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Can Welfare Conditionality Combat High School Dropout?

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Abstract

Based on administrative data, we analyze empirically the effects of stricter conditionality for social assistance receipt on welfare dependency and high school completion rates among Norwegian youths. Our evaluation strategy exploits a geographically differentiated implementation of conditionality. The causal effects are identified on the basis of larger-than-expected within-municipality changes in outcomes that not only coincide with the local timing of conditionality implementation, but do so in a way that correlates with individual ex ante predicted probabilities of becoming a social assistance claimant. We find that stricter conditionality significantly reduces welfare claims and increases high school completion rates.

JEL classification: H55, I29, I38, J18
Keywords: Social assistance, activation, conditionality, welfare reform, school dropout

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1. Introduction

Can a conditionality regime designed to activate, counsel and monitor young welfare recipients play a role in reducing welfare dependency and promoting high school completion among vulnerable youths?

The large share of youths that do not complete high school is a concern in many developed countries; see, e.g. Lamb et al. (2011) and OECD (2013). Secondary education is to an increasing extent considered the basis, not only for further university or vocational education, but also for obtaining a stable foothold in the labor market. Dropout rates are particularly high among youths with socio-economically disadvantaged backgrounds, and probable consequences include high subsequent unemployment and low earnings (Rumberger and Lamb, 2003; Campolieti et al., 2010).

In this paper we analyze the effects on young people of being exposed to a more restrictive practice regarding social assistance claims. There has been an ongoing discussion in Norway of whether parts of the welfare system is too lenient, and in the late 1990s and early 2000s, many local social insurance offices – which traditionally have had a considerable discretion in the determination of policies regarding means-tested social assistance (welfare) – increased their use of such conditions. As we explain in more detail below, the types of conditions ranged from merely requiring claimants to attend counseling meetings with caseworkers to demand participation in fulltime activation programs. In some cases, they also required willingness to undertake a health examination and/or to document (or reduce) personal expenses. Most of the offices that changed policy did so in a quite comprehensive way, in the sense that they increased their use of several conditions simultaneously.

Conditionality can be viewed as a means to offset moral hazard problems embedded in income support programs, as well as a tool for helping in particular young people restraining their myopic selves. Procrastination in intertemporal effort choices is widespread, and a growing empirical literature indicates that many individuals discount the future with a bias toward the present (DellaVigna and Paserman, 2005; Paserman, 2008; Cockx et al., 2014). This implies that activities for which future benefits must be weighed against immediate costs – such as hard work at school – tend to be postponed repeatedly, even when it is optimal from a long term-perspective to get it done. When youths about to drop out from school show up at the social insurance office to seek alternative income support, a strict conditionality regime may in some cases be what is required to convince them to complete their education sooner rather than later.

Our empirical evaluation builds on administrative data, and in the main part of our analysis, we study the incidences of social assistance claims and high-school completion by the age of 21 for Norwegian youths born between 1972 and 1989. These outcome variables are coupled with survey-based information from local municipalities regarding *changes* in conditionality-practices from 1994 through 2004. Approximately half of the Norwegian municipalities provided information about the incidence, nature and timing of such changes. Identification of the causal effects of the changes builds on a before-after-comparison of outcomes, where we use people in municipalities that did not change practice – or changed practice at another point in time – as implicit controls. We do not rely on the standard common trend assumption, though, as we identify causality through the *interaction* between a conditionality-indicator (treatment) and a pre-determined individual social assistance propensity indicator. The intuition behind this strategy is as follows: If, say, the introduction of conditionality for social assistance payments actually had a positive effect on the local high-school completion rate, we should not only observe an increase in the local high-school completion rate, but we should see an increase that is disproportionally large for persons who had a high ex ante likelihood of becoming a social assistance claimant.

There are clearly challenges associated with this identification strategy also; the most important being that local introduction of conditionality may have been triggered by rising social assistance claims in the past, which even in the absence of policy interventions tend to be followed by “regressions toward the mean”. We return to this potential endogenous-policy problem and other threats toward our identification approach after having presented our main empirical strategy and results. The bottom line is that we find no evidence of policy endogeneity, and that our results are highly robust with respect to both the choice of pre-treatment (comparison) period, the way we allow for local (differentiated) trends, and a number of other modeling issues.

Our paper relates to a large existing literature documenting moral hazard problems in social insurance programs; see Krueger and Meyer (2002) for an overview of the literature, and Røed and Zhang (2003; 2005) and Fevang et al. (2015) for recent Norwegian evidence. It also relates to a fast-growing literature on the impacts of activation, monitoring, and sanctions in social insurance as well as welfare programs; see, e.g., Blank (2002), Moffitt (2007), and Røed (2012) for recent reviews. A consensus view coming out of this literature is that activation, as well as monitoring and sanctions, do tend to lower the public costs of providing transfer programs, both by reducing the number of claims and by reducing their average duration. Most of the papers also identify favorable effects on subsequent employment and earnings. A paper of particular relevance for our own contribution is Dahlberg et al. (2009) who investigates the impacts of mandatory activation programs for welfare recipients in Sweden, taking advantage of a gradual introduction of such programs in Stockholm. A key finding of their paper is that activation requirements improve employment and earnings prospects for young persons (aged 18-25) considerably, but have no, or even negative, effects on adults. We are not aware of existing research looking directly at the impacts of social assistance conditionality on high school completion.

Our paper also connects to a series of recent papes showing that educational outcomes can be improved by quite simple interventions like text message reminders (Castleman and Page, 2014; 2016), application facilitation (Bettinger et al., 2012) and information (Hoxby and Turner, 2013), and at-school coaching (Oreopoulos et al. 2014), which suggests that these decisions are not optimal to begin with. See Lavecchia et al. (2014) for an overview of the behavioral economics of education.

Why should social assistance conditionality affect high school completion? As we explain in more detail below, all adults in Norway (i.e., persons aged 18 years or more) who are unable to support themselves, are entitled to means tested social assistance. This represents an economic safety net for adolescents who quit school, but fail to find – or even genuinely search for – gainful employment. However, if economic support is provided conditional on, say, activation only, the alternative of living on welfare may become considerably less attractive, and the perceived risks associated with dropping out of school may become correspondingly larger. Previous evaluations of activation-oriented welfare reforms have indicated that the "screening effect" associated with conditionality is of particular importance. For example, implementation of the Personal Responsibility and Work Opportunity Act (PRWORA) in the US, which entailed the imposition of work-requirements for loan mothers, coincided with a significant drop in caseloads, primarily caused by reduced entry (Moffitt, 2007). In the context of social assistance conditionality for Norwegian youths, we may hypothesize that any positive impacts on high school completion will be mirrored by correspondingly negative impacts on the frequency of welfare claims. In addition, it is possible that some of those who claim welfare despite the stricter use of conditionality are pushed/coerced back to school by the activities implied by the conditions.

Our empirical findings indicate that when a local insurance office increases their use of conditionality, welfare claims among 21-year olds in that area decline substantially, while high school graduation rates increase. For example, for the quarter of individuals estimated to have the highest propensity to receive welfare, the incidence of welfare reception falls by more than 3.1 percentage points, while the high school graduation rate increases by 2.8 percentage points. We also find evidence that the favorable effects of conditionality persist and contribute to higher educational attainment, higher labor earnings, and lower transfer dependency at age 25. The primary mechanism appears to be that conditionality speeds up high school graduations that otherwise would have taken place later on, and thus motivates a smoother and earlier transition from school to work.

2. Institutions and data

According to Norwegian legislation, persons who are neither able to support themselves through work nor covered social insurance programs, are entitled to means-tested social assistance from their municipality. The probability of claiming social assistance during a calendar year peaks at a level close to 7 % by age 20-21, after which it declines monotonously with age; see Figure 1. The high claim rates at age 20-21 are driven by a combination of relatively high rates of unemployment during the school-to-work transition phase, and low levels social insurance coverage; the latter because social insurance entitlements require past work experience and social security contributions.

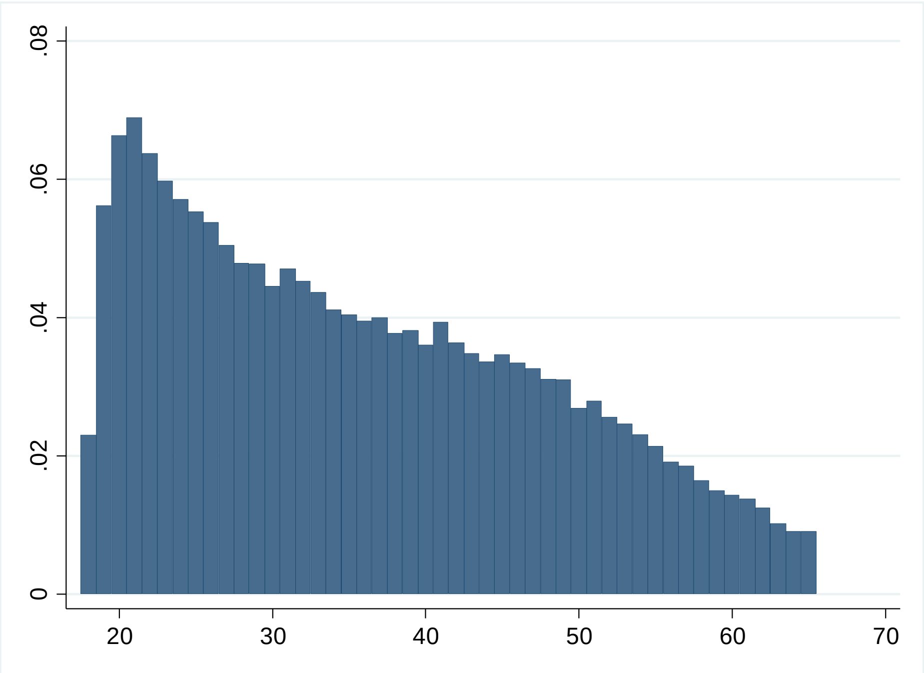

Figure 1. Fraction receiving social assistance (welfare) by age in 2011

The legislation implies that local authorities cannot refuse to help persons in true need. They can set conditions, however, for example in the form of work requirements, provided that the conditions are not disproportionate or unreasonable.[1] Until now, the municipalities have had ample room for discretion regarding the use of such conditions, and the practices have varied a lot across the country.[2] In 2006, Telemark Research Institute (TRI) published a report on the Norwegian system of means-tested social assistance (Brandtzæg et al., 2006). As part of this work, the authors administered a survey to all local social insurance offices in Norway, asking, *inter alia*, about changes during the last 10 years (1994-2004) in the offices' practices regarding the use of conditions for receiving social assistance. It is the answers to these ques-

[1] Lov om sosiale tjenester i arbeids- og velferdsforvaltningen (Sosialtjenesteloven), §§ 18-20.

[2] New legislation implies that activation requirements soon will become compulsory for social assistance claimants who are deemed able to work.

tions that form the basis for identification of the *treatments* evaluated in this paper. Based on the social insurance district of residence at age 21, we match the treatment data to population-based administrative registers containing information about individual social assistance claims, educational and labor market outcomes, as well as a large range of (family) background characteristics for all persons born between 1972 and 1989.

In total 247 of the 470 local insurance offices (located in 433 municipalities) existing in 2005 returned the TRI-survey. Out of these, 46 offices could not be used by us due to missing information about timing, ambiguity with respect to the direction of changes, inconsistent information, or lack of link to individuals (due to multiple offices in the same municipality); see Table 1. Hence, our analysis builds on information from 201 social insurance districts (municipalities), covering roughly 60 % of the Norwegian population in the relevant birth cohorts. Out of these, 43 unambiguously *increased* their use of conditions at some time, and 158 maintained status quo. It is notable that none of the social insurance offices unambiguously *reduced* their use of conditionality. To obtain a better idea on the geographical distribution of the 43 treatment and the 158 control municipalities, Figure 2 provides a map of Norway where the treatment and control municipalities are highlighted. As one can see, both treatment and control municipalities are scattered across the country.

Table 1: Sample restrictions – social insurance districts

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

Despite the lack of geographical concentration, the fact that we can use data from less than half of the Norwegian municipalities does raise questions about generalizability. In Table 2, we show descriptive statistics for three groups of municipalities; those who did not reply and for that reason are kept out of the analysis, those who replied and did not change their policies – which will serve as the control group in the analysis – and those who replied and changed their policies – which constitutes our treatment group. For each group we present descriptive statistics for two years, 1993 and 2010, that are clearly on opposite sides of any policy change. The socioeconomic characteristics, as well as their developments, are similar for the three municipality types. It is notable, however, that the fraction receiving welfare benefits declined most in the treated municipalities and least in the control municipalities.

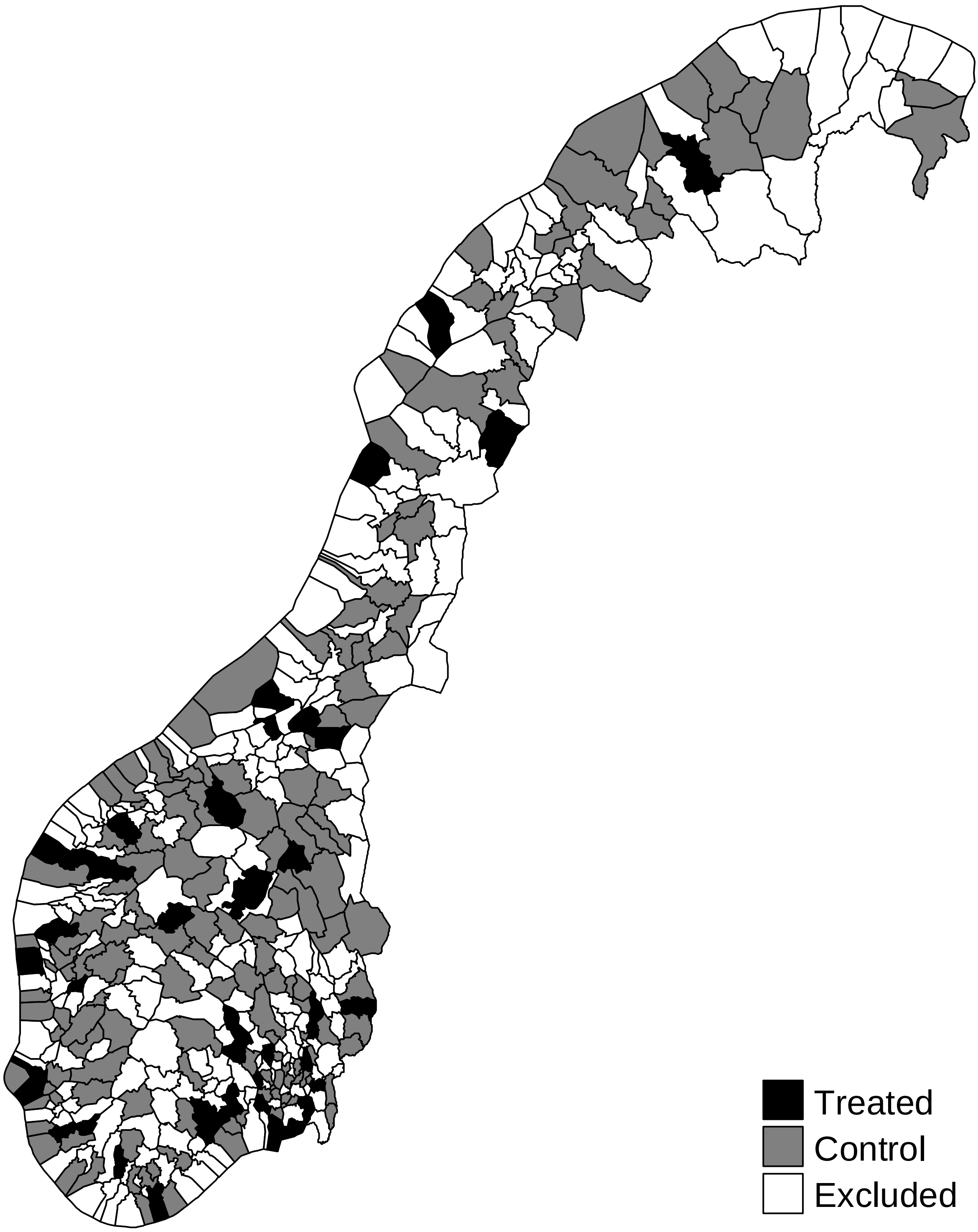

Figure 2: Treatment and control municipalities

Table 2: Municipality characteristics in excluded, control and treated municipalities.

	Excluded municipalities (n= 178)		Control municipalities (n=158)		Treated municipalities (n=43)	
	1993	2010	1993	2010	1993	2010
Inhabitants	11,674	13,409	7,207	7,828	10,392	11,831
Employment rate	0.66	0.71	0.67	0.71	0.65	0.70
Mean income (1,000 NOK, inflated to 2015 value; see note below)	381	431	362	407	354	400
Fraction with tertiary education	0.23	0.34	0.18	0.28	0.17	0.26
Fraction with at least secondary education	0.47	0.66	0.42	0.63	0.41	0.62
Fraction receiving welfare benefits	0.027	0.018	0.021	0.015	0.027	0.016
….below age 30	0.039	0.023	0.033	0.023	0.041	0.024
Fraction receiving disability benefits	0.085	0.107	0.087	0.119	0.090	0.122
Unemployment rate	0.044	0.025	0.040	0.022	0.046	0.025

Note: All variables refer to the age group 18-61 years, and reported means are weighted by population size. Income levels are measured in 1000 NOK, inflated to 2015-value with the adjustment factor used in the Norwegian pension system (approximately corresponding to the average wage growth).

The policy shifts toward stricter conditionality were conducted in different calendar years with a majority of the reforms taking place toward the end of the 1994-2004 period (see Appendix, Table A1 for details). This probably reflects an increasing concern about rising welfare expenditures and a general shift toward more emphasis on activation in social policies; see, e.g., Gubrium et al. (2014).

The TRI-survey distinguished 9 different condition-types. These are described in Table 3, together with an overview of their frequencies in the 43 social insurance offices which implemented at least one of them. On average, the reforming social insurance offices (municipalities) reported to have changed 4.14 such policies at the same time. The four most common conditions used are (i) a requirement of documenting expenses (29 cases), (ii) requirement to participate in a program typically involving work or training (26 cases), (iii) requirement to participate in general counselling (26 cases), and (iv) a requirement to register as an active job seeker (25 cases).

The TRI-report (Brandtzæg et al., 2006) also contains transcriptions of interviews with caseworkers, explaining in more detail why and how conditionality has been used in practice. The interviews indicate that the conditions have first and foremost been used for young clients (below 25 years of age), with a focus on preventing them from starting their potential labor market career as welfare clients. In many cases, the conditions are designed such that they are effective immediately, e.g., by requiring applicants to show up at some structured activity already the following morning. This potentially induces some “second thoughts” about a life on welfare and thus generates a “threat effect” of the type reported by Black et al (2003). And for those who choose to satisfy the conditions, the activities may represent a greatly needed element of structure in the daily life.

Table 3. Policies and conditions changed, conditional on at least one policy change

Activation and work requirements	Number of municipalities	Fraction of treated
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 either 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
Health		
Health examination: Willingness to undertake a health examination.	14	0.33
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: Restriction 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
Total number of conditions changed	175	
Total number of municipalities changing policy	43	

Note that we do not exploit information about the use of conditions in the control municipalities, except that they did not change policy between 1994 and 2004. We are thus not going to compare municipalities with and without conditions in this paper, but focus exclusively on the way *changes* in outcomes coincide with *changes* in conditionality. Note also that we do not have any information about policy changes occurring *after* 2004; hence any such changes will be disregarded in our main statistical analysis. In a more recent survey of Norwegian municipalities (Proba Research, 2013), it has been shown that the trend toward more intensive use of conditions continued after 2004. Hence, it is likely that the group of control municipalities become increasingly “contaminated” by unobserved treatments toward the end of our observation period (2010). We return to this issue in Section 4 below.

Given the apparent large differences in *content*, we would clearly have liked either to evaluate the impacts of different condition-types separately, or to evaluate alternative “reform packages”. However, due to the simultaneity in the implementation of the various conditions and the large number (37) of condition-combinations actually observed, this is simply not doable. In the main part of our analysis, we are therefore going to use the implementation of new condition(s) as a single dichotomous treatment variable. The treatment indicator thus reflects that the local social insurance administration has taken deliberate – and in most cases several – steps to tighten the conditions for paying out social assistance. In as much as 95 % of the treatment-cases, an activation-conditionality was included in the “reform package”. In a supplementary analysis, we also provide separate partial effect estimates for each of the three main types of conditions; i.e., activation related, health related, or personal-economy related, respectively.

Apart from the survey data covering the social insurance office policies, the data used in this paper all stem from administrative registers covering the complete Norwegian population. In our main analysis, we study outcomes for 21-year olds who at that age resided in either one of the control- or treated municipalities. We include in the dataset the cohorts born between 1972 and 1989, who turned 21 years in the years between 1993 and 2010. Since the actual timing of the policy shift within a year is unknown to us we have chosen to exclude the reform-year cohort in the treated municipalities. The data also contain links between children and parents, making it possible for us to include information about the children's parents, including their earnings, country or origin, age and education.

Our main outcomes of interest are social assistance (welfare) reception and high school completion by individuals' 21st year (the standard/normal age of completion is 19). Some descriptive statistics are shown in Table 4. In a follow-up analysis toward the end of the paper, we also examine various labor market and education outcomes at age 25.

Table 4. Descriptive statistics for estimation sample

	Mean	SD
Outcomes		
Welfare uptake at age 21	0.076	
Completed high school by age 21	0.689	
Background characteristics		
Fraction female	0.483	
Parental income, mean over child's age 0-9, 1000 NOK (2015-value)		
…Father	507	206
…Mother	154	148
Parental education, when child is 10 years		
…Father has college degree	0.213	
…Father has high-school	0.513	
…Mother has college degree	0.190	
…Mother has high-school	0.471	
Nationality background		
…Native	0.880	
…Western Europe or North America	0.083	
…Rest of the world	0.037	
Calendar year turning 21	2001.4	5.360
Treated by age 21	0.119	
Local unemployment rate at age 21	0.040	0.026
Number of observations	362,687	
Number of municipalities	201	

3. Empirical analysis

In this section, we set up and estimate statistical models aimed at identifying the causal effect of social assistance conditionality on the probability of actually receiving social assistance during the calendar year in which persons become 21 years, and on the probability of having completed high school by that age.

Within our data window just about 7 % of the adolescents received social assistance during the calendar year they turned 21 years. Stricter conditions for welfare benefits are thus likely to have negligible impacts on the majority of youths, and any causal effects can be expected to be larger the more exposed a person is to the risk of becoming a welfare claimant in the first place. This argument is going to play a key role in our identification strategy. The first step of this strategy is thus to identify individual "exposure risks", based on pre-determined parental characteristics only. In a second step, we interact the predicted propensities with time-varying indicators of conditionality-reform. Intuitively, for a local shift in individual outcomes to be interpreted as causally related to the introduction of conditionality, it is not sufficient that the shift is larger in reforming than in non-reforming municipalities; the differences also needs to be positively correlated to individual predicted exposure risks. Our empirical strategy is similar to the approach used by Markussen and Røed (2015) to evaluate another social program with a small, but imperfectly identifiable, target group.

3.1. Auxiliary regression analysis: The propensity of welfare uptake at age 21

We start out by estimating the propensity of welfare uptake at age 21, based on pre-determined family background characteristics only. To do this we construct a similar dataset as the one used in the main analysis (and described in the previous section), but containing only the 1971 birth-cohort in the treatment and control municipalities; i.e., the last birth-cohort *not* used in our causal analysis (23,852 observations). We then set up a logit regression model with an indicator model for welfare receipt at age 21 (during the calendar year of the 21st birthday) as the dependent variable and a vector of family background characteristics $\mathbf{b_i}$ as explanatory variables. The vector of explanatory variables includes both parents' education at the offspring's age 10 (4 categories for each parent) and their respective cumulative earnings between the offspring's ages 0 and 10. In addition we include dummy variables for parents' country of origin (7 categories). The results from this regression show that family background characteristics are powerful predictors for later social assistance claims; see the Appendix, Table A2, for details. We can thus use these results obtained for the 1971-cohort to make out-of-sample predictions for the 1972-89 cohorts used in our causal analysis. That is, we compute a welfare propensity score $\hat{p}_i$ as

$$\hat{p}_i = \frac{\exp(\mathbf{b}_\mathbf{i}'\hat{\boldsymbol{\pi}})}{1+\exp(\mathbf{b}_\mathbf{i}'\hat{\boldsymbol{\pi}})}, \tag{1}$$

where $\hat{\boldsymbol{\pi}}$ is the vector of parameter estimates (including a constant term) from the 1971-cohort welfare claim regression.

To illustrate the empirical relevance of these predictions for the 1972-89 cohorts used in the causal analysis, we have divided the members of these cohorts into four quartiles, based on their position in the distribution of $\hat{p}_i$, and present in Table 5 descriptive statistics separately for each quartile. A first point to note is that the predicted welfare propensities quite nicely matches the actually realized claims, although actual welfare propensity tended to be somewhat smaller than predicted in all quartiles. A second point to note is how strikingly different family backgrounds persons in the different quartiles tend to have. For example, the likelihood of having a father with a college degree is 31 times higher in the first than in the fourth quartile, whereas the likelihood of having parents who immigrated from a non-western country is 12 times higher in the fourth quartile than in the first.

Table 5. Descriptive statistics by quartile in the predicted welfare propensity distribution. 1972-89 birth cohorts

	Q1	Q2	Q3	Q4
Mean predicted welfare propensity, based on 1971-cohort	0.030	0.057	0.086	0.163
Outcomes				
Welfare uptake at age 21	0.026	0.048	0.078	0.154
Completed high school by age 21	0.849	0.749	0.657	0.502
Background characteristics				
Mean parental income (1000 NOK, 2015-value)				
..Father	698	523	456	349
…Mother	228	168	137	90
Parental education				
…Father with college	0.658	0.118	0.054	0.021
…Father with high school	0.325	0.765	0.600	0.361
…Mother with college	0.539	0.151	0.050	0.019
…Mother with high school	0.442	0.779	0.560	0.102
Nationality				
…Native	0.916	0.924	0.897	0.784
…Western Europe or North America	0.075	0.064	0.083	0.110
…Rest of the world	0.009	0.012	0.020	0.106
Number of observations	90,707	90,688	90,662	90,630

The predicted welfare propensities $\hat{p}_i$ can be used to illustrate how pure changes in the population composition have (or have not) contributed to changes in welfare claims over the cohorts used in the causal analysis. Figure 3 shows the $\hat{p}_i$-values for the 1st, 5th, 10th, 25th, 50th, 75th, 90th, 95th and 99th percentile in the distribution of welfare uptake propensity each cohort/year in the group of treatment and control municipalities. With some exceptions at the very highest percentiles, the figure indicates parallel trends in the treatment and control municipalities. As one can see from panel (a), the predicted claim propensity in the ten upper percentiles increased much more in the treatment municipalities than in the control municipalities. The reason for this is that the fraction of 21 year olds with non-native parents increased more in the treated municipalities, and these youths have a higher predicted welfare uptake. When we focus on natives only in panel (b), we see that the composition of treatment and control groups follow a similar pattern at all percentiles.

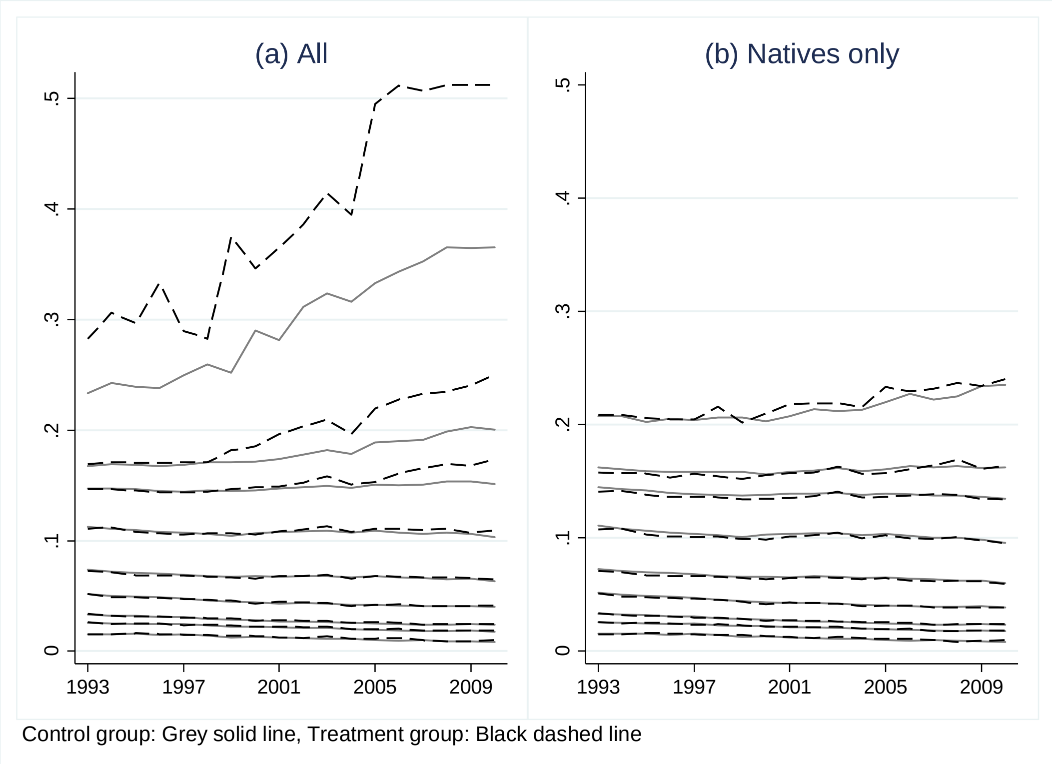

Figure 3. Predicted propensity of welfare uptake $(\hat{p}_i)$ over time in treatment and control municipalities.
Note: The graph draws the 1st, 5th, 10th, 25th, 50th, 75th, 90th, 95th and 99th percentile in the distribution of predicted welfare propensities within each year and within treatment and control municipalities.

3.2. Causal regression analysis: The effects of welfare conditionality

In this section, we examine the impacts of welfare conditionality on individual indicator variables for welfare claims and high school completion, respectively, both measured at age 21. The basic idea of our empirical strategy is to assess whether there is a tendency for outcomes to shift in response to the introduction of conditionality in a way that correlates with predicted propensity of welfare uptake $\hat{p}_i$. Before we turn to the formal regression analyses, we provide a simple graphical exposition of how this identification strategy plays out in the data.

Figure 4 presents (calendar year adjusted) average outcomes for the 10 deciles in the $\hat{p}_i$-distribution divided into three groups: i) the treatment group before the policy shift, ii) the treatment group after the policy shift, and iii) the control group.

Starting out with panel (a), showing welfare uptake at age 21, we see that the three groups are almost identical for the first six deciles in the predicted welfare propensity distribution. However, for the four uppermost deciles, the treated municipalities had a substantially higher welfare uptake before than after the policy shift. A similar picture can be seen in panel (b) showing high school completion by age 21. In the lower seven deciles in the predicted welfare propensity distribution we can hardly see any differences, whereas for the upper three deciles there is a clear shift towards higher completion rates after the policy shift.

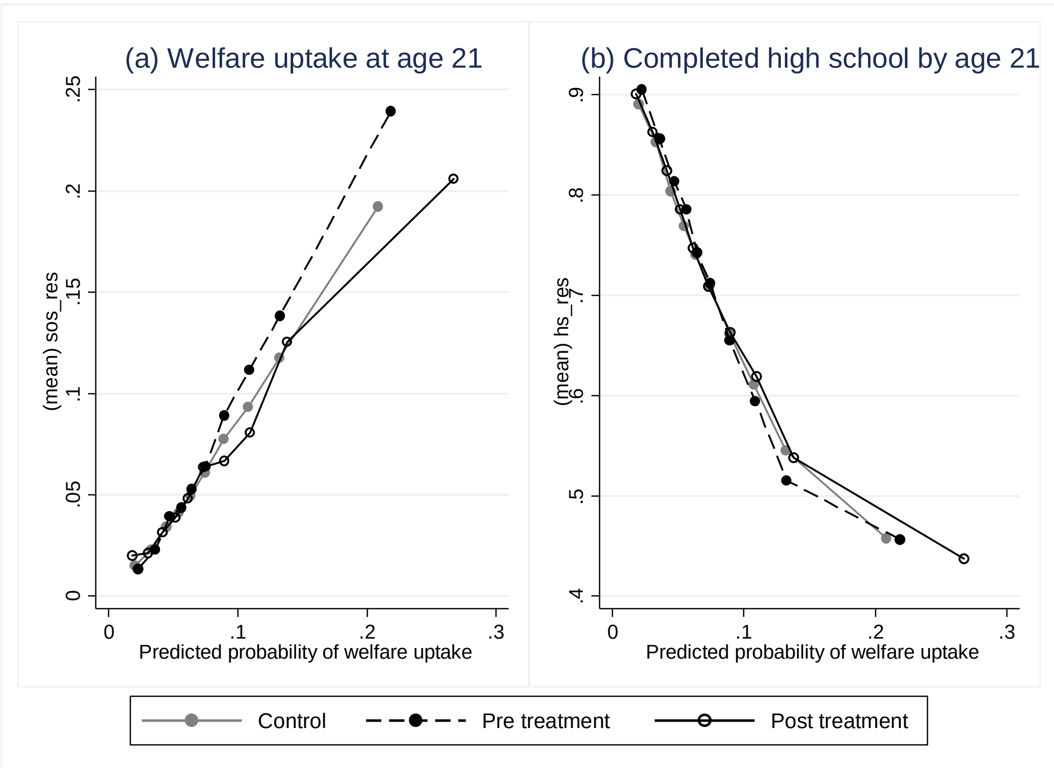

Figure 4. High school completion and welfare uptake before and after treatment
Note: Outcomes have been calendar-year-adjusted by regressing them on calendar year dummy variables, obtaining the residuals, and then adding a constant term such that the outcomes are measured in 2000-levels.

We now turn to the formal regression analysis. Let y_{it} denote the outcome of interest for person i measured in calendar year t and let C_{mt} be an indicator variable equal to 1 in treatment municipalities in all years strictly after the introduction of conditionality and otherwise zero (we drop from the analysis all outcomes measured in the same year as a reform, since in these cases we do not know whether claims were made before or after the introduction of conditionality). In treatment municipalities it will thus be the case that all persons with $C_{mt}=1$ have been exposed to the new conditionality regime *at least* one year at age 21, whereas persons with $C_{mt}=0$ had not been exposed to it at all. We always cluster standard errors at the 201 municipalities. Furthermore, let $\mathbf{x_i}$ be a vector of individual covariates including family background characteristics $(\mathbf{b}_i)$ and gender (see a complete list in Table A3 in the appendix), and let u_{mt} be the municipality-specific unemployment rate in year t. We start out with a simple difference-in-difference (DiD) design, and estimate linear probability models with the following structure

$$y_{it} = \mathbf{x}_\mathbf{i}'\boldsymbol{\beta} + \lambda_m + \sigma_t + \rho u_{mt} + \theta C_{mt} + v_{it}, \tag{2}$$

where (λ_m, σ_t) are municipality and time fixed effects, respectively, and v_{it} is a residual. The coefficient of interest is the intention to treat (ITT) effect θ, which captures the extra shift – over and above the general changes captured by the year fixed effects – occurring in treatment municipalities after the introduction of conditionality. The resultant estimates of θ are provided in Table 6, Column (1), and indicate that the introduction of conditionality reduced the probability of welfare uptake at age 21 by 1.4 percentage points and raised the probability of high school completion by 0.8 percentage points. These affects appear small, and the latter of them is statistically indistinguishable from zero. Model (2) is not particularly informative, however, since it examines an intention to treat effect on a population in which the majority is almost certain to have been unaffected by the treatment; i.e., youths for which social assistance is not a relevant alternative regardless of conditionality regime. As discussed above, given that there are causal effects of conditionality, we would expect them to be larger the larger is the *ex ante* probability of being exposed to it.

To investigate this further, we estimate Equation (2) separately for each of the quartiles in the distribution of predicted welfare propensities $\hat{p}_i$. The results from this exercise are displayed in Table 6, Columns (2)-(5). As expected, we find no effects in the first quartile, and then steadily increasing effects as we move up in the welfare propensity distribution. In the upper quartile, we estimate an ITT effect of conditionality equal to -3.5 percentage points for welfare claims and +2.5 percentage points for high school completion.

Given that conditionality becomes more relevant as we move upwards in the distribution of predicted welfare propensities $\hat{p}_i$ and that its impacts are negligible in the first quartile, we can exploit this property directly as a foundation for a triple difference (3D) identification strategy. We can then also allow for other sources of geographically differentiated calendar time developments by substituting municipality-by-year fixed effects for the separate municipality and year fixed effects used in Equation (2). Let $q = 1, ..., 4$ denote quartile in the $\hat{p}_i$ distribution. The 3D-estimator is then based on a regression equation of the following form:

$$y_{it} = \mathbf{x}_{\mathbf{i}}^{'}\boldsymbol{\beta} + \psi_{mt} + \sum_{q=2}^{4} q(\lambda_{qm} + \sigma_{qt} + \rho_q u_{mt} + \theta_q C_{mt}) + v_{it}, \qquad (3)$$

where ψ_{mt} are now municipality-by-year fixed effects, and $(\lambda_{qm}, \sigma_{qt})$ are additional municipality and year fixed effects relevant for persons in $\hat{p}$-quartile *q (q=2,3,4)*. With this approach, we estimate the intention to treat (ITT) effects in quartiles 2-4 as the “extra” difference in difference that arises in each of these quartiles compared to the first quartile. The results are reported in Table 6, Column (6), and visualized in Figure 5. Given that the unrestricted estimated effect in the first quartile was close to zero anyway (see column (2)), it is no surprise that they are similar to the separately estimated effects reported in Columns (3)-(5).

Table 6. Main results. Estimated intention to treat (ITT) and average treatment effects on the treated (ATET) of welfare conditionality (standard errors in parentheses).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable: Welfare uptake at age 21							
ITT all	-0.014** (0.006)						
ITT quartile 1		-0.001 (0.004)				-	
ITT quartile 2			-0.006 (0.005)			-0.006 (0.005)	
ITT quartile 3				-0.018** (0.009)		-0.019** (0.007)	
ITT quartile 4					-0.035** (0.014)	-0.031** (0.012)	
ATET							-0.241*** (0.065)
Mean of dependent variable	0.08	0.03	0.05	0.08	0.15	0.08	0.08
Dependent variable: Completed high school by age 21							
ITT all	0.008 (0.007)						
ITT quartile 1		-0.007 (0.007)				-	
ITT quartile 2			0.001 (0.010)			0.006 (0.008)	
ITT quartile 3				0.017* (0.009)		0.020** (0.010)	
ITT quartile 4					0.025** (0.012)	0.028** (0.011)	
ATET							0.160*** (0.059)
Mean of dependent variable	0.69	0.85	0.75	0.66	0.50	0.69	0.69
Number of observations	362,693	90,706	90,691	90,660	90,636	362,693	362,693

Note: Quartiles relate to individual predicted welfare propensity as reflected in the $\hat{p}$ distribution. Standard errors are clustered at the 201 municipalities. *(**)(***) indicates statistical significance at the 10(5)(1) percent level.

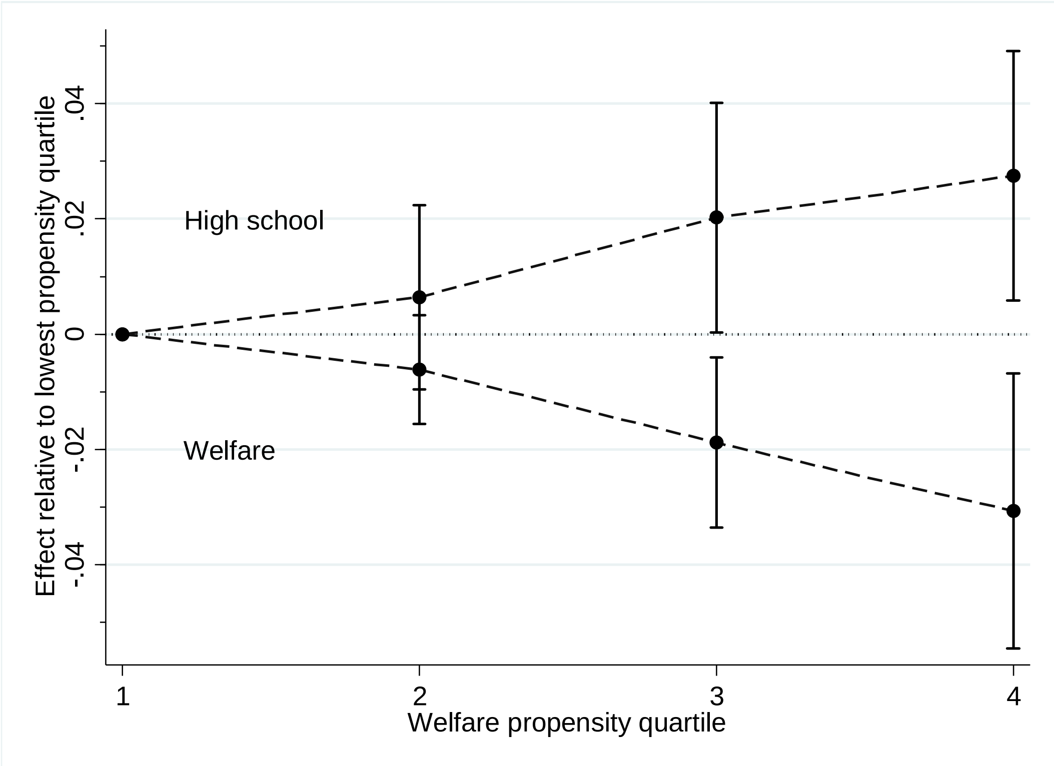

Figure 5. Triple difference estimates of the effect of stricter welfare conditionality.
Note: Estimates from column (6), Table 6. Vertical lines denote 95 % confidence intervals.

What all the estimates presented so far have in common is that they measure the intention to treat effect on a group of persons for which the treatment in question may or may not be relevant. The magnitudes of such effects depend on two factors: i) the fraction of persons actually exposed to the treatment (in our case, the fraction of persons who would claim social assistance in at least one of the regimes) and ii) the average size of the effect for these persons. It is obviously of interest to disentangle these two factors, and thus arrive at estimates that can be interpreted as something akin to the average treatment effect on the treated (ATET). Since we do not observe the individual treatments in our case (the effect does clearly not operate through persons actually claiming social assistance only), we cannot use a standard instrumental variables approach. What we can do, however, is to measure the estimated effects relative to the individual predicted welfare propensity indicators $\hat{p}_i$. With some abuse of language, we refer to this as the ATET.

The regression model can be written as

$$y_{it} = \mathbf{x}_{\mathbf{i}}^{'}\boldsymbol{\beta} + \psi_{mt} + \lambda_{pm} I_m \hat{p}_i + \sigma_{pt} I_t \hat{p}_i + \rho_p \hat{p}_i u_{mt} + \theta_p \hat{p}_i C_{mt} + v_{it}, \tag{4}$$

where (I_m, I_t) are indicator variables for municipality and year, respectively. Hence, in this model, we control for common municipality-by-year fixed effects as well as separate effects of social assistance propensity $\hat{p}_i$ for each municipality and for each year. Hence, it is only the "extra" association between $\hat{p}_i$ and outcomes that show up in treatment municipalities after the introduction of conditionality that identifies the causal impact (ATET) θ_p.

The resultant treatment effects are provided in Table 6, Column (7). Taken at face value, the results indicate that for a youth who would have received social assistance with certainty in the absence of treatment, the introduction of conditionality reduced the claim probability by 24 percentage points and increase the school completion probability by 16 percentage points.

As explained in Section 2, the conditionality reforms involved a number of different elements; conf. Table 3. While almost all of the reforms involved some form of activation requirements, their actual contents differed and they were to varying degrees combined with requirements regarding personal economy and/or health examination. While it is not sufficient variation in the data to either evaluate each condition separately or to evaluate all the different combinations, we have made an attempt to evaluate the partial impacts of the three main conditioning types. Table 7 reports the resultant average treatment effects (ATET). The point estimates based on this specification suggest that the activation- and work-related requirements and the requirement that recipients undertake a health examination are the most important and have comparable effects, while conditions regarding the personal economy are of minor importance at the margin. However, the high correlation between the different conditioning types implies that the effects are estimated with great statistical uncertainty.

Table 7. Estimated effects of conditionality. Partial effects by category of requirement

	(1)	(2)
Requirement type	Welfare uptake	High school completion
Activation and work	-0.187*	0.127
	(0.100)	(0.126)
Health	-0.194	0.139
	(0.123)	(0.135)
Economic	-0.019	-0.018
	(0.128)	(0.131)
Dep. var. mean	0.08	0.69
Number of observations	362,693	362,693

Note: Standard errors are clustered at the municipality level.
*(**)(***) indicates statistical significance at the 10(5)(1) percent level.

4. Specification issues and robustness

In this section, we take a closer look at the assumptions behind our identification strategy, and also evaluate our findings' robustness with respect to various model specification issues.

4.1. Policy endogenity

A possible concern is that the kind of policy changes studied in this paper were introduced as a response to recent events in the municipality, such as increasing welfare payments or high influx of immigrants in need of economic support. A period with increasing caseloads may often be followed by mean reversion, which may then be misinterpreted as an effect of the policy changes. Although our identification strategy does not rely on the absence of mean reversion in general (since we include municipality-by-year fixed effects), it is conceivable that endogenous policy reforms may somehow disturb the $\hat{p}_i$-specific time-developments used to identify causality in our model. We address this concern in two ways. First, we examine the evidence for policy endogeneity by estimating models where the introduction of conditionality is used as the dependent variable. And second, we re-estimate our causal model based on the use of alternative pre- and post-reform time periods.

To assess the empirical evidence for reform endogeneity, we construct a dataset on the municipality-by-year level for all the municipalities included in the control and treatment group and regress the reform indicator on a vector of variables reflecting the current/recent demand for social assistance in the municipality. Specifically we measure the fraction of working age population (18-61 years) receiving welfare benefits and the fraction that is unemployed. We also include the fraction of youths (18-30) that receive welfare benefits. Finally we include the fraction of the population age 18-30 that has an immigrant background.[3] Since a municipality is only allowed to tighten their policy once, we censor the years after such a policy change as the dependent variable then is predetermined to equal zero. We estimate the model with contemporaneous covariates as well as 1-3 lags. All models are estimated using municipality and year fixed effects. The results are shown in Table 8.

[3] Either born in a foreign country or with parents born in a foreign country.

Table 8. Test for reform endogeneity

Dependent variable: Reform in year *t*	(1)	(2)	(3)	(4)
Covariates measured in	*t*	*t-1*	*t-2*	*t-3*
Fraction receiving welfare benefits	-0.118	-0.855	-0.899	-1.253
	(0.888)	(0.943)	(0.999)	(1.067)
Fraction of population aged 18-30 receiving welfare benefits	-0.303	0.254	0.469	0.745
	(0.483)	(0.513)	(0.544)	(0.580)
Unemployment rate	-0.299	-0.110	-0.627	-0.371
	(0.350)	(0.375)	(0.404)	(0.429)
Fraction of population aged 18-30 that is non-native	-0.024	-0.021	-0.097	-0.229
	(0.108)	(0.119)	(0.133)	(0.151)
Joint F-test	0.75	0.30	1.04	1.17
(p-value)	0.560	0.876	0.387	0.320
Municipality FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Number of observations	3280	3079	2878	2677

Note: Standard errors are clustered at the municipality level. *(**)(***) indicates statistical significance at the 10(5)(1) percent level.

The results in Table 8 indicate that neither the uptake of welfare benefits nor the unemployment rate in the municipality explain the policy shifts. None of the estimated coefficients are statistically significant at any conventional level, nor are they jointly significant. We have also estimated these models using differences instead of levels, but this does not change the results in Table 8, qualitatively, at all.

As a further check on the results' sensitivity with respect to pre-reform changes in social assistance claims, we also estimate our main causal model without including observations from the last three years prior to reform in treated municipalities. This hardly changes the results at all; confer Table 9.

Table 9. Estimated effects of conditionality with exclusion of observations from the three-year period prior to reform

Dependent variable: Welfare uptake at age 21		
	(1)	(2)
ITT quartile 2	-0.005 (0.006)	
ITT quartile 3	-0.016** (0.008)	
ITT quartile 4	-0.033** (0.014)	
ATET		-0.255*** (0.077)
Dep. var. mean	0.08	0.08
Dependent variable: Completed high school by age 21		
ITT quartile 2	0.011 (0.009)	
ITT quartile 3	0.024** (0.011)	
ITT quartile 4	0.034*** (0.012)	
ATET (all)		0.172** (0.066)
Dep. var. mean	0.69	0.69
Number of observations	345,694	345,694

Note: Standard errors are clustered at the municipality level.
*(**)(***) indicates statistical significance at the 10(5)(1) percent level.

4.2. Contamination of control group

As explained in Section 2, we do not have any information about possible changes in conditionality after 2004. Given that our analysis comprises data up to 2010, this implies that our control group municipalities to an increasing extent become “contaminated” by unobserved reforms. To assess this concern, we estimate two alternative models; one where we reduce the outcome period such that it ends in 2007, and one where we reduce it to 2005. The results presented in Table 10 indicate that cutting down on the observation period changes the results only marginally.

Table 10. Estimated effects of conditionality with reduced outcome period

	Dropping observations after 2007		Dropping observations after 2005	
	(1)	(2)	(3)	(4)
Dependent variable: Welfare uptake at age 21				
ITT quartile 2	-0.007 (0.007)		-0.006 (0.008)	
ITT quartile 3	-0.018** (0.008)		-0.019* (0.011)	
ITT quartile 4	-0.025*** (0.008)		-0.026* (0.013)	
ATET		-0.209*** (0.057)		-0.196*** (0.074)
Dep. var. mean	0.08	0.08	0.08	0.08
Dependent variable: Completed high school by age 21				
ITT quartile 2	0.007 (0.011)		-0.003 (0.017)	
ITT quartile 3	0.028** (0.012)		0.047** (0.020)	
ITT quartile 4	0.028** (0.013)		0.042*** (0.016)	
ATET (all)		0.172*** (0.069)		0.170* (0.094)
Dep. var. mean	0.69	0.69	0.69	0.69
Number of obs.	290,615	290,615	259,220	259,220

Note: Standard errors are clustered at the municipality level. *(**)(***) indicates statistical significance at the 10(5)(1) percent level.

4.3. Validity of common trend assumptions

The triple-difference identification strategy rely on an identifying assumption regarding the difference in outcomes for individuals with high and low predicted welfare propensity, in treatment and control municipalities: In the absence of any treatment, the difference between individuals with high and low predicted welfare propensity should follow the same path in treatment and control municipalities. Clearly, such an assumption is untestable since it involves a counterfactual situation. However, we are able to test whether this assumption holds in the pre-treatment years. Let T_m be a time-constant indicator variable equal to 1 if municipality m was ever treated and let s_t be a linear time trend. We estimate the following regression models on pre-treatment data only:

$$y_{it} = \mathbf{x}_{\mathbf{it}}^{'}\boldsymbol{\beta} + \psi_{mt} + \rho_p \hat{p}_i u_{mt} + \delta_1 T_m \hat{p}_i + \delta_2 s_t \hat{p}_i + \delta_3 T_m \hat{p}_i s_t + v_{it}. \quad (5)$$

This model mimics our estimation equation (4). It is designed to check whether outcome trends have differed between treatment and control municipalities in a way that correlates with individual welfare propensity $\hat{p}_i$. A statistical test of the assumption that $\delta_3 = 0$ can be interpreted as a test for whether our identifying assumption holds in the pre-treatment years. The model is estimated separately for municipalities grouped after reform year as well as jointly. The control group is included up to the last pre-reform year (same as treatment group). Note however that before 2002 the number of municipalities changing their policy each year is very sparse (see the Appendix, Table A1). Hence, when estimating separately by reform year, these treatment groups become very small and the results hardly informative. We thus present the results for municipalities reforming in 2002 (8 treated municipalities), 2003 (7 treated municipalities), 2004 (17 treated municipalities) as well as for all municipalities together (43 treated municipalities). The results are presented in Table 11. While there apparently are significant time trends in the way social assistance propensity affect the two outcomes (δ_2), there are no indications whatsoever that this pattern differs between treatment and control municipalities (δ_3).

Table 11. Testing the common trend assumption based on pre-reform data only.

Reform year:	2002	2003	2004	All
	(1)	(2)	(3)	(4)
Dependent variable: Welfare uptake at age 21				
$T_m \hat{p}_i (\delta_1)$	44.042	31.782	3.227	5.147
	(99.366)	(68.065)	(21.319)	(24.835)
$s_t \hat{p}_i (\delta_2)$	-0.040***	-0.034***	-0.023***	-0.021**

	(0.011)	(0.010)	(0.008)	(0.008)
$T_m \hat{p}_i s_t (\delta_3)$	-0.022	-0.016	-0.002	-0.003
	(0.050)	(0.034)	(0.011)	(0.0124)
Dependent variable: Completed high school by age 21				
$T_m \hat{p}_i (\delta_1)$	-15.688	16.594	-31.602	-14.884
	(90.171)	(39.338)	(22.376)	(24.477)
$s_t \hat{p}_i (\delta_2)$	0.062***	0.058***	0.042***	0.041***
	(0.016)	(0.013)	(0.010)	(0.010)
$T_m \hat{p}_i s_t (\delta_3)$	0.008	-0.008	0.016	0.007
	(0.045)	(0.020)	(0.011)	(0.012)
No. treated municp.	8	7	17	43
Number of obs.	140,424	160,123	198,810	219,455

Note: Standard errors are clustered at the municipality level. *(**)(***) indicates statistical significance at the 10(5)(1) percent level.

4.4. Sensitivity with respect to immigrant influx

As we saw in section 3.1, there has been a greater influx of immigrants with high social assistance claim propensity into treatment than control municipalities. If anything, this is likely to have masked any favorable effects of conditionality. To assess the extent to which this has disturbed our results, we estimate our two main models, spelled out in equations (3) and (4), on natives only. The results are shown in Table 12. As expected, the estimated coefficients somewhat larger than for the population at large.

Table 12. Estimated effects of conditionality. Results for natives only

	Welfare uptake at age 21		Completed high school by age 21	
	(1)	(2)	(3)	(4)
ITT quartile 2	-0.010***		0.004	
	(0.005)		(0.009)	
ITT quartile 3	-0.021**		0.020*	
	(0.009)		(0.011)	
ITT quartile 4	-0.025**		0.033**	
	(0.011)		(0.014)	
ATET		-0.250**		0.343***
		(0.117)		(0.121)
Dep. var. mean	0.07	0.07	0.69	0.69
Number of obs.	319,312	319,312	319,312	319,312

Note: Standard errors are clustered at the municipality level. *(**)(***) indicates statistical significance at the 10(5)(1) percent level.

4.5. Selective migration

Another concern is that a tightening of welfare policy might have induced selective migration, such that individuals prone to receive welfare might have moved to other municipalities after policy tightening to circumvent the conditions. To deal with this possibility, we construct an estimation sample where we use only information about municipality of residence five years before the outcomes are measured, i.e. when the adolescents were 16 years old. While reducing the problem of endogenous migration, this strategy clearly introduces additional measurement error, and thus attenuates causal effect estimates. From Table 13 we see that the estimated coefficients resulting from using this sample are very similar to the baseline results. The small declines in the estimated coefficients are consistent with attenuation caused by added measurement error.

Table 13. Estimated effects of conditionality. Results using residence 5 years before outcome measurement to assign municipality

	Welfare uptake at age 21		Completed high school by age 21	
	(1)	(2)	(3)	(4)
ITT quartile 2	-0.001		0.003	
	(0.005)		(0.009)	
ITT quartile 3	-0.011		0.010	
	(0.007)		(0.010)	
ITT quartile 4	-0.024**		0.026**	
	(0.011)		(0.011)	
ATET		-0.234***		0.158***
		(0.065)		(0.068)
Dep. var. mean	0.07	0.07	0.70	0.70
Number of obs.	340,833	340,833	340,833	340,833

Note: Standard errors are clustered at the municipality level. *(**)(***) indicates statistical significance at the 10(5)(1) percent level.

4.6. Sensitivity with respect to the included treatment municipalities

As a final robustness check, we have estimated the ATET model systematically leaving out each of the treatment municipalities in a "jackknife" fashion. The results from these 43 regressions for each of our two outcomes, are shown if Figure 6. The results are all well in line with our baseline estimates, thus we can be confident that no single municipality is driving our results.

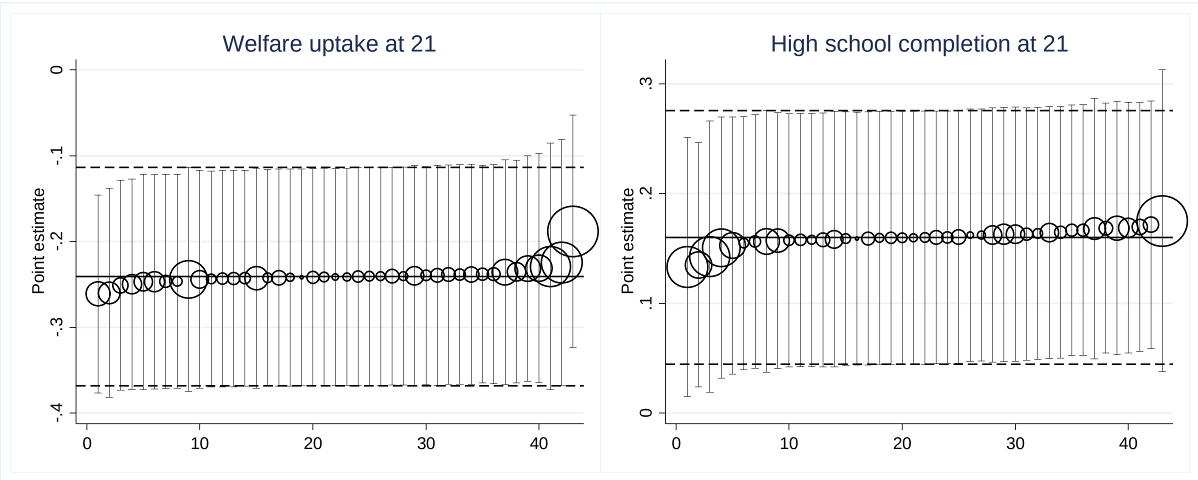

Figure 6. Distribution of point estimates from estimating ATET model systematically leaving out one of the treatment municipalities.
Note: Solid and dashed horizontal lines mark, respectively, the point estimates and 95 % confidence intervals from our baseline results (Table 6, Column (7)). Circles and vertical lines mark point estimates and confidence intervals from regressions where one treatment municipality is left out. Circle sizes indicate the relative size of the left-out municipality.

5. Long term effects

We have seen that a stricter conditionality regime has been successful in reducing young people's welfare dependency and increasing their high school completion rates at age 21. A natural follow-up question is whether this effect persists, and/or whether it affects subsequent labor market careers. To answer this question, we now study the impact of conditionality reform (still measured at age 21) on outcomes measured four years later, using our ATET model in Equation (4). At age 25, many of the youths will have entered the labor market and/or higher education; hence, it is of interest to examine a much broader set of employment, income, and education outcomes at this stage. However, to ensure that none of the pre-treatment comparison cohorts experience any period of increased conditionality, we exclude in this exercise observations from the four years immediately preceding a treatment period. This implies that the post-treatment youths in treatment municipalities were exposed to conditionality in the whole period from age 21 to age 25, whereas the pre-treatment youths were not exposed to conditionality in any of these years. We cannot avoid, however, that an increasing number of control municipalities will have introduced conditionality toward the end of our estimation period; see Section 2. Other things equal, this "contamination" of the control group implies that our estimates will be biased toward zero.

Table 14. Estimated average treatment effects (ATET) at age 25 (standard errors in parentheses).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Welfare uptake	High-school completion	University education	Employ-ment	Level of labor earnings	Level of welfare transfers	Level of social insurance transfers
ATET	-0.120* (0.066)	0.092 (0.102)	0.121* (0.062)	0.179** (0.073)	54.134* (31.222)	-14.278** (6.609)	-23.398** (11.059)
Mean of dependent variable	0.05	0.77	0.35	0.66	295.299	20.210	18.398
Number of observations	255,541	316,976	316,976	316,976	316,976	316,976	316,976

Note: All monetary amounts (Columns (5)-(7)) are measured in 1000 NOK, inflated to 2015-value; see note to Table 2. Due to data availability, outcomes are measured up to 2011 for welfare uptake (Column 1) and to 2013 for the other outcomes (Columns (2)-(7)). Standard errors are clustered at the municipality level. *(**)(***) indicates statistical significance at the 10(5)(1) percent level.

From the results shown in Table 14, we see that the reforms clearly have had favorable long-term effects. There are positive impacts on further (University/College) education, on employment, and on labor earnings, and negative impacts on welfare and social insurance claims. The positive impact on labor earnings is considerably larger than the sum of the negative impacts on welfare and social insurance transfers, implying that there is a positive impact on overall income.

The estimated impact on high school completion is lower by age 25 than by age 21; 0.09 compared to 0.16, and now also statistically insignificant. This suggests that a part of the previously identified effect on high school completion is to bring forward the *timing* of graduation for individuals who would eventually had gone on to complete high school in any case. This interpretation is also consistent with the estimated positive effects on University education as well as employment and earnings at age 25. By contributing to earlier high school completion, conditionality contributes to a timelier start in higher education and/or in the labor market.

6. Conclusion

The evidence presented in this paper indicates that intensifying the use of conditionality for welfare at local social insurance offices has substantial effects on youths from disadvantaged backgrounds. We find significant negative effects on welfare reception and positive effects on high school graduation for this group. The results are robust to a number of specification checks. The favorable impacts of conditionality also appear to persist and contribute to an earlier start in higher education and/or faster entry into the labor market. At age 25, we find that adolescents subjected to welfare conditionality from age 21 (or before) not only have higher labor earnings than they would have had in the absence of this policy, they also have higher incomes in total, including welfare and social assistance. Hence, it appears that conditionality can be a tool to offset procrastination and ease the school-to-work transition for disadvantage youths.

An important limitation of our analysis is that we have not been able to identify separate effects of specific types of conditions. Thus, we cannot contribute directly to a characterization of the optimal design and degree of conditionality. Virtually all (95 %) of the conditionality reforms evaluated in this paper contained some form of activation requirement, however; hence it is probable that this is a critical ingredient. It is also notable that the typical reform consisted of a number condition-types introduced simultaneously (4.1 on average), suggesting that many of the reforms amounted to implementing a local conditionality-culture, essentially giving caseworkers new tools – in addition to just saying yes or no to their social assistance application – to push/nudge claimants into some form of self-sufficiency. The actual design of conditions must in any case be decided on a case-by-case basis, given the legislation's emphasis on reasonableness and proportionality.

The findings reported in this paper should probably be understood in light of the relatively strong obligation that social insurance offices in Norway have to help persons in financial distress, regardless of the problem's cause. Hence, outright rejections of calls for economic support are in many cases problematic, even when the troubles are clearly self-inflicted. In this situation, the option of setting (reasonable) conditions may represent an alternative way of being "strict", which is much easier to use – and hence represents a more credible "threat" – than the option of being strict through rejections.

Appendix

Table A1: Reform years

Year	Number of municipalities implementing conditionality reform
1994	0
1995	1
1996	0
1997	1
1998	2
1999	3
2000	2
2001	2
2002	8
2003	7
2004	17
Total	43

Table A2: Logit coefficients from model (1) explaining and predicting welfare propensity

	Coefficient	St. error
Mean income when child is aged 0-9		
...Father	-1.95e-06***	1.70e-07
...Mother	-6.36e-07***	2.98e-07
Education, father		
...High school	-0.245***	0.051
...Bachelors degree or similar	-0.626***	0.109
...Masters degree or similar (incl. phd)	-0.589***	0.176
Education, mother		
...High school	-0.594***	0.051
...Bachelors degree or similar	-0.882***	0.123
...Masters degree or similar (incl. phd)	-0.121	0.401
Country or origin		
...Western Europe	0.236	0.101
...Eastern Europe	-0.259	0.526
...Africa	0.377	0.444
...Asia	0.979***	0.187
...North America	0.082	0.217
...South America	1.305	0.385
...Oceania	0.991	0.826
Constant	-0.931***	0.077
Likelihood ratio test (Chi-Square(15))	725.75 P=0.0000	
Number of observations	23,852	

Table A3: Covariates included in estimations

	Type	Number of categories
Parental income father, mean over the years the child were 0-9 years of age	Continuous	
Parental income mother, mean over the years the child were 0-9 years of age	Continuous	
Father's education when child is 10 years old	Categorical	4
Mother's education when child is 10 years old	Categorical	4
Nationality	Categorical	7
Gender	Categorical	2

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