

Chapter 15

The Evaluation of a Finnish Youth Guarantee: Lessons for Europe?

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15.1 Introduction

The number of unemployed young people in the EU exceeds the population of Denmark. Youth unemployment brings about economic costs in terms of lost production and social benefit payments. The social costs are potentially 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 offering quality job offers, active labour market measures, better public employment services and apprenticeship schemes. 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 was 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 deliver something new to tackle youth unemployment. Existing evidence from Nordic countries 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

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guarantee 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 due to the locking-in effect during the activation measure, while Hall and Liljeberg report a positive employment effect after the activation period. Hardoy et al. (2006) report somewhat more positive effects for Norway. Their results show an increase in the transition rate from unemployment to employment of a magnitude of 4–11%.

Overall evidence is scarce, but some of the key elements embedded in youth guarantees have been studied in detail. Intensified counselling and increased monitoring have been found to have positive employment effects in, e.g. 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). Studies on mandatory activation emphasize that the mere threat of activation increases employment rates; see Black et al. (2003), Graversen and van Ours (2008) and van den Berg et al. (2009). Finally, the vast literature on the actual treatment effects of active measures has been summarized in recent meta-analyses by Card et al. (2010, 2015). These analyses show heterogeneous effects, varying from positive employment effects of employment subsidies in the private sector to zero effects from public sector placements.

This study contributes to the 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 identified the Finnish youth guarantee as being best practice for other member states. Even though the Commission referred to the 2013 version of the Finnish YG, the principal elements 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 prescheduled procedure, stricter monitoring, job search plans in the early stages of unemployment and guaranteed activation. Since no changes were introduced 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. Unlike in previous studies focusing unemployment durations, our analyses cover the whole target population – for two reasons. First, some of the affected young people may choose not to register as an unemployed jobseeker 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 7 days per year. Our primary finding is that the youth guarantee

reform affected skilled unemployed young persons who already had a vocational secondary education. We find no effects among unskilled young persons with only compulsory schooling. The most likely explanation arises from the fact that early activation was already used among uneducated youngsters before the introduction of the YG.

15.2 The Youth Guarantee

Finland has a 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 3% to 17% in just 3 years. The deep recession was especially harsh among young people, whose unemployment rate peaked at 35%. The recession was followed by a long period of economic growth, but the Finnish youth unemployment rate remained at a much higher level than the EU15 (and EU28) average until the 2009 financial crisis.

In order to tackle youth joblessness, the government introduced the youth guarantee 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 and guaranteed activation. All activation is based on an individualized job search plan. Prior to the 2005 reform, the plan was drafted by the PES within 5 months of unemployment, and this schedule was the same for all unemployed jobseekers, irrespective of age. Prior to the YG scheme, the plan did not necessarily include any activation measures, nor was there any obligation for the local PES to arrange any activation.

The 2005 reform changed the services for young jobseekers in three important ways. First, a preparatory counselling meeting had to take place within 1 month of registering. In this meeting the caseworker assesses the individual service needs of the young jobseeker, explains the activation procedure and drafts a preparatory job search plan. Second, the completion of an individualized job search plan was brought forwards from 5 months to 3 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 include the explicit activation measure agreed upon. The local employment authority is obliged to offer this activation measure within 3 months of signing the contract. At the same time, the job search plan obligates the young jobseeker, and non-compliance can be sanctioned.

The implementation guidelines of the YG divided unemployed young persons into two groups. For skilled young persons with vocational education, the main goal is regular employment, and the most employment-eligible skilled young persons are steered towards an independent job search. The active labour market programmes on offer for the skilled group are job coaching, work practice and subsidized employment. The second group consists of uneducated young persons to whom the main aim is to (re-)enter the ordinary education system. Services for this group include career planning and information on various educational possibilities. These are mere guidelines,

and the most common activation measure for both skill groups, representing more than half of all participants, is work practice. This is nonsalaried 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, over 37,000 young unemployed persons passed the 3-month unemployment spell limit in 2005, and only around 10,400 (28%) young persons had a signed job search plan at that time. In January–August 2006, 70% of 30,700 youths passing the 3-month time limit had a signed plan, and towards the end of 2007, the share had risen to 77%.

The statistics on the costs of the reform are incomplete. As the YG was implemented during a period of declining unemployment, the overall workload at employment agencies was simultaneously decreasing. A survey conducted to examine the effectuation of the reform reveals that employment agencies typically reallocated at least some of the freed resources to youth services and some agencies also recruited new caseworkers, especially vocational psychologists (Pitkänen et al. 2007). Thus, the number of caseworkers allocated to youth services appears to be increased, but there are no statistics to give us the exact number. To illustrate a potential magnitude of this, assume that every one of the 140 employment agencies added one caseworker for the youth services. A rough estimate for this would be around 7 million euros per year. The average cost price of one job search plan was 79 euros in 2003 (National Audit Office of Finland 2005) that gives an additional yearly cost estimate of 1.2 million euros if new plans were prepared for 15,000 youngsters. The reform also increased the number of active measures targeted to young persons. The cost of one additional participant ranges from 2200 to 10,000 euros per man-year depending on the type of measure (Ministry of Finance 2010). This results in additional costs around 2.2–10 million euros for every 1000 additional man-years in active measures. To cover additional costs of the YG, the government granted an additional 20 million euros for the implementation of youth guarantee in 2006. Otherwise, the expenses allocated to the YG are not separable from other expenses in government budgets.

15.3 Empirical Strategy

15.3.1 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 our application, we estimate DiD regressions of the form.

$$y_{it} = \alpha + \lambda_a + \gamma_t + \sum_m^{k=l} \delta_k D_{it-k} + x_{it}'\beta + \varepsilon_{it}, \quad (15.1)$$

where λ_a and γ_t are the main effects controlling for age and time, 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 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 consists of several ingredients, viz. intensified counselling and monitoring, threat effect, locking-in effect and actual effects of active measures. Our intention-to-treat results measure a combination of all these potential effects among the affected age groups. Alternatively, to explore the longer term effects, 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.

15.3.2 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 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 new entrants to the population register who 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 the starting and ending dates of all

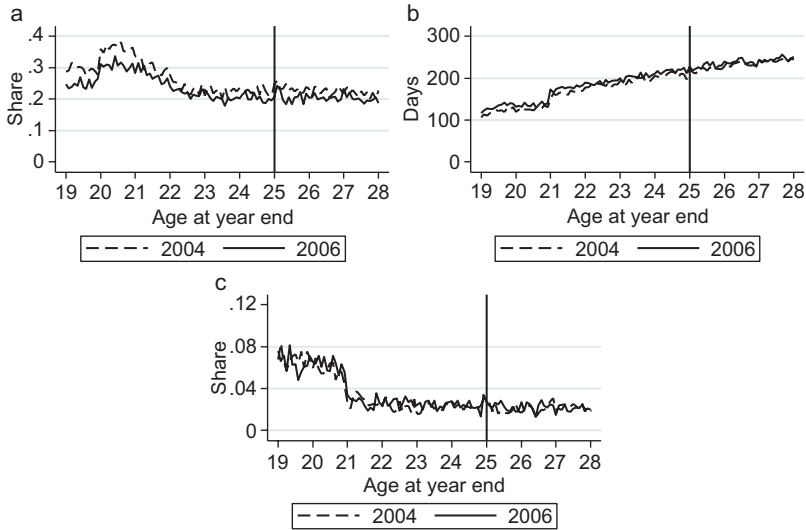


Fig. 15.1 Selected outcome variables: (a) unemployment incidence, (b) days in unsubsidized employment, (c) no taxable income

unemployment and active programme periods comes from the databases of the Social Insurance Institution of Finland and the Ministry of Economic Affairs and Employment. Analogous information on all job contracts come from the registers maintained by the Finnish Centre for Pensions. The data is of 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. This information comes from the registers maintained by the Ministry of Education and Culture. 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.¹

Figure 15.1 plots the means of three outcome variables for 1 year before the reform (2004) and 1 year after the reform (2006). Panel a displays the share of young people registered as unemployed jobseekers, panel b days spent in unsubsidized employment during a calendar year and panel c the share of young people who have no taxable income during a calendar year. In each panel, the lines refer to

¹Psychotropic drugs refer to five Anatomical Therapeutic Chemical (ATC) classification sub-groups: antidepressants (N06A, N06C), antipsychotics (N05A), anxiolytics (N05B), hypnotics and sedatives (N05C) and psychostimulants for ADHD (N06B).

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 15.1 illustrates the limitations in assessing the impacts of the YG. All three outcome variables show a visible jump at the age of 21. Younger cohorts experience more unemployment and have fewer days in unsubsidized employment and less taxable income. The observed jumps follow from two things. The typical age for completing 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, as older individuals have more days in unsubsidized employment. 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.

To ensure the validity of the common trend assumption, our working sample consists of young people between the ages of 23 and 27. To recall, the guarantee sets up a maximum waiting period of 6 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. For this reason, we do not include individuals who turned 25 during a calendar year in our analyses.

15.4 Results

Here we focus on the results based on the working sample consisting 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 6 months of a year, we want to make sure that they can be actually affected by the YG during that year.² The skill division follows from the explicit instruction given to employment offices to divide young people into different groups according to their educational level and 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 attend tertiary education are still studying at ages 23–27.

²We 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.

There are several channels through which the youth guarantee may affect young peoples' lives. One aim 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. Social assistance is nontaxable income provided as a last resort, so it 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 mental health 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)). 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 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.

Table 15.1 reports our main results. 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. These effects are summarized in the last row placed between the dashed lines (DiD2005–DiD2007), which corresponds to the specification with only one treatment dummy covering all reform years. The upper panel A reports the results for young unemployed persons with basic education and the lower panel B for unemployed young people with vocational education.

Almost all the point estimates for the pre-reform period in Table 15.1 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 trends by dropping the youngest and the oldest age groups from the analysis and by comparing 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.

The results reveal substantial heterogeneity. Surprisingly, significant reform effects arise solely among unemployed young people who have graduated from a vocational secondary education institution. The parameter estimates reported in

Table 15.1 The effects of youth guarantee with respect to the level of education

Panel A – Basic education										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Days unemployed	Days employed	Activation ratio	Applied for education	Studying	Taxable income (€)	No taxable income	Social assistance	Psychotropic medication	Antipsychotic 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. R ²	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

(continued)

Table 15.1 (continued)

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

(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 set of explanatory variables consisting of personal information (gender, having a child under 7 years of age, marital status, regional unemployment rate), immigrant status (Russia/Estonia, EU, OECD, Yugoslavia/Iraq/Somalia, other country), mother's and father's information at the age of 14 (biological parent, employment, income, the level of education, a missing parent) 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) *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

panel A show no real improvements among unemployed young people with only compulsory education. The point estimates reported in panel B, however, show a statistically significant reduction in unemployment and an increase in both employment and activation ratio among unemployed young people with vocational education. Our findings show a fall of 7 days per year in unemployment, a similar rise in employment days and an increase of over 2 percentage points in the activation ratio. The rise in the activation ratio corresponds to an increase of over 5 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.

Other impacts of the YG remain modest. Panel B shows 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 and 3 percentage points. Second, the point estimates are not very precise as three out of four 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 under 25 years of age with vocational education. However, as there seems to be a similar, albeit less evident, evolution in purchases of psychotropic drugs among uneducated young persons, we are cautious in attributing all of the observed effect to the YG.

Finally, there are some signs in panel B 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.

15.5 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.

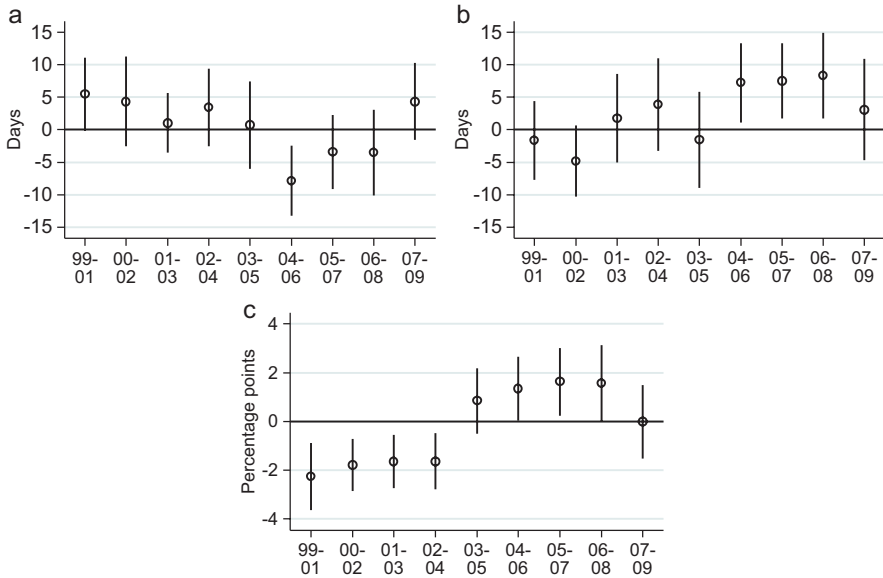


Fig. 15.2 The results of placebo tests: (a) Open unemployment, (b) Unsubsidized employment, (c) Activation ratio

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 8-year estimation window forwards from the year 1994 and leaving the last 3 years for the placebo treatment. These placebo tests are reported in Fig. 15.2, 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 based on clustered standard errors. We focus on three outcome variables for which we found significant reform effects above, so the results are comparable to panel B of Table 15.1, 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.

All the pre-reform placebo effects in panels a and b turn out to be statistically insignificant and oppositely signed to those reported in Table 15.1. Hence there seems to be no element in our empirical specification that systematically produces the reported YG effects on unemployment and employment.³ Note also that it is unsurprising to see that the placebo effects disappear when entering the last placebo period of 2007–2009. 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

³We also estimated the DiD regressions without covariates. These (unreported) results show that the estimates reported in Fig. 15.2 are not sensitive to controlling for background characteristics.

that there could be significant effects in the placebo years 2007–2009 would be if the YG becomes more effective over time. There is no evidence for that.

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. Starting from the period 2003–2005, 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 previously reported reform effect on the activation ratio. 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 15.3 reveals that the negative pre-reform placebo estimates result from a sharp increase in activation among the control group during the years 1998–1999. This coincides with the reform of Finnish labour market policy that was introduced in the beginning of 1998 and gradually implemented during the next 2 years. The main change that affected the activation ratio was the introduction of job search training consisting of a short course lasting a week during which individuals were taught how to seek vacancies, update their CVs, 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.

Unfortunately, the period 1999–2001, when the difference between the activation ratios of the treated and the controls was non-existent, coincides with the first 2 years in our DiD regressions. This would bias our preferred results if job search



Fig. 15.3 Activation ratios for unemployed young persons with vocational education, 1996–2010

training affected the labour market prospects of the participants. If job search training has any effects, the most likely scenario would be that it 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 upwards 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 to explore potential pre-reform differences, we did not cause any bias in our employment and unemployment estimates but slightly overestimated the activation impact of the YG. However, the activation effect still remains 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.

15.6 Conclusions

This paper provides new insight into the effects of activation guarantees offered to young people. 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 educational effect, income effect and mental health effects of the YG.

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 and days in open unemployment reduced and days in nonsubsidized employment increased by 7 days per year. 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 jobseekers, so the new activation periods only affected educated jobseekers.

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 youth populations following the guidelines of the European 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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