Evaluating the mandatory activation of older unemployed

Joost Bollens
HIVA-K.U.Leuven

5-2011

WSE-Report

Steunpunt Werk en Sociale Economie
E. Van Evenstraat 2 blok C – 3000 Leuven
T:32(0)16 32 32 39 F:32(0)16 32 32 40
steunpuntwse@econ.kuleuven.be
www.steunpuntwse.be

KATHOLIEKE UNIVERSITEIT LEUVEN

HIVA ONDERZOEKSINSTITUUT VOOR ARBEID EN SAMENLEVING K.U.LEUVEN

Evaluating the mandatory activation of older unemployed

Joost Bollens
HIVA-K.U.Leuven

Een onderzoek in opdracht van de Vlaamse minister van Financiën, Begroting, Werk, Ruimtelijke Ordening en Sport, in het kader van het VIONA-onderzoeksprogramma

Deze publicatie kwam tot stand met steun van het **Europees Sociaal Fonds**. Het ESF stelt middelen ter beschikking voor initiatieven die bijdragen tot meer en betere jobs voor meer mensen.

Ontdek de werking in Vlaanderen via **www.esf-agentschap.be**.

Kernthema's ESF 2007-2013

Talenten activeren

Arbeidskansen geven

Ondernemen met mensen

ESF

ESF investeert in jouw toekomst.

Bollens Joost

Evaluating the mandatory activation of older unemployed

Bollens Joost – Leuven: HIVA - Katholieke Universiteit Leuven. Steunpunt Werk en Sociale Economie. 2011, 18p.

ISBN-9789088730665

Copyright (2011)

Steunpunt Werk en Sociale Economie
Parkstraat 45 bus 5303 – B-3000 Leuven
T:32(0)16 32 32 39 - F:32(0)16 32 32 40
steunpuntwse@econ.kuleuven.be
www.steunpuntwse.be

HIVA – K.U. Leuven
Parkstraat 47
B-3000 Leuven

Niets uit deze uitgave mag worden verveelvoudigd en/of openbaar gemaakt door middel van druk, fotokopie, microfilm of op welke andere wijze ook, zonder voorafgaande schriftelijke toestemming van de uitgever.

No part of this report may be reproduced in any form, by mimeograph, film or any other means, without permission in writing from the publisher.

Table of Contents

List of Tables

List of Graphics

Abstract

In order to identify the treatment effect of an active labor market program for older unemployed in Belgium, this paper exploits an age-related and time-related obligation to participate in the program. The program consists of job search assistance, counseling and training. Using a difference-in-differences-matching estimator, we find that the program significantly raised transitions to employment by about 3 to 4 percentage points. As there are some indications that compliance with the obligation to participate is not perfect, the actual treatment effect on the treated possibly is substantially higher.

Keywords: Difference-in-differences; Propensity Score Matching; Older unemployed; ALMP;

Jel Classification: C21; J6; J18; J38;

1. Introduction

As was the case in many European countries, the labor market strategy towards the older unemployed in Belgium has known a marked evolution over the last decades. In the 1980s, in a context of massive and rising unemployment, long term unemployed over 55 were exempt from the obligation to search for a job and accept job offers. In 1996, the minimum age was even further reduced to 50 years, implying the virtual deactivation of the older unemployed. At the beginning of the new millennium, the labor market context had drastically changed with much lower levels of unemployment, and emerging labor shortages due to demographic developments. This evolution clearly was reflected by the fact that starting in 2002, the exemption was gradually phased out. As of July 2004, the full exemption only applies to the unemployed of 58 and older.

The participation of older unemployed in active labor market programs, which for obvious reasons was rather limited before these reforms, remained limited after the reforms, as activating policies remained almost exclusively geared towards the unemployed below the age of 50. A good example is e.g. the ambitious federal program to monitor closely and regularly the job search effort of all long term unemployed. When the job search effort is deemed unsatisfactory, unemployment benefit sanctions can be imposed. Participation in an active labor market program is regarded as a signal of search effort. This program was introduced in 2004 and gradually phased in, starting with the unemployed below 30 in 2004, adding the new long term unemployed between 30 and 40 the next year, and eventually adding the long term unemployed between 40 and 50 in 2006. The inclusion of the unemployed above 50 in the program was never even envisaged.

By the end of the decade, this position finally was reconsidered, as is demonstrated by the introduction in 2009 of the mandatory activation of older unemployed in the northern part of Belgium. This measure affects the unemployed between 50 and 52, but policy makers made it clear that they eventually want to extend the measure to all unemployed below the age of 58.

In this paper, we evaluate the impact of this measure on the employment prospects of the older unemployed involved. We contribute to the literature in two ways. Firstly, despite the rather voluminous and growing literature on the evaluation of active labor market policies (see e.g. (Card, Kluve, & Weber, 2010)), comparatively little is known about the effectiveness of active measures that are specifically targeted on the older unemployed. Secondly, this paper also relates to the relatively new and growing literature on the effectiveness of compulsion in active labor market programs (see (Graversen & van Ours, 2009), (Rosholm, 2008)).

2. The mandatory activation scheme

The mandatory activation scheme for the older unemployed was introduced in May 2009, and relates to all the unemployed at the moment when their unemployment duration becomes 3 months, and who at that point have an age lying in the interval (50 ≤ age < 53).

Before this reform, activation was only mandatory until the age of 49. Older unemployed were invited to an information session after three months in unemployment. In this information session they were told what the Public Employment Service (PES) could do for them. After this session the activation story ended for most, as only a minority of the older unemployed chose voluntarily to enter an activation program.

After the reform, the older unemployed are still invited to an information session after three months in unemployment, as was the case before the reform (this session can be given by the PES, or by the trade unions). But from now on, they are expected to choose either of the following activities (or a combination of them): accept job search assistance (including being referred to job vacancies), participate in a job club (where job search skills are taught and applied collectively), or participating in a vocational training. To monitor and counsel the older unemployed, there are specialized counselors who can take into account the specific position of older unemployed on the labor market.

It is explicitly foreseen that in case of non-compliance by the unemployed, the PES has the possibility to report this to the RVA (the service that is responsible for the unemployment benefits). This report can result in an unemployment benefit sanction.

3. The empirical methodology

In identifying the impact of the mandatory activation of older unemployed, we exploit the fact that the scheme was introduced at a given point in time for all new entrants in unemployment within the age group 50-52. This makes it possible to compare the outcomes of new entrants, aged 50-52, in the months after the introduction of the scheme, with the outcomes of new entrants from the same age group in the months before the introduction of the scheme. To control for other changes that might have taken place in the before-after time span, the outcome evolution of new entrants from the age group 53-55 is used. These persons were exempt from a mandatory activation, both before and after its introduction.

3.1 The outcome of interest

As the objective of the mandatory activation scheme unambiguously is to increase the number of older unemployed who find a job, we take the evolution of the number of employed persons in the treatment group as the outcome of interest. Obviously, activation is more than just raising the sheer number of employed, and it would be very interesting to assess how the employment impact of the treatment, if any, relates to the quality or duration of the jobs of activated persons. Such information was however not available.

3.2 Evaluation methodology

Suppose that for each individual i there are two potential outcomes, either Y_{it}^1 or Y_{it}^0. The outcome in period t is equal to Y_{it}^1 if individual i was treated (i.e. was exposed to the mandatory activation scheme). If individual i did not get the treatment in period t, her outcome will be Y_{it}^0. The treatment effect for individual i is equal to the difference of her potential outcomes : $\Delta_i = Y_{it}^1 - Y_{it}^0$ and the average policy impact for the treatment group will be $E\,(Y_{it}^1 - Y_{it}^0 \mid T=1)$, where $T = 1$ for individuals who were treated.

Obviously, if one observes Y_{it}^1, Y_{it}^0 cannot be observed, as nobody can at the same time be treated and be non treated. It is therefore called a counterfactual. Suppose however that a comparison group (T=0) of non treated individuals exists, for which holds that:

$$E\,(Y_{t=1}^0 - Y_{t=0}^0 \mid T=0) = E\,(Y_{t=1}^0 - Y_{t=0}^0 \mid T=1)$$

where t=0 is the period before the reform and t=1 is the period after the reform. This is known as the common trend assumption, and it states that while the no-treatment outcome Y^0 may differ between both groups, the evolution of Y^0 over time is supposed to be identical for both groups. Given the common trend assumption, the counterfactual can be easily replaced by observed quantities:

$$\begin{aligned} E\,(Y_{t=1}^1 - Y_{t=1}^0 \mid T=1) &= E\,(Y_{t=1}^1 - Y_{t=0}^0 \mid T=1) - E\,(Y_{t=1}^0 - Y_{t=0}^0 \mid T=1) \\ &= E\,(Y_{t=1}^1 - Y_{t=0}^0 \mid T=1) - E\,(Y_{t=1}^0 - Y_{t=0}^0 \mid T=0) \end{aligned}$$

which constitutes the difference-in-differences estimator. It then becomes important to find a comparison group for which it is likely that the common trend assumption holds. For the treatment group of new entrants aged 50-52, the comparison group of new entrants aged 53-55 seems a natural choice. They are not treated, neither in t=0 nor in t=1. They are moreover reasonably comparable and close to the treatment group so that it is likely that the growth rate of employment would be very similar in both groups in the absence of the reform.

There is no possibility to test whether the common trend assumption is justified. However, when there are more than two comparison groups, one can at least check whether it is plausible : if the trend in the outcome differs markedly between both comparison groups, one has reason to question the validity of the approach (see (Hastings, 2004) for an application). We have another comparison group, consisting of the new entrants aged 48-49. As the mandatory activation scheme was already in place for them in the period t=0, their treatment status does not change between period t=0 and t=1, and therefore one can argue that they are another plausible candidate to use as comparison group.

It is likely that there will be compositional differences between the treatment group at t=1 on the one hand, and the comparison groups (both at t=0 and t=1) on the other hand. And, since we are using repeated cross sections, it is also likely that there will be a compositional difference between the "treatment group before treatment" (the 50-52 of age who entered unemployment in the months before the reform) and the treatment group after the treatment (the 50-52 of age who entered unemployment in the months after the reform). To give an example, as the reform was introduced in a period of unfavorable business cycle conditions and rising unemployment, one could posit that the share of higher skilled entrants in unemployment in the 50-52 group will be lower in the before period than in the after period : as the higher skilled in general will have less precarious jobs, will have received more firm specific training, etc., their dismissal risk at the beginning of an economic downturn will be lower, but as the downturn continues, eventually will rise.

These compositional differences clearly will be another threat to the identification strategy. Define X_i to be a vector of observable characteristics that are related to the propensity to leave unemployment. Given the compositional differences, the common trend assumption has to be modified: *conditional on X*, the evolution of Y^0 is independent of the treatment status:

$$E\,(Y_{t=1}^0 - Y_{t=0}^0 \mid X, T=0) = E\,(Y_{t=1}^0 - Y_{t=0}^0 \mid X, T=1)$$

The treatment effect will now be given by :

$$E\,(Y_{t=1}^1 - Y_{t=0}^0 \mid X, T=1) - E\,(Y_{t=1}^0 - Y_{t=0}^0 \mid X, T=0)$$

This can be realized by recurring to matching techniques. Given the dimensionality problem, the use of propensity score matching (Rosenbaum & Rubin, 1983) is an appropriate choice. The repeated-cross-sectional nature of our data implies that this has to be done three times: the two non-treated groups (T= 0 at t=0 and at t=1) as well as the group of the treated before treatment (T=1, t=0) have to be matched to the treated after treatment (T=1, t=1).

This procedure gives rise to the difference-in-differences-matching estimator (Heckman, Ichimura, & Todd, 1997), (Smith & Todd, 2005).

$$\sum_{i\in T_{11}} \left\{ \left[\mathrm{Y}_{\mathrm{i1}} - \sum_{j\in T_{10}} \mathrm{w}_{\mathrm{ij}}^{\mathrm{T}_{10}} \mathrm{Y}_{\mathrm{i0}} \right] - \left[\sum_{j\in T_{01}} \mathrm{w}_{\mathrm{ij}}^{\mathrm{T}_{01}} \mathrm{Y}_{\mathrm{i1}} - \sum_{j\in T_{00}} \mathrm{w}_{\mathrm{ij}}^{\mathrm{T}_{00}} \mathrm{Y}_{\mathrm{i0}} \right] \right\} \frac{1}{\mathrm{n}_{\mathrm{T}_{11}}}$$

with T_{kt} the set of the treated (k=1) or untreated (k=0) in period t, and $w_{ij}^{T_{kt}}$ the weight given to individual j from set T_{kt} in the comparison with the treated individual i. The $\mathrm{n}_{\mathrm{T}_{11}}$ represents the number of individuals in T_{11}.

In order to identify the ATT, in addition a common support assumption has to be imposed (Blundell & Costa Dias, 2009): the treated group (T_{11}) can be entirely reproduced in the three comparison groups (T_{01}, T_{00} and T_{10}).

4. The data

We use data from the unemployment register as collected by the VDAB, the public employment service in Flanders. The dataset consists of individual records. Anyone who enters unemployment is recorded in the month of entrance. The dataset moreover has information on the labor market position of the individual at the end of all subsequent months (either unemployed or employed). The dataset also includes a number of other variables - age, educational attainment (low, middle, high), gender and an indicator for foreign origin.

The mandatory activation scheme for the older unemployed was introduced in May 2009, and concerns all the unemployed at the moment where their unemployment duration becomes 3 months, and who have at that point an age lying in the interval ($50 \leq$ age < 53).

We selected for this evaluation all the unemployed who entered unemployment between the beginning of March 2009 and the end of September 2009, who were still in unemployment three months after entering unemployment, and who at that time were in the age group [50, 53[.

As the mandatory activation scheme started somewhere in the course of May 2009, some, albeit not all, of the unemployment spells that started in February 2009 (and had a duration of at least 3 months) were affected by the policy reform. In order to avoid a contamination bias, no spells starting in February 2009 were included in the selection. As of January 2010 a wage subsidy was introduced that targeted specifically the unemployed persons of 50 and older. As this reform obviously also affects the target group of the mandatory activation scheme, we chose not to include any spells starting after the end of September 2009: for these spells it would be (increasingly) difficult to disentangle the effect of both reforms, which would possibly jeopardize our identification strategy [1].

The dataset covers the period August 1995 until March 2010. This implies that it is possible to control for the recent labor market history (before the current unemployment spell), at least if the person has been in unemployment recently. As suggested by (Heckman, Ichimura, & Todd, 1997), and (Blundell,

[1] Of course all unemployment spells of treated persons that started before October and that are still running in January 2010, can be affected by the wage subsidy. Here we assume that it takes some time before a new measure is running at full capacity. Also note that the introduction of the subsidy does not affect the validity of the age group 53-55 as comparison group, as they are also eligible for the subsidy.

Costa Dias, Costas, & Van Reenen, 2004), the recent labor market history can be a crucial component in an non-experimental evaluation, as it is possibly correlated with non-observed characteristics that are driving the employability of the person (assuming that this relation is stable over time). This is realized through a variable measuring the share of months in unemployment within the 36 months that precede the current unemployment spell.

Given that the target group consists of persons aged between 50 and 52, it also makes sense to have a longer term view. Therefore we also created a variable measuring the share of months in unemployment between August 1995 and the beginning of the current unemployment spell.

As already has been set, the sample for the treatment group consists of all the persons who (1) entered unemployment between the beginning of March 2009 and the end of September 2009, who (2) were still in unemployment three months after entering unemployment, and who (3) at that time were in the age group [50, 53[.

The 5 comparison groups were selected along the same lines. The sample for the "treatment group before treatment" consists of all the persons who (1) entered unemployment between the beginning of March 2008 and the end of September 2008, who (2) were still in unemployment three months after entering unemployment, and who (3) at that time were in the age group [50, 53[.

The selection procedure for the other comparison groups was identical, except for the fact that the age requirements were changed to (53 ≤ age < 55) and (48 ≤ age < 50). Table 1 gives some more details about the six samples. The sample averages for the different characteristics are rather comparable, apart from some slight shifts that are consistent with the age differences between the samples.

The dataset covers the period until March 2010, meaning that the labor market status of persons in the sample who became subject to the mandatory activation in September 2009 (or earlier), can be followed during 7 months (September 2009 until March 2010). In a first draft of this paper, for the ones that became subject to the mandatory activation after September 2009, less than 7 months were available, which e.g. made the matching process rather complicated. Therefore additional information was added in order to ensure that all persons who entered unemployment in 2009 can be followed during 7 months after becoming subject to the mandatory activation.

Table 1 Characteristics of the six groups, before matching

	50-52		48-50		53-55	
	2009	2008	2009	2008	2009	2008
number of individuals	3908	3167	3227	2496	2202	1792
educational attainment						
low	2039	1754	1614	1367	1191	1001
	(52.18)	(55.38)	(50.02)	(54.77)	(54.09)	(55.86)
middle	1166	949	966	707	641	535
	(29.84)	(29.97)	(29.93)	(28.33)	(29.11)	(29.85)
high	703	464	647	422	370	256
	(17.99)	(14.65)	(20.05)	(16.91)	(16.80)	(14.29)
gender = female	1920	1614	1569	1380	1018	909
	(49.13)	(50.96)	(48.62)	(55.29)	(46.23)	(50.73)
foreign origin	405	344	486	383	196	158
	(10.36)	(10.86)	(15.06)	(15.34)	(8.90)	(8.82)

5. The PSM-method

The propensity scores were estimated with a probit model. As independent variables we included the available variables that are believed to be good predictors for the propensity to leave unemployment towards employment : gender, the foreign origin indicator, educational attainment (low, middle, high), the month of entering unemployment (i.e. a number between 1 and 12, where the reasoning is that someone who enters unemployment in e.g. the summer, will face other labor market prospects than someone who enters in the winter), and the two labor market history indicators (last 36 months and previous 15 years). As it was intended to remove the influence of compositional differences between the treatment group on the one hand, and the different comparison groups on the other hand, the procedure was repeated 5 times, as 5 comparison groups are defined : the 50-52 of age in the period before, the 53-55 of age before and after, and the 48-50 of age, before and after.

It is important that the estimated propensity score satisfies the balancing property, meaning that observations with the same propensity score must have the same distribution of observable characteristics, independent of treatment status. This property was tested with the Pscore-module (Becker & Ichino, 2002). According to this testing, the balancing property was satisfied in all cases.

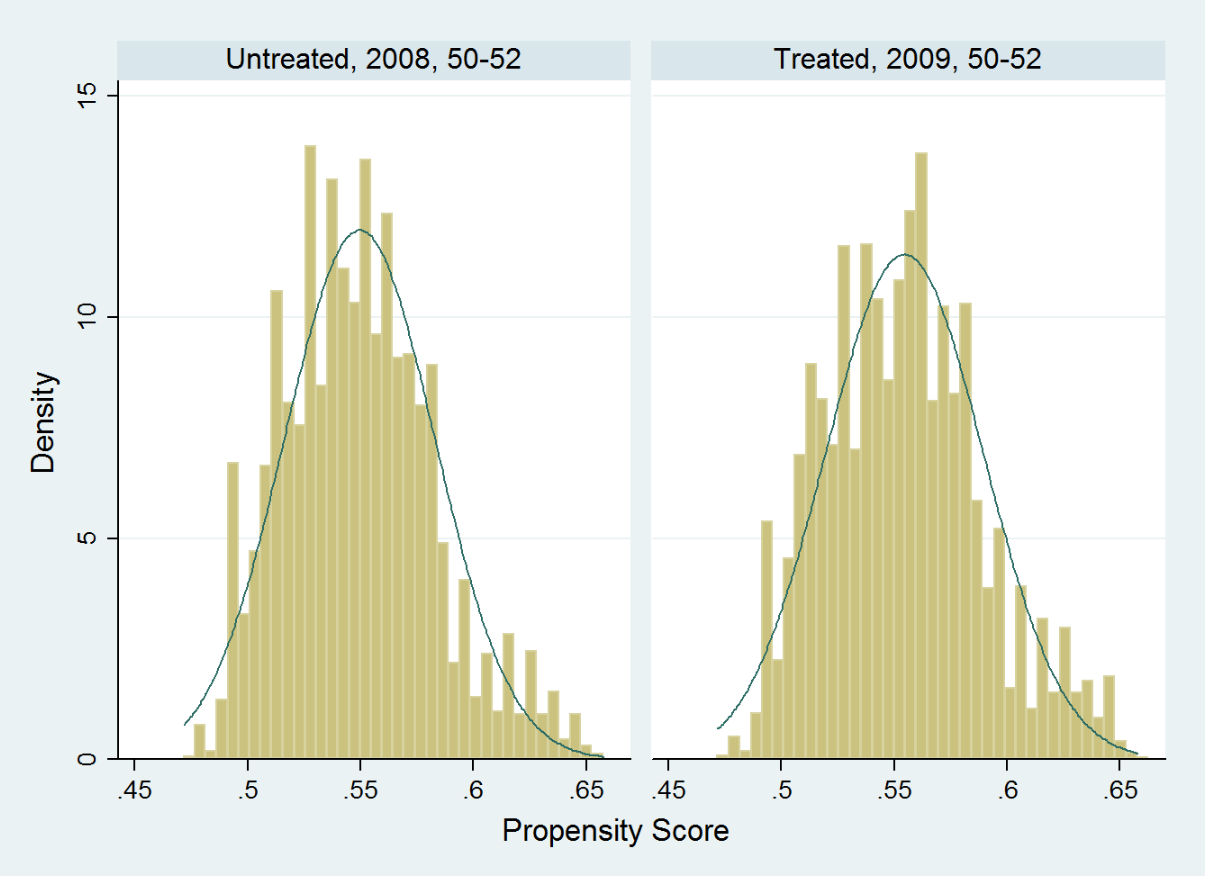
Given the mandatory nature of the treatment, the treatment group is not self selected, and therefore one would not expect great differences in the first place. Graph 1, which compares the histograms of the estimated propensity scores of the treatment group and the group of 50-52 of age, before the reform, shows that they are comparable even before any matching took place. The same holds for the four other cases.

For the Propensity Score Matching, use was made of the Psmatch2 module of (Leuven & Sianesi, 2010). The estimated propensity scores were matched with an epanechnikov kernel:

$$w_{ij} = \frac{3}{4}\,(1 - z^2).\mathbf{1}(|\mathrm{z}| < 1) \quad \text{with} \quad \mathrm{z} = \frac{(\mathrm{p_i} - \mathrm{p_j})}{\mathrm{h}}$$

This is a non-negative, symmetric and unimodal weighting function. The weight w_{ij} given to a non-treated person j is proportionate to the closeness (of the observables, viz. of the propensity scores p_j

and p_i) of person j and the treated person i. Only observations within a given window ($p_i \pm h$) around p_i are taken into account, where h is called the bandwidth, and the window width is equal to 2h.



Graph 1. Histograms of propensity scores, before matching

6. Difference-in-difference matching results

The treated individuals are observed during 7 months after the treatment starts, with s = 4,...,10. For each of these seven months, the dependent variable (either working or not in month s) is regressed on a constant and on three dummy variables, to wit one that indicates whether one belongs to the age group 50-52 or not, one that indicates whether one entered unemployment in 2009 or not, and one indicating whether one belongs to the treatment group or not. The group of the 50-52 of age in 2009 and 2008 are always included, as they are necessary to estimate the first difference. To form the second difference, alternatively the group of the 53-55 of age in 2008 and 2009, or the group of the 48-50 of age in 2008 and 2009 are included. All observations are weighted with the weights created in the matching process. This gives rise to this simple regression model:

$Working_s = \alpha_s + \beta_s.(\text{age group 50-52}) + \gamma_s.(\text{entrance 2009}) + \tau_s.(\text{treatment})$

The interpretation is straightforward. In month s, the percentage of working persons in the group of people aged 50-52, who entered unemployment in 2009, will be equal to $(\alpha+ \beta + \gamma + \tau)*100$, whereas $(\alpha+ \beta)*100$ percent of people aged 50-52 who entered unemployment in 2008 will be working. If the second difference was made with the group of age 53-55, $\alpha*100$ will be the percentage of them that are working in month s, if they entered unemployment in 2008. The treatment effect is given by τ.

The use of an estimated propensity score (instead of the true propensity score) in the matching process typically will introduce some additional variance that has to be taken into account. In order to do this, we used bootstrapped standard errors[2]. Table 2 gives the estimation results for the treatment effect.

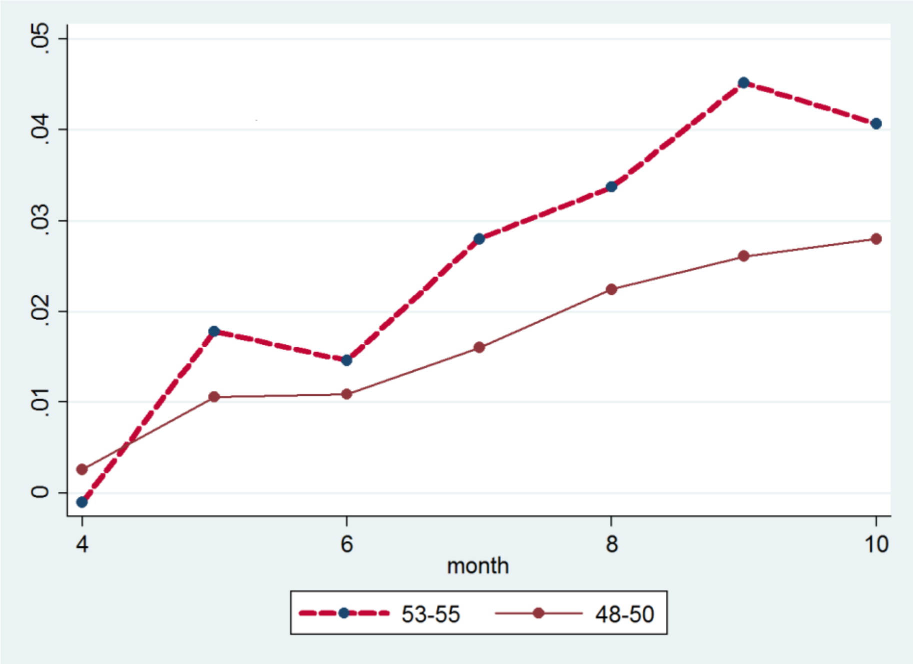
Table 2 Difference-in-differences-matching results (treated individuals : n = 3908)

	Month 4	Month 5	Month 6	Month 7	Month 8	Month 9	Month 10
	Second difference: Group 53-55						
Treatment effect	-0.001	0.018	0.015	0.028	0.034	0.045	0.041
Bootstrapped Standard Error*	0.01	0.012	0.013	0.014	0.014	0.015	0.016
T-value	-0.14	1.47	1.09	1.96	2.30	2.97	2.54
	Second difference: Group 48-50						
Treatment effect	0.002	0.010	0.011	0.016	0.022	0.026	0.028
Bootstrapped Standard Error*	0.010	0.012	0.013	0.014	0.015	0.015	0.016
T-value	0.25	0.86	0.80	1.10	1.48	1.68	1.82

* with 400 replications

For the results in the top half of table 2, the age group of 53-55 was used to correct for trend differences between 2009 and 2008. These results suggest that the mandatory activation of older unemployed does have an impact on the probability that these unemployed return to employment, although the impact comes with a time lag (the treatment effect in month 4, the first month of exposure to the treatment, is basically zero), and it takes some additional months before the estimated treatment effect is big enough to be statistically significant. For e.g. month 9, the result suggest that due to the mandatory activation, there is an employment gain of 4,5 percentage points. As in month 9 in fact 24,6% of the treatment group was working, this suggests that in the absence of the reform only 20,1% would have been employed.

[2] Whereas (Abadie & Imbens, 2008) have shown that the use of the bootstrap is not correct in case of nearest neighbour matching with continuous variables, basically due to the extreme non-smoothness of nearest neighbor matching, it can be conjectured that there will be no problem in case of kernel matching.

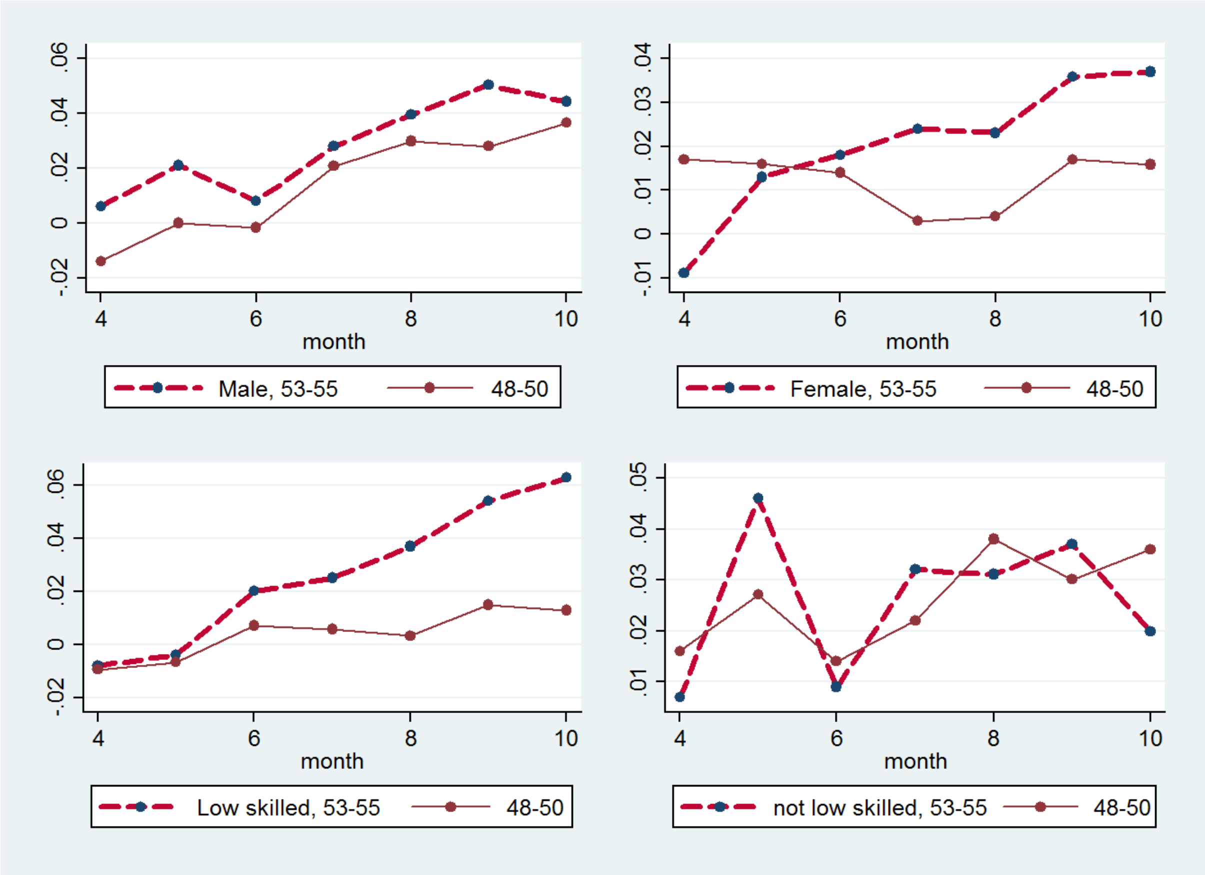


Graph 2. Estimates of the treatment effect

The results in the bottom half of table 2 relate to the use of the age group 48-50 to correct for trend differences between 2009 and 2008. These results are obviously different from the ones in the top half of the table, casting some doubt on the appropriateness of the common trend hypothesis for these data. The graphical representation of these two sets of estimates in graph 2 allows a better comparison. The graph indicates that, although both sets do differ, they clearly show an identical upward trend. In that sense the common trend hypothesis is not refuted by the data. As to the magnitude (and statistical significance) of the treatment effects, of course it does matter whether one takes the age group 50-53 or the age group 48-50 as a control for trend differences between 2009 and 2008.

For these results, it was assumed that the treatment effect for a given month s is constant, i.e. a homogeneous effect that is the same for everybody concerned. One can however check whether there are heterogeneous effects by applying the difference-in-difference-matching approach to specific subsamples. We applied it for 4 subsamples : for men, for women, for the group of low skilled and for the group of those that are not low skilled (i.e. a combination of middle and high skilled unemployed). In order to prevent potential support problems, it is advisable to restrict the analysis to subsamples that are sufficiently large. As can be seen in table 1, each of these subsamples represents roughly 50% of the original sample. The estimation results can be found in table A.1 and are graphically shown in graph 3.

For the subsample of male older unemployed, the results obtained with the age group 48-50 and those obtained with the age group 53-55 evolve rather concurrently, although here, as before, the latter lie consistently above the former. For the other three subsamples, the evolution of the two sets of estimates is less clear cut, although one could argue that for the subsample of the not-low skilled, the two lines are reasonably close, apart from the observation in month 5. If, for the time being, we restrict the discussion to the estimates obtained with the age group 53-55, one can conclude that the results for males, females and low skilled do not deviate substantially from the general picture shown in graph 2.



Graph 3. Estimates of the treatment effect for specific subsamples

7. Discussion

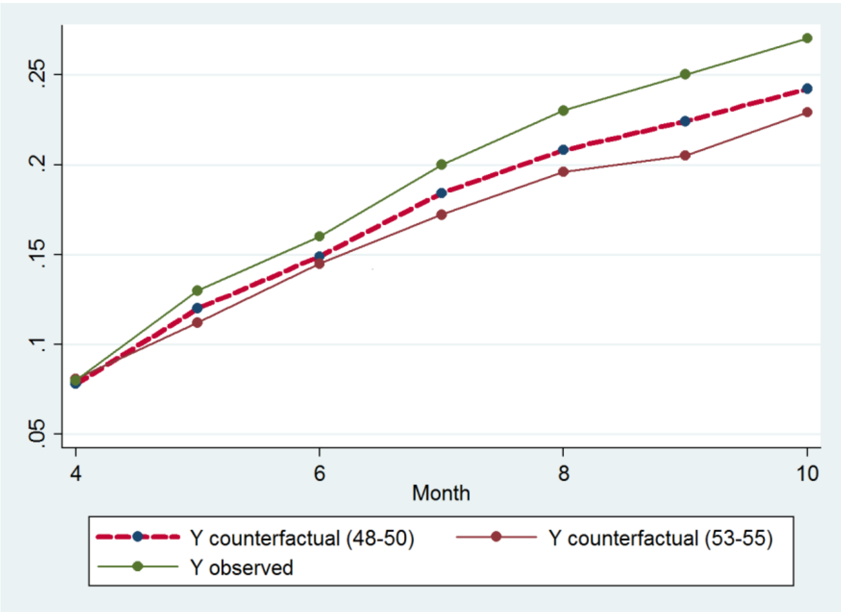
The results obtained with the age group 48-50 as correction for the time trend differ from the ones obtained with the age group 53-55. This is not to say that the two sets of results are conflicting, both suggest that the introduction of the mandatory activation indeed had some effect, and moreover both sets suggest that this effect gradually increases in the months after the start of exposure to mandatory activation. There is however a difference in magnitude. Whereas the former are generally smaller and consistently too small to be statistically distinguishable from zero, the latter are in general higher, and, especially in the months 7 until 10 of the unemployment spell, statistically significant.

Therefore the key question is which of both sets is more reliable, if any. We argue that there are two reasons for suggesting that the comparison group of age 53-55 is to be preferred in the given setting.

First, as already has been said, in fact for the unemployed below the age of 50, a mandatory activation scheme was already in place in 2008. As the situation remains the same for unemployed below the age of 50 who entered unemployment in 2009, we argued that the treatment status does not change between 2008 and 2009, and therefore that the entrants below the age of 50 were a possible candidate to correct for the time trend 2009-2008. If, in one way or another, the mandatory activation had an influence on the time trend 2009-2008 of the group 48-50, which is rather implausible, but not inconceivable, the choice of this group to correct for the trend is no longer valid. This very mechanism cannot invalidate the choice of the age group 53-55 as correction for the time trend, as they neither in 2008 nor in 2009 underwent a mandatory activation.

A second, more substantial reason why the case for the age group 53-55 as comparison group in our opinion is more compelling, is the fact that the age of 50 is an important socio-psychological barrier in the Belgian labor market, and, everything else the same, goes together with a reduction in one's employability. This does not necessarily imply that the age group 48-50 is not a valid choice, as in the end we are only interested in their evolution over the period 2009-2008, but here again the age group 53-55 seems to be closer to the treatment group and therefore more suited as comparison group.

Another question is whether the results are substantial. Ultimately, this question can only be tackled meaningfully in a cost-effectiveness or a cost-benefit analysis, as it is necessary to relate the impact of an instrument to the cost of its implementation. Information about the cost of the mandatory activation of the older unemployed, however, is not available. As the mandatory activation of the older unemployed predominantly is directed towards activities that stimulate job search and give counseling, and less towards vocational training, one conjecture is that this instrument will be one of the less expensive active measures, relatively speaking, as vocational training is known to be expensive. Interviews done with PES-counselors on the other hand, suggest that counseling the older unemployed is more time intensive and therefore requires more resources (WSE(2010)). With those caveats in mind, it is meaningful to compare the actual, observed shares of employed persons in the treatment group after treatment, with the estimated counterfactual shares of employment, as this relates the treatment effect to the actual situation.



Graph 4. Observed and counterfactual shares of employed persons in the treatment group

Graph 4 gives an overview. The graph indicates that, although a treatment effect of say 4 percentage points (in the later months) might not be considered as being important per se, it is non negligible if related to the rather small propensity to leave unemployment for a job in the group of the older unemployed.

The difference-in-differences-matching approach, given the common support condition, will identify the average treatment on the treated (see section 3). Here, the assumption was made that the mandatory nature of the measure implies that all persons in the treatment group will receive a treatment. This, however, does not seem to be the case. In a report of the department of Work (WSE 2010), it was mentioned that for a given group of unemployed in the target age group of 50-52, 6 months after they became eligible for the mandatory activation, barely 47% indeed had received a treatment. This points to a substantial rate of non-compliance. Although these figures relate to 2010, there is no reason to assume that the situation would have been particularly better in 2009. This has important implications for our estimation results. For one, the treatment effects mentioned earlier will underestimate the true ATT. If we take a compliance rate of nearly 50% as an example, the effective treatment effects could potentially be twice the size of the ones mentioned earlier. Another implication is that now also the (unknown) average treatment effect on the untreated (ATU) becomes important. Assume that the choice between complying and non-complying is selective, e.g. because persons who believe that they will benefit from participation have more reason to comply than persons who don't believe they will benefit, and assume that these beliefs on average are correct. In that not too unrealistic case the ATU will be smaller than the ATT, implying that the external validity of our results is lower than what would have been the case with a mandatory measure under full compliance.

A further complication arises if one considers the possibility that the introduction of a mandatory activation program can bring along threat effects (Rosholm, 2008) : some unemployed dislike participating in an active labor market program and will therefore react to an increase in the participation risk by increasing their job search or by lowering their reservation wage, resulting in a higher job finding rate. This threat effect can be considered to be an element of the treatment, as in the absence of the treatment there would be no threat effect. As persons that react this way typically will be non-compliers (in the officially measured sense, i.e. persons who did not get a formal treatment), one can conclude that the officially measured rate of non compliance underestimates the effective compliance with the measure. Or in other words, the more important the threat effect, the smaller the underestimation of the true ATT mentioned above.

8. Conclusion

The mandatory activation scheme for the older unemployed was introduced in May 2009, and is targeted at the 50-52-year-old, new entrants in unemployment, who remain in unemployment for at least three months. The program offers varying combinations of job search training & job search assistance, counseling and job training.

To identify the impact of the program, we exploit the age-related and time-related obligation to participate in the program. The introduction from the scheme at a given point in time for all new entrants in unemployment within the age group 50-52, makes it possible to compare the outcomes of new entrants, aged 50-52, in the period after the introduction of the scheme, with the outcomes of new entrants from the same age group in the period before the introduction of the scheme. To control for other changes that might have taken place in the before-after time span, the outcome evolution of new entrants from the age group 53-55 was used. These persons were exempt from a mandatory activation, both before and after its introduction. This gives rise to a difference-in-differences-design, which is further elaborated with a matching approach.

The estimation results suggest that the mandatory activation of older unemployed does have an impact on the probability that these unemployed return to employment, although the impact comes with a time lag. There is an employment gain of 3 to 4,5 percentage points, four to seven months after one became eligible for the treatment.

The outcome evolution over the before-after time span of new entrants from the age group 48-50 was used to obtain alternative diff-in-diff-matching estimates. The estimates are in line with the ones mentioned above, but generally smaller, and therefore not statistically significant. We argue that the results with the 53-55-group are the preferred results.

As there are some indications that compliance with the obligation to participate is far from perfect, the actual treatment effect on the treated possibly is substantially higher. However, this is attenuated by a possible threat effect.

There are some areas of further work. In the first place there are the longer-term effects of the measure. We followed the older unemployed during 7 months after their exposure to the treatment and found, especially towards the end of this period, evidence for positive treatment effects. The question remains as to how long-lived these effects will be, i.e. what will happen in the months that follow. As more data will become available for all individuals, including the ones that entered unemployment at a later date, this can be analyzed further. Secondly, and related to the previous issue, there is the question whether the treatment effects are independent of labor market conditions and the business cycle at the moment when someone becomes eligible for the treatment. Thirdly, the influence of non-compliance and the possibility of a threat effect need to be further elaborated. And finally, as we believe that in the public choice between two or more courses of action, cost effectiveness ultimately is more important than the net-effectiveness as such, it is important to perform a more in depth analysis of the costs of the measure.

Reference List

Abadie, A. & Imbens, G. W. (2008). On the failure of the bootstrap for matching estimators. ECONOMETRICA, 76, 1537-1557.

Becker, S. O. & Ichino, A. (2002). Estimation of average treatment effects based on propensity scores. STATA JOURNAL, 2, 358-377.

Blundell, R. & Costa Dias, M. (2009). Alternative Approaches to Evaluation in Empirical Microeconomics. Journal of Human Resources, 44, 565-640.

Blundell, R., Costa Dias, M., Costas, M., & Van Reenen, J. (2004). Evaluating the employment impact of a mandatory job search program. Journal of the European Economic Assoiation, 2, 569-606.

Card, D., Kluve, J., & Weber, A. (2010). Active Labor Market Policy Evaluations: A Meta-Analysis. The Economic Journal, 120, F452-F477.

Graversen, B. K. & van Ours, J. C. (2009). How a Mandatory Activation Program Reduces Unemployment Durations: The Effects of Distance. IZA Discussion Paper No.4079.

Hastings, J. (2004). Vertical Relationships and Competition in Retail Gasoline Markets: EmpiricalEvidence from Contract Changes in Southern California. AMERICAN ECONOMIC REVIEW, 94, 317-328.

Heckman, J., Ichimura, H., & Todd, P. (1997). Matching as an Econometric Evaluation Estimator: Evidence from evaluating a Job Training Programme. Review of Economic Studies, 64, 605-654.

Leuven, E. & Sianesi, B. (2010). PSMATCH2: Stata module to perform full Mahalanobis and propensity score matching, common support graphing, and covariate imbalance testing. N..

Rosenbaum, P. & Rubin, D. (1983). The Central Role of the Propensity Score in Observational Studies for Causal Effects. Biometrika, 70, 41-50.

Rosholm, M. (2008). Experimental Evidence on the Nature of the Danish Employment Miracle. IZA Discussion Paper No.3620.

Smith, J. A. & Todd, P. E. (2005). Does Matching Overcome LaLonde's Critique of Nonexperimental Estimators? Journal of Econometrics, 125, 305.

WSE (2010). Evaluatie 50+: Synthese, versie 3 juni 2010

Table A.1 Difference-in-differences-matching results for specific subsamples

	Month 4	Month 5	Month 6	Month 7	Month 8	Month 9	Month 10
	Men						
	Second difference : Group 53-55						
Treatment effect	0.006	0.021	0.008	0.028	0.039	**0.050**	**0.044**
Bootstrapped Standard Error*	0.013	0.017	0.019	0.020	0.021	0.021	0.021
	Second difference : Group 48-50						
Treatment effect	-0.014	-0.0001	-0.002	0.020	0.029	0.028	0.036
Bootstrapped Standard Error*	0.015	0.019	0.020	0.022	0.023	0.023	0.024
	Women						
	Second difference : Group 53-55						
Treatment effect	-0.008	0.013	0.018	0.024	0.023	0.035	0.037
Bootstrapped Standard Error*	0.013	0.017	0.019	0.021	0.022	0.023	0.023
	Second difference : Group 48-50						
Treatment effect	0.017	0.016	0.013	0.003	0.004	0.017	0.015
Bootstrapped Standard Error*	0.014	0.017	0.019	0.019	0.020	0.021	0.023
	Low skilled						
	Second difference : Group 53-55						
Treatment effect	-0.008	-0.004	0.020	0.025	0.037	**0.054**	**0.063**
Bootstrapped Standard Error*	0.012	0.014	0.016	0.018	0.019	0.019	0.020
	Second difference : Group 48-50						
Treatment effect	-0.009	-0.007	0.007	0.005	0.003	0.014	0.012
Bootstrapped Standard Error*	0.014	0.017	0.019	0.020	0.020	0.020	0.021
	Not-low skilled						
	Second difference : Group 53-55						
Treatment effect	0.007	0.046	0.009	0.325	0.031	0.037	0.019
Bootstrapped Standard Error*	0.015	0.019	0.020	0.022	0.024	0.025	0.026
	Second difference : Group 48-50						
Treatment effect	0.016	0.027	0.014	0.022	0.038	0.030	0.036
Bootstrapped Standard Error*	0.015	0.018	0.021	0.022	0.023	0.023	0.023

* with 400 replications