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Distributional effects of welfare reform for young adults: An unconditional quantile regression approach ☆

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A B S T R A C T

The paper evaluates the distributional effects on earnings and income of requiring young welfare recipients to fulfill conditions related to work and activation. It exploits within-social insurance office variation in policy arising from a geographically staggered reform in Norway. The reform reduced welfare uptake and for women had large, positive effects in the lower part of the earnings distribution. The effect on the distribution of total income is also positive, thus gains in earnings more than offset reduced welfare benefits. Fewer welfare payments and smaller caseloads make the policy highly cost-effective.

1. Introduction

Many countries have implemented or tested mandatory welfare programs for young adults to handle moral hazard problems while also providing social insurance.[1] Activation programs may reduce the incidence and duration of benefit claims. However, such strategies may have negative distributional effects if individuals who are already in a difficult financial situation lose what little income they have. Recent evidence of this can be found in the case of an activation program for young welfare recipients in the Netherlands (Cammeraat et al., 2017). Negative distributional effects might be a problem even in the case of a positive average earnings response. For example, Avram et al. (2018) found that job search requirements for single parents on welfare in the UK led to more people finding work but also increased transitions to non-claimant unemployment or health-related benefits.

In the 1990s and 2000s, social insurance offices in Norwegian municipalities increased the number of conditions that young welfare recipients had to comply with to receive benefits. These requirements could take many forms, but for young recipients they were primarily related to activation and work. This paper studies the distributional effects on earnings and total income of this tightening of policy, exploiting the fact that different social insurance offices changed their policies at different points in time. The paper is related to the works of Hernæs et al. (2017) and Bratsberg et al. (2019), who studied the effect of the same sequence of policy changes on youths. Both these studies exploited the geographically staggered implementation of increased conditions using various double and triple differences designs. Hernæs et al. (2017) found that the increased use of conditions increased the high school completion rate for 21 year olds, while Bratsberg et al. (2019) found that it reduced crime among 18–19 year

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[1] See Blundell et al. (2004), Dahlberg et al. (2009), Persson and Vikman (2014) for mandatory activation programs targeted specifically towards young adults, and Black et al. (2003), Graversen and van Ours (2008), Tuomala 2011) and Maibom et al. (2017) for more general mandatory activation programs.

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old boys. The present paper exploits the same source of variation in policy but studies labor market outcomes and people who have entered their prime working lives.

Welfare policy affects both those actually receiving welfare and a wider population with only a potential connection to the welfare system, which might still be impacted through a "threat effect" (Black et al., (2003); Koning, (2015)) or a general "regime effect" (Arni et al., 2015). The main contribution of this paper is estimating quantile treatment effects that cover both these groups, as well as spillover effects, by estimating unconditional effects and using data on the complete population.

The paper analyzes effects on the unconditional distribution of earnings and income for men and women in a Difference-in-Differences (DiD)-related design. The estimation follows the recentered influence regression (RIF) approach of Firpo et al. (2009) and focuses on the reduced form effects on all individuals aged 26–30 residing in the treatment areas. As in a regular DiD application, the analysis is essentially a comparison of the change in outcomes from before to after increased conditionality with the change that occurred in the same period in places in which there was no change in policy. For mean effects, these pre vs. post and treatment vs. control comparisons are made once; in contrast, when the outcome of interest is the distribution, the comparisons are made several times. Following the RIF approach, this takes the form of a series of analyses of how earnings are distributed around a set of thresholds corresponding to the quantiles of the empirical earnings distribution, resulting in estimates of treatment effects along the distribution.[2] Other applications of the RIF method for policy evaluation using repeated cross-sectional data are Havnes and Mogstad (2015) on child care and Dube (2018) on minimum wages.

I find substantial positive effects of increased use of conditions in parts of the lower end of the earnings distribution for women and no or small negative effects for men. For women, earnings at the 20th percentile increase by around 25 percent, or € 2000 per year. As expected, there are no effects in the upper part of the distribution. Further, I find that although welfare payments decline, the effect on total income for women is also positive, indicating that they were able to find gainful employment that, overall, improved their financial circumstances. In addition to reducing welfare payments, the results from data on operating expenses at the office level suggest that the reduced caseload from the reform more than made up for the increased workload; thus, the reform was highly cost-effective.

Since activation and requirements related to obtaining work are the main tools of social insurance offices for managing young claimants, the paper relates to the literature on active labor market policies (for reviews, see Card et al., 2010, 2015; Kluve et al., 2016). This literature has focused on program participants and has largely analyzed mean effects. Autor et al. (2017) recently evaluated distributional effects on the earnings distribution of a welfare-to-work program in Detroit. The authors also studied program participants and analyzed effects on the conditional earnings distribution, as in traditional quantile regression. The current paper also relates to the literature on US welfare reform that evaluated distributional effects and was part of the debate about whether income and/or consumption declined for a subset of individuals targeted by the reform (Blank and Schoeni, 2003; Bitler et al., 2006; Meyer and Sullivan, 2008). As the US welfare reform mostly concerned single mothers receiving cash benefits, individuals outside the program or covered by other programs have received little attention. I find evidence that the effects on welfare recipients are driving the results, supporting the focus of this stream of literature on program participants.

The rest of the paper is organized as follows. Section 2 describes the institutional setting and the reform, Section 3 presents the empirical strategy, Section 4 contains results and discussion, and Section 5 concludes the paper.

[2] Studying effects on the overall (unconditional) distribution contrasts with traditional quantile regression, which estimates effects on each quantile conditional on the control variables (Koenker and Bassett, 1978).

2. Welfare reform in Norway

2.1. Institutional setting

In most Norwegian social insurance programs, individuals can only claim benefits if they have earned the right to do so based on previous social security contributions (e.g., unemployment benefits) or have gone through a lengthy bureaucratic process (e.g., disability benefits). People not covered by these programs and unable to support themselves have the right to means-tested social assistance ("welfare") from their local social insurance office to cover basic needs, such as food and housing. This system has always been administered and financed at the municipality level. Consequently, the local social insurance offices have had a large degree of autonomy in determining the implementation. One important aspect has been conditions the welfare claimant must comply with in order to receive the benefit, particularly requirements to participate in workfare programs. Norwegian conditionality and workfare programs have more in common with the US system than with other European systems (Gubrium et al., 2014), but the wide autonomy of local offices is a trait not found in the US system. Because of the decentralized nature of the Norwegian system, the local social insurance offices have had a large degree of discretion in their use of conditions, subject to the legal stipulation that the conditions should not be disproportionally burdensome or unreasonable as well as to guidelines and interpretations in an accompanying governmental circular provided by the government.

There has been an ongoing discussion in Norway about whether parts of the welfare system are too lenient, and in the late 1990s and early 2000s, many social insurance offices increased their use of conditions for welfare programs. To get an overview of the variation in policy around the country, in 2005 the Norwegian Directorate for Health and Social Affairs tasked Telemark Research Institute (TRI) with writing a report on the Norwegian system of means-tested social assistance (Brandtzæg et al., 2006). As part of this work, TRI administered a survey to the country's 470 local social insurance offices. In this paper, I use information from the part of the survey that concerned the use of conditions for welfare. The survey, administered in 2005, asked whether there had been any changes in the office's use of conditions for welfare during the period 1994–2004, and if so, when they had occurred. Consequently, nine specific conditions were listed (see Table 1). This list of conditions was based on examples of possible conditions from the circular. Thus, the respondents reported on generally accepted types of conditions and had no incentive to misrepresent their policies. For each of these conditions, a reply indicating more use, less use, or no change was recorded.

Of the 470 offices, 223 did not reply. Of the 247 replies, 33 were discarded due to missing or inconsistent information regarding the timing of policy changes. Further, for seven offices there was no link between individuals and offices because multiple offices are operating in the largest municipalities while residency data are available only at the municipality level. This is the case for the two largest cities, Oslo and Bergen, which are thus excluded from the analysis. The requirement that individuals, for whom residency is available only at the municipality level, be unambiguously linked to a social insurance office implies that there is a 1:1 correspondence between social insurance office areas and municipalities for the estimation sample. There was a clear move towards more use of conditions. 43 of the offices reported more use of at least one type of condition and reduced use of none, while six reported a mix of more and less use. To clearly compare offices that increased their use of conditions with those that maintained the status quo, I also exclude the six offices with an ambiguous policy change. Table A1 in the appendix lists these sample restrictions, which result in 201 offices being included in the final sample. In this paper, I only use information

Table 1
Types of conditions for welfare and number of offices reporting increased use in the period 1994–2004.

Activation and work requirements	Number of offices with increased use	Fraction of treated offices
Participate in program: A requirement to take part in a work/training or educational program.	26	0.60
Work for welfare: Requirement to participate in a work program organized by the municipality or others.	15	0.35
Register as seeking work: A requirement to register as an active job-seeker, keeping an updated CV etc.	25	0.58
General counseling: Attend counseling meetings with caseworker or others to discuss the current situation.	26	0.60
Career counseling: Attend career counseling meeting(s) with caseworker or others to improve work prospects.	10	0.23
At least one activation/work requirement	41	0.95
Economic		
Document expenses: A requirement to show documentation for housing costs and other additional costs exceeding the welfare benefit	29	0.67
How to use the benefit: Restrictions on how the recipient spend the benefit	17	0.40
Move to cheaper housing: Refuse to cover housing costs exceeding the norm and require that one move to cheaper housing for obtaining housing support.	16	0.37
At least one economic condition	34	0.79
Health		
Medical examination: Willingness to undergo a medical examination.	14	0.33
Total number of conditions changed	175	
Total number of offices changing policy	43	

Note: Based on the 43 offices reporting an unambiguous increase in the use of conditions for welfare in the period 1994–2004.

about people residing in areas covered by these 201 offices, amounting to around 60% of the Norwegian population.

Table 1 lists the types of conditions reported by the social insurance offices and the number of offices reporting increased use for each condition. Five conditions are activation or work requirements, three concern the economic situation of the claimant, and one is health related. Since the conditions are quite different, it would have been interesting to evaluate the effect of particular conditions or combinations of conditions. Unfortunately, because offices increased their use of several conditions at once, on average more than four, the changes are highly correlated. This is even more of a problem given that inference has to be group-based, as it is not known whether or not a particular individual receives a specific condition.[3] Dividing the 43 treatment municipalities into even smaller groups, whose changes would still correlate with those of others, would cause the estimates to be based on an unreliably small number of units. A dummy variable treatment is relatively robust to these concerns. Specifying the treatment in such a conservative way should also help avoid measurement error since treatment is then not dependent on reporting and judgements about particular items, which may be prone to measurement error. Treatment is therefore coded as a dummy variable, which permanently switches from 0-1 for a given office when the office intensifies its use of conditions for welfare.

The 43 offices with an unambiguous change to increased use of conditions thus constitute the treatment group. They intensified their use of conditions for welfare programs at various times throughout the period 1994-2004, with the majority doing so in the latter half of the period. In this context, with relatively young people, the treatment dummy should be interpreted as a comprehensive policy shift towards greater work and activation requirements. Such an interpretation is supported by other parts of the report, which also contained information about which groups were targeted by conditionality as well as qualitative information from interviews with caseworkers and office directors. Young welfare clients were by far the group for which conditions were applied the most-97% of respondents reported that they “often” used conditions for welfare programs for this group and thus we can be reasonably sure that young people as a group were in fact exposed to the use of conditions. Moreover, the offices also emphasized that they make an effort to avoid passive arrangements for young people, for whom conditions typically involved some sort of activation requirement in the form of actual work or training/education. Hence, even though many offices also increased their use of conditions other than activation and work requirements, for young people, the activation-related ones were the main tools the offices employed. This is also confirmed by a qualitative study of four more recently reformed municipalities, which all required young claimants to actually attend several times a week, with sanctions for absences (Dahl and Lima, 2016).

2.2. *Descriptive statistics of treatment, control and excluded municipalities*

The rest of the data come from administrative registers covering the complete Norwegian population. The first year with available earnings data is 1993, and 2004 is the last year with information about conditionality policies. I include 2005 in the dataset in order to have post-treatment observations for all treatment areas; thus, the sample period is 1993-2005. The treatment, control, and excluded municipalities are spread throughout the country (see Fig. A1 in the appendix for a map). In Table 2, we can see that the three groups are also quite similar with regard to broad, observable socioeconomic characteristics in 1993 and 2005, the first and final year of the sample period, respectively. The

[3] This resembles evaluations of US welfare reforms, where it has often been challenging to disentangle different program components (Blank, 2002). The reform studied in the present paper was simpler, as it did not involve changes to quantities such as tax rates, earnings disregards, time limits, or income or asset limits, though the fact that most of the offices changed their use of several conditions at once means the same challenge is also present.

Table 2
Descriptive statistics of treatment, control and excluded municipalities.

	Treatment (n = 43) Mean (SD)		Control (n = 158) Mean (SD)		t-test of mean difference, C-T t (p-value)		Excluded (n = 227) Mean (SD)		t-test of mean difference, T-E t (p-value)	
	1993	2005	1993	2005	1993	2005	1993	2005	1993	2005
Inhabitants	10,392	11,235	7207	7581	−1.99	−2.04	11,674	12,621	−0.23	−0.22
	(13,713)	(15,226)	(7689)	(8709)	(0.05)	(0.04)	(36,342)	(4,0733)	(0.82)	(0.83)
Employment rate	0.647	0.692	0.667	0.701	1.90	0.10	0.662	0.697	−0.44	1.12
	(0.038)	(0.035)	(0.041)	(0.034)	(0.06)	(0.92)	(0.034)	(0.027)	(0.66)	(0.26)
Income (1000 €)	37	41	38	41	0.80	−0.27	40	44	0.31	1.12
	(5.1)	(5.7)	(5.1)	(5.7)	(0.42)	(0.79)	(5.1)	(5.7)	(0.76)	(0.26)
Unemployment rate	0.046	0.028	0.040	0.027	−1.44	0.52	0.044	0.030	0.38	−1.25
	(0.010)	(0.007)	(0.011)	(0.008)	(0.15)	(0.60)	(0.010)	(0.007)	(0.70)	(0.21)
Fraction with tertiary education	0.167	0.235	0.182	0.252	1.34	1.08	0.227	0.309	−1.35	−1.05
	(0.038)	(0.047)	(0.060)	(0.071)	(0.18)	(0.28)	(0.077)	(0.091)	(0.18)	(0.30)
Fraction with at least secondary education	0.412	0.577	0.419	0.585	0.33	0.64	0.465	0.619	0.01	0.19
	(0.055)	(0.052)	(0.072)	(0.059)	(0.74)	(0.52)	(0.083)	(0.066)	(0.99)	(0.85)
Fraction receiving welfare benefits	0.027	0.019	0.021	0.017	−1.98	−0.69	0.027	0.020	1.56	0.55
	(0.010)	(0.007)	(0.008)	(0.006)	(0.05)	(0.49)	(0.010)	(0.006)	(0.12)	(0.58)
Fraction receiving disability benefits	0.090	0.104	0.087	0.102	−0.90	−0.22	0.085	0.094	−0.45	−0.72
	(0.022)	(0.022)	(0.025)	(0.026)	(0.37)	(0.82)	(0.021)	(0.023)	(0.65)	(0.47)
Fraction immigrants	0.088	0.141	0.071	0.115	−0.86	−0.91	0.113	0.177	−0.14	0.28
	(0.039)	(0.052)	(0.034)	(0.046)	(0.39)	(0.36)	(0.061)	(0.090)	(0.89)	(0.78)
Fraction of population in working age	0.588	0.590	0.586	0.584	−1.18	−1.57	0.593	0.602	1.28	1.60
	(0.027)	(0.019)	(0.032)	(0.022)	(0.24)	(0.12)	(0.026)	(0.030)	(0.20)	(0.11)

Note: All variables refer to the age group 18–61 years if not specified otherwise, and reported means are weighted by population size. Income is yearly, inflated to 2013-value with the adjustment factor used in the Norwegian pension system (approximately corresponding to the average wage growth) and converted to Euros with the exchange rate €=9.1 NOK.

treatment municipalities are, on average, somewhat larger than the control municipalities, which also started with a somewhat higher employment rate and a lower share of people on welfare. The differences in other characteristics are not statistically significant at conventional levels.

The individual-level data used in the paper only go back to 1993 and thus cannot be used to construct pre-trends. However, Statistics Norway publishes unemployment and employment data at the municipality level for 2539 year olds from 1990 onwards. Fig. A2 in the appendix shows pre-trends for the unemployment rate (registered unemployed persons as share of the population) and the employment rate (persons with least 20 contracted hours/week as share of the population) for the treatment and control groups. It is reassuring that they seem to follow quite similar trends pre1993.

As noted above, the offices did not have an incentive to mischaracterize their policies or strategically decide whether to respond to the survey. However, a large number of social insurance offices did not respond, raising concerns about external validity. After the survey was administered in 2005, TRI investigated the reasons for the relatively low response rate and found that unforeseen problems related to e-mail communication played a large role. First, they did not find valid e-mail addresses for some of the welfare section leaders. Second, some municipalities had "secure zones" in their IT systems that prevented the survey from appearing in their email. Third, some respondents were less familiar with the use of electronic communication than was assumed.[4] These problems may or may not reflect other factors relevant to welfare policy or local labor markets, and thus we do not know whether the treatment and control groups are representative of the whole country. Nevertheless, Table 2 offers some hope for broader applicability of the results, as the excluded municipalities are not statistically different from the treatment group in any of the observable characteristics.

2.3. Estimation sample

I analyze effects on individuals 26–30 years of age. The cutoff at age 30 is motivated by the fact that conditions for welfare and the policy discussion around them are focused on young welfare recipients, often considered to be those below 30 years of age (Proba, 2013). This focus is evidenced by the national introduction of mandatory activation requirements for welfare recipients below age 30 in 2017, for which the results in this paper may provide some guidance. The data window for the present paper is 1993–2005, well before the countrywide reform in 2017. I set a lower cutoff at 26 in order to avoid complications related to the timing of higher education, which may be affected by the policy changes. Fig. 1 shows that the probability of claiming welfare during the calendar year 1993 is highest for individuals in their beginning to mid-20 s and declines with age, a pattern driven by people increasingly earning the right to other benefits through work or gaining access to disability programs. This pattern is stable over time and provides another rationale for the focus on relatively young recipients. Of the age group I analyze in this paper, 26–30 year olds, 8% received welfare at some time in 1993. Welfare policy affects both those actually receiving welfare as well as a wider population with only a potential connection to the welfare system. To capture effects on both these groups as well as spill-over effects, I focus on the reduced form effects on everyone from 26 to 30 years of age residing in the treatment areas.

The increased use of conditions indicates a clear tightening of policy, as sanctions are often used in the event of non-compliance with requirements (Brandtzæg et al., 2006; Terum et al., 2015). This tightening could both decrease inflows to and increase outflows from welfare. Lower welfare payments would give an incentive to increase labor supply to offset some of the reduced income. It is possible that fulfilling mandatory requirements could take time away from other types of

[4] These problems are documented in Brandtzæg et al. (2006, chapter 2).

Table 3
Estimation sample. Descriptive statistics at baseline (year=1993), 26–30 year olds.

	Welfare recipients		*t*-test of mean difference, M-W	Others		*t*-test of mean difference, M-W
	Men	Women		Men	Women	
	Mean (SD)	Mean (SD)	t (p-value)	Mean (SD)	Mean (SD)	t (p-value)
Age	27.93	27.87	2.26	27.96	27.98	−1.94
	(1.40)	(1.41)	(0.02)	(1.41)	(1.41)	(0.05)
High school	0.28	0.23	5.46	0.60	0.56	13.90
completed	(0.45)	(0.42)	(0.00)	(0.49)	(0.50)	(0.00)
University/college	0.07	0.06	2.89	0.20	0.25	−18.68
Completed	(0.26)	(0.23)	(0.00)	(0.40)	(0.43)	(0.00)
Earnings,	11,165	7407	13.11	39,923	26,143	102.29
mean, €	(14,741)	(11,451)	(0.00)	(24,416)	(19,398)	(0.00)
Earnings, median, €	3871	847		44,815	27,207	
Employed	0.39	0.27	11.67	0.82	0.72	37.64
	(0.49)	(0.45)	(0.00)	(0.39)	(0.45)	(0.00)
Have children	0.47	0.78	−31.12	0.44	0.67	−78.17
	(0.50)	(0.41)	(0.00)	(0.50)	(0.47)	(0.00)
Immigrant	0.25	0.14	12.58	0.08	0.09	−5.28
background	(0.43)	(0.35)	(0.00)	(0.28)	(0.29)	(0.00)
N	4964	3893		56,396	51,949	

Note: All variables except age and earnings are measured as dummy variables. Earnings are yearly, inflated to 2013-value with the adjustment factor used in the Norwegian pension system (approximately corresponding to the average wage growth) and converted to Euros with the exchange rate €=9.1 NOK. Employed defined as having yearly earnings of at least one Norwegian “basic amount” (G), corresponding to € 9377.

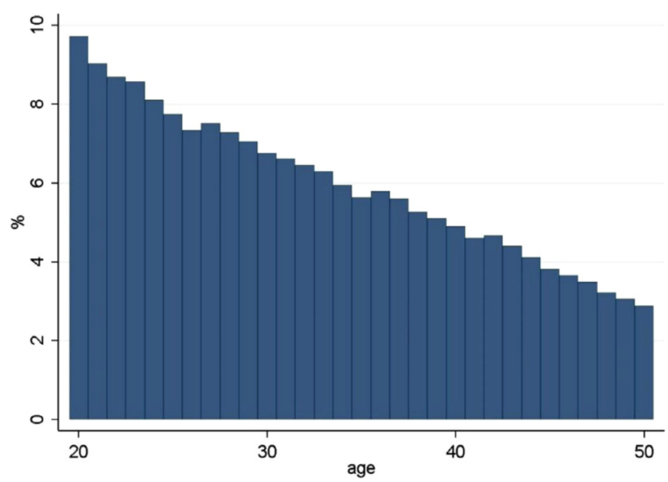

Fig. 1. Share receiving welfare by age in 1993, 26–50 year olds.
Note: Calculated based on inhabitants in the whole country.

work or job search; however, given that an explicit aim of the policy is to make recipients self-sustaining, and as the law stipulates that requirements should not be disproportionally burdensome or unreasonable, such cases would likely not dominate. However, there is also an important productivity-enhancing element in the use of conditions. Specifically, they may lead to training or work experience, which in turn increase employment and earnings and make welfare unnecessary. This element would be expected to increase the earnings of those actually on welfare and hasten their eventual transition out of welfare recipiency. Overall, therefore, welfare payments should decline and earnings should increase for those affected by the policy change. As it was within the power of the social insurance offices to individualize benefits and sanctions, impacts on both the intensive and extensive margins are possible. Whether or not increased earnings would compensate for the loss of welfare is clearly ambiguous. It will thus be important to investigate the effect on all income combined.

Table 3 shows characteristics of welfare recipients and others in the estimation sample for 1993, the first year of the analysis and before any of the policy changes had occurred. Recipients of welfare are very different from non-recipients; in particular, they have less education and much lower earnings and employment rates. Table 3 also reveals substantial gender differences, both among welfare recipients and non-recipients. First, men are employed to a greater extent, and, among welfare recipients, men's median earnings are more than twice as much as those of females. The gender differences in earnings and employment imply that there is a larger scope for increases in the labor supply of women. Second, more women than men have children, especially among welfare recipients. Having children may restrict possible increases in labor supply; however, affordable, government-sponsored childcare is widely available in Norway. Third, among welfare recipients, more men have immigrant backgrounds, which may be a barrier in the labor market. Finally, previous research based on Norwegian data has also found that women transitioned quicker than men from welfare, suggesting that women are more responsive to the assistance received from the social insurance offices (Fevang et al., 2004). Because of these gender differences, all of which are highly statistically significant, the main results will be presented by gender.

3. Empirical strategy

3.1. Identification

I compare outcomes for individuals measured before and after implementation of conditionality. At the core of the empirical strategy lies a linear difference-in-differences (DiD) model, set out in Eq. (1). y_{it} denotes the outcome of interest for person i in year t, primarily welfare uptake some time during the year, measured as a dummy variable, or yearly earnings. Municipality fixed effects γ_m capture all factors that are fixed at the municipality (office) level, such as local area health and worker characteristics, while time fixed effects δ_t capture time-varying factors that are common across muncipalities, such as aggregate business cycles or other time trends. Time fixed effects are essential, as the social insurance office plays an important part of the social safety net protecting against poverty in economic downturns. The treatment variable T_{mt} is 0 for all municipalities in the beginning of the time period, then for a given municipality turn permanently to 1 when the social insurance office in the municipality increases its use of conditions for welfare. Finally, I include a small set of time-varying municipality level characteristics x_{mt}, consisting of the share of population with tertiary education, average age of working age population and share of immi-

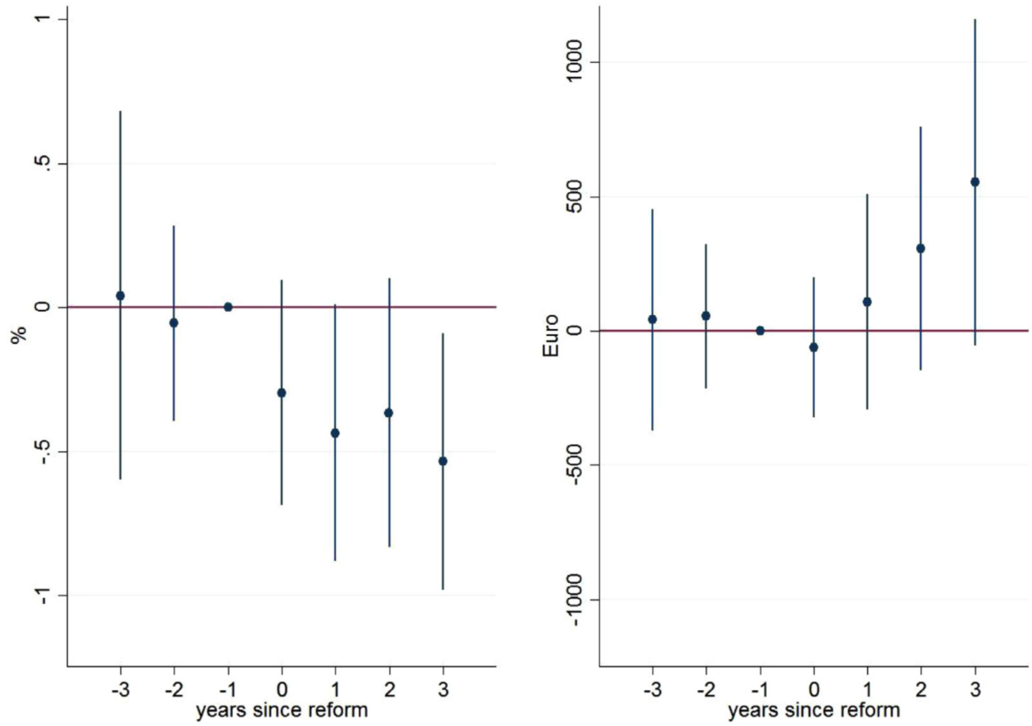

Fig. 2. Event study estimates of welfare uptake (left) and earnings (right).
Note: Estimates relative to t-1 based on regressions including office fixed effects, cohort fixed effects and municipality covariates (share of population with tertiary education, average age of working age population and share of immigrants in the office area). Standard errors are clustered at the 201 offices. Welfare uptake is defined as receiving welfare at least once during the year. Earnings are yearly earnings. Vertical bars indicate 95% confidence intervals.

grants.

$$y_{it} = \beta T_{mt} + \gamma_m + \delta_t + x_{mt} + \varepsilon_o \tag{1}$$

As discussed above, the treatment indicates a policy change consisting of a combination of a greater use of activation and a higher degree of monitoring than what was previously the case. Eq. (1) will be used first as a standard difference-in-differences model to estimate mean impacts on welfare uptake and earnings, and later at the heart of the distributional analysis, set out in Section 3.2. In the difference-in-differences analyses, the standard errors are clustered at the office level.

Whether to increase use of conditionality was decided by the social insurance offices themselves, and as I do not observe the factors that influenced those decisions, it is possible that the introduction of the reforms correlate with pre-existing trends in the municipality. To investigate pre-treatment time trends in outcomes, I employ an event-study specification along the lines of Jakobson, LaLonde and Sullivan (1993). Specifically, I expand the treatment variable T_{mt} in Eq. (1) to a series of one-year dummy variables indicating time relative to the reform year. Fig. 2 displays the results from this specification, where the year prior to the reform is the omitted category. As there are reforms occurring towards both the beginning and the end of the period, only coefficients from three years before and after the reform are displayed and the sample period is expanded to 2007 to avoid compositional effects from an unbalanced sample. It is reassuring that the estimated coefficients before the reform are close to 0 and that the estimates go in the expexcted direction from the reform onwards.

It is also worth noting that neither Hernæs et al. (2017) nor Bratsberg et al. (2019), who studied 18–21 year olds, found much difference between their double and the triple difference estimates. In this paper, a triple difference design is not feasible, since the outcome of interest is the entire distributions of earnings and income. However, as welfare policy should have very little impact on the upper part of the distributions, a related check of the identification strategy is whether substantial effects are estimated there.

3.2. Econometric model for distributional analysis

One way to evaluate distributional effects is to implement conventional quantile regression, which estimates effects on each quantile conditional on the control variables (Koenker and Bassett, 1978). However, when it comes to welfare policy, including explanatory variables in quantile regression models renders coefficients that do not reflect the impact of these variables on quantiles in an absolute sense. Therefore, a transformation of outcome measures is needed to obtain unbiased estimates. Firpo et al. (2009) showed how to do this with the recentered influence function (RIF) regression approach under a selection-on-observables assumption.

The influence function $IF(Y; \nu, F_Y)$ of a distributional statistic $\nu(F_Y)$ for some variable Y with cumulative distribution F_Y measures the influence of a specific observation on that distributional statistic. In the following, I focus on quantiles and the variable earnings. For a quantile τ, we have $IF(Y; q_\tau, F_Y) = (\tau - 1\{y \le q_\tau\})/f_Y(q_\tau)$, where q_τ denotes the τth quantile of the distribution of earnings, and f_Y the (empirical) density function evaluated at q_τ. Consider the median as an example. Then $\tau = 0.5$, $q_{0.5}$ is the median value of earnings, and $IF(Y; q_{0.5}, F_Y) = (0.5 - 1\{y \le q_{0.5}\})/f_Y(q_{0.5})$. This will evaluate to two different values for all observations of earnings, depending on whether the value is below or above the median value $q_{0.5}$. The same will be the case for other quantiles.

The *recentering* in the RIF approach involves adding the statistic in question, such that $RIF(Y; q_\tau, F_Y) = IF(Y; q_\tau, F_Y) + \nu(F_Y)$. In the case of quantiles, $RIF(Y; q_\tau, F_Y) = IF(Y; q_\tau, F_Y) + q_\tau = (\tau - 1\{y \le q_\tau\})/(f_Y(q_\tau)) + q_\tau$. In the example with the median, $RIF(Y; q_{0.5}, F_Y) = (0.5 - 1\{y \le q_{0.5}\})/(f_Y(q_{0.5})) + q_{0.5}$, with $q_{0.5}$ the median value of earnings and $f_Y(q_{0.5})$ the density function of earnings evaluated at the median value. For any quantile τ, with corresponding q_τ (the τth quantile of the distribution of earnings), the RIF is also bivalued, depending on $1\{y \le q_\tau\}$. The RIF regression approach models the conditional expectation of RIF$(Y;\ q_\tau,\ F_Y)$ as a function of a set of explanatory variables X. Firpo et al. (2009) show that the marginal effect on the unconditional quantile can be estimated by the resulting *unconditional quantile regression* E[RIF$(Y;\ q_\tau, F_Y)|X$]. Since the RIF is bivalued, it can be estimated with limited dependent variable models.

Some more intuition is provided by Fortin et al. (2011): The RIF regression approach can be seen as first defining a series of earnings cutoffs q_τ corresponding to specified quantiles of the empirical earnings distribution and then for each such cutoff estimating the marginal effects of the covariates on the probability of being above that cutoff. Intuitively, these estimates, which are in terms of probabilities, correspond to marginal effects on the cumulative distribution function of earnings. To get to the quantile treatment effects, which for earnings should be in monetary amounts, the estimate for any specific quantile is divided by an estimate of the density of the earnings distribution at that point. Again thinking in terms of effects on the CDF, dividing by the density can be seen as inverting the estimated (vertical) effects on the CDF to corresponding (horizontal) quantile treatment effects, assuming that the relationship between the counterfactual proportions and quantiles are locally linear.

In this paper, the series of earnings cutoffs q_τ are defined by every 5th quantile of the empirical earnings distribution. I follow the baseline approach of Firpo et al. (2009) of estimating by ordinary least squares, i.e. using a linear probability model, and a kernel density estimate of the slope of the CDF of the earnings distribution at each particular quantile.[5] The probability model is the model specified in Eq. (1) above. The identifying assumption is that in the absence of treatment, the change in the population shares at each threshold would have been the same in the treatment and the comparison group. Because of the uncertainty in the estimate of the kernel density estimate, standard errors are bootstrapped (with 499 replications). To allow for correlations at the municipality level, the standard errors are block bootsrapped with the municipality as the block. The estimated distributional effects are presented graphically in percent of earnings at each quantile, with the underlying numbers reported in tables in the appendix. The treatment variable T_{mt} is here constructed as a two-element vector containing separate treatment indicators for men and women.

I employ the RIF approach as the preferred method because it allows straightforward inclusion of covariates, as opposed to other nonlinear difference-in-differences methods such as the quantile DiD and the Changes-in-Changes estimators (Athey and Imbens, 2006), and because it is less computationally demanding than the distribution regression approach of Chernozhukov et al. (2013). However, as a focus on marginal changes and the aforementioned assumption of a locally linear relationship between the counterfactual proportions and the counterfactual quantiles are important elements in the RIF approach, I estimate the main model using the method of Chernozhukov et al. (2013) for as a robustness check. This approach is based on estimating the complete counterfactual distribution using a large number of so-called distribution regressions, as opposed to approximating it with a local linearization.

4. Results

4.1. Earnings effects

Table 4 shows estimated average effects of welfare conditionality on welfare uptake and earnings for 26–30-year olds. The implementation of conditionality reduces welfare uptake by 0.41 percentage points, corresponding to a reduction of 7%. The estimated average effects on other variables are quite small. However, the fact that changes in welfare policy mainly affect people with a low earning potential suggests going beyond the mean impact and analyzing the effects on the distribution. It is likely that the relatively small average effects mask an effect of higher earnings among low earners and no effects among high earners.[6]

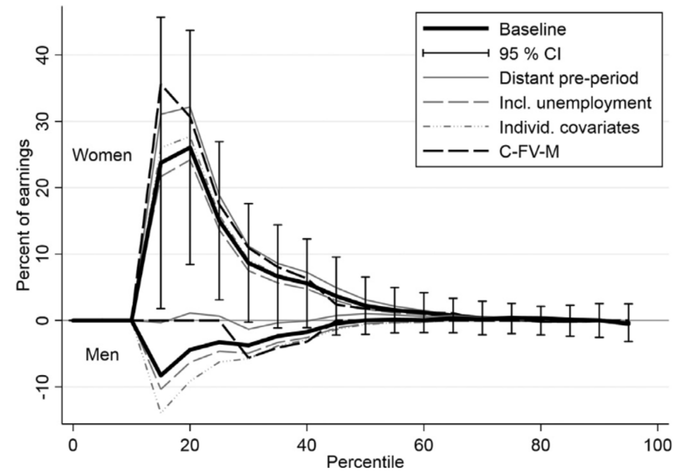

Fig. 3. Main quantile treatment effect estimates on earnings, 26–30 year olds. Note: Treatment effect estimates at each fifth percentile in percent of the level at each percentile. All specifications include office and cohort fixed effects, gender, share of population with tertiary education, average age of working age population and share of immigrants. “Distant pre-period” excludes observations three years or less before treatment. “Individ. covariates” includes fixed effects for age and immigrant status. “C-FV-M” denotes results obtained using the distribution regression method of Chernozhukov et al. (2013). Standard errors are block bootstrapped with 499 replications with the municipality as the block. Vertical bars indicate 95% confidence intervals for the baseline results for women.

Fig. 3 shows the baseline quantile treatment effect (QTE) estimates on earnings. The numbers underlying the figure can be found in the appendix in Tables A2 and A3. According to the baseline results, conditionality increases earnings for women substantially in the lower part of the earnings distribution by 2025% at the 15th-25th percentiles. Welfare recipients often have relatively low earning potential, and hence it is reasonable that the estimated effects will appear in the lower end of the distribution. As expected, the estimated effects decline towards 0 in the upper part of the distribution. The point estimate for women at the 20th percentile is around € 2000, or € 170 per month. Regardless of whether the positive results are brought about by reduced moral hazard problems or increased job opportunities or productivity at the individual level, getting women into more high-paying jobs is often seen as a worthy policy-goal in itself. The estimated QTEs for men are consistently close to and never significantly different from zero.

There is a substantial gender difference in the effects of the policy. One explanation for this could be that, as women worked less and had lower earnings (see Table 3), they had more room for increased labor supply. A complimentary explanation is that women may be more responsive to training opportunities than men, which was an early finding in the US literature on active labor market programs for disadvantaged groups (Heckman et al., 1999). It is possible to interpret the negative point estimates for men as reflecting increased competition from women, consistent with evidence from the US welfare reform

[5] The kernel density estimate is based on an Epanechnikov kernel and the STATA default “optimal” bandwidth.

[6] The incidence of welfare uptake decreases with age, and the estimated effect is substantially smaller than in Hernæs et al. (2017), who found 1.1 percentage points reduction in welfare uptake for 21-year olds from the same reform. Hernæs et al. (2017) also found an increase of 1.2 percentage points in high school completion, while Bratsberg et al. (2019) found a mean effect of −0.4 percentage points on crime for 18-21 year olds. These papers did not study income, which is a somewhat ambiguous outcome for young people, who may benefit more from education.

Table 4
Estimated intention to treat effects of welfare conditionality (standard errors in parentheses), 26–30 year olds.

	Welfare uptake	Earnings, €	Any earnings	Employed	Total income, €
ITT	–0.0041	237	0.0025	0.0061	26
	(0.0024)*	(254)	(0.0031)	(0.0040)	(329)
Office fixed effects	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes
Municip. covariates	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	0.06	34,062	0.89	0.78	42,607
N observations	1450,061	1450,061	1450,061	1450,061	1450,061

Note: Welfare uptake, Any earnings and Employed are measured as dummy variables. Yearly earnings and income, in 2013 value. Municipality covariates include share of population with tertiary education, average age of working age population and share of immigrants in the office area. Standard errors are clustered at the 201 offices. *(**)(***) indicates statistical significance at the 10(5)(1) percent level.

(Groves, 2016); however, the uncertainty is too large for any firm conclusions on this.

It is possible to calculate a joint test of the hypothesis that the reform had no effect on earnings based on the distribution of the bootstrap estimations. When doing this for the estimates on the lower half of the earnings distribution, where any treatment effects are primarily expected, the null hypothesis of no effect is rejected at the 5% level, as the estimated effect is positive in more than 95% of the bootstrap samples. When taking the estimated results for all the analyzed percentiles into account, the estimated effect is positive in only 77% of the bootstrap samples, and thus the null hypothesis of no overall effect cannot be rejected. A test based on effects across the whole distribution might be too conservative, as a policy tool targeting some of the poorest and least resourceful individuals in society is not expected to have effects in the upper parts of the distribution. More fundamentally, separate hypotheses about effects occurring at different places along the distribution are clearly not independent. However, as there is some uncertainty regarding an overall effect, Chernozhukov et al. (2013) method is used as a robustness check below.

4.2. Robustness

4.2.1. Endogeneous policy change

The main threat to identification is that the policy change may be endogenous; for example, a (local) economic downturn may trigger the implementation of conditionality, which may appear to have an effect simply because of mean reversion of the business cycle. Mean reversion could also be an issue if survey responses depended on recent deveopments in the municipality. To challenge the baseline specification at this point, I provide estimates that are based only on pre-treatment periods four or more years prior to the policy change ("distant pre-period") and perform a sensitivity check in which contemporaneous unemployment is included as a covariate. I do not include municipality-specific time trends because these could readily pick up a treatment effect that is increasing with time due to learning and accumulated exposure to the stricter regime.

The results from these specifications are displayed by the gray lines in Fig. 3. The QTE estimates are stable across specifications. Of particular importance is the specification "Distant pre-period," which excludes observations three years or less before treatment. This serves as a check of the possibility that the office changes its policy after a few bad years, which could depress the baseline against which the treatment is compared. From the figure, we see that this was not the case, and leaving out these observations increases the estimates. The inclusion of unemployment or individual-level covariates (age and immigrant status) has little effect on the results much.

The fact that the survey is retrospective raises a concern about whether the timing of events far back in time is accurately recalled. To investigate whether this may be a problem, the quantile treatment effects are estimated using only the policy changes occurring since 2000. The results are quite similar to the baseline estimates (see Fig. A3 in the appendix). If anything, the estimates from the more recent reforms are somewhat larger, which is to be expected if earlier changes are more subject to measurement error.

4.2.2. Selective migration

The possibility of selective migration presents another concern. Although Edmark (2009) found no migration effects in Sweden of a similar type of welfare reform based on activation requirements, Fiva (2009), using Norwegian data, found effects of the level of the welfare benefit on migration. The welfare reform that I study, which was geographically based, could have induced some people to move, e.g. to a place with less demanding requirements. If these individuals would have been low earners if they had stayed, the likelihood of attaining a particular earnings treshold would artificially seem to have increased. Selective migration could also occur through other channels, for instance if the treatment opens the door for jobs or other options elsewhere, either on its own or through higher earnings. The problem in both cases is that the actual municipality of residence is potentially endogenous. To avoid this potential endogeneity, one could fix individuals to where they lived before the treatment. However, that would introduce a large degree of measurement error due to (non-selective) migration. To handle the measurement error, I use instrumental variables. Specifically, (current) treatment status in an indiviudual's municipality of residence five years earlier is used as an instrumental variable for treatment status in the individual's actual municipality of residence. This aproach assigns conditionality regime based on where one resided five years ago and assumes that this status only impacts earnings through the conditionality regime in one's actual municipality of residence. In this specification I exclude post-treatment observations from more than three years after the reform in order to ensure that the instrument is measured before the reform. Residential mobility is fairly low in Norway, and the first stage is strong, with a coefficient on the instrument of 0.78 and an F-statistic of 614. They way in which instrumental variables should be handled with the UQR method is not established, thus I perform only the first step of the procedure: estimating the treatment effect on the probability of earning above given percentiles of the earnings distribution, with and without instrumentation. The results are graphed in Fig. A4 and presented in detail in Table A4 in the appendix. The IV results closely mirror the baseline estimates the probability of earning above the lower percentiles of the earnings distribution increases, and it declines for higher percentiles – therefore selective migration does not appear to be driving the results in this case.

4.2.3. Chernozhukov, Fernández-Val-and Melly (C-FV-M) distribution regression estimator

One potential concern with the RIF approach is that the assumption of a locally linear relationship between the counterfactual proportions and the counterfactual quantiles might be misleading. In contrast, the distribution regression method of Chernozhukov et al. (2013) estimates the complete counterfactual distribution. The black, dashed line in Fig. 3 shows the main results when implementing this more computationally demanding estimator. It is reassuring that the results are very similar to the baseline results from the RIF approach.[7] The C-FV-M method also provides uniform confidence bands and hypothesis tests based on the Kolmogorov-Smirnov test statistic and is able to reject the hypothesis of no effect on all quantiles at the 1% level. The results are provided in detail in Table A5 in the appendix.

4.2.4. Country-wide representativeness of the results

The preceding results are based on only around half of the country's municipalities. This is because the policies and policy changes of social insurance offices that did not respond to the survey are unknown. However, given that there was a general trend towards increased use of conditions in this period, it is likely that many offices from the excluded group also tightened their policies. An interesting robustness check therefore involves estimating a separate treatment "effect" for the excluded group as a whole.[8] If the estimation sample is representative of the country as a whole, this estimate should resemble the main treatment effect but be smaller, as it would not account for the likelihood that many offices in the excluded group did not change their policies. Fig. A5 in the appendix shows that this is indeed the case. Although the uncertainty is considerable, the estimate for the excluded group is also statistically significant. The results are provided in detail in Table A6 in the appendix.

4.3. Total income effects

Although the policy is successful in boosting labor supply and increasing wage earnings, it is important to analyze its effect on total income (all income combined). Even if earnings increase, it is not clear whether the effects on total income would also be positive, as welfare payments are reduced. The estimated quantile treatment effects on the sum of income from all sources is shown in Fig. 4, with the baseline estimates on earnings reproduced for reference. The estimates are shown in detail in Table A7 in the appendix.

Fewer people have zero income than have zero earnings. Thus, in contrast to with earnings, for total income it is feasible to obtain estimated effects also in the first percentiles. The results show that there are substantial positive effects in the lower end of the distribution for women. Although the impact on welfare, in the sense of utility, is ambiguous, it is encouraging from a policy perspective that greater use of conditionality does not appear to reduce total income.

For men, it is somewhat puzzling that the estimated effect on total income is close to 0 in light of the negative estimated effect on earnings. In principle, within-household transfers could account for the unchanged total income for men. However, this seems unlikely, as such transfers would not typically be formalized and recorded in administrative registers. Benefit substitution also does not seem to be the explanation, as point estimates of the effect on receiving unemployment benefits or disability benefits or participating in a government labor market program are all negative, although we cannot rule out the possiblity that men receive income from some unobserved program. Thus, the discrepancy between the negative effect on earnings and zero effect on total income for men remains a puzzle. However, it must be noted that this discrepancy is only a few hundred Euros per year at most, and the estimates on which it is based are fairly small and not statistically significant.

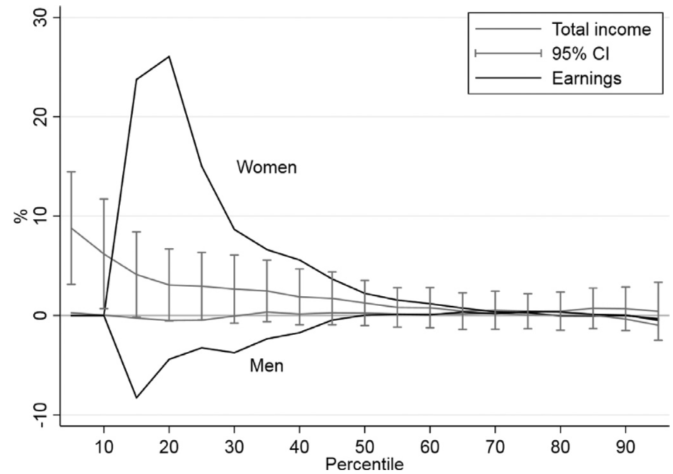

Fig. 4. Quantile treatment effect estimates on total income, 26–30 year olds. Note: Income from all sources, including welfare. Confidence interval shown only for total income for women. Treatment effect estimates at each fifth percentile in percent of the level at each percentile. All specifications include office and cohort fixed effects, gender, share of population with tertiary education, average age of working age population and share of immigrants. Standard errors are block bootstrapped with 499 replications with the municipality as the block. Vertical bars indicate 95% confidence intervals.

4.4. Mechanisms

It is possible to gain some insight into the mechanism by investigating the effects on flows in and out of welfare and between welfare and work. Table 5 displays how various transitions are affected by the reform. Columns (1) and (2) show that the exit rate from welfare was much more strongly affected than the entry rate. The entry rate is calculated on the basis of all individuals who did not receive welfare in the previous year, while the exit rate is calculated based on those who did—a much smaller group. Taking the difference in the size of the groups intro account, it is clear that the impact on exits was also larger in absolute terms.[9] This suggests that "threat effects" on potential entrants into welfare programs were limited and supports a focus on the program participants.

Without detailed data on the types of conditions applied at the individual level, it is not possible to disentangle whether the conditions worked through raising the cost of receiving welfare, better counseling and support in the job application process, supporting work-relevant experience, or some combination of all of these. However, differentiating the exits according to whether they led to employment or unemployment provides some insight. For both men and women, the estimated effect on exits was evenly split between employment and unemployment, see Columns (3) and (4). These estimates are imprecise and should be interpreted with caution. However, the fact that a sizeable share is estimated to have exited to unemployment suggests at least some role for voluntary exit due to the increased participation costs related to having to meet up and participate in an organized activity. The interviews with office caseworkers reveal that a mechanism in terms of acquiring basic skills is appropriate for at least a subset of young claimants. According to the caseworkers, members of this group are in need of support and guidance to obtain some structure in their daily lives, and the experience with work requirements is particularly positive for them. According to Brandtzæg et al., 2006), "Many need to learn what is demanded at a workplace, among other things that one has to be precise, that it is an advantage to have breakfast before going to work, that one has to give notice if sick, that one needs to go to the physician to get a sickness

[7] Two simplifying choices have been made in order to reduce estimation time: First, a linear probability model, in stead of a logistic or probit model, is used when estimating the conditional distribution. Second, the conditional distribution is approximated by estimating a distribution regression at 100 different cutoff values of the unconditional earnings distribution. The point estimates when specifying a logistic function or 500 cutoff values are very similar.

[8] I am grateful to an anonymous reviewer for suggesting this idea.

[9] For women: Entry: 605130*−0,0012 = −726. Exit: 39028*0,0340 = 1327. For men: Entry: 648107*0,0005 = 324. Exit: 46359*0,0146 = 677.

Table 5
Estimated intention to treat effects on welfare entry and exit and labor supply, 26–30 year olds.

	(1) Welfare Entry	(2) Exit	(3) Exit to Empl.	(4) Unempl.	(5) Labor supply Extensive	(6) Intensive
Women	−0.0012	0.0340	0.0180	0.0160	0.0026	452
	(0.0014)	(0.0148)**	(0.0122)	(0.0130)	(0.0039)	(200)**
Dep. var. mean	0.02	0.42	0.21	0.21	0.87	30,593
Number of observations	605,130	39,028	39,028	39,028	701,789	520,211
Men	0.0005	0.0146	0.0075	0.0071	0.0025	−211
	(0.0009)	(0.0123)	(0.0077)	(0.0107)	(0.0036)	(317)
Dep. var. mean	0.02	0.34	0.23	0.11	0.91	44,149
Number of observations	648,107	46,359	46,359	46,359	748,272	584,831

Note: Welfare uptake indicates receiving welfare some time during the year. Entry to welfare is estimated on people not on welfare the previous year, exit is estimated on people receiving welfare the previous year. Employed defined as having yearly earnings of at least one Norwegian "basic amount" (G), corresponding to € 9377. Extensive margin measured as a dummy variable indicating whether the individuals had above zero earnings. Intensive margin measured by earnings of people with above zero earnings the previous year. All specifications include office and cohort fixed effects, share of population with tertiary education, average age of working age population and share of immigrants. Standard errors are clustered at the municipality level. *(**)(***) indicates statistical significance at the 10(5)(1) percent level.

certificate." While these are basic concepts, not everyone has had the opportunity to learn them growing up. Such positive experiences are reflected in interviews with youths actually facing activation requirements. For example, one said that it was good to get practice in waking up in the morning. Another agreed that having to work to receive the social assistance benefit was a reasonable requirement and that they "would only have been at home if not. Good to get up in the mornings" (Brandtzæg et al., 2006)).

Finally, Columns (5) and (6) investigate the extensive and intensive margins of the labor supply response. For both men and women, the share of people with positive earnings increased by around 0.25 percentage points, as shown in Column (5). The intensive margin is measured by the earnings of people with above zero earnings in the previous year.[10] Column (6) shows a substantial increase along the intensive margin for women, with average yearly earnings of previously working women increasing by € 452 and a smaller, less precise negative estimate for men.

Table A8 in the appendix shows results at the household level for married couples based on whether the household received welfare benefits in the previous year. For couples receiving welfare in the previous year, welfare reception drops by 14 percentage points, and there are substantial, although imprecisely estimated, positive effects on earnings for both the wife and the husband at the household level. This suggests that both spouses increased their labor supply in response to increased conditionality. For couples not previously on welfare, the estimated effects are not of comparable magnitudes, again suggesting that it was the program participants who were primarily affected by the policy change.

With any activation program, there is a danger of lock-in effects, as the activities may impede obtaining work elsewhere. Fig. 5 shows estimated effects when distinguishing between short- (1–2 years) and long-term (3+ years) exposure to the treatment. Any lock-in effects appear to be minimal, as there is a substantial response in the short term (1–2 years). We can also note that the long-term effects (3+ years) are very similar to the short-term ones, consistent with small returns of work experience for low earners (Card and Hyslop, 2005; Dustmann and Meghir, 2005).

[10] Although the intensive margin effect is estimated based on people with positive earnings the previous year, it will to some extent be impacted by compositional changes, given an extensive margin effect.

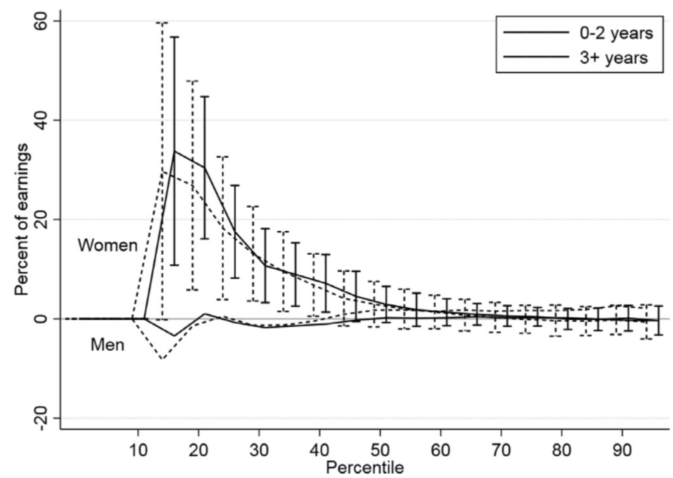

Fig. 5. Short and long-term quantile treatment effect estimates on earnings, 26–30 year olds.
Note: Estimated effects of exposure to conditionality for 0–2 years vs. 3 or more years. Treatment effect estimates at each fifth percentile in percent of earnings at each percentile. All specifications include office and cohort fixed effects, gender, share of population with tertiary education, average age of working age population and share of immigrants. Data until 2007 included in order to have a balanced sample of municipalities for both exposure periods. Standard errors are block bootstrapped with 499 replications with the municipality as the block. Vertical bars indicate 95% confidence intervals for the baseline results for women.

4.5. Cost-effectiveness

So far, we have seen that the welfare reform was successful in getting people off welfare and into work. This reduced public expenditures on welfare and increased tax revenue. However, this is not enough to pass a cost–benefit test, as it could still be the case that the new policy required an inordinate amount of resources in the offices. To examine this question, I use information about the municipalities' operating expenses related to welfare, published in the Kostra database by Statistics Norway. These data are only available since 2003, and thus the treatment effect will be identified solely on the basis of the reforms that took place in 2004. The increased use of conditionality is likely to have impacted operating expenses in two opposing ways. First, as the number of cases decreased, overall expenses related to case handling would also

Table 6
Estimated intention to treat effects on local social insurance office operating expenses per inhabitant 2003–2005, €.

	(1)	(2)	(3)
Operating expenses related to...	Welfare	Overall social assistance	Substance abuse
ITT	−32.1	−46.1	−0.1
	(9.7)	(12.0)	(2.8)
	***	***	
Dep. var. mean	140	229	17
Number of observations	576	576	576

Note: All regressions contain municipality and office fixed effects and municipality characteristics (share of population with tertiary education, average age of working age population, share of immigrants). Standard errors are clustered at the municipality level.
*(**)(***) indicates statistical significance at the 10(5)(1) percent level.

decrease. Second, more use of conditions means more follow-up work per case, which would increase expenses. The estimated effect on overall operating expenses related to welfare will be the net effect of these two opposing tendencies.

The results of the baseline DiD analysis are shown in Table 6. Encouragingly, operating expenses related to welfare decreased, as shown in column (1). This suggests that the savings due to a reduced caseload more than compensated for higher expenses related to following up the conditions. Column (2) shows that the operating expenses related to all types of social assistance also declined substantially. Thus, the savings related specifically to welfare appears not to have been passed on as costs to other offices.

Finally, from the TRI report (Brandtzæg et al., 2006), we know that people suffering from substance abuse are rarely subject to strict requirements, which are not viewed as productive in their case. Therefore, the increased use of conditions should have little effect on this group. Column (3) therefore provides a placebo test using operating expenses related to people suffering from substance abuse. A significant negative estimate here would imply that the caseload related to this group also fell, which would be worrying, as it should not be affected. Although the estimate is not precise, it shows that there was no clear reduction in expenditure related to substance abuse.

These findings, together with the savings related to a reduced number of welfare checks paid out and increased tax revenue, imply that the reform was highly cost-effective.

5. Conclusion

The main finding is that attaching conditions to welfare payments for young people reduced welfare uptake and increased both earnings and total income for women at the lower end of the earnings and income distributions. I find evidence that effects on program participants are the most important. The policy under study, related to activation and work-related requirements, is highly cost-effective. It moves welfare recipients into work and results in savings for the social insurance system by reducing both administrative costs and welfare payments.

It is important to mention that the reform occurred in a beneficial environment, which may help to explain the good results. First, the reforming municipalities were responsible for undertaking and implementing the changes and therefore likely had a large degree of ownership of the reform and a strategy for implementing it. This may be hard to replicate in the case of changes mandated from a higher authority. Second, the social insurance offices had a large degree of discretion in deciding who should face conditions and what to demand of them. This may be beneficial compared to uniform requirements if caseworkers have relevant information about how to adapt the conditionality policy. Nevertheless, the policy represents a promising avenue to explore for other countries in need of social insurance system reform.

Appendix

Figs. A1–A5 and Tables A1–A8

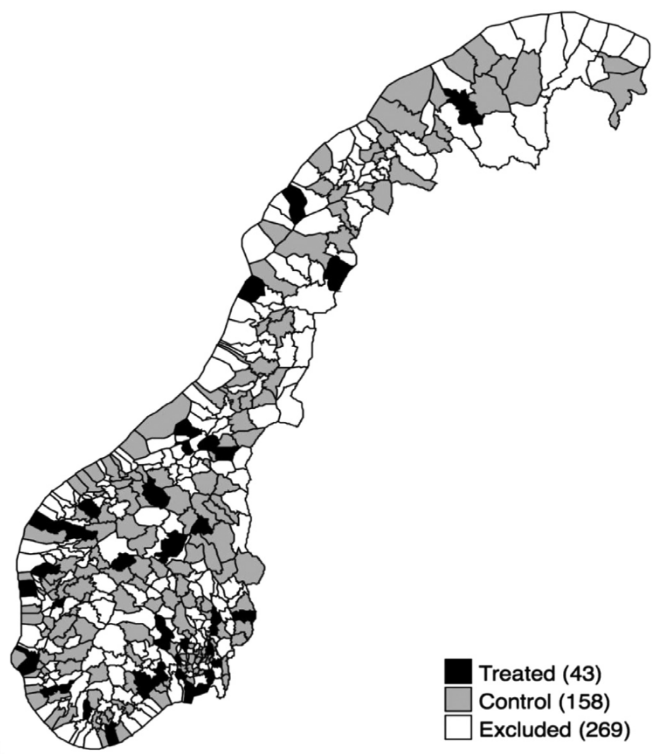

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Fig. A1. Treatment, control and excluded offices.

Table A1
Sample restrictions – survey data.

Number of social insurance districts in Norway	470
- Non-responding districts	−223
= Offices with returned surveys	247
- Missing time information	−32
- Cannot link office to individuals	−7
- Ambiguous policy change	−6
- Inconsistent information	−1
= Final sample	201
...of which:	
Treated	43
Control	158

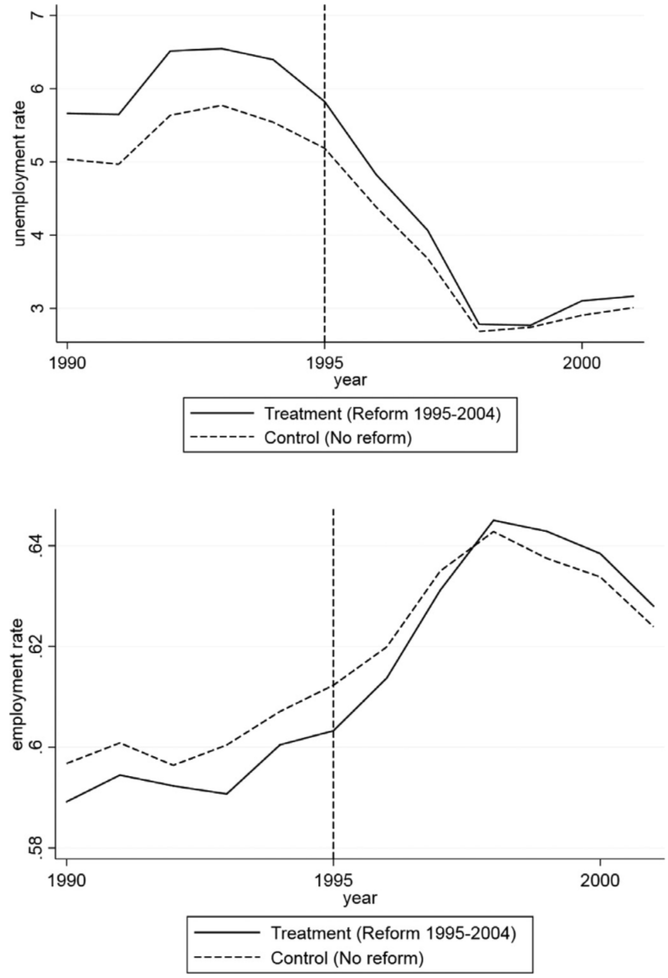

Fig. A2. Trends in unemployment (upper panel) and employment (lower panel) for 25–39 year-olds in treatment and control municipalities.
Note: Vertical dotted line indicates year of the first treatment in the treatment group. The unemployment rate is defined as registered unemployed persons as share of the population; the employment rate is defined as persons with contracted hours of at least 20 h/week as share of the population.

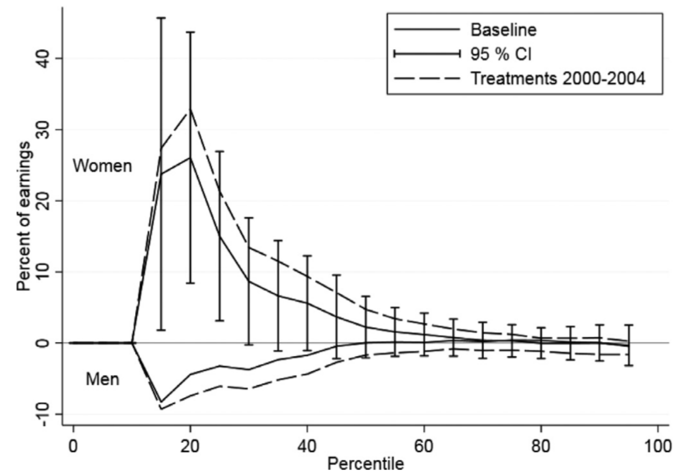

Fig. A3. Quantile treatment effects of policy changes taking place 2000–2004, 26–30 year olds.
Note: Standard errors are block bootstrapped with 499 replications with the municipality as the block. All specifications include office and cohort fixed effects, gender, share of population with tertiary education, average age of working age population and share of immigrants.

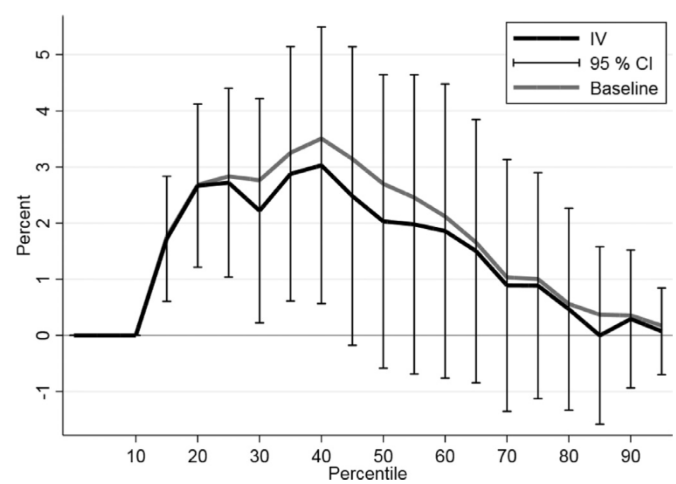

Fig. A4. Instrumental variables and baseline estimates of the effect of earning above 5th to 95th percentile, women, 26–30 year olds.
Note: All specifications include office and cohort fixed effects, gender, share of population with tertiary education, average age of working age population and share of immigrants. Standard errors are clustered at the municipality level.

Table A2
Main quantile treatment effects on earnings and robustness results, 26–30 year olds. In percent of earnings at each percentile.

Baseline					Distant pre-period		Incl. unempl.		Individ. cov.		Earnings at percentile.
Percentile	M	se	W	se	M	W	M	W	M	W	
0	0	0	0	0	0	0	0	0	0	0	0
5	0	0	0	0	0	0	0	0	0	0	0
10	0	0	0	0	0	0	0	0	0	0	0
15	−8,3	10,9	23,7	11,2	−0,3	31,1	−10,4	21,7	−13,9	26,0	2472
20	−4,4	9,2	26,1	9,0	1,1	32,2	−6,3	24,2	−9,1	27,7	7466
25	−3,3	5,9	15,0	6,1	0,7	18,9	−4,6	13,7	−6,3	15,9	13,245
30	−3,7	4,9	8,7	4,6	−1,3	11,2	−4,9	7,5	−5,7	9,1	18,903
35	−2,4	4,1	6,6	4,0	−0,4	8,6	−3,3	5,7	−3,8	6,9	23,981
40	−1,7	3,7	5,6	3,4	−0,1	7,2	−2,6	4,8	−2,9	5,8	28,752
45	−0,5	3,1	3,7	3,0	0,7	5,0	−1,1	3,0	−1,3	3,8	33,192
50	0,0	2,2	2,2	2,2	1,0	3,1	−0,5	1,7	−0,6	2,3	36,992
55	0,1	1,9	1,6	1,7	0,9	2,2	−0,3	1,2	−0,3	1,6	40,129
60	0,1	1,5	1,2	1,5	0,6	1,5	−0,3	0,9	−0,3	1,2	42,870
65	0,4	1,3	0,7	1,3	0,7	0,9	0,1	0,5	0,1	0,8	45,403
70	0,2	1,2	0,4	1,3	0,4	0,4	−0,1	0,1	−0,1	0,4	47,847
75	0,4	1,1	0,3	1,2	0,7	0,3	0,1	0,0	0,2	0,3	50,380
80	0,4	1,1	0,0	1,1	0,4	−0,2	0,1	−0,3	0,2	0,0	53,237
85	0,1	1,2	0,0	1,2	0,3	−0,2	−0,1	−0,3	−0,1	−0,1	56,820
90	0,0	1,2	0,0	1,3	0,2	−0,2	−0,2	−0,2	−0,1	0,0	61,665
95	−0,5	1,2	−0,3	1,5	−0,1	−0,4	−0,7	−0,5	−0,6	−0,4	70,036

Note: M and W indicate point estimates for men and women, respectively. Standard errors are block bootstrapped with 499 replications with the municipality as the block. All specifications include office and cohort fixed effects, gender, share of population with tertiary education, average age of working age population and share of immigrants.

Table A3
Main quantile treatment effects on earnings and robustness results, 26–30 year olds. Absolute amounts, €.

Baseline					Distant pre-period		Incl. unempl.		Individ. cov.	
Percentile	M	se	W	se	M	W	M	W	M	W
0	0	0	0	0	0	0	0	0	0	0
5	0	0	0	0	0	0	0	0	0	0
10	0	0	0	0	0	0	0	0	0	0
15	−205	268	587	277	−8	755	−256	536	−344	643
20	−330	690	1946	672	83	2375	−471	1806	−682	2070
25	−431	777	1990	805	87	2477	−614	1809	−828	2100
30	−708	920	1637	861	−247	2099	−931	1417	−1086	1722
35	−564	973	1590	950	−85	2049	−796	1360	−917	1659
40	−497	1062	1608	978	−23	2074	−741	1366	−828	1666
45	−154	1042	1219	994	246	1637	−377	998	−444	1264
50	11	811	824	815	378	1152	−178	637	−222	854
55	50	754	623	700	342	864	−113	462	−135	642
60	30	625	509	658	238	638	−115	365	−123	519
65	159	580	339	601	338	411	24	205	29	343
70	88	586	175	618	214	192	−43	44	−26	174
75	197	552	149	585	332	144	66	19	91	144
80	188	583	−13	586	228	−125	59	−141	86	−23
85	71	674	−24	679	174	−99	−57	−150	−31	−38
90	25	719	4	800	99	−96	−112	−132	−75	−18
95	−342	836	−231	1016	−63	−261	−494	−382	−446	−268

Note: M and W indicate point estimates for men and women, respectively. Standard errors are block bootstrapped with 499 replications with the municipality as the block. All specifications include office and cohort fixed effects, gender, share of population with tertiary education, average age of working age population and share of immigrants.

Table A4
Instrumental variable and baseline estimates of the effect of earning above 5th to 95th percentile, 26–30 year olds.

IV					Baseline			
Percentile	M	se	W	se	M	se	W	se
0	0	0	0	0	0	0	0	0
5	0	0	0	0	0	0	0	0
10	0	0	0	0	0	0	0	0
15	−1.06	0.63	1.72	0.57	−0.96	0.55	1.76	0.56
20	−0.77	0.75	2.67	0.74	−0.68	0.57	2.68	0.70
25	−0.74	0.85	2.72	0.86	−0.81	0.63	2.83	0.77
30	−1.29	1.07	2.22	1.02	−1.19	0.77	2.76	0.92
35	−1.10	1.26	2.88	1.16	−1.03	0.91	3.25	1.07
40	−1.15	1.40	3.03	1.26	−1.06	1.01	3.51	1.20
45	−0.86	1.50	2.48	1.36	−0.70	1.08	3.14	1.30
50	−0.71	1.54	2.03	1.33	−0.53	1.09	2.70	1.25
55	−0.68	1.54	1.98	1.36	−0.52	1.09	2.46	1.28
60	−0.48	1.49	1.86	1.34	−0.31	1.06	2.12	1.23
65	−0.14	1.45	1.50	1.20	0.02	0.99	1.65	1.13
70	−0.36	1.41	0.89	1.14	−0.07	0.99	1.03	1.08
75	−0.32	1.31	0.89	1.03	0.10	0.96	1.00	1.00
80	−0.19	1.18	0.47	0.92	0.23	0.90	0.56	0.91
85	−0.22	1.04	0.00	0.81	0.24	0.85	0.37	0.77
90	0.11	0.73	0.29	0.63	0.19	0.59	0.36	0.58
95	0.10	0.48	0.07	0.39	0.07	0.38	0.17	0.35

Note: Actual treatment instrumented by treatment status in the municipality of residence five years earlier. M indicates point estimates for men, W indicates point estimates for women. Baseline results are calculated at the same sample as the IV results. All specifications include office and cohort fixed effects, gender, share of population with tertiary education, average age of working age population and share of immigrants. Standard errors clustered at the municipality level.

Table A6
Country-wide representativeness of the results, 26–30 year olds.

Percent						Absolute		
Percentile	M	se	W	se	M	se	W	se
0	0	0	0	0	0	0	0	0
5	0	0	0	0	0	0	0	0
10	0	0	0	0	0	0	0	0
15	−2,0	6,7	13,9	6,1	−49	167	344	151
20	−1,5	8,5	22,0	8,3	−110	632	1641	621
25	−0,8	6,2	17,1	6,8	−109	820	2259	901
30	−0,6	5,7	12,3	5,0	−119	1075	2318	951
35	−0,8	4,5	9,9	4,2	−192	1080	2370	1006
40	−1,1	3,9	7,6	3,4	−326	1116	2194	984
45	−0,3	3,4	4,9	2,7	−83	1140	1620	886
50	−0,1	2,6	2,4	1,9	−23	966	903	720
55	−0,3	2,1	1,7	1,3	−127	835	689	540
60	−0,4	1,6	1,3	1,1	−167	687	539	460
65	−0,2	1,4	1,4	0,9	−86	650	616	410
70	−0,4	1,3	1,0	0,8	−175	625	494	382
75	−0,5	1,3	0,6	0,7	−234	653	319	375
80	−0,5	1,2	0,3	0,7	−247	632	151	392
85	−0,6	1,4	0,2	0,7	−329	809	133	425
90	−1,2	1,5	−0,3	0,9	−737	900	−204	580
95	−2,3	1,6	−1,0	1,0	−1635	1123	−691	727

Note: Estimated effects for offices without responses to the survey ("excluded group") between 1993 and 2005 relative to the control group. Estimated "treatment effects" for excluded group. M indicates point estimates for men, W indicates point estimates for women. Standard errors are block bootstrapped with 499 replications with the municipality as the block. All specifications include office and cohort fixed effects, gender, share of population with tertiary education, average age of working age population and share of immigrants.

Table A5
Distribution regression quantile treatment effects on earnings, 26–30 year olds.

Percent							Absolute					
P	M	95% CI	W	95% CI		M	95% CI		W		95% CI	
0	0	0	0	0	0	0	0	0	0	0	0	0
5	0	0	0	0	0	0	0	0	0	0	0	0
10	0	0	0	0	0	0	0	0	0	392	−105	888
15	0	−70,0	70,0	35,6	−33,1	104,3	0	−1731	1731	880	−818	2579
20	0	−26,0	26,0	30,7	2,9	58,5	0	−1938	1938	2291	216	4366
25	0	−15,7	15,7	17,5	2,1	32,9	0	−2085	2085	2316	276	4357
30	−5,6	−16,6	5,3	10,9	1,4	20,5	−1 066	−3138	1006	2068	261	3875
35	−4,1	−12,4	4,2	8,0	1,4	14,7	−982	−2980	1015	1928	331	3525
40	−3,2	−9,9	3,5	6,3	0,5	12,1	−921	−2848	1007	1819	145	3493
45	0	−4,0	4,0	2,4	−2,5	7,3	0	−1322	1322	798	−833	2429
50	0	−1,7	1,7	1,8	0,0	3,5	0	−640	640	650	−8	1309
55	0	−1,8	1,8	1,4	0,0	2,8	0	−715	715	569	16	1122
60	0	−1,3	1,3	1,2	−0,2	2,7	0	−572	572	522	−104	1148
65	0	−1,9	1,9	1,1	−0,8	3,0	0	−865	865	502	−342	1345
70	0	−1,5	1,5	0	−1,7	1,7	0	−709	709	0	−830	830
75	0	−2,0	2,0	0	−1,3	1,3	0	−1014	1014	0	−662	662
80	0	−2,0	2,0	0	−0,3	0,3	0	−1066	1066	0	−178	178
85	0	−1,7	1,7	0	0	0	0	−958	958	0	0	0
90	0	−0,7	0,7	0	0	0	0	−432	432	0	0	0
95	0	0	0	0	0	0	0	0	0	0	0	0

Note: Results obtained using the distribution regression method of Chernozhukov et al. (2013). M indicates point estimates for men, W indicates point estimates for women. Uniform confidence intervals. All specifications include office and cohort fixed effects, gender, share of population with tertiary education, average age of working age population and share of immigrants.

Table A7
Quantile treatment effects on total income, 26–30 year olds.

Percentile	Baseline,% M	W	Income	Baseline, € M	W
0	0.0	0.0	0	0	0
5	0.3	8.8	8775	25	772
10	0.0	6.2	15,519	7	962
15	−0.3	4.1	21,148	−56	873
20	−0.5	3.1	25,288	−124	778
25	−0.5	3.0	28,844	−135	851
30	−0.1	2.7	32,118	−19	854
35	0.4	2.5	35,281	124	873
40	0.2	1.9	38,117	59	714
45	0.3	1.7	40,631	113	701
50	0.3	1.3	42,921	110	541
55	0.2	0.8	45,046	71	367
60	0.2	0.8	47,116	73	368
65	0.2	0.4	49,183	75	215
70	0.2	0.5	51,352	125	270
75	0.1	0.4	53,782	28	233
80	0.0	0.4	56,658	19	250
85	0.0	0.7	60,297	18	434
90	−0.4	0.7	65,476	−235	444
95	−1.0	0.4	74,979	−731	319

Note: M and W indicate point estimates for men and women, respectively. Baseline results are in percent of income at each percentile. All specifications include office and cohort fixed effects, gender, share of population with tertiary education, average age of working age population and share of immigrants.

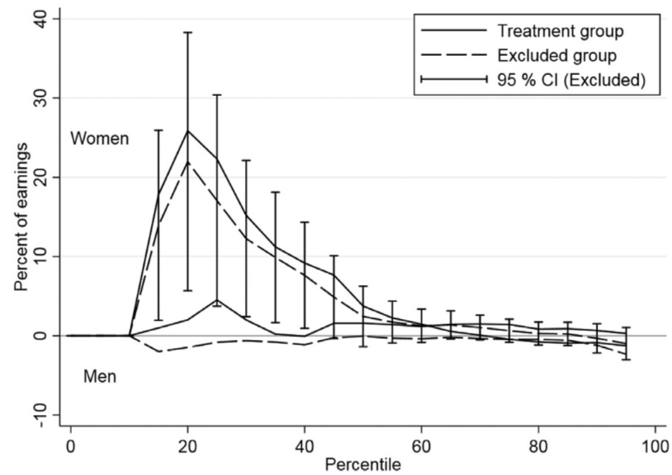

Fig. A5. Quantile treatment effect estimates on earnings for excluded municipalities, 26–30 year olds.
Note: Estimated effects for offices without responses to the survey ("excluded group") between 1993 and 2005 relative to the control group. Regressions include office and cohort fixed effects, gender, share of population with tertiary education, average age of working age population and share of immigrants. Standard errors are block bootstrapped with 499 replications with the municipality as the block. Vertical bars indicate 95% confidence intervals for the baseline results for the excluded group.

Table A8
Estimated effects at the household level, 26–30 year olds.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Households on welfare previous year				Households not on welfare previous year			
	Welfare	Earnings (cols 2–4)			Welfare	Earnings (cols 6–8)		
	hh.	hh.	wife	husband	hh.	hh.	wife	husband
ITT	−0.1400	4117	2280	1836	0.0009	413	377	−790
	(0.0541)**	(3373)	(2012)	(3102)	(0.0021)	(731)	(384)	(526)
Dep. var. mean	0.48	46,517	17,133	30,431	0.01	83,333	29,623	53,710
Number of observations	2957	2957	2957	2957	142,202	142,202	142,202	142,202

Note: Welfare uptake indicates receiving welfare some time during the year. Columns (1)-(4) estimated on people receiving welfare the previous year. Columns (5)-(8) estimated on people not on welfare the previous year. Earnings are yearly, measured in € at 2015 value. All specifications include office and cohort fixed effects, share of population with tertiary education, average age of working age population and share of immigrants. Standard errors are clustered at the municipality level. *(**)(***) indicates statistical significance at the 10(5)(1) percent level.

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