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Abstract

One view of Active Labour Market Programmes (ALMPs) is that they are 'most needed' in slack labour markets, where more unemployed workers require help finding jobs. But ALMPs might be less effective in such labour markets because there are fewer vacancies with which programme participants can match. In this paper we use data over a nine year period, across 300 local labour markets, to show that the unemployment exit and job entry impacts of participating in a mandatory ALMP for unemployed young people – the British New Deal for Young People (NDYP) – were negatively correlated with unemployment rates.

JEL-Classification: J64, J68

Keywords: Active Labour Market Programmes, New Deal for Young People, unemployment, evaluation, heterogeneous effects

1. Introduction

Active Labour Market Programmes (ALMPs) are used across the OECD in an effort to help the unemployed, or particular groups of unemployed, into jobs. These programmes usually involve one or more supply side measures such as job search assistance, training and work placements, together with benefits sanctions for non-compliance. Some ALMPs also feature demand side measures such as wage subsidies for firms to hire programme participants. Evaluations are numerous and their findings diverse, although evidence is often found of positive impacts on unemployment exit, job entry, and/or future earnings (for reviews see Heckman et al., 1999; Martin, 2000; Blank, 2002; Kluve, 2006).

Of particular interest in this literature is the question of whether similar ALMPs might have heterogeneous impacts according to the observed or unobserved characteristics of their participants and/or the particular nature of their provision in different areas or different times. A related but conceptually distinct question, yet to be widely addressed, is whether programme impacts vary systematically with labour market conditions. If this is the case then we have an additional explanation for some of the diversity of findings in evaluations of different programmes, at different times, and in different locations. And whether one views ALMPs as ‘most needed’ in slack labour markets (where more unemployed workers require help) or in tight labour markets (where more vacancies require filling), this question is also of critical importance for policy makers designing programmes for different labour market contexts. Given the recent rapid increase in unemployment rates experienced across much of the OECD, is what we learned about the impacts of ALMPs in more favourable labour market contexts still relevant?

This paper uses data over a nine year period, across 300 local labour markets, to examine whether the unemployment exit and job entry impacts of participating in a mandatory ALMP for unemployed young people – the British New Deal for Young People (NDYP) – varied systematically with local unemployment rates. The NDYP has a number of useful characteristics for this purpose: it is delivered locally but eligibility and structure are set nationally; it is predominantly but not exclusively a supply side programme, with elements that are typical of ALMPs internationally; participation in the programme is mandatory for the target group; it has already been widely evaluated with all studies indicating significant programme participation impacts on unemployment exit and/or job entry (e.g. see Blundell et al., 2004); and because the programme ran nationally for a long period of time, our data feature substantial variation in unemployment rates across both space and time.

The rest of the paper is set out as follows. Section 2 sets out a simple framework for why we might expect differential ALMP impacts across labour markets. Section 3 discusses a handful of existing empirical studies that have examined this issue. Section 4 presents more detail on the NDYP, its institutional context and its existing evaluations. Section 5 summarises our data. Section 6 discusses identification and presents some initial indications of programme impacts and whether they vary across labour markets. Section 7 presents econometric estimates of programme impacts on unemployment exit and job entry and their variation with unemployment rates. Section 8 introduces controls for heterogeneous impacts by observed participant characteristics and local differences in the precise nature of programme provision. Section 9 concludes.

2. A Simple Framework

Consider an exemplar ALMP, predominantly supply side in nature, covering either a range of local labour markets at a particular point in time, the same labour market over a period of time, or both. The ALMP is governed by uniform eligibility rules and contains a uniform set of measures, although there may be local or temporal variation in how the programme is implemented in terms of the duration or weighting given to each measure. Participation in the ALMP is mandatory for those in its target group. Our interest is in how participation in such an ALMP affects the probabilities of unemployment exit and job entry for its participants. (We do not have data on subsequent earnings.) But rather than focus on the average treatment effect for the treated, we explore how treatment effects might vary in different labour market contexts.[1]

Three potential sources of variation exist. First, ALMPs might have greater impacts in tight labour markets because more or better vacancies exist (e.g. Bloom et al., 2001). (Taken to extremes, there is little point intervening to, say, increase search intensity, if there are no job vacancies offering above the reservation wage in the local area.) On the other hand the added value of ALMPs may be lower in tight labour markets because many of the unemployed would have found jobs in any case (e.g. Gueron and Pauly, 1991). If neither mechanism dominates at all unemployment rates then the

[1] To keep the discussion simple we assume no firm or worker migration between labour markets, although this is not critical.

overall relationship between unemployment rates and programme impacts may be non-monotonic.

Second, ALMPs may have heterogeneous impacts on their participants according to observed or unobserved participant characteristics (e.g. Heckman et al., 1999). Such characteristics might be uncorrelated with labour market conditions, but it is more likely that the composition of the unemployed differs when unemployment rates are high compared to when they are low. Kluve (2006), for example, suggests that unemployed workers will have less favourable characteristics – they will be less employable for a given vacancy – in tight labour markets. Friedlander (1988) suggests that from a policy maker's perspective it may be difficult to help those with the least employable characteristics, but also that it might be difficult to help those with the most favourable characteristics since they will gain little from programme participation. This provides another mechanism for a potentially non-monotonic relationship between unemployment rates and programme impacts.

Third, the policy 'treatment' – the precise nature of the measures received – may vary across individuals, in different areas, or at different times (e.g. Blank, 2002), and this variation may itself be correlated with labour market tightness (e.g. Dorsett, 2006). This may be because the particular bundle of measures each individual participant receives is correlated with his or her characteristics, which are themselves correlated with labour market tightness. Alternatively, programme providers in tight labour markets might focus more on, say, job search measures, whereas those in slack labour markets might focus more on, say, training measures, or *vice versa*. Again there is no reason to assume any such variation is monotonically related to unemployment rates.

Taken together we are left with the possibility of programme participation impacts that vary with labour market conditions with uncertain sign and monotonicity, and although such differences in impacts may be small, or may cancel out, it would seem more appropriate when evaluating programmes across different labour markets to start by ruling them in rather than ruling them out.

3. Existing Literature

Few papers report empirical evidence on this particular issue, although measures of prevailing labour market conditions – usually unemployment rates – are commonly *controlled for*, independent of programme impacts, in ALMP or welfare to work programme evaluations. Blank (2002) suggests that a relationship between programme impacts and unemployment rates might help explain why some studies find larger impacts than others, but cites no formal evidence of such a relationship. Bloom et al. (2001), Kluve (2006) and Lechner and Wunsch (2009), however, do explicitly test for such a relationship, although each study is rather different and they draw different conclusions. In a related study, Jurajda and Tannery (2003) explores differential impacts of extended unemployment insurance entitlement across labour markets.

Bloom et al. (2001) reviews random assignment based evaluations of three welfare to work programmes operating in 59 sites across the US: California's Greater Avenues for Independence Program, Florida's Project Independence, and the National Evaluation of Welfare-to-Work Strategies. They find bigger impacts on the future earnings of participants in areas of low unemployment relative to areas of high unemployment, with a one percentage point increase in the county level unemployment rate reducing the programme impact on future earnings by an average of $94 per year. The precise nature of the programmes provided is also shown to be an important determinant of impact, with better outcomes for ALMPs with more immediate job search focus. Participant characteristics, e.g. education level, past employment and earnings experience and past welfare receipt, do not appear to influence programme impacts in a systematic way. Note, however, that the study only looks for linear relationships between programme impacts and these various factors.

Kluve (2006) presents a cross-country meta-analysis of national level European ALMPs drawing on the findings of 137 existing evaluation studies. The main dimension along which programme impacts on employment probability vary in these studies is the type of ALMP, with wage subsidies and job search services and sanctions being the most effective. But Kluve also examines how programme impacts relate to national unemployment rates, finding a positive relationship between programme impact and unemployment rate which disappears when country dummies are included. As for Bloom et al. (2001) this study looks only for linear relationships between impacts and unemployment. Further, because Kluve draws on macro level studies there is no explicit consideration of how local implementation differences of a given programme, or differences in the characteristics of participants, affect programme impacts. To the extent that these are correlated with national unemployment rates, the positive/zero relationship found between unemployment and programme impact could be capturing a combination of some or all of the mechanisms discussed in Section 2.

Lechner and Wunsch (2009) study differences in German ALMP impacts, on subsequent earnings and unemployment and employment probabilities, using micro data over ten years with unemployment rates specified at the national level. Again in a linear framework they find larger programme impacts when unemployment is higher, whether they control for programme type and/or participant characteristics or not. They also explore differences across regions, again finding a positive relationship between ALMP impacts and regional unemployment rates, albeit of smaller magnitude and only marginal statistical significance.

Jurajda and Tannery (2003) examine whether extending the duration of unemployment insurance entitlement has differential adverse impacts on job entry and recall hazards in labour markets characterised by different unemployment rates – Philadelphia and Pittsburgh – in the early 1980s. In this case the 'treatment' – the extension of benefit entitlement duration – is the same in both markets, so any variation in impacts is explained by differences in labour market tightness and/or in the characteristics of the unemployed. Also adopting a linear specification, they find a statistically significant negative relationship between unemployment rates and the adverse 'extension' effect which becomes insignificant when they control for heterogeneity across individuals.

That these studies draw different conclusions is perhaps not surprising given that they differ in terms of programmes covered, level of aggregation, outcome measures, and degree of control for heterogeneous programme impacts across participant characteristics and/or by programme provision. One factor that is common to all four studies, however, is that they specify linear relationships between unemployment rates and programme impacts. In contrast, we have suggested that such relationships could be non-linear. This also ties in with evidence of non-monotonic heterogeneous treatment effects by observed participant characteristics. For example, Friedlander (1988) studied variation in programme impacts across observed characteristics for a number of early 1980s US welfare-to-work programme, finding programme impacts for those with characteristics that place them in the middle of the 'employability distribution' were higher than for those at either end of the distribution. More recently Aakvik et al. (2005) found non-linear variation in treatment effects of a Norwegian ALMP across quintiles of the distributions of observed characteristics including age and education level. Having said that, unemployment rate distributions in the Bloom et al. (2001), Jurajda and Tannery (2003), Kluve (2006), and Lechner and Wunsch (2009) studies are sufficiently similar to suggest that the assumption of linear relationships between unemployment rates and programme impacts is unlikely to be the main driver of their contrasting conclusions.

4. The New Deal for Young People

Unemployment benefit, which takes either an insurance-based or a social assistance form, goes by the name of Jobseeker's Allowance (JSA) in Britain. Insurance-based JSA is paid at a national rate independent of prior earnings, lasts up to six months in any particular spell and requires a work history for eligibility. Income-based JSA (social assistance for the unemployed), paid to those not eligible for insurance-based JSA either because of insufficient work history or because they have exhausted their current entitlement, is also paid at a national rate but is means tested. Both types of

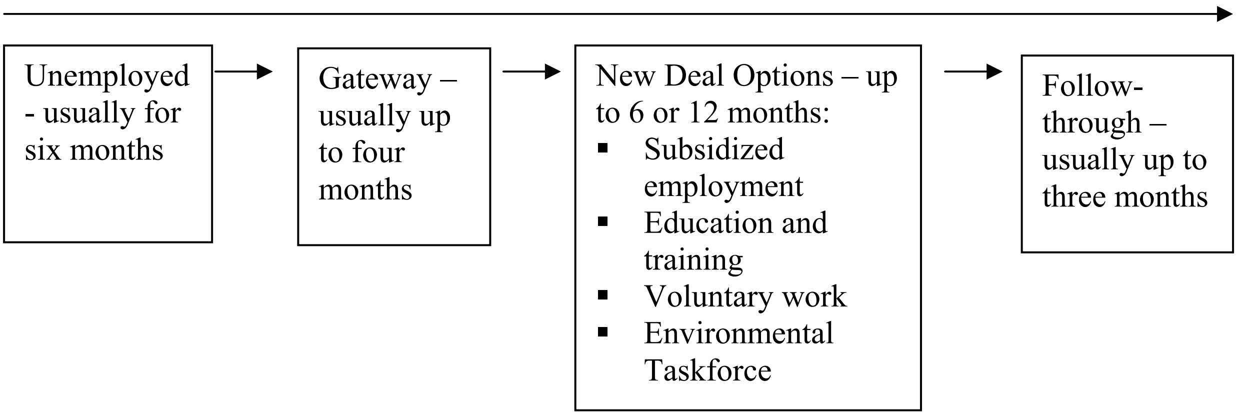
JSA are covered by the same conditionality in terms of demonstrating availability for work and providing evidence of job search activity, which is enforced via fortnightly, face-to-face signing interviews in the local benefit office (now called Jobcentre Plus offices).

Following the introduction of NDYP on 6th April 1998, a young person aged between 18 and 24 years that had been unemployed and claiming either form of JSA for six months had to report for a job search interview with a personal advisor or face benefit sanctions. Further meetings with the personal adviser, offering individually tailored job search assistance, followed. This was known as *Gateway* and was intended to last up to four months, but in some areas often lasted longer (see Dorsett, 2006). If at the end of that time the young person was still unemployed, a compulsory *Option* had to be taken up.[2] There were four Options: (i) full time education or government supported training courses, (ii) work placements in the voluntary sector or (iii) on the Environmental Taskforce (essentially low level public sector work experience), and (iv) subsidised jobs in the local labour market. Young people on an option were counted as having left registered unemployment although, with the exception of those in subsidised jobs, they still received benefits with a small supplement and were encouraged to continue job searching. If, after completing an Option, a young person was still without a job, they entered a *Follow-through* stage, went back on the unemployment register, and received further one-on-one job search assistance. If the young person was still unemployed after three months on Follow-through the clock started again from zero and a further six months of unemployment led to a second NDYP 'episode' with re-entry into Gateway. Figure 1 summarises. The programme

[2] To May 2009 there had been just over 2 million entrants to NDYP, one third of whom reached the Option stage of the programme.

ran uninterrupted across Britain until being replaced by Flexible New Deal in 2009 (see Department for Work and Pensions, 2007).

Fig 1: NDYP Timeline



NDYP has been the subject of numerous evaluations (e.g. Riley and Young, 2001; Wilkinson, 2003; White, 2004; Blundell et al. 2004; De Giorgi, 2005; Beale et al. 2008; McVicar and Podivinsky, 2009). Most have taken a partial equilibrium approach using the age restrictions of the policy to identify programme impacts. The benchmark study is Blundell et al. (2004) who also exploit the split between ten Pathfinder Areas which introduced NDYP in January 1998 and the rest of Britain which introduced NDYP in April 1998 to provide an alternative identification strategy for the programme's early stages. They present difference-in-differences (DID) estimates of the impact of NDYP participation on the probability of being unemployed or being employed by the end of the tenth month of unemployment, conditional on having been unemployed for six months, for young men.[3] They find evidence of large, statistically significant and reasonably robust NDYP participation

[3] Their main focus is on men because of differential trends for the two female age groups prior to the introduction of NDYP, although they do report estimates for women.

effects, e.g. a 20-40% increase in the probability of participants being employed ten months after the start of an unemployment spell. Some of this – they suggest around half – is accounted for by take up of NDYP Options. They find no evidence of 'threat' effects on those about to enter NDYP (see Black et al., 2003). Other evaluation studies have presented estimates broadly in line with Blundell et al. (2004).

White (2004), Dorsett (2006) and McVicar and Podivinsky (2009) all consider the possibility of heterogeneous NDYP effects across space, but with different foci. White (2004) finds heterogeneous NDYP impacts on unemployment exit rates according to the nature of the implementation of the programme in different areas, with greater 'work focus' leading to more positive impacts like Bloom et al. (2001). Dorsett (2006) compares outcomes for participants taking different routes through NDYP and finds higher rates of job entry for participants that stay longer on Gateway than for those entering options, with the exception of the subsidized employment option. He also finds that Gateway is more likely to be extended beyond four months in tighter labour markets. McVicar and Podivinsky (2009) find that the impacts of NDYP participation on hazard rates for a variety of exits from unemployment differ across twelve UK regions. For some exit destinations these differences appear weakly correlated with regional unemployment rates and/or average claimant characteristics at the regional level, but the study draws no firm conclusions in this respect. Other studies that consider such differences stop short of providing estimates of NDYP impacts against a defined counterfactual (e.g. Turok and Webster, 1998; Sunley et al., 2005).

5. The Data

In common with many existing evaluations of NDYP (e.g. Blundell et al., 2004) we use JUVOS data here[4]. JUVOS tracks all (claimant) unemployment spells and exit destinations for a five percent sample of the British working age population from 1996 onwards.[5] We restrict our attention to spells starting between 1st October 1996 and 31st December 2005 – with all spells ongoing as of 31st December 2005 treated as right-censored – and only for males aged 18-29 years at start of spell. This gives us information on 384,646 spells across 135,736 individuals, over a nine year period spanning the introduction of NDYP, for the eligible age group (18-24 year olds) and their closest comparators (25-29 year olds). This sample is larger and spans a longer time period than any of the previously published NDYP evaluations. As well as information on exit destination, the data include information on start date and end date of spell, age, occupation sought (interpreted as a proxy for education level, as in Dorsett (2006)), marital status, and – crucially – on the location of the benefit office at which the claimant is registered and their home postcode district.[6] This information allows us to assign individuals to Units of Delivery (UoDs) (the administrative units delivering NDYP) and to Travel to Work Areas (TTWAs) (geographical areas which

[4]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.

[5] Information is actually available on spells from 1982 but exit destinations are only added from 1996. The recording of exit destinations in the administrative data from which the JUVOS sample are drawn is not perfect, with many spells ending 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 and it has been suggested that around half of these are in fact exits to employment (National Audit Office, 2002). We test robustness to grouping these exits together with exits to employment or treating them as separate destinations.

[6] At the time of the introduction of NDYP there were close to 1,000 benefit offices across Britain and 3,000 Postcode Districts (the first three or four digits of a UK postcode).

approximate to local labour markets).[7] All these covariates are treated as time invariant within spells. Table 1 presents sample means and counts of spells for the full sample and for the two age groups pre and post NDYP.

Although the JUVOS data do not include information on whether individual claimants are participating in NDYP or not, given the mandatory nature of the programme we can *assume* that all JSA claimants in the relevant age group become NDYP participants on day one of their seventh month of unemployment. A similar approach is taken by Wilkinson (2003) and McVicar and Podivinsky (2009). In what follows participation in NDYP is therefore captured by a time varying binary dummy equal to one for those aged 18-24 years for that part of any unemployment spell beyond six months of duration and since 6th April 1998, and zero otherwise.[8] By examining outcomes at various different points after the beginning of the spell, or by right censoring unemployment spells at different durations, this dummy can be used to emphasise Gateway effects and Option entry to varying degrees, although they cannot be fully separated because some participants take up Options before completing four months of Gateway and Option take-up is treated as an exit from the current unemployment spell.

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[7] TTWAs are defined as geographical areas in which 75% or more of the resident employed population work, and are generally centered on larger towns or cities. Britain is divided into around 300 TTWAs areas and 144 UoDs.

[8] Our results are robust to inclusion or omission of Pathfinder areas that introduced NDYP three months earlier.

Table 1: Sample Means (Standard Deviations) and Other Descriptive Statistics

	All Spells	Pre NDYP Spells 18-24s	Pre NDYP Spells, 25-29s	Post NDYP Spells, 18-24s	Post NDYP Spells, 25-29s
Unemployment rate at start of spell, % (st. deviation)	3.44 (1.38)	4.68 (1.50)	4.70 (1.50)	3.15 (1.15)	3.13 (1.17)
Married/cohabit	.108	.115	.272	.053	.164
Age, years	23.2	21.1	27.1	21.0	27.1
Managerial job sought	.024	.025	.039	.017	.033
Professional job sought	.027	.023	.046	.018	.039
Associate professional job sought	.059	.054	.071	.052	.071
Administrative job sought	.111	.173	.125	.107	.087
Skilled trades job sought	.129	.147	.187	.114	.134
Personal service job sought	.044	.081	.067	.038	.034
Sales job sought	.088	.100	.058	.103	.062
Process/ operative job sought	.091	.099	.136	.072	.111
Past unemployment duration, days	251	33.4	33.1	251	404
No. complete spells > six months	77833	3962	2713	35861	24155
No. complete spells	384646	37517	23187	203081	105932
Duration of complete spells, days	111.4	75.0	77.4	92.5	125.4
Not unemployed after ten months, given unemployed after six months, % of spells	.602	.759	.747	.672	.462
Employed after ten months, given unemployed after six months, % of spells	.257	.390	.378	.266	.207

Notes: Pre-NDYP (post-NDYP) spells omit those ongoing as of 6/4/1998 (31/12/2005) for the purposes of counting number of complete spells, number of spells>six months and duration of complete spells. All other descriptive statistics are expressed as a proportion of spells (rather than as a proportion of individuals) and are based on observed covariates measured at start of spell (including right censored spells). Age groups are defined by age measured six months after start of spell.

There are two exceptions to the six months entry rule. First, some groups facing particular barriers to employment, with the largest being unemployed single parents, can *choose* to enter NDYP early (i.e. before being unemployed for six months). But by omitting females we minimise this problem.[9] Second, those re-entering unemployment directly from a NDYP option, or after a spell off JSA not long enough to 'set the clock back to zero', either enter the Follow Through stage of NDYP or go straight back into the Gateway stage without the six months wait. We treat all new spells of unemployment for 18-24 year olds that start within six months of a previous NDYP episode ending as being covered by these provisions, and we label them as Follow Through spells.[10]

Our measure of the state of the labour market is the local adult (claimant) unemployment rate, available monthly at the TTWA level, which we treat as exogenous. To keep the data manageable we use the unemployment rate at the start of the spell and treat it as time invariant within spells, although we test robustness to using the unemployment rate six months into spells instead. There is considerable variation in TTWA unemployment rates across space, e.g. ranging from 1% in Newbury to 8.3% in Hartlepool in April 1998. Although they tend to move quite slowly month on month, because of our nine year data coverage there is also substantial variation across time, with the (unweighted) average of these TTWA unemployment rates falling from 4.8% in October 1996 to 2.1% in December 2005.

6. Identification and Preliminary Estimates

First consider the identification of the NDYP participation treatment effect *not* differentiated by local unemployment rates. Because NDYP was not introduced

[9] We also make this restriction because Blundell et al. (2004) find that 18-24 year old and 25-29 year old females were following divergent trends prior to the introduction of NDYP.
[10] As a test of robustness we also re-estimate with all spells for an individual following a first spell in NDYP dropped from the sample.

experimentally there is no readymade control group to help us evaluate its effects on participants. Most NDYP evaluations have dealt with this problem by using the age-based eligibility rules of NDYP to define a treatment group (18-24 year olds with unemployment spells of at least six months duration) and a comparison group (25-29 year olds, 25-30 year olds or 30-39 year olds, with unemployment spells of at least six months duration) and using some variant of DID to estimate treatment effects on the treated. We adopt a similar approach here in a duration analysis framework.

The validity of the age-based approach is discussed elsewhere (e.g. Blundell et al., 2004), but briefly it requires that the two age groups had been following similar trends prior to the introduction of the NDYP, and that NDYP does not lead to significant inter age group substitution effects. Blundell et al. (2004) show both assumptions to be supported by the JUVOS data for males. Ideally our treatment and comparison groups would also be similar in all other respects. But unemployed 18-24 year olds and unemployed 25-29 year olds are unavoidably different, in age but also in characteristics associated with age such as marital status and accumulated unemployment experience. Further, our duration analysis approach means that unemployment spells shorter than six months for 18-24 year olds can also contribute to the comparison group. For these reasons, together with the mandatory nature of NDYP participation for the target group and our desire to keep things simple so as to focus on differences across labour markets, we stop short of using matching methods on our sample. We do, however, control for the observable characteristics in the JUVOS data along with dummies for UoDs, time quadratics specific to each age group and, in one robustness test, random intercepts for individual time invariant unobserved heterogeneity.

The JUVOS data allow us to examine NDYP impacts on a number of outcomes of interest. Because we know the duration of all (uncensored) unemployment spells in our sample to the nearest day we can estimate the impact of NDYP participation on the daily hazard rate for exiting unemployment using a duration analysis approach. We can also exploit the exit destination data in JUVOS to estimate the NDYP impact on the job entry hazard, which as for Blundell et al. (2004) includes entry to 'NDYP Option' jobs. Other studies of ALMP impacts to adopt such a duration analysis approach include Jensen et al. (2003) and McVicar and Podivinsky (2009). We also follow Blundell et al. (2004) by estimating probits for the NDYP impacts on the probability of no longer being unemployed and the probability of being employed ten months after starting an unemployment spell, conditional upon having been unemployed for six months, to test robustness.[11]

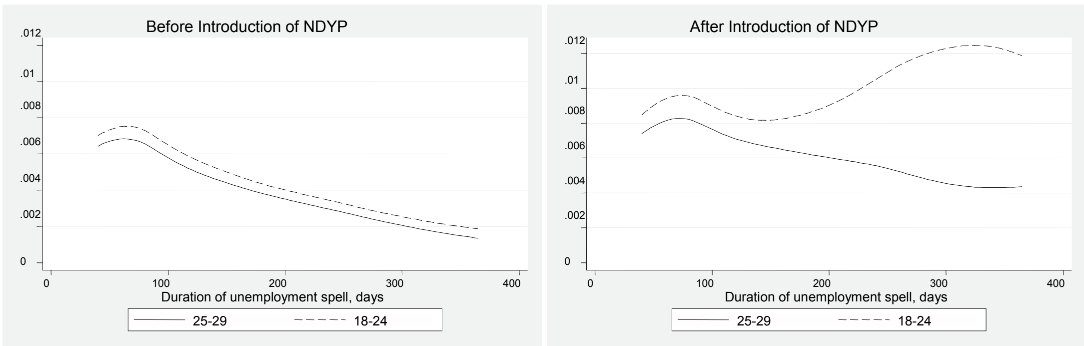
Because NDYP is a *mandatory* programme for 18-24 year olds unemployed for six months the standard selection problem associated with individuals *choosing* (or being chosen) to participate in the ALMP is much diminished (for a discussion of selection issues in this context see Heckman et al., 1999). But to ignore selection issues altogether assumes that NDYP has no impact on outcomes at less than six months duration. But claimants in the eligible age group may cease to claim in the month prior to NDYP entry in order to avoid the programme, e.g. because they receive a letter summoning them to participate or simply because they know such a letter is imminent (see Blundell et al., 2004). The evidence on the existence and sign of such effects in the case of NDYP is mixed, with different studies finding zero or small

[11] Like Blundell et al. (2004) we assume those that exited unemployment to employment and have not re-entered unemployment at the point of measurement are still employed.

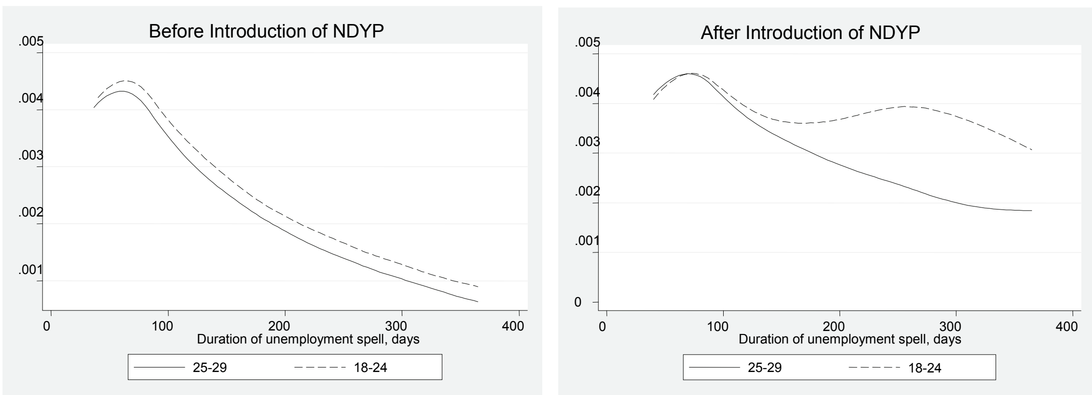
effects of either sign prior to six months duration (see Riley and Young, 2001; Wilkinson, 2003; Blundell et al., 2004; De Giorgi, 2005; McVicar and Podivinsky, 2009). Here, because hazard rates *beyond* a certain month are in any case conditioned on *reaching* that month, we simply test robustness to defining the NDYP treatment as starting after five months rather than six months. For the probits we follow Blundell et al. (2004) and condition explicitly on reaching the seventh month of unemployment.

Figure 2: Kaplan-Meier Hazard Functions by Age Group, Pre & Post NDYP

(a) Unemployment Exit



(b) Job Entry



Kaplan-Meier (KM) hazard functions for 18-24 year olds and 25-29 year olds before and after NDYP, for unemployment exit and for job entry, are shown in Figure 2. They give a clear visual representation of the impact of NDYP on the younger age group: from very similar shaped KM hazards prior to NDYP we move to very different KM hazards after NDYP, with the divergence beginning at just under 200 days, i.e. at the time of entry to NDYP.

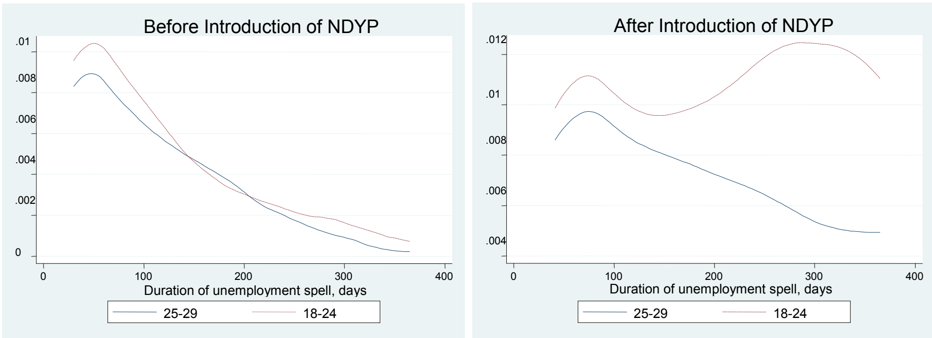
Table 1 (towards the bottom) reports mean unemployment spell duration for each age group before and after the introduction of NDYP. For 18-24 year olds we expect unemployment durations to be shorter, on average, for spells starting after the introduction of NDYP than for spells starting before the introduction of NDYP. This is obscured in the table, however, because the mean durations treat spells starting before NDYP as right censored at the date of its introduction and because the sample period before the introduction of NDYP is considerably shorter than the period following the introduction of NDYP. But the *difference* in average uncensored spell duration between the before and after periods is much greater for the 25-29 year old age group than for the 18-24 year olds age group, with a simple unconditional DID estimate suggesting spell duration for 18-24s following the introduction of NDYP is 30.5 days shorter than it would otherwise have been. Similar DID estimates of the impact of NDYP participation on the probability of not being unemployed (being employed) after ten months, conditional on being unemployed after six months, suggest a 20 (five) percentage point increase.

Now consider identification of heterogeneous NDYP participation impacts across labour markets. We explore this by including an interaction term between NDYP participation and the local unemployment rate in the various specifications of the empirical model. Jurajda and Tannery (2003) adopt a similar strategy. To allow this relationship to be non-monotonic we also interact the NDYP participation dummy with the square of the local unemployment rate, and we test sensitivity to adding further powers.

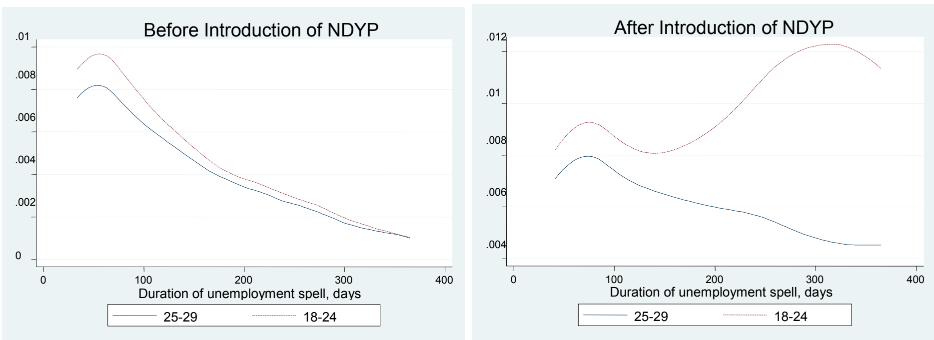
Can we pick up systematic variation of NDYP impacts with local unemployment rates in the raw data? Figures 3 and 4 present KM hazards for the two age groups, before and after the introduction of NDYP, for exits from unemployment and for job entry, but now separately by TTWA unemployment rate quartiles. All show similar patterns to those in Figure 2, but the gaps between the post-NDYP hazards for job entry (Figure 4) are larger for middle unemployment quartiles than for the highest and lowest unemployment quartiles. This is suggestive of an inverse-U shaped relationship between NDYP treatment effect and local unemployment rate. There is a similar suggestion in the KM hazards for exit from unemployment, although it's not so clear (see Figure 3).

Figure 3: Kaplan-Meier Hazard Functions by Age Group & TTWA Unemployment Rate Quartile, Pre & Post NDYP, Unemployment Exit

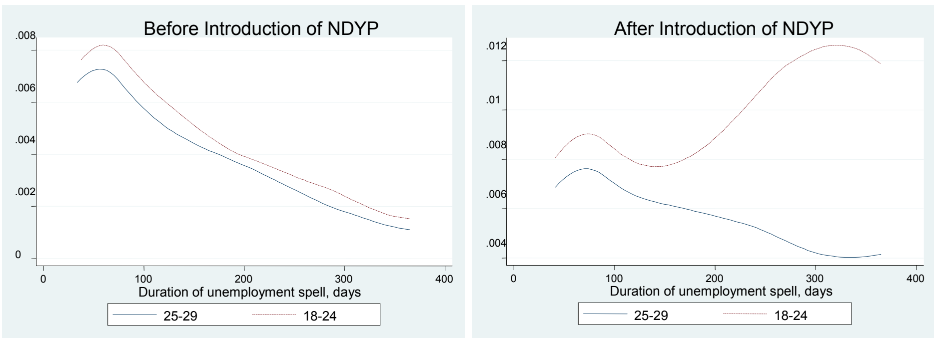
(a) Lowest Unemployment Quartile



(b) Unemployment Quartile 2



(c) Unemployment Quartile 3



(d) Highest Unemployment Quartile

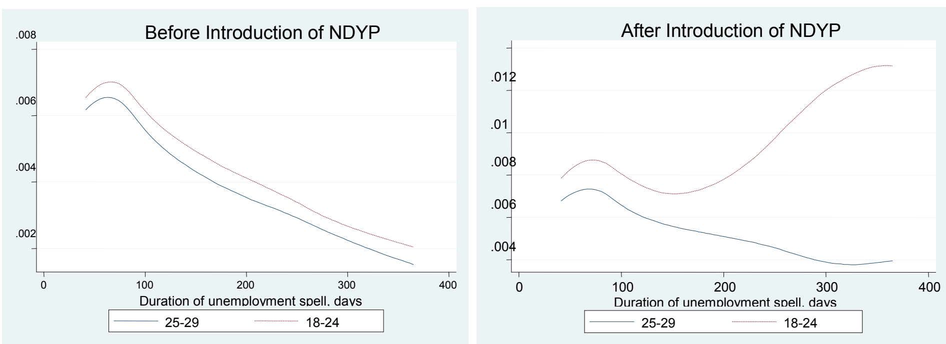
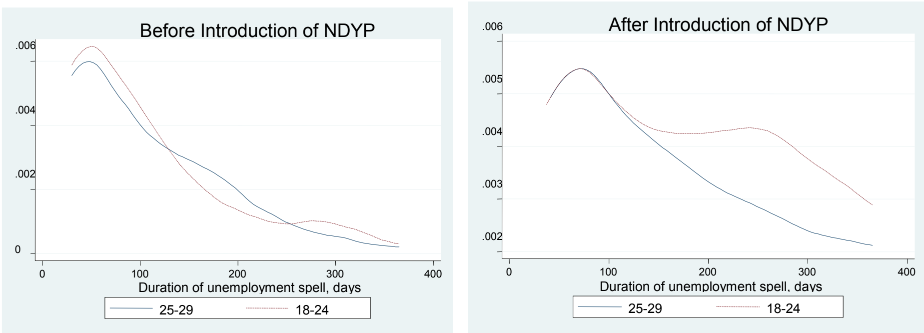
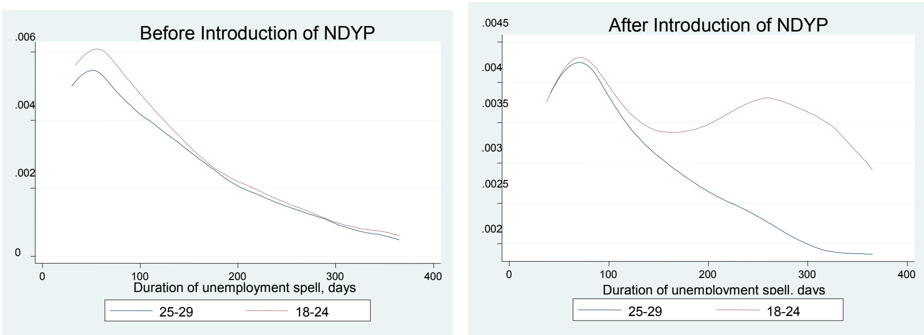


Figure 4: Kaplan-Meier Hazard Functions by Age Group & TTWA Unemployment Rate Quartile, Pre & Post NDYP, Job Entry

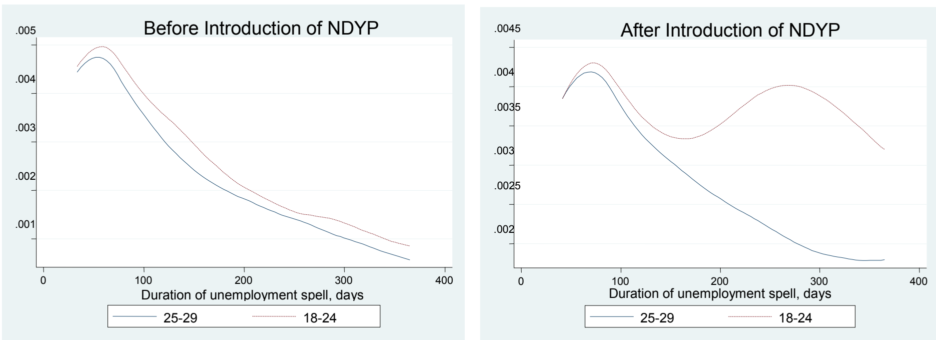
(a) Lowest Unemployment Quartile



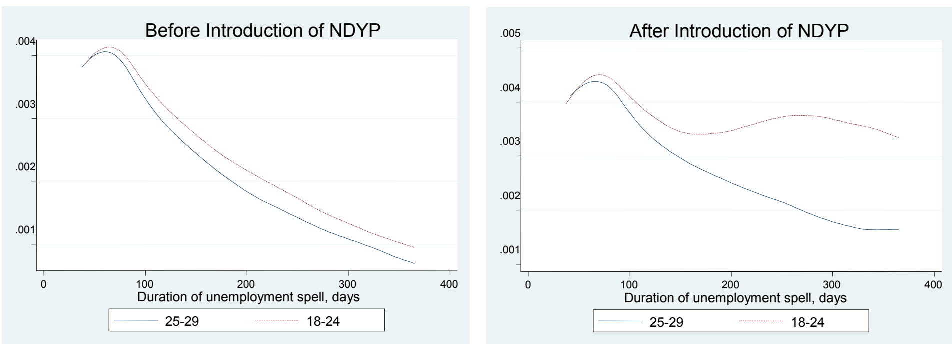
(b) Unemployment Quartile 2



(c) Unemployment Quartile 3



(d) Highest Unemployment Quartile



The final step in the identification process is to control as far as possible for other dimensions along which NDYP treatment effects might systematically vary. First consider differences in provision across the UoDs. We know from White (2004) that work-focused UoDs increase the subsequent chances of unemployment exit relative to 'free option choice' UoDs, and from Dorsett (2006) that this UoD focus itself partly reflects the state of the local labour market. So differences in provision may confound the NDYP-unemployment rate interaction coefficients if not controlled for. In Section 8 we therefore also interact the NDYP participation dummy with dummies for the 144 UoDs. Second consider the possibility of heterogeneous NDYP impacts across individuals with different characteristics. To the extent that such characteristics are correlated with local unemployment rates they may also confound the estimated NDYP*unemployment interactive effects if not accounted for. We therefore include interactives of the NDYP participation dummy with all the *observed* characteristics covariates, making a reasonable conjecture about the likely magnitude of heterogeneous NDYP impacts across *unobserved* participant characteristics.

7. Variation in NDYP Impacts by Unemployment Rate

In common with much of the empirical unemployment duration literature, we take a reduced form Cox Proportional Hazards (CPH) approach to estimation (see van den Berg, 2001) as given below:

$$\begin{aligned} h(t) = h_0(t)\exp(&\beta_1 x_1 + \ldots + \beta_N x_N + \varphi U + \gamma_1 UoD_1 + \ldots + \gamma_{144} UoD_{144} + \delta_1 NDYP + \delta_2 NDYP * U \\ &+ \delta_3 NDYP * U^2 + \delta_4 FOLLOW) \end{aligned} \quad (1)$$

In (1), $h(t)$ is the hazard rate, $h_0(t)$ is the baseline hazard, *NDYP* is a binary dummy for NDYP participation, *FOLLOW* is a binary dummy for participation in Follow Through, U denotes the local unemployment rate, $x_1 \ldots x_N$ are observed individual characteristics and UoD_1 *to* UoD_{144} are the UoD dummies. Note that we do not control for differences in NDYP impacts by observed characteristics or by local differences in provision. For tractability, and in common with most applications of duration analysis where more than one type of 'failure' (exit destination) is possible, we assume independent competing risks when estimating the hazard for job entry (in practice, spells with any other exit destination are treated as right censored). The daily data are treated as continuous and all spells are treated as right censored after twelve months.

We adopt the CPH model because it allows for flexibility in the shape of the baseline hazard (again see van den Berg, 2001). The cost to this flexibility is that, unlike in a parametric mixed proportional hazard (MPH) model, we cannot explicitly control for unobserved heterogeneity at the individual level with such a large data set (see Han and Hausman, 1990), although we do test sensitivity to this by also estimating (1) as an MPH model combining a Weibull baseline with gamma-distributed unobserved heterogeneity.

The CPH estimates for the unemployment exit hazard function are presented in Table 2. Estimates from five variants of the model are presented, although we leave discussion of models 4 and 5 for Section 8. Results are presented in coefficient form, i.e. (some of) the βs and δs from the above equation, and are interpretable as semi-elasticities.

First consider Model 1. The estimates suggest that participation in NDYP increases the unemployment exit hazard rate by 50%, i.e. a large, positive and highly statistically significant NDYP treatment effect. Note that this combines Gateway effects on ‘non-Option’ exits together with Option entry effects given we have only right censored spells after twelve months. Participation in Follow Through also has a positive and statistically significant impact on the hazard, but of smaller magnitude. The local unemployment rate is negatively and significantly correlated with the hazard rate and all observed individual characteristics covariates are statistically significant with the anticipated signs. Including UoD dummies and time quadratics specific to the 18-24 age group and the 25-29 age group (Model 2) makes little difference to these estimates, although the magnitude of the NDYP and Follow Through coefficients falls slightly.

Table 2: Cox PH Estimates, Unemployment Exit, Coefficients (St. Errors)

	Model 1	Model 2	Model 3	Model 4	Model 5
NDYP	.503*** (.009)	.446*** (.009)	.213*** (.038)	.300*** (.039)	.747*** (.063)
NDYP* Unemp. rate			.151*** (.020)	.153*** (.020)	-.003 (.025)
NDYP* Unemp. rate^2			-.021*** (.003)	-.022*** (.003)	-.011*** (.003)
NDYP*age				.016*** (.003)	.020*** (.003)
NDYP*cohabit				-.085*** (.017)	-.080*** (.017)
NDYP*managerial				-.274*** (.050)	-.265*** (.050)
NDYP*professional				-.336*** (.049)	-337*** (.049)
NDYP*associate prof				-.137*** (.026)	-.135*** (.026)
NDYP*admin				-.176*** (.019)	-.153*** (.019)
NDYP*skilled trade				-.133*** (.018)	-.120*** (.019)
NDYP*personal service				-.170*** (.029)	-.135*** (.029)
NDYP*sales				-.044** (.020)	-.052*** (.020)
NDYP*process ops.				-.051** (.022)	-.038* (.022)
NDYP*past unemp. duration				-.0002*** (.00002)	-.0002*** (.00002)
NDYP*UoD dummies	No	No	No	No	Yes***
Follow Through	.108*** (.010)	.100*** (.010)	.100*** (.010)	.095*** (.010)	.096*** (.010)
Unemployment rate	-.092*** (.002)	-.058*** (.003)	-.057*** (.003)	-.057*** (.003)	-.052*** (.003)
Characteristics controls	Yes***	Yes***	Yes***	Yes***	Yes***
UoD dummies	No	Yes***	Yes***	Yes***	Yes***
Age-group specific time quadratics	No	Yes***	Yes***	Yes***	Yes***
No. Spells	384646	384646	384646	384646	384646
No. Failures	353826	353826	353826	353826	353826
No. Individuals	135736	135736	135736	135736	135736
Log pseudo-likelihood	-4249445	-4244890	-4244858	-4244727	-4244453

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. NDYP is a binary dummy for NDYP participation which equals 1 for participants during those parts of unemployment spells covered by NDYP and 0 otherwise. Follow Through is a binary dummy for participation in the Follow Through stage of NDYP, equal to 1 for the first three months of any unemployment spell starting within 6 months of a previous spell in Gateway, for the relevant age group. Standard errors are clustered at the individual level.

Our primary interest is not in the impact of NDYP participation *per se*, however, but in whether/how it varies with local unemployment rates. Model 3 adds the quadratic NDYP*unemployment terms to Model 2.[12] The coefficient on the interactive term in the level of unemployment is positive and highly statistically significant, and the coefficient on the interactive term in the square of unemployment is negative and highly statistically significant. In other words Model 3 suggests an inverse-U shaped relationship between the impact of NDYP participation on the unemployment exit hazard rate and the unemployment rate. The magnitude of this non-linear interactive effect is large: combining the coefficients from the standalone and interactive NDYP dummies suggests participation in NDYP increases the hazard by 48% at the mean unemployment rate but only by 44% at plus or minus one standard deviation. Figure 5 plots the estimated participation effect on the hazard across unemployment rates ranging from two standard deviations below to two standard deviations above the mean, and also shows robustness to inclusion of higher order interactive terms up to an order five polynomial.

To further test the sensitivity of this basic result the first two columns of Table 3 present estimates of the key parameters for a number of model/sample variations: replacing the unemployment rate at start of spell with unemployment rate six months after the start of spell; excluding those with a previous NDYP episode; excluding Pathfinder areas; specifying the NDYP treatment as starting after five months rather than after six months; right-censoring unemployment spells at ten, nine and eight months rather than at twelve months; comparing only 24 and 25 year olds to focus on differences closer to the age-based discontinuity in programme eligibility; estimating

[12] The coefficient of the standalone NDYP dummy can now be interpreted as the impact of NDYP in a labour market with a zero unemployment rate.

a MPH model with Weibull baseline and gamma unobserved heterogeneity in place of the CPH model; and finally, the marginal effects from a probit model of the probability of being not unemployed ten months after the start of an unemployment spell conditional on still being unemployed after six months. Although precise magnitudes vary, all but one of these variants suggest a similar inverse-U shaped relationship between the impact of NDYP participation on the hazard for unemployment exit and the unemployment rate.

Figure 5: Sensitivity of NDYP Impact on the Hazard Rate for Unemployment Exit by TTWA Unemployment Rate, NDYP Interacting with Different Order Unemployment Rate Polynomials, Model 3

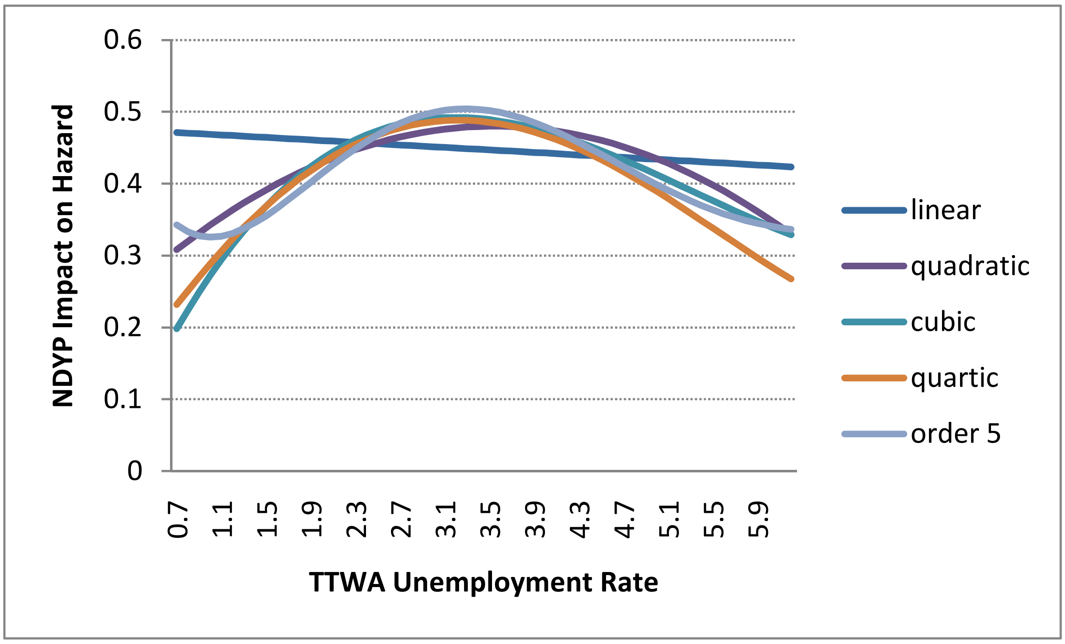


Table 3: Sensitivity Analysis, Unemployment Exit, Models 3 & 5

	Model 3		Model 5	
	NDYP* Unemp. rate	NDYP* Unemp. rate^2	NDYP* Unemp. rate	NDYP* Unemp. rate^2
CPH, as reported in Table 3	.151*** (.020)	-.021*** (.003)	-.003 (.025)	-.011*** (.003)
CPH, NDYP interacted with unemployment rate at 6 months	.163*** (.021)	-.023*** (.003)	-.011 (.027)	-.011*** (.003)
CPH, excluding those with a previous NDYP episode	.114*** (.021)	-.017*** (.003)	-.021 (.027)	-.006* (.003)
CPH, excluding Pathfinder Areas	.153*** (.021)	-.020*** (.003)	.016 (.027)	-.010*** (.003)
CPH, NDYP = 1 after 5 months	.102*** (.017)	-.015*** (.002)	-.032 (.021)	-.005* (.003)
CPH, spells right censored at 10 months	.126*** (.024)	-.020*** (.003)	-.025 (.031)	-.009** (.004)
CPH, spells right censored at 9 months	.118*** (.027)	-.020*** (.004)	-.029 (.034)	-.010** (.004)
CPH, spells right censored at 8 months	.124*** (.032)	-.023*** (.004)	-.059 (.042)	-.010* (.005)
CPH, 24 year olds vs. 25 year olds only	.199** (.082)	-.028*** (.011)	.090 (.110)	-.026* (.014)
MPH model, Weibull baseline, gamma-distributed unobserved heterogeneity	-.010 (.007)	-.009*** (.001)	.025 (.030)	-.032*** (.004)
NDYP* Unemp. Rate polynomials order 1-5	See Figure 5		See Figure 5	
Probit marginal effects for probability of not being unemployed after 10 months, conditional on being unemployed after 6 months	.026*** (.003)	-.005*** (.0004)	.007** (.003)	-.006*** (.0005)

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. Standard errors in parentheses are clustered at the individual level. For the probit models, the estimates are presented as marginal effects and are interpretable as the percentage point impact on the outcome variable of a one unit change in the relevant explanatory variable calculated at the sample mean, with spells for those with previous NDYP episodes are excluded. Age, married/cohabit and age group specific quadratics in time are included in the selection probit but excluded from the outcome probit. The correlation coefficients (p-values) between the selection and outcome probits are .918*** (.013) and .885*** (.016) respectively.

Now consider the CPH estimates for the job entry hazard presented in Table 4. Again, estimates from five variants of the model are presented, with discussion of models 4 and 5 left for Section 8. Models 1 and 2 suggest NDYP participation also boosts the hazard rate for job entry by around 40%, although participation in Follow Through is correlated with a *lower* job entry hazard. Turning to Model 3, again we see a positive and highly statistically significant coefficient on the interactive term in the level of unemployment and a negative and highly statistically significant coefficient on the interactive term in the square of unemployment. As for the unemployment exit hazard, the magnitude of this interactive effect is large: combining the coefficients from the standalone and interactive NDYP dummies suggests participation in NDYP increases the hazard by 44% at the mean unemployment rate but only by 40% at plus or minus one standard deviation. Figure 6 plots the estimated participation effect on the hazard across unemployment rates ranging from two standard deviations below to two standard deviations above the mean, and again shows robustness to inclusion of higher order interactive terms.

Figure 6: Sensitivity of NDYP Impact on the Hazard Rate for Job Entry by TTWA Unemployment Rate, NDYP Interacting with Different Order Unemployment Rate Polynomials, Model 3

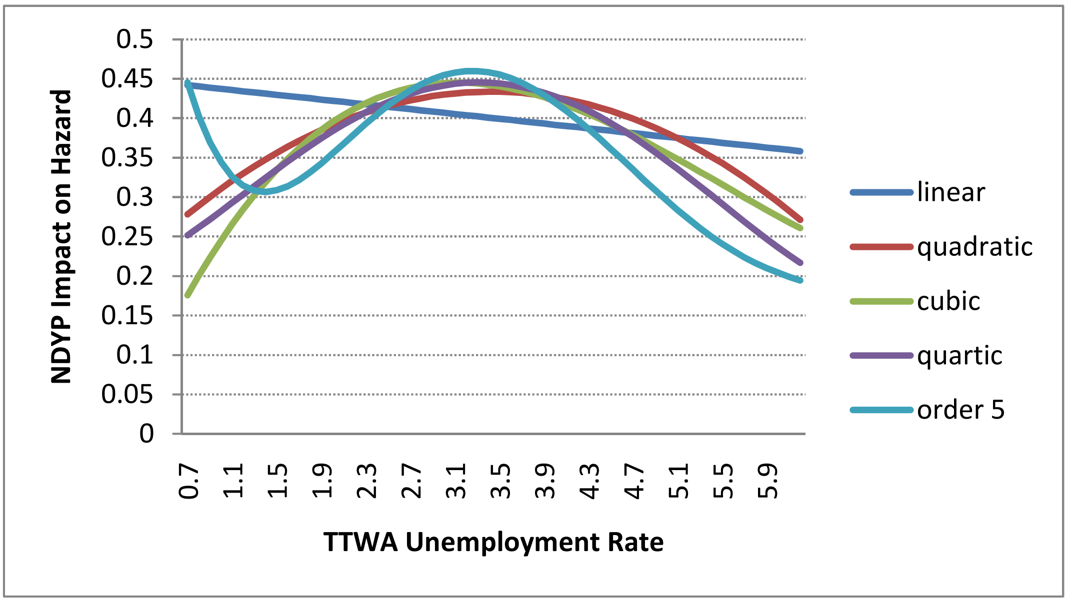


Table 4: Cox PH Estimates, Job Entry, Coefficients (St. Errors)

	Model 1	Model 2	Model 3	Model 4	Model 5
NDYP	.453*** (.014)	.398*** (.014)	.188*** (.065)	.184*** (.068)	.497*** (.110)
NDYP* Unemp. rate			.144*** (.035)	.151*** (.035)	.025 (.044)
NDYP* Unemp. rate^2			-.021*** (.005)	-.022*** (.005)	-.011** (.005)
NDYP*age				.033*** (.005)	.035*** (005)
NDYP*cohabit				-.103*** (.028)	-.096*** (.028)
NDYP*managerial				-.135* (.076)	-.139* (.076)
NDYP*professional				-.213*** (.075)	-.223*** (.075)
NDYP*associate prof				-.128*** (.044)	-.133*** (.045)
NDYP*admin				-.213*** (.032)	-.211*** (.033)
NDYP*skilled trade				-.143*** (.032)	-.133*** (.032)
NDYP*personal service				-.157*** (.049)	-.140*** (.049)
NDYP*sales				-.068* (.035)	-.088** (.035)
NDYP*process ops.				-.055 (.038)	-.044 (.038)
NDYP*past unemp. duration				-.0001*** (.00003)	-.0001*** (.00003)
NDYP*UoD dummies			No	No	Yes***
Follow Through	-.223*** (.018)	-.217*** (.018)	-.216*** (.018)	-.217*** (.018)	-216*** (.018)
Unemployment rate	-.097*** (.002)	-.073*** (.004)	-.072*** (.004)	-.072*** (.004)	-.070*** (.004)
Characteristics controls	Yes***	Yes***	Yes***	Yes***	Yes***
UoD dummies	No	Yes***	Yes***	Yes***	Yes***
Age-group specific time quadratics	No	Yes***	Yes***	Yes***	Yes***
No. Spells	384646	384646	384646	384646	384646
No. Failures	177595	177595	177595	177595	177595
No. Individuals	135736	135736	135736	135736	135736
Log pseudo-likelihood	-2140712	-2133167	-2133154	-2133103	-2132936

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. Standard errors are clustered at the individual level.

Table 5 shows sensitivity to model/sample variations, with an additional variant that reclassifies all exits to unknown destinations as exits to employment. As before, all but one of these variants suggest a similar inverse-U shaped relationship between the impact of NDYP participation on the hazard for job entry and the unemployment rate.

Table 5: Sensitivity Analysis, Job Entry, Models 3 & 5

	Model 3		Model 5	
	NDYP* Unemp. rate	NDYP* Unemp. rate^2	NDYP* Unemp. rate	NDYP* Unemp. rate^2
CPH, as reported in Table 4	.144*** (.035)	-.021*** (.005)	.025 (.044)	-.011** (.005)
CPH, NDYP interacted with unemployment rate at 6 months	.156*** (.036)	-.023*** (.005)	.019 (.046)	-.011* (.006)
CPH, excluding those with a previous NDYP episode	.118*** (.037)	-.017*** (.005)	.004 (.047)	-.006 (.006)
CPH, excluding Pathfinder Areas	.150*** (.037)	-.021*** (.005)	.036 (.046)	-.011* (.006)
CPH, NDYP = 1 after 5 months	.073** (.029)	-.012*** (.004)	-.031 (.035)	-.002 (.004)
CPH, spells right censored at 10 months	.106*** (.039)	-.017*** (.005)	-.001 (.049)	-.008 (.006)
CPH, spells right censored at 9 months	.094** (.042)	-.016*** (.006)	-.008 (.054)	-.008 (.007)
CPH, spells right censored at 8 months	.067 (049)	-.013** (.006)	-.077 (.063)	-.002 (.008)
CPH, 24 year olds vs. 25 year olds only	.313** (.146)	-.043** (.019)	.104+ (.122)	-.029*+ (.015)
CPH, exits to employment + exits to unknown destination	.154*** (.026)	-.023*** (.003)	.009 (.031)	-.013*** (.004)
MPH model, Weibull baseline, gamma-distributed unobserved heterogeneity	.180*** (.038)	-.034*** (.005)	.080* (.047)	-.036*** (.006)
NDYP* Unemp. Rate polynomials order 1-5	See Figure 6		See Figure 6	
Probit marginal effects for probability of being employed after 10 months, conditional on being unemployed after 6 months	.018*** (.003)	-.003*** (.0004)	.014*** (.003)	-.003*** (.0004)

Notes: ***, ** and * denote statistical significance at 99%, 95% and 90% respectively. Standard errors in parentheses are clustered at the individual level. For the probit selection models the correlation coefficients (p-values) between the selection and outcome probits are .580*** (.054) and .617*** (.056) respectively. + based on sample restricted to those aged between 23.5 and 26.5 years at start of spell.

Recall in Section 2 that a number of different mechanisms – related to the availability and quality of vacancies, to the characteristics of the individual, and to differences in the delivery of the programme in different localities – may drive these results. (So far we have not controlled for heterogeneous impacts along the latter two dimensions.) This is also the case for the Kluve (2006) estimates (no relationship), and for some Lechner and Wunsch (2009) estimates (positive relationship). If we drop the squared interactive term and include only the interactive term in levels, we obtain either no relationship (exits from unemployment) or a negative relationship (job entry) between programme impact and unemployment rate (see Figures 5 and 6). The implication is that differences in specification along linear/non-linear lines are not sufficient to drive these contrasting results.

8. Controlling for Differential Impacts Along Other Dimensions

Here we do control for heterogeneous programme impacts by participant characteristics and by differences in provision, more in line with the Lechner and Wunsch (2009) study. We do so in two steps: first including covariates interacting the NDYP dummy with observed participant characteristics (Model 4); second including these interactive covariates alongside covariates interacting the NDYP dummy with the set of UoD dummies to capture differences in programme provision (Model 5).

The corresponding hazard rate (for model 5) is given by the following:

$$\begin{aligned} h(t) = h_0(t)\exp(&\beta_1 x_1 + \ldots + \beta_N x_N + \varphi U + \gamma_1 UoD_1 + \ldots + \gamma_{144} UoD_{144} + \delta_1 NDYP + \delta_2 NDYP * U + \delta_3 NDYP * U^2 \\ &+ \delta_4 FOLLOW + \theta_1 NDYP * x_1 + \ldots + \theta_N NDYP * x_N + \eta_1 UoD_1 * NDYP + \ldots + \eta_{143} UoD_{143} * NDYP) \end{aligned} \quad (2)$$

CPH estimates for Model 4 are presented in Table 2 column four (for exits from unemployment) and Table 4 column four (for job entry). In both cases the additional interactives are all (or almost all) statistically significant, suggesting that NDYP participation has a bigger impact on the unemployment exit/job entry hazard for older members of the 18-24 age group compared to younger ones; for singles; for those seeking unskilled jobs; and for those with less previous experience of unemployment. The coefficients on the NDYP*unemployment interactive terms barely change, suggesting the inverse-U shape found in both versions of Model 3 is not driven by differences in the characteristics of unemployed young people across labour markets.[13] Given this robustness it also seems unlikely that the inverse-U shaped relationships found in Section 7 were driven by heterogeneous programme impacts across *unobserved* individual characteristics.

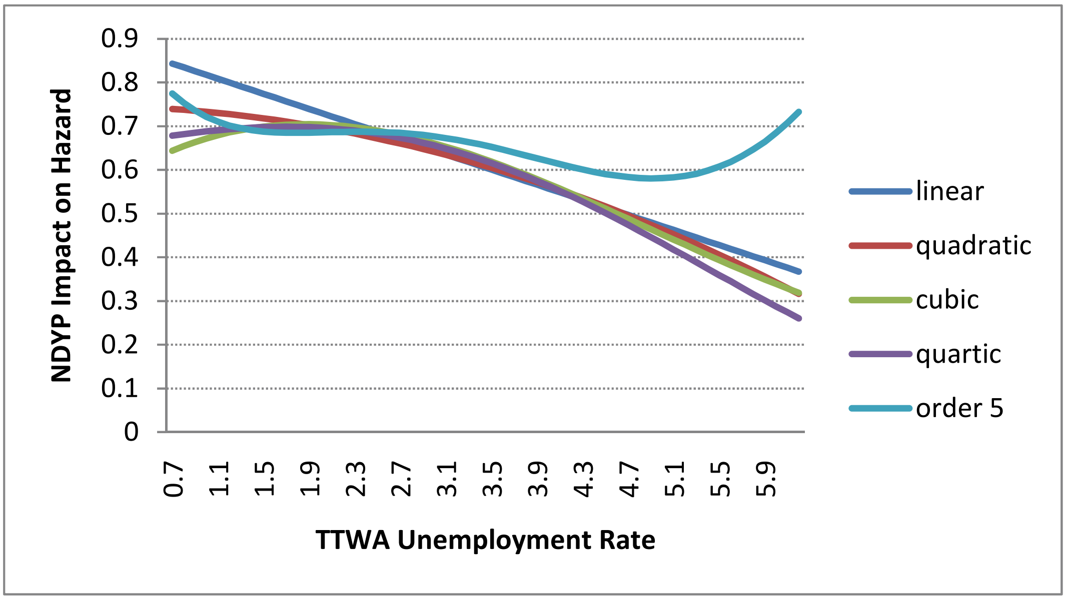
The equivalent estimates for Model 5 are presented in Table 2 column five (exits from unemployment) and Table 4 column five (job entry). In both cases the interactive NDYP*UoD dummies are jointly statistically significant, suggesting either that differences in the way NDYP is provided across UoDs lead to substantial variation in its impacts, or that the UoD dummies are capturing some other unobserved variation across space. We cannot rule out that the NDYP*UoD interactive dummies are partly capturing variation in programme impacts with unemployment rates, but because UoD geographies differ from TTWA geographies – there are twice as many TTWAs as there are UoDs – there is still substantial variation in unemployment rates across space within UoDs as well as across time.[14]

The inclusion of the NDYP*UoD interactive dummies has a very interesting effect on the coefficients on the interactive NDYP*unemployed terms in levels and squares.[15] In both cases the levels term is much smaller in magnitude and no longer statistically

[13] The coefficient on the standalone NDYP dummy now (somewhat artificially) captures the impact of NDYP participation for an individual with all covariates set equal to zero.
[14] The standard deviation for unemployment rates across the whole sample is 1.38 compared to an average standard deviation in unemployment rates within UoDs of 1.00.
[15] The coefficient on the standalone NDYP dummy now captures the impact of NDYP participation for an individual with all covariates set equal to zero and in the omitted UoD (Birmingham).

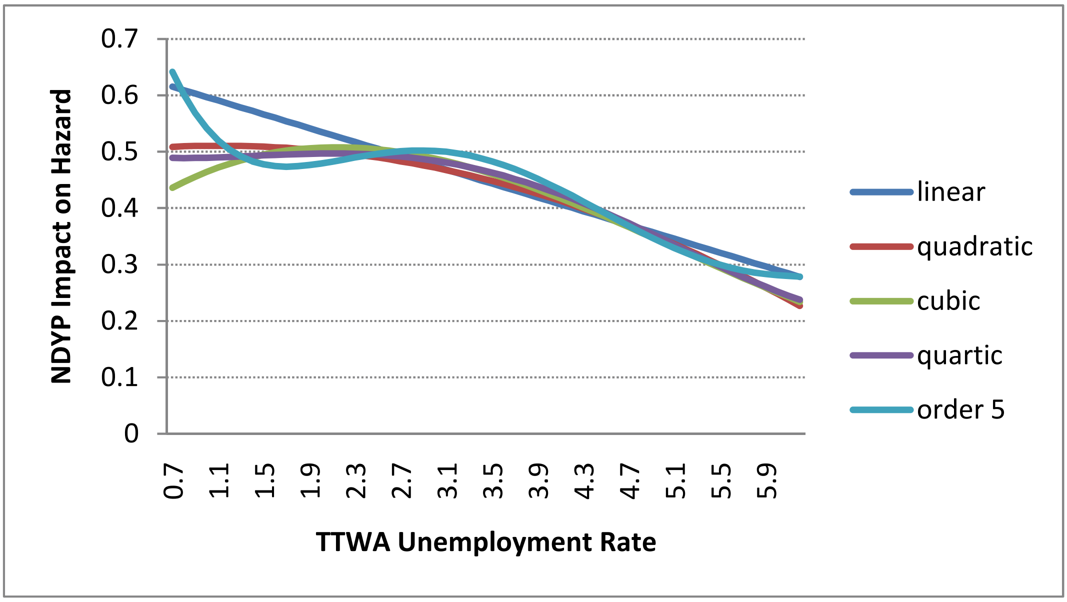
significant, and the negative coefficient on the squared interactive term halves in magnitude but remains statistically significant. So rather than inverse-U shaped relationships between programme impacts and unemployment rate we obtain negative relationships that, at least around the mean, are quite well approximated by straight lines, even when we include higher order interactive terms (see Figures 7 and 8). The magnitude of the variation is still large: moving up the unemployment rate distribution from the mean by one standard deviation reduces the programme impact on the hazard for unemployment exit from 61% to 49% and for job entry from 45% to 36%.[16] But the suggestion is that some of the variation in programme impacts that we previously put down to variation in unemployment rates, particularly at the lower end of the unemployment rate distribution, is in fact driven by variation in programme provision that is itself correlated with unemployment rates.

Figure 7: Sensitivity of NDYP Impact on the Hazard Rate for Unemployment Exit by TTWA Unemployment Rate, NDYP Interacting with Different Order Unemployment Rate Polynomials, Model 5



[16] Remember these are for a particular (rather strange) individual with all covariates set to zero and in Birmingham UoD.

Figure 8: Sensitivity of NDYP Impact on the Hazard Rate for Job Entry by TTWA Unemployment Rate, NDYP Interacting with Different Order Unemployment Rate Polynomials, Model 5



Columns three and four of Tables 3 and 5 present estimates from variations of the sample and model, as described in the previous section, to explore the robustness of these results. First consider the unemployment exit hazard: in all versions of the hazard model the levels interactive term is statistically insignificant and the squared interactive term is significant and negative. In the probit for not being unemployed after ten months the levels interactive term is positive and statistically significant at the 95% level. So although we cannot entirely rule out the possibility that we still have an inverse-U shaped relationship between programme impact and unemployment rates, the weight of the evidence points towards something close to a linear negative relationship. The estimates for the different versions of the job entry hazard model are more mixed, with some variants of the model suggesting no relationship, or only a marginally significant relationship, with the squared interactive term.

The negative relationship between programme impacts and unemployment rates suggests that the dominant mechanism, once heterogeneous impacts by individual characteristics and difference in provision are controlled for, is the relative lack of vacancies in slack local labour markets. In this respect our results are more in line with Bloom et al. (2001) than Lechner and Wunsch (2009). Given the sensitivity of the job entry result, however, we cannot *entirely* rule out the possibility that there is no relationship between unemployment rates and programme impacts on job entry, other things being equal. This is more in line with Kluve (2006) and Lechner and Wunsch (2009). If this is the case then our downward sloping relationship between programme impact and unemployment exit must be at least partly driven by differential impacts of NDYP participation on exits to destinations *other than* employment, e.g. to education and training. We know that such exits, particularly through the full time education and training Option, have been a key part of NDYP's overall impact (e.g. Wilkinson, 2003; McVicar and Podivinsky, 2009). Given poorer job prospects, are young people in slack labour markets less willing to take this Option – longer in duration than the other Options – than those in tight labour markets?

9. Concluding Remarks

This paper is the first to demonstrate that a large scale British ALMP – the NDYP – had heterogeneous impacts systematically related to local labour market conditions. The precise nature of this relationship is robust to controlling for heterogeneous programme impacts by participant characteristics, but is sensitive to whether heterogeneous programme impacts due to local differences in NDYP provision, themselves correlated with unemployment rates, are controlled for. When such differences in provision are *not* controlled for, the impact of programme participation on the unemployment exit and job entry hazards has an inverse-U shaped relationship with unemployment rates. Our interpretation is that this reflects the combined effects of two underlying mechanisms: first, that there are fewer vacancies with which to match programme participants in slack areas; and second, that programme providers in tight labour markets are more likely to sanction extending Gateway beyond four months, whereas those in slack labour markets are more likely to push participants to enter Options within four months (Dorsett, 2006). When such differences in provision *are* controlled for – better isolating the relative lack of vacancies mechanism – the impact of programme participation on the unemployment exit and job entry hazards has a monotonic, downward sloping relationship with local unemployment rates. In short, the NDYP was least effective where it was most needed.

To what extent might these conclusions generalise beyond the NDYP? Clearly the theoretical arguments, including questions relating to monotonicity and sensitivity to controlling for other dimensions along which programme impacts might vary, are not specific to any particular ALMP. Further, despite differences in specification, our results are broadly consistent with those from the Bloom et al. (2001) and are not entirely inconsistent with those of Kluve (2006). We do not find the same signed relationship between programme impacts and unemployment rates as Lechner and Wunsch (2009), but the programmes considered are rather different, and we are able to exploit variation in unemployment rates over 300 local labour markets as well as over time, whereas Lechner and Wunsch (2009) base their positive relationship between program impacts and unemployment rates only on variation in national rates over time (the relationship is insignificant when they use regional rates).

If ALMPs are least effective where and when they are most needed, as suggested here for the British NDYP, then policy makers are faced with two (related) problems. First, providing similar ALMPs across different local labour markets at a given point in time may exacerbate existing spatial differences in unemployment, so ALMPs may need to be targeted with this in mind and/or coupled with additional demand side measures in high unemployment areas (e.g. Sunley et al., 2005). Second, ALMPs that were effective in the relatively tight national labour markets that preceded the global financial crisis of 2008 may be less effective now unemployment has risen substantially across many OECD countries.

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