The labour market impacts of a youth guarantee: lessons for Europe?

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Abstract

This paper examines the youth guarantee programme introduced in Finland 2005. The reform consisted of early intervention, monitoring and individualized job search plans that guarantee activation measures for unemployed young persons. Using the age threshold set at 25 years, we find that the youth guarantee moderately increased unsubsidized employment while having a negligible impact on unemployment in the age range of 23-24. We also show that the positive impacts of the youth guarantee only materialize among unemployed young persons with a vocational education. There are no signs that the guarantee improved the labour market prospects of young uneducated people.

1. INTRODUCTION

Currently, the number of unemployed young people in the EU exceeds the population of Denmark. Youth unemployment brings about massive economic costs in terms of lost production and social benefit payments. The social costs that may materialize in the future are even more alarming. There is a real possibility that unemployment at younger ages causes future unemployment, and increases social exclusion. Against this background, the European Commission launched the European Youth Guarantee Initiative in 2013 to ensure an active opportunity for young people within four months after leaving education or becoming unemployed. The intention was to create opportunities by offering quality job offers, active labour market measures, better public employment services and apprenticeship schemes (EU Commission, 2013). Member states were requested to draw up a Youth Guarantee Implementation Plan by spring 2014. The stakes are high, since a total of 6 billion euros of additional EU financing are to be dedicated to the youth unemployment problem in 2014-2015, let alone the estimated total cost of 21 billion in national budgets prioritized for this youth initiative (ILO 2012).

The initiative sounds appealing but the question remains to what extent youth guarantees really deliver something new to tackle youth unemployment. Existing empirical evidence from Nordic countries that have implemented guarantees for decades indicates mixed results. Carling and Larsson (2005) examined the 1998 Swedish municipal youth guarantee targeted at unemployed persons below the age of 25. Hall and Liljeberg (2011) analysed the 2007 Swedish youth job guarantee reform implemented by the public employment services. Both studies report a positive employment effect prior to the activation period. Carling and Larsson found no overall improvement, mainly because of the locking-in effect during participation in the programme, while Hall and Liljeberg report a positive employment effect after the activation period. Hardoy et al. (2006) report somewhat more positive employment effects for Norway. Their results show an increase in the transition rate from unemployment to employment of a magnitude of 4-11%.

This study contributes to the scarce evidence on the overall impacts of youth guarantees by analysing the youth guarantee (YG) reform introduced in Finland in 2005. This reform is particularly interesting as the European Commission recently identified the Finnish youth guarantee as being best practice for other member states. Even though the Commission referred to the current version of the Finnish YG, the principal elements of it were already introduced in the 2005 reform. The key elements include the target group being all inactive young persons under the age of 25, early intervention with a pre-scheduled procedure, stricter monitoring, job search plans in the early stages of unemployment and guaranteed activation. In particular, a draft job search plan had to be drawn up within one month after registering as an unemployed job seeker. The actual signing of an individualized job search plan was brought forward to three months from the previous five months. An entirely new element was that the plan had to include an offer of a job, education, training, or some other active measure. This

offer had to be provided within three months of signing the plan. Since no such guarantee was introduced nor any time limits changed among older age cohorts, we are able to use this age limit in identifying the impact of the youth guarantee.

The effects of the YG reform are analysed within a difference-in-differences (DiD) framework. We begin the analysis by focusing on unemployment duration and subsequent transitions. We then expand our analysis to cover the whole target population - for two reasons. First, some of the affected young people may choose not to register as an unemployed job seeker in order to avoid early intervention and stricter monitoring, see Dahlberg et al. (2008). If they are mainly disadvantaged young people, the DiD results of survival analysis will be biased upwards. Second, the YG has a strong emphasis on preventing social exclusion. Without taking a stance on how to define or measure social exclusion, it is probable that unemployment spells are only partially correlated with it. To get some insight into the effects on both unemployment entry and marginalization, we explore several outcome variables. These include unemployment incidence, application for and enrolment in education, income, use of social assistance and mental health.

Our results show no compositional change in unemployment entry, a small 2 percentage point increase in the activation ratio and a positive employment effect of a magnitude of 5 days. Our primary finding is that the youth guarantee reform mainly affected skilled unemployed young persons who already had a vocational secondary education. We find no effects among the most disadvantaged group of unemployed young people, i.e. those unskilled young persons with only compulsory schooling. The most likely explanation for this arises from the fact that early activation was already used among uneducated youngsters before the introduction of the YG. To fulfil the performance goals set, local employment services seem to have focused on skilled unemployed young persons.

2. THE YOUTH GUARANTEE

Finland has a long history of high youth unemployment. A severe banking crisis, together with the collapse of Soviet trade in the early 1990s, raised the overall unemployment rate from around 3% to nearly 17% in just three years. Figure 1 shows that the deep recession was especially harsh among young people, whose unemployment rate peaked at nearly 35%. The recession was followed by a long period of economic growth, which narrowed the gap between Finland and the EU15 in adult unemployment rates by the millennium. The youth unemployment rate, however, remained at a much higher level than the EU15 (and EU28) average until the 2009 financial crisis.

Figure 1. Youth and adult unemployment rates in Finland and EU15, 1990–2013.
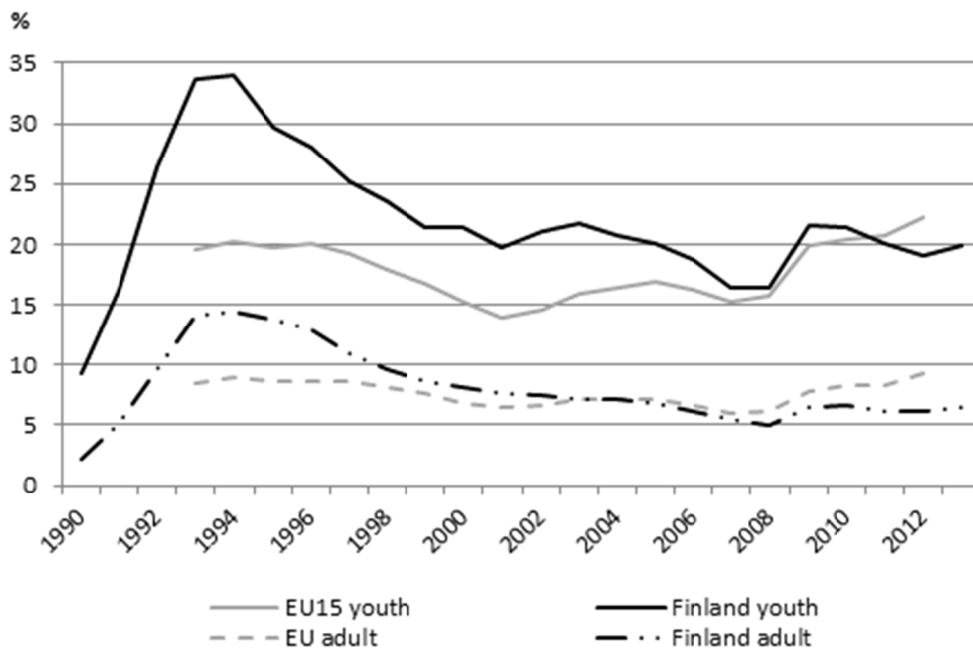
Notes: (i) Source: Eurostat; (ii) Youth refers to 15–24-year-olds and adult refers to 25–74-year-olds

In order to tackle youth joblessness, the government introduced the youth guarantee programme as part of its general Employment Programme on 1 January 2005. The YG scheme targeted under 25-year-olds, and the aim was to reduce youth unemployment and marginalization by early intervention together with guaranteed activation. Figure 2 illustrates the changes the reform induced in the operation of public employment services (PES). All activation is based on an individualized job search plan. Prior to the 2005 reform, the plan was drafted by the PES within five months of unemployment and this schedule was the same for all unemployed job-seekers, irrespective of age. Prior to the YG scheme, the mutually agreed job search plan did not necessarily include any activation measures, nor was there any obligation for the local PES to offer or arrange any activation.

The 2005 reform changed the services for young jobseekers (17–24 years) in three important ways. First, a preparatory counselling meeting had to take place within one month of registering. In this meeting the caseworker assesses the individual service needs of the young jobseeker, and explains the activation procedure. A preparatory job search plan is drafted. Second, the completion of an individualized job search plan was brought forward from five months to three months. To emphasize early intervention, the Ministry of Employment advised employment offices to be in regular contact with the under-25s also between these two time points. Third, the job search plan had to explicitly include the activation measure agreed upon. The plan is mutually binding. The local employment authority is obliged to offer the activation measure included in the plan within three months of signing the contract, hence the name "youth guarantee". At the same time, the job search plan obligates the young job seeker, and non-compliance can be sanctioned.

Figure 2. Youth activation by PES before and after the 2005 Youth Guarantee reform

The implementation guidelines of the YG divide unemployed young persons into two groups according to their skills level[1]. The division is between skilled young persons with vocational education and unskilled persons with only compulsory schooling or (non-vocational) secondary education. For skilled young persons the main goal is regular employment, and the most employment-eligible skilled young persons are steered towards an independent job search. If a skilled young person is likely to have difficulties in a job search, she is directed to intensified services such as job search training. The active labour market programmes on offer for the skilled group are job coaching, work practice and subsidized employment, either in the private or (rarely) in the public sector. The main aim of the policy for unskilled young persons is to make them (re-)enter the ordinary education system. These services include career planning and information on various educational possibilities. For both skill groups the most common activation measure, representing more than half of all participants, is work practice. This is non-salaried employment with compensation paid at the level of the minimum unemployment allowance. There are some differences in the distribution of activation measures between skill groups, the skilled receiving slightly more vocational labour market training and job placements in the private sector. The 2005 reform induced only minor changes in these differences.

The youth guarantee was gradually implemented after 1 January 2005. Implementation started rather slowly, possibly because the reform was carried out through a ministerial guidance letter to local authorities, not by new or amended legislation. According to the final report on the Employment Programme[2], over 37,000 young unemployed persons passed the three-month unemployment spell limit in 2005, and only around 10,400 (28%) young persons had a signed job search plan at that time.

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[1] Hallituksen työllisyyden politiikkaohjelman toimeenpano: nuorten koulutus- ja yhteiskuntatakuu sekä työpajatoiminnan vakinaistaminen (Implementation of the Government's Employment Policy: Youth Education and the Youth Guarantee and the Regularization of Workshops), Ministry of Education and Ministry of Employment.
[2] Työllisyys nousussa – Työllisyysohjelman loppuraportti (Rising Employment – Final Report of the Employment Programme), p. 17.

In January – August 2006, 70% of 30,700 youths passing the three-month time limit had a signed plan, and towards the end of 2007 the share had risen to 77%.

3. PREVIOUS LITERATURE

Carling and Larsson (2005) analysed the 1998 Swedish municipal youth guarantee, which was targeted at unemployed persons below the age of 25. They found a small positive employment effect just prior to the activation period, but the negative locking-in effect during programme participation resulted in a negligible overall impact on unemployment duration. Hall and Liljeberg (2011) analysed the 2007 Swedish youth job guarantee reform that had the same target group (unemployed 18-24-year-olds) but this time the programme was implemented by the local PES. They report a positive employment effect for 2008 after an unemployment spell exceeded the activation threshold of 90 days. They also found a positive effect in 2009, but it showed up prior to the activation period with no impact after the start of activation. The long-term effects were found to be negligible. Norway temporarily extended its youth guarantee from those under the age of 20 to cover 20-24-year-olds during the period 1995-1998. Hardoy et al. (2006) conclude that this extension increased the transition rate to employment by 4% for the short-term unemployed and by almost 11% for the long-term unemployed. The corresponding figures for transitions to active measures were 12% and 35%, respectively. They also explored the impact of the youth guarantee on regular education, for which they find no effects.

One youth programme which closely resembles the Nordic guarantees is the New Deal for Young People in the UK that was targeted at 18 to 24-year-old unemployed persons. This particular scheme has been found to have positive effects both in the short and in the long run. Blundell et al. (2004) report that the six-month gateway period that comprised frequent meetings with a mentor increased the employment rate by 5 percentage points. De Giorgi (2005) focused on the longer-term effects of the New Deal. He found that the combined effects of job search assistance, training, wage subsidies and job experience improved the probability of employment by almost 5 percentage points.

Even though there is scarce direct evidence of the effect of youth guarantees, some of the key elements embedded in these programmes have been studied in detail. First of all, intensified counselling and increased monitoring have been found to have positive employment effects in e.g. Dolton and O'Neill (1996), van den Berg and van der Klaauw (2006) and Micklewright and Nagy (2010). Any non-compliance before or during the activation period is bound to result in sanctions that are shown to enhance exits from welfare in Abbring et al. (2005), van der Klaauw and van Ours (2013) and van den Berg et. al. (2014). The effects of mandatory activation have been analysed in Black et al. (2003), Graversen and van Ours (2008), and van den Berg et al. (2009). These results emphasize that the mere threat of activation increases employment rates through the perceived leisure cost of unemployment. Rosholm and Svarer (2008) point out several reasons why some unemployed job

seekers do not want to enter activation measures. They may expect the payoff of the programme to be small, they may fear a stigmatizing effect or they may simply see a reduction in leisure time as being undesirable. Finally, the vast literature on the actual treatment effects of active measures has been summarized in two recent meta-analyses by Kluve (2010) and Card et al. (2010). These analyses show heterogeneous effects, varying from positive employment effects of employment subsidies in the private sector to zero effects from public sector placements.

4. EMPIRICAL STRATEGY

4.1. Identification

Our empirical approach is based on the age limit of the youth guarantee, which targeted extensive activation to young people under the age of 25. The age limit creates a quasi-experimental difference-in-difference design where the target group consists of young persons under the age of 25 while slightly older persons serve as the control group. This setting allows us to estimate the causal effect of the YG reform with two assumptions, viz. individuals do not self-select into the treatment and control groups, and these groups share common outcome trends in the absence of reform. The first assumption holds as the selection is based on a predetermined age. The second assumption is trickier as individuals of different ages have different opportunities to respond to economic shocks. In what follows, we test the hypothesis of common trends by carrying out placebo tests for several pre-reform years.

In line with previous studies, we begin our analysis by examining unemployment spells. The duration of unemployment spells for young people is typically short. In our data one third of the spells terminate within one month. This, together with the administrative practices of how public employment offices register ending dates, results in numerous spells having exactly the same duration. Because of tied survival times, we grouped the data into discrete intervals by months of unemployment and use a proportional discrete time hazard model, see e.g. Allison (1984) and Jenkins (1995, 2005). To estimate how the YG affects exits from unemployment, we specify the instantaneous hazard for the jth month in unemployment as

$$h_j(x_{ij}) = 1 - \exp\left[-\exp\left(\alpha_0 + \lambda_a + \gamma_t + \delta_j D_{ij} + x'_{ij}\beta + \alpha_j\right)\right], \qquad (1)$$

where α_j characterizes the baseline hazard, λ_a and γ_t are the main effects controlling for age and time effects, and x_{ij} is a vector of individual covariates. D_{ij} is an indicator that is assigned a value of one if a person is under the age of 25 and the youth guarantee is in place. Our primary interest is in the parameter δ_j which measures the difference in changes in the hazard estimates between the treated and the controls after the implementation of the YG reform. We use equation (1) in a competing risk

framework with three exit routes, viz. unsubsidized employment, active measures and transitions out of the labour force, and model the distribution of the unobserved heterogeneity as being normal.

Analyses based on equation (1) provide only part of the story. To explore the unemployment entry effect, income effects and reasons for transitions out of the labour force, we broaden our view from unemployment spells to the population level. In our application, we estimate DiD regressions of the form

$$y_{it} = \alpha + \lambda_a + \gamma_t + \sum_{k=-l}^{m} \delta_k D_{it-k} + x'_{it}\beta + \varepsilon_{it}, \qquad (2)$$

where λ_a and γ_t are again the main effects controlling for age and time effects, respectively. x_{it} includes individual-level characteristics, and D_{it-k} is an indicator variable equal to one if an individual i is under the age of 25 in year t-k. Our primary interest is the parameters δ_k which measure the relative change in outcome y between the treatment and the control groups. These parameters allow for l leads, which we exploit in testing for any pre-reform differences between the age groups, see Autor (2003). If our specification passes these pre-reform tests, we interpret the point estimates of m lagged treatment indicators as the intention-to-treat effects of the YG on outcome y.

The treatment here consists of several ingredients, viz. intensified counselling and monitoring, threat effect, locking-in effects and actual effects of active measures. We interpret our intention-to-treat results as a combination of all these potential effects among the affected age groups. We believe that this provides a relevant measure for assessing the reform effect among the affected age groups. Alternatively, to explore the longer term effects on the young people affected, one could follow two groups of individuals, of which one group was younger and the other group was older than 25 in the beginning of 2005. We have not done any such analysis here as the number of observations is considerably smaller and the estimates less precise, especially when exploring heterogeneous effects.

4.2. Data

Our data was collected from several official registers. The actual linking of different data sources was carried out by Statistics Finland using personal social security numbers. The resulting data set is a 20% random sample of the whole population of young people born in 1967-1990. All these individuals are followed over the years 1987-2010 and in each of these years a 20% random sample of individuals who are new entrants to the population register and are born in 1967-1990 is added to our sample.

The data is primarily created for examining youth labour markets. It includes the usual background information from the Population Register, such as year, month and place of birth, gender, number of children, marital status, place of residence, education etc. Detailed information on earnings and social benefits originate from the Tax Administration, the Social Insurance Institution of Finland and the National Institute for Health and Welfare. Information on all unemployment spells and all active

programme spells comes from the databases of the Social Insurance Institution of Finland and the Ministry of Employment and the Economy. The starting and ending dates of all job contracts that individuals have had over the years come from the registers maintained by the Finnish Centre for Pensions. The data on unemployment spells is of very high quality as people's benefits and pensions are based on this information. Information on parents and their biological maternity/paternity status is added to our data by linking the social security numbers of adults living in the same household as a child to the child's social security number. We also know whether a young person has applied for further education, whether she has been accepted, and whether she is in an educational institution at present. This information comes from the registers maintained by the Ministry of Education. Finally, outcome variables measuring psychotropic drug purchases originate from the Drug Prescription Register maintained by the Social Insurance Institution. The data in this register covers all pharmacies and it is estimated to cover 97-98% of all reimbursed prescriptions[3]. Table A1 gives summary statistics of all the variables used in the estimations.

To ensure the validity of the common trend assumption, our working sample consists of young people between the ages of 23 and 27. As discussed earlier, the guarantee sets up a maximum waiting period of six months before a young person under the age of 25 starts an activation measure. This creates some ambiguity in determining who is actually affected by the reform. We do not know whether an employment agency considers a person whose age at the beginning of an unemployment spell is e.g. 24 years and 10 months as belonging to the treatment group or not. In duration analysis we solve this problem by dropping all spells that started when a person was between 24.5 and 25 years of age. As the DiD regressions are based on yearly data, we drop all individuals who turned 25 during a calendar year. There are also differences in time periods. The duration analyses include all unemployment spells that started during the years 2003-2006, and in the DiD regressions the time period covers the years 2000-2007. We discuss the reasons for choosing the latter period in what follows. We also show that the results are not sensitive to selecting a shorter time period for analysis.

4.3. Descriptive analysis

Figure 3 plots the means of six outcome variables for one year before the reform (2004) and one year after the reform (2006). Panel A displays the share of young people registered as unemployed job seekers, and Panel B days spent in unsubsidized employment during a calendar year. In panel C, we plot the activation ratio that is created by dividing days spent in active measures by days spent in total unemployment (open unemployment + active measures). Panels D - F show, in respective order, the shares of young people who have applied for further education, have no taxable income or receive

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[3] Psychotropic drugs refer to five Anatomical Therapeutic Chemical (ATC) classification subgroups: antidepressants (N06A, N06C), antipsychotics (N05A), anxiolytics (N05B), hypnotics and sedatives (N05C) and psychostimulants for ADHD (N06B).

social assistance. In each panel, the lines refer to averages by month of birth for individuals aged 19-27. The vertical line shows the age limit of 25 years set in the youth guarantee.
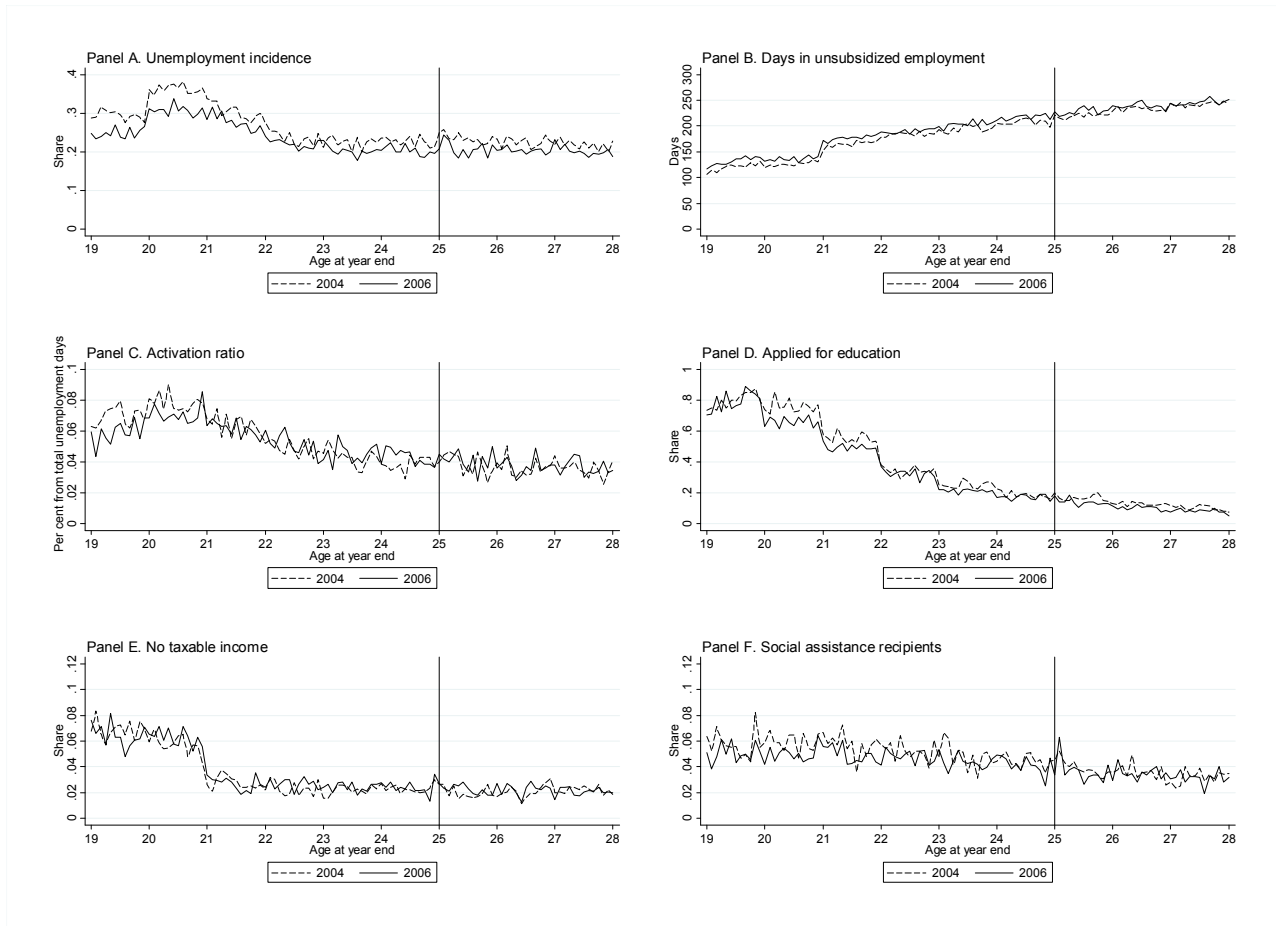
Figure 3. Selected outcome variables

Figure 3 illustrates the limitations in assessing the impacts of the YG. Several outcome variables show a visible jump at the age of 21. Younger cohorts experience more unemployment, have fewer days in unsubsidized employment and less taxable income. This follows from two things. The typical age for completing both general and vocational secondary education is 19 and the majority of boys attend military service soon after graduation. This effectively rules out the inclusion of younger age groups in our analyses. In addition, there are evident differences in older age groups. The two outcome variables that remain roughly similar through the ages 22-27 are the share of individuals experiencing unemployment (20%) and the share of individuals with no taxable income (2%). All the other variables display clear upward or downward trends. Older individuals have more days in unsubsidized employment, and they are less likely to apply for further education or receive social assistance. These differences raise a question about the validity of our research setting where we use slightly older individuals as a control group for slightly younger individuals. The aspect of the data that is beneficial

for our purposes is that we have several pre- and post-periods. This allows us to formally test the assumption of similar trends between different age groups that is vital for identification.

Figure 3 also gives us the first indications of the impact of the reform. The solid line representing the year of 2006 shows improvements in labour market outcomes when compared to the pre-reform year of 2004 marked by the dashed line. But these improvements have happened across the age distribution and there are no clear indications that changes differ on the two sides of the age limit of 25. It is evident from these figures that the YG reform was not the only factor behind a reduction in youth unemployment that happened after 2005.

5. RESULTS

5.1 Baseline results

We begin our analyses by focusing on the impact of the YG reform on transitions during the first 12 months of an unemployment spell[4]. The panels in Figure 4 show the predicted hazard for the treated as solid lines and for the controls as dashed lines. For both groups the period after the reform is separated from the pre-period by markers. The first thing to notice is that all transitions are more common during the post-reform period. The impact of the YG can be examined by comparing the relative change between the treated and the controls. Panel A shows that during the first three months a change in employment hazard is actually smaller among affected young persons. After an unemployment spell exceeds four months the lines start to separate, implying that the reform had a positive impact on transitions from unemployment to unsubsidized employment. The effect is estimated to be in the magnitude of 1-3 percentage points. As the number of unemployed young people who experience longer spells is small, the uncertainty increases at longer spells. Because of that, only one treatment dummy at the duration of 10 months turns out to be statistically significant and this is something one might expect to find even by accident at the conventional levels of significance.

We find a little more convincing evidence on the impacts of the YG in Panel B, which plots hazards from unemployment to active measures. The lines of the treated and the controls separate after two months of unemployment, and the 1-1.5 percentage point differences found in the fourth and sixth month are both statistically highly significant. This coincides nicely with the time period during which activation guarantees written in personalized job search plans should materialize. There are also some fairly large parameter estimates after an unemployment spell exceeds 10 months, but owing to the small number of observations at longer durations, the point estimates fail to be statistically significant.

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[4] These evaluations are based on a proportional discrete time model in which the hazard rate is as in equation (1), all spells exceeding 12 months are treated as censored, and the unobserved heterogeneity follows a normal distribution. We also experimented with continuous time models and models with no unobserved heterogeneity. The results are very similar to those reported.

Panel B also reveals that, despite its name, the YG failed to be a real guarantee as the exit rates to active measures remain fairly modest.

By far the most convincing evidence occurs when exploring exits out of the labour force. We find sizeable and statistically significant differences between the treatment and the control groups at durations between 2-4 months. The observed increase in the first months of unemployment is likely to arise from the more intensive job counselling and monitoring introduced as part of the youth guarantee. Whether this is a good or a bad sign depends on the exact destination of an exit. If there are no opportunities other than ending the unemployment spell, this could lead to a more severe problem of social exclusion. If, on the other hand, intensive job counselling results in additional education, there are clear benefits for society as a whole.

Figure 4. Predicted hazard rates for different age groups before and after the 2005 youth guarantee
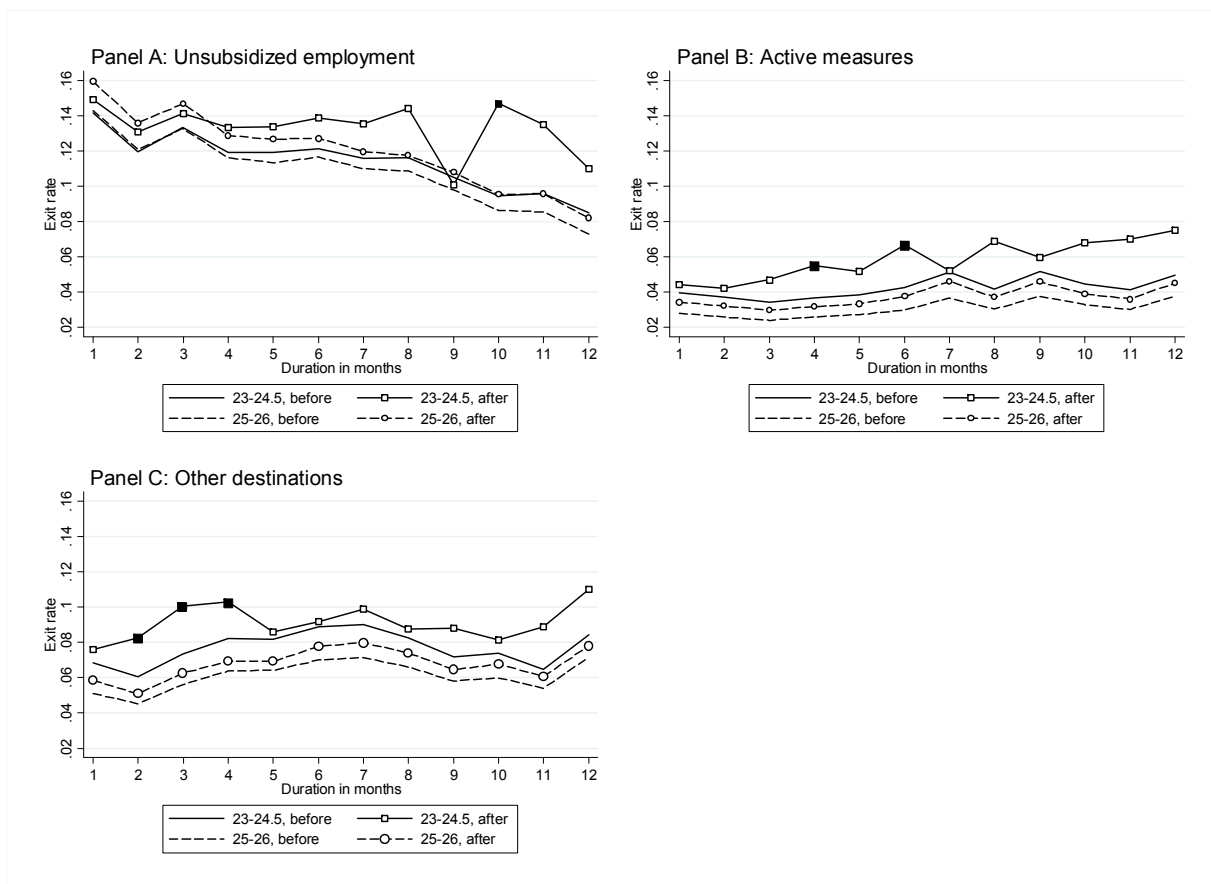
Notes: (i) The predicted probabilities are based on equation 1 which includes the set of explanatory variables displayed in Table A1 and dummy indicators for the NUTS3 place of residence (20 regions); (ii) Statistically significant point estimates (at the 5% significance level) are marked using black squares; (iii) Before indicates that unemployment spells started during 2003-2004 and After indicates that unemployment spells started 2005-2006.

The finding that the YG increased transitions out of the labour force calls for more analysis of the potential effects that youth guarantees might have. Before reporting these additional results it is worth

checking whether DiD regressions can reproduce the results reported in Figure 4. Here we base our statistical inference on clustered standard errors as there is likely to be correlation within groups and possibly across time. The combination of within-group and serial correlation is tricky, and according to Angrist and Pischke (2009) there is no consensus on how to best solve this problem. The simplest approach would be to cluster at the group level only but this cannot be done in our application. The small number of age cohort clusters would lead to badly biased standard errors. It is not, however, evident that our main concern should be correlation within age groups. A more likely scenario is that e.g. economic shocks are more commonly shared among young people living in the same region than among young people of the same age. For this reason, we cluster the standard error with respect to local labour markets.

Table 1 reports the entry effect in the first column, and the baseline results in columns 2-4. These correspond to the DiD regression set up in equation (2). Our three variables that assess the pre-reform differences between the age groups are in rows DiD2002 – DiD2004. The reform effects for separate years are reported in rows DiD2005-DiD2007. The reform effect is summarized in the last row placed between the dashed lines (DiD 2005-07), which corresponds to the specification with only one treatment dummy covering all reform years. The upper panel A reports the pre- and post-reform effects when we include controls for year effects and age effects and the lower panel B reports the corresponding results after controlling for additional covariates, X_{it}.

Table 1 shows no significant pre-treatment differences between the age groups. The one exception is found in the employment regression in Panel A but these differences are eliminated by the inclusion of background variables as shown in Panel B. The first column explores the existence of an unemployment entry effect. This is of considerable interest as previous studies on youth guarantees have primarily analysed unemployment spells, and their results might suffer from selection issues if the threat of intensive job counselling and mandatory activation discouraged affected young people from registering as unemployed job-seekers. There is, however, no evidence of an entry effect as our results indicate that the 2005 reform had no impact on the incidence of unemployment. At the population level the results show an increase of some five days in unsubsidized employment. All the parameter estimates in the unemployment regressions are estimated to be very close to zero and statistically insignificant. Finally, the results on the activation of unemployed young people are well in line with our previous analyses of hazards. Here our preferred specification shows that the YG increased the activation ratio by 1.5 percentage points.

Table 1: Baseline estimation results - population level

	PANEL A – WITHOUT COVARIATES			
	COEFFICIENTS			
	Unemployment incidence	Days unemployed	Days employed	Activation ratio among unemployed
DiD 2002	-0.002	1.00	-5.31***	0.56
	(0.004)	(0.79)	(1.77)	(0.67)
DiD 2003	-0.001	0.89	-4.96***	0.51
	(0.005)	(1.08)	(1.84)	(0.64)
DiD 2004	-0.000	0.79	-2.72*	0.20
	(0.006)	(1.09)	(1.82)	(0.65)
DiD 2005	0.001	0.32	0.40	1.37**
	(0.005)	(1.14)	(1.66)	(0.69)
DiD 2006	-0.004	0.01	2.44	2.00***
	(0.005)	(1.08)	(2.09)	(0.63)
DiD 2007	0.002	0.76	2.02	2.51***
	(0.006)	(0.80)	(2.56)	(0.54)
DiD 2005-07	-0.000	0.36	1.63	1.94***
	(0.005)	(0.95)	(1.88)	(0.45)
N	419,538	419,538	419,538	71,931
Adj. R²	0.00	0.00	0.02	0.01
	PANEL B – WITH COVARIATES			
	COEFFICIENTS			
	Unemployment incidence	Days unemployed	Days employed	Activation ratio among unemployed
DiD 2002	-0.007*	-0.32	-2.08	0.21
	(0.004)	(0.72)	(1.75)	(0.67)
DiD 2003	-0.008*	-0.86	-0.39	0.03
	(0.004)	(0.98)	(1.77)	(0.65)
DiD 2004	-0.005	-0.56	2.59	-0.28
	(0.005)	(0.93)	(1.65)	(0.62)
DiD 2005	-0.001	-0.49	4.71***	0.89
	(0.004)	(0.95)	(1.56)	(0.76)
DiD 2006	-0.005	-0.35	5.43***	1.64***
	(0.005)	(0.89)	(1.44)	(0.62)
DiD 2007	-0.000	0.39	4.38**	1.94***
	(0.005)	(0.63)	(1.77)	(0.53)
DiD 2005-07	-0.002	-0.15	4.84***	1.47***
	(0.004)	(0.76)	(1.35)	(0.47)
N	419,538	419,538	419,538	71,931
Adj. R²	0.09	0.08	0.13	0.04

Notes: (i) Unemployment refers to open unemployment and employment refers to unsubsidized employment; (ii) All estimations include the main effects for age groups and years. The estimations reported in Panel B also include the set of explanatory variables displayed in Table A1 and dummy indicators for place of residence measured at the NUTS3 level (20 regions); (iii) The standard errors are clustered with respect to residential areas created by combining NUTS3 place of residence, truncated NUTS2 unemployment rate and the degree of urbanization;; (iv) *** $p<0.01$, ** $p<0.05$, * $p<0.1$

5.2. Heterogeneous results

Next, we turn to additional results that extend our analyses in two ways. The first expansion is motivated by the explicit instruction given to employment offices to divide young people into different groups according to their educational level. To explore heterogeneous effects at different levels of education, we extract a sample of young people who were unemployed during the first half of a year and who have either compulsory or vocational education. To recall, under the YG a young person has to be offered an activation measure before the sixth month in unemployment. By focusing on young people who have been unemployed during the first six months of a year, we want to make sure that they can be actually affected by the YG during that year[5]. The skill division follows from the Finnish educational system. The mean age of graduating from a Finnish university is over 28 years of age, which means that the majority of young people who have completed upper secondary education are still studying at ages 23-27. Thus the composition of young people with a tertiary degree is likely to differ between the two age groups studied. Those who enter and graduate from tertiary education at early ages may have better labour market prospects than others due to e.g. motivation or ability.

The second extension relates to various outcomes in young peoples' lives that might also be affected by the youth guarantee. One aim of the reform was to encourage uneducated young persons to return to the ordinary educational system. We study this via two outcomes, viz. applying for education and actually being enrolled in an educational establishment. Potential income gains are explored via three outcomes: taxable income, the share of youngsters with no taxable income and the share of young people receiving social assistance. Since social assistance is non-taxable income provided as a last resort, this particular measure gives us a good indication of whether the YG reform affected youngsters who face the most severe difficulties in supporting themselves. Finally, we explore the impacts of the YG on the mental health of affected young persons by examining the use of antipsychotic drugs. Here we have two outcome measures. The first explores purchases of any psychotropic drugs within a year in one of the five ATC categories. The second registers only purchases of antipsychotic drugs (N05A) that are prescribed for more severe mental health conditions. The causal effect of activation measures on mental health is relatively unstudied in economics, but occupational psychologists have some experimental evidence that active measures, such as a job search programme, reduce levels of depressive symptoms, see e.g. Vinokour et al. (2000) and Vuori and Silvonen (2005). Provided that there is enough correlation between symptoms and psychotropic drug usage, these findings imply that an increase in activation measures might also show up as a reduction in the use of psychotropic drugs. At this point it is worth recalling that participation in an active labour market programme is only one part of the YG. Other parts of the YG, most notably

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[5] We estimated the same DiD regressions with the sample of young people who experienced unemployment during a year. The results are close to those reported and all of the conclusions remain the same.

early intervention and intensified monitoring, might also increase drug use if they result in the diagnosis of a previously hidden mental health problem that requires medication.

Almost all the point estimates for the pre-reform period in Table 2 are small and statistically insignificant, implying that the target and the control group experience similar trends before the reform. The only difficulties arise when exploring the employment days and activation ratios of young people with no further education. In the case of employment days, it would be possible to equalize the pre-reform trend by dropping the youngest and the oldest age groups from the analysis and by comparing only 24-year-olds to 26-year-olds. Since the (unreported) post-reform effects remained insignificantly different from zero even in this case, we decided to report the results for the same age groups in both panels. We believe that this makes the analysis more transparent and helps comparisons between levels of education.

Table 2 reveals that the post-reform effect masks substantial heterogeneity with respect to the level of education. Surprisingly, significant reform effects arise solely among unemployed young people who have graduated from a vocational secondary education institution. The parameter estimates reported in Panel A show no real improvements among unemployed young people with only compulsory education. If one focused solely on the first year of the YG reform, one could argue that the guarantee had a negative impact among unskilled young people. The results are qualitatively similar in later years, but they turn out to be statistically insignificant. This is also the case when testing the overall significance of post-reform estimates, which leads us to conclude that the youth guarantee had no impact on any outcome among uneducated and unemployed young people.

The point estimates reported in panel B refer to unemployed young people with vocational education, and they show a statistically significant reduction in unemployment and an increase in both employment and activation ratios. On average, our findings show a fall of seven days in unemployment, a similar rise in employment days and an increase of over two percentage points in the activation ratio. The rise in the activation ratio corresponds to an increase of over five days in active measures. In relative terms, this increase is by far the greatest as the affected group spent 34 days in activation measures before the reform. The corresponding figures for days in unemployment and employment were 149 days and 126 days, respectively.

Table 2. Treatment effect heterogeneity with respect to the level of education – unemployed young people

PANEL A – BASIC EDUCATION

	(1) Days unemployed	(2) Days employed	(3) Activation ratio	(4) Applied for education	(5) Studying	(6) Taxable income (€)	(7) No taxable income	(8) Social assistance	(9) Psychotropic medication	(10) Anti-psychotic medication
DiD 2002	-0.92	10.64**	-1.44	n/a	0.009	-3	0.010	-0.022	-0.001	0.002
DiD 2003	9.42	8.27	-1.91	n/a	-0.004	-11	0.008	-0.006	-0.005	-0.017
DiD 2004	-2.24	13.12**	-2.68**	0.02	-0.004	81	-0.003	0.003	0.002	0.002
DiD 2005	7.88*	3.78	-2.46*	-0.01	-0.010	-33	0.031***	0.000	-0.018	0.008
DiD 2006	7.29	-0.31	-1.37	0.02	-0.007	-501	0.017	0.011	-0.006	0.000
DiD 2007	8.16	2.82	-1.51	-0.03	-0.007	-613*	0.001	0.022	-0.011	-0.009
DiD 2005-07	7.77	2.09	-1.79	-0.00	-0.008	-399	0.017	0.011	-0.011	-0.000
Adj. R²	0.10	0.04	0.06	0.04	0.03	0.07	0.05	0.05	0.04	0.01
N	17,892	17,892	17,892	11,059	17,892	17,892	17,892	17,892	17,892	17,892
Time period	2000-07	2000-07	2000-07	2003-07	2000-07	2000-07	2000-07	2000-07	2000-07	2000-07

PANEL B – VOCATIONAL EDUCATION

	(1) Days unemployed	(2) Days employed	(3) Activation ratio	(4) Applied for education	(5) Studying	(6) Taxable income (€)	(7) No taxable income	(8) Social assistance	(9) Psychotropic medication	(10) Anti-psychotic medication
DiD 2002	1.11	-4.97	1.11	n/a	0.05***	-441	0.002*	-0.013	-0.004	0.005
DiD 2003	-4.53	6.68	0.45	n/a	0.02	481	0.002	-0.011	-0.005	0.002
DiD 2004	2.59	-1.99	1.10	0.03	0.01	113	0.003	-0.006	0.008	0.007
DiD 2005	-9.03**	4.44	2.09*	0.01	0.02	123	0.004	-0.012	0.006	0.004
DiD 2006	-5.73	10.19**	1.94**	-0.01	0.01	401	-0.002	-0.021*	-0.010	-0.009
DiD 2007	-6.27*	6.61*	2.85***	-0.04*	0.02	530	-0.003	-0.012	-0.028**	-0.001
DiD 2005-07	-7.06**	7.05**	2.27***	-0.02	0.02	343	-0.001	-0.015*	-0.009	-0.002
Adj. R²	0.04	0.03	0.02	0.04	0.02	0.07	0.00	0.03	0.02	0.01
N	34,155	34,155	34,155	20,709	34,155	34,155	34,155	34,155	34,155	34,155
Time period	2000-07	2000-07	2000-07	2003-07	2000-07	2000-07	2000-07	2000-07	2000-07	2000-07

Notes: (i) See table 1; (ii) All estimations include main effects for age groups and years, the set of explanatory variables displayed in Table A1 and dummy indicators for place of residence measured at the NUTS3 level (20 regions).

Other impacts of the YG remain modest. There are some indications of reductions both in the need for social assistance and in purchases of psychotropic drugs. These findings coincide well with the results reporting improvements in employment prospects, but we do not want to push this interpretation too far for several reasons. First, the point estimates are relatively small, varying between 1-3 percentage points. Second, the point estimates are not very precise as four out of five marked point estimates reach only the 10% significance level. Third, the most convincing finding, according to which purchases of psychotropic drugs declined by 3 percentage points in 2007 (baseline being 11 percentage points), coincides with a change in the Pharmaceutical Reimbursement System introduced on 1 June 2006. One potentially problematic change introduced then was that cheap drugs costing less than 9 euros began to be registered in the prescription register. This resulted in an increase of almost 20% in registered psychotropic drug purchases. It is not totally evident why this increase should be relatively smaller among unemployed young people with vocational education and under 25 years of age. However, as there seems to be a similar, albeit less evident, evolution in purchases of psychotropic drugs among uneducated young persons in this age group, we are cautious in attributing all of the observed effect to the YG.

Finally, there are some signs that the YG increased taxable income, but these estimates fail to be statistically significant. This finding might imply that improvements in employment happened in low-wage jobs since the net increase in taxable income obtained by deducting lost unemployment benefits from gained wages is statistically zero. This conclusion has to be considered with caution since a net increase in earnings caused by seven more days in employment may not be big enough to be measured accurately.

5.3. Interpretation

One question that calls for closer attention is why the 2005 reform primarily affected more skilled unemployed young people with vocational education. It is especially illogical that the YG increased activation among this group as it is well documented that uneducated youngsters face more severe difficulties in labour markets and they are also more vulnerable to the discouragement effects of unemployment. Our explanation for the allocation of active measures between the two groups of young job seekers is provided in Figure 5. This shows the empirical hazard rates for exits from unemployment to active measures for the years 2000-2010 at different lengths of unemployment. Panel A reports the hazard rates for uneducated young people and panel B for those with a vocational education. The solid curves refer to the hazard of exit from unemployment to an active measure within the first 30 days in unemployment, the dashed line with a hollow triangle marker indicates the corresponding hazard of unemployment spells between 30 and 60 days provided that an unemployment spell has lasted at least 30 days, and so on. The 2005 YG reform is marked with a vertical line.

Figure 5. Empirical hazard rates from unemployment to active measures in different phases of young people's unemployment spells, 2000-2010
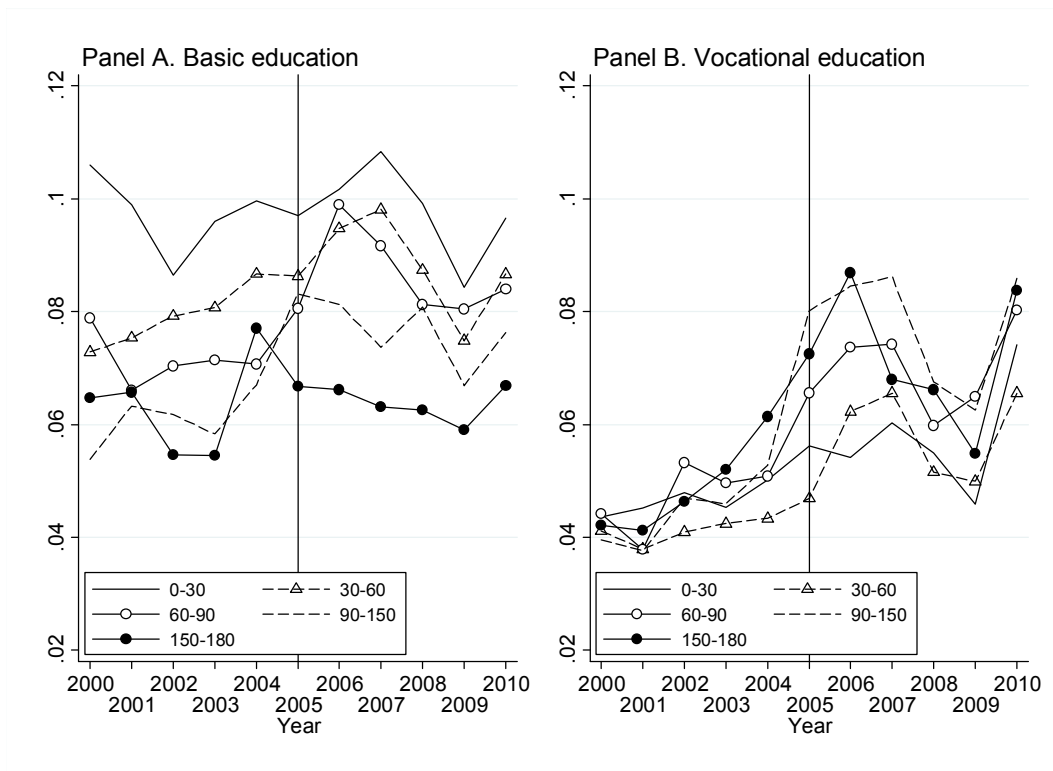
Notes: (i) The time intervals refer to days; (ii) The last two categories (90-150, 150-180) refer to the weighted average of two 30-day intervals.

Figure 5 shows that before the 2005 reform uneducated persons had a higher risk of entering active measures than those with vocational education, regardless of the length of unemployment spell. The difference is especially striking at the beginning of unemployment. Within the first 30 days of unemployment some 10% of uneducated young people experienced a transition to active measures, compared to some 4% among those who had vocational education. During the next 30 days, the corresponding figures are 8% and 4%, respectively. This follows from the emphasis on early intervention adopted by the labour administration for uneducated young persons already in the late 1990s. In this group early intervention is justified since the labour administration wants to keep unemployment experiences at the minimum in this low-skilled group and to help them gain further education via active measures. But Figure 5 indicates that early intervention with uneducated young people did not coincide well with the fixed time points of three and six months that were set in the 2005 youth guarantee. As the labour administration tried to fulfil the promise of an activation guarantee, it increased activation among those unemployed youngsters who were less intensively activated in the early stages of their unemployment spells i.e. those with a vocational education.

Do our findings tell us something about the effectiveness of active labour market programmes among educated young unemployed people? Probably, since we only observe positive effects in this

group, whose members were more likely to participate in active programmes owing to the YG reform. Before pushing this interpretation too far, it has to be noted that the YG also introduced a number of other changes, and our data does not allow us to separate e.g. the counselling/monitoring effect from the participation effect. A proper evaluation of the importance of active programmes requires them to be assessed separately from other changes. In our case, the actual increase in the activation rate is not likely to be large enough to create a reliable quasi-experimental research setting. For these reasons, we interpret our findings as a combination of different effects and do not try to assess their relative importance in our context.

6. ROBUSTNESS CHECKS

The difference-in-difference estimates in previous sections eliminate the age and time effects assuming that the outcome variables evolve similarly in the treated and control groups. We have already tested this assumption by including pre-reform DiD variables in our estimations. Our data enables us to do even more than that. Since our data starts from the year 1987 it is possible to repeat the above analysis for the years before the introduction of the guarantee. We carry out a number of these regressions by moving the eight-year estimation window forwards from the year 1994 and leaving the last three years for the placebo treatment. The results of the placebo tests are reported in Figure 6, in which the horizontal axis shows the years of the placebo treatments, the dots show the point estimates and the vertical lines indicate their confidence intervals. We focus on three outcome variables for which we found significant reform effects above, so the results are comparable to Panel B of Table 2, with one exception. The first year for which we have parental data is 1987, so the oldest birth cohort for whom we get parental information is only 21 years of age in 1994. Hence we had to drop parental information from our placebo regressions.

Figure 6 shows that in Panels A and B all the pre-reform placebo effects turn out to be statistically insignificant, and in a majority of cases also oppositely signed to those reported in Table 2. Hence there seems to be no element in our empirical specification that systematically produces the reported YG effects on unemployment and employment[6]. One further point regarding the placebo effects is that it is not surprising that they disappear when entering the last placebo period of 2007-09. After 2007, the control group consists entirely of young people who were in the treatment group during the first year of the YG. The only way that there could be significant effects in the placebo years 2007-09 would be if the YG becomes more effective over time. There is no evidence for that.

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[6] We also estimated the DiD regressions without covariates. These (unreported) results show that the estimates reported in Figure 6 are not sensitive to controlling for background characteristics. This particular finding indicates that the common trend problem reported for the employment regression at the population level in Table 1 was caused by differences in the education levels between the age groups.

Figure 6. The results of placebo tests on young unemployed people with vocational education
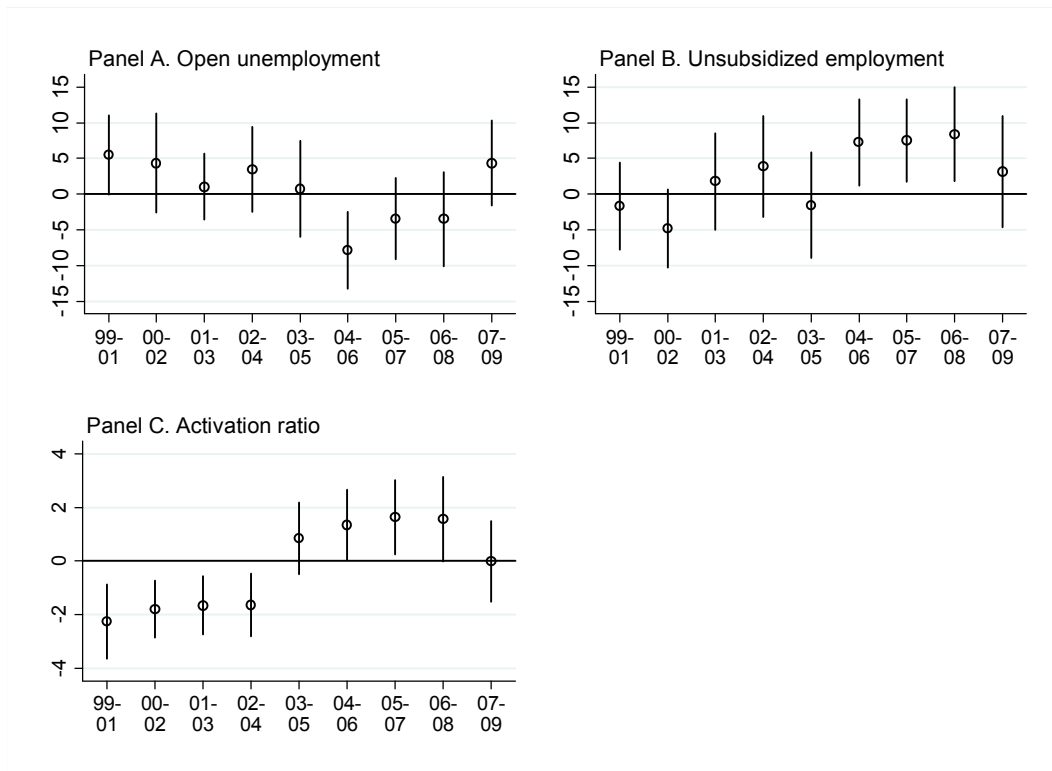
Notes: (i) Panels A and B refer to days and panel C to percentage points; (ii) The dots display the coefficients of dummy variables that are set to unity for the age group of 23-24 during the period of three years shown on the horizontal axis; (iii) The vertical lines show the 95% confidence intervals based on clustered standard errors.

If panels A and B pass all the placebo tests, panel C shows statistically very significant differences between the treatment and the control group before the actual YG reform. All the pre-reform estimates are found to be negative, which tells us that activation of the slightly older control group was intensified during these years relative to the treatment group of slightly younger individuals. Starting from the period 2003-05, when the first actual reform year starts to affect the treatment dummy, the parameter estimates jump from negative to positive, which implies a change in the composition of activation. The highly significant pre-reform effects, however, cast serious doubt on the robustness of the actual reform effect on the activation ratio, which was reported to be over two percentage points in Table 2. To gain a better understanding of this we plot the activation ratios for the two groups of unemployed young persons with vocational education in 1994-2010.

Figure 7. Activation ratios for unemployed young persons with vocational education, 1996-2010

Figure 7 reveals that the negative pre-reform placebo estimates result from a sharp increase in activation among the control group during the years 1998 and 1999. This coincides with the reform of Finnish labour market policy that was introduced in the beginning of 1998 and gradually implemented during the next two years. The main changes in the 1998 reform were the introduction of job search plans, regular meetings with the labour authorities and job search training. The activation ratio was especially affected by job search training consisting of a short course lasting a week during which individuals were taught how to seek vacancies, update their CV's, write job applications etc. Our placebo results pick up the effects of this reform as job search training was targeted more intensively for slightly older persons.

There is one more thing to be worried about. The period 1999-2001, when the difference between the activation ratios of the treated and the controls was non-existent, coincides with the first two years in our DiD regressions. This would bias our preferred results if job search training affected the labour market prospects of the participants. If job search training has any effects, the most likely scenario would be that participation in a job search training course improves the participant's possibilities to get a job. In this particular case our results for the unemployment and employment effects would be biased downwards as participation in job search training was more common among the control group during the reference years. There is, however, fairly convincing evidence based on two randomized experiments according to which job search training has no impact on the further employment prospects of participants, see Hämäläinen et al. (2008). This would imply that our results concerning

the unemployment and employment effects of the YG are unaffected by the 1998 reform, but the activation results are likely to be upward-biased. To gain some insight into this, we re-estimated our DiD regressions using the estimation period 2002-2007. As there are only three pre-reform time periods before the 2005 YG reform, we did not include any pre-reform effects in these estimations. Our (unreported) point estimates are -6.88**, 7.19* and 1.38** for unemployment, employment and the activation ratio, respectively. To get a fair comparison point for these estimates we re-estimated the previously reported models by excluding all pre-reform variables. This resulted in parameter estimates of -6.89**, 7.09** and 1.74***.

These results confirm that by maximizing the estimation period and exploring potential pre-reform differences we did not cause any bias in our employment and unemployment estimates, but slightly overestimated the impact that the youth guarantee had on the activation of unemployed young persons with vocational education. However, the activation effect still remains both statistically significant and positive. One additional piece of information is that in these re-estimations we again failed to find any increase in the activation ratios of low-skilled unemployed young persons with no further education.

7. CONCLUSIONS

This paper provides new insight into the effects of activation guarantees offered to young people by the public sector. Our contribution is twofold. First, we provide new evidence from Finland by analysing the 2005 youth guarantee which applied to young persons under the age of 25. Second, we broaden the analyses carried out in previous studies by examining the unemployment entry effect, educational effect, income effect and psychotropic medication usage effect of the YG. Our baseline results resemble previous studies. We find only limited impact on transitions from unemployment to employment and show that the 2005 reform was far from a subjective activation guarantee. Disturbingly, there is also some evidence of out-of-labour force transitions among the young persons concerned.

Further examinations give support to previous studies that have mainly focused on transitions out of unemployment as we do not find any evidence of the young persons concerned opting out of registering as unemployed job seekers after monitoring and activation is intensified. The most surprising of our findings is that the 2005 youth guarantee only affected unemployed young people with vocational education. We find no increase in activation, or any change in other outcomes, among the most vulnerable group of youngsters consisting of unemployed and uneducated young people. In contrast, among unemployed young persons with vocational education the activation ratio was increased by 2 percentage points, days in open unemployment reduced by 7 days and days in non-subsidized employment increased by 7 days. There are also some, albeit less convincing, implications

that the YG reduced both the need for social assistance and purchases of psychotropic drugs among educated young persons. We attribute the heterogeneous effects to the activation rules that prevailed before the 2005 reform. Early intervention was already in place among uneducated job seekers, so the new activation periods only affected educated job seekers.

All in all, the 2005 youth guarantee strengthened the activation of young people under the age of 25 but it did not offer any subjective rights for activation measures. This has also been the case in other countries as well as with the renewed version of the YG that was introduced in Finland in 2013. It is far from evident that the labour authority should offer an activation measure for every unemployed young person in the first place. But it is probable that more countries will intensify their activation of their youth populations following the guidelines of the EU Commission. Our results indicate that this would not necessarily lead to more intensified activation among young people who have the most severe difficulties in entering the labour market. One thing to bear in mind when introducing a youth guarantee scheme with prescheduled activation time points is to think about how it interacts with existing rules and practices for allocating activation measures to different groups of young people.

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Table A1: Descriptive statistics

	POPULATION				UNEMPLOYED			
	2003-2004		2005-2006		2003-2004		2005-2006	
	23-24	26-27	23-24	26-27	23-24	26-27	23-24	26-27
DEPENDENT VARIABLES								
Unemployment incidence	0.23	0.22	0.21	0.21	1	1	1	1
Days in open unemployment	27	31	23	27	141	160	128	152
Days in unsubsidized employment	200	238	208	241	120	121	122	124
Activation ratio	3.97	3.41	4.37	3.71	12.99	10.72	14.92	11.31
Applied for education	0.21	0.11	0.21	0.10	0.36	0.17	0.36	0.16
Studying	0.49	0.28	0.50	0.31	0.18	0.13	0.21	0.15
Taxable income	12 715	18 453	13 599	19 185	10 424	12 372	10 684	12 846
No taxable income	0.02	0.02	0.02	0.02	0.03	0.01	0.04	0.01
Social assistance recipient	0.05	0.04	0.04	0.03	0.14	0.12	0.15	0.12
Psychotropic medication	0.05	0.06	0.07	0.08	0.09	0.10	0.11	0.12
Antipsychotic medication	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.02
PERSONAL CHARACTERISTICS								
Basic education	0.16	0.13	0.17	0.15	0.26	0.23	0.28	0.25
A-level	0.37	0.17	0.36	0.18	0.09	0.07	0.09	0.07
Secondary education	0.37	0.36	0.37	0.37	0.52	0.43	0.53	0.43
Bachelor	0.09	0.25	0.08	0.22	0.12	0.22	0.09	0.18
Master	0.01	0.09	0.01	0.09	0.01	0.06	0.01	0.07
Male	0.51	0.52	0.51	0.51	0.53	0.51	0.52	0.52
Child less than 7	0.13	0.25	0.13	0.24	0.15	0.27	0.15	0.25
Married	0.09	0.22	0.09	0.21	0.10	0.19	0.09	0.19
Regional unemployment rate	11.36	11.17	9.84	9.63	12.28	12.09	10.80	10.54
IMMIGRANT FROM								
Russia/Estonia	0.010	0.011	0.011	0.012	0.015	0.016	0.014	0.016
EU	0.003	0.006	0.003	0.006	0.002	0.004	0.004	0.005
OECD	0.002	0.003	0.002	0.003	0.001	0.003	0.003	0.004
Yugoslavia/Iran/Iraq/Somalia	0.004	0.005	0.004	0.005	0.009	0.011	0.012	0.013
Other country	0.008	0.010	0.009	0.014	0.010	0.014	0.012	0.019
PARENTAL INFORMATION AT THE AGE OF 14								
Mother biological	0.93	0.92	0.92	0.91	0.90	0.89	0.89	0.88
Mother employed	0.71	0.78	0.72	0.71	0.63	0.70	0.62	0.62
Mother's income	14 732	14 236	15 715	14 197	12 583	12 357	13 133	12 412
Mother has secondary education	0.36	0.35	0.37	0.36	0.41	0.37	0.41	0.38
Mother has tertiary education	0.28	0.24	0.31	0.26	0.17	0.16	0.19	0.18
Mother missing	0.04	0.03	0.04	0.03	0.05	0.05	0.06	0.05
Father biological	0.77	0.78	0.75	0.76	0.69	0.71	0.67	0.69
Father employed	0.67	0.73	0.68	0.67	0.58	0.65	0.57	0.58
Father's income	19 854	20 325	21 333	19 260	15 336	16 616	16 342	15 676
Father has secondary education	0.30	0.29	0.31	0.29	0.32	0.30	0.34	0.30
Father has tertiary education	0.26	0.24	0.26	0.24	0.14	0.16	0.14	0.17
Father missing	0.15	0.12	0.16	0.14	0.19	0.16	0.20	0.17