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ABSTRACT

Welfare Activation and Youth Crime*

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 in combination with geographically differentiated implementation of the reform. We find that the reform reduced crime among teenage boys from economically disadvantaged families. Stronger reform effects on weekday versus weekend crime, reduced school dropout, and favorable long-run outcomes in terms of crime and educational attainment, point to both incapacitation and human capital accumulation as key mechanisms. Despite lowered social assistance take-up we uncover no indication that loss of income support pushed youth into crime.

JEL Classification: H55, I29, I38, J18

Keywords: youth crime, social assistance, activation

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1 Introduction

In many countries, there has been a development toward making welfare programs activation oriented, with benefit entitlement tied to requirements such as community work and job training (Blank, 2002; Moffitt, 2007; Dahlberg et al., 2009; Røed, 2012; OECD, 2013; Persson and Vikman, 2014). 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 shove into activities that improve their prospects for self-sufficiency. Policy makers face a possible tradeoff, however: Even if strict eligibility conditions prevent excessive benefit claims and help some claimants toward self-sufficiency, there is the risk that some of those who do not meet the requirements end up in poverty. This may in turn induce antisocial and outright criminal behavior.

In this paper, we study the effects of activation requirements in the Norwegian social assistance program on youth crime, basing identification on a reform sequence that tightened activation requirements at different times across municipalities. The activation requirements, implemented by local authorities, covered community service, participation in work or training programs, general work counseling, and active job search. The study builds on Hernæs et al. (2017), who examined the same reform and found that stricter eligibility conditions caused a decline in social assistance claims and led to a higher rate of high-school completion. In the present paper, we exploit the gradual implementation of the reform and examine its impact on youth crime, with a particular focus on those growing up in economically disadvantaged families.

While stricter activation requirements may push some who lose economic support into crime in order to finance their basic needs, there are other potential mechanisms that will reduce criminal activity. First, there could be a direct incapacitation effect arising from the simple fact that when youth are kept occupied in activation or in school, there is less time and opportunity left

for committing crime; 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 activation boosts human capital and improves future economic prospects, and perhaps installs basic social norms, it also raises moral or economic costs 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., 2018; Bell et al., 2016) or compulsory schooling reforms (e.g., Hjalmarsson et al., 2015). While incapacitation effects take place at the time of activation, human capital effects will materialize in terms of educational qualifications and favorable long-term outcomes. Finally, social interaction among youth is likely to reinforce any individual crime-reducing impacts of activation requirements.

Our empirical evaluation builds on individual data from administrative records. Annual crime outcomes of youth are paired with survey-based information from local social insurance offices regarding *changes* in their use of activation requirements for social assistance. We combine individual level information on criminal offences, residential location, and family background to identify and estimate treatment effects of activation requirements on the probability of committing crime. Our identification strategy uses before-after comparisons of youth birth cohorts 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 controls. This approach relies on the common trend assumption; i.e., that developments in treatment and control municipalities would have been parallel in the absence of the reform. We provide extensive evidence backing this assumption.

The second margin exploits that family background characteristics are powerful predictors of social assistance take-up among youth. After all, most youth never get in touch with the social assistance program, and for these non-compliers we should not expect a social assistance reform to directly affect social assistance take-up nor criminal behavior. Hence, to the extent that we can identify those for whom activation requirements are (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 receiving social assistance as a function of observed family background characteristics. We then take this estimated model to our analysis population and compute for each individual the predicted probability of belonging to the target group of potential social assistance claimants. This gives us an additional control group of youth with a negligible probability of being affected by social assistance reforms. By combining the two sources of non-exposure (non-treated municipality or not in the target group) as controls, we can identify causal effects based on a triple difference strategy. As it turns out, however, we uncover no indication of differential trends in treated and non-treated municipalities; hence our identification strategy boils down to a clean difference-in-difference analysis *within* the group of youth from disadvantaged families with a non-negligible probability of exposure to the reform. Robustness checks show that our findings are not the result of endogenous policy choice or sorting of youth across treated and non-treated municipalities.

Because crime rates among young women are almost negligible compared to those of young men, we focus on outcomes of males age 18-21.[1] Our results show that activation requirements significantly reduce crime, with the effect concentrated among 18 and 19-year old boys with a

[1]The online appendix shows selected results for young women; see Table A4.

family background that places them in the upper quartile of the predicted social assistance claim distribution. For these youth, our estimates imply that stricter activation requirements for social assistance reduce the probability of committing crime by 1.9 percentage points – or 35 percent. This effect comes almost fully from an estimated reduction in the probability of *combining* social assistance take-up and criminal activity. We find no evidence whatsoever of increased crime without social assistance, as we would have expected if stricter requirements pushed youth into economically motivated crime.

Our evidence points to multiple channels for the crime-reducing effect of activation requirements. We find larger contemporaneous effects on weekday than weekend crime—particularly for property crime—speaking to incapacitation as a key mechanism. Further, we uncover significant reform effects on high-school completion and long-term crime outcomes, suggesting that human capital acquisition and higher opportunity costs of crime are important channels. We also uncover moderate effects on crime committed by 16 and 17-year olds—who are below the eligibility age for social assistance and therefore not directly affected by the reform—consistent with social spillover effects. Finally, we find significant effects on other types of crime, particularly drug-related crimes, hinting that activation requirements in the social assistance program may have broad behavioral implications for teenage boys from economically disadvantaged families.

2 Crime and social assistance in Norway

Our analyses of crime and social assistance receipt draw on individual data from the crime statistics register system of Statistics Norway and the register of the Norwegian Social Security Administration. A pseudonymous personal identifier enables us to link records to the central population register and the national education database. The crime register builds on police records and contains information on all solved cases associated with criminal offenses. The

solved cases give complete information on each offense including the identity of the perpetrator, the type of crime, and the date of the offense. In the present study, we limit the analyses to cases with a legal decision against the perpetrator and include all types of crime except for traffic violations (see Lyngstad and Skardhamar, 2011, for further details on the crime register).[2]

Social assistance represents the last layer of income insurance for those 18 or older, ensuring means for basic housing, food, and clothing. The assistance is means-tested with no maximum duration. Youth are normally entitled to social assistance benefits on an individual basis, regardless of whether or not they live with their parents. A curious exception to this rule is when the youth is enrolled in school; in such cases the legislation allows for holding parents economically responsible for their offspring even after turning 18 (Children Act § 68). As students typically graduate from upper secondary education the year they turn 19 or 20, this legislation creates a perverse incentive to drop out of school in order to collect benefits.[3] The social assistance program is administered by the local municipality, and although national legislation prevents local authorities from denying aid to those unable to cover their basic needs, municipalities are free to set conditions, for example in the form of activation requirements, as long as they are not disproportionate or unreasonable.[4] Participation in an activation program is typically rewarded with a small bonus that comes on top of the social assistance benefit. The criminal record of an individual does not affect their social assistance eligibility.

Figure 1 shows, by age and gender, the fraction convicted of at least one offence committed during the year (panels A and D), the fraction receiving social assistance (panels B and E), and

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[2] Throughout this paper we study *detected crime* only. For simplicity we use phrases such as "crime," "crime participation," and "offender" well aware that we only observe a subset of all criminal activity.
[3] There is no minimum school leaving age in Norway, although compulsory schooling is 10 years typically ending the year of the 16th birthday.
[4] Act relating to Social Services (the Social Services Act); Lov om sosiale tjenester i arbeids- og velferdsforvaltningen (Sosialtjenesteloven), §§ 18-20.

the median and interquartile range of annual benefits paid out to social assistance recipients (panels C and F).[5] For men, the crime participation rate peaks at three percent at age 20. Criminal activity among women is negligible and only one sixth that of men. For both genders, the probability of social assistance receipt reaches seven percent at ages 20-21, after which it declines monotonously with age. Median benefits paid 19-year olds are about $2000 per year, rising to $3400 for men and $2700 for women at age 40. One explanation for the

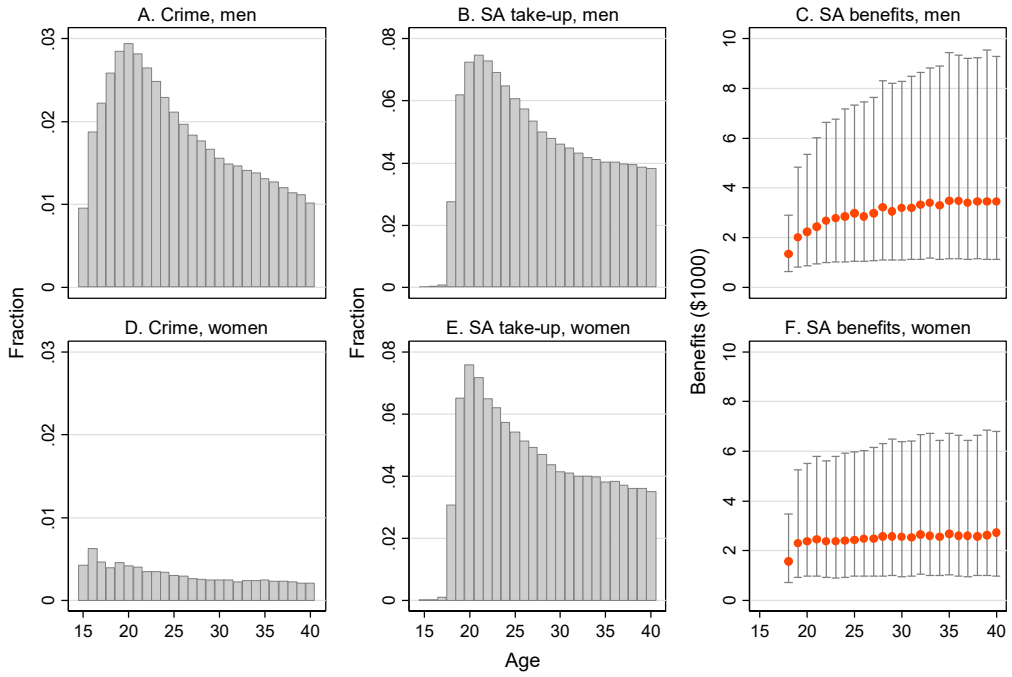

Fig 1: Crime, social assistance take-up, and social assistance benefits by age and gender

Note: Panels A and D show the fraction convicted of at least one criminal offence committed during the calendar year they turned 15 through 40; panels B and E the fraction receiving any social assistance during the year; and panels C and F the median amount as well as the interquartile range of benefits paid recipients during the year. Benefits are inflated to 2017 currency and converted to USD using the average exchange rate of 2017. 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. Panels C and F omit benefits paid to 378 minors during the observation window.

[5] Because our analyses require information on parental earnings and education during childhood, the data underlying the figure and the analyses that follow are restricted to those born in Norway to two Norwegian-born parents.

high rates of youth social assistance receipt is the absence of other types of social insurance coverage such as unemployment insurance, where entitlement depends on past work experience and earnings. As our study aims to identify policy effects on youth crime, we focus on boys above the eligibility threshold for social assistance (18) and through the year they turn 21.

Table 1 illustrates that youth crime and social assistance receipt are closely interconnected. Among boys age 18-21, those receiving social assistance are almost ten times as likely to have a criminal conviction as non-recipients (e.g., 17.7 vs. 1.9 percent at age 19). Youth on social assistance also commit more crimes on weekdays: among 19-year olds, those receiving social assistance are 14 times more likely to be convicted of an offence committed on a week-day than those without social assistance (12.5 vs. 0.9 percent). Next, the modes of crime differ by group. Among social assistance recipients, property and drug crimes dominate: at age 19, 52.0 percent of the offenders are convicted of a property crime and 51.0 percent of a drug crime. In contrast, among non-social assistance recipients, the most frequent crime type is the "other" category, which in the relevant age range largely reflects acts of vandalism. Further, among offenders, social assistance recipients commit more crimes than non-recipients: among 19-year old boys, criminal social assistance recipients commit on average 4.8 offences per year compared to 2.3 among non-recipients. Multiplying together the crime rate, the average number of cases, and the observation count, the final row shows that, for example, among 19-year old boys in our data, social assistance clients committed 7 513 offences per year, compared to 5 847 for non-recipients. Although only six percent of 19-year old boys receive social assistance, they account for 56 percent of all criminal offences committed by the age group. It should therefore come as no surprise if reforms changing the conditions for social assistance eligibility have important repercussions for youth crime.

Table 1: Crime by social assistance receipt and age, boys

	Social assistance (SA) recipients				Non-SA recipients			
Age:	18	19	20	21	18	19	20	21
Crime participation	0.186	0.177	0.171	0.173	0.021	0.019	0.018	0.016
Weekday crime	0.135	0.125	0.123	0.121	0.011	0.009	0.009	0.008
Weekend crime only	0.052	0.052	0.047	0.051	0.010	0.010	0.009	0.008
Type of crime:								
Property	0.528	0.520	0.515	0.519	0.361	0.307	0.287	0.270
Violence	0.332	0.323	0.314	0.275	0.365	0.349	0.349	0.357
Drugs	0.444	0.510	0.518	0.528	0.201	0.212	0.263	0.295
Other	0.413	0.405	0.377	0.396	0.397	0.406	0.419	0.386
Cases per criminal	4.6	4.8	4.5	4.7	2.5	2.3	2.4	2.4
Observations	3 985	8 843	10 217	10 488	140 795	133 796	130 962	130 048
Overall number cases	3 410	7 513	7 862	8 528	7 392	5 847	5 658	4 994

Note: Population consists of those born in Norway to two Norwegian-born parents; observation period is 2001-2006. Crimes exclude traffic violations and refer to crimes committed during the calendar year the individual turned the given age. The crime rate gives the fraction with at least one offence. Similarly, social assistance recipients count those who received at least one social assistance payment during the calendar year. Weekend crime also counts crimes during school holidays. The violence category includes sexual assault; the mode crime in the "other" category is vandalism.

3 The social assistance reform

As of January 1, 2017, national legislation imposes some form of activation, such as community service or job training, for all able-bodied social assistance claimants below 30. Leading up to this legislation, three reports commissioned by the Ministry of Labor document how local authorities over time gradually strengthened eligibility criteria involving activation (Brandtzæg et al., 2006; Proba Research, 2013; 2015).[6] Our study draws on the survey conducted by Telemark Research Institute (TRI), in which all local social insurance offices in the country were asked, *inter alia*, about changes in the conditions for social assistance receipt during the period 1994-

[6] During the 2004-2012 period, about one half of the social insurance office managers surveyed by Proba Research (2013) reported changes in practices and tightened activation requirements. In a study of practices as of 2014, 70 percent of office managers stated that social assistance take-up was subject to activation requirements, with 41 percent having tightened requirements since 2010 (Proba Research, 2015).

2004 (Brandtzæg et al., 2006).[7] The survey resulted in a sample of municipalities with data on local practices that can be directly linked to (potential) claimants in 201 of the 428 municipalities in the country. Forty-three of the municipalities strengthened their activation requirements at some point during the period, while 158 maintained status quo throughout the data window.

The policy shifts towards stricter activation requirements occurred in different calendar years with the majority towards the end of the observation period.[8] The time pattern likely reflects rising unemployment in 2003, general concerns about growing welfare expenditures, and a shift towards greater emphasis on activation in social policy; see, e.g., Duell et al. (2009) and Gubrium et al. (2014). The policy change saw parallels in a number of countries also strengthening activation requirements in their welfare programs (OECD, 2013), although actual implementation varied substantially both across European countries (Martin, 2015) and US states (Anderson et al., 2014). Norway is known for its "strong activation approach" (OECD, 2015, p. 112) and the use of activation requirements in workfare programs has more in common with US social policy than that found in other European countries (Gubrium et al., 2014).

The reform under study was targeted at young welfare clients. Based on extensive interviews of caseworkers, Brandtzæg et al. (2006) describe the reform as focused on imposing structure in the daily lives of young social assistance clients. Activation involved showing up regularly at some organized activity, sometimes already the next morning. As one caseworker explains, "…they learn what working life is all about – that you start at 8 in the morning and not just drop in at 10. They learn to work with scheduled breaks, and that it is important to eat breakfast before they leave home. It is more than work. The results are unbelievable. Some exit to work

[7] Unfortunately, the data describing practices in Proba Research (2013; 2015) have been destroyed.
[8] 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.

– some choose to go back to school...” (Brandtzæg et al., 2006, p. 80, our translation). Such experiences were echoed in interviews with youth affected by the reform: One youth reported that it was good receiving training in getting up in the morning, another that having to work for the social assistance benefit was a reasonable requirement and “…(w)ould only have stayed at home if not. Good to get up in the mornings” (Brandtzæg et al., 2006, p. 84, our translation).

Further description comes from the qualitative study of practices in four municipalities by Dahl and Lima (2016). The study highlights that a key motive of activation requirements is to oblige participants to meet at a regular time in the morning. In one municipality, the program lasted for four weeks, while in the others “as long as needed.” Absence without a valid excuse led to immediate cuts in benefits, and longer absences to complete termination of benefits. In two of the municipalities, participants engaged in communal work for the municipality or for charities, while the program was course-oriented with a focus on training, counseling, and job search in the other two. In the work-oriented municipalities, caseworkers emphasized the deterrence effects of the requirements: In one municipality, 16 percent of the youth called for a first compulsory meeting had their application turned down because they did not show up. Another 19 percent were rejected after the first meeting, and six percent chose to withdraw their application because they did not accept the specific requirements. In other words, the reform induced threat effects of the type discussed by Black et al. (2003), and some of the youth who were discouraged from claiming benefits may have come to the realization that a life on welfare is not particularly attractive and therefore returned to school.

Given the variation in content, we would 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 and the lack of detailed information about their precise content, this is not doable.

We therefore use the implementation of stricter requirements as a single dichotomous treatment variable. The treatment indicator thus reflects that the local social insurance administration took deliberate – and in most cases several – steps to tighten activation and work requirements for paying out social assistance benefits to young clients.

4 Data, youth outcomes, and family background

Apart from the TRI survey data covering social insurance office practices, 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 1973 and 1988 with links between children and parents, making it possible to add information about parents such as their educational attainment and earnings. To ensure complete records of family background characteristics, we restrict the analyses to those born in Norway to two Norwegian-born parents.

The setup yields three observational groups: youth living in the 43 treatment municipalities in the TRI survey data that reformed their social assistance requirements; youth in the 158 municipalities that did not change policy and who will serve as the control group; and, finally, those in the 227 municipalities with missing data. The survey municipalities cover only 40 percent of youth age 18-21 during the observation period, raising questions about generalizability. In Appendix Table A1, we show that youth crime and school enrollment rates were similar in treatment and control municipalities prior to the reform. For social assistance, pre-reform take-up rates were slightly higher in treatment regions. Regarding external validity, the excluded municipalities are larger (and include large cities), but not different in terms of youth crime, social assistance, and school enrollment.

Most youth never experience any need for social assistance and are therefore unlikely to respond to the reform. Family background can be used to identify the complier group as those

who receive social assistance tend to come from economically disadvantaged families with low levels of parental educational attainment and labor market participation. Hence, by exploiting data on family background characteristics, we can identify *a priori* the youth that are most likely to become social assistance claimants and therefore will be exposed to stricter activation requirements if they live in a treatment municipality. For this purpose, we set up an auxiliary logit regression model where we estimate the probability of social assistance receipt between 18 and 21, with detailed family background characteristics as explanatory variables. This model is estimated using youth living in the municipalities not in the TRI survey data and consequently not included in the analysis of treatment effects.[9] We use the estimated coefficients from this auxiliary regression to predict the individual social assistance propensity for all youth, including those living in the treatment and control municipalities. Finally, we divide the population into quartiles based on the predicted social assistance propensity.

Table 2 contains descriptive statistics for youth living in treated and non-treated municipalities by quartile of the predicted social assistance propensity distribution. As expected, the table reveals 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 crime participation rates are four to five times higher in the latter than in the former group. It is thus clear that family background characteristics provide a useful foundation for predicting social assistance claims as well as criminal behavior. This is also illustrated by the large differences in family background characteristics across quartiles. For example, while

[9] 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 regression also includes dummy variables for birth year. See appendix Table A2.

more than 75 percent of the youth in Q1 have a father with a college degree, this is the case for less than one percent in Q4. Finally, the table also shows that the distributions of outcomes and parental characteristics across quartiles are very similar in treated and non-treated municipalities.

Table 2: Descriptive statistics, regression samples

	Treated municipalities				Non-treated municipalities			
	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
Crime	0.011	0.018	0.028	0.050	0.011	0.019	0.027	0.047
Social assistance	0.019	0.034	0.064	0.150	0.018	0.037	0.062	0.134
In school	0.887	0.817	0.765	0.672	0.881	0.814	0.763	0.681
Post reform	0.241	0.252	0.257	0.244	0	0	0	0
Father earnings	54 899	44 893	40 719	31 625	54 731	43 494	39 198	30 600
Mother earnings	19 814	12 710	10 273	6 754	21 125	14 102	11 049	7 516
Father high school	0.249	0.858	0.622	0.330	0.242	0.857	0.619	0.327
Father college	0.751	0.109	0.051	0.006	0.758	0.108	0.052	0.006
Mother high school	0.359	0.871	0.575	0.132	0.332	0.854	0.590	0.139
Mother college	0.640	0.099	0.022	0.005	0.667	0.114	0.025	0.008
Unemployment rate	0.029	0.029	0.029	0.030	0.025	0.026	0.026	0.027
Observations	34 467	41 712	42 360	42 601	90 141	104 673	109 544	109 165

Note: Q1-Q4 denote quartiles of the predicted social assistance propensity distribution. Samples are restricted boys age 18-21 and born in Norway to two Norwegian-born parents. Crime and social assistance rates describe the fraction of youth with at least one crime conviction or a social assistance payout during the year. Earnings are annual over the offspring age interval 1-10, inflated to 2017 currency and converted to USD using the average 2017 exchange rate. Observation period is 1992-2006. As the allocation into quartiles is based on the population in *all* municipalities, including those not in the survey data, 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 stricter activation requirements for social assistance on the probability of being convicted of a criminal offence committed during the calendar year. As the reform is likely to affect social assistance take-up directly, in our main analyses of reform effects on crime we do *not* condition the data on actual receipt of social

assistance. Indeed, below we show that the reform induced considerable change in the composition of social assistance claimants and non-claimants in terms of their criminal proclivity (as identified by their criminal record at ages 16 and 17).

For ease of interpretation, we use linear probability models to estimate the causal effect of interest.[10] We start out with a simple difference-in-differences (DiD) model, where we ignore individual social assistance propensities 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 T_{mt} be a treatment indicator set to unity in treatment municipalities in all years after the tightening of activation requirements and zero otherwise. We drop outcomes measured in the reform year from the analysis, as we in these cases do not know whether the offence took place before or after the reform. Furthermore, let $\mathbf{x_i}$ 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:

$$y_{imat} = \mathbf{x_i'}\boldsymbol{\beta} + \lambda_m + \sigma_t + \alpha_a + \rho u_{mt} + \theta T_{mt} + v_{imat}, \quad (1)$$

where $(\lambda_m, \sigma_t, \alpha_a)$ are municipality, time, and age fixed effects, respectively, and v_{imat} is a residual. As illustrated in the prior section, parental characteristics are powerful predictors of youth crime. This can also be seen directly from the coefficient estimates of family background characteristics reported in Table 3. For example, youth with a college-educated father are 1.6 percentage points less likely to commit a crime compared to children of high-school dropout fathers, other things equal. Youth crime is also strongly declining in parental earnings.

[10] Results are similar within a logit framework; see appendix Table A5.

The main 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 following implementation of stricter activation requirements. The estimate is reported in Table 3, column 1. Taken at face value, the coefficient estimate implies that the reform lowered the annual crime rate of boys age 18-21 by 0.4 percentage point, or 14.3 percent of the pre-reform average in treatment municipalities. This average reform effect needs, however, to be interpreted in light of the fact that the full age group includes a large fraction of non-compliers for whom social assistance policies are irrelevant.

Given the substantial heterogeneity in social assistance take-up by family background, the common effect assumption in column 1 is likely to mask differential reform effects. To investigate this, we next examine differences in estimated effects across the 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 $q=1,2,3,4,$ and zero otherwise. We then set up a linear probability model with the following structure:

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

Table 3: Estimated reform effects, boys age 18-21

	(1)	(2)	(3)
Reform	-0.004**		
	(0.002)		
Reform*			
Quartile 4		-0.012***	
		(0.004)	
Quartile 3		-0.003	
		(0.004)	
Quartile 2		0.002	
		(0.002)	
Quartile 1		-0.003	
		(0.002)	
Reform*			
Disadvantaged (Q4)*			
Age 18			-0.020***
			(0.006)
Age 19			-0.018***
			(0.005)
Age 20			-0.002
			(0.007)
Age 21			-0.006
			(0.005)
Reform*			
Non-disadvantaged (Q1-3)*			
Age 18			0.001
			(0.002)
Age 19			-0.000
			(0.002)
Age 20			-0.001
			(0.002)
Age 21			-0.004
			(0.003)
Father earnings 1-10	-0.057***	-0.055***	-0.049***
	(0.004)	(0.004)	(0.004)
Mother earnings 1-10	-0.018***	-0.016***	-0.011***
	(0.004)	(0.004)	(0.004)
Father high school	-0.013***	-0.012***	-0.010***
	(0.001)	(0.001)	(0.001)
Father college	-0.016***	-0.018***	-0.014***
	(0.001)	(0.001)	(0.001)
Mother high school	-0.016***	-0.015***	-0.013***
	(0.001)	(0.001)	(0.001)
Mother college	-0.020***	-0.021***	-0.016***
	(0.001)	(0.002)	(0.002)
Local unemployment	-0.041	-0.013	-0.024
	(0.079)	(0.080)	(0.081)

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

Note: Sample mean of dependent variable is 0.027, and pre-reform mean in treated municipalities is 0.028. 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 model flexibility of reform effects and control variables, col 2 adds interaction terms between year and municipality fixed effects and quartiles, while col 3 adds interaction terms between year and municipality fixed effects and disadvantaged background by age.

Equation (2) is essentially a repetition of Equation (1), with the important exception that treatment and fixed effects are now estimated separately for the different quartiles of the predicted social assistance propensity distribution. The parameter θ_q here represents the reform effect for youth belonging to quartile *q*.

Heterogeneous effects are presented in Table 3, column 2. We find that the effect is largest for youth that are most likely to be exposed to the reform (i.e., Q_4), with negligible and insignificant coefficient estimates for the remaining quartiles. For youth with the most disadvantaged family background, the treatment effect implies a reduction of 1.2 percentage points in the annual crime rate, or 24 percent of the sample mean for this group. The estimates suggest that youth 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, as we show in a robustness exercise in the next section, when we re-estimate the equation with municipality-by-year fixed effects, estimates from the triple difference model are very similar to those from the double difference model.

Given that the effect appears 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 (labeled "non-disadvantaged") as controls. We then set up a third version of our linear probability model as

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

where the subscripts *D* and *ND* indicate disadvantaged and non-disadvantaged youth, respectively. 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 3, column 3. It is notable that reform effects are solely concentrated among teenagers with a disadvantaged family background. There is no effect among youth in their early twenties or among those without a disadvantaged background. In the remainder of this paper, we therefore focus exclusively on teenagers from disadvantage families. Hence, our analysis will be built on regressions of the type presented in Equation (1), but with only disadvantaged boys (Q_4) age 18-19 included in the analysis.

6 Causality and robustness checks

Before we discuss mechanisms, we first examine pre and post-reform trends in crime in treated and non-treated municipalities in order to ascertain that the identified effects really have a causal interpretation. Figure 2 shows the estimated coefficients (with 95% confidence intervals) by years since implementation of the reform (normalized to zero in the year of the reform) on the crime propensity of living in a treatment municipality, separately for disadvantaged (panel A) and non-disadvantaged (panel B) youth. It is clear from these graphs that, before the reform, there were no deviating crime trend in treated municipalities for either disadvantaged or non-disadvantaged boys. Following the reform, there was a marked reduction in criminal activity among disadvantaged boys in treated municipalities. This pattern corroborates the causal interpretation of our effect estimate.

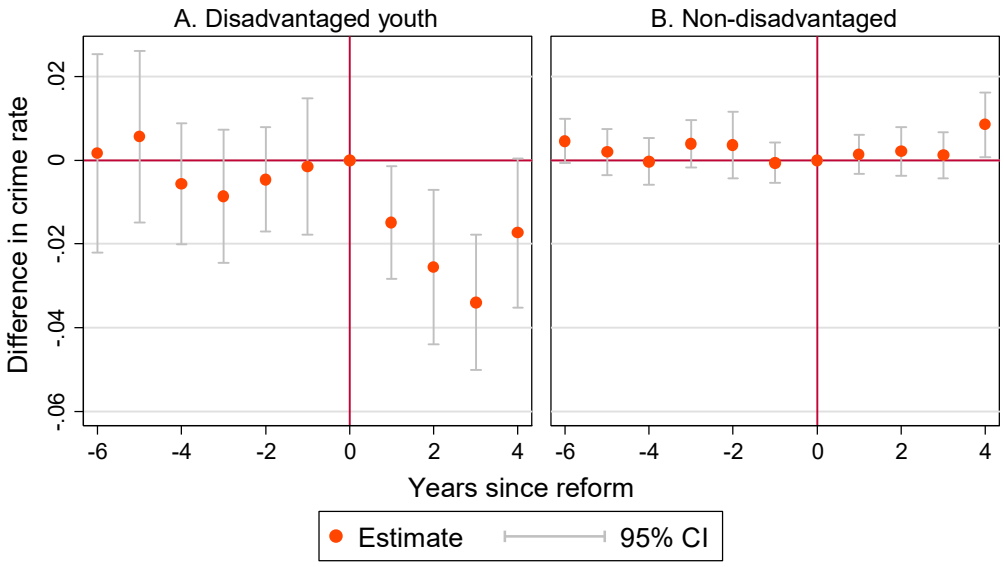

Fig 2: Pre-reform trends in crime among boys age 18-19 from disadvantaged vs. non-disadvantaged families

Note: Scatter points are estimated coefficients from regression models that 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, years since reform, and municipality fixed effects, with standard errors clustered within 201 municipalities. Regressions have 79 914 (Panel A) and 222 789 (Panel B) observations, and include observations from the reform year.

Next, we take a closer look at the key assumptions behind our identification strategy and check the robustness of our effect estimate with respect to various specification issues. First, Table 4, column (1), reports the baseline DiD estimate of the reform effect when the sample is limited to 18 and 19-year old boys from disadvantaged families and with the coefficient restricted to be the same for both ages. [11] The estimate shows that the reform had a sizeable effect on crime

[11] In the appendix, we present results from separate regressions by age, showing that estimates are very similar for 18 and 19-year old boys. Refer also back to Table 3, column (3).

in this population—reducing the fraction with a criminal conviction by 1.9 percentage point or 35 percent of the pre-reform mean in treatment municipalities.

Table 4: Estimated reform effect on crime, boys age 18-19 from disadvantaged families

	(1) DiD (baseline)	(2) Triple difference	(3) DiD with municipality trends	(4) Instrumen-tal variable	(5) Drop 3-year pre-treat-ment period	(6) Within-family
Reform effect	-0.019***	-0.020***	-0.020**	-0.019***	-0.021***	-0.024*
	(0.005)	(0.005)	(0.008)	(0.006)	(0.005)	(0.014)
Pre-reform mean	0.053	0.053	0.053	0.049	0.052	0.051
Coefficient/mean	-0.352	-0.372	-0.374	-0.382	-0.404	-0.476
Observations	78 474	297 432	78 474	75 826	74 021	71 937
Families						29 068

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

Note: Standard errors are clustered within 201 municipalities. 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. Triple difference estimator in col 2 adds youth from non-disadvantaged families to the sample in order to form within municipality and year estimate. Specification in col 3 adds municipality specific trends to the DiD model. IV regression in col 4 instruments the reform variable with treatment in the municipality of residence at age 15. Regression in col 5 drops observations 3, 2 and 1 year before treatment in municipalities that implement the reform. Sample in col 6 is restricted to families with at least two brothers in the data.

As explained in the prior section, identification in the double difference strategy builds on the change in crime among disadvantaged youth from the pre to the post-treatment period in treated municipalities compared to the change in control municipalities, and rests on the common trend assumption. We now add a third difference to this setup, based on the assumption that non-disadvantaged youth remained unaffected by the reform. This makes it possible to include municipality-by-year fixed effects in the regression model, and thus remove the assumption of common trends. To implement the triple difference strategy, we estimate a version of Equation (3) for boys age 18-19 setting $\theta_{ND} = 0$ and adding municipality-by-year dummy variables to the regression model. Table 4, column (2), presents the result. Although the foundation for identification has changed quite substantially, it is notable that the estimate of the reform effect

is almost identical to that based on the DiD strategy. The DiD setup does not permit accounting for municipality-by-year fixed effects, but when we instead augment the model with municipality-specific linear trends, the estimated reform effect is indistinguishable from that from the triple difference estimate; see column (3).

A concern with the identification strategy is that tightening of welfare policy might induce selective migration, such that disadvantaged youth prone to go on welfare move to other municipalities around the time of the reform in order to circumvent the stricter requirements. Although Edmark (2009), analyzing Swedish activation programs similar to those we study, uncovers no evidence of migration effects, Fiva (2009) finds that the generosity of local welfare policies affects residential choice in Norway; hence we need to take the possibility of selective migration seriously. In the fourth column of Table 4 we therefore report results 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) estimate of the reform effect is very similar to our baseline estimate.

In a second check on the roles of selective migration and reform endogeneity, we exclude observations from the three years just prior to the reform in order to avoid that our treatment effect estimate captures temporarily high crime rates in the years immediately preceding the reform. Again, the result appears very robust; see Table 4, column (5).

As a final check of whether our baseline estimate reflects sorting, we re-estimate the regression model including family fixed effects, controlling for all unobserved factors shared by brothers. In this, we restrict the sample to families with at least two sons in the data (29 068 families). Identification of the reform effect draws on brothers in treatment municipalities on each side of

the reform. Unfortunately, the data set includes only 576 such families, resulting in low precision of the within-family estimator. Nonetheless, as column (6) shows, the within-family estimate of the reform effect is similar to that in column (1). If anything, the various robustness checks indicate that our baseline estimate might slightly understate the true effect of the reform effect on youth crime.

7 Extensive vs. intensive margins

Thus far, we have considered criminal activity as a dichotomous outcome. As we showed in Table 1, however, the average criminal youth commits more than one offence during the year.[12] Hence, in addition to the extensive margin examined so far, there are potentially reform effects also along the intensive margin.

Figure 3 illustrates this further by presenting the pre and post-reform cumulative distributions of the total number of criminal cases for disadvantaged youth in the treated municipalities. Starting to the left in the figure we see that those having one case only account for 11-12 percent of the total number of cases, whereas boys with seven or more cases per year accounted for roughly 50 percent of all cases. Interestingly, the pre and post-reform distributions look quite similar. In fact, a two-sample Kolmogorov-Smirnov test for equality of distribution functions fails to reject the null hypothesis that the two distributions are similar (p-value 0.60). That the reform changed the fraction of youth committing crimes, but not the distribution of the number of cases per person, points towards that the reform mainly affecting the extensive margin, leaving the intensive margin largely unchanged.

[12] Not shown in tables, in the sample of disadvantaged youth age 18-19 offenders are on average convicted of 3.5 crimes per year. The distribution is highly skewed with the 10 percent most active offenders responsible for more than 40 percent of all offences. Further, almost 60 percent of the offenders have committed more than one crime.

In Table 5 we address this further as we examine reform effects across the distribution of criminal offences per individual, using the same regression model as in Table 4, column (1).

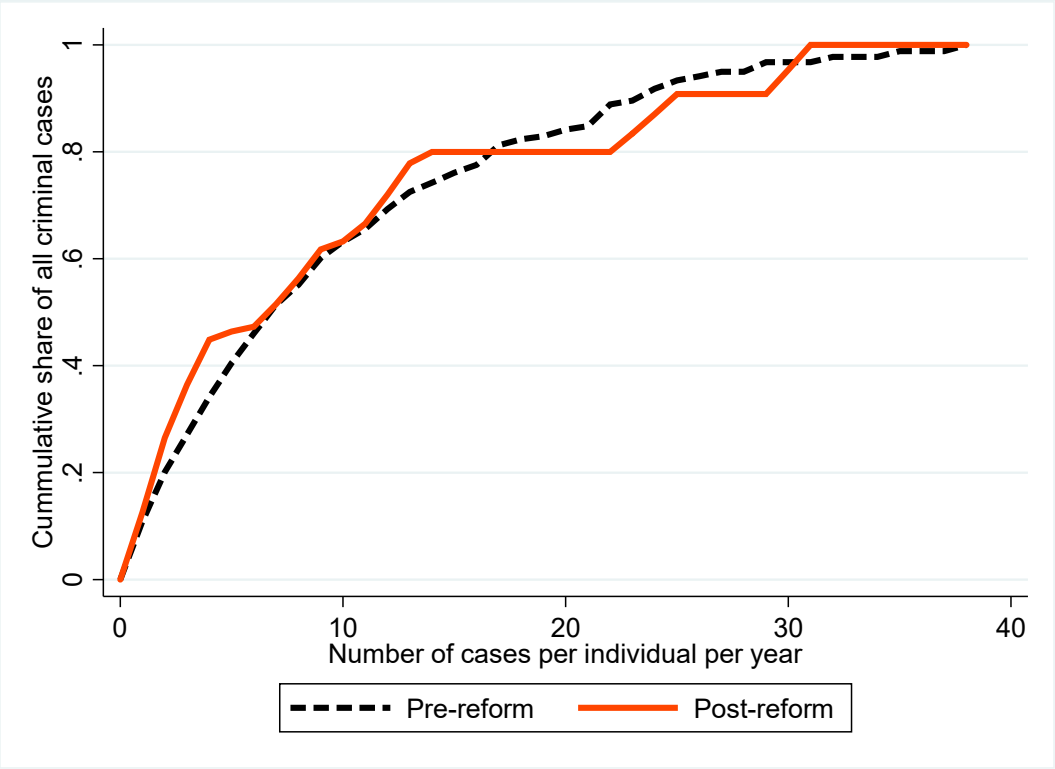

Fig 3: Pre and post-reform distributions of criminal offences by the number of cases per individual

Note: Figure shows the cumulative distribution of criminal offences across perpetrators by the number of charges per year in the 43 treatment municipalities before and after the reform. Sample is limited to boys age 18-19 from disadvantaged families. Sample sizes are 16 031 and 4 888 youth in the pre and post-reform periods, respectively.

First, column (1) repeats the estimated extensive margin effect in the sample. Columns (2)-(4) then show estimates obtained when we use indicators for two or more, four or more, or six or more criminal offences per year as the dependent variable. Focusing on the coefficient estimates normalized by the pre-reform mean (bottom row of the table), we note that the relative impacts are of similar size, around 35 percent, across the distribution of charges. This confirms the descriptive evidence presented above, that the extensive margin is the main channel for the reform effects. In column (5) we show the estimated effect using the number of offences as the

dependent variable. The relative impact on the number of charges are very similar to those in the preceding columns, which strengthens the conclusion that the reform first of all affected crime at the extensive margin. This is also confirmed by column (6) where we show the effect on the number of charges conditional on having at least one charge. The coefficient here is positive, small and not statistically significant.

Table 5: Estimated reform effects across distribution of criminal charges, boys age 18-19 from disadvantaged families

	(1) Crime (at least one offence)	(2) Two or more offences	(3) Four or more offences	(4) Six or more offences	(5) Number of offences (linear model)	(6) Number of offences (conditional on > 0)
Reform effect	-0.019***	-0.009**	-0.007***	-0.004*	-0.057*	0.153
	(0.005)	(0.004)	(0.003)	(0.002)	(0.034)	(0.537)
Pre-reform mean	0.053	0.031	0.016	0.010	0.170	3.521
Coefficient/mean	-0.352	-0.305	-0.419	-0.345	-0.333	0.044

*/**/***Statistically significant at the 10/5/1 percent level.
Note: Standard errors are clustered within 201 municipalities. Regressions have 78 474 (cols 1-5) and 3 778 (col 6) 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. Dependent variable in cols 5 and 6 is the number of criminal charges during the year. Sample in col 6 is restricted to youth with at least one criminal charge.

8 Mechanisms

In this section, we discuss why stricter social assistance activation requirements reduce crime rates among 18 and 19-year old boys from disadvantaged families. As the reform caused a decline in social assistance claims and led to a higher rate of high-school completion (Hernæs et al., 2017), it is natural to understand the reform's crime-reducing effect in the context of its impacts on school dropout and youth take-up of social assistance. Table 6, columns (1)-(4), display estimates of reform effects on the combined states of social assistance and school enrollment. The estimates show that, following the reform, school enrollment increased by 1.4

percentage point (i.e., the sum of coefficients in columns 1 and 2) whereas social assistance claims declined by 3.1 percentage points (sum of columns 2 and 4). Notably, the fraction of youth enrolled in school without claiming social assistance increased by fully 2.8 percentage points (column 1) while the fraction combining school and social assistance fell by 1.4 points (column 2). Moreover, the share of disadvantaged youth not in school *and* claiming social assistance fell by 1.7 percentage point (28%; see column 4). As the group combining enrollment and social assistance will include a number of youth in the process of dropping out of school,[13] the significant, negative coefficients in columns (2) and (4) hint that the activation requirements of the reform discouraged youth from leaving school for social assistance. Moving on to the decomposition of the reform's crime-reducing effect in columns (5)-(8), we note that most of the crime reduction is indeed associated with the drop in social assistance claims. Although interpretation of coefficient estimates is impeded by the significant reduction in both groups, the estimates in columns (6) and (8) show a particularly large decline in the

Table 6: Estimated reform effects on combinations of school enrollment, social assistance (SA), and crime; boys age 18-19 from disadvantaged families

	(1) Enrolled/ no SA	(2) Enrolled/ SA	(3) Dropout/ no SA	(4) Dropout/ SA	(5) Crime/ enrolled/ no SA	(6) Crime/ enrolled/ SA	(7) Crime/ dropout/ no SA	(8) Crime/ dropout/ SA
Reform effect	0.028***	-0.014**	0.003	-0.017***	-0.004	-0.009***	-0.000	-0.005**
	(0.009)	(0.007)	(0.007)	(0.005)	(0.003)	(0.002)	(0.002)	(0.002)
Pre-reform mean	0.743	0.086	0.112	0.059	0.018	0.013	0.007	0.014
Coeff/mean	0.038	-0.164	0.022	-0.281	-0.241	-0.665	-0.045	-0.372

*/**/***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.

[13] Not shown in the table, among those who combined school and social assistance in the spring semester, 55 percent were not enrolled and 76 percent claimed social assistance in the fall.

crime rate among youth combining enrollment and social assistance and a substantial decline among school dropouts on social assistance.[14]

With this backdrop, the remainder of this section aims at disentangling four main avenues for causal influence. The first is an incapacitation effect operating through time spent in activation or in school, thus leaving less time for criminal activity. The second is a human capital effect working through increased school attendance or participation in activation. The third is a potentially offsetting "necessity crime" effect among those who lose access to social assistance and perhaps resort to crime in order to compensate for the loss of income. And the fourth is a possible peer (or norm) effect on youth not directly exposed to the activation requirements themselves.

8.1 Incapacitation

The positive reform effects on school enrollment, together with the large negative impact on the probability of combining crime and social assistance, point toward the presence of incapacitation effects. To shed further light on the role of incapacitation, Table 7 gives a detailed analysis of reform effects on crime committed on weekdays vs. weekends and on different types of crime. If incapacitation were a chief mechanism behind reduced crime, we would expect effects to be particularly large for crime committed during the time periods typically filled by activation or school, i.e., on weekdays outside holidays. The results reported in Table 7, panel A,

[14] Tests of equality of coefficient estimates in columns (6) and (8) fail to indicate statistically significant differences, whether based on the unadjusted estimates (p-value 0.26) or estimates adjusted for pre-reform means (p-value 0.16).

offer some support for this hypothesis, as the reform-induced crime reductions are significantly larger on weekdays than weekends/holidays.

Table 7: Estimated reform effect on crime by day of week and type of crime, boys age 18-19 from disadvantaged families

	(1)	(2)	(3)	(4)
	Any day of week	Weekday	Weekend only	Test weekday vs weekend
A. Any crime				
Reform effect	-0.019***	-0.013***	-0.005**	-0.008*
	(0.005)	(0.004)	(0.003)	(0.005)
Pre-reform mean	0.053	0.032	0.020	
Coefficient/mean	-0.352	-0.411	-0.260	
B. Property crime				
Reform effect	-0.010***	-0.008***	-0.001	-0.007*
	(0.003)	(0.003)	(0.002)	(0.004)
Pre-reform mean	0.028	0.020	0.008	
Coefficient/mean	-0.341	-0.408	-0.174	
C. Violent crime				
Reform effect	-0.006***	-0.001	-0.004**	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Pre-reform mean	0.014	0.005	0.009	
Coefficient/mean	-0.390	-0.279	-0.456	
D. Drug crime				
Reform effect	-0.007***	-0.004**	-0.003**	-0.001
	(0.003)	(0.002)	(0.001)	(0.002)
Pre-reform mean	0.015	0.009	0.006	
Coefficient/mean	-0.482	-0.440	-0.549	
E. Other crime				
Reform effect	-0.005*	-0.001	-0.004**	0.003
	(0.003)	(0.002)	(0.002)	(0.002)
Pre-reform mean	0.019	0.009	0.010	
Coefficient/mean	-0.255	-0.105	-0.384	

*/**/***Statistically significant at the 10/5/1 percent level.
Note: Note: Standard errors are clustered within 201 municipalities. Regressions have 78 474 observations. Weekend includes school holidays. See also notes to Table 6.

Prior evidence indicates that incapacitation effects induced by more time spent in school are particularly large for property crime, whereas, e.g., violent crime may even increase with school

attendance as a result of more interaction between juveniles (Jacob and Lefgren, 2003; Luallen, 2006). To examine this, Table 7 also reports separate estimated reform effects for crime committed on weekdays and weekends/holidays by crime type. Our results confirm that the differences between weekday and weekend effects are particularly large for property crime. In fact, it is only for property crime that we identify significant differences by day of week. Even so, the evidence in Table 7 reveals sizeable effects on weekend and non-property crimes hinting that the social assistance reform had important implications for youth crime above and beyond that given by mere incapacitation of youth in school or activation programs.

8.2 Human capital

While the direct incapacitation effects are strictly of short-term nature, human capital effects are likely to be more persistent. 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). To assess the presence of such long-term effects of the reform evaluated in this study, we now redefine our crime outcome such that it captures crimes committed at ages higher than 19. A possible challenge here is that the introduction of activation requirements at age 19 also implies that these requirements are in place during the early twenties; hence we may worry that impacts observed at higher ages capture the concurrent effects of activation requirements rather than the effects of exposure at age 18-19. Further, some of the control municipalities may have introduced reforms after our observation window for social assistance reforms, contaminating our treatment at older ages. However, as Section 3 explained, the reform prioritized activation of younger claimants. Besides, in the present context we can

almost rule out these channels as the evidence in Table 3 showed that the reform did not affect the criminal behavior among individuals in their early twenties.[15]

Figure 4 first presents DiD-estimates for the effects of being exposed to activation requirements at age 18-19 on high-school completion and the incidence of any crime at each age between 19 and 25. The estimates show that the favorable crime-reducing effects of activation are highly persistent during the early to mid-twenties, just slightly lower than the contemporaneous effects at ages 18-19. The impact on high-school completion is present already at age 19 (which is the typical graduation age for the academic track), suggesting that the strong long-term effect on crime encapsulates a considerable human capital component. This interpretation is corroborated by a more detailed examination of estimated reform effects at age 25;

[15] Studying long-term outcomes, we track education and crime of the youth in our sample through 2012, which is outside the observation window for reforms. Unfortunately, accounting for both teenage and contemporaneous reforms places severe restrictions on the sample, resulting in thin identification of reform effects. Nonetheless, as shown in appendix Table A8, this exercise yields estimates of teenage reform effects in line with those discussed in this section and no indication of a contemporaneous reform effect on those in their twenties.

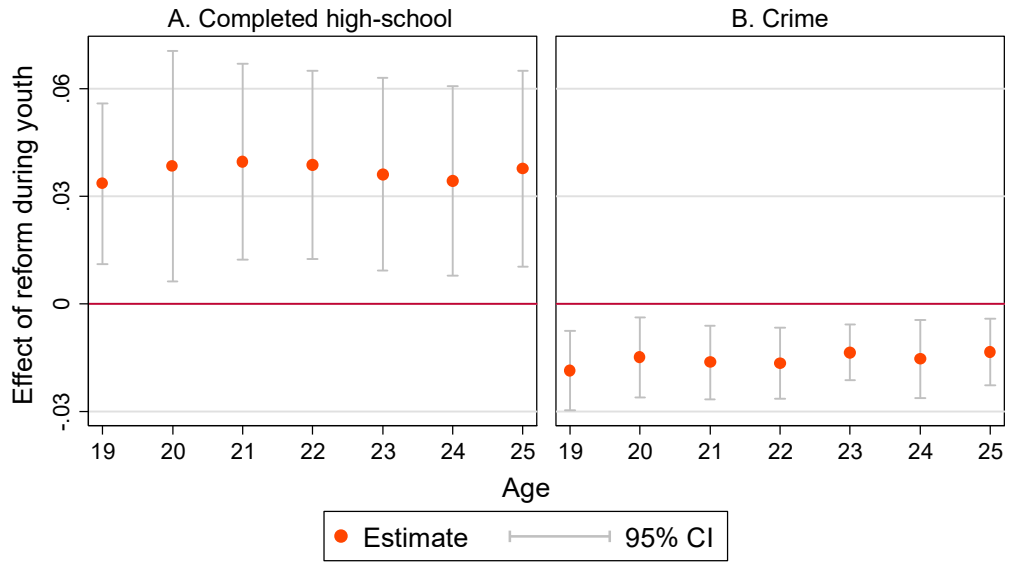

Fig 4: Estimated effect of reform during youth on attainment and crime ages 19-25, boys from disadvantaged families

Note: The figure displays DiD-estimates for the effect of exposure to activation requirements at age 18 on high-school completion and the incidence of any crime at each age between 19 and 25.

see Table 8. In total, the crime rate at age 25 is estimated to have been reduced by 1.5 percentage points (36 %) as a result of being exposed to stricter activation requirements at age 18-19 (column 2). At the same time, the high-school completion rate at age 25 is estimated to have increased by 3.8 percentage points (6.7 %), which is highly significant both from a substantive and a statistical point of view; see column (1).

Table 8 also reports the estimated long-term reform effect across crime types. If human capital accumulation is an important mechanism, we would expect to find the largest effects on criminal activities that are causally reduced by educational attainment. Indeed, the large effect for property crime and no effect for violence, shown in columns (3) and (4), square with prior evidence from Italy (Buonanno and Leonida, 2006) and the United Kingdom (Machin et al.,

2011) showing higher impacts of education on property crime that other types of crime. It should be noted, however, that neither Lochner and Moretti (2004) nor Hjalmarsson et al. (2015) uncover differential effects of education on property and violent crimes in data from the United States and Sweden. Finally, we note that living through the social assistance

Table 8: Estimated effects of reform during youth on high-school completion and crime age 25, boys from disadvantaged families

	(1) Completed high school	(2) Any crime	(3) Property crime	(4) Violent crime	(5) Drug crime	(6) Other crime
Reform effect	0.037***	-0.014***	-0.010***	-0.000	-0.010***	-0.007**
	(0.014)	(0.005)	(0.003)	(0.002)	(0.003)	(0.003)
Pre-reform mean	0.554	0.040	0.020	0.011	0.019	0.015
Coefficient/mean	0.067	-0.333	-0.513	-0.018	-0.530	-0.438

*/**/***Statistically significant at the 10/5/1 percent level.
Note: Standard errors are clustered within 201 municipalities. 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 year, and municipality fixed effects. Samples are restricted to those alive at age 25; regressions have 39 782 observations.

reform during their late teens significantly reduced the incidence of drug and other crimes even at age 25 (see columns 5 and 6), again pointing to the broader implications of tightening activation requirements for young social assistance recipients.

8.3 Necessity crime

Social assistance receipt dropped following the reform (see Table 6), and in spite of the overall reduction in youth crime, it is possible that some disadvantaged youth were pushed into criminal activity by the loss of income support. However, referring back to Table 6, we note that there is no indication of increased criminal activity in combination with not receiving social assistance, despite the fact that the non-SA group became significantly larger following the reform;

see columns (5) and (7) compared to columns (1) and (3). As an additional check on the evidence for increased necessity crimes, we report in Table 9, column (1), the estimated reform effects on crime *conditional* on social assistance status. Again, we find no evidence in support of the hypothesis that criminal activity increased among non-claimants. The reform had a significant negative effect on crime both among claimants and those without social assistance.

An obvious problem with these conditional estimates is that social assistance status is endogenous and strongly affected by the reform. As the composition of the groups with and without social assistance is likely to have changed with the reform, the interaction effect in column (1) is difficult to interpret. We can gain some insight into such compositional change by studying how the reform effect on social assistance claims interacts with criminal activity recorded prior to social assistance eligibility age. For this purpose, we define an additional indicator variable set to unity if some criminal activity was recorded at ages 16 or 17 (and zero otherwise). Although we suspect that peer effects may imply that this variable is not entirely exogenous with respect to the reform (see the next subsection), the interaction effect between

Table 9: Estimated reform effects on crime and social assistance receipt, boys age 18-19 from disadvantaged families

	(1) Crime	(2) Social assistance (SA)	(3) Crime
Reform	-0.011**	-0.013	-0.008*
	(0.005)	(0.008)	(0.004)
Pre-reform mean[i]	0.030	0.105	0.034
Coefficient/mean	-0.377	-0.120	-0.250
Reform*social assistance	-0.033*		
	(0.017)		
Pre-reform mean[ii]	0.307		
Coefficient[iii]/mean	-0.213		
Social assistance	0.137***		
	(0.005)		
Reform*crime age 16/17		-0.125***	-0.057***
		(0.033)	(0.021)
Pre-reform mean[ii]		0.410	0.307
Coefficient[iii]/mean		-0.336	-0.213
Crime age 16/17		0.243***	0.249***
		(0.011)	(0.010)

*/**/***Statistically significant at the 10/5/1 percent level.
[i] Reported mean is for the reference group, i.e., those with no SA (col 1) or no crime age 16/17 (cols 2 and 3).
[ii] Reported mean is for those with SA (col 1) or crime age 16/17 (cols 2 and 3).
[iii] Coefficient is the sum of main and interaction coefficients.
Note: Standard errors are clustered within 201 municipalities. Regressions have 78 474 (col 1) and 63 463 (cols 2 and 3) observations. Sample mean of crime at age 16 or 17 is 0.068. 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.

criminal history and the reform on social assistance take-up is informative about how the reform affected sorting into the social assistance program. The results shown in Table 9, column (2), indicate that those with a criminal record prior to age 18 to a much larger extent than others were steered away from social assistance by the stricter activation requirements. This suggests that, following the reform, non-claimants were less favorably selected in terms of crime propensity. Thus, the finding that criminal activity did not increase within the non-claimant group is unlikely to be explained by sorting, strengthening the evidence that the reform did not push

disadvantaged youth into necessity crime. The final column of Table 9 shows estimated reform effects on crime at ages 18-19, conditional on crime at ages 16-17. Youth crime is highly persistent (the lagged coefficient is 0.25), but the reform led to reduced crime in both groups and, in relative terms, the reform effect is very similar for disadvantaged youth with and without a criminal history.

8.4 Peer effects

Despite contributing to increased school enrollment, the reform significantly reduced the frequency of the combined outcome of enrollment and crime; confer Table 6, columns (5) and (6). This indicates a considerable drop in criminal activity even among those who would have stayed in school regardless of the reform, suggesting that there must have been some crime-reducing reform effects beyond the incapacitation and human capital mechanisms discussed above. One possibility is that the reform generated some knock-on effects through peer influences. Youth crimes are frequently committed by companions together; hence for each crime event there are typically more than one youth – and thus more than one criminal charge – involved. Peer effects on crime are hard to identify, but recent evidence from Dutch data suggests that juvenile crime is positively (but weakly) affected by the offender rate in the neighborhood of residence (Bernasco et al., 2017).

To examine the case for peer effects, we again use the incidences of crime committed at ages 16 and 17, this time as an outcome measure. We estimate the effect of the reform on crime among boys age 16-17, but as those under 18 are not entitled to social assistance, they will not be directly affected by the reform. However, if the reform reduced crime among their older peers, we would expect that these younger boys to a lesser extent were pulled into criminal activities in the presence of peer effects. Table 10 presents the estimated reform effects on school enrollment and crime for 16 and 17-year old boys in our data. The point estimate in

column (2) indicates that there indeed may have been a crime-reducing effect of the reform even for minors. The effect estimate is much lower than for 18-19 year olds and although not statistically significant at conventional levels (p=0.155), we interpret the size of the point estimate as indicative of peer influences.

Table 10: Estimated reform effects on minors age 16 and 17, boys from disadvantaged families

	(1) Enrolled in school	(2) Crime	(3) Weekday crime	(4) Weekend crime only	(5) Test weekday vs weekend
Reform effect	0.002	-0.008	-0.007*	-0.001	-0.006
	(0.005)	(0.006)	(0.004)	(0.003)	(0.004)
Pre-reform mean	0.931	0.046	0.028	0.018	
Coefficient/mean	0.002	-0.181	-0.252	-0.067	

*Statistically significant at the 10 percent level.
Note: Standard errors are clustered within 201 municipalities. Regressions have 77 396 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.

When we split the crime effect for minors by day of week, there are indications that the effect is concentrated on weekdays rather than weekends, which is the same pattern as that uncovered for 18 and 19-year old boys in Table 7. However, as we see no effect on school enrollment among minors and therefore no school incapacitation effect (see column 1), the day-of-week pattern points to peer influences from the older boys as the plausible channel. The indication of peer effects among minors also suggests that the overall reform effects seen among 18 and 19-year olds are partly explained by social interactions.

While the small crime-reducing effect of the reform on minors is consistent with peer effects, it also reassuringly confirms that the larger effects identified for 18 and 19-year olds are not driven by other contemporaneous policy changes in reforming municipalities, e.g., related to educational policy. If that were the case, we would have expected to find school enrollment

effects also for minors as well as crime effects more similar to those uncovered for 18 and 19-year old boys.

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 youth with a disadvantaged background. We find significant effects on all forms of crime: property, violence, drugs, and other crimes. We also confirm previously reported evidence that activation requirements reduced social assistance take-up and lowered high-school dropout. The results appear robust as there are parallel trends in youth crime in treatment and control municipalities during pre-reform years, and the estimated reform effect is not sensitive to alternative identification strategies and a number of specification checks.

The crime-reducing effects are concentrated among 18 and 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 35 percent reduction in the probability of committing a detected crime. We present evidence that the favorable effects partly arise from an incapacitation effect related to participation in activation and/or school attendance, possibly in combination with impacts of a more structured daily life. We also find considerable long-term effects in the form of increased high-school completion and reduced crime rates at ages 20 through 25, suggesting that human capital effects are important. It appears that the activation requirements of the reform made life on social assistance less attractive, and discouraged some adolescents from dropping out of school. Higher school attendance during teenage years is also likely to raise human capital, implying that the opportunity cost of committing crimes may have

increased for some youth. As the effects identified in this paper imply reduced crime participation rates also among youth who would have stayed in school even in the absence of reform, we conclude that there have been favorable effects beyond those from incapacitation and human capital investments. We provide evidence that these effects involve peer influences. In addition to a possible peer effect operating through the transmission of social norms, we argue that a plausible channel for peer effects in youth crime is that many of these crimes are committed in groups, and thus involve more than one offender.

Importantly, we find no indication of an offsetting crime-inducing effect among those without benefits. If anything, the probability of committing crime without social assistance declines slightly. In a generous welfare state, with extensive insurance for individuals with low income, moral hazard may induce young people to leave school. Social assistance in combination with strict activation requirements for youth may achieve both a considerable reduction in 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 programs and in education appears to substitute for time spent on criminal activities.

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Online appendix tables to accompany

"Welfare Activation and Youth Crime"

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Table A1: Sample characteristics in treated, control, and excluded municipalities, 1996 and 2006

	Treated		Control		Excluded	
	1996	2006	1996	2006	1996	2006
Crime	0.026	0.027	0.026	0.029	0.025	0.026
Social assistance	0.079	0.053	0.072	0.054	0.071	0.051
In school	0.767	0.804	0.759	0.806	0.761	0.810
Father earnings age 1-10	41 683	44 566	40 628	43 610	43 170	47 593
Mother earnings age 1-10	9 803	16 914	10 703	18 017	11 165	19 080
Father high school	0.535	0.517	0.530	0.513	0.506	0.476
Father college	0.199	0.213	0.198	0.221	0.245	0.279
Mother high school	0.535	0.429	0.531	0.421	0.522	0.402
Mother college	0.151	0.220	0.157	0.236	0.186	0.281
Unemployment rate	0.033	0.020	0.029	0.019	0.033	0.021
Observations	11 502	11 019	29 303	28 630	59 285	58 618
Number of municipalities	43		158		227	

Note: Samples are restricted to boys age 18-21, born in Norway to two Norwegian-born parents. Earnings are annual, inflated to 2017 currency and converted to USD using the average 2017 exchange rate.

Table A2: Logit regression model used to predict youth social assistance propensity

	(1) Mean	(2) Coefficient (standard error)
Log father's earnings age 1-10	14.912	-0.540***
		(0.010)
Log mother's earnings age 1-10	13.545	-0.139***
		(0.005)
Father's earnings missing	0.012	0.881***
		(0.059)
Mother's earnings missing	0.045	0.449***
		(0.032)
Father deceased age 10	0.009	-0.111**
		(0.054)
Mother deceased age 10	0.003	0.101
		(0.097)
Father's education		
Short upper secondary	0.245	-0.462***
		(0.016)
Upper secondary	0.210	-0.559***
		(0.018)
Some college	0.039	-0.539***
		(0.037)
College	0.165	-0.888***
		(0.025)
Master's degree	0.087	-1.113***
		(0.043)
PhD	0.007	-1.196***
		(0.157)
Missing	0.013	0.015
		(0.059)
Mother's education		
Short upper secondary	0.310	-0.631***
		(0.015)
Upper secondary	0.125	-0.823***
		(0.024)
Some college	0.026	-0.820***
		(0.050)
College	0.205	-1.062***
		(0.025)
Master's degree	0.023	-1.347***
		(0.090)
PhD	0.001	-1.833***
		(0.584)
Missing	0.003	0.535***
		(0.075)

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

Note: Standard errors are reported in parentheses. Dependent variable is an indicator for social assistance receipt between ages 18 and 21. Sample consists of boys born 1973-1990 to two Norwegian-born parents and who resided in one of the 228 municipalities not included in our study population. Regression has 266 711 observations and controls for birth year.

Table A3: Reform effect on crime, boys from disadvantaged families. Separate regressions by age

	(1) Age 18	(2) Age 19	(3) Age 20	(4) Age 21	(5) Age 18-21
Reform effect	-0.019***	-0.018***	-0.004	-0.004	-0.038***
	(0.006)	(0.006)	(0.007)	(0.006)	(0.010)
Pre-reform mean	0.050	0.055	0.053	0.048	0.123
Coefficient/mean	-0.387	-0.320	-0.078	-0.072	-0.307
Observations	39 093	39 381	36 593	33 771	32 688

*/**/***Statistically significant at the 10/5/1 percent level.
Note: Standard errors are clustered within 201 municipalities. Dependent variable in col 5 is an indicator for whether the youth committed any crime between ages 18 and 21; the reform indicator is set to unity if the municipality implemented the reform before age 18 and the sample is restricted to non-movers. 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 year and municipality fixed effects.

Table A4: Reform effects on crime and combinations of school enrollment and social assistance (SA), girls age 18-19 from disadvantaged families

	(1) Crime	(2) Enrolled/no SA	(3) Enrolled/SA	(4) Dropout/no SA	(5) Dropout/SA
Table reference:	4(1)	6(1)	6(2)	6(3)	6(4)
Reform effect	-0.000	0.015*	-0.004	-0.003	-0.009*
	(0.002)	(0.008)	(0.007)	(0.006)	(0.005)
Pre-reform mean	0.006	0.760	0.096	0.085	0.059
Coefficient/mean	-0.065	0.020	-0.038	-0.036	-0.146

*Statistically significant at the 10 percent level.
Note: Standard errors are clustered within 201 municipalities. Regressions have 74 576 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.

Table A5: Estimated reform effects, logit coefficients; boys from disadvantaged families

	(1) Crime	(2) Dropout/ no SA	(3) Dropout/SA	(4) Weekday crime	(5) Weekend crime only	(6) Crime age 25
Table ref:	4(1)	6(3)	6(4)	7(2)	7(3)	8(2)
Reform	-0.415***	0.030	-0.394***	-0.497***	-0.267**	-0.495**
	(0.102)	(0.080)	(0.109)	(0.149)	(0.130)	(0.202)

/*Statistically significant at the 5/1 percent level.
Note: Standard errors are clustered within 201 municipalities. Sample in cols 1-5 consists of boys age 18 and 19 and has 78 474 observations; col 6 has 39 782 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.

Table A6: Reform effect on crime, boys from disadvantaged families. Additional robustness checks

	(1)	(2)	(3)	(4)
Reform	-0.020***	-0.018***	-0.017***	-0.016***
	(0.005)	(0.005)	(0.005)	(0.005)
Reform*local unemployment	-0.290			
	(0.409)			
Pre-reform mean	0.053	0.054	0.050	0.052
Coefficient/mean	-0.376	-0.336	-0.330	-0.302
Observations	78 474	75 565	67 975	65 066
Municipalities	201	194	184	177
Comment	Interaction model	Drop early reformers	Drop late reformers	Drop early and late reformers

***Statistically significant at the 1 percent level.
Note: Standard errors are clustered within municipalities. Sample in col 2 omits municipalities that implemented the reform before year 2000, and that in col 3 municipalities that implemented the reform after 2003. 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. In col 1, main effect is evaluated at sample mean of local unemployment.

Table A7: Family background, local unemployment, and adult welfare caseload by reform status

	(1) Father earnings age 1-10	(2) Mother earnings age 1-10	(3) Father high school	(4) Father college	(5) Mother high school	(6) Mother college	(7) Local unemployment rate	(8) Welfare caseload 30+
Panel A								
Reform	-0.001	0.000	-0.009	0.004*	0.008	-0.000	-0.002**	-0.002
	(0.002)	(0.002)	(0.014)	(0.002)	(0.011)	(0.002)	(0.001)	(0.001)
Panel B								
Reform	-0.003	0.004	-0.036**	0.002	0.013	0.002	0.001	-0.000
	(0.004)	(0.002)	(0.016)	(0.003)	(0.014)	(0.003)	(0.002)	(0.001)

*/**Statistically significant at the 10/5 percent level.
Note: Standard errors are clustered within 201 municipalities. Regressions in cols 1-6 have 78 474 observations; cols 7-8 have 2 972 observations. Dependent variable in col 8 is the ratio of social assistance clients age 30-66 to municipality population age 30-66; pre-reform sample mean is 0.036. Models control for year and municipality fixed effects. Panel B adds municipality-specific trends.

Table A8: Estimated effects of contemporaneous vs. teenage reform on crime ages 20-25; boys from disadvantaged families

	(1) Completed high-school	(2) Any crime	(3) Property crime	(4) Violent crime	(5) Drug crime	(6) Other crime
A. Table 8 estimates						
Reform during teens	0.037***	-0.014***	-0.010***	-0.000	-0.010***	-0.007**
	(0.014)	(0.005)	(0.003)	(0.002)	(0.003)	(0.003)
B. Replicate Table 8 in restricted sample, age 25						
Reform during teens	0.044	-0.015	-0.001	0.001	-0.012*	-0.009*
	(0.045)	(0.016)	(0.010)	(0.007)	(0.006)	(0.005)
C. Replicate Table 8 in restricted sample and stacked data, ages 20-25						
Reform during teens	0.032	-0.012**	-0.008**	-0.002	-0.004	-0.006*
	(0.020)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)
D. Extended specifi-cation						
Reform during teens	0.023	-0.013**	-0.008**	-0.002	-0.003	-0.007**
	(0.020)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)
Contemporaneous reform	0.017	-0.000	0.001	-0.002	-0.002	0.002
	(0.011)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)

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

Note: Standard errors are clustered within 201 municipalities. Panel A has 39 782 observations; panel B restricts the sample to those with reform data at ages 19 and 25 and has 24 577 observations; and panels C and D have 186 118 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 (panels C and D), year, and municipality fixed effects.

Table A9: Estimated effects of reform on crime age 19 by social assistance receipt age 18; boys from disadvantaged families

	(1)	(2)	(3)	(4)
	Table 9, col (1)	Age 19 only, SA age 18	Age 19 only, SA age 18	Age 19 only, SA age 18
Reform	-0.011**	-0.010*	-0.011**	-0.015
	(0.005)	(0.006)	(0.005)	(0.010)
Pre-reform mean[i]	0.030	0.039	0.040	0.051
Coefficient/mean	-0.377	-0.256	-0.284	-0.293
Reform*social assistance	-0.033*	-0.046*	-0.047*	-0.009
	(0.017)	(0.027)	(0.027)	(0.044)
Pre-reform mean[ii]	0.307	0.199	0.202	0.250
Coefficient[iii]/mean	-0.213	-0.283	-0.288	-0.097
Social assistance	0.137***	0.146***	0.147***	0.145***
	(0.005)	(0.008)	(0.008)	(0.010)
Comment			Add reform year	Keep only reform and next year in treatment municipalities
Observations	78 474	39 381	40 102	30 326
Of which post-reform	4 888	2 392	2 392	727

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

[i] Reported mean is for the reference group, i.e., those with no SA.

[ii] Reported mean is for those with SA.

[iii] Coefficient is the sum of main and interaction coefficients.

Note: Standard errors are clustered within 201 municipalities. 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. Mean social assistance take-up rate at age 18 is 0.080 in cols 2 and 3 and .076 in col 4.