Dahl, Espen S.; Hernaes, Øystein

Working Paper

Making Activation for Young Welfare Recipients Mandatory

IZA Discussion Papers, No. 15170

Provided in Cooperation with:
IZA – Institute of Labor Economics

IZA – Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 15170

Making Activation for Young Welfare Recipients Mandatory

Espen S. Dahl
Øystein Hernæs

MARCH 2022

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 15170

Making Activation for Young Welfare Recipients Mandatory

Espen S. Dahl
Norwegian Directorate of Labour and Welfare and University of Oslo

Øystein Hernæs
The Ragnar Frisch Centre for Economic Research and IZA

MARCH 2022

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Making Activation for Young Welfare Recipients Mandatory*

Activation policies to promote self-sufficiency among recipients of welfare and other types of benefits are becoming more common in many welfare states. We evaluate a law change in Norway making welfare receipt conditional on participation in an activation program for all welfare recipients below the age of 30. Analyzing the program's staggered implementation across municipalities with several modern event study estimators, we estimate that the law change had quite precise 0-effects on benefit receipt, work and education. We also do not find any effects on the probability of being out of work or of being in employment, education or labor market programs. Qualitative evidence suggests that the zero effect may be due to the law change only impacting the participation of recipients with low expected gain from activation.

JEL Classification: H55, I38, J18

Keywords: social assistance, activation, conditionality, welfare reform, labor

Corresponding author:
Øystein Hernæs
The Ragnar Frisch Centre for Economic Research
The Frisch Centre
Gaustadalleen 21
0349 Oslo
Norway

E-mail: o.m.hernas@frisch.uio.no

* Administrative register data from Statistics Norway have been essential for this project. This research is funded by the Norwegian Directorate of Labour and Welfare (NAV) and Research Council of Norway (grant number 270772). We thank Edwin Leuven, Gaute Torsvik, Harald Dale-Olsen, and participants at the EALE 2021 Conference for valuable comments.

1 Introduction

Welfare benefits are one of the pillars of a welfare system: it is given to those with no other means of income, either through work or other types of benefits. The use of activation policies for welfare recipients is a common tool to reduce dependency on such benefits. Welfare recipiency is meant to be temporary and activation policies have been introduced to enhance the self-sufficiency of the recipients.

In this paper, we analyze a law change regarding the Norwegian welfare system. As of January 1, 2017, welfare recipients below age 30 must participate in an activation program. Municipalities that did not have an activation program prior to this law taking effect, were required to organize one. While most municipalities already used such policies, others implemented it with the law change, creating the natural experiment analyzed in our paper.

This paper contributes to an already substantial literature examining active labor market policies. The effects of active labor market policies in general have been summarized by Card *et al.* (2010) and Card *et al.* (2015). Hernæs *et al.* (2017) looked at the gradual introduction of conditionality for welfare recipients in Norway during the late 1990s and early 2000s and found that it led to a lower share of youths receiving welfare benefits and more youths completing high school. Similarly, Dahlberg *et al.* (2009), looking at the gradual implementation of mandatory activation programs in Stockholm city districts, found reduced welfare participation and increased employment. But not all studies find that activation policies have positive effects. Avram *et al.* (2018) found that introducing work search requirements for single parents in the UK has heterogeneous impacts, with some single parents moving into work and others going on disability benefits or non-claimant unemployment. This is similar to what both Manning (2009) and Petrongolo (2009) found when looking at job search requirements for the unemployed in the UK. They conclude that some claimants may find the search requirements too burdensome and stop looking altogether. Cammeraat *et al.* (2017) studied the effects of stricter conditionality for young welfare recipients in Netherlands finding a reduced number of NEETs (not in employment, education or training) on welfare, an increased number of NEETs not on welfare, and no effect on the total number of NEETs. Evaluating the increased use of strong sanctions for young welfare recipients in Germany, van den Berg *et al.* (2020) found that such sanctions led to an increased probability of finding a job, albeit with low wages. Further, the strong sanctions also lead other recipients to exit the labor force.

In our setting, different municipalities implemented the new policy at different times. We take an event-study approach in order to investigate possible pre-treatment trends and dynamic treatment effects. To avoid problems with the traditional event-study specification in the case of heterogeneous treatment effects (Borusyak & Jaravel, 2017; Callaway & Sant'Anna, 2021; de Chaisemartin & DHaultfœuille, 2020; Sun & Abraham, 2021), we implement the estimators of de Chaisemartin & D'Haultfœuille (2020), Borusyak & Jaravel (2017), and Sun & Abraham (2021). We focus on people younger than 30 in the quarter of the population with the highest estimated propensity to receive welfare. We analyze a comprehensive set of outcomes related to welfare recipiency, social insurance transfers, work and education, but do not find any significant effects of the new law requiring that municipalities offer and require participation in activation programs. Qualitative evidence suggests that these zero effects may be related to the fact that the municipalities had long had the opportunity to require participation in activation programs and to a large extent had done so. Individuals with the most to gain from an activation program perhaps had therefore already participated. The possibility of exceptions and a limited possibility of sanctions also likely reduced the number of recipients who actually participated in an activity because of the law change. The findings underscore the importance of taking conditions on the ground into account when introducing reforms, especially when implementation of the reform is decentralized.

The rest of the paper is organized as follows. The next section provides an overview of how welfare benefits are organized in Norway. We show how the use of activation policies varies between municipalities and discuss how these variations can lead to different outcomes for welfare recipients. In section 3 we show our empirical approach. Our results are shown in section 4 and the findings discussed in section 5.

2 Institutional setting and data

In the Norwegian welfare system, welfare benefits represent a benefit of last resort. Those who have no or low income from work or other benefits and little or no other means of income through savings or their household are eligible for welfare benefits. Unlike many other social insurance benefits, this benefit is administered and financed at the municipal level. The benefit is regulated through the Social Services Act. As can be seen from Figure 1, the recipiency rate is highest for people in their 20s and falls steadily with age.

Citing, among other things, concerns that receiving these types of benefits made recipients passive, the government changed the Social Services Act from January 1, 2017. This new law introduced a requirement for municipalities to set participation

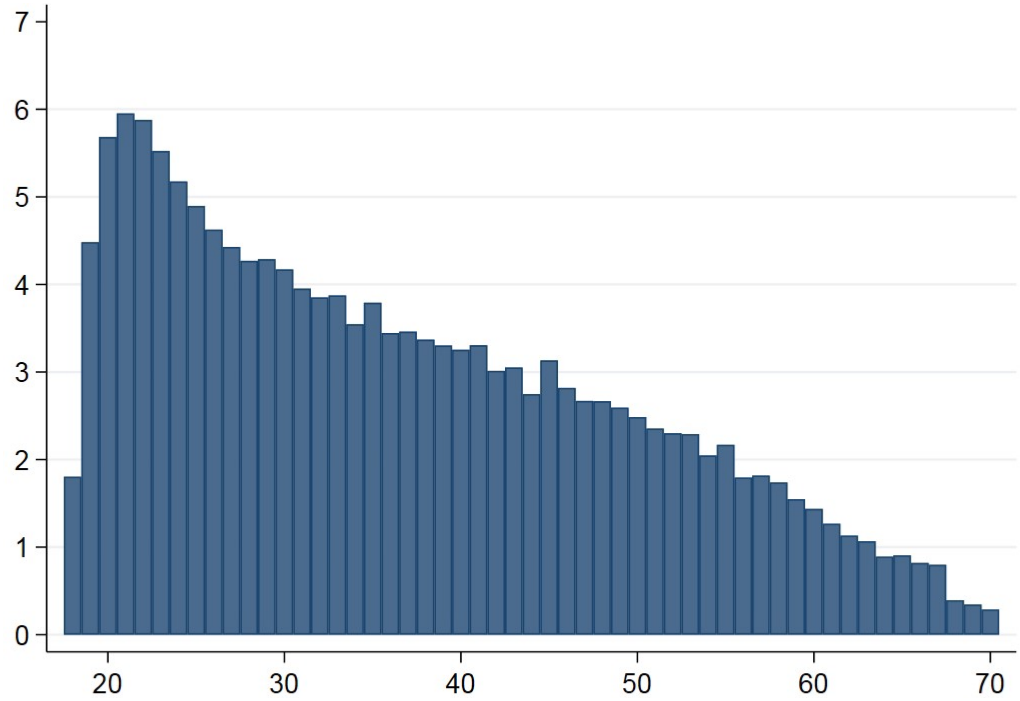


Figure 1. Percentage receiving welfare at least once during the year by age group, 2017

in an activation program as a condition for receiving welfare benefits for recipients younger than 30. These conditions could include attending guidance interviews, search for relevant work, or participation in work-oriented courses and initiatives. Recipients who lack work experience could be conditioned to participate in work training or other work-oriented activities to gain such experience. Others could be offered participation in education or other training measures. Prior to this reform, it was up to the municipalities whether to set conditions for welfare recipients and which conditions to set, with activation policies being one type of condition. Other conditions could still be used also after the law change.

To determine whether the municipalities used activation policies, we relied on information from a survey distributed to local social insurance offices in the spring of 2018 (Dahl & Lima, 2018).[1] In the survey, which was undertaken a little over a year after the law change, the social insurance offices were asked when they started to require welfare recipients under 30 to participate in an activation program. Local offices have always had substantial discretion over how to organize their social work and have to a large degree exploited the opportunity to set conditions for welfare recipients (Brandtzæg *et al.*, 2006; Proba, 2015; Dahl & Lima, 2016, 2018).

[1] There is one social insurance office in each municipality, while there are more than one in the four largest cities. There are 15 offices in the capital, Oslo, four in Bergen and Stavanger, and two in Trondheim.

A total of 271 separate offices replied to the question about when they had started mandatory activation. Of these, 122 offices reported that they already had activation policies in place before 2016. This group serves as the main control group in our analysis. A further 45 offices implemented activation policies in the beginning of or midway through 2016, 94 at some point during 2017, the year of the reform, and 10 in January 2018. Mandatory activation was introduced at six different points in time. The timing is illustrated in Figure 2. Regardless of the timing of introduction of activation policies, most municipalities report that there are more young recipients who are met with activation policies after the reform (Dahl & Lima, 2017). Above 50 percent report this to a large or very large degree, and around 30 percent to some degree.

However, a substantial share of the offices that reported having introduced mandatory activation before 2016 also reported that that the law change led to an increased number of people aged 18–29 with an activation condition. This could mean that these offices had not fully introduced activation for all and that they made changes in practice around the reform, in which case they may not be suitable as a control group. We therefore do a robustness check leaving out these municipalities. Specifically, from the group of 122 offices reporting that they had introduced mandatory activation before 2016, we keep only those 37 offices that also report that they did not observe an increased number of cases in conjunction with the law change. This leaves a balanced sample of 186 offices for this robustness exercise.

Table 1 shows some observable characteristics of the municipalities by year of implementation of activation for all. The largest difference between the groups of municipalities is that those that adopted the policy in 2016 or before, i.e. before the reform, are substantially larger than later adopters.

From administrative registers, we combine information about individuals' welfare status, employment, income, education, labor market program participation, disability status and demographic and family background. Whether an individual must meet a particular condition to participate in an activation program is not recorded in the data. We also do not have information about actual activation program participation, exceptions or sanctions. We use the following monthly outcome measures in our analysis: Welfare benefit receipt, welfare exit and entry, labor income, registered at an educational institution (high school or higher education), participating in a government labor market program, receiving disability benefit receipt and NEET (not employed, in education, or labor market program).

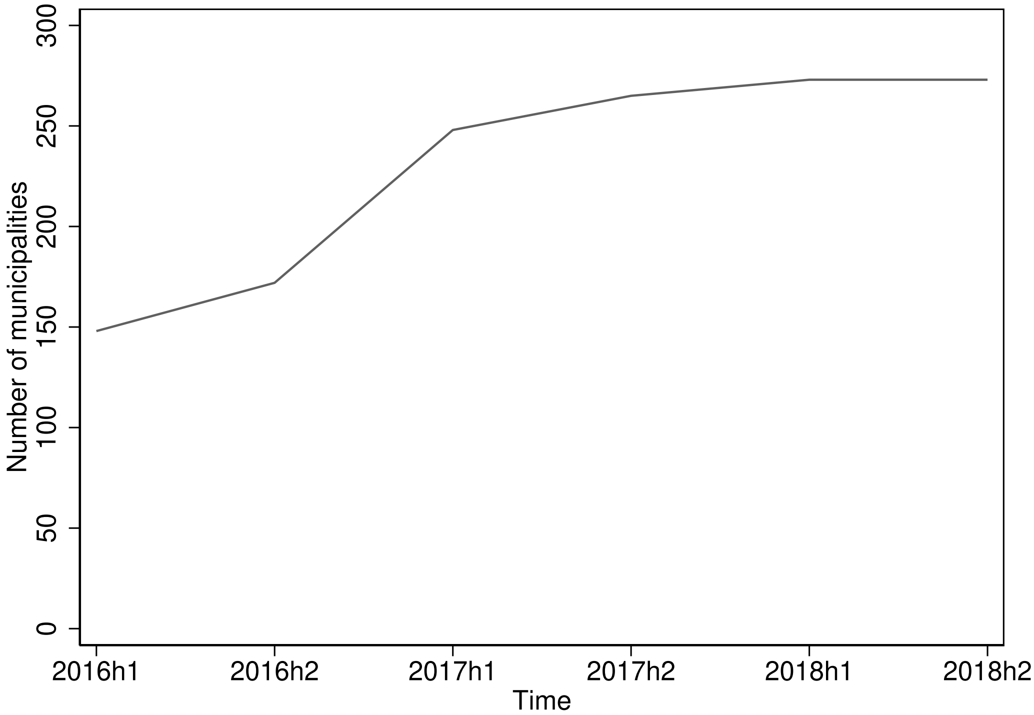


Figure 2. Number of municipalities with activation for all welfare recipients. 2016-2018.

Table 1. Municipality characteristics in 2015 by year of reform implementation

	Pre-2015	2016	2017	2018	No information
Number of persons 18-40 years	12 247	14 456	8 203	5 199	9 332
High school completed	0.73	0.74	0.72	0.67	0.73
University education ecompleted	0.38	0.40	0.36	0.30	0.35
Total income, 1000 €	40.3	40.6	39.4	37.4	38.9
Work income, 1000 €	34.9	35.4	34.3	31.3	33.5
Work income = 0	0.13	0.13	0.12	0.14	0.13
Work income > 1 basic amount	0.71	0.71	0.72	0.68	0.70
Work income > 2 basic amounts	0.61	0.61	0.62	0.58	0.60
Unemployed	0.04	0.04	0.04	0.04	0.04
Temporary or permanent disability benefit receipt	0.07	0.07	0.07	0.08	0.08
Welfare benefit receipt	0.05	0.05	0.05	0.06	0.05
NEET	0.08	0.08	0.07	0.09	0.08
Number of municipalities	122	45	94	10	185

NOK amounts inflated to 2019-value with the adjustment factor ("basic amount") used in the Norwegian pension system (approximately corresponding to the average wage growth).

3 Empirical approach

To estimate the causal effect of the law change, we use the fact that we know which municipalities introduced activation policies with the law change, and which already had such policies in place. Ideally, we would like to capture the average treatment effect of being required to participate in an activation program; however, we do not observe who is subject to an activation obligation at the individual level. Instead, we focus on the intention-to-treat (ITT) effects at the municipal level, comparing the development of municipalities that introduced activation for all at the time of the law change with municipalities that did not change their policy. The population of interest is people who are in contact with the local social insurance office and are affected by their policies. Since we cannot observe this population directly, we attempt to determine it by estimating individuals' propensity to receive welfare.

3.1 Comparing municipalities introducing activation policies with the new bill

The standard the two-way fixed effects estimation equation is given below, where we compare the municipalities that already used activation policies with those that introduced it with the new bill.The model uses multiple treated municipalities and is estimated using a linear probability model:

$$Y_{ijt} = \delta D_{jt} + \beta_1 X_{it} + \beta_2 M_{jt} + \lambda_t + \gamma_j + \varepsilon_{ijt}. \tag{1}$$

Here, Y_{ijt} is the outcome variable of interest for individual i in municipality j in period t, λ_t is the time fixed effect, γ_j the municipality fixed effect, D_{jt} is the treatment indicator of whether the municipality has introduced an activation policy or not, X_{it} and M_{jt} are vectors of observed characteristics at individual and municipality levels, respectively, and ε_{ijt} are the error terms. The aim is usually to estimate the treatment effect coefficient δ. Under homogeneous treatment effects and parallel trends in the absence of treatment, δ has the effect of making activation mandatory. Standard errors are clustered at the office level.

Throughout, municipalities that reported having introduced a mandatory activation policy before 2016 form the control group. This is potentially problematic as those earlier policy changes may have had long-lasting dynamic effects. We assume that most of these changes happened far enough back in time that potential long-term effects have stabilized and in practice consider this group as never-treated. As a check on this assumption, we also estimate a model using only municipalities from this group that reported that the law change had led only "to a small degree" or "not at all" to more people aged 18-29 having an activation requirement.

One natural margin to evaluate would seem to be people above or below agd 30, which would even allow for a triple difference approach. However, an initial report on the refom found that these groups displayed very different trends in welfare receipt in the years prior to the reform, thus those above age 30 are not a good counterfactual in this case to those below age 30 Hernæs (2021).

3.2 Estimating welfare propensities to predict likely welfare recipients

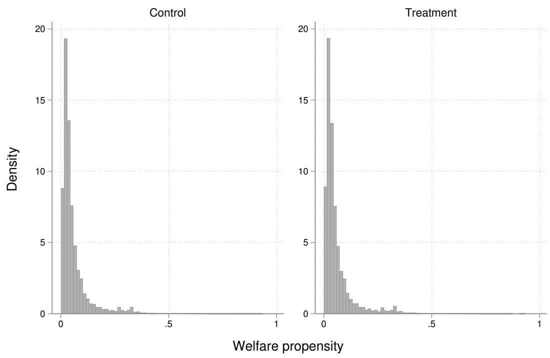
Relatively few people are in contact with their local social insurance office and are affected by their policies regarding social assistance. Thus average effects for the population as a whole will hardly be discernible. To focus more closely on the target group for social policy, we estimate the probability of receiving welfare benefits based on observable, pre-treatment characteristics. This is similar to what was done by Hernæs *et al.* (2017). We primarily use information about the parents of those eligible for welfare benefits to construct the propensity scores for welfare uptake :

$$\hat{p}_i = \frac{exp\,(b_i'\hat{\pi})}{1 + exp\,(b_i'\hat{\pi})} \tag{2}$$

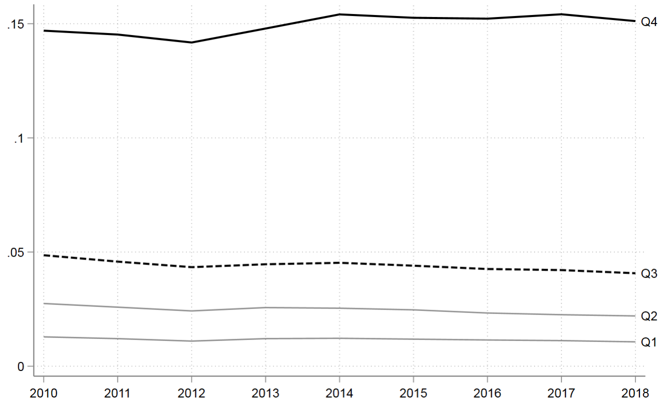
The estimation uses parents' income, education, welfare status and marriage status when the person was 15, as well as immigrant background and sex. The relationship between these variables and actual welfare receipt is estimated for the years 2010–2015 separately for ages 20–29, see table A1 in the appendix, and predicted for each individual for 2016–2019. This gives a probability for welfare receipt for each person for each of the years 2016–2019. To avoid placing too much emphasis on the actual probabilities, we group the estimated probabilities into quartiles and estimate intention to treat effects separately for each quartile.

Figure 3 displays the distribution of estimated welfare propensities in the treatment and control groups and the evolution of actual welfare receipt by year for the four quartiles. Panel a shows that the distribution is very similar for the treatment and control groups. Panel b shows that the prediction provides substantial information, with most actual welfare recipients coming from the high propensity welfare group Q4. It is also clear that average welfare receipt remains quite stable for a number of years. In our analysis, we focus on the fourth quartile, i.e. those with the highest predicted propensity to receive welfare, because these individuals are much more likely than the other groups to actually be in contact with the social insurance office. For instance, actual welfare receipt in Q4 is more than three times the level in Q3, the next highest group.

Table 2 shows descriptive statistics by quartile. As expected, the share receiving



(a) Welfare propensity



(b) Actual welfare receipt

Figure 3. a: Distribution of welfare propensity in control and treatment groups 2015-2019. b: Average actual welfare receipt by welfare propensity quartile.

welfare benefits increases with each quartile. Similarly, higher quartiles includes a larger share of foreign-born individuals, as well as those with no or low income, with transfers, with disability and NEET, and smaller shares of individuals who completed high school, or are in education.

3.3 Event-study approach

The two-way fixed effects model assumes that the effect takes the form of an instantaneous jump in the level of the outcome variable at the introduction of the policy. Recent literature has emphasized how the two-way fixed effects approach may give a misleading estimate of the average treatment effect if the treatment effect is dynamic or heterogeneous in time or for different units (Athey & Imbens, 2021; Borusyak & Jaravel, 2017; Callaway & Sant'Anna, 2021; de Chaisemartin & DHaultfœuille, 2020; Gardner, n.d.; Goodman-Bacon, 2021; Imai & Kim, 2020; Sun & Abraham, 2021). These problems are particularly severe in cases of staggered treatment design without a never-treated comparison group, but are present even with a never-treated control group if the treatment effect is dynamic (Borusyak & Jaravel, 2017; Sun & Abraham, 2021). There may be ample room for this in our study, as offices had different policies to begin with and because it may take time for a new policy to work.

We therefore estimate event-study models. The standard way to do this is to replace the treatment indicator with a series of dummy variables that indicate the distance in time from the introduction of the activation policy, "event-time" for each group. To avoid potential problems arising from using treated units as control units, we use a specification that is fully saturated in the event-time dimension. To avoid

Table 2. Descriptive statistics 2015-2019 by welfare propensity quartile.

	Q1	Q2	Q3	Q4
Estimated propensity of welfare receipt	0.01	0.02	0.04	0.13
Actual welfare benefit receipt	0.01	0.02	0.04	0.14
Female	0.56	0.52	0.42	0.45
Age	25.1	25.1	25.1	25.1
Foreign born	0.05	0.18	0.21	0.38
Completed high school	0.84	0.75	0.67	0.48
In education	0.57	0.47	0.42	0.39
Work income = 0	0.09	0.10	0.12	0.24
Work income, 1000 €	28.3	28.2	29.2	22.1
Total income, 1000 €	32.4	31.7	32.8	28.1
Tax-exempt transfers, 1000 €	1.5	1.4	1.3	2.3
Temp. or permanent disability benefit receipt	0.04	0.04	0.06	0.10
NEET	0.03	0.05	0.07	0.14
Average income, mother, 1000 € (at child's age 15)	43.7	28.7	24.9	14.2
Average income, father, 1000 € (at child's age 15)	87.7	56.6	48.1	26.8
Welfare receipt, mother (at child's age 15)	0.00	0.00	0.01	0.15
Welfare receipt, father (at child's age 15)	0.01	0.01	0.02	0.13
N observations	173,573	173,577	173,576	173,581

Welfare propensity estimated by regressing welfare receipt at ages 20–29 in the years 2010–2015 on parents' income, education, welfare and disability benefit receipt and marriage status when the person was 15 years old as well as immigrant background and sex. This information is used to predict the probability of welfare receipt for each person for the years 2016–2019, which is further grouped into four quartiles.

compositional effects due to differences in the number of pre- and post-treatment periods among the municipalities, we mainly restrict our attention to estimates that are based on a balanced sample of units (+/-12 months) when reporting and discussing the results.

However, even the fully saturated event-study specification may be misleading if treatment is staggered and the treatment effect is not homogeneous across municipalities, as estimates at one event time may be "contaminated" by heterogeneity at other times (Sun & Abraham, 2021). Sun & Abraham (2021) consider "cohort"-specific dynamic effect paths, where a cohort consists of municipalities that introduce mandatory activation at the same point in time. These paths are then weighted together according to the cohorts' share of the sample at each event time to form an estimate of the average dynamic effect paths. Callaway & Sant'Anna (2021) similarly focus on "group-time" effects, where groups are based on the calendar time period when units are treated, and different ways of aggregating these effects but also allow for the inclusion of time-varying, pre-treatment covariates and provide simultaneous confidence intervals for the dynamic effect path. de Chaisemartin & D'Haultfœuille (2020) also consider treatment effects at the group-time level that are then aggregated. They consider a more general framework with multi-valued treatments that may switch on or off. Another approach is taken by Borusyak *et al.*, 2021, who propose to use never- or not-yet-treated observations to directly impute the missing potential outcomes of treated observations, e.g. by using a two-way fixed effects model. Treatment effects for individual observations can then be aggregated in similar ways as with other estimators.

In the following, we use the estimators proposed by Borusyak *et al.* (2021); de Chaisemartin & DHaultfœuille (2020); Sun & Abraham (2021) as well as the standard pooled OLS event-study to provide estimates of dynamic effects and of average treatment effects.[2] Standard errors are clustered at the municipality level and estimates are weighted by population. In our case, with staggered, absorbing adoption of a binary treatment, no control variables and a never-treated control group, the "interaction-weighted" estimator proposed by Sun & Abraham (2021) gives the same post-treatment estimates as the estimator in Callaway & Sant'Anna (2021).

The event-study approach permits inspection of possible anticipation, phase-in and other dynamic effects and ensures that estimates for the periods just around the reported policy change dates, which may be measured with error, are isolated.

[2]To implement the estimators, we are indebted to the user-written Stata modules de Chaisemartin *et al.* (2019); Sun (2021); Borusyak (2021a,b).

4 Results

4.1 Main results

Figure 4 displays results for social assistance receipt for the high welfare propensity group (Q4). Social assistance, or welfare, is the most immediate outcome, as the reform was targeted explicitly at recipients of social assistance. A corresponding figure with all welfare propensity quartiles is included in the appendix, see Figure A1.

In interpreting the estimates, it is important to note a few differences across the estimators. The pooled OLS event study, the interaction-weighted estimator of Sun & Abraham, 2021 (SA) and the DID_l estimator of de Chaisemartin & D'Haultfœuille, 2020 (dCDH) all use event time -1 as the baseline for post-treatment estimates. Borusyak *et al.*, 2021 (BJS) point out that normalizing effect estimates relative to a specific period assumes away anticipation effects in that period and propose using the whole pre-treatment period as the reference period. This is part of what these authors call a "separation of testing from estimation," where the estimation of treatment effects is always done under assumptions of parallel trends and no anticipation.

There are also some important differences regarding the pre-treatment estimates. Both OLS and SA use event time -1 as the baseline for both pre-treatment and post-treatment estimates. In contrast, dCDH does not have a common baseline for the pre-treatment estimates, but estimates difference-in-differences changes from one period to the next, thus testing parallel trends over consecutive periods. In line with the idea of separating testing from estimation, BJS estimates a separate regression on the non-treated observations, using the same model used for extrapolation expanded with some number of pre-treatment period indicators. Those estimates are thus relative to differences in the period before the pre-treatment indicators start and do not impact the estimated post-treatment effects.

The single-period point estimates are quite similar across estimators and mostly fall in the range of +/-0.2 percentage points, or around +/- 1.5 percent of the welfare benefit receipt level of 14% for this group. The point estimates thus do not indicate an effect of the introduction of mandatory activation on social assistance receipt. OLS closely tracks the other estimates, as would be expected if indeed there is no effect. There is also no evidence of a phase-in effect, which would be problematic for the design.

The OLS, BJS and dCDH confidence intervals are narrow, with most of them covering a range of up to around 0.8 percentage points. For most periods, we can rule

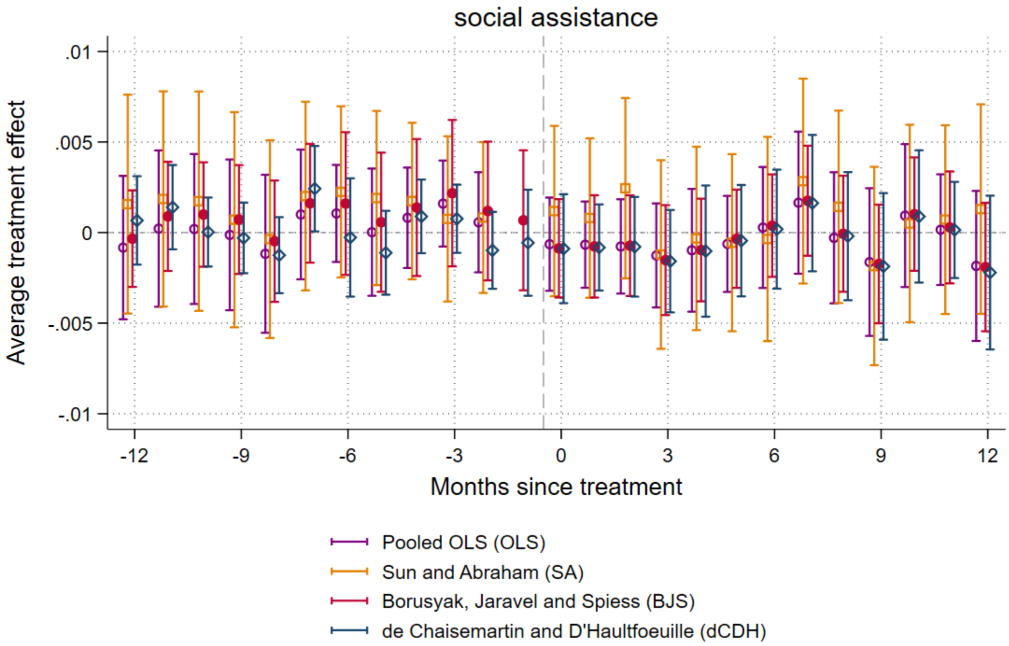


Figure 4. Estimated effects, in months around treatment time 0.

Note: Error bars indicate 95 % confidence interval.

out negative effects up to +/-0.5 percentage points, or around +/-3.5 percent of the level of 14%, at the 5% significance level. The SA estimates have substantially wider error bars than the others. A drawback of the SA estimator is that it does not use the not-yet treated municipalities, which in this instance are quite numerous and large and thus contain much information, as a comparison group. SA is also demanding in the sense that it estimates a full set of pre- and post-treatment coefficients for all treatment introduction points.

In the following, we focus on the group with the highest estimated welfare propensity. Figure 5 displays estimates for the other outcomes. To make the figure easier to read, we restrict our attention to OLS, BJS and dCDH, as the SA estimates are less precise. A full set of estimates can be found in Figure A2 in the appendix.

We do not find clear evidence that the reform had an effect on our outcomes in the 12 months following its introduction. One noticeable and consistent pattern is an increase in use of the qualification program, which offers training and vocational training to those who have been or are currently at risk of long-term dependence on financial assistance. One interpretation of this is that municipalities used the qualification program to fulfill the activation requirement, however it should be noted that the estimates are very small, with confidence intervals covering +/-0.1 percentage points change. There are hints of an effect on being registered in

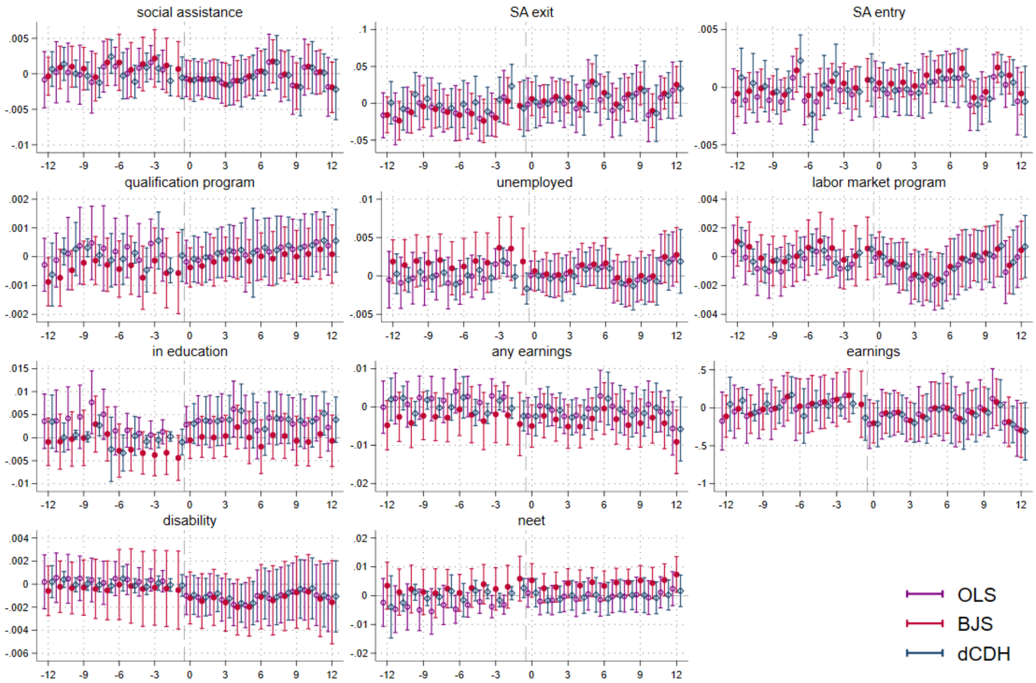


Figure 5. Estimated treatment effects, all outcomes, in months around treatment time 0.

Note: Error bars indicate 95 % confidence interval.

education with the OLS and dCDH estimators, but not with the BJS estimator. Estimates on the other outcomes jump up and down, with no clear change after the implementation of mandatory activation.

4.2 More robust control group

Restriction of comparison group (offices reporting to have introduced the policy before 2016) to only those 37 offices that in 2018 reported that the law change had led only "to a small extent" or "not at all" to more people aged 18-29 with an activation condition. The treatment effects using this set of municipalities are shown in Figure 6. The results are very similar to those obtained using the full set of municipalities.

4.3 Longer horizon

In Figure 7 we follow our outcomes two years after treatment start. For readability, the figure displays only up to six pre-treatment estimates. The longer horizon does not change the conclusion that the reform does not seem to have had an effect on any of our extensive set of outcomes. For some outcomes, there are some suggestive time

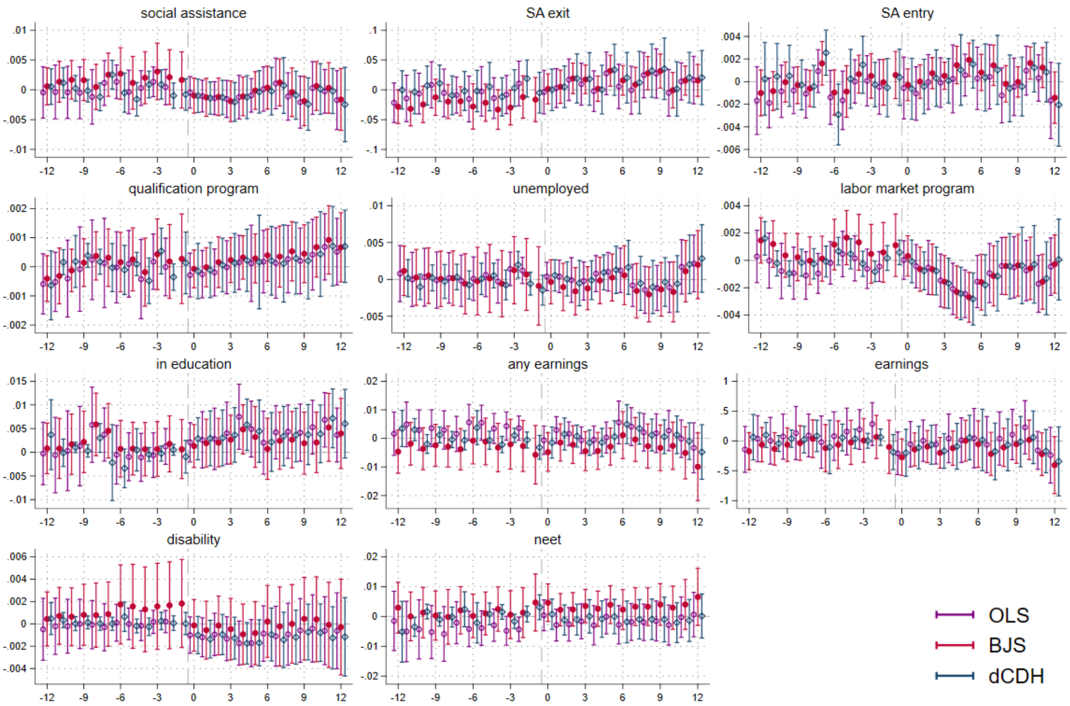


Figure 6. Treatment effects using only municipalities with self-reported small or no change in practice from the law change as control group.

Note: Shaded area indicates 95 % confidence interval. Horizontal lines are point estimate and 95 % confidence interval for the estimated average effect.

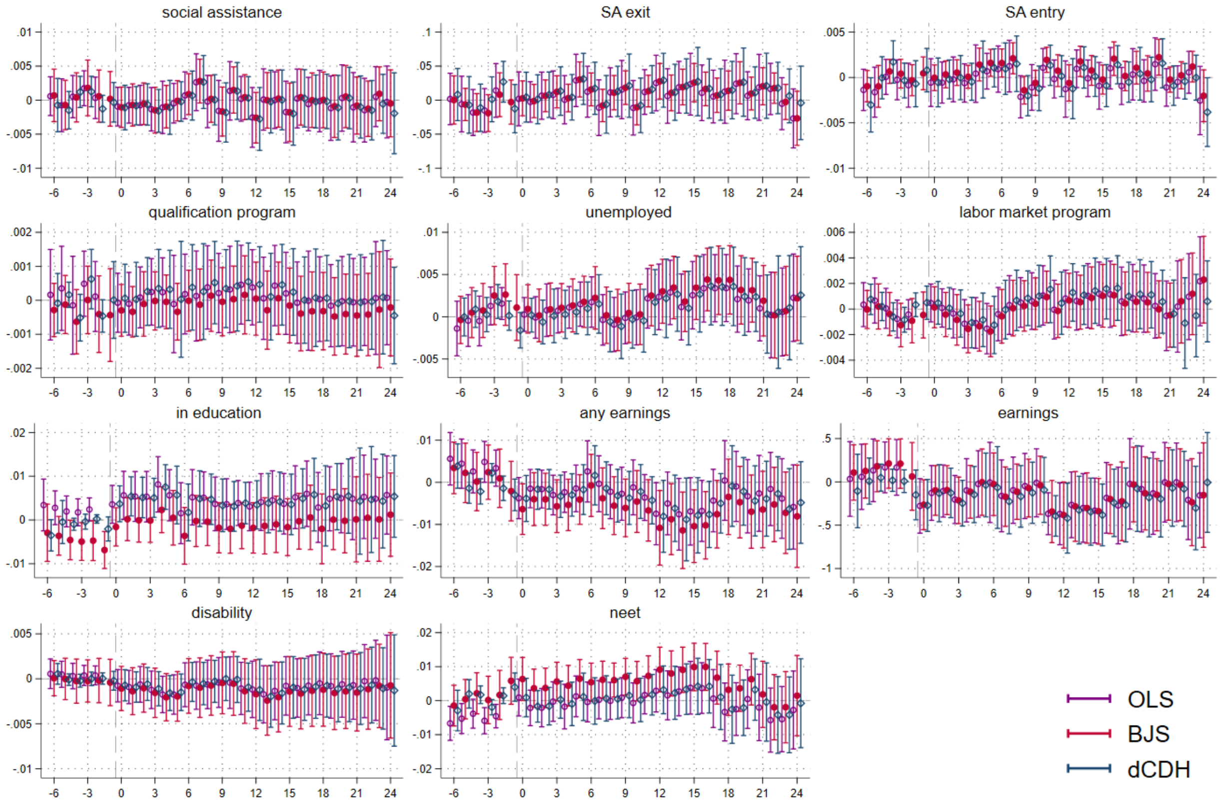


Figure 7. Long-term treatment effects (24 months)

Note: Error bars indicate 95 % confidence interval.

trends in the treatment estimates; however, this is expected to some extent when analyzing many outcomes. In any case, these apparent effects are not statistically significant and fade out with time.

5 Discussion

We do not find evidence that the law change making activation program participation mandatory had an effect on a wide range of outcomes. It is plausible that this has much to do with the fact that setting activation conditions was an option that was extensively used before the reform. Thus, those with the highest expected benefit from such participation had likely already participated, and the reform may primarily have increased the participation of more marginal beneficiaries, who were often a quite long way from self-sufficiency. This interpretation is supported by qualitative interviews with social insurance office employees and welfare recipients in six different offices (Lidén & Trætteberg, 2019). In a similar vein, some of the affected welfare offices may have been only marginal beneficiaries themselves, in the sense that the offices that had not already managed to organize an activation program and were compelled to do so by the new law, may not have organized particularly effective programs. These are important factors to take into account when deciding whether to introduce new reforms.

Given this, it is reassuring to note that we also do not find any negative, unintended consequences, such as an increase in the number of individuals outside employment and education. The clear stipulations in the law about conditions not being overly burdensome, possibilities for exceptions, and limited room for sanctions may also have contributed to the law change having relatively little impact in practice.

If welfare recipients who were exposed to activation program participation as a consequence of the law change had a low expected benefit from participating, this may help explain why the results are different from the positive experiences from earlier periods. At that point, the use of conditions increased from a lower level and those who were affected by changes in that period may have had a larger potential to succeed in work or education. The labor market for people with few qualifications is probably also much more competitive today.

It is worth noting that the obligation of municipalities to require participation in activation programs may serve a useful function despite the fact that this article did not identify a substantial or significant effect of this reform. Indeed, even though the reform put more obligations on the social insurance offices where they previously had discretion, most case workers reported being satisfied with it Terum *et al.* (2017); Dahl & Lima (2018); Lidén & Trætteberg (2019).

References

Athey, Susan, & Imbens, Guido W. 2021. Design-based analysis in difference-in-differences settings with staggered adoption. *Journal of Econometrics*.

Avram, Silvia, Brewer, Mike, & Salvatori, Andrea. 2018. Can't work or won't work: Quasi-experimental evidence on work search requirements for single parents. *Labour Economics*, **51**, 63–85.

Borusyak, K, Jaravel, X, & Spiess, J. 2021. *Revisiting Event Study Designs: Robust and Efficient Estimation*. Tech. rept. Working Paper.

Borusyak, Kirill. 2021a. DID_IMPUTATION: Stata module to perform treatment effect estimation and pre-trend testing in event studies.

Borusyak, Kirill. 2021b. EVENT_PLOT: Stata module to plot the staggered-adoption diff-in-diff (" event study") estimates.

Borusyak, Kirill, & Jaravel, Xavier. 2017. Revisiting event study designs. *Available at SSRN 2826228.*

Brandtzæg, Bent Aslak, Flermoen, Solveig, Lunder, Trond Erik, Løyland, Knut, Møller, Geir, & Sannes, Joar. 2006. Fastsetting av satser, utmåling av økonomisk sosialhjelp og vilkårsbruk i sosialtjenesten.

Callaway, Brantly, & Sant'Anna, Pedro H.C. 2021. Difference-in-Differences with multiple time periods. *Journal of Econometrics*, **225**(2), 200–230. Themed Issue: Treatment Effect 1.

Cammeraat, Emile, Jongen, Egbert, & Koning, P.W.C. 2017. *Preventing NEETs During the Great Recession: The Effect of a Mandatory Activation Program for Young Welfare Recipients.* WorkingPaper. IZA Institute for the Study of Labor.

Card, David, Kluve, Jochen, & Weber, Andrea. 2010. Active Labour Market Policy Evaluations: A Meta-Analysis. *The Economic Journal*, **120**, F452–F477.

Card, David, Kluve, Jochen, & Weber, Andrea. 2015. What Works? A Meta Analysis of Recent Active Labor Market Program Evaluations. *IZA Discussion Paper Series*, July.

Dahl, Espen Steinung, & Lima, Ivar. 2018. Nav-kontorenes erfaringer med aktivitetsplikt for unge sosialhjelpsmottakere. *Arbeid og velferd*, **4**(2018), 19–35.

Dahl, Espen Steinung, & Lima, Ivar Andreas Åsland. 2016. Krav om å stå opp om morra'n: virker det? *Arbeid og Velferd*, 115–128.

Dahl, Espen Steinung, & Lima, Ivar Andreas Åsland. 2017. Vilkår om aktivitet for sosiahjelp i 2015: Gjør kommunene det som virker? *Arbeid og Velferd*, 105–121.

Dahlberg, Matz, Johansson, Kajsa, & Mörk, Eva. 2009. On Mandatory Activation of Welfare Recipients. *IZA Discussion Paper Series*, January.

de Chaisemartin, Clément, & D'Haultfœuille, Xavier. 2020. Difference-in-Differences Estimators of Intertemporal Treatment Effects. *Available at SSRN 3731856.*

de Chaisemartin, Clèment, & DHaultfœuille, Xavier. 2020. Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review*, **110**(9), 2964–96.

de Chaisemartin, Clément, D'Haultfoeuille, Xavier, & Guyonvarch, Yannick. 2019. DID_MULTIPLEGT: Stata module to estimate sharp Difference-in-Difference designs with multiple groups and periods.

Gardner, John. *Two-stage differences in differences.* Tech. rept. Working Paper.

Goodman-Bacon, Andrew. 2021. *Difference-in-differences with variation in treatment timing.* Tech. rept. 2. Themed Issue: Treatment Effect 1.

Hernæs, Øystein, Markussen, Simen, & Røed, Knut. 2017. Can Welfare Conditionality Combat High School Dropout? *Labour Economics*, **48**(October), 144–156.

Hernæs, Øystein M. 2021. *Delrapport 2: Kvantitativ evaluering av innføring av plikt for kommunene til å stille vilkår om aktivitet til sosialhjelpsmottakere under 30 år.*

Imai, Kosuke, & Kim, In Song. 2020. On the use of two-way fixed effects regression models for causal inference with panel data. *Political Analysis*, 1–11.

Lidén, Hilde, & Trætteberg, Håkon Solbu. 2019. Aktivitetsplikt for unge mottakere av sosialhjelp: Delrapport 1. *Rapport–Institutt for samfunnsforskning.*

Manning, Alan. 2009. You Can't Always Get What You Want: The Impact of the UK Jobseeker's Allowance. *Labour Economics*, **16**(3), 239–250.

Petrongolo, Barbra. 2009. The Long-term Effects of Job Search Requirements: Evidence From the UK JSA Reform. *Journal of Public Economics*, **93**, 1234–1253.

Proba. 2015. *Kommunenes praksis for bruk av vilkår ved tildeling av økonomisk sosialhjelp.* Tech. rept. 2015-09. Proba.

Sun, Liyang. 2021. eventstudyinteract: Stata module: interaction weighted estimator for event study.

Sun, Liyang, & Abraham, Sarah. 2021. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, **225**(2), 175–199. Themed Issue: Treatment Effect 1.

Terum, Lars Inge, Torsvik, Gaute, & Øverbye, Einar. 2017. Når vilkår og aktivitetskrav brytes. Frontlinjearbeideres tilnærming til sanksjoner. *Søkelys på arbeidslivet*, **34**(03), 147–166.

van den Berg, Gerard J., Uhlendorff, Arne, & Wolff, Joachim. 2020 (July). *The impact of sanctions for young welfare recipients on transitions to work and wages and on dropping out.* Working Paper Series DP15037. IFAU - Institute for Evaluation of Labour Market and Education Policy.

A Propensity scores

Propensity scores are shown in table A1.

Table A1. Propensity scores by age.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	20	21	22	23	24	25	26	27	28	29
Father's income (log)	-0.152	-0.157	-0.171	-0.171	-0.163	-0.160	-0.148	-0.142	-0.149	-0.145
	(-18.80)	(-19.76)	(-21.37)	(-20.96)	(-19.51)	(-18.69)	(-16.86)	(-15.88)	(-16.43)	(-15.96)
Father's income missing	-0.204	-0.250	-0.333	-0.382	-0.350	-0.295	-0.244	-0.281	-0.264	-0.215
	(-4.81)	(-5.99)	(-7.87)	(-8.86)	(-8.01)	(-6.69)	(-5.46)	(-6.18)	(-5.80)	(-4.73)
Mother's income (log)	-0.157	-0.146	-0.140	-0.136	-0.138	-0.130	-0.130	-0.120	-0.0995	-0.0959
	(-23.56)	(-22.20)	(-20.96)	(-19.81)	(-19.56)	(-17.83)	(-17.37)	(-15.56)	(-12.46)	(-11.88)
Mother's income missing	-0.0677	-0.127	-0.103	-0.101	-0.112	-0.135	-0.160	-0.218	-0.151	-0.145
	(-1.94)	(-3.65)	(-2.89)	(-2.77)	(-2.96)	(-3.47)	(-3.96)	(-5.18)	(-3.51)	(-3.31)
Father's education										
No education (base)										
Primary	0.255	0.213	0.237	0.130	0.353	0.224	0.209	0.243	0.218	0.233
	(2.24)	(1.88)	(2.08)	(1.10)	(2.81)	(1.79)	(1.61)	(1.75)	(1.48)	(1.45)
Lower secondary	0.448	0.464	0.358	0.290	0.428	0.207	0.0942	0.106	0.104	0.254
	(4.62)	(4.88)	(3.78)	(2.98)	(4.09)	(2.03)	(0.89)	(0.94)	(0.89)	(1.99)
Upper secondary, basic	0.151	0.167	0.00316	-0.0457	0.0570	-0.192	-0.311	-0.257	-0.251	-0.103
	(1.53)	(1.72)	(0.03)	(-0.46)	(0.54)	(-1.84)	(-2.89)	(-2.24)	(-2.11)	(-0.79)
Upper secondary, final year	0.0457	0.0884	-0.00720	-0.0642	0.0951	-0.158	-0.257	-0.215	-0.201	-0.0166
	(0.47)	(0.92)	(-0.08)	(-0.66)	(0.90)	(-1.54)	(-2.41)	(-1.89)	(-1.70)	(-0.13)
Post-secondary non-tertiary	0.00655	0.0746	-0.0910	-0.126	0.0256	-0.249	-0.306	-0.293	-0.254	-0.168
	(0.06)	(0.74)	(-0.90)	(-1.20)	(0.23)	(-2.26)	(-2.68)	(-2.41)	(-2.01)	(-1.23)
First stage of tertiary,	-0.330	-0.218	-0.314	-0.367	-0.242	-0.409	-0.469	-0.506	-0.449	-0.342
undergraduate	(-3.33)	(-2.24)	(-3.24)	(-3.69)	(-2.27)	(-3.91)	(-4.33)	(-4.37)	(-3.73)	(-2.61)
First stage of tertiary,	-0.683	-0.536	-0.631	-0.633	-0.507	-0.685	-0.828	-0.836	-0.818	-0.648
graduate	(-6.27)	(-5.08)	(-6.00)	(-5.88)	(-4.42)	(-6.07)	(-7.06)	(-6.69)	(-6.29)	(-4.63)
Second stage of tertiary	-0.948	-0.607	-0.676	-0.845	-0.536	-0.828	-1.107	-0.949	-1.457	-0.792
	(-4.93)	(-3.57)	(-4.01)	(-4.71)	(-3.02)	(-4.50)	(-5.49)	(-4.69)	(-5.77)	(-3.59)
Unspecified	0.231	0.197	0.104	0.0913	0.237	0.0149	-0.0350	0.0398	-0.0137	0.159
	(2.30)	(1.99)	(1.05)	(0.90)	(2.18)	(0.14)	(-0.32)	(0.34)	(-0.11)	(1.21)
Mother's education										
No education (base)										
Primary	0.174	0.238	0.169	0.191	0.261	0.0929	0.310	0.269	0.283	0.293
	(1.95)	(2.73)	(1.87)	(2.07)	(2.74)	(0.96)	(3.08)	(2.52)	(2.51)	(2.55)
Lower secondary	0.440	0.412	0.517	0.471	0.486	0.351	0.497	0.454	0.447	0.326
	(6.07)	(5.81)	(7.14)	(6.44)	(6.38)	(4.61)	(6.19)	(5.42)	(5.09)	(3.65)
Upper secondary, basic	0.0586	0.0262	0.144	0.101	0.159	-0.0281	0.103	0.0338	0.0216	-0.0935
	(0.78)	(0.35)	(1.91)	(1.32)	(2.01)	(-0.35)	(1.23)	(0.39)	(0.24)	(-1.02)
Upper secondary, final year	0.0428	0.0349	0.186	0.146	0.184	0.0408	0.178	0.135	0.141	0.0676
	(0.58)	(0.48)	(2.52)	(1.96)	(2.36)	(0.53)	(2.17)	(1.58)	(1.56)	(0.74)
Post-secondary non-tertiary	-0.234	-0.204	-0.0287	-0.0565	0.169	-0.0505	0.118	0.0409	0.104	-0.115
	(-2.56)	(-2.31)	(-0.32)	(-0.62)	(1.83)	(-0.53)	(1.19)	(0.39)	(0.96)	(-1.03)
First stage of tertiary,	-0.403	-0.411	-0.183	-0.223	-0.158	-0.241	-0.126	-0.141	-0.185	-0.266
undergraduate	(-5.35)	(-5.59)	(-2.44)	(-2.94)	(-1.99)	(-3.04)	(-1.50)	(-1.61)	(-2.02)	(-2.86)
First stage of tertiary,	-0.811	-0.688	-0.456	-0.407	-0.324	-0.474	-0.222	-0.370	-0.311	-0.565
graduate	(-7.85)	(-7.07)	(-4.63)	(-4.08)	(-3.13)	(-4.48)	(-2.02)	(-3.13)	(-2.55)	(-4.39)
Second stage of tertiary	-1.016	-1.007	-1.007	-0.754	-0.449	-0.641	-0.608	-0.456	-1.183	-0.455
	(-3.50)	(-3.71)	(-3.34)	(-2.68)	(-1.74)	(-2.27)	(-1.92)	(-1.49)	(-2.57)	(-1.42)
Unspecified	0.341	0.335	0.382	0.287	0.256	0.141	0.308	0.342	0.416	0.218
	(4.56)	(4.57)	(5.09)	(3.76)	(3.21)	(1.76)	(3.68)	(3.92)	(4.55)	(2.34)
N observations	394 521	395 895	397 288	396 899	396 111	394 515	392 972	392 533	393 125	394 611

t-statistics in parenthesis.

Table A1. Propensity scores by age (cont'd).

	(1) 20	(2) 21	(3) 22	(4) 23	(5) 24	(6) 25	(7) 26	(8) 27	(9) 28	(10) 29
Father unmarried (base)										
Father married	-0.340	-0.329	-0.312	-0.343	-0.322	-0.364	-0.333	-0.342	-0.329	-0.269
	(-17.18)	(-16.98)	(-15.85)	(-16.80)	(-15.22)	(-16.55)	(-14.43)	(-14.17)	(-13.18)	(-10.46)
Father married (missing)	-0.150	-0.157	-0.215	-0.209	-0.185	-0.255	-0.278	-0.265	-0.231	-0.198
	(-4.32)	(-4.65)	(-6.33)	(-6.09)	(-5.36)	(-7.18)	(-7.65)	(-7.24)	(-6.31)	(-5.42)
Mother unmarried (base)										
Mother married	-0.400	-0.405	-0.388	-0.354	-0.365	-0.333	-0.360	-0.362	-0.371	-0.382
	(-20.25)	(-20.96)	(-19.76)	(-17.52)	(-17.47)	(-15.38)	(-15.94)	(-15.38)	(-15.31)	(-15.31)
Mother married (missing)	0.103	0.0642	-0.0128	-0.0877	-0.170	-0.202	-0.268	-0.269	-0.273	-0.191
	(2.65)	(1.69)	(-0.33)	(-2.19)	(-4.12)	(-4.80)	(-6.18)	(-6.06)	(-6.05)	(-4.23)
Father's welfare receipt at child aged:										
10	0.122	0.163	0.101	0.0870	0.120	0.160	0.108	0.236	0.222	0.0975
	(3.73)	(5.04)	(3.05)	(2.56)	(3.41)	(4.44)	(2.89)	(6.22)	(5.70)	(2.44)
11	0.0844	0.000120	-0.00672	0.0505	0.0446	-0.0133	0.108	0.0590	0.0696	0.130
	(2.28)	(0.00)	(-0.18)	(1.32)	(1.12)	(-0.32)	(2.55)	(1.35)	(1.57)	(2.97)
12	0.0886	0.0824	0.130	0.0171	0.0385	0.0981	0.0544	0.0963	0.147	0.138
	(2.35)	(2.21)	(3.45)	(0.44)	(0.95)	(2.34)	(1.25)	(2.16)	(3.22)	(2.96)
13	0.0866	0.136	0.0819	0.184	0.127	0.0769	0.122	0.0689	-0.00260	0.0558
	(2.28)	(3.62)	(2.15)	(4.72)	(3.15)	(1.83)	(2.80)	(1.51)	(-0.05)	(1.16)
14	0.131	0.0970	0.144	0.118	0.106	0.0899	0.0120	0.0353	0.0647	0.0418
	(3.41)	(2.56)	(3.74)	(3.00)	(2.62)	(2.14)	(0.27)	(0.77)	(1.36)	(0.86)
15	0.259	0.307	0.244	0.304	0.300	0.284	0.316	0.249	0.183	0.213
	(7.52)	(9.04)	(7.07)	(8.61)	(8.28)	(7.63)	(8.17)	(6.16)	(4.31)	(4.87)
Mother's welfare receipt at child aged:										
10	0.389	0.330	0.358	0.359	0.379	0.352	0.295	0.240	0.236	0.207
	(13.77)	(11.63)	(12.35)	(11.96)	(12.20)	(10.84)	(8.73)	(6.85)	(6.61)	(5.62)
11	0.173	0.193	0.190	0.184	0.103	0.136	0.177	0.217	0.271	0.205
	(5.44)	(6.13)	(5.90)	(5.56)	(2.99)	(3.77)	(4.72)	(5.58)	(6.85)	(5.17)
12	0.108	0.103	0.0162	0.0733	0.0821	0.0361	0.103	0.0936	0.0858	0.114
	(3.31)	(3.18)	(0.49)	(2.15)	(2.32)	(0.98)	(2.68)	(2.35)	(2.11)	(2.75)
13	0.166	0.153	0.206	0.127	0.104	0.123	0.125	0.146	0.0989	0.192
	(5.02)	(4.69)	(6.23)	(3.71)	(2.91)	(3.32)	(3.24)	(3.64)	(2.39)	(4.56)
14	0.162	0.217	0.208	0.152	0.192	0.205	0.208	0.148	0.215	0.170
	(4.84)	(6.56)	(6.20)	(4.38)	(5.39)	(5.56)	(5.43)	(3.68)	(5.21)	(4.01)
15	0.339	0.286	0.314	0.332	0.324	0.309	0.249	0.266	0.275	0.312
	(11.31)	(9.57)	(10.31)	(10.60)	(10.10)	(9.32)	(7.21)	(7.39)	(7.43)	(8.21)
Father disabled	0.107	0.0359	-0.0340	-0.0736	-0.0741	-0.0835	-0.0699	-0.0966	-0.177	-0.211
	(3.64)	(1.22)	(-1.12)	(-2.35)	(-2.30)	(-2.50)	(-2.01)	(-2.65)	(-4.62)	(-5.27)
Mother disabled	0.107	0.0787	0.0585	0.0463	0.0191	-0.0114	0.00387	-0.00176	0.00414	0.00383
	(3.91)	(2.90)	(2.14)	(1.64)	(0.65)	(-0.38)	(0.12)	(-0.05)	(0.12)	(0.11)
Immigration category										
Born in Norway to Norwegian-born parents (base)										
Immigrants	-0.443	-0.474	-0.448	-0.478	-0.450	-0.459	-0.516	-0.555	-0.705	-0.778
	(-8.72)	(-9.56)	(-8.87)	(-9.18)	(-8.38)	(-8.23)	(-8.92)	(-9.12)	(-11.14)	(-11.79)
Norwegian-born to	-1.035	-1.107	-1.171	-1.112	-1.089	-1.191	-1.143	-1.171	-1.324	-1.407
immigrant parents	(-18.70)	(-20.08)	(-20.14)	(-18.16)	(-16.51)	(-16.48)	(-15.04)	(-14.18)	(-14.80)	(-14.78)
Foreign-born with one	-0.181	-0.0319	-0.0230	0.00803	0.0568	0.0678	0.0411	0.00155	-0.0276	0.0873
Norwegian-born parent	(-2.54)	(-0.47)	(-0.33)	(0.11)	(0.74)	(0.83)	(0.47)	(0.02)	(-0.29)	(0.91)
Norwegian-born with one	-0.152	-0.200	-0.289	-0.292	-0.242	-0.271	-0.350	-0.214	-0.307	-0.229
foreign-born parent	(-3.62)	(-4.86)	(-6.74)	(-6.57)	(-5.23)	(-5.50)	(-6.71)	(-3.93)	(-5.35)	(-3.87)
Foreign-born to	0.252	0.311	0.328	0.241	0.359	0.301	0.359	0.334	0.171	0.169
Norwegian-born parents	(3.40)	(4.52)	(4.81)	(3.37)	(4.95)	(3.87)	(4.54)	(3.99)	(1.95)	(1.89)
Immigration background										
Norwegian										
Western Europe	-0.0238	-0.0312	-0.0721	-0.0445	-0.0656	-0.0940	-0.0461	-0.0825	-0.0520	-0.0753
	(-0.74)	(-0.98)	(-2.20)	(-1.31)	(-1.83)	(-2.48)	(-1.17)	(-1.96)	(-1.20)	(-1.65)
Eastern and South-Eastern	0.0152	-0.0218	-0.148	-0.262	-0.405	-0.517	-0.544	-0.524	-0.543	-0.461
Europe	(0.31)	(-0.46)	(-3.03)	(-5.19)	(-7.77)	(-9.58)	(-9.85)	(-9.27)	(-9.35)	(-7.83)
Africa	0.905	1.238	1.486	1.667	1.816	1.920	2.026	2.132	2.239	2.259
	(20.19)	(28.71)	(34.16)	(37.68)	(40.17)	(41.27)	(42.34)	(42.78)	(43.80)	(42.71)
Asia	0.404	0.540	0.591	0.581	0.542	0.566	0.576	0.575	0.701	0.712
	(9.52)	(13.07)	(13.95)	(13.23)	(11.89)	(11.93)	(11.75)	(11.21)	(13.36)	(13.12)
North-America	-0.0779	-0.0753	-0.00547	-0.0457	-0.00279	-0.158	-0.0493	-0.0356	-0.0821	-0.108
	(-1.32)	(-1.31)	(-0.09)	(-0.76)	(-0.05)	(-2.41)	(-0.75)	(-0.53)	(-1.17)	(-1.49)
South-America	0.646	0.724	0.788	0.839	0.794	0.739	0.863	0.829	0.888	0.761
	(9.56)	(11.04)	(12.06)	(12.53)	(11.58)	(10.22)	(12.00)	(11.07)	(11.62)	(9.65)
Oceania	-0.178	-0.00538	-0.218	-0.423	-0.402	-0.416	-0.928	-0.592	-1.042	-0.876
	(-0.87)	(-0.03)	(-1.00)	(-1.72)	(-1.64)	(-1.68)	(-2.99)	(-2.27)	(-3.21)	(-3.06)
N observations	394 521	395 895	397 288	396 899	396 111	394 515	392 972	392 533	393 125	394 611

t-statistics in parenthesis.

Table A1. Propensity scores by age (cont'd).

	(1) 20	(2) 21	(3) 22	(4) 23	(5) 24	(6) 25	(7) 26	(8) 27	(9) 28	(10) 29
Women	-0.151	-0.196	-0.209	-0.218	-0.201	-0.235	-0.221	-0.258	-0.238	-0.251
	(-10.98)	(-14.58)	(-15.35)	(-15.60)	(-14.09)	(-16.04)	(-14.76)	(-16.86)	(-15.37)	(-16.05)
Year										
2010 (base)										
2011	-0.0861	-0.0507	-0.0638	-0.0668	-0.0195	-0.0327	-0.0193	-0.0198	-0.0539	-0.0554
	(-3.70)	(-2.20)	(-2.67)	(-2.69)	(-0.75)	(-1.25)	(-0.72)	(-0.73)	(-1.96)	(-1.96)
2012	-0.107	-0.126	-0.0690	-0.119	-0.0717	-0.115	-0.0807	-0.0589	-0.0876	-0.125
	(-4.60)	(-5.39)	(-2.92)	(-4.79)	(-2.79)	(-4.39)	(-3.00)	(-2.17)	(-3.19)	(-4.36)
2013	-0.141	-0.0772	-0.0531	-0.0270	-0.0101	-0.0570	-0.0683	-0.0215	-0.0161	-0.0588
	(-6.01)	(-3.34)	(-2.25)	(-1.11)	(-0.40)	(-2.23)	(-2.58)	(-0.80)	(-0.60)	(-2.10)
2014	-0.130	-0.0579	-0.0174	-0.0160	0.0520	-0.0101	-0.00974	-0.0323	0.00750	-0.0250
	(-5.52)	(-2.51)	(-0.74)	(-0.66)	(2.10)	(-0.40)	(-0.38)	(-1.21)	(0.28)	(-0.90)
2015	-0.181	-0.114	-0.0476	-0.0266	0.0242	-0.0234	-0.00841	-0.0135	-0.0238	-0.0190
	(-7.65)	(-4.86)	(-2.02)	(-1.09)	(0.97)	(-0.93)	(-0.33)	(-0.52)	(-0.89)	(-0.69)
Constant	-1.399	-1.404	-1.458	-1.444	-1.755	-1.414	-1.584	-1.659	-1.753	-1.857
	(-11.44)	(-11.75)	(-12.20)	(-11.88)	(-13.60)	(-11.25)	(-12.13)	(-12.08)	(-12.36)	(-12.38)
N observations	394 521	395 895	397 288	396 899	396 111	394 515	392 972	392 533	393 125	394 611

t-statistics in parenthesis.

Treatment effects for social assistance

Treatment effects for social assistance receipt for all welfare propensity groups (Figure A1).

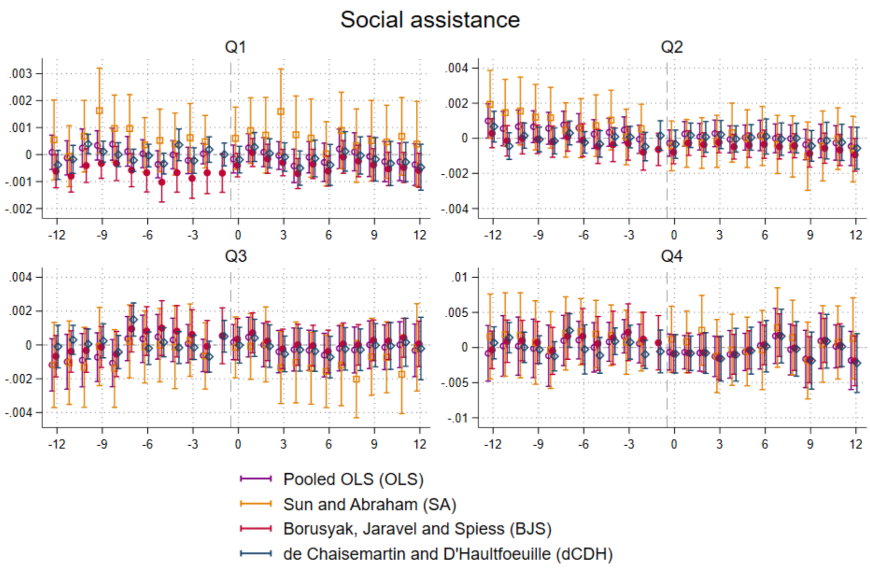


Figure A1. Treatment effects for social assistance for all welfare propensity groups.

Note: Error bars indicate 95 % confidence interval.

Estimates with Sun-Abrahams-estimator included (Figure A2).

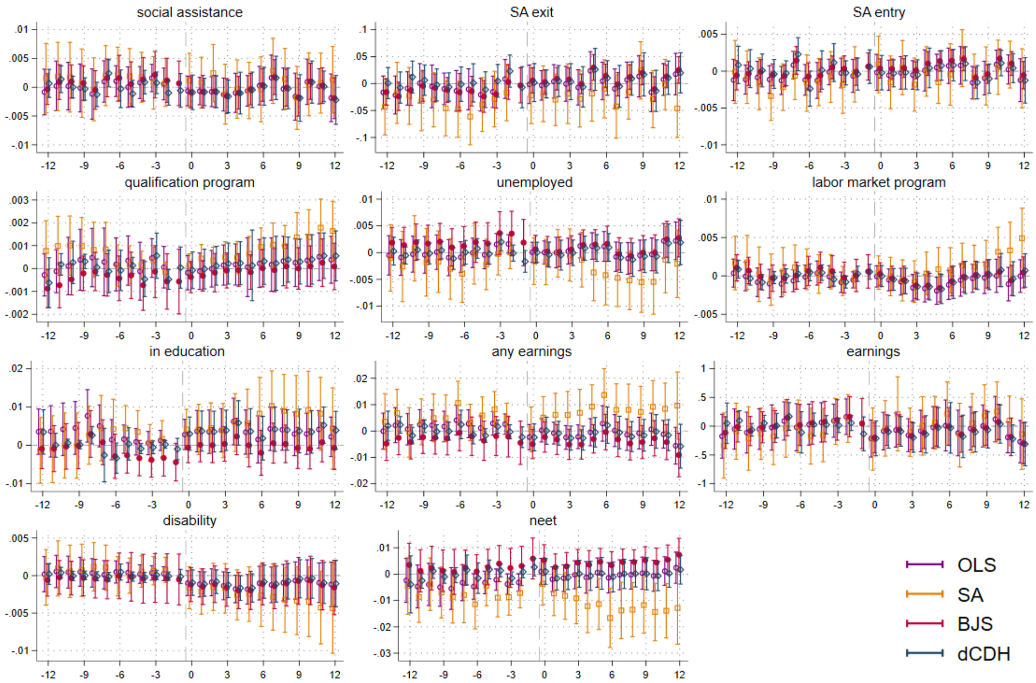


Figure A2. Estimated treatment effects, all outcomes, in months around treatment time 0.

Note: Error bars indicate 95 % confidence interval.