYP impact on the hazard rate by a further 1 percentage point. Unemployment rates range from 0.8% to 15.9% in our sample with a mean of 6% and standard deviation of 2.2%. The estimated NDYP effect on the hazard rate therefore ranges between 17% and 32%. So the data suggest that the treatment effect of NDYP (on *all exits* from unemployment) is partly dependent on the state of the local labour market, with the 'smaller deadweight loss' mechanism dominating any 'lack of vacancies' mechanism.

Narendranathan and Stewart (1993) warn that the Weibull restriction (imposing monotonicity) for the baseline hazard can potentially bias the estimated effects of time varying covariates as well as the estimated shape of the baseline hazard itself. In this case the NDYP and interactive NDYP*Unemployment covariates are both time varying. We check the sensitivity of their estimated effects – following the procedure suggested by Narendranathan and Stewart (1993) – by re-estimating the model as a Cox PH model (where the shape of the baseline hazard is not restricted). Results are presented in column four of Table 1. Our conclusions remain the same: NDYP increases the hazard rate and the size of the treatment effect is positively related to the local unemployment rate.[28]

[28] Unobserved heterogeneity cannot be included in a Cox PH model without the presence of multiple integrals of the same order as the number of individuals in the risk set (Han and Hausman, 1990). In our case, with close to 100,000 individuals, estimation of such a model is problematical. We can straightforwardly estimate a Cox MPH model on a random sub sample of individuals drawn from the data set, however, and do so for 440 individuals observed over 1000 spells. This exercise again suggests significant (gamma) unobserved heterogeneity. The estimated hazard ratios are very close to

A potentially important assumption behind the results presented in Table 1 is that the (observed and unobserved) covariates enter the hazard function multiplicatively, i.e. the proportionality assumption. As discussed in Section 3 this is not generally suggested by theory. In this particular case we might be uncomfortable with the assumption that NDYP participation has a proportional effect on the hazard at all stages of Gateway. This would not be the case, for example, if there is an initial entry ('shock') effect or if participation effects increase as option entry is approached towards 10 months into an unemployment spell. Grambsch and Therneu (1994) provide a test of this assumption (in the context of a Cox model). Running this test suggests the assumption of proportionality may be inappropriate, although this appears to be driven more by some of the control variables than the NDYP dummy). In such cases the model can be stratified by the covariates with the least proportional impacts, i.e. the shape of the baseline hazard is allowed to vary across the different strata. This is problematical in our case, however, given that the main culprits are continuous (e.g. age, accumulated unemployment duration in previous spells, local unemployment rate) and that stratification by unemployment rate means we can no longer identify a separate differential treatment effect by unemployment rate.

An alternative to stratification is to move away from PH/MPH models altogether, e.g. to the Accelerated Failure Time (AFT) approach. AFT models have been rarely estimated in an economics context, however, and Van den Berg (2001) argues they are even further removed from the motivating theory – and likewise the WTW evaluation literature that provides the background to our study – than MPH models.

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those shown in Table 1. NDYP increases the hazard rate and the treatment effect is positively related to the local unemployment rate, although the small size of the sub sample means these relationships appear statistically insignificant.

Nevertheless he suggests they may be of some use where aims are limited to investigating the impact of treatment or control variables on the duration of unemployment spells in an atheoretical manner. We estimate such a model to check whether the positive relationship between the NDYP treatment effect and the local unemployment rate is sensitive to the proportionality assumption. The AFT model is given by Equation (2):

(2) $$\ln t_j = X_j \beta + z_j$$

for spells j=1,..N and where X_j can include an unobserved heterogeneity term. Here we specify an extreme value distribution for z_j (yielding the Weibull regression model) and gamma distributed unobserved heterogeneity. Results are presented in Table 2.

<Table 2 around here>

For the AFT model we report coefficients indicating the impact of a one unit change in the covariate on the log duration, so negative values correspond with *shorter* durations (which in the hazard metric would have been indicated by a hazard ratio greater than one) and *vice versa*. All controls indicate a similar directional effect on spell duration as in Table 1. The NDYP dummy indicates a treatment effect that shortens the duration of spells (corresponding to an increased hazard rate in Table 1). As in the proportional hazard framework, this estimated treatment effect is larger where local unemployment rates are higher. In short, the inference that the size of

NDYP effects on exits from unemployment is positively related to the local unemployment rate is robust to both MPH and AFT specifications.

Of course what Tables 1 and 2 pick up is the impact of NDYP on *all* exits from unemployment, i.e. exits that are undifferentiated by destination. Existing evidence at the broad regional level (McVicar and Podivinsky, 2003b), however, together with intuition based on search models as discussed in the introduction to this paper, suggest that the sign of the differential part of the WTW impact could vary across exit types. In other words the estimated positive relationship between the NDYP impact and unemployment on overall exits might hide oppositely signed – and possibly larger – component differential impacts on exits to employment and other types of exits. Table 3 presents results – again in hazard ratio form – for an independent competing risks MPH model, separately identifying exits to employment, exits to inactive (education, government training placements or other benefits), and 'unknown' (mostly failure to sign on). Given the robustness of the estimates in the case of exits aggregated across destinations, we limit our analysis to the simple parametric MPH model with Weibull baseline and gamma distributed unobserved heterogeneity. The estimated baseline hazards are gently upward sloping for exits to employment and to inactivity and gently downward sloping for exits to unknown destinations. The gamma frailty is significant in each case. Controls have expected signs and are mostly significant.

<Table 3 around here>

First consider exits to employment (arguably the most important for policy makers). As was the case for exits overall, participation in NDYP increases the hazard rate, by around 17% on average. Notice this is slightly smaller than the impact on overall exits. Also notice that this effect is *weaker* in *high* unemployment areas and stronger in low unemployment areas, with a 1 percentage point increase in the unemployment rate *reducing* the NDYP impact on the hazard by around 1.5 percentage points. The differential part of the NDYP impact on exits to employment is larger and oppositely signed than it was in the case of overall exits. This negative sign on the interactive dummy (i.e. the hazard ratio below one) suggests that it is the 'lack of vacancies' or 'discouraged worker' differential WTW effects that dominate the 'higher value added' differential effect for exits to employment in slack labour markets. The direction of this differential impact is consistent with the Bloom et al. (2001) differential WTW impact on future earnings (bigger impact in low unemployment areas) and, more specifically, with Dorsett's (2006) indirect findings on NDYP and job entry across labour markets.

Second consider exits to inactivity (education, training placements and moving to other benefits). The hazard rate for such exits is strongly affected by participation in NDYP – in fact it *doubles* during participation. This is likely to reflect both entry to NDYP training option placements as well unemployment benefit claimants switching to other benefits for which NDYP participation is not compulsory (see McVicar and Podivinsky, 2003a). It also appears that this NDYP impact is stronger in areas with higher unemployment than in areas with lower unemployment, i.e. again there is a statistically significant effect of the interactive covariate on the hazard rate. The magnitude of this effect is larger still (than in the case of exits to employment), with a 1 percentage point increase in unemployment increasing the NDYP effect on the hazard rate by a further 2 percentage points. This, again, is consistent with the 'discouraged worker' WTW effect being stronger in slack labour markets because WTW requires unemployed workers to move further from their optimal search effort in such markets (driving them to their outside option).

Finally consider exits to unknown destinations. Here again NDYP participation increases the hazard rate, by 27%. There is a similar dependence of the treatment effect on local unemployment rates to the case of exits to employment, i.e. a 1 percentage point increase in unemployment rates *decreases* the NDYP treatment effect by 1.5 percentage points. Our explanation is that most of these exits in fact represent exits to employment (e.g. see National Audit Office, 2002), so the interpretation and intuition for the results carry over from above. Note that when exit types are aggregated we will observe (as shown in Table 1) a smaller overall differential impact that is some weighted sum of the impacts on the competing risks.

5. Concluding Remarks

In this paper we have shown that a large scale WTW programme – the NDYP – has had differential impacts systematically related to local labour market conditions. In areas of high unemployment, the treatment effect on exits to employment has been weaker, and the treatment effect on exits to inactivity has been stronger, than in low unemployment areas. Our interpretation of these differential WTW impacts is that some unemployed workers in slack labour markets are discouraged from making the additional search effort required by a WTW programme and consequently 'drop out' of registered unemployment, e.g. to claim alternative benefits or to enter education or training programmes.

When exits from unemployment are aggregated across destinations, our results are consistent with the suggestion that the added value of WTW programmes – at least in terms of reducing registered unemployment – might be bigger in slack labour markets because they engender a greater increase in search effort in such markets. This single risk finding, however, is merely a weighted sum of the oppositely signed differential WTW impacts on the competing risks of exits to employment, inactivity and ‘unknown’, not all of which are likely to be treated as equally desirable by policy makers. Disaggregation of exits from unemployment into their component competing risks is therefore shown to be important here, and policy advice based on single risk analysis potentially misleading.

Despite widespread speculation on the existence of differential treatment effects in existing papers and evaluations of the NDYP, ours is the first study to show explicitly that such differential impacts exist. There are good reasons to believe these findings on the NDYP would generalise well for other WTW programmes. Clearly the theoretical arguments for the existence of such differential treatment effects can be generalized beyond our particular application here. Further, although few papers have explored whether there are differential WTW impacts by local unemployment rates in the US, those that have done so have tended to confirm their existence. The evidence presented here is consistent with, though complementary to, these earlier studies, being (i) based on non-experimental evaluation of a national UK programme rather than experimental evaluations of local US programmes; and (ii) focusing on programme impacts on (short term) exits from unemployment rather than programme impacts on earnings.

In terms of policy, the paper has implications for the benchmarking of NDYP performance at local level, e.g. comparisons of performance across similar local labour markets may be more informative than comparisons across contrasting local labour markets. This also holds for target setting for those implementing NDYP on the ground. Further, with UK unemployment rates beginning to rise, we might anticipate an NDYP in the future that moves less young people into work and more young people into inactivity than has so far been the case. More generally, as Sunley et al. (2005) have pointed out in the case of the NDYP, a WTW programme that has a greater impact in low unemployment areas than in high unemployment areas will act to exacerbate existing spatial differences in unemployment. If this is undesirable then there may be a case for complementing WTW programmes like the NDYP with spatial demand side policies.

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Fig 1: NDYP Timeline

Unemployed - usually for six months → Gateway – usually up to four months → New Deal Options – up to 6 or 12 months: → Follow-through – usually up to three months

New Deal Options – up to 6 or 12 months:

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

Table 1: PH & MPH Models, All Exits from Unemployment, Hazard Ratios (P-values)

	Weibull Baseline, No Unobserved Heterogeneity	Weibull Baseline with Gamma Unobserved heterogeneity,	Cox, No Unobserved Heterogeneity
NDYP*U rate	1.012 (.001)	1.009 (.025)	1.011 (.002)
NDYP	1.074 (.002)	1.241 (.000)	1.480 (.000)
U rate	.952 (.000)	.949 (.000)	.962 (.000)
Cohabit	.993 (.349)	.999 (.897)	.994 (.480)
Age	.984 (.000)	.992 (.000)	.993 (.000)
SOC 1,2 sought	1.230 (.000)	1.260 (.000)	1.197 (.000)
Past U duration	.531 (.000)	.526 (.000)	.556 (.000)
East Anglia	1.116 (.000)	1.148 (.000)	1.116 (.000)
South West	1.201 (.000)	1.220 (.000)	1.188 (.000)
West Midlands	1.025 (.009)	1.018 (.138)	1.019 (.030)
East Midlands	1.097 (.000)	1.090 (.000)	1.086 (.000)
Yorks & Humb	1.171 (.000)	1.165 (.000)	1.151 (.000)
North West	1.123 (.000)	1.116 (.000)	1.105 (.000)
North	1.184 (.000)	1.164 (.000)	1.150 (.000)
Wales	1.111 (.000)	1.090 (.000)	1.097 (.000)
Scotland	1.214 (.000)	1.198 (.000)	1.183 (.000)
N Ireland	.917 (.000)	.905 (.000)	.909 (.000)
Weibull p	.902 (.000)	1.024 (.000)	
Gamma θ		.283 (.000)	
No. spells	229522	229522	229522
No. failures	214358	214358	214358
No. subjects	97876	97876	97876

Table 2: AFT Model, All Exits from Unemployment, Coefficients (P-values)

	Weibull, Gamma
NDYP*U rate	-.008 (.025)
NDYP	-.211 (.000)
U rate	.051 (.000)
Cohabit	.001 (.897)
Age	.008 (.000)
SOC 1,2 sought	-.226 (.000)
Past U duration	.627 (.000)
East Anglia	-.135 (.000)
South West	-.194 (.000)
West Midlands	-.017 (.138)
East Midlands	-.084 (.000)
Yorks & Humb	-.149 (.000)
North West	-.107 (.000)
North	-.148 (.000)
Wales	-.085 (.000)
Scotland	-.177 (.000)
N Ireland	.097 (.000)
Weibull p/Normal σ	1.023 (.000)
Gamma θ	.282 (.000)
No. spells	229522
No. failures	214358
No. subjects	97876

Table 3: MPH Model, Weibull Baseline, Gamma Frailty, Independent Competing Risks, Hazard Ratios (P-values)

	Exits to Employment	**Exits to Inactive**	**Exits to Unknown**
NDYP*U rate	.985 (.016)	1.020 (.005)	.986 (.039)
NDYP	1.174 (.000)	2.003 (.000)	1.272 (.000)
U rate	.945 (.000)	.967 (.000)	.948 (.000)
Cohabit	1.049 (.001)	1.080 (.002)	.861 (.000)
Age	1.020 (.000)	.958 (.000)	.962 (.000)
SOC 1,2 sought	1.306 (.000)	.907 (.077)	1.224 (.000)
Past U duration	.333 (.000)	1.874 (.000)	.599 (.000)
East Anglia	1.505 (.000)	1.052 (.280)	.780 (.000)
South West	1.549 (.000)	1.254 (.000)	.852 (.000)
West Midlands	1.188 (.000)	1.114 (.000)	.811 (.000)
East Midlands	1.369 (.000)	1.184 (.000)	.777 (.000)
Yorks & Humb	1.406 (.000)	1.314 (.000)	.872 (.000)
North West	1.323 (.000)	1.340 (.000)	.829 (.000)
North	1.548 (.000)	1.621 (.000)	.655 (.000)
Wales	1.369 (.000)	1.384 (.000)	.712 (.000)
Scotland	1.408 (.000)	1.666 (.000)	.843 (.000)
N Ireland	.935 (.017)	1.285 (.000)	.749 (.000)
Weibull p	1.099 (.000)	1.135 (.000)	.941 (.000)
Gamma/IV θ	.850 (.000)	.343 (.000)	.626 (.000)
No. spells	229522	229522	229522
No. failures	115151	26487	72720
No. subjects	97876	97876	97876