Preliminary draft, 27.3.2017

Welfare Activation and Youth Crime*

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Abstract

We evaluate the impact on youth crime of a welfare reform that tightened activation requirements for social assistance clients. The evaluation strategy exploits administrative individual data from Norway in combination with a geographically differentiated implementation of the reform. We find that activation requirements significantly reduced the crime rate of 18 and 19 year old boys from economically disadvantaged families. The reform also reduced social assistance take-up, but we uncover no indication of substitution whereby loss of income support push teenage boys into crime. The evidence points to incapacitation effects from school attendance as a chief mechanism behind our findings.

JEL classification: H55, I29, I38, J18

Keywords: Youth crime, social assistance, activation

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* We are grateful to the Telemark Research Institute for making their survey data available for this study. We also acknowledge support from the Norwegian Research Council (project "Hooks for Change," #202453). The paper is part of the research activities of the centre of Equality, Social Organization, and Performance, University of Oslo. Administrative register data from Statistics Norway have been essential for this project.

1 Introduction

In many countries, there has been a development toward making welfare and social insurance programs activation oriented, with benefit entitlement tied to requirements such as willingness to participate in community work or training (Blank, 2002; Moffitt, 2007; Dahlberg et al., 2009; Røed, 2012). This development has primarily been motivated by the aim of offsetting moral hazard problems, but also by the more paternalistic view that some claimants need a push into activities that improve their ability to become self-sufficient in the future. However, policy makers may face a tradeoff: While strict eligibility conditions probably prevent excessive benefit claims and help some claimants toward self-sufficiency, there is a risk that some of those who are unable or unwilling to meet the requirements end up in a poverty trap, without access to any legal means of economic support. This may in turn raise the prevalence of antisocial and outright criminal behavior.

In the present paper, we examine empirically the relationship between activation requirements in the Norwegian social assistance program and the prevalence of criminal behavior among young potential claimants. The analysis draws on reform sequence that tightened activation requirements at different times in different municipalities. The study relates closely to Hernæs et al. (2016), who examined the same reform and found that stricter eligibility conditions not only caused a considerable decline in social assistance claims among adolescents, but also led to a higher rate of high-school completion. In the present paper, we take this analysis a step further and examine the impacts on juvenile crime, with a particular focus on youth growing up in economically disadvantaged families.

While policy makers may worry about higher crime rates among those rejected access to essential economic assistance, it is also possible that a stricter activation regime reduces crime. This may happen for at least two reasons. First, there could be a direct incapacitation effect arising from the simple fact that when juveniles are kept occupied in activation or in school, there is less time and opportunity left for committing crimes; see, e.g., Jacob and Lefgren (2003), Luallen (2006), Anderson (2014), and Fallesen et al. (2014) for studies of contemporaneous associations between schooling and crime. Second, to the extent that school attendance improves future economic prospects, it also raises the opportunity cost of crime (Lochner, 2004), consistent with mounting evidence on the effects of education on crime drawing on state variation in school leaving age (e.g., Lochner and Moretti, 2004; Beatton et al., 2016; Bell et al., 2016) or compulsory schooling reforms (e.g., Hjalmarsson et al., 2015).

Our empirical evaluation builds on individual data from administrative records. We use these data to study the incidences of criminal activity and social assistance claims during ages 18 to 21. Annual crime and social assistance outcomes are paired with survey-based information from local social insurance offices regarding *changes* in their use of activation requirements for social assistance implemented between 1994 and 2004. Offices from approximately half of the municipalities in Norway have provided information about the incidence, nature, and timing of such changes (Brandtzæg et al., 2006). We combine these sources of information to identify and estimate treatment effects of activation requirements on the probabilities of committing crime and receiving social assistance. Our identification strategy builds on before-after comparisons of outcomes along two margins. The first is a simple difference-in-differences analysis where we examine responses to the reform in treatment municipalities and use residents of municipalities that did not change practice – or changed practice at a different point in time – as implicit controls. This approach relies on a common trend assumption; i.e., that the developments in treatment and control municipalities would have been parallel in the absence of the reform.

The second margin exploits the fact that the individual probability of belonging to the treatment target group differs systematically across family background characteristics. After all, most adolescents never get in touch with the social assistance program, and for these individuals we should not expect a social assistance reform to affect social assistance take-up nor criminal behavior. Hence, to the extent that we can identify those for whom the treatment is (approximately) irrelevant, we can use them as an additional control group. For this purpose, we utilize youth in municipalities that are not included in the survey data to construct a prediction model for the likelihood of becoming a social assistance claimant as a function of observed family background characteristics. We then take this model to our analysis population and compute for each adolescent the predicted probability of belonging to the target group of the reform. This potentially gives us an additional control group, namely youth with a negligible probability of exposure to treatment. By combining the two sources of non-exposure (non-treated municipality or not in the target group) as implicit controls, we can identify causal effects based on a triple difference strategy. As it turns out, however, we do not identify any reform response at all among those predicted to be largely unaffected by treatment; hence our identification strategy boils down to a clean difference-in-difference analysis *within* the group of adolescents with a non-negligible probability of exposure to the reform.

Still, some challenges to our identification strategy remain. The most important challenge is perhaps that local introduction of activation requirements 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 “regression toward the mean.” We return to this endogenous-policy problem and other threats to 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.

Because crime rates among young women are almost negligible compared to those of young men, we focus entirely on outcomes of men in this paper. Our results show that activation requirements not only reduce social assistance take-up and school leaving, but also significantly reduce crime. The latter effect is concentrated among 18 and 19 year old boys with a family background that places them in the upper quartile of the predicted social assistance claim distribution. For these youths, our estimates imply that activation requirements for social assistance reduce the probability of committing a crime by 1.8 percentage points – or 40 percent. This effect comes almost fully from an estimated reduction in the probability of *combining* social assistance take-up and criminal activity. We also find a positive reform effect on school attendance, indicating an incapacitation effect on crime. Finally, there is no indication that stricter requirements push adolescents into economically motivated crime.

2 Social assistance and crime in Norway

Figure 1 presents the fraction receiving social assistance and the fraction convicted of a crime, respectively, by age and gender in Norway. The probability of claiming social assistance during a given calendar year peaks around seven percent at ages 20-21, after which it declines monotonously with age; see panels A and B. High social assistance claim rates at ages 20-21 are driven by a combination of relatively high rates of unemployment during the school-to-work transition phase, and the absence of other types of social insurance coverage (such as unemployment insurance) where entitlement typically depends on past work experience and earnings. The crime-age profile by gender is illustrated in panels C and D. Criminal activity (offending) is taken from police records on solved cases identifying individuals with at least one offence during the calendar year. We only include incidents classified as felonies with a criminal justice sanction, and exclude misdemeanors such as shoplifting and traffic violations. For both genders, crime rates peak around 20, but criminal activity among women is negligible and only one fifth of that of men. As our study aims to identify policy effects on youth crime, we focus on boys above the eligibility threshold for social assistance which is 18 and through the year they turn 21.

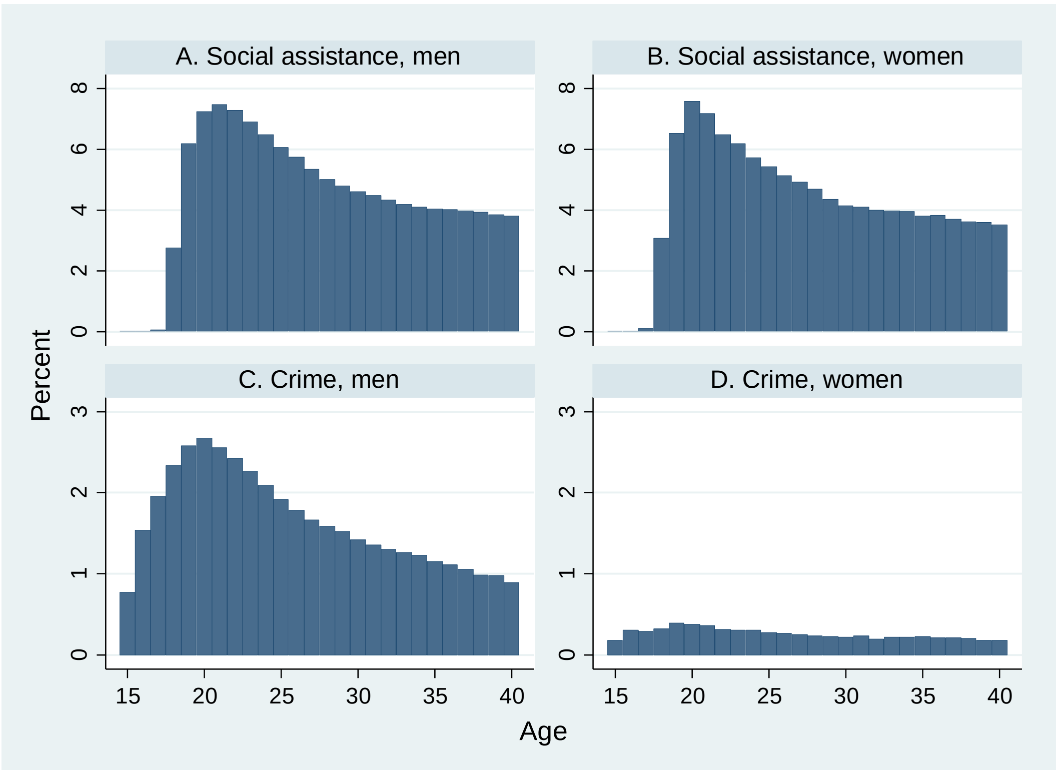

Fig 1: Social assistance and crime by age and gender

Note: Rates describe the fraction receiving social assistance or convicted of a felony crime committed during the year they turn 15 through 40. Population is restricted to those born in Norway to two Norwegian-born parents; observation period is 2001-2006. Observation counts are 4 153 798 men and 3 964 916 women.

Table 1 illustrates that youth social assistance and crime are highly related. Among boys age 18-21, those on social assistance are ten times as likely to have a criminal conviction as those not on social assistance (16.6 percent vs 1.7 percent). Conversely, among those convicted of a crime, the probability that they also receive social assistance is 7 times that of boys not charged with a crime (38.6 percent vs 5.2 percent). As more than one in three young criminals receive social assistance, policies that change social assistance criteria and access to this support are also expected to influence crime rates.

Table 1: Social assistance and crime, boys age 18-21 (percent)

		Crime		
		No	Yes	
Social	No	92.4	1.6	94.0
assistance	Yes	5.0	1.0	6.0
		97.4	2.6	100.0

Note: Table entries are cell frequencies. Population is restricted to those born in Norway to two Norwegian-born parents; observation period is 2001-2006. Observation count is 231 549.

3 The social assistance reform

The widespread take-up of social assistance among young adults is a major concern among policy makers and it is frequently argued that the welfare system is too lenient toward this group. National legislation prevents local authorities from declining aid to persons unable to cover their basic needs. They can set conditions, however, for example in the form of work requirements, as long as conditions are not disproportionate or unreasonable.[1] Given that it is problematic to make significant cuts in the (already low) levels of income support, tightening eligibility requirements may stand out as a more credible strategy for offsetting moral hazard problems.

Over the past decades, there has been ample room for discretion in the use of such conditions, and three reports commissioned by the Ministry of labor describe changes in practices across

[1] Act relating to Social Services (the Social Services Act); Lov om sosiale tjenester i arbeids- og velferdsforvaltningen (Sosialtjenesteloven), §§ 18-20.

municipalities over the 1994-2014 period (Brandtzæg et al., 2006; Proba Research, 2013; 2015). Today, national legislation specifies that some form of activation is compulsory for all able-bodied social assistance claimants below the age of 30. Leading up to the introduction of statutory activation requirements in 2017, the survey evidence in the cited reports shows that local authorities gradually strengthened eligibility criteria involving activation. Describing the 2004-2012 period, about half of the social insurance office managers surveyed by Proba Research (2013) reported that local practices had changed, with the vast majority tightening activation requirements. In a study of practices as of 2014, 70 percent of office managers reported that social assistance take-up was subject to activation requirements, with 41 percent having tightened activation requirements since 2010 (Proba Research, 2015).

Our study builds on the 2005 survey conducted by Telemark Research Institute (TRI), in which all local social insurance offices in the country were asked, *inter alia*, about changes during the past ten years (1994-2004) in their use of conditions for receiving social assistance (Brandtzæg et al., 2006).[2] In total, 247 of the 470 local insurance offices (located in 433 municipalities) returned the survey. Of these, 46 offices could not be used in the present study because of missing information on the timing of reforms, ambiguity with respect to changes, inconsistent information, or, in a handful of cases of multiple offices in the same municipality, because we cannot link the office to individuals; see Table 2. In result, our analysis builds on information from 201 social insurance districts (municipalities), covering roughly 40 percent of all youth age 18-21. Of the municipalities with a valid record, 43 strengthened their activation requirements at some point during the observation window, while 158 maintained status quo.

[2] Unfortunately, the data describing practices in Proba Research (2013; 2015) have been destroyed.

Table 2: 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

The policy shifts toward greater use of eligibility conditions for social assistance occurred in different calendar years with the majority of the reforms taking place toward the end of the 1994-2004 period.[3] The exact time pattern likely reflects rising unemployment around 2003 and 2004, growing concern about rising welfare expenditures, and the general shift toward greater emphasis on activation in social policy; see, e.g., Gubrium et al. (2014).

The TRI survey distinguished between different types of requirements. Although there was some variation across offices, activation requirements typically involved participation in work or training programs, general work counselling, active job search, and medical exams. In 34 of the 43 treatment municipalities, the reform (also) tightened requirements related to personal expenses (such as documentation of expenses and limits on housing and other expenses).

It is notable that during the observation window, none of the municipalities became more lenient and reduced their use of social assistance eligibility conditions. As shown in Hernæs et al. (2016), the 43 treatment and the 158 control municipalities are scattered across the country, with no apparent geographical concentration. The fact that we can use data from fewer than half of the municipalities raises, however, questions about generalizability. In Ta-

[3] The 43 reforms were timed as follows: 1995:1, 1997:1, 1998:2; 1999:3, 2000:2, 2001:2, 2002:8, 2003:7, 2004:17.

ble 3, we show descriptive statistics for three groups of municipalities; those with missing responses or for other reasons left out of the analysis, those who responded and did not change policy – who will serve as the control group in our analysis – and, finally, those who replied and changed their policies – who constitute our treatment group. For each group we present descriptive statistics for two years, 1996 and 2006, at the opposite sides of policy changes in the study period. For internal validity, we see that crime and school enrollment rates as well as family background characteristics in 1996 were fairly similar for treatment and control municipalities. For social assistance, pre-reform take-up rates are slightly higher in treatment regions. While family background characteristics remain similar across treatment and control regions in the post-treatment year of 2006, there are some weak indications of differential change in outcome measures in treatment compared to control municipalities.

Table 3: Sample characteristics in excluded, control, and treated municipalities, 1996 and 2006

	Excluded		Control		Treated	
	1996	2006	1996	2006	1996	2006
Social assistance	0.071	0.051	0.072	0.054	0.079	0.053
Crime	0.022	0.023	0.024	0.026	0.024	0.025
In school	0.761	0.810	0.759	0.806	0.767	0.804
Father earnings age 1-10	317	350	299	321	307	328
Mother earnings age 1-10	42	140	41	133	37	125
Father high school	0.506	0.476	0.530	0.513	0.535	0.517
Father college	0.245	0.279	0.198	0.221	0.199	0.213
Mother high school	0.522	0.402	0.531	0.421	0.535	0.429
Mother college	0.186	0.281	0.157	0.236	0.151	0.220
Unemployment rate	0.033	0.021	0.029	0.019	0.033	0.020
Observations	59 285	58 618	29 303	28 630	11 502	11 019
Number of municipalities	228		158		43	

Note: Samples are restricted to boys age 18-21, born in Norway to two Norwegian-born parents. Earnings are annual in 1000 NOK, inflated to 2012 currency.

Regarding external validity, the excluded municipalities are larger (and include large cities), but not very different in terms of crime, social assistance, and parental characteristics among youth 18-21 years of age.

Evaluating this reform faces many of the same challenges as evaluations of US welfare reforms; see Blank (2002). Given the potential differences in *content*, we would clearly have liked either to evaluate the impacts of different requirements—such as training vs active job search—separately, or to evaluate alternative "reform packages." Unfortunately, due to the simultaneity in the implementation of the various requirements, this is simply not doable. Activation requirements were primarily targeted at young welfare clients (Brandtzæg et al., 2006): 97 percent of respondents reported that they used conditions for welfare toward this group. In a recent qualitative study of four reforming municipalities, Dahl and Lima (2016) report that the reform entailed a strong focus on obliging young claimants to actually show up daily (or almost daily) at the office. In some of the cases, conditions were designed such that they were effective immediately, e.g., by requiring applicants to participate in some structured activity already the following morning. This potentially induced some "second thoughts" about life on welfare and thus generates a "threat effect" of the type discussed by Black et al. (2003). And for those who chose to fulfill the conditions, the activities may have brought a greatly needed element of structure in their daily life, a point emphasized by caseworkers (Brandtzæg et al., 2006, p. 115).

Thus, although the reform in one sense represented a clear tightening of welfare policy, in that it imposed more requirements on potential welfare recipients, the activation-related element was substantial for young claimants. In the main part of our analysis, we therefore use the implementation of new requirements 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 activation and work requirements for paying out social assistance to young clients. In a supplementary robustness analysis, we also report partial effect estimates for conditions related to personal expenses.

4 Youth outcomes and family background

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. We include in the dataset the cohorts born between 1972 and 1989 with links between children and parents, making it possible for us to add information about parents, such as education and earnings. For this reason, we restrict the analyses to those born to two Norwegian-born parents. We study outcomes of males aged 18-21 who at the given age resided in one of the 201 control or treatment municipalities with a valid record in the TRI survey data.

Most youth never experience any need for social assistance and are therefore very unlikely to be exposed to the treatment evaluated in this paper. Moreover, those who do receive social assistance tend to come from economically disadvantaged families where parents have low levels of education and low rates of labor market participation. Hence, by exploiting data on family background characteristics, we can identify *a priori* adolescents that are most likely to become social assistance claimants and thus exposed to social assistance activation requirements if they live in a treatment municipality. We do this by setting up an auxiliary logit regression model, where we estimate the probability of receiving social assistance between 18 and 21, with detailed family background characteristics as explanatory variables. This model is estimated using youth living in the 228 municipalities not in the TRI survey data and consequently not included in the analysis of treatment effects.[4] We use the estimated coefficients from this auxiliary regression to predict the individual social assistance propensity for all youths, including those living in the treatment and control municipalities. Finally, we divide the population into quartiles based on the predicted social assistance propensity.

Table 4 shows descriptive statistics for youths living in treated and non-treated municipalities by quartile of the predicted social assistance propensity distribution. As can be seen from the first row of the table, there are, as expected, considerable differences in social assistance take-up across quartiles. While the realized claim rates are below two percent in the quartile with the lowest predicted claim probability (Q1), they are 13-15 percent in the quartile with the highest predicted probability (Q4). It is also notable that the crime rates are 4-5 times higher in the latter than in the former group. It is thus clear that family background characteristics provide a rather solid foundation for predicting social assistance claims as well as criminal behavior. This is also illustrated by the large differences in family background characteristics across the four quartiles. For example, while more than 75 percent of the youths in Q1 have a father with a college degree, this is the case for less than one percent of the youths in Q4. Table 4 also shows that the distributions of outcomes and parental characteristics across quartiles are very similar for treated and non-treated municipalities.

[4] The regression has 266,711 observations. The family background characteristics include (the logs of) the father's and mother's respective earnings at offspring ages 1-10, dummy variables for zero incomes, dummy variables for deceased father/mother, and father's and mother's educational attainment (each represented by eight dummy variables). The regressions also include dummy variables for birth year.

Table 4: Descriptive statistics, regression samples

	Non-treated municipalities				Treated municipalities			
	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
Social assistance	0.018	0.037	0.062	0.135	0.019	0.034	0.064	0.150
Crime	0.010	0.017	0.024	0.043	0.009	0.016	0.025	0.047
In school	0.881	0.814	0.763	0.681	0.887	0.817	0.765	0.672
Post reform	0	0	0	0	0.241	0.252	0.257	0.244
Father earnings	403	320	288	225	404	330	299	233
Mother earnings	123	84	64	41	114	75	60	37
Father high school	0.242	0.857	0.619	0.327	0.249	0.858	0.622	0.330
Father college	0.758	0.108	0.052	0.006	0.751	0.109	0.051	0.006
Mother high school	0.332	0.854	0.590	0.139	0.359	0.871	0.575	0.132
Mother college	0.667	0.114	0.025	0.008	0.640	0.099	0.022	0.005
Unemployment rate	0.025	0.026	0.026	0.027	0.029	0.029	0.029	0.030
Observations	90 141	104 673	109 544	109 165	34 467	41 712	42 360	42 601

Note: Samples are restricted to 18-21 year old boys, born in Norway to two Norwegian-born parents. Earnings are annual in 1000 NOK, inflated to 2012 currency, and measured over the offspring age interval 1-10. Observation period is 1992-2006. As the allocation into quartiles is based on the population in *all* municipalities, including those not participating in the survey, sample sizes vary somewhat from quartile to quartile in the analysis population.

5 Reform effects

In this section, we identify and estimate the causal effects of intensifying activation requirements for social assistance on the probability of actually receiving social assistance and the probability of being convicted of a criminal felony during the calendar year. As the reform is likely to affect take-up directly, we estimate a reform effect that is *not* conditional on actual receipt of social assistance. Annual social assistance take-up and crime are both measured at the extensive margins (yes/no). For ease of interpretation, we use linear probability models to estimate the causal effects of interest.[5] We start out with a simple difference-in-difference (DiD) model, where we do not exploit the predictions for individual social assistance propen-

[5] Results are similar within a logit framework, and are available upon request.

sity described in the previous section. Let y_{imat} denote the outcome of interest for person i residing in municipality m and turning age a in calendar year t, and let C_{mt} be a treatment indicator set to unity in treatment municipalities in all years strictly after the introduction of activation requirements and zero otherwise. (We drop from the analysis all outcomes measured in the reform year, as we in these cases do not know whether claims were made before or after the reform). Furthermore, let $\mathbf{x}_\mathbf{i}$ be a vector of family background characteristics and let u_{mt} be the municipality-specific unemployment rate in year t. The DiD model then has the following structure:

$$y_{imat} = \mathbf{x}_\mathbf{i}^{'}\boldsymbol{\beta} + \lambda_m + \sigma_t + \alpha_a + \rho u_{mt} + \theta C_{mt} + v_{imat}, \tag{1}$$

where $(\lambda_m, \sigma_t, \alpha_a)$ are municipality, time, and age fixed effects, respectively, and v_{imat} is a residual. The coefficient of interest is 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 activation requirements.

The resultant estimates of θ are reported in Table 5, for both social assistance (column 1) and crime (column 4). The reported standard errors are clustered within the 201 municipalities. Taken at face value, the reported estimates imply that the introduction of activation requirements on average reduced the yearly probability of social assistance take-up among boys age 18-21 by 0.8 percentage points and lowered the crime rate by 0.3 percentage points. These effects appear small, and the crime effect is only borderline statistically significant. However, these small effect estimates need to be interpreted in light of the fact that the evaluated treatment can only have affected a small share of the population; hence the intention-to-treat nature of our estimates imply that they are heavily attenuated by the large majority of untreated observations.

As illustrated in Section 4, family background characteristics are strong predictors for social assistance take-up as well as for crime. This can also be seen directly from the estimates reported in Table 5, columns (1) and (4). For example, youths with a college-educated father are 4.3 percentage points less likely to receive social assistance and 1.5 percentage points less likely to commit a crime compared to children of high-school dropout fathers, other things equal. Both crime and social assistance are declining in parental earnings. While social assistance is strongly affected by local labor market conditions, (temporal changes in) the municipality unemployment rate is not correlated with youth crime.

Given the substantial heterogeneity in social assistance take-up by family background, the common effect assumption in columns (1) and (4) is likely to mask differential treatment effects, basically because it examines an intention to treat effect for a group where a majority is not affected by the reform. Among youths for which circumstances that trigger need for social assistance are rare, stricter social assistance conditions are hardly binding. In other words, the compliers to this treatment are likely to be found in groups with a high likelihood of being a social assistance recipient.

To investigate this further, we next examine differences in estimated effects between the groups belonging to different quartiles of the predicted probability distribution of becoming a social assistance claimant. Let Q_q be an indicator variable set to unity for a youth belonging to quartile Q_q, $q=1,2,3,4$, and zero otherwise. We then set up linear probability models with the following structure:

$$y_{imat} = \mathbf{x}_{\mathbf{i}}^{'}\boldsymbol{\beta} + \rho u_{mt} + \sum_{q=1}^{4}(\sigma_{qt} + \lambda_{qm} + \alpha_{qa} + \theta_q C_{mt})Q_q + v_{imat}\ . \qquad (2)$$

Equation (2) is essentially a repetition of Equation (1), with the important exception that the treatment effect as well as the fixed effects are now estimated separately for the different quartiles of the predicted social assistance propensity distribution. The parameter θ_q here represents the intention-to-treat effect for youths belonging to quartile *q*. And, since we know that the actual exposure to the reform has been more intensive the higher is *q*, causality should imply that $|\theta_4| > |\theta_3| > |\theta_2| > |\theta_1|$.

The results of this exercise are presented in Table 5, columns (2) and (5). For both social assistance and crime, we find that the estimated intention-to-treat effects do tend to be larger the more likely it is that a youth is exposed to the reform. However, it also appears that the effects are almost exclusively concentrated among youths belonging to the most exposed group; i.e., Q_4. Only for this quartile do we find effects that are substantively as well as statistically significant. For these youths, the treatment effects imply reductions of 2.7 percentage points in social assistance take-up and 1.1 percentage points in the annual crime rate. Compared to the mean outcomes for Q_4 in the treated municipalities (see Table 4), these effects indicate that the stricter conditions reduced social assistance take-up by 18 percent and crime by 23 percent.

The estimated effects on crime in column (5) suggest that youths with a family background implying a negligible probability of exposure to treatment could be used as a control group within a triple difference setup, i.e., by assuming that the coefficient $\theta_1 = 0$. In fact, when we re-estimate the equation with municipality-by-year fixed effects, estimates from the triple difference model are very similar to those reported in Table 5 (results available on request).

Table 5. Estimated reform effects. Boys age 18-21

	(1)	(2)	(3)	(4)	(5)	(6)
	Social assistance			Crime		
Reform	-0.008**			-0.003*		
	(0.004)			(0.002)		
Reform ×						
Quartile 4		-0.027***			-0.011**	
		(0.009)			(0.004)	
Quartile 3		-0.007			-0.003	
		(0.0046			(0.003)	
Quartile 2		-0.001			0.003	
		(0.004)			(0.002)	
Quartile 1		0.001			-0.002	
		(0.003)			(0.002)	
Reform × Disadvantaged (Q4) ×						
Age 18			-0.032***			-0.017***
			(0.011)			(0.006)
Age 19			-0.034***			-0.018***
			(0.011)			(0.005)
Age 20			-0.026**			-0.002
			(0.010)			(0.007)
Age 21			-0.019*			-0.005
			(0.010)			(0.005)
Reform×Non-disadvant (Q1-3) ×						
Age 18			0.004			0.001
			(0.003)			(0.002)
Age 19			-0.006			-0.000
			(0.004)			(0.002)
Age 20			-0.003			-0.000
			(0.005)			(0.002)
Age 21			-0.006			-0.003
			(0.005)			(0.003)
Father earnings 1-10	-0.185***	-0.173***	-0.155***	-0.053***	-0.052***	-0.046***
	(0.013)	(0.013)	(0.012)	(0.004)	(0.004)	(0.003)
Mother earnings 1-10	-0.141***	-0.144***	-0.125***	-0.023***	-0.020***	-0.016***
	(0.009)	(0.012)	(0.010)	(0.004)	(0.005)	(0.004)
Father high school	-0.039***	-0.029***	-0.025***	-0.012***	-0.011***	-0.009***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Father college	-0.043***	-0.042***	-0.030***	-0.015***	-0.017***	-0.013***
	(0.002)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
Mother high school	-0.051***	-0.036***	-0.030***	-0.015***	-0.014***	-0.012***
	(0.003)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
Mother college	-0.055***	-0.051***	-0.037***	-0.018***	-0.020***	-0.015***
	(0.003)	(0.004)	(0.003)	(0.001)	(0.002)	(0.002)
Local unemployment	1.090***	1.080***	1.063***	-0.045	-0.022	-0.031
	(0.191)	(0.193)	(0.193)	(0.077)	(0.078)	(0.079)
Mean dep var		0.066			0.024	

*/**/***Statistically significant at the 10/5/1 percent level.

Note: Standard errors are clustered within 201 municipalities. Regressions have 564 071 observations. Models control for age, year, and municipality fixed effects. To preserve concordance between flexibility of reform effects and control variables, cols 2 and 5 add interaction terms between year and municipality fixed effects and quartiles, while cols 3 and 6 add interaction terms between year and municipality fixed effects and disadvantaged background by age.

Given that the effects appear to be concentrated in the group with the highest social assistance exposure (Q_4), we now move on to a closer inspection of the impacts for this group, while using the other three as implicit controls. For ease of exposition, we then label the members of Q_4 as being "disadvantaged" (in the sense of having a disadvantaged family background) and the members of the other three groups as being non-disadvantaged. We then set up a third version of our linear probability model as

$$\begin{aligned} (3) \qquad y_{imat} = \mathbf{x}_{\mathbf{i}}^{'}\boldsymbol{\beta} + \rho u_{mt} + (\sigma_{Dat} + \lambda_{Dma} + \alpha_{Da} + \theta_{Da} C_{mt})D \\ + (\sigma_{NDat} + \lambda_{NDm} a + \alpha_{NDa} + \theta_{NDa} C_{mt})(1-D) + v_{imat} \end{aligned}$$

where $D = Q_4$. The subscripts "*D*" and "*ND*" are used to distinguish parameters estimated for the disadvantaged and non-disadvantaged youths. Apart from having merged the three least exposed quartiles into a single (non-disadvantaged) group, the difference between equations (3) and (2) is that we now also estimate the treatment effects separately for each age. The results are presented in Table 5, columns (3) and (6). For crime, it is notable that the reform effects are solely concentrated among teenagers from a disadvantaged background. There is no effect, whatsoever, among youths in their early twenties or among those without a disadvantaged background. The pattern is similar for social assistance, even though the estimates suggest reform effects even for 20-21 year olds with a disadvantaged background.

In the next section, we explore potential mechanisms. Since crime is the main outcome of interest, we focus this discussion on 18-19 year olds from disadvantaged families. Given that the estimated effects for the group of non-disadvantaged youths are close to zero, we base identification on a difference-in-difference (DiD) strategy implemented for the group of disadvantaged youth only.

6 Mechanisms

In this section, we discuss why social assistance activation requirements reduce crime rates among 18 and 19 year old boys from disadvantaged families. Given the finding of Hernæs et al. (2016) that the reform increased high-school completion rates, it is possible that the crime reduction reflects an incapacitation effect related to increased time spent in school. Alternatively, it may result from incapacitation directly related to the activities imposed by caseworkers as a condition for continued payouts. Finally, one may hypothesize that activation requirements contribute to the installment of some basic social norms about individual rights and duties.

The social assistance program represents the last layer of income insurance for persons aged 18 or older who are unable to support themselves economically. The program transfers money to ensure coverage of the basic needs for housing, food, clothing, etc. However, as long as youth are enrolled in regular primary or secondary education, it follows from the legislation that parents can be held economically responsible for their offspring even when they have become eligible themselves after turning 18 (Children Act § 68). If parents are deemed to have sufficient economic resources, caseworkers can therefore reject social assistance applications of adult offspring because they are still enrolled in secondary education. For some youths, this parental responsibility may give a perverse incentive to quit school in order to claim social assistance without involving parents.

To shed some light on possible mechanisms, we examine how the reform affected various *combined* outcomes involving crime, social assistance claims, and school enrolment. In addition, we study in detail what types of crime were affected and what requirements were most effective. Table 6 first reports estimates for various combined outcomes. The estimates are based on the difference-in-difference model described in Equation (1), but with only disadvantaged teenagers included in the analysis; i.e., 18 and 19 year olds belonging to the upper quartile (Q4) of the social assistance propensity distribution.

Table 6. Estimated reform effects. Boys age 18-19 from disadvantaged families

	(1) Social assistance	(2) Crime	(3) Crime and social assistance	(4) Crime w/o social assistance	(5) Social assistance w/o crime	(6) Enrolled in school	(7) Enrolled in school w/o crime
Reform effect	-0.031***	-0.018***	-0.013***	-0.004	-0.017**	0.014*	0.027***
	(0.010)	(0.005)	(0.004)	(0.003)	(0.008)	(0.007)	(0.007)
Mean dep var	0.123	0.044	0.020	0.024	0.103	0.846	0.817
Coefficient/mean	-0.249	-0.397	-0.681	-0.167	-0.167	0.017	0.033

*/**/***Statistically significant at the 10/5/1 percent level.
Note: Standard errors are clustered within 201 municipalities. Regressions have 78 474 observations. Models control for father earnings, mother earnings, father attainment high school, father attainment at least college, mother attainment high school, mother attainment at least college, local unemployment, and age, year, and municipality fixed effects.

Columns (1) and (2) first repeat the estimated effects on social assistance take-up and crime for this sub-group. In columns (3) to (5) we define combinations of crime and social assistance as alternative outcomes measures.[6] Note first that, in the segment of teenage boys with a disadvantaged family background, 45 percent of those who committed a crime in a given year also received social assistance, compared to 11 percent of those without a crime conviction. The estimation results confirm that activation requirements have the strongest effect on the probability of *combining* criminal activity and social assistance (column 3), while the propensity to commit crime without claiming social assistance is largely unaffected (column

[6] Note that the estimates in columns (3) and (5) add up to the estimate in column (1), whereas the estimates in columns (3) and (4) add up the estimate in column (2).

4). The latter is of some interest, as one could have expected an offsetting *positive* effect on crime in column 4 if restricted access to social assistance pushed adolescents into crime in order to replace the loss of income support. Moreover, in line with Hernæs et al. (2016), we find a significant positive effect on school enrollment (column 6). Together, these findings point to incapacitation from school attendance as a mechanism behind the reduction in youth crime. It is notable, however, that school enrollment increases even more when we examine crime-free participation (column 7). The implication is that activation requirements not only prevent some teenagers from dropping out of school, but also curb criminal activity among those who remain in school anyway. This suggests that stricter social assistance conditions have a disciplinary effect, e.g., by buttressing some basic social norms.

Even if the total impact of the reform is a reduction in the crime rate, we can have unintended effects on crimes that generate income. If this mechanism is present, we would expect to see positive effects on property crime. In Table 7, columns (1)-(4), we find strikingly similar effects of the reform on different types of crime, again suggesting that youths are not pushed into crime in order to replace the loss of social assistance income. There are also no differences in the effects with respect to the severity of the crime. In Table 7, columns (5)-(6), we report estimates separately for major and minor felonies, where the former category covers felonies with an incarceration sentence. The estimated coefficients are roughly the same. For comparison, we also include in column (7) the estimated effect on misdemeanors, often related to traffic episodes. Misdemeanors have not been interpreted as crime in this paper, and as we can see from the reported estimate, there are no effects of activation requirements on misdemeanors.

Table 7. Estimated reform effects by type of crime. Boys age 18-19 from disadvantaged families

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Type of criminal felony					
	Property	Violence	Drugs	Vandalism	Major felony	Minor felony	Mis-demeanor
Reform effect	-0.009***	-0.006**	-0.007***	-0.003	-0.007***	-0.010***	-0.004
	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
Mean dep var	0.023	0.015	0.012	0.012	0.022	0.022	0.017
Coeff/mean	-0.381	-0.399	-0.585	-0.223	-0.330	-0.462	-0.232

*/**/***Statistically significant at the 10/5/1 percent level.
Note: Standard errors are clustered within 201 municipalities. Regressions have 78 474 observations. Models control for father earnings, mother earnings, father attainment high school, father attainment at least college, mother attainment high school, mother attainment at least college, local unemployment, and age, year, and municipality fixed effects. Major felonies in col 5 include those sanctioned with incarceration; minor felonies in col 6 are those with less severe sentences. Misdemeanors in col 7 are not counted in the crime outcome elsewhere in this study.

7 Long-term effects

As the reform appears to have affected both school attendance and teenage criminal activities, it is probable that the reform also has had more lasting effects on crime, social assistance take-up, and labor market outcomes. In particular, by staying in school or acquiring relevant experience through an activation program and committing less crime during teenage years, labor market opportunities and peer composition may improve several years down the road (see, e.g., Fella and Gallipoli, 2014, for a structural model of education and crime designed to study effects of high school subsidies). In this section, we examine the reform effects on outcomes observed over a three-year observation window around age 25. A possible challenge here is that the introduction of activation requirements before age 18 or 19 also implies that these requirements were in place during their early twenties; hence we may worry that impacts observed around age 25 capture the concurrent effects of activation requirements rather than the effects of exposure at ages 18 and 19. However, at least for the crime outcome, we can rule out this channel as the evidence presented in Table 5 showed that the reform did not affect individuals in their early twenties at all.

Table 8 presents our estimates of long-term effects. First, the estimated effect on any crime during ages 24-26 is significant and the magnitude is a reduction of fully 24 percent of the mean crime rate at this age. This is, as we would expect, smaller than the concurrent effect at age 18-19 (which is a 40 percent reduction, see Table 6). The drop appears significant for major as well as minor felonies. Second, we also find that exposure to the reform at age 18-19 reduces annual social assistance take-up at age 24-26 as well at the likelihood of being out of employment, education, and training (NEET). Finally, there are no statistically significant effects on labor earnings nor school/college enrollment. It should be noted that these two outcomes are somewhat ambiguous, as the fact that the reform affected the timing of high-school completion means that it also affected the timing of entry into both the labor market and further education.

Table 8. Long-term effects of social assistance reform during late teens. Boys age 24-26 from disadvantaged families

	(1) Crime	(2) Major felony	(3) Minor felony	(4) Social assistance	(5) Labor earnings	(6) In education	(7) NEET
Reform at age 18-19	-0.016***	-0.012***	-0.004	-0.019***	-2264	0.005	-0.011*
	(0.003)	(0.004)	(0.003)	(0.008)	(6126)	(0.010)	(0.007)
Mean dep var	0.066	0.041	0.024	0.119	279 347	0.214	0.116
Coeff/mean	-0.242	-0.285	-0.170	-0.152	-0.008	0.025	-0.099

*/**/***Statistically significant at the 10/5/1 percent level.
Note: Standard errors are clustered within 201 municipalities. Regressions have 43 765 observations. Models control for father earnings, mother earnings, father attainment high school, father attainment at least college, mother attainment high school, mother attainment at least college, and birth year and municipality (at age 18/19) fixed effects.

8 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.

8.1 The counterfactual parallel trend assumption

The difference-in-difference (DiD) strategy used in this paper hinges on the assumption that the difference between youth in treated and non-treated municipalities would have remained as before the reform had the reform not been implemented. We have already seen that the results are robust with respect to a triple difference strategy, where we implicitly use youth with a negligible probability of exposure to the reform to control for any non-parallel general trends. However, the parallel trend assumption could still be violated for the group of teenagers with a disadvantaged family background.

To assess the validity of this concern, Figure 2 displays the difference between average outcomes of youth in treatment and control municipalities, centered at the reform year. To make the non-treated observations comparable, we randomly allocate a treatment year so that the frequency distribution of calendar year is the same in the treated and non-treated samples.

For crime, the pre-reform rates are not only parallel, but they are actually very similar in the treatment and control municipalities. Panel A illustrates the negative effect we identity on crime among youths from disadvantaged background. In light of the pre-reform similarity, the zero-effect for youth from non-disadvantaged backgrounds appears well founded (see panels B and D). For social assistance, the pre-reform differential is not constant. There is no clear difference in trends in treatment and non-treatment municipalities, however, and therefore no indication that the reform came during a period of more rapid decline in social assistance take-up rates in treatment regions.

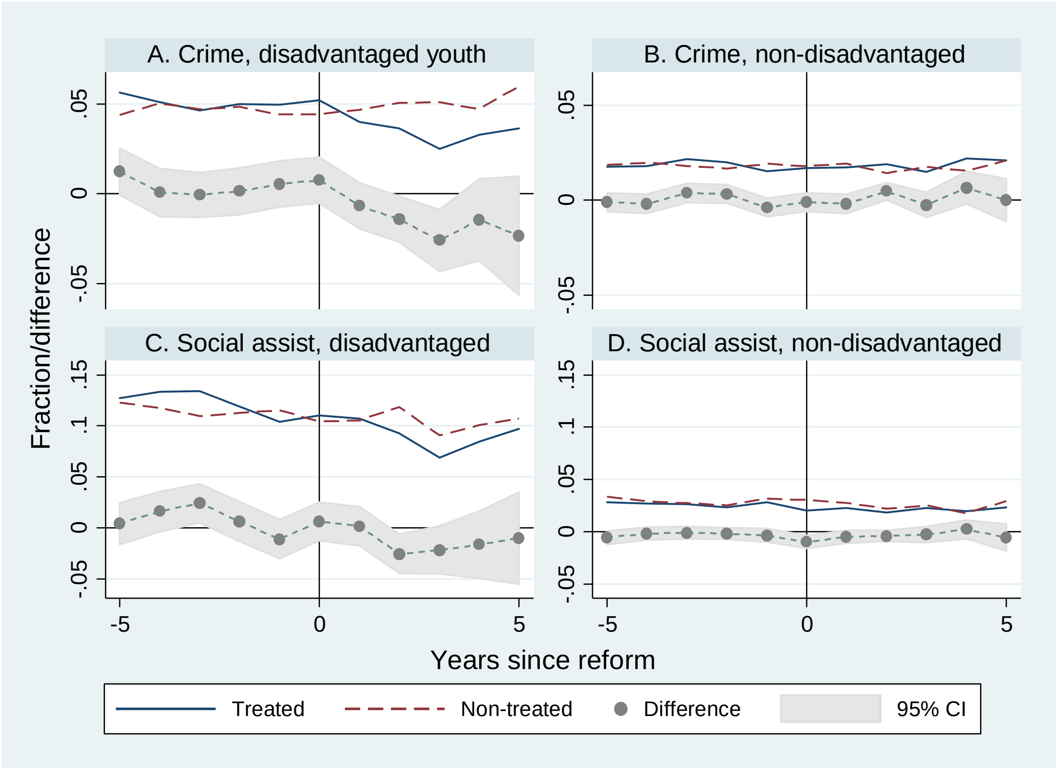

Fig 2: Trends in youth crime and social assistance, treated vs. non-treated municipalities

Note: Samples are restricted to boys age 18 and 19. For the purpose of drawing counterfactual trend lines, non-treated observations are randomly assigned a treatment year so that the frequency distribution of calendar year is the same in the treated and non-treated samples. Confidence intervals reflect year-by-year comparisons of outcomes.

8.2 Selective migration and policy endogeneity

Another concern is that a tightening of welfare policy might induce selective migration, such that individuals prone to receive welfare move to other municipalities around the time of the reform in order to circumvent the tighter requirements. Although Edmark (2009), analyzing Swedish activation programs similar to the ones we study, uncovered no evidence of migration effects, Fiva (2009) finds that the generosity of local welfare policies affect residential choice in Norway; hence we need to take the possibility of selective migration seriously.

In Table 9 we first report estimates from an instrumental variable approach where the treatment status of the municipality of residence at age 15 is used as an instrumental variable for actual treatment status. As residential mobility at ages 15-19 is limited in our data, this instrument is powerful and the (stage two) reform effects on crime, social assistance, and school enrollment in columns (1)-(3) are very similar to those reported above (see Table 6).

Table 9. Robustness analyses. Reform effects, boys age 18-19 from disadvantaged families

	(1)	(2)	(3)	(4)	(5)	(6)
	Instrumental variables			Drop 3-year pre-treatment period		
	Crime	Social assistance	Enrolled in school	Crime	Social assistance	Enrolled in school
Reform effect	-0.018***	-0.026**	0.014*	-0.020***	-0.037***	0.014*
	(0.005)	(0.012)	(0.008)	(0.005)	(0.014)	(0.008)

*/**/***Statistically significant at the 10/5/1 percent level.
Note: Standard errors are clustered within 201 municipalities. IV regressions in cols 1-3 instrument the reform variable with treatment in the municipality of residence at age 15; regressions have 76 001 observations. Regressions in cols 4-6 drop observations 3, 2 and 1 year before treatment in municipalities that implement the reform; observation count is 74 021.

In our second check on the role of selective migration and endogenous reform, we exclude the three years just before the reform to avoid that our treatment effects are inflated by temporary high rates of social assistance and crime in the years immediately preceding the reform (see Table 9, columns 4-6). Again, the results are very robust.

8.3 Other contemporaneous reforms

A third concern is that our results are driven by other local educational or social policies introduced at the same time as the social assistance reform. We are not aware of any such reforms that could account for the findings reported in this paper. However, if such reforms indeed took place, they would presumably also affect criminal activity and school enrolment among younger cohorts. Hence, as an indirect test of the hypothesis that some other local policy that systematically coincided with the social assistance reform evaluated in this paper may have influenced our results, we examine the reform impacts on youth age 16 and 17. Since these youths are above the age of criminal responsibility, but not yet old enough to be eligible for social assistance, this may be interpreted as a placebo analysis. Table 10 presents the results. The estimates are close to zero (and fairly precisely estimated).

Table 10. Reform effects on minors. Boys from disadvantaged families

	(1)	(2)	(3)	(4)
	Age 16		Age 17	
	Crime	Enrolled in school, fall	Crime	Enrolled in school
Reform effect	-0.006	0.000	-0.004	0.000
	(0.005)	(0.007)	(0.006)	(0.004)
Observations	47 483	47 483	48 169	48 169
Mean dep var	0.024	0.909	0.031	0.897

*/**/***Statistically significant at the 10/5/1 percent level.
Note: Standard errors are clustered within 201 municipalities.

As explained in Section 3, 34 of the 43 treatment municipalities combined their enhanced activation requirements with some restriction on personal expenses, either in the form of stricter documentation requirements or limits on expenditures. Even if the variation in local practices is limited in our data, it is of interest to distinguish activation requirements from those primarily related to conditions placed on welfare clients' personal economy. Table 11 reports the resultant partial effects, for social assistance claims, crime, and school enrollment.

As the two types of requirements are highly correlated, the effect estimates lack precision. Nevertheless, the crime-reducing effect estimate of activation remains statistically significant, but there are no indications that restrictive practices on personal expenses affects crime (or other outcomes). The effects of activation on social assistance and schooling are similar to those reported in Table 6, even if they are not statistically significant.

Table 11. Effects of contemporaneous personal expense requirements. Boys age 18-19 from disadvantaged families

	(1)	(2)	(3)
	Crime	Social Assistance	Enrolled in school
Requirement type:			
Activation	-0.018***	-0.042	0.018
	(0.005)	(0.031)	(0.015)
Personal expenses	0.001	0.015	-0.005
	(0.006)	(0.033)	(0.016)

*/**/***Statistically significant at the 10/5/1 percent level.
Note: Standard errors are clustered within 201 municipalities. Regressions have 79 176 observations. Models control for father earnings, mother earnings, father attainment high school, father attainment at least college, mother attainment high school, mother attainment at least college, local unemployment, and age, year, and municipality fixed effects.

8.4 Definition of disadvantaged background

Our definition of disadvantaged background is motivated by differential reform impacts across the quartiles in the propensity distribution of becoming a social assistance claimant. The choice of quartiles may appear arbitrary and the question arises whether our results hinge on this classification.

In Table 12 we report separate effect estimates for each vigintile (5-percent bin) within the upper quartile of the social assistance propensity distribution. For crime, the effects are very similar within the top 20 percentiles of the distribution. In the lower end of the upper quartile, however, the effect is zero, suggesting that our results would be even stronger were we chose a stricter definition of disadvantaged background. This pattern is similar for social assistance and school enrollment even if the effects on schooling are less precisely estimated.

Table 12. Reform effects by vigintile of the predicted social assistance propensity distribution. Boys 18-19 from disadvantaged families

	(1) Crime	(2) Social assistance	(3) Enrolled in school
V20 (96-100 percentiles)	-0.022**	-0.042***	0.006
	(0.009)	(0.014)	(0.017)
V19	-0.019**	-0.044***	0.024*
	(0.007)	(0.015)	(0.013)
V18	-0.021***	-0.034**	0.013
	(0.007)	(0.013)	(0.013)
V17	-0.019**	-0.027**	0.034***
	(0.008)	(0.011)	(0.010)
V16 (76-80 percentiles)	-0.008	-0.009	-0.005
	(0.006)	(0.013)	(0.015)

*/**/***Statistically significant at the 10/5/1 percent level.
Note: Standard errors are clustered within 201 municipalities.

All in all, the various checks reveal that the reform effects on crime as well as social assistance and school enrollment are robust, and not driven by an endogenous reform, selective migration or other contemporaneous reforms affecting adolescents in general or by how we define those most likely to be affected by the reform.

9 Conclusions

The evidence presented in this paper shows that intensifying the use of activation requirements for social assistance take-up enforced by local social insurance offices in Norway have had substantial favorable effects on youths from disadvantaged backgrounds. We find significant negative effects on crime as well as social assistance take-up. We also confirm previously reported evidence that activation requirements affected educational careers as fewer teenagers left school before high school graduation. These results are robust to a number of specification checks.

The favorable effects on youth crime are concentrated among 18-19 year old boys from disadvantaged families. For this group, the estimated effects are highly significant, both from a substantive and from a statistical point of view, with a 40 percent reduction in the probability of committing a detected crime. We present suggestive evidence that the favorable effects partly arise from an incapacitation effect related to school attendance, possibly in combination with impacts of a more structured daily life. It appears that the activation requirements implied by the reform made a life on social assistance less attractive, and perhaps discouraged some adolescents from dropping out of school in order to obtain independence from their parents, who legally maintain economic responsibility for their (adult) offspring as long as they are enrolled in secondary education. Higher school attendance is also likely to reflect that the likelihood of obtaining a valuable education has increased as a result of the conditionality policy, implying that the opportunity cost of committing crimes may have increased for some youths.

Importantly, we find no indication of an offsetting crime-inducing effect among those who lose their social assistance benefits. If anything the probability of committing a crime without having access to income support declines slightly (although this effect is not statistically significant in our analysis). At least in the context of a relatively generous welfare state, where some form of income support for those who are unable to support themselves is considered a basic individual right, it appears that activation requirements for youths may achieve both a considerable reduction in the caseloads and a higher degree of school completion, without triggering adverse side effects in the form of higher crime rates. To the contrary, the increased time spent on activation and education appears to substitute for time spent on criminal activities.

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