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The causal effects of employment on mental health and criminality for disabled workers

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Abstract

We study to what extent employment generates spillover effects on other life domains for persons with a work disability. Our empirical strategy is built around a unique exogenous shock in job probability caused by the 2015 reform of the activation policy for disabled workers in the Netherlands. We find that being employed reduces the probability of using mental health care by 7 percentage points, engaging in criminal activity by 3 percentage points and using non-medical home care by 8 percentage points. Relative to the baseline prevalence in our sample of disabled persons, these effects range between 30 and 60 percent.

Keywords: work disability, activation policy, mental health, criminality

JEL Classification: J68, H75, I18

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1 Introduction

To what extent does employment have spillover effects on other life domains, like mental health and criminal behavior? This paper studies this question for persons with a work disability. Since long it is known that disability has strong negative effects on labor market participation (see e.g. Stern, 1989). This is mainly due to disabled workers not being able to earn the minimum wage by themselves, while there is also some evidence that employers discriminate against persons with a disability (Baldwin and Johnson, 1994 and DeLeire, 2001). The low participation rate for the disabled raises concerns about their social exclusion and low standards of living, with potentially adverse effects on (mental) health status and prosocial behavior. In many advanced economies, improving the job opportunities for disabled persons is therefore high on the political agenda – as part of a broader policy to promote inclusive labor markets – and vast investments are made to achieve this goal (ILO/OECD, 2018).

From a policy perspective, it is important to know the full benefits and costs of such investments. Most evaluations of active labor market policies for the disabled merely focus on the direct monetary benefits for either the supported worker (e.g. higher after-tax income) or the taxpayer (e.g. reduced claim on welfare and unemployment benefits).¹ However, in case these policies succeed in increasing labor participation of the disabled they may in addition generate beneficial effects on other important life domains like health and social behavior. This would not only benefit disabled workers in the form of a higher quality of life and lower out-of-pocket payments on health care, but also society as a whole in the form of lower public expenditures on health care and crime. To what extent these spillover effects materialize in practice is not well known yet. Our paper contributes to a better understanding of the full benefits of activation policies targeted at disabled people.²

In order to assess the potential spillover effects of employment for disabled people, we exploit a unique exogenous shock in job probability for this group of workers. To be more specific, our identification strategy relies on the random variation in job probability caused by the 2015 reform of the activation policy for disabled workers in the Netherlands. Until 2015, persons with a work disability – either physical, cognitive or mental – were eligible for jobs in sheltered workshops owned by municipal enterprises. There were typically more people eligible than there were actual jobs available. As a result, there existed a waiting list for such jobs that operated on a first-in-first-out basis. As of the 1st of January 2015, those who were still sitting on the waiting list saw their rights to get employment at a sheltered workshop withdrawn. These people instead fell under the new regime in which municipalities are responsible for helping their disabled citizens obtain employment, preferably in the form of (subsidized) regular jobs.

Hence, for persons who were sitting on the waiting list at the end of 2013 there was still one year left to enter sheltered employment under the old regime. By contrast, those who were on the waiting list at the end of 2014 lost their rights to a job in a sheltered workshop. A comparison of these two cohorts on a large set of observables reveals that they are very similar to each other. However, they do differ significantly in their employment probability in the years right after the policy reform. The probability that a person belonging to the '13-cohort held a job one year after he or she entered the waiting list is about twice as large as for someone of the '14-cohort (50 versus 25 percent). By contrast, the job probability of the '13-cohort is very similar to the

¹ See e.g. Cimerá, 2012.

² As is stated by Crépon and van den Berg (2016): “[Cost-benefit analysis]...are needed to learn more about gains in terms of benefits...as well as the costs of the program. In order to carry out such analysis, it is necessary to have access to a larger set of outcome variables – more than a person’s employment status, which remains most frequently the only variable considered.”

job probabilities of the cohorts that entered the waiting list before 2013. This ensures us that there was no sorting of disabled workers into sheltered jobs in the period between the announcement of the policy reform and the actual reform. Altogether, this makes the year in which a person entered the waiting list a strong and valid instrumental variable (IV) for employment status.

Our IV estimates indicate that workers with a disability perform substantially better on various domains of life if they hold a paid job. To be more specific, we find that being employed reduces the probability of (a) using mental health care by 7%-points, (b) engaging in criminal activity by 3 %-points and (c) using non-medical home care by 8%-points. Given that in the population under study about 20 percent of all people use mental health care, 5 percent engage in criminal activity and 25 percent uses non-medical health care, the estimated effects are substantial. Our findings thus imply that by not taking into account these positive spillover effects, most of the existing cost-benefit analyses of activation policies for the disabled paint a too pessimistic picture of their net benefit to society. Heterogeneity analyses show that the effects are larger for men than for women, and larger for persons with a mental disorder than for people with other disabilities. Concerning criminality, it turns out that the full-sample effect is almost fully driven by men.

By identifying the health and crime effects for those whose treatment status depends on whether they entered the waiting list in 2013 or in 2014, we estimate a local average treatment effect (LATE; see Imbens and Angrist, 1994). As long as subjects cannot influence whether they fall under the old or under the new regime, our design suffers much less from endogeneity issues than studies that rely on firm closure or mass layoffs as the main source of variation in job status (see next section). In case workers anticipate future firm closures or mass layoffs and, as a result, sort themselves into high and low quality firms, these events cannot be considered truly exogenous to health and crime outcomes (see e.g. Albagli et al., 2020).

In principle, there are various life domains one could look at to assess the spillover effects of employment. There are various reasons why we focus on mental health and criminality in this paper. First of all, these life domains are typically determinant for a person's life satisfaction. Arguably, someone who is in good mental health and is not involved in criminal behavior has, *ceteris paribus*, a better quality of life than a sick person or someone who engages in criminal conduct. Secondly, any positive spillover effects from work on these life domains are also of great importance for the rest of society, given the vast public expenditures on both mental health care and crime control. And finally, we consider a group of workers that perform particularly poorly on these life domains a consequence of the common disabilities within this population being related to mental health problems, behavioral disorders and the like. Hence, the potential gains – both from a personal and societal perspective – from finding employment for this group of people are substantial.

The rest of the paper is structured as follows. In the next section, we give an overview of the existing literature on the spillover effects of activation policies and (un)employment in general on other life domains. Section 3 describes the institutional setting, while Section 4 discusses the data. Section 5 presents our empirical strategy, after which we discuss our empirical findings in Section 6. The paper ends with some concluding remarks.

2 Related literature

From a theoretical perspective, it is not evident how employment impacts one's (mental) health condition and inclination to criminal behavior. On the one hand, a paid job could potentially exert beneficial effects on these life domains through an increased self-confidence or through an enhanced well-being due to the adherence to the social norm (see e.g. Clark, 2003). It may also lower the incentive to commit property crimes, as the financial consequences of being caught are higher for someone who holds a paid job than for an unemployed person (see e.g. Becker, 1968). On the other hand, work could be a source of stress and in some

jobs there is a relatively high risk of injury.³ Furthermore, in case alcohol, tobacco and recreational drugs are normal goods a paid job may lead to an unhealthier consumption behavior through an income effect.⁴ Thus, to what extent employment affects these other domains of life is ultimately an empirical question.

Our paper fits into the relatively small empirical literature on the spillover effects of activation policies on life domains other than the work-life. Burns et al. (2007) perform a randomized trial in six European centers to study the effects of the individual placement and support (IPS) program – a type of place-and-train intervention – for people with severe mental health disorders. They find that IPS leads to better employment opportunities, and also lowers hospital admissions by about 11 percent relative to vocational training (the control service). Another experimental evaluation of an employment program is the National Job Corps Study by Schochet et al. (2008). Job Corps is the largest U.S. education and job training program for disadvantaged youth between the ages of 16 and 24. Their study shows that Job Corps, besides increasing educational attainment and (short-term) earnings, reduces criminal activity.

Another related strand of literature deals with the health effects of job loss using a quasi-experimental design rather than a fully randomized set-up. As mentioned above, in this literature firm closure or mass layoffs are seen as a source of exogenous variation that can be exploited to estimate the causal impact of involuntary job loss on one's health status. The results in this literature are contradictory. There is some evidence of an adverse effect of job loss on mortality (Sullivan and von Wachter, 2009; Eliason and Storrie, 2009a; Browning and Heinesen, 2012), on physical health (Eliason and Storrie, 2009b), and on mental health (Kuhn et al.; 2009). In general, the effects turn out to be stronger for men than for women. However, other studies, exploiting the same source of exogeneity, do not find any effect of involuntary employment on health-related outcomes (Browning et al., 2006; Salm, 2009; Schmitz, 2011).

Khanna et al. (2002) use a similar quasi-experimental design to study the effects of job loss due to mass-layoffs on criminality for the case of Colombia. Convincingly showing that their results do not suffer from selection bias, they find a significant spike in arrests in the year of job loss and one year later. This effect is fully driven by men and is stronger in case of weak job replacement opportunities and limited access to consumption credit. Other papers examining the impact of (un)employment on criminal activity adopt a more macroeconomic approach. Raphael and Winter (2001), Gould et al. (2002), and Lin (2008) find that labor market conditions in the U.S. have a significant impact on the number of financial crimes committed. The estimated decline in the crime rate due to a 1% fall in unemployment ranges between 2% and 5% in these studies. Fluctuations in labor market conditions do not seem to impact non-financial criminal activity.

Our paper differs from the studies exploiting mass-layoffs in two important aspects, next to the already discussed difference in the degree of exogeneity of the shock considered. First, since becoming unemployed due to firm closure is not exclusive to a specific subgroup of the labor force, studies on job loss typically consider the effects for the average worker. We instead focus on people with a disability, who are more often unemployed, in poor (mental) health and engaged in criminality than the average worker. Second, whereas the papers on firm closure examine the health and crime effects of a switch from employment into unemployment, we consider the impact of a switch in the opposite way. As long as job status renders asymmetric effects on health and crime outcomes, our results are more policy-relevant for assessing the effectiveness of activation policies that target the (long-term) unemployed.

³ See Fenwick and Tausig (1994) for a study on the relation between employment and (work-related) stress. Viscusi (1993) provides an overview of the literature on work-related risks.

⁴ Ruhm and Black (2002) show for the U.S. that the consumption of alcohol is procyclical.

A paper that does consider the spillover effects of a transition from unemployment to employment for a group that is comparable with our population under study, is by Huber et al. (2011). Using data on welfare recipients in Germany, they find that entering employment has a beneficial effect on (self-reported) mental health, particularly in the case of men and individuals with an adverse initial health status. Rather than employing a quasi-experimental setup, they apply a matching procedure and control for heterogeneity in health and other conditions at the initial stage in order to eliminate estimation bias due to reversed causality.

3 Institutional background

Sheltered employment for disabled people in the Netherlands was institutionalized in 1969 by the introduction of the Social Service Employment Act (SSEA). The aim of this law was “to create an unambiguous framework for offering work under adapted conditions to persons who are capable of working regularly but are not able to work under normal circumstances”.⁵ Psychological problems and cognitive disabilities – or a combination of both – were the most common forms of disability among people covered under this law, but also persons with a physical disability were eligible for sheltered jobs. Eligibility was assessed by an autonomous national authority. By 2014 roughly 100,000 people held a SSEA-job, which accounts for about 1 percent of the total labor force in the Netherlands.

By far most of the SSEA-workers were employed in sheltered workshops, ran by municipal enterprises. Jobs typically involved the assembly of semi-finished products, consumer goods repairs, and forest and park maintenance. The collective labor agreement for SSEA-workers stipulated that the minimum wage for people working in sheltered workshops was the same as for regular workers. Hence, the personnel costs of a SSEA-worker typically exceeded their labor productivity. To compensate for this, the municipal organizations were subsidized by the resident municipality. In turn, municipalities received funds from the central government based on the number of people eligible for sheltered employment residing in the municipality.

Only few of the SSEA-workers moved on to regular employment, typically those with mild disabilities and thus relatively high productivity.⁶ As a result, the number of SSEA-eligibles working in sheltered workshops grew steadily over the years until central government put a cap on the number of sheltered workers. As a consequence, waiting lists for SSEA-jobs were introduced. These waiting lists operated on a first-in-first-out basis, although placement managers had some discretion to place a person who was not at the top of the list if they felt that this would result in a better match with the vacant job. By 2014 the average waiting list duration was 23 months.⁷

Since 2015, the Participation Act (PA) has replaced the SSEA. One of its main policy goals is to foster the chances for disabled workers to find (supported) employment with a regular employer. The underlying rationale for this objective is twofold. Firstly, regular or supported employment is seen to better fit the principles of an inclusive society than sheltered employment. Secondly, placing disabled persons at regular employers by granting wage subsidies is generally considered to be less costly than creating jobs at sheltered workshops. In order to create incentives to place disabled workers in a cost-effective way, municipalities receive a lump-sum, unconditional grant from the central government.⁸

⁵ <https://zoek.officielebekendmakingen.nl/kst-24787-3.html> (Tweede Kamer der Staten-Generaal, 1996, in Dutch).

⁶ Around 8 percent of the SSEA-eligibles found a job at a regular employer, with the employer being granted a wage subsidy by the resident municipality. These workers received on-the-job assistance from trained job coaches.

⁷ See Hartevelde et al., 2015 (in Dutch).

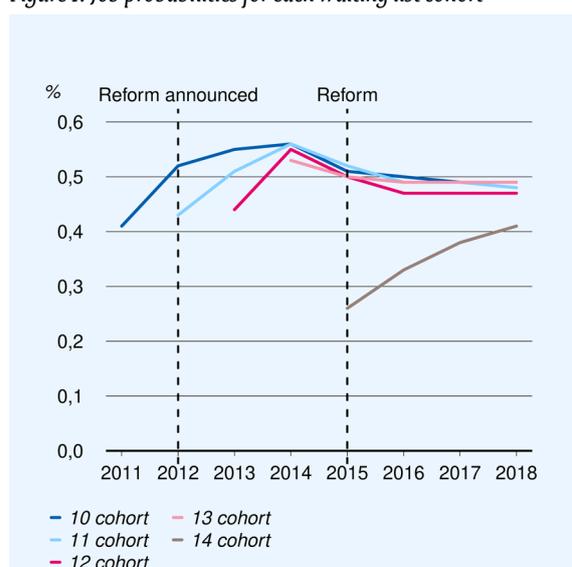
⁸ The yearly amount of the grant received by the municipality is determined by the municipal characteristics, such as the age composition of the local population in the year before.

The abolishment of the SSEA also dictated the closure of the sheltered workshops for new influx as of the 1st of January 2015. Only persons who were already on a SSEA-contract at this date retained their entitlement to a sheltered job and the corresponding terms of employment as being stipulated by the SSEA. As a consequence, the vast majority of people who were placed in a sheltered workshop under the SSEA remained working there in the years after the policy reform. By contrast, those on the waiting list at the end of 2014 lost their SSEA-rights and fell under the new regime. Municipalities are responsible for guiding these people to a job with a regular employer. In order to do so, they can offer the hiring employer a wage subsidy that bridges the gap between the worker's productivity and the minimum wage. Only in cases of severe disabilities, municipalities are allowed to offer sheltered employment after the 1st of January 2015.

Despite the goal of activating a greater number of people with a disability, the employment opportunities of those who lost their SSEA-rights fell dramatically in the years right after the policy reform. Figure 1 shows the job probabilities for disabled workers, clustered by the year in which they entered the waiting list. Here and throughout the rest of the paper, we consider a person to be employed in a given year if he or she holds a paid job for at least one day during this year. All pre-2014 cohorts show a similar pattern over the years: their job probability increases in the years after they enrolled on the waiting list and eventually levels off at about 50 percent. We see this similarity as evidence that there was no sorting of disabled workers into sheltered jobs around the and the policy reform date, ensuring the exogeneity of the shock.

The 2014 cohort, however, faces a much lower job probability right after the policy reform and this difference persists in later years, although to a lesser extent. Part of this drop in employment appears to be temporary: anecdotal evidence suggests that it took some time for all parties involved – municipalities, employers, and employees – to adjust to the new situation. However, part of the decreased job probability seems to be permanent. There could be various reasons for this, including municipalities' incentives to save on wage subsidies. What is important for our estimation purposes, is that those who lost their SSEA-rights faced a downward shock in employment probability. As long as this shock is not related to individual characteristics, the year in which someone entered the waiting list qualifies as an instrumental variable for employment status. We assess the validity of this instrument in the next two sections.

Figure 1: Job probabilities for each waiting list cohort



4 Data

The core of our data set consists of persons who were eligible for sheltered employment under the SSEA. This data set contains their disability type, the dates at which they entered and exited the waiting list, and the reason for exiting the waiting list. These data are augmented by information about labor market outcomes in later years and various demographic and socio-economic background variables, such as gender, age, migration background and the type of income support the person receives. All of these data are provided by Statistics Netherlands.

4.1 Sample selection

The raw data at hand contain all SSEA-eligibles who enrolled on the waiting list during the period 2010-2014. Our final data set only includes those who, during this period, entered the waiting list for the first time either in 2013 or in 2014 but did not find a job within the very same year. We thus exclude both those who are given high priority and those who remained on the waiting list for several years, thereby minimizing the risk of selection bias.⁹ As explained above, the policy reform on the 1st of January 2015 created a large difference in job probability between these two cohorts. As long as the year in which someone was sitting on the waiting list is not confounded by unobservable factors that also affect our outcome variables, this one-year difference can be used as a source of exogenous variation in job probability.

Our '13- and '14-cohorts turn out to be very similar on an extensive set of demographic and socio-economic characteristics, as can be seen from Table 1. The main difference is that our '14 cohort is on average one year younger, and accordingly, is slightly more likely to live with their parents and to receive work and income support for the young and disabled. To check that these small differences do not impact our results, we perform a robustness analysis in which we only include people aged 30 to 50.¹⁰ In that case, no significant differences between the two cohorts remain, see Table A.1 in the Appendix. The corresponding estimates, reported in the Results section, are very similar to the results for the full sample, assuring that our outcomes are not driven by the age imbalance.

Furthermore, we do not find any significant difference between the two cohorts in the prevalence of mental health care use and criminal activity in the pre-treatment period 2012-2013. The two cohorts being very similar on these different characteristics is an indication that there are no systematic differences between the two groups that could bias our estimates. This idea is supported by the fact the SSEA dictated that the ranking of the waiting list was based on the date eligibility was granted, while the assessments were done by an independent body with no (financial) interest in having certain SSEA-eligibles being placed first.¹¹

The one-but-last row of Table 1 shows that the two cohorts do differ substantially in the percentage of people who found employment in the year after they were approved for sheltered employment and enrolled onto the waiting list for the first time. Given the similarity between the two cohorts on all other observable aspects, we

⁹ Placement managers could deviate from the first-in-first out rule if doing so would result in a substantially better match, implying that those who were a poor match with any type of SSEA-job typically remained on the waiting list for several years. Furthermore, some municipalities geared their activation policies towards certain target groups (e.g. young adults with a disability) by giving these groups priority access to SSEA-employment.

¹⁰ We choose the age range of 30 to 50 for this robustness check because the kernel densities of age for both cohorts show near identical densities for this range (not shown here).

¹¹ See VNG/Cedris, 2008 (in Dutch).

attribute this gap in job probability to the year in which the cohort entered the waiting list. This makes the cohort-year a strong and valid instrument, as we discuss in more detail in the next section.

Table 1: Comparison between '13- and '14'-cohort on background characteristics and pre-treatment prevalences

	'13-cohort	'14-cohort	Difference	p-value
	in %	in %		
Demographic and socio-economic characteristics				
Female	36.3	36.8	-0.5	0.70
Migration background	32.3	30.8	1.5	0.17
Average age (when entering the waiting list, in years)	38.5	37.4	1.1	0.00
Household type:				
Single	35.1	34.2	0.9	0.42
Couple	27.3	27.0	0.3	0.81
Single parent	8.5	7.2	1.3	0.05
Institutional household	6.2	6.4	-0.2	0.81
Living with parent(s)	20.8	23.1	-2.3	0.02
Other	2.2	2.1	0.1	0.83
Disability type:				
Physical/cognitive	27.4	26.1	1.3	0.22
Mental	52.9	54.7	-1.8	0.12
Other	19.7	19.2	0.5	0.56
Type of welfare assistance:				
Welfare benefits	51.1	53.2	-2.1	0.07
Work and Income Support for Disabled Youth	20.0	17.3	2.7	0.00
Unemployment Insurance benefits	9.2	9.1	0.1	0.96
Other	19.6	20.4	-0.8	0.42
Prevalence in the (pre-treatment) period '12-'13				
Mental health care use	40.5	41.4	-0.9	0.44
Home care use	25.8	25.4	0.5	0.69
Criminal activity	12.2	12.1	0.1	0.96
Being employed in the post-treatment year				
Being employed in the post-treatment year	53.5	25.9	27.6	0.00
Observations	3673	3611		

4.1 Outcome variables

Concerning these outcome variables, our data set contains detailed mental health indicators and criminal records at the individual level for the period 2010-2017. With regard to mental health indicators, we have data on the use of mental health care and non-medical home care. Concerning non-medical home care, we only include those forms of care that are mainly targeted at people who, due to psychological or psychosocial problems, find it difficult to look after themselves. For both mental health categories, we construct a dummy variable that assumes the value one if the individual makes use of the care type in a given year and consider this outcome to reflect a worse mental health care status than non-use, *ceteris paribus*.¹² These indicator variables thus exhibit both between-person and within-person variation. We exploit the panel structure of our data by adding mental health care and nonmedical home care use in the pre-treatment period to our regressions.

The dummy variables give us the prevalence of health care use in our sample, as plotted in Figure 3 – the dark blue lines in panels (a) and (b). Comparing this with the prevalence in the total Dutch population – the light blue lines in panels (a) and (b) – reveals that for both forms of care the use in our sample far exceeds the national average.¹³ For instance, the fraction of persons in our sample using mental health care is approximately four times as large as the percentage users in the total population.

Criminal records contain all persons who have been accused of at least one criminal offence in a given year.¹⁴ These include financial and property crimes (e.g. robbery) as well as non-financial offences (e.g. assault). We construct a dummy variable for criminality in the same manner as we have done for our mental health indicators. Panel (d) of Figure 2 shows that the prevalence of criminal activity in our sample is roughly four times as high as the national average. For both our sample of SSEA-eligibles and for the population as a whole the percentage of people being accused of a crime is declining from 2011 onwards.¹⁵

¹² This assumption would be problematic if access to these forms of care is associated with personal characteristics that in turn are correlated with the actual health status (e.g. higher-educated persons both being healthier and having better access to the health care system than low-educated people). However, we are confident that this is not an issue with our data as both types of care considered here are accessible to all via public provision or mandatory insurance.

¹³ Note that here we plot the yearly prevalence, whereas Table 1 reports the prevalence for a two-year period therefore showing higher percentages.

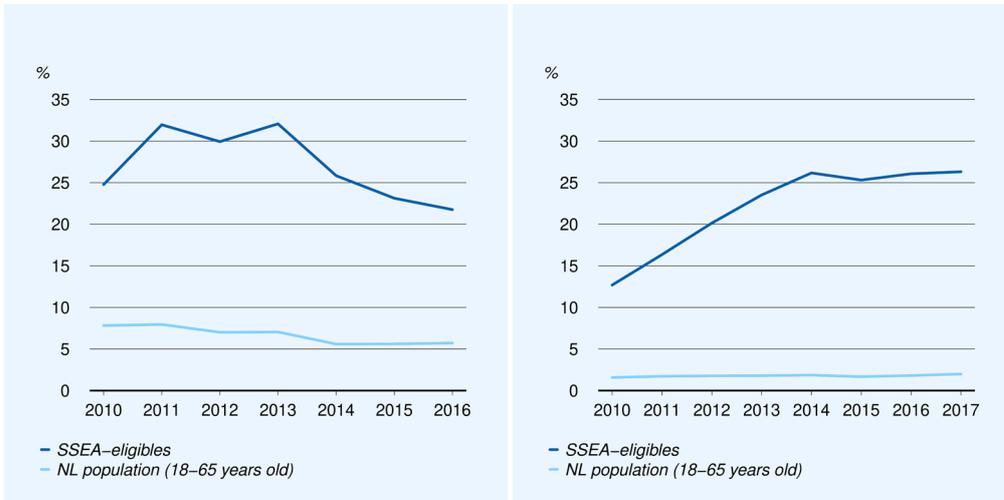
¹⁴ Due to the possibility of false accusations these data may not reflect true criminal activity in all cases. Unfortunately, we do not have data on actual verdicts.

¹⁵ Relative to the overall Dutch population, SSEA-eligibles are more often suspect of assaults and sexual offences and less frequent suspect of serious traffic violations and drug-related crime.

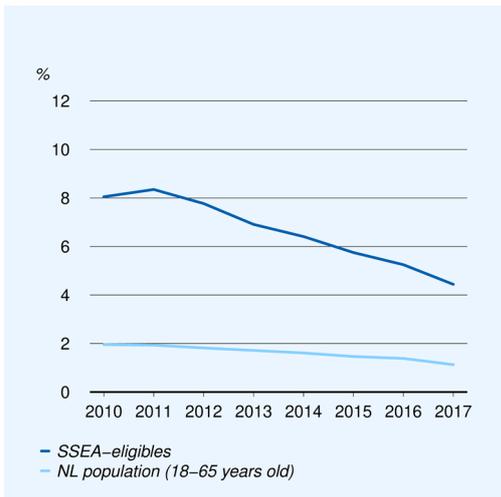
Figure 3: Prevalence of health care use and criminal activity: our sample versus the Dutch population

(a) Prevalence of mental health care use

(b) Prevalence of non-medical home care use



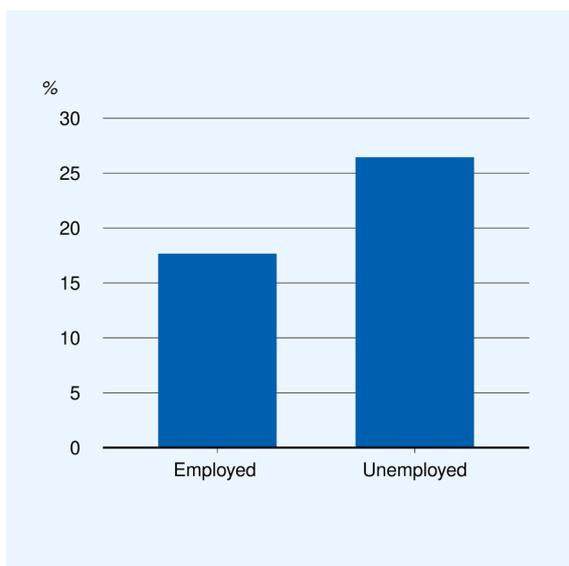
(c) Prevalence of criminal activity



5 Empirical strategy

The positive association between unemployment on the one hand and poor health conditions and criminal activity on the other is a well-known societal phenomenon. This positive correlation is also found in our data. Figure 4 displays the health care use in 2015 for the employed and for the unemployed in our sample. Among the employed the use of mental health care is about 9%-points (or 33 percent) lower than among the unemployed. For the other outcome variables, we find similar correlations.

Figure 4: Prevalence of mental health care use in 2015 for the employed and unemployed within our sample



However, both unobserved heterogeneity (e.g. more able and better motivated persons have greater chances to find and hold a job) and reverse causality (e.g. healthier people sort into better jobs) impede interpreting this positive association as the causal impact of job status on other life domains. As already touched upon above, using the exogenous variation in job status provided by policy reform allows to control for these individual idiosyncrasies. We now discuss in more detail our empirical strategy.

5.1 Specification

We estimate the effect of having a job on our outcome variables within a LATE framework.¹⁶ That is, we consider the year in which someone is first eligible for sheltered employment as an exogenous treatment that is received by the '13-cohort but not by the '14-cohort. As long as this treatment affects the outcome variables only through job probabilities, it can be used as an instrument for the endogenous employment variable. The first stage of this IV approach is then as follows:

$$(1) \text{ Job}_i = \gamma_0 + \gamma_1 W_i + v_i$$

Job_i is an indicator that equals 1 if individual i is employed at $t+1$ and 0 otherwise. W_i equals 1 if individual i belongs to the '13-cohort and 0 if he or she belongs to the '14-cohort. The variable v_i is the error term. The parameter of interest is γ_1 , which gives the increase in the job probability due to belonging to the '13-cohort rather than the '14-cohort. As shown in the previous section, the estimate of this parameter is around 25 %-points and significantly different from zero.

In the second stage we relate the outcome variables of interest Y_i to the estimated job probability following from equation (1):

$$(2) Y_i = \beta_0 + \beta_1 \widehat{\text{Job}}_i + \varepsilon_i,$$

¹⁶ The first-in-first out rule of the waiting list might appear to the reader as a clear case for a regression discontinuity approach. However, as noted before it was possible to deviate from this rule if doing so resulted in a better match between a person on the waiting list and the vacant job. As a result, in the data no 'cutoff' date can be identified after which the job probability for the population under study abruptly drops.

where ε is the error term. The parameter of interest is β_1 , which is the LATE-effect of being employed on the various outcome variables. This effect can be interpreted as the causal impact of having a job on the outcome variables for the compliers. The group of compliers consists of persons who hold a job because they enrolled on the waiting list in 2013 but would have been unemployed had they entered one year later. Below, we discuss in more detail to what extent the LATE-assumptions are expected to hold in our setting.

5.2 Measuring the job indicator and outcome variables

First, we elaborate on what measurement periods to choose for our job indicator and our outcome variables. In a regular LATE-setup, both the control and treatment group are created at the same time and monitored accordingly. By contrast, in our case the treatment group is composed one year later than the control group as the waiting list year determines treatment status. Hence, it is at forehand not obvious whether or not one should take the same measurement periods for both cohorts to construct the variables of interest. It is therefore important to motivate the choices we make.

To construct our job indicator Job_i , we measure one's employment status one year after entering the waiting list. That is, for the '13-cohort we take the employment status in 2014 and for '14-cohort the employment status in 2015. By doing so we make sure that differences in job status are not driven by unemployment duration. If, for instance, job search intensity declines with unemployment duration, taking the same measurement period for both cohorts – say 2015 – would bias the true effect of the waiting list year on employment probability. On the other hand, taking different measurement periods could potentially lead to differences in job probability between cohorts that are driven by year-to-year fluctuations in the business cycle. To rule this out, we have repeated our analysis using the same measurement period for job status (2015) for both cohorts as a robustness check. The resulting estimates are very similar to our baseline results and are further discussed in the Results section.

Concerning our outcome variables, we use the outcomes in 2016 for both cohorts. Using the same outcome measurement year ensures that our results are not biased by trends at the macro level or administrative changes in the data registrations.^{17,18} As a robustness check, we repeat our analysis using the 2017 values for all outcome variables. We expect slightly less significant results using the outcomes in later years since the employment probabilities of our treatment and control groups converge somewhat after 2015 so that less variation remains to identify the effects of having a job (see Figure 1). Considering the same measurement period for both cohorts however creates exposure time effects, as outcomes are measured after 3 years for the '13-cohort and only after 2 years for the '14-cohort.¹⁹ Differences in exposure time could lead to biased estimates if there is persistence in mental health care use or criminality over the years. For instance, care treatments exceeding one year cause persistence in care use.

To test for the presence of such an exposure effect, we perform a placebo analysis in which we compare outcomes of the '13 cohort with the cohort that entered the waiting list in 2012. Given that the differences in

¹⁷ Note that at the macro level there is a downward trend in both criminal activity and mental health care use and an upward trend in home care use, as can be seen from Figure 3.

¹⁸ With respect to changes in registration methods, we note that in 2014 the mental health care system has been reformed with the aim to reduce the use of specialized, and therefore expensive types of mental health care by focusing more on preventive care. Together with this reform, a new registration method for mental health care use has been introduced. Concerning non-medical home care, a major reform took place in 2015 when municipalities become responsible for a large part of this type of care. Also this reform has led to a change in the mode of registration for the use of non-medical home care.

¹⁹ Moreover, age effects may also play a role because there is a difference in age between our comparison groups in the year of measurement. The waiting list group from 2013 will by construction be one year older than the group in 2014 when we measure their outcomes in the very same year. As age can influence both outcomes and job probability, we control for this variable by including age (in the measurement year) in all our specifications.

job probabilities among these cohorts are negligible (see Figure 1) and that these two cohorts are comparable in background characteristics (see Table A.2 in the Appendix), a significant effect of the waiting list year on the outcome variables would cast doubt about the validity of our approach. We run this test by estimating reduced form regressions within the LATE setup discussed above:

$$(3) Y_i = \beta_0 + \beta_1 W_i + v_i$$

In this specification we relate the outcome variables to an indicator for the waiting list year. In our placebo analysis this indicator equals 1 for the '13-cohort and 0 for the '12-cohort. In all regressions we control for age, lagged outcomes and the other the background characteristics reported in Table 1. We also include regional fixed effects in order to control for heterogeneity in labor market conditions and the use of sheltered employment as part of activation policies among regions in the Netherlands.

Table 2, columns 1-3 present the results of this analysis for three different years and for each of our outcome variables – criminality, non-medical home care use, mental health care use. For comparison reasons, columns 4-5 show the reduced form estimates belonging to our main analysis. The main message from this table is that in all of our placebo tests the effects turn out to be insignificant, which lends credence to the validity of our design. By contrast, all reduced form estimates for our main analysis have the expected (negative) sign and are significantly different from zero, except for criminality in the year 2017. In the next section, we elaborate more on the results for our main analysis when we present our IV estimates.

Table 2: Placebo tests using '12 cohort as the control group

	Placebo tests			Main analysis (reduced form)	
	2015	2016	2017	2016	2017
Criminality	0.005 (0.005) ^a	-0.001 (0.005)	-0.006 (0.005)	-0.011 ^{***b} (0.005)	-0.006 (0.005)
Mental health care use	0.017 (0.009)	0.011 (0.009)	0.011 (0.009)	-0.022 ^{**} (0.009)	-0.019 ^{**} (0.009)
Non-medical health care use	0.014 (0.008)	0.007 (0.009)	0.006 (0.009)	-0.025 ^{***} (0.009)	-0.031 ^{***} (0.009)
Controls ^d	Yes	Yes	Yes	Yes	Yes
Observations	7350 ^c	7318	7281	7284	7245

Notes: (a) Robust standard errors in parentheses. (b) ***, **: significant at the 1 and 5 %-level. (c) For each measurement year, we exclude persons who died during or before this year. (d) We include the following controls: age (in the year of measurement), age squared, gender, migration background, household type, disability type, type of welfare assistance, lagged value of the outcome variable and regional fixed effects.

5.3 Validity of our IV-design

Three conditions should hold for interpreting β_1 in equation (2) as the causal effect of being employed on the outcome variables. Firstly, the instrument should be sufficiently correlated with the endogenous regressor (employment status). This is clearly the case, as employment in the '13-cohort is about 25%-points higher than in the '14-cohort with a corresponding F-statistic exceeding 800 which passes any criterion test.

Secondly, unobserved characteristics of persons on the waiting list should not be correlated with either the outcome variables or the instrument. For instance, the waiting list in 2013 should not contain significantly more persons with a mild disability – and therefore a relatively high productivity – than the 2014 waiting list. Then, covariate balance between the treatment and control group provides confidence that these groups are similar in unobserved attributes. Comparing the two cohorts on a large set of observables (see Table 1 in the

Data section) indeed confirms balance between the two cohorts. Still, in all our regressions we control for this set of background characteristics to account for the small differences between the cohorts. This includes lagged values of our outcome variables, so the outcomes in the pre-treatment period. By exploiting the panel structure of the data in this way, we control for any remaining time-invariant unobserved heterogeneity.

Thirdly, the exclusion restriction should hold. This requires that the loss of SSEA-rights – as a result of entering the waiting list in 2014 rather than 2013 - should only influence outcomes via the fall in job probability. This assumption may not hold if the loss of these rights directly increases stress levels, which could negatively affect one’s mental health. To investigate this, we test whether the year of entering the waiting list impacts stress levels. Although we do not observe stress levels directly, we can look at the use of anxiolytics, a drug that is prescribed to reduce stress and anxiety. Table 3 shows that the year of entering the waiting list has no significant impact on this outcome just before (2014) or just after (2015) the policy reform. This provides supportive evidence that our instrumental variable impacts the outcomes through job probability only and not directly through stress resulting from the prospect of losing SSEA-rights.

Table 3: Test to examine the presence of a direct effect of the waiting list year on the use of anti-stress medication

	Use of anxiolytics	
	2014	2015
Waiting list year (‘13-cohort=1)	0.005 (0.006)	-0.002 (0.006)
Controls	Yes	Yes
Observations	7340	7311

Notes: (a) Robust standard errors in parentheses. (b) For each measurement year, we exclude persons who died during or before this year. (c) We include the following controls: age (in the year of measurement), age squared, gender, migration background, household type, disability type, type of welfare assistance, lagged value of the outcome variable and regional fixed effects.

6 Results

We start this section by discussing the estimation results for our preferred specification, in which all outcome variables are measured in the year 2016. Thereafter we discuss the results when using 2017 outcomes. Finally, we perform some heterogeneity and robustness analyses respectively using the outcomes for our preferred year 2016.

6.1 Main results

Table 4 presents the main estimation results, for the specifications without and with controls. As controls we include age, lagged outcomes and the other the background characteristics reported in Table 1. We also include regional fixed effects in order to control for heterogeneity in labor market conditions and the use of sheltered employment as part of activation policies among regions in the Netherlands. Adding controls has no significant effect on the estimates, as expected since the 2013 and 2014 cohorts are very similar in observable characteristics.

In our preferred specification – with controls, outcomes measured in 2016 – we find that being employed reduces the probability of engaging in criminal activity by 3.5%-points, the use of non-medical home care by 8 %-points and the use of mental health care by 7%-points. These effects are substantial: relative to the baseline prevalence in our sample of disabled persons, they range between 30 percent (mental health care and non-medical home care) and 60 percent (criminality).

Table 4: IV-estimates for the full sample

	2016	2016	2017	2017
Criminality	-0.045** (0.019)	-0.035** (0.017)	-0.032 (0.018)	-0.019 (0.016)
Mental health care use	-0.085** (0.035)	-0.071** (0.030)	-0.068** (0.034)	-0.063** (0.029)
Non-medical health care use	-0.082*** (0.037)	-0.081*** (0.028)	-0.106*** (0.038)	-0.100*** (0.030)
Controls	No	Yes	No	Yes
First-stage coefficient	0.276*** (0.011)	0.306*** (0.011)	0.276*** (0.011)	0.306*** (0.011)
Observations	7284	7284	7245	7245

Notes: (a) Robust standard errors in parentheses. (b) ***, **: significant at the 1 and 5 %-level. (c) For each measurement year, we exclude persons who died during or before this year. (d) We include the following controls: age (in the year of measurement), age squared, gender, migration background, household type, disability type, type of welfare assistance, lagged value of the outcome variable and regional fixed effects.

The estimates for criminality and mental health care become somewhat smaller when using 2017 as the year of measurement, with the estimate for criminality being no longer significant. This is to be expected, as outcomes for the different cohorts are likely to converge if job probabilities converge. The 2017 estimate for non-medical home care is very similar to the 2016 estimate.

Table A.3 in the Appendix reports the full regression output for the second stage, using 2016 as the year of measurement. In addition to the job status effect, we see that the use of mental health care is positively associated with age, being female, having a migration background, and having a mental disorder. Criminal activity is positively correlated with being male and having a migration background, while the use of nonmedical health care is associated with age but not with gender or migration background.

6.2 Heterogeneity analyses

In addition to our main analysis, we present two heterogeneity analyses. We first examine whether the effects of employment differ by gender. This analysis is motivated by the findings of existing studies that (un)employment has a stronger impact on men than it has on women. In line with this literature, Table 3 shows that men are indeed more heavily affected when it comes to criminality and the use of non-medical home care. The estimate for non-medical home care is nine times larger for men than for women (-0.124 vs. -0.014) and the estimate for criminality three times larger (-0.048 versus -0.016), although the latter difference is not statistically significant. These outcomes imply that the full-sample effect of having a job on non-medical home care and criminal activity is almost fully driven by men. Concerning the effect on the use of mental health care, the difference between men and women is less pronounced.

Next, we assess to what extent outcomes depend on the type of disability by splitting our sample into persons with a mental disorder and people with another type of disability (either cognitive or physical). This is motivated by our conjecture that employment status has the largest impact on people with a psychological disorder, because we expect them to be more susceptible to deviant behavior and psychological distress than persons with a physical or cognitive disability in case of long-term unemployment. Table 5 reveals that our results are consistent with this ex-ante expectation. For all three outcome variables, the estimates for people with a mental disorder are larger than for persons with another disability although the differences are not

statistically significant. For instance, we find that the effect on mental health care use is almost three times higher for people with a psychological disorder than for the rest of the sample (-0.096 versus -0.036).

Table 5: Heterogeneity analyses

	Men	Women	Mental disability	Other
Criminality	-0.048 (0.025)	-0.016 (0.017)	-0.039 (0.024)	-0.027 (0.024)
Mental health care use	-0.075 (0.039)	-0.065 (0.046)	-0.096** (0.045)	-0.036 (0.036)
Non-medical health care use	-0.124** (0.037)	-0.014 (0.043)	-0.100*** (0.039)	-0.048 (0.040)
Controls	Yes	Yes	Yes	Yes
First-stage coefficient	0.294*** (0.014)	0.327*** (0.017)	0.307*** (0.015)	0.307*** (0.016)
Observations	4623	2661	3919	3365

Notes: (a) Robust standard errors in parentheses. (b) ***, **: significant at the 1 and 5 %-level. (c) For each measurement year, we exclude persons who died during or before this year. (d) We include the following controls: age (in the year of measurement), age squared, gender, migration background, household type, disability type, type of welfare assistance, lagged value of the outcome variable and regional fixed effects.

6.3 Robustness analysis

Table 6 presents two robustness analyses. Firstly, we present the results only including persons aged between 30 and 50 year old. This ensures that there is no age difference between the two cohorts. Secondly, we present results for the full sample when using the job status in 2015 for both cohorts rather than the employment status after one year. Note that by doing so, we rule out that the first stage results are affected by macroeconomic fluctuations. Both robustness analyses give qualitatively similar results as our baseline regressions: all estimates are significant and similar to our main estimates.

Table 6: Robustness checks

	30-50 yrs. old	Job status in 2015
Criminality	-0.051** (0.025)	-0.039** (0.019)
Mental health care use	-0.079** (0.045)	-0.079** (0.033)
Non-medical health care use	-0.093** (0.040)	-0.090*** (0.031)
Controls	Yes	Yes
First-stage coefficient	0.304*** (0.015)	0.274*** (0.011)
Observations	3472	7284

Notes: (a) Robust standard errors in parentheses. (b) ***, **: significant at the 1 and 5 %-level. (c) For each measurement year, we exclude persons who died during or before this year. (d) We include the following controls: age (in the year of measurement), age squared, gender, migration background, household type, disability type, type of welfare assistance, lagged value of the outcome variable and regional fixed effects.

7 Concluding remarks

Our results show that persons with a disability experience substantial improvements in multiple domains of life if they obtain employment. Relative to the baseline prevalence in the group of disabled persons, the reductions in mental health care use, nonmedical home care and criminal activity range between 30 and 60 percent. These findings show that the benefits from activation policies targeted at this part of the labor force extend beyond the savings on unemployment or welfare benefits. Additional benefits come both in the form of a better quality of life for the worker in question and in the form of lower expenditures on collectively funded services for the society as a whole.

Due to the somewhat temporary nature of our exogenous shock –job probabilities for disabled people plummeting right after the policy reform and recovering to some degree in the years thereafter – we are only able to study the relatively short-run spillover effects of having a paid job. We expect our results to be a lower bound of the effects in the longer run: due to persistent negative effects of unemployment on later labor market chances (i.e. scarring), differences in the job market position as well as in health status and criminality between the employed and the unemployed are likely to diverge over time.

To what extent can our findings be extrapolated to other active labor market policies – including policies targeted at other groups of jobseekers? First of all, we cannot be sure that our results for sheltered employment carry over to regular (supported) employment. However, it seems likely that they do to some extent if common aspects of being employed, such as having an income, a daily structure, and a network of colleagues, drive the results.

Secondly, due to our LATE-approach the estimated treatment effects are only identified for the compliers – those who found employment *because* they received the treatment. Hence, whether the effects also apply to other groups within the sample (i.e. the always-takers and the never-takers) is unknown. For policy makers, however, this is less relevant since activation policies are typically targeted at those who obtain employment only through the help of a program. Indeed, this group coincides with the compliers in our analysis.

Thirdly, an important question that remains is whether our results also generalize to the average (non-disabled) jobseeker. We consider a rather specific group of jobseekers who have worse job opportunities and more adverse outcomes in other domains of life than the average labor market participant. Having said that, our results are supportive of some of the previous research on this topic that, for more general worker populations, find that unemployment has adverse effects on mental health and criminality. Since other studies come to contrasting findings, further research is needed to give a conclusive answer to the question how (un)employment impacts these life domains in general.

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Appendix

Table A.1: Background characteristics and pre-treatment prevalences for the age group 30-50

	'13-cohort	'14-cohort	Vershil	p-waarde
	in %	in %		
Demographic and socio-economic characteristics				
Female	37.9	37.2	0.7	0.65
Migration background	36.3	33.5	2.8	0.09
Average age (when entering the waiting list. in years)	40.8	40.7	0.1	0.60
Household type:				
Single	41.3	41.4	-0.1	0.97
Couple	32.3	33.6	-1.3	0.40
Single parent	11.8	10.5	1.3	0.24
Institutional household	5.4	5.2	0.2	0.78
Living with parents	7.6	7.5	0.1	0.97
Other	1.6	1.7	-0.1	0.86
Disability type:				
Physical/cognitive	21.3	21.3	0.0	0.999
Mental	59.5	60.0	-0.5	0.79
Other	19.2	18.8	0.4	0.74
Type of welfare assistance:				
Welfare benefits	64.5	63.0	1.5	0.36
Work and Employment Support for Disabled Youth	6.2	6.3	-0.1	0.95
Unemployment Insurance benefits	8.7	10.2	-1.5	0.14
Other	20.6	20.6	-0.0	0.98
Prevalence in the (pre-treatment) period '12-'13				
Mental health care use	46.6	46.3	0.3	0.87
Home care use	11.9	12.6	-0.7	0.51
Criminal activity	27.8	27.3	0.5	0.73
Observations	1776	1696		

Table A.2: Comparison between '12- and '13-cohort on background characteristics and pre-treatment prevalences

	'12-cohort	'13-cohort	Difference	p-value
	in %	in %		
Demographic and socio-economic characteristics				

Female	37.4	36.3	1.1	0.34
Migration background	32.5	32.3	0.2	0.88
Average age (when entering the waiting list, in years)	39.4	38.5	0.9	0.00
Household type:				
Single	35.3	35.1	0.2	0.87
Couple	29.5	27.3	2.2	0.81
Single parent	8.1	8.5	-0.4	0.53
Institutional household	5.7	6.2	-0.5	0.32
Living with parents	19.8	20.8	-1.0	0.32
Other	1.7	2.2	-0.5	0.19
Disability type:				
Physical/cognitive	25.1	27.4	-2.3	0.03
Mental	55.9	52.9	3.0	0.01
Other	19.0	19.7	-0.7	0.41
Type of welfare assistance:				
Welfare benefits	50.9	51.1	-0.2	0.82
Work and Employment Support for Disabled Youth	18.5	20.0	-1.5	0.08
Unemployment Insurance benefits	8.2	9.2	-1.0	0.13
Other	22.5	19.6	2.9	0.00
Prevalence in the (pre-treatment) period '11-'12				
Mental health care use	43.7	42.5	1.2	0.29
Home care use	23.9	25.8	-1.9	0.06
Criminal activity	10.7	12.9	-2.2	0.00
Observations	3645	3673		

Table A.3 Full regression output for our preferred specification (year of measurement is 2016)

	Criminality	Mental health care use	Non-medical care use
Job	-0.035** (0.017)	-0.071** (0.030)	-0.081*** (0.028)
Man	0.033*** (0.005)	-0.027*** (0.010)	0.0001 (0.009)
Migration background	0.016*** (0.006)	0.025** (0.010)	0.001 (0.009)
Age	-0.003 (0.002)	0.012*** (0.003)	0.006** (0.003)

Age squared	0.0001 (0.0000)	-0.0002*** (0.0000)	-0.0001*** (0.0000)
Household type:			
Single	ref	ref ^c	ref
Couple	0.0004 (0.0065)	-0.034*** (0.012)	-0.067*** (0.011)
Single parent	-0.012 (0.008)	-0.075*** (0.019)	-0.046*** (0.016)
Institutional household	0.016 (0.015)	0.045** (0.022)	-0.033 (0.019)
Living with parents	-0.013 (0.010)	-0.036** (0.016)	-0.041** (0.016)
Other	-0.016 (0.017)	-0.053 (0.029)	-0.028 (0.032)
Disability type:			
Physical/cognitive	ref	ref	ref
Mental	-0.002 (0.007)	0.091*** (0.011)	-0.006 (0.011)
Other	-0.014 (0.007)	-0.014 (0.012)	-0.063*** (0.013)
Type of welfare assistance:			
Welfare benefits	ref	ref	ref
Work and Income Support for Disabled Youth	-0.024** (0.009)	0.004 (0.015)	0.014 (0.016)
Work and Income Support for Disabled other	-0.017** (0.008)	0.056*** (0.016)	-0.008 (0.013)
Unemployment Insurance benefits	-0.005 (0.008)	-0.001 (0.017)	-0.002 (0.015)
Other	-0.018** (0.009)	0.022 (0.019)	-0.015 (0.018)
Pre-treatment use...	0.149*** (0.014)	0.252*** (0.010)	-0.535*** (0.012)
First stage coefficient	0.306*** (0.011)	0.306*** (0.011)	0.303*** (0.011)
F-test of excluded instruments	822.64	819.19	822.60
Regional fixed effects	Yes	Yes	Yes
Observations	7284	7248	7284
Estimator	IV	IV	IV

Notes: (a) Robust standard errors in parentheses. (b) ***, **: significant at the 1 and 5 %-level. (c) For each measurement year, we exclude persons who died during or before this year.