

## HEALTH, INCENTIVES AND ACTIVATION

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VERY PRELIMINARY

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**Abstract:** We study individual reactions to a labour market programme targeted at young unemployed in Sweden. The programme, which was introduced in 2007, involved both monetary incentives i.e. changes in the tax/transfer system designed to increase employment, as well as mandatory activation. We use a difference-in-difference strategy to analyse whether there are differences with respect to past health and school drop-out status in the way individuals reacted to the incentives and/or activation associated with the programme. Our hypothesis is that individuals who are in a difficult overall life situation may not be in a position to react to monetary incentives, and may be better helped by actual activation; whereas other individuals may respond to monetary incentives or the threat of activation (in which case activation programmes work more as a screening device). We use register data covering the entire Swedish population, including very detailed information on health.

**Keywords:** incentives, activation, unemployment, health, school drop-outs

**JEL-codes:** H30, H51, J64, J68

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## 1. Introduction

We analyse responses to an activation programme (Youth Job Guarantee) targeted at young unemployed that was introduced in Sweden in 2007. The programme involved a mix of actual activation and, for a subset of the target group, financial incentives i.e. changes in the tax/transfer system designed to increase employment. We focus on possible heterogeneous responses to the reform, that is, whether individuals with different personal characteristics reacted differently to the programme. More specifically, we are interested in attributes such as bad health, school drop-out status, and their effect on labour market outcomes: which types of policies are effective in improving the labour market prospects of those individuals who might otherwise be in a disadvantaged position and might for example have a high risk of social exclusion?

A key motivating idea behind the paper is that individuals may be in a different position to react to finely-tuned economic policies, depending on their overall life situation. Literature in behavioural economics has highlighted the fact that individuals may make suboptimal decisions for example due to limited attention or excessive and/or inconsistent discounting of future payoffs. Further, Shah et al. (2012) show how limited attention and impatience may be related to one's circumstances: For example related to poverty, if an individual is forced to concentrate on how to make ends meet on a daily basis, he may be ill-equipped to concentrate on making difficult long-run decisions. In our setting, bad health may play a similar role<sup>1</sup>: if an individual's attention is drawn to health problems, this may hamper his capacity to concentrate on long-run decisions related to job search, calculating participation tax rates, and so on. It is therefore interesting to analyse (i) whether there are systematic differences (for example with respect to health) in how individuals react to financial incentives associated with tax/benefit systems, designed to encourage participation in the labour market; and on the other hand (ii) whether individuals who are in a difficult overall situation (and therefore possibly ill-equipped to respond to financial incentives) may benefit from mandatory activation measures?

We utilise duration analysis combined with a difference-in-difference strategy to estimate the causal effect of the Youth Job Guarantee programme on the length of the unemployment spell. We use detailed register data on unemployment spells and individual background characteristics such as past health status (with very detailed measures such as the number and type of drugs taken by the individual). The data covers the entire Swedish population (all unemployment spells during the period under study). Related to activation, we estimate both a threat effect (an effect of the programme before actual activation starts) as well as an effect of participating in the activation programme itself. We are particularly interested in how these effects differ according to individual background characteristics such as health.

Related earlier literature includes Nordberg (2008), who finds that individual health status affects the transition from rehabilitation to work. Rosholm and Svarer (2008) find that in general there is a strong threat effect from active labour market policies, but not for the long-term unemployed. Black et al. (2003) find the effects of activation to differ across quantiles of the untreated outcome

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<sup>1</sup> In the case of poverty, there is scarcity regarding current financial resources. In our case, we can think of health as a scarce resource for some individuals, having similar effects as the scarcity implied by poverty.

distribution. Kluge (2006) provides a meta-analysis of the effects of recent European active labour market programmes. We contribute to this literature by providing an analysis of the relationship between health status and labour market outcomes using detailed measures of past health, and an increased understanding of whether the effects of activation and other labour market policies differ according to background characteristics such as health status.

We find that individuals with (mental) health problems and school drop-outs exit unemployment more slowly than others. Our preliminary results indicate that the reform appears to have helped individuals from a wide variety of backgrounds, both those with health problems and those without. The effects arise both before activation (threat effect) as well as during activation, though the latter effect appears stronger. We find some weak evidence that activation may have been particularly helpful for some individuals who are in a relatively disadvantaged labour market position (notably school drop-outs), but the differences between groups are not statistically significant. We do not find strong differences with respect to health in the overall effects of the reform.<sup>2</sup>

The paper proceeds as follows. Section 2 reviews the theoretical ideas behind our empirical analysis. Section 3 describes the reform, while the data is described in Section 4. The empirical methodology and some preliminary results are presented in Section 5. Section 6 concludes.

## **2. Background**

Most analyses of optimal tax and benefit schemes, as well as employment policy, have until recently relied on assuming that individuals fully understand the effects of public policies on their financial situation, and react in ways predicted by traditional economic theory. However, tax and transfer systems are highly complex, and it can be difficult to calculate e.g. the effective tax rates on employment. For example, Chetty and Saez (2009) showed that providing simple information about the U.S. earned income tax credit (EITC) induced significant labour supply responses for some taxpayers, which suggests that individuals' perceptions of the EITC were not correct to start with. Further, the first employment spell might not be very profitable, but it can serve as a stepping stone to better paid jobs. But if individuals are short-sighted, these potential long-term benefits can be discarded in the short-term employment decision. Related to this idea, DellaVigna and Paserman (2005) found that more impatient individuals search less intensively for work.

Shah et al. (2012) have shown how scarcity may affect the way individuals allocate their attention, and hence their ability to make optimal decisions, in particular to plan ahead. Due to the complex interaction of the tax and benefit system at low incomes, it is possible that characteristics such as the health status and cognitive abilities of individuals affect how they react to the incentive structure. As for health, if an individual is preoccupied with health problems (but might still in principle be able to work), such problems may limit his ability to make long-term plans.<sup>3</sup> Thus, a key feature of our analysis is to examine the potential heterogeneity in the treatment effects of

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<sup>2</sup> It should be noted however that we have not yet analysed the possible differential effects of the financial incentives associated with the reform. All results are very preliminary.

<sup>3</sup> In the context of Shah et al. (2012), short-sighted behaviour is not seen as arising due to some inherent personal characteristics, but rather as a result of one's circumstances.

labour market policies: Do differences in health or certain personality characteristics influence the way the recipients of welfare benefits react to monetary incentives and activation programmes?

Besley and Coate (1992) argue that a key rationale for work requirements in poverty alleviation programmes is their role in screening between those who are truly in need of poor support and those who are not. Activation policies in employment programmes may play a similar role. Within our context, individuals may differ (in addition to other dimensions) in their ability to understand the incentive structures involved in the tax and benefit system. Labour market policy may then have two purposes: (i) deter from benefits those individuals who are responsive to (monetary) incentives and would be able to find work on their own (call them type S for strong) but do not do so e.g. because benefits are too generous or easy to obtain; this is the *screening effect*; and (ii) help those unemployed individuals to find work or suitable education who do not respond to traditional monetary incentives, or for other reasons are not able to find work on their own (type W for weak); call this the *activation effect*<sup>4</sup>. If both screening and activation effects are at work, we should observe a certain type of pattern in exit from unemployment: Type S individuals should exit unemployment before actual activation starts. Type W individuals, on the other hand, would enter the activation phase, and hopefully find employment as a result. It is likely that policies involving only monetary incentives may achieve the first, but not the second aim. We aim to analyse whether such patterns are present in our data.

### 3. The reform

The activation programme that we study is the Youth Job Guarantee programme that started in Sweden in December 2007. The programme involved activation that started after 90 days of unemployment, and it involved all unemployed individuals who were under 25 years of age. The activation was mandatory for those in the targeted age group, and a refusal to participate incurred sanctions.

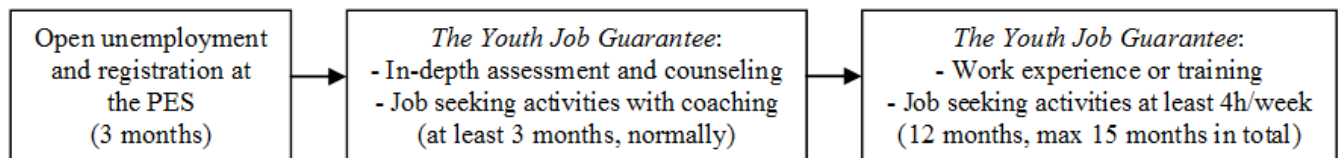
The structure of the programme is illustrated in Figure 1. For the first three months (90 days) of the unemployment spell, there was open unemployment. After 90 days, the employment office undertook an in-depth assessment of the situation of individuals in the target group. In the first phase of activation that started after 90 days, the programme mainly took the form of job search assistance. After a further 90 days, the individual who were still unemployed transferred into a second phase of activation that involved more intense job search activities and also short periods of training or work placement to gain work experience. The contents of the programme were relatively flexible and were tailored according to individual needs.

A further feature of the reform was that for some (well-defined) groups of unemployed, the unemployment benefit declined faster over time than it had done prior to the reform. The earnings related unemployment benefit was normally 80 % of prior earnings for the first 200 days

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<sup>4</sup> This effect has some similarities with the *deterrent effect* discussed in Besley and Coate (1992), where the aim is to encourage poverty-reducing investment. In a similar vein, participation in activation programmes can be seen as a form of investment that helps the individual find a job later on; however, in our context this should not be seen as a deterrent effect, as we view these individuals as being unable to find work on their own (for various reasons that may be related to psychology, health and so on).

of unemployment, and declined to 70 % for the next 100 days. For some individuals participating in the Youth Job Guarantee programme, the rules were different: the 80 % replacement rate applied only for the first 100 days of unemployment, declined to 70 % for days 101-200 and further to 65% for days 201-300. Therefore, for some individuals, the reform involved elements of both activation and financial incentives. However, the individual was unaffected by the faster reduction of benefits if s/he (i) had children; or (ii) was only eligible to the basic unemployment benefit; or (iii) had an earnings related benefit that would have exceeded the maximum amount of benefits.



**Figure 1. The Youth Job Guarantee programme.**

#### 4. Data

We combine data on individual’s employment status with information on their (past) health and other relevant personal characteristics. The data on unemployment spells comes from the registers of the public employment service, and the data on health status from hospital and drugs registers. These registers include individual-level information for example on all drugs prescriptions and doctor / hospital visits (including codes for any diagnoses) in a given year, both for public and private health care. The health measures that we use relate to use of drugs related to a neurological condition or for mental illness (the latter is a subset of the former), total number of prescriptions, and treatments received in specialized health care (both in general as well as separating treatment for mental illness). Our data covers the entire Swedish population.

We use a difference-in-difference strategy to estimate the causal effect of the reform on the duration of unemployment. The programme was targeted at all under 25-year-old unemployed. The treatment group that we study consists of 24 year-old-unemployed and the control group includes 25-year-old unemployed who were born in the same year as individuals in the treatment group. That is, we focus on individuals born during a single year, to make the treatment and control groups as comparable as possible. For example, looking at the effects of the reform in 2008, we take individuals who were born in 1983, and assign them into the treatment and control groups, depending on whether they had turned 25 by the time their unemployment spell had lasted 90 days.

For the pre-reform sample, we use unemployment spells that started in December 2006 - July 2007. The reasons for such a restriction are two-fold. First, those who became unemployed in September 2007 would be assigned to the programme when it started in December 2007; whereas those whose unemployment spells had started earlier (and had passed their 90 days mark before December 2007) were not assigned to the programme. Second, since we focus on individuals who were born in a single year, in January most of them will be 24 and in December most of them will be 25. The above restriction on the month of spell start allows us to obtain a

more balanced sample, with enough individuals in both the treatment and the control group: the sample is chosen in such a way that individuals in the sample would have their 90 day mark between March and October.

For the post-reform sample, we use unemployment spells that started in December 2007 – July 2008 for the 2008 sample, and in December 2008 – July 2009 for the 2009 sample. That is, we use the same restrictions on the month of spell start for both the pre-reform and the post-reform samples.

Table 1 provides some descriptive statistics related to the background characteristics of the individuals in the sample for 2008. Column (1) includes all unemployed individuals born in 1983, column (2) includes all participants in the Youth Job Guarantee programme in 2008 and columns (3) and (4) includes individuals in our treatment and control groups. One key message of Table 1 is that there appear to be no differences between individuals in the treatment group, except for the fact that a slightly higher fraction of individuals in the control group have a university degree. This is likely explained by the fact that individuals in the control group are by construction slightly older. We control for both age and education in the analysis.

**Table 1. Descriptive statistics: background characteristics (2008 sample).**

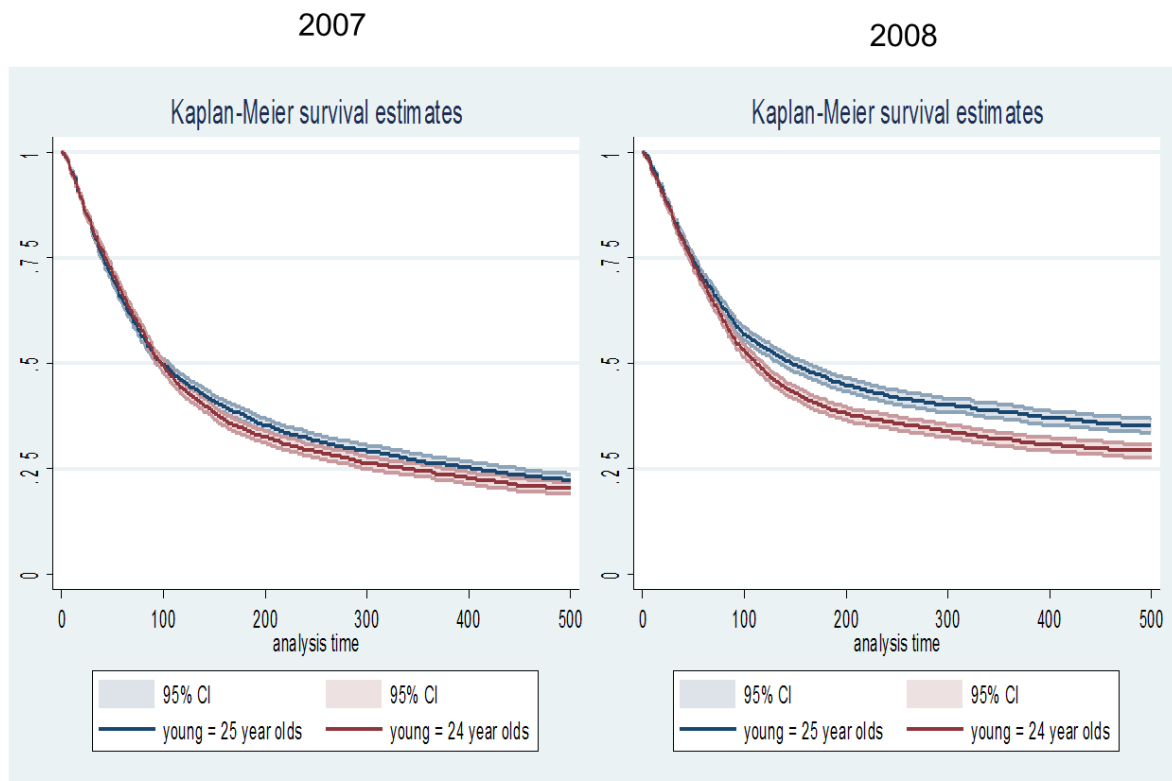
	(1) All registered, born 1983	(2) All participants in YJG 2008	(3) Treatment group 2008	(4) Control group 2008
Days registered in previous spells, total	440.8	322.3	437.2	432.1
Total number of previous spells	3.265	2.389	3.404	3.375
Total number of previous programs	1.162	1.352	1.076	0.970
Age at spellstart+90 days	24.89	22.71	24.72	25.27
Country of birth, non-nordic	0.297	0.173	0.266	0.271
1=male,0=female	0.507	0.591	0.516	0.492
Registered disability	0.0762	0.0619	0.0532	0.0551
Upper secondary school	0.602	0.774	0.619	0.602
University	0.190	0.0722	0.199	0.222
1=Children staying at home <18 year old	0.267	0.651	0.261	0.257
1=Married	0.0920	0.0213	0.0697	0.0838
1=Social benefit take up >0 SEK,2007	0.266	0.223	0.229	0.240
1=working in Nov 2007, 0=not working	0.492	0.574	0.561	0.532
Income from work, 100*SEK,2007	755.0	900.3	902.3	840.4

Table 2 provides descriptive statistics on the main health indicators used in the analysis. One difference compared to Table 1 is that column (1) now includes all Swedes born in 1983 (whereas data in Table 1 comes from the registers of the employment service and hence includes only unemployed individuals). The first variable is the number of prescriptions the individual had in the previous year, whereas the rest of the variables are dummies for whether the individual took a drug for neurological condition or for mental illness, and whether they were treated in a hospital (inpatient) or whether they were treated for a mental illness in either a hospital or in outpatient specialised care. We can observe that unemployed individuals (columns (2) to (4)) appear to have worse health than other individuals of their age (column (1)). On the other hand, individuals in column 2 (all participants in the job guarantee programme) appear healthier than individuals in the treatment and control groups; this is likely explained by the fact that the average individual in the job guarantee programme is younger than the average individual in the treatment or the control group. Again, individuals in the control group closely resemble those in the treatment group.

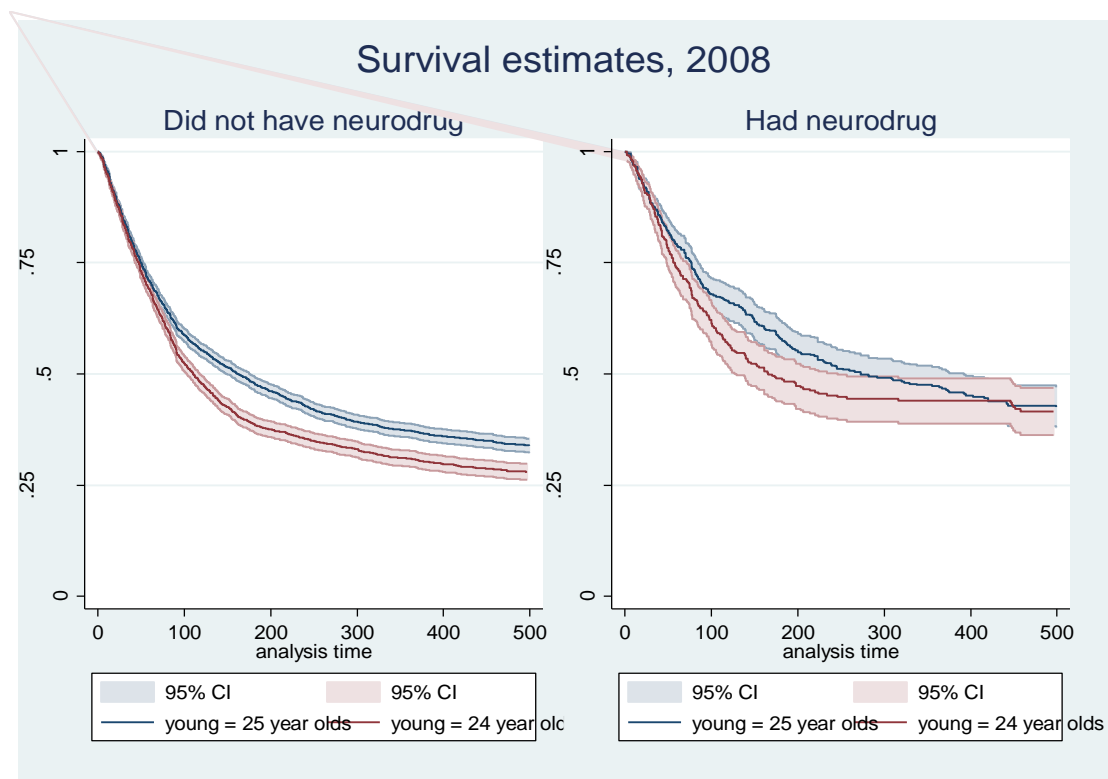
**Table 2. Descriptive statistics: health indicators in previous year (2008 sample).**

	(1) Others born in 1983	(2) All participants in YJG, 2008	(3) Treatment group 2008	(4) Control group 2008
# of prescriptions	1.897	1.786	2.105	2.128
neurodrug	0.126	0.141	0.181	0.193
drug for mental illness	0.0731	0.0810	0.117	0.122
inpatient	0.0745	0.0609	0.0996	0.108
had more than 2 weeks sick-leave	0.0618	0.0665	0.0893	0.0970
outpatient (mental illness)	0.0296	0.0387	0.0600	0.0595
inpatient (mental illness)	0.00633	0.00810	0.0145	0.0187

Figures 2, 3 and 4 provide some first descriptive analyses related to observed unemployment duration in our data. Figure 2 plots the Kaplan-Meier survival estimates for unemployment duration for the treatment (red) and control (blue) groups both before the reform (2007) and after the reform (2008). The figure indicates that before the reform the confidence intervals of the survival rates for the treated and the controls overlap, whereas in 2008 the treatment group has lower survival rates (and hence shorter unemployment spells) than individuals in the control group. This is a first indication that the reform may have had an effect on unemployment duration.



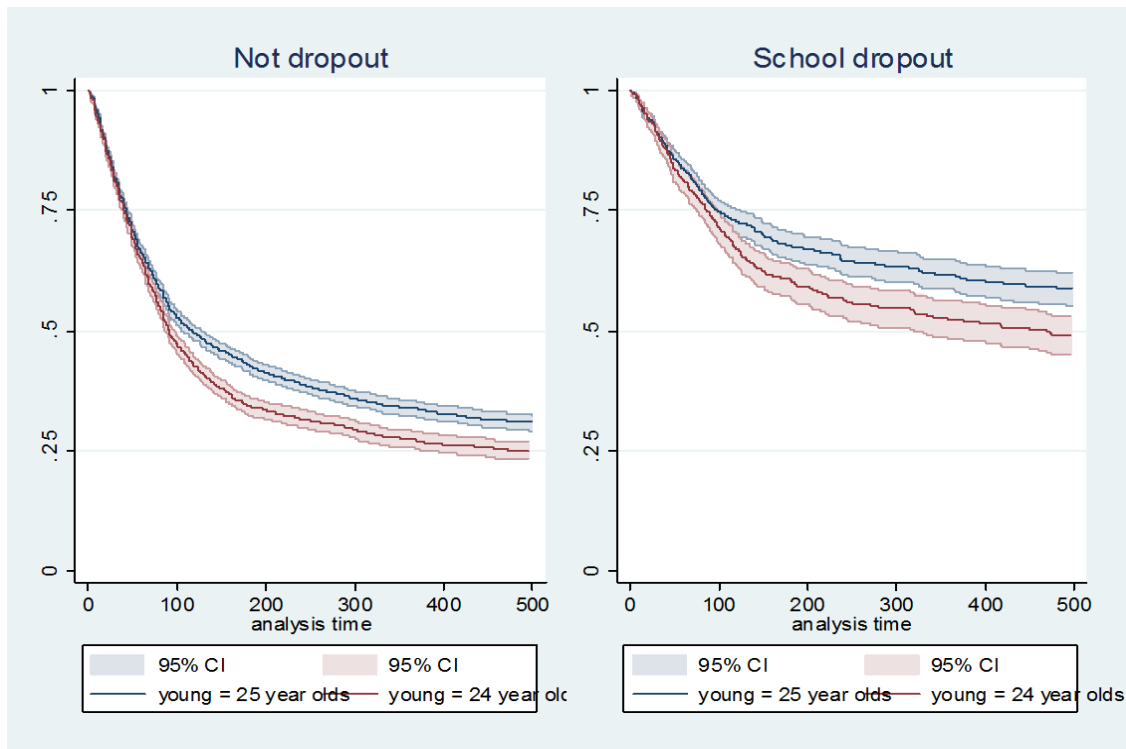
**Figure 2. Kaplan-Meier survival estimates for unemployment duration for the treatment and control groups before the reform (2007, left panel) and after the reform (2008, right panel).**



**Figure 3. Kaplan-Meier survival estimates for unemployment duration for individuals who used a neurological drug (right panel) or did not use such a drug (left panel), 2008.**



## Survival estimates, 2008



**Figure 4. Kaplan-Meier survival estimates for unemployment duration for school drop-outs (right panel) and others (left panel), 2008.**

Figures 3 and 4 look at differences with respect to certain background characteristics. (Note that the figures are drawn for 2008 only, and hence cannot provide reliable evidence on reform effects; the purpose is to illustrate differences in unemployment duration for individuals with different background characteristics.) Figure 3 plots the survival estimates separately for individuals who had a prescription for any neurological drugs (right panel), and those who did not (left panel). The figure indicates that survival rates were higher for those with a prescription for neurological drugs. Figure 4 plots the survival rates for school drop-outs (right panel) and others. We consider school drop-out status to be another possible indicator of a disadvantaged labour market position. Again, survival rates are higher for school drop-outs than others.

## 5. Econometric analysis

### 5.1 Method

We use duration analysis combined with a difference-in-difference strategy to estimate the causal effect of the reform on the probability of an unemployed individual finding a job. The construction of the treatment and control groups for the analysis was described in Section 3. The identifying assumption is that in the absence of the reform, the unemployment duration of individuals in the treatment and control groups would have changed in a similar fashion from 2007 to 2008 (or 2009). Importantly, the estimation sample has been designed in such a way that there were no

other relevant reforms that would have affected either the treatment or the control group in 2008 or 2009.<sup>5</sup>

In the estimations, the dependent variable is the probability of finding employment. The main explanatory variable is participation in the Youth Job Guarantee programme, where we estimate both a threat effect (which we define as the effect of the reform on the probability of finding employment during days 30-90 of the unemployment spell) and a programme effect (the effect of the reform on the probability of finding employment after 90 days of unemployment). We control for a wide variety of background characteristics of the individuals.<sup>6</sup>

We estimate a Cox proportional hazards model  $\lambda(t|X) = h_0(t)\exp(\beta'X)$  where  $\lambda(t|X)$  is the hazard rate for finding work (conditional on covariates  $X$ ),  $h_0(t)$  is the baseline hazard at time  $t$ , and

$$\beta'X = \beta_1young + \beta_2after + \beta_3I30 * after + \beta_4I90 * after + \beta_5I30 * after * young + \beta_6I90 * after * young + \gamma'Z. \quad (1)$$

In equation (1), *young* is a dummy for the individual being 24 years old at 90 days of unemployment, *after* includes a separate dummy for both post-reform years 2008 and 2009, *I30* is an indicator dummy for days 30 to 90 of the unemployment spell, *I90* is a dummy for days 90+ of the unemployment spell and  $Z$  is a vector of control variables. The coefficient  $\beta_5$  therefore provides an estimate of the threat effect associated with activation, whereas the coefficient  $\beta_6$  provides an estimate of the actual programme effect.

## 5.2 Results

Table 3 provides estimates of the overall programme effect for the entire sample of unemployment spells.<sup>7</sup> We only report the estimates of the treatment effects in the tables, but controls (see footnote 6) are included in all regressions. The interaction terms related to the *I30*-dummies provide estimates of the threat effect (separately for both treatment years 2008 and 2009) and the interaction terms related to the *I90*-dummies provide estimates of the programme effect. It appears that the programme had strong positive effects on job finding rates: both the threat effect and the effect of the activation itself are fairly large and highly significant. Effects of actual activation appear to be slightly larger than the threat effect.

**Table 3. Effects of the reform on the hazard of finding employment, whole sample.**

VARIABLES	_t
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<sup>5</sup> This consideration for example prohibits us from using 25-year olds who were born in the previous year as controls, as there was a simultaneous payroll tax reform that affected individual in the year they turned 26 (related to that reform, the selection of individuals was based on year of birth, not age on a specific date as in our case).

<sup>6</sup> The controls include age in days, gender, marital status, number of children, education, region, unemployment history, country of birth, whether the individual had a registered disability, past earnings and month of spell start.

<sup>7</sup> The overall reform effects have also been analysed in Hall and Liljeberg (2011).

I90_young_08	1.328***
	(0.101)
I90_young_09	1.439***
	(0.106)
I30_young_08	1.161***
	(0.0794)
I30_young_09	1.213***
	(0.0873)
Number of unemployment spells	30 178
Robust standard errors in parentheses.	
*** denotes significance at 1 % level.	

We next turn to look at how the effects of the reform differ by individual background. We have a large number of health indicators and other background variables in the data, and an obvious first question is which indicators we should use. From the point of view of our motivating idea – does activation help those in need of activation, or individuals with a difficult overall life situation – it is interesting and informative to first look at how individuals with different background characteristics differ in the job finding rates overall (not yet thinking about any reform effects). If we find that certain groups tend to have more difficulties in finding work than others, it makes sense to concentrate on those groups, and analyse whether activation has an effect on their job finding rates or not.

To achieve this, we first therefore take a look at how the various background characteristics that we are interested in affect the hazard of finding a job *before the reform*. The results are presented in Table 4. Two groups stand out: Having dropped out from school appears to lower the probability of finding a job by approximately 22%. Another group with notably low job finding rates are individuals who were treated for mental illness (including both inpatient and outpatient care).

**Table 4. Effect of health and drop-out status on hazard of finding employment (pre-reform sample).**

VARIABLES	_t
Had a neuro-drug	0.934
	(0.0435)
Received more than two drugs	0.987
	(0.0325)
Was treated in specialised care	1.062
	(0.0396)

Had more than 2 weeks sick leave	0.877**
	(0.0353)
Was treated for mental illness	0.833**
	(0.0479)
Dropout	0.781***
	(0.0341)
Number of unemployment spells	9 036
Robust standard errors in parentheses.	
*** denotes significance at 1 % level.	

The following tables report the results related to the effect of the reform separately for certain groups of interest. The second column of Table 5 includes individuals who were treated for mental illness as well as those who took a drug for a neurological condition. The latter category is included to provide a more comprehensive measure of mental problems and to achieve a larger sample. Drugs for the treatment of mental illness form the largest category within neurological drugs; it also includes drugs to treat certain behavioural disorders, which might be of interest. The left column provides the results for individuals who did not receive the above mentioned treatment / drugs. According to the results, both individuals with mental health problems and those without appear to respond activation (at least in the first year of the reform). The threat effect is significant only for those without mental health problems. The differences between the point estimates of the two groups are however not significant. The effects for individuals with mental health problems are estimated rather imprecisely; the sample size is relatively small.

The results reported in Table 6 indicate that individuals who have dropped out from school appear to benefit from activation more than individuals who have completed their basic education. Again, the effect for dropouts is however relatively imprecisely estimated, and the difference between the effects for the two groups is not statistically significant. The threat effect is significant only for those who have completed their basic education (though the point estimates are of a similar magnitude).

**Table 5. Effects of the reform on the hazard of finding employment, by past mental health status.**

	No mental health problems	Mental health problems <sup>1</sup>
VARIABLES	_t	_t
I90_young_08	1.313***	1.389*
	(0.109)	(0.259)
I90_young_09	1.513***	1.050
	(0.118)	(0.230)
I30_young_08	1.133*	1.294
	(0.0841)	(0.224)

l30_young_09	1.248***	1.028
	(0.0956)	(0.219)
Number of unemployment spells	24 545	5 633
Robust standard errors in parentheses. *** denotes significance at 1 % level.		
<sup>1</sup> Mental health problems measured by having received treatment for mental illness (inpatient + outpatient care) or having taken a neurological drug in the previous year.		

**Table 6. Effects of the reform on the hazard of finding employment, by school drop-out status.**

	Not school drop-out	School drop-out
VARIABLES	<u>t</u>	<u>t</u>
l90_young_08	1.243***	1.978***
	(0.102)	(0.410)
l90_young_09	1.462***	1.296
	(0.113)	(0.307)
l30_young_08	1.150*	1.259
	(0.0842)	(0.243)
l30_young_09	1.200**	1.358
	(0.0906)	(0.325)
Number of unemployment spells	23 126	7 052
Robust standard errors in parentheses. *** denotes significance at 1 % level.		

### 5.3 Robustness

The results reported in the previous section are very preliminary and should be interpreted cautiously. One problem with the current results is that is that some of them appear to be sensitive to how one controls for the month of entry into unemployment. Recall that we have constructed the sample in such a way that only individuals born in a single year are included, and those who have turned 25 before the 90<sup>th</sup> day of their unemployment spell are in the control group and the younger individuals in the treatment group. (Recall that constructing the sample in this way was necessary to rule out effects of other reforms targeted at older unemployed.) This implies that we have more individuals in the treatment group at the beginning of the year and vice versa for the end of the year. We might try different ways to take this into account. First, we have restricted the sample to spells where the 90<sup>th</sup> day occurs between March and August, so that the first and last months of the year (where the treatment and control groups would be very unbalanced) have been excluded. We have also included dummies for the month of entry into

unemployment. Other possibilities would be to weight the sample in such a way that the treatment and control groups would be of equal size in any given month. One might also consider stratified estimation by month of entry. Unfortunately, some of the results appear to be sensitive to these details, and more work is needed to solve these issues.

## 6. Conclusion

We have analysed the relationship between individual characteristics such as health status, and their employment outcomes, using detailed Swedish register data and an activation reform targeted at young unemployed. Our preliminary results indicate that individuals with (past) health problems and school drop-outs exit unemployment more slowly than others. The activation reform appears to have helped individuals from a variety of backgrounds. Further analysis is needed to properly disentangle the effects of monetary incentives and activation measures on individuals with different background characteristics, and the reform that we study provides an opportunity to carry out such an analysis.

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