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Can welfare conditionality combat high school dropout?☆

Øystein Hernæs[a,b,*], Simen Markussen[a], Knut Røed[a]

[a] The Ragnar Frisch Centre for Economic Research, Norway
[b] Institute for Social Research, P. box 3233 Elisenberg, 0208 Oslo, Norway

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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.

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 are 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 medical 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 imposing a more structured daily life on inactive adolescents, and thus prevent more serious marginalization. When youths about to drop out from school show up at the social insurance office to seek income support, a strict conditionality regime may in some cases be what is required to convince them to complete their schooling rather than to have to participate in strenuous training or community work.

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 1984. 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

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* Corresponding author at: The Ragnar Frisch Centre for Economic Research, Gaustadalléen 21, 0349 Oslo, Norway.
E-mail addresses: ohernaes@gmail.com (Ø. Hernæs), simen.markussen@frisch.uio.no (S. Markussen), knut.roed@frisch.uio.no (K. Røed).

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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. (2017) 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.

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. Yet, as long as the applicant is enrolled in regular secondary education, social assistance claims may be rejected with reference to the parents' economic situation, even when the applicant is above 18 years. Hence, social assistance to young adults primarily represents an economic safety net for pupils from very poor families and for adolescents who quit school, but fail to find – or even genuinely search for – gainful employment. A key role of conditionality in this context is to raise the potential cost of quitting school, as the alternative of living on welfare may become considerably less attractive. 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. While the former mechanism implies that conditionality causes high school completion to substitute for social assistance claims, the latter implies that it complements them.

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 around 3.1 percentage points, while the high school graduation rate increases by 2.2 percentage points. The favorable effects on high school completion is fully explained by a higher probability of completing *without* claiming social assistance; hence conditionality induces high school completion to substitute for social assistance claims. 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.

2. Institutions and data

According to Norwegian legislation, adult persons (aged 18 or more) who are neither able to support themselves through work nor covered by social insurance programs, are entitled to means-tested social assistance from their municipality. There is one possible exception from this rule, however, and that is if the young adult is still in regular secondary education, and the parents are deemed to have sufficient economic resources to support their adult offspring. In this case, the caseworker *may* obligate the parents to support their offspring economically. This is subjected to a discretionary decision, however, and social assistance cannot be rejected unless it is clear that the parents actually take on their economic responsibility. Yet, it cannot be ruled out that the legislation's reference to continued parental responsibility during high school enrolment may represent an incentive to quit school for some (potential) social assistance claimants.

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 Fig. 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 of social insurance coverage; the latter because social insurance entitlements require past work experience and social security contributions.

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] In the period covered by our data, 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 questions 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 1984.

In total 247 of the 470 local insurance offices (located in 433 municipalities) existing in 2005 returned the TRI-survey.[3] 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 infor-

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

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

[3] With the exception of the largest cities (Oslo and Bergen) there is one single social insurance office in each municipality, ensuring that adolescents living in the same municipality at the same point in time have all been subjected to the same treatment status. Since we do not have sufficiently detailed information about address to link adolescents in Oslo and Bergen to the correct social insurance office, we have dropped these municipalities from the analysis.

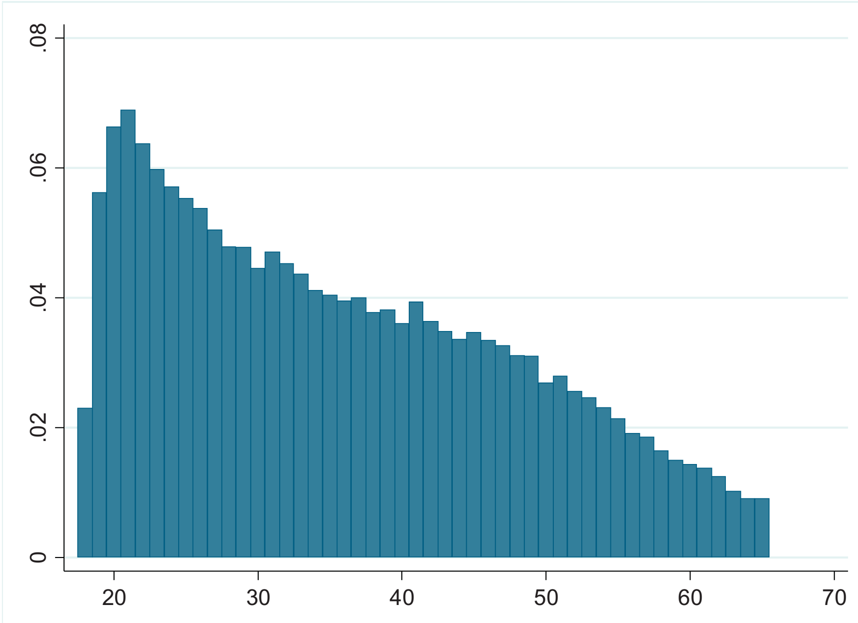

Fig. 1. Fraction receiving social assistance (welfare) by age in 2011.

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

mation, or lack of link to individuals; 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, Fig. 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.

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 2005, that are 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.

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).

Young welfare recipients were by far the group for whom conditionality was applied the most – 97% of respondents reported that they "often" used conditions towards this group. 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, and that conditionality therefore typically involved some sort of activation requirement, either in the form of community work, or training/education. 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, a point also emphasized by case-workers (Brandtzæg et al., 2006, p. 115).

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. 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, in order to avoid contam-

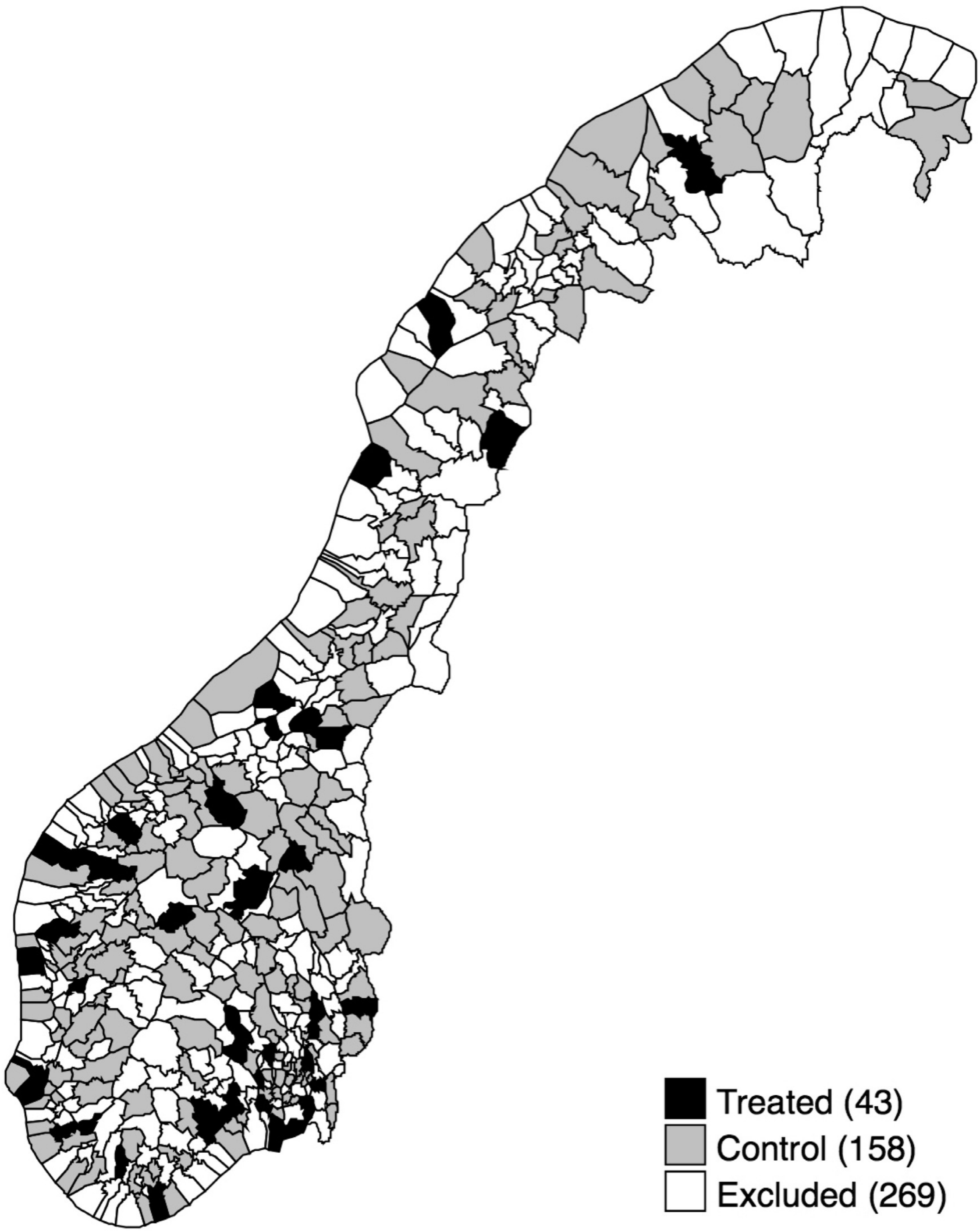

Fig. 2. Treatment and control municipalities.

ination of control municipalities in the form of unobserved treatments, we let 2005 be our last observation year.

Given the apparent large differences in *content*, we would 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 89% 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 1984, who turned 21 years in the years between 1993 and 2005. Since the actual timing of the policy shift within

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

	Excluded municipalities ($n = 178$)		Control municipalities ($n = 158$)		Treated municipalities ($n = 43$)	
	1993	2005	1993	2005	1993	2005
Inhabitants	11,674	12,621	7,207	7,581	10,392	11,235
Employment rate	0.66	0.69	0.67	0.70	0.65	0.69
Mean income (1000 NOK, inflated to 2015 value; see note below)	361	399	343	374	334	369
Fraction with tertiary education	0.23	0.31	0.18	0.25	0.17	0.24
Fraction with at least secondary education	0.47	0.62	0.42	0.58	0.41	0.58
Fraction receiving welfare benefits	0.027	0.020	0.021	0.017	0.027	0.019
….below age 30	0.039	0.028	0.033	0.027	0.041	0.030
Fraction receiving disability benefits	0.085	0.094	0.087	0.102	0.090	0.104
Unemployment rate	0.044	0.030	0.040	0.027	0.046	0.028

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).

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

Activation and work requirements	Number of municipalities	Fraction of treated	Fraction of treated persons
Participate in program: A requirement to take part in a work/training or educational program.	26	0.60	0.72
Work for welfare: Requirement to participate in a work program either organized by the municipality or others.	15	0.35	0.21
Register as seeking work: A requirement to register as an active job-seeker, keeping an updated CV etc.	25	0.58	0.62
General counseling: Attend counseling meetings with caseworker or others to discuss the current situation.	26	0.60	0.59
Career counseling: Attend career counseling meeting(s) with caseworker or others to improve work prospects.	10	0.23	0.18
At least one activation/work requirement	41	0.95	0.89
Health			
Health examination: Willingness to undertake a health examination.	14	0.33	0.22
Economic			
Document expenses: A requirement to show documentation for housing costs and other additional costs exceeding the welfare benefit	29	0.67	0.65
How to use the benefit: Restriction on how the recipient spend the benefit	17	0.40	0.37
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	0.48
At least one economic condition	34	0.79	0.79
Total number of conditions changed	175		
Total number of municipalities changing policy	43		

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.

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 8% 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 (2016) 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

Table 4
Descriptive statistics for estimation sample.

	Mean	SD
Outcomes		
Welfare uptake at age 21	0.081	
Completed high school by age 21	0.693	
Background characteristics		
Fraction female	0.484	
Parental income, mean over child's age 0–9, 1000 NOK (2015-value)		
…Father	503	190
…Mother	133	130
Parental education, when child is 10 years		
…Father has college degree	0.209	
…Father has high-school	0.516	
…Mother has college degree	0.171	
…Mother has high-school	0.493	
Nationality background		
…Native	0.893	
…Western Europe or North America	0.076	
…Rest of the world	0.030	
Calendar year turning 21	1998.7	3.777
Treated by age 21	0.051	
Local unemployment rate at age 21	0.048	0.026
Number of observations	259,220	
Number of municipalities	201	

Table 5
Descriptive statistics by quartile in the predicted welfare propensity distribution. 1972–84 birth cohorts.

	Q1	Q2	Q3	Q4
Mean predicted welfare propensity, based on 1971-cohort	0.031	0.058	0.086	0.158
Outcomes				
Welfare uptake at age 21	0.030	0.052	0.084	0.159
Completed high school by age 21	0.850	0.751	0.662	0.510
Background characteristics				
Mean parental income (1000 NOK, 2015-value)				
..Father	678	517	454	363
…Mother	201	141	110	79
Parental education				
…Father with college	0.657	0.109	0.051	0.018
…Father with high school	0.327	0.782	0.590	0.365
…Mother with college	0.509	0.124	0.039	0.014
…Mother with high school	0.476	0.813	0.593	0.090
Nationality				
…Native	0.922	0.932	0.907	0.815
…Western Europe or North America	0.071	0.058	0.077	0.100
…Rest of the world	0.008	0.010	0.016	0.085
Number of observations	64,798	64,808	64,805	64,809

can thus use these results obtained for the 1971-cohort to make out-of-sample predictions for the 1972–84 cohorts used in our causal analysis. That is, we compute a welfare propensity score $\hat{p}_i$ as

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

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 1972–84 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. 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 37 times higher in the first than in the fourth quartile, whereas the likelihood of having parents who immigrated from a non-western country is 11 times higher in the fourth quartile than in the first.

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. Fig. 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 for 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. The predicted claim propensity in the ten upper percentiles increased somewhat more in the treatment municipalities than in the control municipalities toward the end of the observation period. 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.

The fact that the fraction of immigrant youths increased more in the treatment than in the control municipalities may raise concerns regarding our ability to disentangle the impacts of this particular change from the causal impacts of the reforms. As immigrant youths tend to have higher welfare uptake propensities, and also somewhat lower high school completion rates, if unaccounted for, the rising immigrant share may mask any favorable treatment effects. As we explain below, we do account for immigrant status in our analysis, and we will return to a more specific robustness analysis with respect to this topic in Section 4.4. Another concern could be that the rising share of immigrants in the

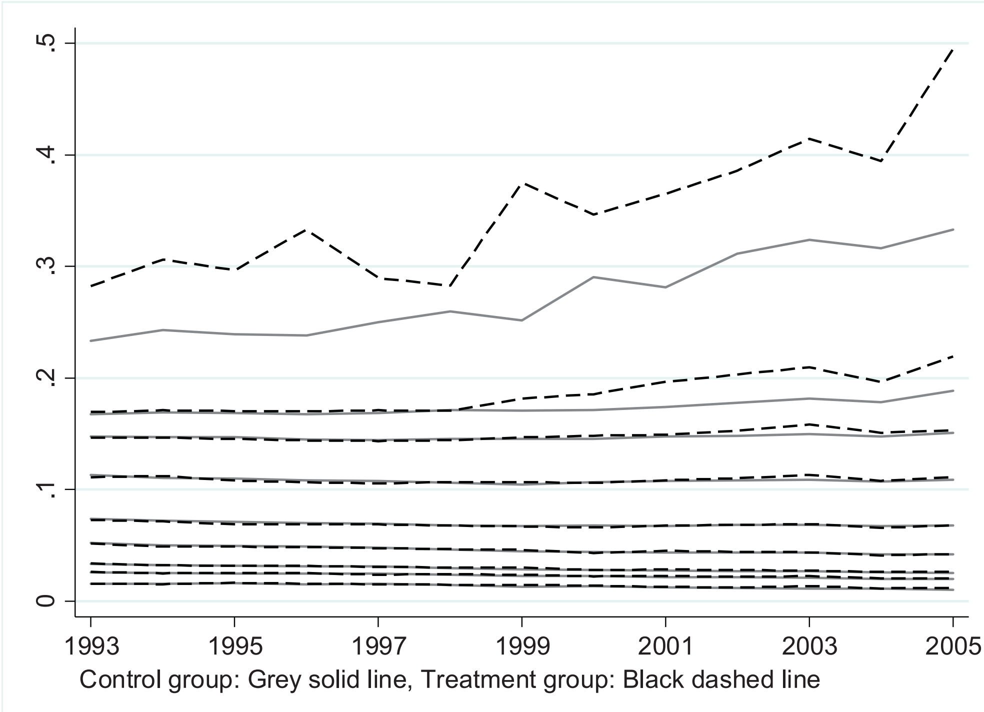

Fig. 3. Predicted propensity of welfare uptake ($\hat{p}_i$) over time in treatment and control municipalities.
Note: The graph draws the 1st, 5th, 10th, 25th, 50th, 75th, 90th, 95th and 99th percentile in the distribution of predicted welfare propensities within each year and within treatment and control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

treatment municipalities resulted in different high school passing standards for all, including natives. This is highly unlikely, however, as the high schools in Norway are the responsibility of *counties* and not municipalities (with 23 municipalities in each county on average), and as high school completion also involves a number of anonymous national tests.

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. Fig. 4 presents (calendar year adjusted) average outcomes for the 10 deciles in the $\hat{p}_i$-distribution for the treatment group before and after the policy shift.

Starting out with panel (a), showing welfare uptake at age 21, we see that the two 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.

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

$$y_{it} = \mathbf{x_i}'\boldsymbol{\beta} + \lambda_m + \sigma_t + \rho u_{mt} + \theta C_{mt} + \upsilon_{it}, \tag{2}$$

where (λ_m, σ_t) are municipality and time fixed effects, respectively, and ν_{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 1.1 percentage points and raised the probability of high school completion by 1.2 percentage points. These affects appear small. 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 Eq. (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 increasing effects as we move up in the welfare propensity distribution. In the upper quartile, we estimate an ITT effect of conditionality equal to −3.1 per-

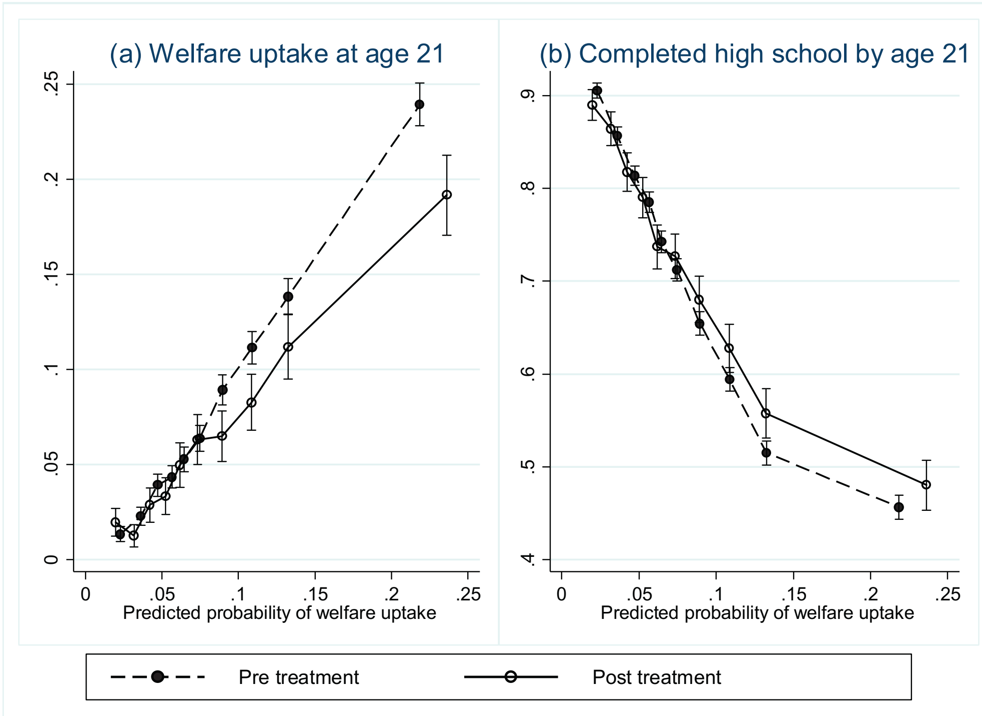

Fig. 4. High school completion and welfare uptake before and after treatment
Note: Outcomes have been calendar-year-adjusted by regressing them on calendar year dummy variables, obtaining the residuals, and then adding a constant term such that the outcomes are measured in 2000-levels.

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)
A. Dependent variable: Welfare uptake at age 21							
ITT all	−0.011**						
	(0.006)						
ITT quartile 1		−0.001					
		(0.003)				–	
ITT quartile 2			−0.007			−0.006	
			(0.005)			(0.006)	
ITT quartile 3				−0.009		−0.009	
				(0.010)		(0.010))	
ITT quartile 4					−0.031**	−0.025**	
					(0.010)	(0.010)	
ATET							−0.196***
							(0.074)
Mean of dependent variable	0.08	0.03	0.05	0.08	0.16	0.08	0.08
B. Dependent variable: Completed high school by age 21							
ITT all	0.012*						
	(0.007)						
ITT quartile 1		−0.005					
		(0.010)				–	
ITT quartile 2			0.004			0.009	
			(0.010)			(0.014)	
ITT quartile 3				0.028**		0.032**	
				(0.011)		(0.016)	
ITT quartile 4					0.022*	0.024**	
					(0.013)	(0.016)	
ATET							0.170*
							(0.094)
Mean of dependent variable	0.69	0.85	0.75	0.66	0.51	0.69	0.69
Number of observations	259,220	64,798	64,808	64,805	64,809	259,220	259,220

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.

centage points for welfare claims and +2.2 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 Eq. (2). Let $q = 1, \ldots, 4$ denote quartile in the $\hat{p}_i$ distribution. The 3D-estimator is then based on a regression equation of the following form:

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

where I_q is an indicator for quartile q, ψ_{mt} are 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). 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).

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 would clearly be of interest to disentangle these two factors empirically, 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 operates through both persons actually claiming social assistance, and persons potentially claiming), we cannot use a standard instrumental variables approach, as in, e.g., Markussen and Røed (2016). What we can do, however, is to measure the estimated effects relative to the individual predicted welfare propensity indicators $\hat{p}_i$. This way, we can estimate the average effect of conditionality relative to the fraction of adolescents actually exposed to the treatment; i.e., the effect obtained per affected person. 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 20 percentage points and increase the school completion probability by 17 percentage points.

3.3. Mechanisms

The literature on treatment effects makes a distinction between "regime effects" (*ex ante*) and "participation effects" (*ex post*); see, e.g., Arni et al. (2015). In our case, a potential *ex ante* effect is that conditionality makes a life on welfare less attractive, in which case we expect lower claim propensities, possibly in combination with higher rates of high school completion. As explained in Section 2, the legislation that makes parents' economically responsible for their adult offspring as long as they are enrolled in high school may represent a perverse incentive for some adolescents to quit school prematurely; and by making social assistance less attractive, conditionality may serve to offset that incentive. A potential *ex post* effect is that the conditions imposed on actual welfare claimants contribute to a more structured daily life, possibly including valuable work experience or training. This could in turn effect high school completion positively if it subsequently inspires a return to high school, or negatively if the required activities substitute for regular education. Unfortunately, our statistical approach is not designed to make a clean distinction between these two mechanisms, given that any regime effects will endogenously alter the composition of actual claimants, and hence make it difficult to disentangle selection and causality in this group. What we can do, however, is to examine whether the favorable school completion effects are associated with more or less social assistance uptake.

In Table 7 we report estimates for outcomes constituted by all possible combinations of welfare uptake and high school completion, based on our baseline ATET model in Eq. (4). The coefficients reported in this table relates to the ATET-effects reported in Table 6, such that the coefficients in Table 7's Columns (1) and (2) add up to the coefficient in Table 6's Column 7, panel A, whereas the coefficients in Table 7's Columns (1) and (3) add up to the coefficient in Table 6's Column 7, panel B. What these estimates essentially show is that the favorable effect on high school completion materializes in combination with the absence of welfare uptake at age 21; see Column (3). This suggests that *ex ante* regime effects must have been important; i.e., that the higher propensity to complete high school in some sense substituted for, rather than accompanied, welfare claims.[4] The probability of combining welfare uptake with high school completion actually declined; see Column (1). This is the opposite of what we would expect to see if stricter conditionality increased high school completion primarily by requiring welfare recipients to go back to school as a precondition for continued receipt.

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 either to 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 8 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.

How demanding was the policy implementation for the offices? Data on the budgets of the local insurance offices show that operating expenses related to welfare decreased both in the treatment year and later. This suggests that the treatment effect of a reduced caseload more than made up for some of the conditions requiring higher expenses at the office. The fact that there are also savings related to a reduced number

[4] Given this finding, it would have been interesting to examine whether the effect on high school completion is larger for adolescents with parents who have sufficient economic resources to be made responsible for their adult kids as long as they are registered in school. However, it is difficult to disentangle this mechanism from the fact that the propensity to claim social assistance in the first place is an order of magnitude higher for offspring with poor parents. Hence, by, say, interacting the causal effects of interest with indicators for poor parents, this interaction would also capture a much higher exposure to treatment.

Table 7
Estimated average treatment effects on the treated (ATET) on outcome combinations.

	(1) Welfare uptake *and* HS compl. at age 21	(2) Welfare uptake *but not* HS compl. at age 21	(3) No welfare uptake *but* HS compl. at age 21	(4) No welfare uptake *and no* HS compl. at age 21
ATET	−0.028	−0.168**	0.198**	−0.002
	(0.039)	(0.070)	(0.086)	(0.078)
Dep. var. mean	0.02	0.06	0.67	0.25
Number of observations	259,220	259,220	259,220	259,220

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

Table 8
Estimated effects of conditionality. Partial average treatment effects (ATET) by category of requirement.

Requirement type	(1) Welfare uptake	(2) High school completion
Activation and work	−0.099	0.303*
	(0.079)	(0.164)
Health	−0.099	0.168
	(0.132)	(0.211)
Economic	−0.109	−0.254
	(0.113)	(0.189)
Dep. var. mean	0.08	0.69
Number of observations	259,220	259,220

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

of welfare checks (which are paid from the municipality budget) imply that welfare conditionality is a highly cost-effective policy.

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 endogeneity

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 examine the evidence for policy endogeneity by estimating models where the introduction of conditionality is used as the dependent variable.

To do this, 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.[5] 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 in Table 9 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 9, qualitatively, at all. As a final check on the results' sensitivity with respect to pre-reform changes in social assistance claims, we have 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 (not shown).

4.2. Validity of common trend assumptions

The triple-difference identification strategy relies 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 $\boldsymbol{s_t}$ be a vector of calendar year dummy variables. We estimate the following regression models on pre-treatment data only:

$$y_{it} = \mathbf{x}_{\mathbf{it}}^{\prime}\boldsymbol{\beta} + \psi_{mt} + \rho_p \hat{p}_i u_{mt} + \hat{p}_i\left(\delta_1 T_m + \mathbf{s_t}\boldsymbol{\delta_2} + T_m \mathbf{s_t}\boldsymbol{\delta}_3\right) + v_{it}. \quad (5)$$

This model mimics our estimation Eq. (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 $\boldsymbol{\delta_3} = \mathbf{0}$ can be interpreted as a conservative test for whether our identifying assumption holds in the pre-treatment years, as it will be rejected if there was a development in the outcome that correlated with welfare propensity in *any* of the pre-reform years. In addition to this very flexible specification, we also substitute more smooth time trends for the term $\mathbf{s_t}\boldsymbol{\delta}_3$ in Eq. (5), while keeping the term $\mathbf{s_t}\boldsymbol{\delta_2}$ completely flexible (with year dummy variables). The results are presented in Table 10. We only report the coefficients and tests of interest; i.e., those related to the interaction of welfare propensity, time, and treatment status. And while we report coefficient estimates and tests for joint significance for the polynomial models, we only report the joint significance test statistic for the dummy variable model.

As it turns out, none of the models provides any indication whatsoever that our key common trend assumption is violated in the pre-treatment period.

4.3. Sensitivity with respect to the foundation for the estimated welfare propensity ($\hat{p}_i$)

In order to predict individual welfare propensities ($\hat{p}_i$), we have used the last birth-cohort *not* used in our causal analysis; i.e., those born in

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

Table 9
Test for reform endogeneity.

Dependent variable: Reform in year t Covariates measured in	(1) t	(2) $t-1$	(3) $t-2$	(4) $t-3$
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.

Table 10
Testing the common trend assumption based on pre-reform data only.

Reform year:	Linear trend (1)	Quadratic trend (2)	Cubic trend (3)	Dummy variables (4)
Dependent variable: Welfare uptake at age 21				
δ_{31}	−0.002	0.007	0.099	
	(0.012)	(0.035)	(0.083)	
δ_{32}		0.001	−0.028	
		(0.003)	(0.021)	
δ_{33}			0.0018	
			(0.0014)	
$F(2200)$		0.02		
		($p = 0.976$)		
$F(3200)$			0.62	
			($p = 0.603$)	
$F(10,200)$				1.45
				($p = 0.159$)
Dependent variable: Completed high school by age 21				
δ_{31}	0.006	0.013	−0.035	
	(0.128)	(0.053)	(0.113)	
δ_{32}		−0.001	0.012	
		(0.005)	(0.025)	
δ_{33}			0.001	
			(0.002)	
$F(2200)$		0.10		
		($p = 0.907$)		
$F(3200)$			0.16	
			($p = 0.923$)	
$F(10,200)$				0.95
				($p = 0.49$)
Number of obs.	219,455	219,455	219,455	219,455

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

1971. Alternatively, we could have used all the cohorts contemporary to our analysis population, but who lived in municipalities excluded from our analysis (due to missing data on conditionality), or those born in control municipalities. While none of these approaches are perfect – the former because it incorporates an unknown mixture of treatment and controls and the latter because it is subjected to the endogenous stratification problem discussed by Abadie et al. (2013) – they do have the advantage that they allow for the inclusion of calendar year fixed effects, and hence to accommodate time changes in welfare propensity. To assess our findings' robustness with respect to the way we predict welfare propensity, we have re-estimated the ATET model based on these alternatives. The results are shown in Table 11. They turn out to be similar to the baseline results in Table 6, Column (7), although the estimated impacts become somewhat larger.

4.4. Other specification issues

We have also examined a number of other specification issues for which we do not present full results here, but refer to a previous working paper version (Hernæs et al., 2016). As we explained 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 have estimated the models on natives only. As expected, the estimated causal then become a bit larger than for the population at large.

Another potential concern is that a tightening of welfare policy might have induced selective regional migration, such that individuals prone to receive welfare have moved away from policy tightening municipalities; see Fiva (2009). To deal with this potential problem, we have

Table 11
Estimated average treatment effects on the treated (ATET) with alternative estimation samples for welfare propensity.

	Welfare propensity based on			
	Control municipalities		Excluded municipalities	
	(1) Welfare uptake	(2) High school completion	(3) Welfare uptake	(4) High school completion
ATET	−0.232**	0.215**	−0.225**	0.196**
	(0.103)	(0.084)	(0.104)	(0.082)
Dep. var. mean	0.08	0.67	0.08	0.67
Number of observations	259,220	259,220	259,220	259,220

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

Table 12
Estimated average treatment effects (ATET) at age 25 (standard errors in parentheses).

	(1) Welfare uptake	(2) High-school completion	(3) University education	(4) Employ-ment	(5) Level of labor earnings	(6) Level of welfare transfers	(7) Level of social insurance transfers
ATET	−0.127*	0.136	0.156**	0.215**	64.525*	−9.061	−29.445*
	(0.068)	(0.127)	(0.079)	(0.098)	(34.388)	(8.218)	(16.085)
Mean of dependent variable	0.05	0.77	0.34	0.67	282.680	20.702	16.885
Number of observations	236,016	236,016	236,016	236,016	236,016	236,016	236,016

Note: All monetary amounts (Columns (5)–(7)) are measured in 1000 NOK, inflated to 2015-value; see note to Table 2. Standard errors are clustered at the municipality level.
*(**)(***) indicates statistical significance at the 10(5)(1) percent level.

constructed an estimation sample where we use information about residence at age 16 only. While reducing the problem of endogenous migration, this strategy clearly introduces additional measurement error, and thus attenuates the causal effect estimates. And this is essentially what comes out of this exercise. All the causal estimates are similar, but in most cases slightly smaller in absolute value, to those reported in the main analysis.

As a final robustness exercise, we have re-estimated the ATET model systematically leaving out each of the treatment municipalities in a "jackknife" fashion, to make sure that our results do not hinge on the inclusion/exclusion of particular municipalities. Again, we find that the results are robust.

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 Eq. (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.

From the results shown in Table 12, 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 slightly lower by age 25 than by age 21; 0.14 compared to 0.17, 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 have 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 has the potential for easing 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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