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The effect of education policy on crime:
an intergenerational perspective

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The effect of education policy on crime: an intergenerational perspective*

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Abstract

A number of studies have shown that education reforms extending compulsory schooling reduce criminal behavior of those affected by the reform. We consider the effects of a major Swedish educational reform on crime by exploiting its staggered implementation across Sweden. We first show that the reform reduced crime rates for the generation directly affected by the reform. We then show that the benefits extended to the next generation with large reductions in the crime rates of the children of those affected. The effect operates only through the father and points in the direction of improved parenting rather than resources.

Keywords: Comprehensive school; economics of crime; returns to education; returns to human capital
JEL-code: I20; I21; I28; K42; N34

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1 Introduction

Crime imposes huge costs on society and has an exceptionally strong intergenerational link. Earlier papers have demonstrated that crime and education are related and that indeed policies that increase education can reduce crime (see Lochner and Moretti (2004)). This is important because it shows the broader impact of educational reform and a way of improving outcomes for adults, beyond deterrence and punishment. There are strong theoretical reasons why this should be the case (Becker (1981), Lochner (2004), Freeman (1999)). An outstanding question is, however, to what extent education policies have long term effects on criminal behavior in the sense that it also affects criminal behavior of the children of those directly affected by educational reforms. There are good reasons to expect so, considering the strong intergenerational correlations in criminality and the fact that education policies can affect parental resources as well as skills important for parenting.

In this paper we empirically demonstrate that education policy, which increases compulsory schooling, can reduce the crime rates of the children of those directly affected by the reform. We study the effects on criminality of the comprehensive school reform in Sweden that was implemented as a social experiment between 1949 and 1962. Meghir and Palme (2005) show that this reform significantly increased the number of years of schooling as well as labor earnings for those assigned to the reform, in particular for children from low SES families. We show that the children who were assigned to the reform significantly decreased their criminal activities later in life. We then go on to demonstrate that the male children of those affected by the reform have substantial reductions in crime rates. The effect is only present if the father was affected by the reform. This points away from increased resources as the main mechanism changing the intergenerational

outcomes and more towards improved parenting and the importance of role models (see Cunha, Heckman, and Schennach (2010)).

The two earlier papers by Lochner and Moretti (2004) and Machin, Marie, and Vujić (2011) respectively study the relation between compulsory schooling laws and criminal behavior. Lochner and Moretti (2004) use changes in compulsory schooling laws across time between US states to identify the effect of increasing education on crime. Machin, Marie, and Vujić (2011) compare criminal behavior of the cohorts just before and just after the implementation of the comprehensive school system in Britain. We use data containing individual information on all convictions and prison sentences, along with detailed background characteristics covering the entire population. The dataset also links information on three generations. In addition to that, our paper extends two important aspects of the previous literature. First, we compare the criminal behavior of two groups, distinguished by the school system they were exposed to, but active in the same labor markets at the same point in time, and who belong to the same cohort. This means that we are able to identify the effect of the education reform net of general equilibrium effects; separately from possible cohort effects, effects originating from regional or local labor market shocks; or any secular trends in criminal behavior on the national level. Second, by linking the individuals affected by the reform to data on their children we are then able to estimate the effect of the reform on the next generation. Our result point out the importance that educational reform can have on improving intergenerational outcomes in ways not documented before.

The paper is organized as follows: Section 2 discusses previous theoretical and empirical work on the relation between both own education and criminal behavior as well as parental education and criminal behavior; Section 3 provides an overview of the comprehensive school reform in Sweden; Section 4 describes the data; Section 5 presents empirical results

on the association between educational attainments and criminal behavior as well as intergenerational associations of crime; Section 6 discusses our identification strategy; Section 7 presents the effects of the education reform on educational attainment of both generations before showing our main empirical results on the effect of the comprehensive school reform and various crime outcomes of both generations followed by empirically results on possible mechanisms; Section 8 concludes.

2 The Impact of Education on Crime

2.1 The Impact of Education on Crime within a Generation

The links between economic incentives and crime have been established both theoretically and empirically in earlier studies. A prominent example is Freeman (1999) who outlines an economic model of crime where the choice between criminal and legal activity is determined by comparing the expected utility of each. Grogger (1998), Gould, Weinberg, and Mustard (2002) Machin and Meghir (2004) and Edmark (2006)[1] demonstrate the importance of wages and labor market opportunities in driving crime. One implication of this is that improved education may reduce crime.

A number of papers have looked at the link between education and crime directly. These include Lochner and Moretti (2004) and Machin, Marie, and Vujić (2011), cited above. A more theoretically based approach was offered by Lochner (2004) who develops a life cycle model of education and crime and estimates a negative education-crime relationship. A study, based on this human capital approach by Williams and Sickles (2002) finds that years of schooling has a significant negative effect on crime in adulthood,

[1]For Sweden Edmark (2006) shows the relationship between unemployment rates and property crimes on county level.

and that there is a relationship between crime and other measures of human capital. Earlier studies support this empirical evidence on the education-crime relationship; for example Freeman (1996) states for the 1991 US Census that two thirds of US prison inmates are high-school drop-outs and 12 percent of 24-35 year old high school drop outs were incarcerated in 1993. This negative correlation between crime and education has also been documented in the criminology and sociology literature, for example Sabates and Feinstein (2008a).[2]

In the Appendix to this paper we develop a simple theoretical model in order to better understand the mechanisms through which an education reform may affect criminal behavior of both the cohort directly affected by the reform and their children. The model shows that an increase in compulsory schooling reduces the available time for crime early on;[3] it increases human capital and thus reduces further the incentive to commit crimes and may increase the chance of remaining in school beyond the new compulsory level. It may also draw increased investments from parents further increasing human capital. This reduces crime in the young (school period) ages. As an adult, the result is increased human capital, which will reduce adult crime. If there is a habit formation aspect of crime, the early decline will be reinforced. Thus crime will decline relative to the group that was not affected by the reform.

An education reform may also generate general equilibrium effects. Gallipoli and Fella (2006) estimate a general equilibrium model of crime and education. They find that increases in education have a significant impact in reducing crime. However, they point out that the general equilibrium effects, operating through changes in wages as the number of educated individuals increases, can be substantial.

[2]See also Sabates and Feinstein (2008b).

[3]Jacob and Lefgren (2003) give some evidence on this effect and refer to it as the incapacitation effect.

As shown in Meghir and Palme (2005) the Swedish school reform significantly increased the number of years of schooling as well as labor earnings of those individuals who went through the new school system, in particular for individuals originating from homes with low educated fathers, i.e. not more than statutory level of schooling. For those individuals we would expect a decrease in criminal behavior due to the reform. For individuals affected by the reform but having parents with more than statutory education the impact is less clear cut. For this group there is no effect on educational attainment. However, it changed the way they were educated because it abolished early selection and tracking, which affected primarily this group. It can be argued that quality of education for this group was diluted for this reason *and* because the increase in compulsory schooling, affecting the other group, could have reduced the quality of the peers.[4] For this reason we cannot be confident that human capital increased for this group. This is why in our empirical analysis we present overall results as well as results separately on the lower socio-economic group.

2.2 Parental Education and Children's Crime

Intergenerational associations of criminal behavior have been documented in the criminology literature. From the economics point of view this question relates to the investments that parents make on their children and the way that parental education may affect such investments, see Becker (1981).[5] In the Swedish context Hjalmarsson and Lindquist (forthcoming) document a strong correlation between crime of fathers and children of both genders using the Stockholm Birth Cohort Study. In a second

[4] A previous study by Deming (forthcoming) highlights the importance of school quality and it's potential impact on crime.

[5] For some empirical work see for example Carneiro, Meghir, and Parey (forthcoming). Moreover, there is direct evidence that better childhood environments and early education can reduce crime rates, see for example the results form the Perry pre-school experiment presented in Schweinhart, Montie, Xiang, Barnett, Belfield, and Nores (2005) and Cunha and Heckman (2007).

Swedish study the same authors Hjalmarsson and Lindquist (2010) focus on parent-child correlations in crime using adoption data, to aim to determine through which factors mothers and fathers influence child criminality, which follows the approach of Björklund, Lindahl, and Plug (2006).

The theoretical model presented in the Appendix does also help us to better understand the possible effects of the criminal behavior of children to the cohort primarily affected by the reform. The children of the affected generation all experience the same education system because the reform was rolled out nationally in 1962. They only differ by the fact that some have parents who faced the new education system and as a result have more parental education and more resources. These differences will lead to higher parental investments in their children and eventually higher educational attainment relative to the children in the comparison group, whose parents did not go through the reform, see for example Holmlund, Lindhal, and Plug (forthcoming). Educational attainment may increase because, according to mounting evidence, an increase in early parental investments in children improves cognition and social skills and hence reduces the costs of education. In addition, the increased parental resources allow more transfers to children alleviating financial constraints for education. These channels imply an increase in human capital reducing crimes at all life stages, as described above.

In addition to the mechanisms brought forward by our theoretical model, one can think of at least four indirect effects of parental reform assignment on child criminal behavior. These effects are addressed empirically in Section 7.5 on mechanisms. *(1) Assortative mating.* In the context of an educational reform, this may imply that those assigned to the reform tend to get married with people with higher educational attainments and/or earnings, which may have an augmenting effect on parental resources; *(2) Fertility.* There may be a causal effect of the attained educational level

on fertility behavior, see e.g. Hotz, Klerman, and Willis (1997) which may affect the criminal behavior of the children, since the parents are able to devote more resources to fewer children; *(3) Parental criminal behavior.* Previous empirical research has shown a very strong link in criminal behavior across generations. Although, it is not likely to be an entirely causal effect it is conceivable that parts of it come from parental role model effects. Since there may be a direct effect of the reform on parental criminal behavior, there may also be a secondary effect on the children's risk of being convicted for a crime; *(4) Mobility.* We know from previous studies that there is a strong element of peer group effects in criminal behavior, see e.g. Glaeser, Sacerdote, and Scheinkman (1996). The education reform may have induced those assigned to the reform in the parental generation to move out from criminal areas, which, in turn, may have affected the criminal behavior in the child generation.

3 The 1950 Swedish Education Reform and the Social Experiment

3.1 The Reform

Prior to the implementation of the comprehensive school reform, pupils attended a common basic compulsory school (*folkskolan*) until grade six. After the sixth grade pupils were selected to either continue one or, in mainly urban areas, two years in the basic compulsory school, or to attend the three year junior secondary school (*realskolan*). The selection of pupils into the two different school tracks was based on their past grades. The pre-reform compulsory school was in most cases administered at the municipality level. The junior secondary school was a prerequisite for the subsequent upper secondary school, which, in turn, was required for higher education.

By 1940 a consensus emerged that the education system had to be reformed. First, by that time, Sweden, compared to other countries, had a relatively short compulsory education: the student finished compulsory school at age 13 or 14. As a comparison, enrolment rates in high-schools were above 80 percent in most parts of the United States (see Goldin (1999)). Second, an increasing proportion of students wanted to continue on to junior secondary school. The share of students who actually continued in the junior secondary school increased from about 10 percent in 1930 to about 40 percent in 1950 (see Erikson and Jonsson (1993)). The resources for that kind of education were, however, not sufficient to meet the demand. Finally, the fact that the school curriculum differed across municipalities and that there was no unified path to higher education were seen as limitations of the existing educational system.

In 1948 a parliamentary school committee proposed a school reform that implemented a new nine-year compulsory comprehensive school.[6] The comprehensive school reform had three main elements:

1. An extension of the number of years of compulsory schooling to 9 years in the entire country.

2. Abolition of early selection. Although pupils in the comprehensive schools were able to choose between three tracks after the sixth grade - one track including vocational training, a general track, and an academic level preparing for later upper secondary school - they were kept in common schools and classes until the ninth grade.

3. Introduction of a national curriculum. The pre-reform compulsory schools were administrated by municipalities and the pre-reform curriculum varied between municipalities.

[6]The school reform and its development are described in Meghir and Palme (2003), Meghir and Palme (2005), and Holmlund (2007). For more detailed reference on the reform, see Marklund (1980) and Marklund (1981).

3.2 The Social Experiment

The social experiment with the new comprehensive nine-year compulsory school started during an assessment period between 1949 and 1962, when the final curriculum was decided.[7] The proposed new school system, as described above, was introduced in municipalities or parts of city communities, which in 1952 numbered 1,055 (including 18 city communities). The selection of municipalities was not random. However, the decision to select the areas was based on an attempt to choose areas that were representative for the entire country, both in terms of demographics as well as geographically. At first the National Board of Education contacted the municipalities, or sometimes they themselves applied to participate. From this pool of applicants a "representative" sample of municipalities was chosen.

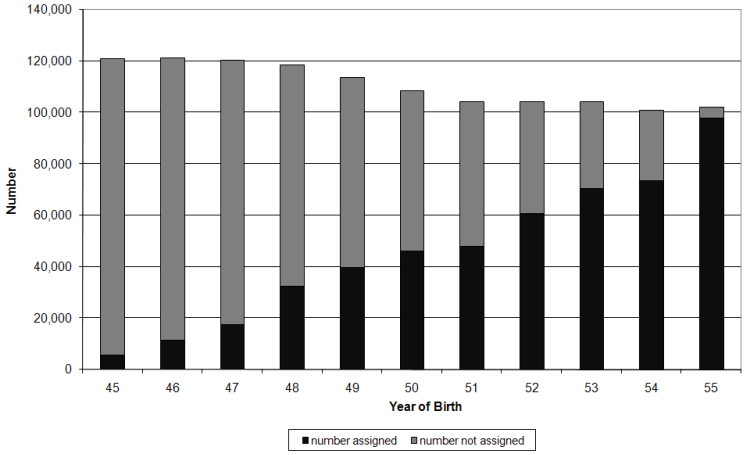
There were at least two reasons as to why a nationwide experiment was set up before the implementation of the new school. First, there was a widespread belief in scientific evaluations among the generation of Swedish politicians who were active at that time, in particular among those involved in education policy.[8] In their view, an experiment was a means for improving different aspects of the proposed new school. Second, and more importantly, it was a way of resolving different views, primarily between those who wanted to maintain the selective school system and those advocating for a comprehensive school, within the parliamentary school committee. An experiment with a comprehensive school was a first step towards a compromise.

When a municipality introduced the new school system it implemented it either for the cohort of pupils who where in fifth grade at the time of the decision or for those who were currently in the first grade, effectively

[7] The official evaluation National School Board (1959) was mainly of administrative nature. Details on this evaluation are also described in Marklund (1981).

[8] See Marklund (1981) for several quotes on that.

Figure 1: Proportion of individuals in sample assigned to the reform



delaying the start of the programme. Table 11 in the Appendix shows the take up rates of the reform between 1949 and 1962. In our analysis we consider cohorts born between 1945 and 1955. Figure 1 shows the number of observations in each one-year birth cohort and the proportion assigned to the reform.

4 The Data

4.1 Data Sources and Descriptive Statistics

We use a sample originally obtained from Sweden's population census. To link individuals across generations we used the multi-generation register, provided by Statistics Sweden.[9] We are able to link and use three generations in our analysis: the *parent generation* which is the generation directly affected by the reform and it consists of all individuals born in Sweden between 1945 and 1955, their parents and their children labeled as the *grandparent generation* and the *children generation*, accordingly.[10]

[9]Statistics Sweden(2003) Flergenerationregistret 2002. En beskrivning av innehåll och kvalitet. Statistics Sweden. Avdelning för Befolknings och Välfärdsstatistik.

[10]Even though we have information on biological and adoptive parents and children, we exclude all individuals who have been adopted, or who have adopted children themselves.

This corresponds to 1,340,857 persons, 658,056 males and 655,801 females in the *parent generation*. From the birth certificates we know date of birth, parish of birth and gender. We restrict our sample of the *children generation* to those who have reached the age of criminal responsibility (age 15) in 2008, the last year for which we have crime records. This corresponds to 1,621,758 children, 833,564 sons and 788,194 daughters in the *children generation* that were born between 1959 and 1991.

The reform assignment variable is obtained in two steps. First, we use the name of the church parish of birth in order to obtain the birth municipality code according to the 1952 Swedish municipality division. Second, based on the year and municipality of birth, we use an algorithm based on historical evidence on reform implementation in each municipality provided by and described in Holmlund (2007) to assign reform status to each individual in the sample of those affected by the reform.

Information on the individual's highest education level was obtained and matched on to our sample from the Swedish National Education Register. For the *grandparent generation* we used data from the 1970 census, which only provides information on individuals younger than age 60 in the year of the census, allowing us to obtain education information for 78.4 percent of mothers and for 65.8 percent of fathers of the *parent generation*. We analyze the effects of the reform separately for those individuals originating from the *low educated grandparent generation*. This is defined as those individuals of the *grandparent generation* with the lowest pre-reform statutory level of compulsory schooling. Hence, we analyze the effects of the reform separately for the *parent generation* with low educated fathers, and for the *children generation* with low educated grandfathers, which amounts to roughly 63-65 percent of the sample with available education information.[11]

[11] Table 12 in the Appendix summarizes the number of available observations in each generation and subgroup.

Information on all convictions in entire Sweden covering the time period between 1981 and 2008 is provided by the Swedish National Council for Crime Prevention (Brå) and has been linked to individuals in our data set using the unique personal identifying number. This means we are able to link individuals to actual convictions, which is an advantage of our study compared to previous studies on education reform effects on criminal behavior (Lochner and Moretti (2004) and Machin, Marie, and Vujić (2011)). We have detailed information on the number of crimes the person has been convicted for in each trial, the date of conviction, as well as the penalty for each crime. One conviction/court trial often covers several crimes.

Table 1: Number of all convictions in Sweden 1981-2008, for cohorts born 1945-1955 and their children

	Ever convicted		Ever convicted to prison	
	Male	Female	Male	Female
Panel A: Parent generation				
Number	173,395	46,633	36,870	3,126
Percent	25.31	7.11	5.38	0.48
Panel B: Children generation				
Number	220,494	69,843	28,588	2,001
Percent	26.45	8.86	3.43	0.25

Table 1 shows the number of convicted persons for the two generations, the 1945-1955 cohorts and their children, covered by our data on convictions. Over this time window, 25 percent of all males in the *parent generation* have been convicted at least once, and over 5 percent have been to prison. Only 7 percent of women have been convicted, and 0.5 percent have received a prison sentence. Importantly, the data on criminal convictions only cover the time range between 1981 to 2008, which means that the generations born between 1945 and 1955 will be between the ages of 26 to 63, whereas their children's convictions cover the ages of 15 to

49. The picture for the children generation looks very similar to the one of their parents, with slightly higher percentages of the population having been convicted, possibly attributable to the younger age window.

Table 13 in the Appendix shows the crime-age distribution for the entire data set not only covering the cohorts of interest. The largest amount of convictions are for people between 15 and 24, followed by the age range 25 to 34, and further decreasing with age. This pattern of convictions by age is also shown in figures 1 and 2 in the Appendix that show the average rate of convictions by age and by cohorts for the cohorts 1970-1989 using men in our children sample.

The stated conviction rates for men of roughly 25 percent is a surprisingly high proportion of the population, which prompted us to look into this in greater detail. First, note that the type of crimes included in our data have to be severe enough to involve a trial and a conviction in court. This includes the more serious traffic violations such as driving without a licence, driving under the influence of alcohol or drugs, and causing bodily harm, but does not include speeding or parking tickets. As such they do represent serious anti-social behavior. Unfortunately, we were not yet given the specific type of crime for which an individual in our data has been convicted for.[12] However, a good idea of the composition of crime can be obtained in Table 15 in the Appendix where we show a breakdown of type of crime convictions in 2009.

In addition to the data on convictions we have data on all suspected crimes between 1991 and 2009. It includes a variable that gives a detailed code on the type of suspected crime.[13] Although this data overstates actual charges and crimes we use it to provide an idea of the distribution of traffic

[12]We are waiting to obtain a variable that indicates the type of crime from the Brå crime registry.

[13]Detailed coding of crime types in: Kodning av brott, Anvisningar och regler, Version 8.0, Reviderad 1. Juli 2010, brå brottsförebyggande rådet.

crimes. Table 16 presents all categories that are related to traffic violations and the number of offences between January 1991 and June 2009. The total number of suspected crimes during this time were 4,073,985 of which 16.9 percent were traffic crimes. Again, all of these traffic crime categories are severe violations. Additional support of such high conviction rates in Sweden is provided by other Swedish studies that have shown similar conviction rates, see Hjalmarsson and Lindquist (forthcoming), Hjalmarsson and Lindquist (2010), and Grönqvist (2011).

4.2 Parental Background, Education and Crime

Table 2 shows the Linear Probability Model estimation results of whether an individual has ever been convicted and ever been convicted to a prison sentence on years of own schooling (Panel A), as well as on years of father's and mother's schooling for the children generation, (Panel B). One year of own schooling for men in the parent generation is associated with a decrease of the probability of a conviction by 1.9 percentage points and a decrease in the probability of a prison sentence by 0.8 percentage points; these correspond to a 7.5% reduction in convictions and 15% reduction in prison sentences respectively.[14]

Panel B also shows a very strong association between both mother's and father's education and son's criminal behavior, even controlling for the child's own education.[15]

Finally Table 3 illustrates the intergenerational links of crime. The probability of ever being convicted increases by over 15 percentage points if a son has a convicted father. This corresponds to a 61 percent increase of the total share of convicted sons. Children whose father have ever been

[14] When computing the standard errors we cluster by birth municipality.

[15] We present the relationship between crime and the levels of education in the Appendix (see Tables 17 and 18), revealing a steep decline in crime participation associated with higher levels of own and parental education. A similar decline is also recorded for incarceration rates.

Table 2: Linear probability model estimates of the association between own or parental education and criminal convictions

Panel A: Men born 45-55		
Dependent variables:	Probability conviction $\bar{p} = 0.2531$	Probability prison $\bar{p} = 0.0538$
Years of schooling, own	-1.916***	-0.806***
	(0.093)	(0.057)
Corresponding percentage change	-7.570	-14.981
Birth cohort/municipality dummies	y	y
Observations	662,875	662,875
Panel B: Sons of men and women born 45-55		
Dependent variables:	Probability conviction $\bar{p} = 0.2645$	Probability prison $\bar{p} = 0.0343$
Years of schooling, own	-2.635***	-0.585***
	(0.029)	(0.013)
Corresponding percentage change	-9.962	-17.055
Years of schooling, father	-0.568***	-0.118***
	(0.035)	(0.014)
Corresponding percentage change	-2.147	-3.440
Years of schooling, mother	-0.598***	-0.144***
	(0.026)	(0.009)
Corresponding percentage change	-2.261	-4.198
Birth cohort/municipality dummies	y	y
Observations	675,625	675,625

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Results scaled by 100. Robust standard errors in parentheses, clustered by birth municipality. All regressions include a full set of birth cohort dummies and birth municipality dummies of the individual. Sample of sons for whom at least one parent was born 45-55.

convicted to a prison sentence are 8.5 percentage points more likely to end up in prison, which translates to a 292 percent increase in the share of sons convicted to prison.

Table 3: Linear probability model estimates of the association between the son's probability of ever being convicted or imprisoned and the father having ever been convicted or imprisoned

Panel A: Sons of men born 45-55		
	(1)	(2)
Dependent variables:	Probability conviction	Probability prison
	$\bar{p} = 0.245$	$\bar{p} = 0.029$
Father convicted/imprisoned	15.039***	8.464***
	(0.252)	(0.204)
Percentage change	61.384	291.862
Birth cohort/municipality dummies	y	y
Observations	559,085	559,085
Panel B: Sons of men born 45-55 with low educated father		
Dependent variables:	Probability conviction	Probability prison
	$\bar{p} = 0.247$	$\bar{p} = 0.028$
Father convicted/imprisoned	14.361***	8.409***
	(0.255)	(0.275)
Percentage change	58.142	300.321
Birth cohort/municipality dummies	y	y
Observations	241,716	241,716

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Effects scaled by 100. Robust standard errors in parentheses, clustered by municipality of sons. All regressions include a full set of birth cohort and municipality dummies of son.

5 Empirical Strategy

The main outcome variables we use are whether an individual was ever convicted during the observation window 1981-2008 and whether someone has ever received a prison sentence. Finally, we also consider whether someone has been convicted more than once as opposed to once or not at all (recidivism) and the number of convictions (including zero).

All the analysis is done for males only and we distinguish them by the education of the *grandparent generation*.[16] We present two sets of estimates. The first relate to the impact of the reform on the parent generation, i.e.

[16]The female crime rate is very small and has not been affected by the reform.

the generation affected by the educational reform directly. The second relate to the impact of the reform on the children of the parent generation.

The youngest person in the parent generation sample is 26 when the crime records made available to us start. Hence, the effect we estimate is not attributable to simply keeping the kids off the streets by getting them to attend school. On the other hand we are missing part of the crime career of individuals, because a lot of the crime happens at a younger age; this is not a cause for bias since we observe the same data for the comparison groups as well. For the child generation we observe the criminal history from the start. Any impacts we we estimate for the child generation are not due to different schooling systems since they all attend the same reformed system.

Since the reform was not randomized we control for potential differences across treatment and control municipalities using a difference in differences approach. This compares the change in the crime across cohorts in municipalities that implemented the reform for the younger cohort but not the older one to the change in crime rate across the same cohorts living in municipalities where there was no change in policy for these same cohorts. In practice we do this for all cohorts in our window and all municipalities. Thus our approach is best described by the regression

$$y^*_{i,m,t} = \alpha + \beta_1 R_{i,m,t} + \gamma_1' t_i + \gamma_2' M_i + \epsilon_{i,m,t},$$

where $y^*_{i,m,t}$ is the latent crime "intensity" outcome observed for person i born in municipality m and in birth cohort t. A conviction corresponds to $y^*_{i,m,t} > 0$. $R_{i,m,t}$ is the reform indicator, which equals one if individual i belongs to a municipality and cohort that has been assigned to the new school system; t_i is a vector of indicator variables indicating to which cohort individual i belongs to and M_i is a vector of indicator variables in-

dicating in which municipality individual i was born. $\epsilon_{i,m,t}$ is conditionally independent of $R_{i,m,t}$.

Based on the latent equation above we first use the linear probability model, which we estimate by GLS. The main reason for this specification is computational convenience: there are about 1,000 municipality and 11 cohort fixed effects.

As an alternative, we also estimate a Logit model and we solve the computational problem by using minimum distance: first we group the data by municipality and cohort and estimate the within-cell conviction probability (P_{mt}). We then use minimum distance to impose the restriction that this probability is generated from a logistic distribution with a linear index as in the latent equation above by fitting the log-odds ratio as follows

$$\log(\frac{P_{mt}}{1-P_{mt}}) = \delta_0 + \delta_1 R_{m,t} + \delta_2' t_i + \delta_4' M_i.$$

In practice we need to drop all cells where the log odds ratio is not defined.[17] Implicitly the Logit and the LP models deal with such cells and the nonlinear form of the probabilities in a different way and hence we needed to check if the results differ: they do not.

The key identifying assumption that delivers the difference in differences approach is that in the absence of the reform, crime propensity can be written as $y_{it}^* = F(t, \varepsilon_i)$ where the distribution of the unobservable ε_i is independent of cohort t but can vary across municipalities and where $F(.,.)$ is strictly monotonic in this unobservable. In terms of an economic model, If we think of this as human capital then this means that individuals with higher human capital always commit less crime. The linear specification above imposes the monotonicity assumption. The discrete nature of

[17] This amounts to about 6 percent of cells.

the dependent variable also requires a distributional assumption on ε_i for identification.[18]

6 Results

6.1 The Reform and Educational Attainment

Panel A in Table 4 shows the estimates of the effects of the education reform on years of schooling for the parent generation. The results are presented for all men born between 1945 and 1955 as well as separately for those with a low educated father and those with a father who has obtained more than the lowest pre-reform education level, respectively.

The reform significantly increased years of schooling for men of the affected generation. The overall effect is larger for those individuals with low educated fathers, as reported in Meghir and Palme (2005). However in this broader and larger sample we find a significant effect (at the 10% level) on those with higher educated grandparents.

Panel B in Table 4 shows the effects of father's or mother's reform assignment on years of schooling of their sons, separated by education levels of their grandfather. Across the generations the effects of the reform on years of schooling seems to diminish. None of the estimates for the child generation are significantly different from zero.

6.2 The Reform and Crime in the Parent Generation

Table 5 and 6 show the estimates of the effect of being assigned to the reform on three different outcomes: the probability of ever being convicted, recidivism[19] and total number of convictions. Table 5 shows the results for the entire sample, while Table 6 shows the corresponding results separately for the sub-sample of those with a low educated grandparent generation:

[18] see Athey and Imbens (2006) and Altonji and Blank (1999)

[19] being convicted at least twice versus once or not at all

Table 4: Reform effects on years of schooling for the generation directly affected by the reform and their sons

	(1)	(2)	(3)
Sample:	All	Low educ	High educ
Panel A: Men born 45-55			
Dependent variable: Own years of schooling			
Reform	0.216***	0.324***	0.061*
	(0.044)	(0.029)	(0.036)
Observations	602,084	261,873	138,829
Panel B: Sons of parent generation			
Dependent variable: Son's years of schooling			
Reform father	-0.021	0.024	-0.015
	(0.032)	(0.036)	(0.057)
Observations	325,766	143,729	64,948
Reform mother	0.002	0.025	-0.009
	(0.027)	(0.030)	(0.050)
Observations	421,541	185,831	82,764

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Effects are scaled by 100, robust standard errors in parentheses. Panel A: standard errors clustered by municipality of birth; all regressions include a full set of birth municipality dummies and birth cohort dummies of individual. Panel B: standard errors clustered by father's or mother's birth municipality; all regressions include a full set of birth municipality and birth cohort dummies of father or mother.

for this group the impact of the reform on the educational attainment of the parent generation is strongest.

In addition, we split up the results on the basis of different cohort groups because the overall results may be diluted by the fact that the oldest birth cohorts are observed from an age where crime rates are relatively low. For example, the oldest cohort included, those born in 1945, are aged 36 when we start to record their criminal behavior.

We use a linear probability model for the outcomes of ever being con-

victed as well as recidivism and a negative binomial model for the number of convictions. All specifications include fixed effects for birth municipality as well as birth cohorts and the standard errors are corrected for clustering within municipality of birth, allowing for both spatial and serial correlation.

Column 1 in Table 5 shows the results for the entire sample. The point estimate is significant but not very precise. However, if we restrict the sample to cohorts where data allow us to observe most of the criminal careers, we obtain significant and large effects on all outcomes. On the probability of being convicted, the estimate for the youngest cohort, born in 1954 or 1955, is highly significant suggesting a 1.3 percentage points decrease in crime; this corresponds to a 5 percent decrease in the probability of ever being convicted as a result of being assigned to the post reform school system. Comparing the results in Tables 5 and 6 suggests that the effect is somewhat stronger in the group originating from homes with low educated fathers whose educational outcomes were more strongly affected by the reform.

To put these effects into perspective to years of schooling we compute an indirect least squares estimate, reported in Tables 5 and 6. This instrumental variable approach relies on the assumption that the reform only affected our outcomes through its impact on parental education. The ILS estimate[20] suggests that one year of schooling decreases the probability of ever being convicted for men born 1952-1955 by 4.8 percentage points and by 3.5 percentage points for those with low educated fathers.

Hjalmarsson, Holmlund, and Lindquist (2011) confirm our estimates on an extended data-set including convictions going back to 1973. They confirm

[20]This is computed as the ratio of the reduced form estimate of the reform effects on the probability of a conviction (Tables 5 and 6) over the first stage estimate of the reform effects on years of schooling (Panel A in Table 4). The first stage results show an 0.216 and 0.324 increase in years of schooling for men and men with low educated fathers, respectively.

Table 5: Estimates of the effects of the education reform on the probability of ever being convicted, being convicted at least twice and the total number of crimes individuals have been convicted for, by birth cohort groups, all education levels of father.

	(1)	(2)	(3)	(4)	(5)	(6)
Sample of all men born:	**45-55**	**50-55**	**51-55**	**52-55**	**53-55**	**54-55**
Dependent variable: indicator for having been convicted at least once						
Probability conviction	0.253	0.268	0.271	0.275	0.278	0.284
Reform	-0.645	-0.456	-0.532*	-1.028***	-1.076**	-1.329***
	(0.405)	(0.305)	(0.318)	(0.396)	(0.490)	(0.479)
Percentage change	-2.548	-1.700	-1.960	-3.744	-3.866	-4.685
Years of schooling (ILS)	-2.986	-2.111	-2.463	-4.759	-4.981	-6.153
Dependent variable: indicator for having been convicted at least twice						
Probability recidivism	0.133	0.146	0.149	0.151	0.153	0.157
Reform	-0.671*	-0.279	-0.124	-0.530*	-0.552	-0.749*
	(0.392)	(0.227)	(0.243)	(0.283)	(0.346)	(0.452)
Percentage change	-5.045	-1.911	-0.832	-3.510	-3.608	-4.771
Dependent variable: number of crimes convicted for						
Average number of crimes	1.309	1.578	1.646	1.696	1.748	1.828
Reform	0.027	-0.071	-0.038	-0.122	-0.078	-0.121
	(0.035)	(0.062)	(0.067)	(0.086)	(0.083)	(0.092)
Observations	622,583	319,093	263,592	210,399	157,155	103,761

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Marginal effects are scaled by 100. Robust standard errors in parantheses, clustered by municipality of birth. All regressions include a full set of birth municipality and birth cohort dummies. The first two sets of reported estimates in both panels are from a linear probability estimation with weighted least squares for the dependent variables probability of being convicted at least once and probability of being convicted at least twice. The used weights are: $\sqrt{x'b(1-x'b)}$, which are obtained from a first stage OLS estimation. The last set of estimates in both panels for the dependent variable number of total crimes convicted for are the marginal effects of the negative binomial estimation. The calculations for the implicit IV are based on the estimations of the effects of the reform on years of schooling for the parent generation in Table 4.

our general finding that the reform has an impact on own criminal behavior and that the results get stronger when the period in the life cycle with the highest rate of criminality are included in the sample. All our results are consistent with findings by Lochner and Moretti (2004) and Machin, Marie, and Vujić (2011) for the US and the UK respectively.

The reform also had an impact on other crime outcome variables, recidivism and number of convicted crimes, and the probability of having ever been convicted to a prison sentence. The latter results are only presented in the

Table 6: Estimates of the effects of the education reform on the probability of ever being convicted, being convicted at least twice and the total number of crimes individuals have been convicted for, by birth cohort groups, low education level of fathers.

	(1)	(2)	(3)	(4)	(5)	(6)
Sample of men with low educated fathers						
born in cohorts:	**45-55**	**50-55**	**51-55**	**52-55**	**53-55**	**54-55**
Dependent variable: indicator for having been convicted at least once						
Probability conviction	0.240	0.252	0.255	0.259	0.263	0.269
Reform	-0.263	-0.494	-0.579	-1.119*	-1.103	-2.094**
	(0.304)	(0.434)	(0.486)	(0.661)	(0.912)	(0.970)
Percentage change	-1.098	-1.963	-2.269	-4.324	-4.192	-7.776
Years of Schooling (ILS)	-0.812	-1.525	-1.787	-3.454	-3.404	-6.463
Dependent variable: indicator for having been convicted at least twice						
Probability recidivism	0.123	0.133	0.136	0.139	0.142	0.146
Reform	-0.214	-0.199	-0.151	-0.503	-0.471	-1.140
	(0.233)	(0.327)	(0.366)	(0.459)	(0.646)	(0.714)
Percentage change	-1.740	-1.496	-1.110	-3.619	-3.317	-7.808
Dependent variable: number of crimes convicted for						
Average number of crimes	1.253	1.473	1.539	1.587	1.647	1.717
Reform	-0.030	-0.137*	-0.117	-0.250**	-0.236*	-0.177
	(0.047)	(0.081)	(0.096)	(0.124)	(0.128)	(0.168)
Observations	264,679	150,620	125,952	101,266	76,207	50,222

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Marginal effects are scaled by 100. Robust standard errors in parantheses, clustered by municipality of birth. All regressions include a full set of birth municipality and birth cohort dummies. The first two sets of reported estimates in both panels are from a linear probability estimation with weighted least squares for the dependent variables probability of being convicted at least once and probability of being convicted at least twice. The used weights are: $\sqrt{x'b(1-x'b)}$, which are obtained from a first stage OLS estimation. The last set of estimates in both panels for the dependent variable number of total crimes convicted for are the marginal effects of the negative binomial estimation. The calculations for the implicit IV are based on the estimations of the effects of the reform on years of schooling for the parent generation in Table 4.

Appendix in Table 20. The results indicate that the reform decreased the probability of recidivism and prison sentence for some of the cohorts and more strongly so for those originating from a low education background. We also find an effect on the number of convicted crimes on a 10 percent significance level for some cohorts and again stronger effects for the low SES group.

As a robustness check for our estimates we reestimate the model using

the Logit specification. The results from this exercise, reported in the Appendix, show results very similar to those displayed in Tables 5 and 6, although the precision is somewhat inferior.

6.3 The Reform and Crime in the Child Generation

Table 7 reports the results of the difference-in-differences estimation of the effects of the school reform on the three outcomes - probability of being convicted, recidivism and number of convictions - for the child generation.[21] For the first two outcomes we estimate linear probability models and for the third negative binomial models. Again, for the probability of being convicted, we additionally estimate a Logit model based on cohort-municipality cells reported in the Appendix as a sensitivity analysis. We estimate two specifications. In the first one, we estimate the effects of a father who attended the new school system on son's criminal behavior and in the second one the corresponding effects of a mother attending the post reform school system. In addition, we present separate results for those with low educated grandfathers.

The reform significantly reduces the probability of having ever been convicted for the sons of those (fathers) who were assigned to the reform by 0.6 percentage points. Since the average share of convicted individuals in this cohort was about 26.5 percent the reduction in criminality was approximately 2.5 percent.[22] The effect is stronger in the group with a low educated paternal grandfather: the reduction in the probability of a conviction is 1.02 percentage points, which translates to a 4.13 percent decrease in the share of convicted sons of low educated grandfathers. The results for the additional outcome variable that measures recidivism, the probability of having been convicted at least twice are presented in column (3) for all sons, and for those with low educated grandparents in column

[21] The results for prison convictions can be found in the Appendix.

[22] The marginal effects of the Logit Model estimates are very similar, see Appendix.

Table 7: Estimates of father's and mother's reform assignment on the probability of their sons having ever been convicted, having ever been convicted more than twice and the number of crimes convicted for.

Dependent variables:	Son convicted at least once		Son convicted at least twice		Number of crimes son convicted for	
Sample:	All	Low educ	All	Low educ	All	Low educ
Panel A: Father's reform assignment						
Average dependent var	0.265	0.247	0.131	0.131	1.245	1.210
Reform father	-0.650***	-1.02***	-0.321	-0.637**	-0.065**	-0.040
	(0.219)	(0.361)	(0.233)	(0.273)	(0.031)	(0.050)
Percentage change	-2.456	-4.129	-2.450	-4.863		
Observations	563,754	243,082	563,754	243,082	563,754	243,082
Panel B: Mother's reform assignment						
Average dependent var	0.265	0.278	0.153	0.150	1.538	1.442
Reform mother	-0.159	-0.041	0.041	0.117	0.010	0.069
	(0.249)	(0.331)	(0.214)	(0.281)	(0.046)	(0.055)
Percentage change	-0.600	-0.147	0.268	0.780		
Observations	595,138	255,075	595,138	255,075	595,138	255,075

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Marginal effects are scaled by 100. Robust Standard errors in parentheses, clustered by municipality of birth of the father (Panel A) or mother (Panel B). All regressions include a full set of birth municipality dummies and birth cohort dummies of father or mother. Columns (1)-(4) present estimates from a linear probability model with weighted least squares, the used weights are: $\sqrt{x'b(1-x'b)}$ obtained from first stage OLS estimations. Columns (5)-(6) report the marginal effects of a negative binomial model.

(4). In the group with low educated grandfathers, having a father who was assigned to the new school system significantly reduces the probability of becoming a repeat offender. We find a strong effect of father's reform assignment on the total number of crimes for the overall sample of sons, shown in column (5).[23]

There are no significant effects of reform assignment of mothers on the probability of a conviction of their sons neither for the entire sample of all sons, nor for those with a low educated maternal grandfather. This also

[23]We gain more precision of the results presented Table 7 when we exclude those sons who only appear one, two or three years in the crime records, see Table 23 in the Appendix. More specifically, we repeated the linear probability estimation for sons excluding those who are 15 years, 15 or 16 years, and 15-17 years in the last year for which we have crime records.

holds for the recidivism outcome variable presented in columns (3) and (4) and the number of convictions shown in columns (5) and (6).[24]

6.4 The Common Trends Assumption

An identifying assumption underlying the differences-in-differences estimator is that any trend in the outcome variable is common in the treatment and comparison groups over the period of comparison. This assumption is untestable because it relates to the counterfactual change in the treatment group. However, an indication can be obtained by testing whether the trends are common in the two sets of groups before the reform and indeed after the reform as well.

In our sample we have 12 groups of municipalities indexed by which cohort was first assigned to the reform. We used only the municipalities that first implemented the reform for the 1947 cohort onwards (i.e. 10 of the 12 set of municipalities) and compared the trend of criminal behavior of individuals across these municipalities for all cohorts that were not affected by the reform. The pooled regression of these groups is $y = \alpha + \beta t + \gamma' m + \delta' t * m + \nu$, where m is a set of dummies indicating the group to which the municipality belongs based on the cohort for which it first implemented the reform; t is a linear trend that represents the cohorts 1945-1955. A joint test of $\delta_1 = \delta_2 = ... = \delta_{10} = 0$ gives a F statistic of $F(9;7{,}090)$=1.15 with P=0.323, with 7,090 cohort-municipality cells before treatment. This implies the hypothesis of common trends in crime for the pre-treatment cohorts for all groups of municipalities cannot be rejected.

For post-treatment trends in crime we only compare crime between the municipalities that implemented the reform for cohorts born before 1954.

[24] We repeated the analysis for the children generation using the suspected crime data and find a negative but not significant relationship between father's reform assignment and sons probability of having ever been suspected for a crime. Results are provided by the authors upon request. Descriptive tables on suspected crime rates are provided in the Appendix in Table 14.

We compare the criminal behavior of individuals across these municipalities for the cohorts affected by the reform. A joint test of equality of the coefficients on the interaction term of the above pooled model yields F(9, 4,808) with P=0.1303, where 4,808 is the number of cohort-municipality cells that are treated. This means that the hypothesis of a common trend in criminal behavior for the treated cohorts is the same across the groups of municipalities that implemented the reform for different cohorts cannot be rejected. Both these tests are strong evidence in favor of the key identifying assumption for our difference-in-differences approach to the problem.

6.5 Mechanisms

The key result of our paper is that the reform reduced the criminal behavior of fathers and sons by large and comparable amounts. The persistence of the effects of this policy puts a different perspective on the value of such reforms because the benefits are multiplied by improving intergenerational outcomes. We now make an attempt to provide evidence on the mechanisms that could have led to such improvements.

6.5.1 Resources and Assortative Mating

Meghir and Palme (2005) document that the reform increased the overall amount of income for those for whom the grandfather generation is low educated. Moreover,as shown in Table 8 the reform caused men to marry women with higher earnings by about US $800 a year.[25] The spouse is also less likely to be unemployed. These results point to an increased level of resources due to the reform, at least for those from a lower SES background. This could lead to increased investment in children. However this cannot be the complete story because we obtain no effect on crime when the person assigned to the reform was the mother, although this implies an equivalent increase in resources.

[25] see Meghir, Palme, and Simeonova (2011)

Table 8: Reform effects on assortative mating of men in cohorts directly affected by the reform

Dependent variables:	(1) Spouse education	(2) Spouse annual labor earnings in SEK	(3) Spouse unemployed
Reform	0.0499	5,462**	-0.003***
	(0.061)	(2,672)	(0.001)
Observations	681,764	657,591	675,591

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Dependent variables measured in 2004. Robust standard errors in parantheses, clustered by municipality of birth. All regressions include a full set of birth municipality and birth cohort dummies.

6.5.2 Fertility

The reform could affect fertility bahavior by reducing the total number of children, increasing the age of first birth or changing the spacing of births, all of which could affect the time and monetary resources invested in kids. It could also decrease unwanted pregnancies and births.[26] In Table 9 we consider some of these possibilities. Although all results are in the direction that would imply an improvement in the quality of children we cannot be conclusive because the estimates are not significant. The only highly significant result is the number of children associated with teenage fathers.[27] However, since only 1.7 percent of men have children as teenagers and, more importantly, the reform only decreased this probability by between 0.2 and 0.3 percent, this is not enough to explain a large part of our results.

6.5.3 Mobility

A further potential channel for the improvement of child outcomes may come from improved neighbourhoods and peers. Indeed such a possibil-

[26]Previous studies provide evidence that unwanted or unplanned children might be more likely to become offenders, see Donohue and Levitt (2001) and Hunt (2006).

[27]The results for women are qualitatively the same, the only difference is that women who were assigned to the reform are less likely to ever have a child on a 10 percent significance level.

Table 9: Estimation of the effects of the reform on the probability of ever having a child, the number of children, the age at birth of first child and the probability of teenage paternity.

Dependent variables:	ever child	number children	age birth first child	teenage
Specification	LP	Poisson	Neg binomial	LP
Sample: Men born 45-55				
Average dep var	0.813	1.896	27.054	0.017
Reform	-0.093	-0.004	0.106	-0.263**
	(0.185)	(0.007)	(0.075)	(0.106)
Observations	622,583	622,583	505,679	622,583
Sample: Men born 45-55 with low educated fathers				
Average dep var	0.822	1.912	26.524	0.019
Reform	-0.096	0.001	0.064	-0.210**
	(0.273)	(0.010)	(0.048)	(0.100)
Observations	264,679	264,679	217,517	264,679

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Marginal effects are scaled by 100. Robust standard errors in parantheses, clustered by birth municipality. All estimations include a full set of municipality dummies and cohort dummies.

ity was an important motivation for "Move to Opportunity" (see Kling, Liebman, and Katz (2007)). To investigate whether the parent generation moved to better neighbourhoods following the reform we classify all municipalities according to their average income in 1960, i.e. before anyone affected by the reform entered the labor market[28]

Table 10 shows results for: indicator variable if individual lives in a different municipality in 1991 compared to their birth municipality in column (1), indicator variable if individual moved from a lower than median income birth municipality at 1960 levels to a higher than median income municipality at 1960 levels in 1991 in column (2), the reversed direction from high to low income municipalities in columns (3), and if individuals moved from or remained in a municipality with the same 1960 income classification in columns (4) and (5). The results of these estimations show

[28]Details on the classification of municipalities is provided in the Appendix.

no significant impact of the reform on moves from or to low income municipalities and no impact of moves at all. Although the peer group may have improved through better education this was not further reinforced by moving to different/better neighborhoods.

Table 10: Reform effects on mobility of individuals in cohorts directly affected by the reform, by income levels of municipalities before the reform

	(1)	(2)	(3)	(4)	(5)
Dependent variables:	Moving	Low to High	High to Low	Low to Low	High to High
Panel A: Men born 45-55					
Reform	-0.562	-0.098	-0.024	0.098	0.024
	(0.666)	(0.288)	(0.226)	(0.288)	(0.226)
Observations	591,425	591,425	591,425	591,425	591,425
Panel B: Women born 45-55					
Reform	-0.809	-0.055	-0.006	0.055	0.006
	(0.756)	(0.331)	(0.320)	(0.331)	(0.320)
Observations	611,142	611,142	611,142	611,142	611,142

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Effects are scaled by 100, robust standard errors, clustered by municipality of birth in parantheses. All regressions include a full set of birth municipality dummies and birth cohort dummies of individual. The dependent variables indicated on top of each column are defined as indicator variables indicating if the individual has moved away from their birth municipality by 1991 (column (1)), moved away from a lower or higher than median income municipality to a lower or higher than median income municipality (column (2)-column (5)). Income levels are measured in 1960, before the reform was implemented. Codes of birth municipalities are transformed into those that they correspond to from 1976 onwards. Low to low/high to high include both cases where individuals move to another municipality by 1991 that also was of lower/higher than median income in 1960, and those who remain in the same municipality.

6.5.4 Father as a role model

Section 5 showed a very strong association between father's criminal behavior and that of the son. As shown in Table 3, among convicted fathers the probability of the child being convicted is higher by 15 percentage points, or more than 60 percent. For prison sentences the association is even stronger: if the father has been convicted to a prison sentence, the probability that the son is also convicted to prison is higher by more than 290 percent. We cannot establish the extent to which this relationship is causal. However we note that the reform did decrease fathers' crime

and improved their educational outcomes. So a possible channel is that fathers who went through the reform are better role models with improved education and lower crime rates. It has been shown that better education for the mothers improves child outcomes in a number of ways.[29] These results suggest that improving paternal quality affects criminal behavior of sons.

7 Conclusions

Educational reforms have been studied extensively for their impact on educational attainment and labor market outcomes. However, they can also have other important effects such as improvements in health and reduction in crime, which have been documented in the literature. Here we investigate the intergenerational effects that education can have on crime, by exploiting an important educational reform and the rich administrative data available in Sweden, linking three generations of individuals. Establishing these longer term persistent effects is crucial for our understanding of the real benefits of such interventions. In an earlier paper Meghir and Palme (2005) demonstrated that the educational reform to the Swedish educational system, which we use here, had substantial effects on educational attainment and earnings, particularly for those with low educated parents.

Using administrative data that compares individuals of the same cohorts, but educated under different systems, we find strong negative and significant effects of the reform on crime. Thus, for the youngest cohorts, born between 1954 and 1955, the point estimate suggests a 1.3 percentage points, corresponding to 5 percent decrease in the probability of being convicted from being assigned to the post reform school system. In the group from homes with low educated fathers the effect seems to be somewhat larger, which is consistent with a larger effect on educational attainment.

[29] See for example Carneiro, Meghir, and Parey (forthcoming)

The striking result of this paper, however, is the effect of the reform on the sons of those originally affected: there is a significant effect of paternal assignment to the reform on the probability of being convicted corresponding to an average reduction in crime of about 2.5 percent.

The intergenerational effect of education on crime can operate through several different channels. We do not find clear evidence of a specific channel, although there is evidence of an increase in resources and of improved role models since the fathers crime rates were reduced. The fact that the intergenerational effects of crime are driven exclusively by the father attending the new school system and not by the mother points towards improved parenting and role models as a key mechanism. The persistent intergenerational impact of the reform shows the potential of education policy to induce broader social change.

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8 APPENDIX

8.1 Theory

To better understand the mechanisms through which educational reform can affect participation in crime for both generations consider the following simple model. Human capital is produced by investments in various stages of the child's life as well as by overall educational attainment E_c. Suppose there are two stages, early investments I_0 and investments during schooling I_1. The efficiency of investments depends on the educational level of the parent, E_p. Denote the human capital production function by

$$H = H(I_0, I_1, E_c|E_p)$$

where $H'_{I_0} > 0$, $H'_{I_1} > 0$, $H'_{E_c} > 0$ and $H''_{I_0} < 0$, $H''_{I_1} < 0$, $H''_{E_c} < 0$. Parents are assumed to care about child quality, which here is just their human capital. Ignoring dynamics for simplicity, they solve the problem[30]

$$\max_{C,I_0,I_1} \{u(C, H) \text{ st } C + I_0 + I_1 = Y^p \text{ and } H = H(I_0, I_1, E_c|E_p)\}$$

where C is parental consumption. In this simple context investments in children will increase as parental resources Y^P increase, so long as H is a normal good. The first order conditions for investments are

$$u'_H \frac{\partial H}{\partial I_0} = u'_C$$

$$u'_H \frac{\partial H}{\partial I_1} = u'_C$$

An increase in the marginal productivity of such investments (say due to an increase in parental education E_p) will lead to more investments in the

[30]The problem is dynamic sequential, but nothing would be gained in introducing this notation here.

children at both stages. This will happen both because the productivity of investments may increase and because parental resources Y^P go up. The next step is to see how these changes can affect participation in crime.

Consider a very simple model of crime choice in two stages of life. First, is the educational stage, where the individual can either engage in education, crime or work. Then follows the *adult* stage where there is no education choice. We start by the latter.

Committing a crime is a period by period decision with no dynamics (for simplicity). Working leads to income $Y(H)$ which depends on human capital H. Crime on the other hand yields a return R with some probability $p(H)$. Being caught, with probability $1-p(H)$ leads to punishment $K(H)$. As discussed in Lochner and Moretti (2004), the dependence of K on H represents the opportunity cost of being incarcerated. Moreover, it also costs $c(H)$ to participate. This cost can reflect the aversion that one may have to anti-social behavior. We assume that $c(H)$ is increasing in H.

Participation in crime is determined by the condition

$$p(H)R - (1-p(H))K(H) - c(H) - Y(H) > 0 \Longleftrightarrow \text{ engage in crime}$$

An increase in H will increase earnings $Y(H)$ and participation costs $c(H)$, both implying a reduction in crime. A possible mitigating effect is that better human capital may make crime more effective and reduce the probability of capture $p(H)$. In our empirical analysis we only measure convictions; we assume that a reduction in convictions reflects a reduction in crime participation and not more effective criminals. Thus, other than the potential effect on $p(H)$, increasing human capital will decrease participation in crime.

It is also useful to consider the earlier period, when the individual still

has the option of being in school. Define the future value as $V(H_1) = E\max_{\kappa,w}(V^{\kappa}(H_1), V^{w}(H_1))$ with V^{κ} denoting the value of crime and V^{w} the value for work. $c^{ed}(H_0)$ denotes the cost of education, which we assume are declining in H_0 (initial human capital). In this first period the value of education, crime and work respectively are given by

$$V^{ed} = -c^{ed}(H_0) + \beta V(H_1^+)$$

$$V^{w} = Y(H_0) + \beta V(H_0)$$

$$V^{k} = p(H_0)R - (1 - p(H_0))K(H_0) - c(H_0) + \beta V(H_0)$$

where H_1^+ denotes that education allows the individual to enter the next period with higher human capital. The individual choose the activity with the greatest value. First, note that if schooling is compulsory, then there is a mechanical reduction in crime, simply because the opportunity to commit an offence is no longer there (or reduced in practice). Second, an increase in human capital will increase the value of both schooling and work; the former because it will reduce the costs of schooling $c^{ed}(H_0)$ as well as the future value $V(H_1^+)$, the latter because it will increase the current wage as well as the future value $V(H_0)$. So first period crime will decline; whether education will go up is in this context ambiguous.

8.2 Reform Appendix

Table 11: Quantitative development of the comprehensive school experiment 1949 to 1962.

Year	Municipalities		Number of	Number of
	Cumulative Number	Percentage share	classes	students
1949/50	14	1.3	172	2 483
1950/51	20	1.9	379	7 529
1951/52	25	2.4	682	14 635
1952/53	30	2.9	1 009	22 725
1953/54	37	3.5	1 525	35 784
1954/55	46	4.4	2 516	61 498
1955/56	59	5.6	3 394	84 941
1956/57	71	6.7	4 393	109 694
1957/58	96	9.1	5 702	143 370
1958/59	142	13.5	8 036	196 343
1959/60	217	20.6	11 191	266 042
1960/61	295	28.0	14 283	333 094
1961/62	415	39.4	18 665	436 595

Note: The 1952 division of municipalities (total: 1 052). Source: Marklund

8.3 Data appendix

Table 12: The Sample

	Number observations		
	All	Male	Female
Parent generation:			
Cohorts 1945-1955	1,340,857	685,056	655,801
Father's education available	881,742	452,433	429,309
Of which low educated father	560,273	287,396	272,877
Percent	63.54	63.52	63.56
Children generation:			
All children of parent generation	1,621,758	833,564	788,194
Paternal grandfather's education available	802,451	412,619	389,832
Of which with low educated grandfather	511,980	263,319	248,661
Percent	63.80	63.82	63.79
Maternal grandfather's education available	836,632	430,357	406,275
Of which with low educated grandfather	538,228	276,779	261,449
Percent	64.33	64.31	64.35

Notes: We only present the number of observations that are available on father's and grandfather's education level, because we will condition on father's or grandfather's education level in the analysis. We only have information on the highest level of education for those individuals that are not older than 60 years in the year of the 1970 census. We report the number of individuals in each sample, the number of individuals for which we have information on the highest level of education on their fathers or grandfathers and the share of those for which we have this information with the lowest education level. For the children generation with low educated grandfathers on their father's side of the family, we consider those children whose father was born between 1945 and 1955. For the children generation with low educated grandfathers on the mother's side of the family we consider those whose mother was born between 1945 and 1955.

Table 13: Number of all convictions in Sweden between 1981-2008

	All	Male	Female
Number of convicted persons	1,249,569	966,790	282,779
Number of persons convicted to prison	366,639	344,919	21,720
Number of convictions in total	3,014,811	2,534,337	480,474
Number of prison sentences in total	1,204,711	1,115,428	89,283
Convictions by age groups			
age 15 -24	1,128,125	950,413	177,712
age 25-34	710,177	605,445	104,732
age 35-44	577,693	483,821	93,872
age 45-54	355,396	296,971	58,425
age 55-64	161,367	133,788	27,579
age 65-80	76,296	59,138	17,158
age $>$ 80	5,757	4,761	996

Notes: 78% to 85% of the convictions are males.

Table 14: Data on all suspected crimes in Sweden between 1991-2009. Sons of men or women born 1945-1955.

	Number of persons suspected for a crime 1991-2009		
	All crimes	Excluding traffic	Excluding some traffic
Sample: Sons of men born 45-55			
	129,683	117,279	124,487
Percent of sample	20.95	18.94	20.11
Sample: Sons of men born 45-55 with low educated father			
	54,542	48,888	52,222
Percent of sample	20.71	18.57	19.83
Sample: Sons of women born 45-55			
	133,953	120,748	129,217
Percent of sample	20.50	18.48	19.78
Sample: Sons of women born 45-55 with low educated father			
	55,210	49,294	129,217
Percent of sample	19.95	17.81	19.78

Notes: The category Excluding traffic excludes all traffic crime categories. All traffic crime categories are listed in Table 16. The category Excluding some traffic excludes the traffic crime categories "Driving without a license", "Allowed driving without license" and "Override provision".

Figure 2: Life cycle convicted crimes

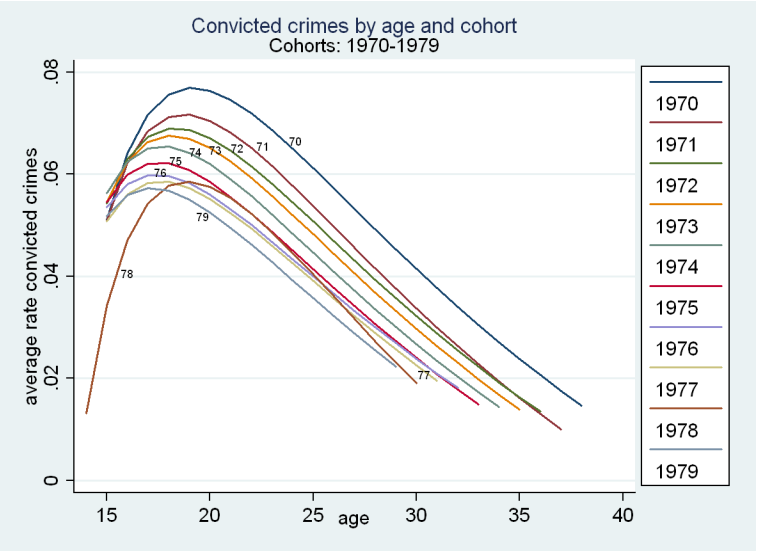

Figure 3: Life cycle convicted crimes

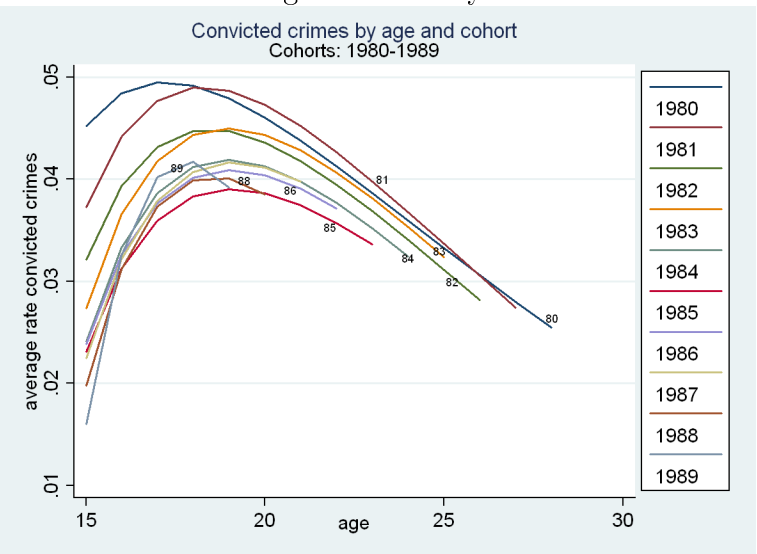

Table 15: Persons found guilty of criminal offences, by principal offence

	Number of convictions, 2009	Share, %
Crimes against penal code	**59,542**	**42.1**
Of which		
Crimes against life and health	*9,744*	*6.9*
Of which		
Murder and man-slaughter	150	0.1
Assault, gross assault	9,268	6.5
Sexual offences	*1,090*	*0.8*
Of which:		
Rape	256	0.2
Theft, robbery, other offences of stealing	*29,393*	*20.8*
Of which:		
Theft, gross theft	9,233	6.5
Petty theft	17,953	12.7
Robbery, gross robbery	1,049	0.7
Vehicle theft	824	0.6
Fraud and other dishonesty	*3,175*	*2.2*
Crimes inflicting damage	*3,316*	*2.3*
Violent threat to public servant	*2,544*	*1.8*
Other	*10,280*	*7.3*
Crimes to other penal legislation	**82,035**	**57.9**
Crimes against the Road traffic offences act	*47,020*	*33.2*
Of which		
Drunken driving, gross drunken driving	13,253	9.4
Crimes against the Narcotics drugs act	*18,525*	*13.1*
Crimes against the Act on smuggling	*2,076*	*1.5*
Other	*14,414*	*10.2*
All crimes	**141,577**	**100**

Notes: Persons found guilty of criminal offences, by principal offence, 2009. Source: Kriminalstatistik, Rättsstatistisk årsbok, Statistisk årsbok, Statistiska Meddelanden (R 11 SM).

Table 16: Traffic crimes persons were suspected for, January 1991 - June 2009.

Total number of crimes	**4,073,985**	
Total number of traffic crimes	**687,522 (16.876%)**	
Description of traffic violation	**Number of cases**	**Percentage of traffic crimes**
Driving or aggravated driving without a license	423,809	61.64
Drink-driving or aggravated drink-driving under the influence of alcohol alone or under the influence of both alcohol and drugs	167,958	24.43
Driving or aggravated driving under the influence of drugs alone	49,828	7.25
Hit and run	19,346	2.81
Allowed driving without a license	9,941	1.45
Gross negligence in traffic	9,018	1.31
Cause of danger for another in connection with traffic or in traffic	3,896	0.57
Causing bodily injury or disease related to traffic accident	1,393	0.2
Involuntary manslaughter in connection with accident	1,201	0.17
Override provision	414	0.06
Other traffic offense, imprisonment in range of penalties	321	0.05
Maritime Act: Drunkenness	314	0.05
Other crimes against maritime law, imprisonment in the range of penalties	83	0.01

Notes: Criminal offences related to traffic violations persons were suspected for. Register data on all crimes individuals have been suspected for between January 1991 and June 2009 in Sweden.

Table 17: Linear probability estimates of the association between own education and criminal behavior. Men born between 1945-1955.

Dependent variables	Probability conviction $\bar{p} = 0.2531$	Probability prison $\bar{p} = 0.0538$
Education Levels		
Vocational	-0.161	0.031
	(1.190)	(0.273)
Upper secondary	-7.471***	-3.928***
	(1.028)	(0.287)
Upper secondary + $\geq$ 1 year	-10.549***	-5.113***
	(0.913)	(0.288)
College/University	-13.782***	-5.929***
	(0.923)	(0.395)
PhD	-19.759***	-7.183***
	(0.713)	(0.545)
Observations	684,625	684,625

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Results are scaled by 100. Robust standard errors in parentheses, clustered by birth municipality. All regressions include a full set of birth cohort dummies and birth municipality dummies.

8.4 Result appendix

8.4.1 Additional Results - First Generation

To avoid the computational difficulties involved in estimating a logit model with 1,000 municipality fixed effects and 11 cohorts we use a minimum distance procedure. We collapse the sample to 10,744 municipality-cohort cells by computing the log-odds ratio within each cell. For 691 municipality-cohort cells the proportion of observed crime was zero and hence the log-odds ratio is not defined. For 108 cells we cannot assign the reform status, which leaves us with 9,949 municipality-cohort observations. We then regress the log-odds ratio on the municipality and cohort dummies as well as on the reform indicator using GLS. Each cell was weighted by $\sqrt{p_c(1-p_c)N_c}$, where N_c is the cell size and p_c is the within cell probability of a conviction. The corresponding marginal effects for different cohorts of the logit model are presented in Table 19. Table 20 shows the linear proba-

Table 18: Linear probability model estimates of the association between parental education and own criminal behavior. Sons of parents born 1945-1955.

Dependent variables	Probability conviction $\bar{p} = 0.2645$	Probability prison $\bar{p} = 0.0343$
Panel A: Education levels father		
Vocational	-2.075***	-0.694***
	(0.214)	(0.088)
Upper secondary	-8.083***	-2.342***
	(0.339)	(0.142)
Upper secondary + $\geq$ 1 year	-9.719***	-2.457***
	(0.388)	(0.135)
College/University	-12.535***	-2.900***
	(0.463)	(0.214)
PhD	-13.829***	-3.029***
	(0.551)	(0.303)
Observations	754,121	754,121
Panel B: Education levels mother		
Vocational	-4.356***	-1.556***
	(0.291)	(0.102)
Upper secondary	-8.119***	-2.648***
	(0.473)	(0.181)
Upper secondary + $\geq$ 1 year	-10.015***	-2.854***
	(0.381)	(0.173)
College/University	-12.324***	-3.242***
	(0.508)	(0.230)
PhD	-14.553***	-3.443***
	(1.059)	(0.390)
Observations	754,121	754,121

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered by birth municipality. Each education level is indicated as a indicator variable. The omitted education level is the lowest education level combined levels 1 and 2. All regressions include a full set of birth cohort dummies and birth municipality dummies.

bility model estimates for the dependent variable prison sentence. For this dependent variable it is not possible to repeat the procedure for the logit estimation, since the proportion of prison sentences is too small, which prevents us from computing the log-odds ratios.

Table 19: Logit estimates of the effects of the education reform on the probability of ever being convicted; by birth cohort groups, separated by education level of fathers.

	(1)	(2)	(3)	(4)	(5)	(6)
Cohorts	**45-55**	**50-55**	**51-55**	**52-55**	**53-55**	**54-55**
Dependent variable: having been convicted at least once						
Panel A: Sample of all men						
Probability conviction	0.253	0.268	0.271	0.275	0.278	0.284
Reform	-0.023	-0.284	-0.434	-1.274*	-0.999	-1.395
	(0.281)	(0.422)	(0.500)	(0.654)	(0.911)	(1.361)
Percentage change Share convicted	-0.091	-1.057	-1.598	-4.639	-3.591	-4.916
Observations	622,583	319,093	263,592	210,399	157,155	103,761
Panel B: Sample of men with low educated father						
Probability conviction	0.234	0.252	0.255	0.259	0.263	0.269
Reform	-0.022	-0.272	-0.417	-1.227*	-0.965	-1.351
	(0.271)	(0.405)	(0.480)	(0.630)	(0.879)	(1.318)
Percentage change Share convicted	-0.093	-1.081	-1.633	-4.741	-3.666	-5.015
Observations	264,679	150,620	125,952	101,266	76,207	50,222

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. We report maginal effects of a logit estimation, scaled by 100. Robust standard errors in parantheses, clustered by municipality of birth. All regressions include a full set of birth municipality and birth cohort dummies.

8.4.2 Additional Results - Children Generation

When we collapse the data set by cohort-municipality level as a first step to estimating the logit model, we do it by father's cohort-municipality level which gives us 10,607 cells for the father's sample, and 10,247 for the sample with low educated paternal grandfathers. For the specification with mother's reform assignment we collapse the data by mother's cohort-municipality level which leads to 10,647 for the entire sample and 10,324 for the low educated maternal grandfathers sample.

Table 20: Estimates of the effects of the education reform on the probability of ever being convicted to a prison sentence; by birth cohort groups, separated by education level of fathers.

	(1)	(2)	(3)	(4)	(5)	(6)
Cohorts	**45-55**	**50-55**	**51-55**	**52-55**	**53-55**	**54-55**
Dependent variable: indicator for having been convicted to a prison sentence at least once						
Panel A: Sample of all men						
Probability prison conviction	0.054	0.060	0.062	0.063	0.064	0.066
Reform	-0.149	-0.064	0.038	-0.097	-0.111	-0.094
	(0.160)	(0.173)	(0.192)	(0.272)	(0.328)	(0.228)
Percentage change	-2.770	-1.065	0.617	-1.547	-1.732	-1.416
Observations	622,583	319,093	263,592	210,399	157,155	103,761
Panel B: Sample of men with low educated father						
Probability prison conviction	0.051	0.055	0.057	0.058	0.059	0.061
Reform	-0.049	-0.158	-0.303	-0.551*	-0.778**	-1.011
	(0.171)	(0.217)	(0.245)	(0.312)	(0.394)	(0.716)
Percentage change	-0.970	-2.852	-5.316	-9.484	-13.120	-16.574
Observations	264,679	150,620	125,952	101,266	76,207	50,222

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Marginal effects are scaled by 100. Robust standard errors in parantheses, clustered by municipality of birth. All regressions include a full set of birth municipality and birth cohort dummies. The reported estimates in both panels are from a linear probability estimation with weighted least squares for the dependent variable probability of being convicted to a prison sentence at least once. The used weights are: $\sqrt{x'b(1-x'b)}$, which are obtained from a first stage OLS estimation.

The log-odds-ratio estimates from the logit model translate into a marginal effect of a 0.646 percentage points decrease in the probability of a conviction, which is very similar to the marginal effect obtained from the linear probability model (LP column). Hence, the logit model suggests similar to the linear probability model, that father's reform assignment significantly reduces the total share of convicted men by about 2.5 percent.

As can be seen in Table 22, the results of the linear probability model for the effects of the reform status of father and mother on the probability

of a prison sentence of sons show no significant effects.

Table 21: Logit estimates of father's and mother's reform assignment on the probability of their sons having ever been convicted.

	(1)	(2)	(3)	(4)
	Logit	Marginal effect	Logit	Marginal effect
Sample:	All	All	Low educ	Low educ
Dependent variable: son has ever been convicted to prison				
Panel A: Father's reform assignment				
Probability conviction	0.265	0.265	0.247	0.247
Reform father	-0.033**	-0.646***	-0.052***	-0.972***
	(0.015)	(0.296)	(0.020)	(0.366)
Percentage change		-2.443		-3.938
Observations	563,754	563,754	243,082	243,082
Panel B: Mother's reform assignment				
Probability conviction	0.265	0.265	0.278	0.278
Reform mother	0.012	0.225	0.021	0.419
	(0.014)	(0.277)	(0.018)	(0.363)
Percentage change		0.851		1.509
Observations	595,138	595,138	255,075	255,075

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Marginal effects of the Logit estimates are scaled by 100. Robust Standard errors in parentheses, clustered by municipality of birth of the father (Panel A) or mother (Panel B). All regressions include a full set of birth municipality dummies and birth cohort dummies of father or mother.

Table 22: Estimates of father's and mother's reform assignment on the probability of their sons having ever been convicted to a prison sentence.

	(1)	(2)
Sample:	All	Low educ
Dependent variable: son has ever been convicted to prison		
Panel A: Father's reform assignment		
Probability prison	$\bar{p} = 0.034$	$\bar{p} = 0.028$
Reform father	0.015	0.005
	(0.093)	(0.123)
Percentage Change	0.448	0.169
Observations	563,754	243,082
Panel B: Mother's reform assignment		
Probability prison	$p = 0.034$	$p = 0.028$
Reform mother	0.038	0.004
	(0.093)	(0.130)
Percentage change	1.093	0.108
Observations	595,138	255,075

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Marginal effects are scaled by 100. Robust Standard errors in parentheses, clustered by municipality of birth of the father (Panel A) or mother (Panel B). All regressions include a full set of birth municipality dummies and birth cohort dummies of father or mother. Presented estimates from a linear probability model with weighted least squares, the used weights are: $\sqrt{x'b(1-x'b)}$ obtained from first stage OLS estimations. Column (1) presents results for all sons of men or women born 45-55, and column (2) for those sons whose father or mother has a low educated father.

Table 23: Estimates of father's and mother's reform assignment on the probability of their sons having ever been convicted; sample restricted to sons older than 16, 17 or 18 when crime is recorded.

Panel A: Father's reform assignment						
Sample	All≥16	Low educ≥16	All≥17	Low educ≥17	All≥18	Low educ≥18
Reform	-0.675***	-1.036***	-0.707***	-1.024***	-0.736***	-1.041***
	(0.212)	(0.363)	(0.210)	(0.372)	(0.214)	(0.374)
Observations	556,390	240,029	546,869	236,121	535,200	231,182
Panel B: Mother's reform assignment						
Sample	All≥16	Low educ≥16	All≥17	Low educ≥17	All≥18	Low educ≥18
Reform mother	-0.146	-0.042	-0.139	-0.046	-0.135	-0.054
	(0.250)	(0.332)	(0.250)	(0.333)	(0.251)	(0.333)
Observations	592,491	254,026	588,578	252,452	583,207	250,226

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Marginal effects are scaled by 100. Robust Standard errors in parentheses, clustered by municipality of birth of the father (Panel A) or mother (Panel B). All regressions include a full set of birth municipality dummies and birth cohort dummies of father or mother. Estimates are from a linear probability model with weighted least squares, the used weights are: $\sqrt{x'b(1-x'b)}$ obtained from first stage OLS estimations. Samples are indicated by "All" which are sons of father or mothers born 45-55, and Low educ sons with low educated grandfathers. Samples are additionally restricted to sons who are at least 16, 17 or 18 in the last year of available crime records in 2008.

8.4.3 Classifying Municipalities

Individuals who were assigned to the reform might be more likely to move to higher income/lower crime areas later in life. Our strategy to study this is to use pre-reform municipality income levels from the year 1960 to classify municipalities into lower than median income and higher than median income municipalities before the reform, since the reform itself may have affected post reform municipality characteristics.[31] Our main focus is to see whether individuals assigned to the reform are more likely to move from low to high income municipalities. For this mobility analysis across municipalities we will use individual information on birth municipalities, the municipality of residence in 1991 and municipality income levels in 1960. For each individual we will assign the income level of their birth municipality and whether it was below or above median income in 1960. Furthermore, we determine where each individual lived in 1991 and assign the income level of that municipality but at the pre-reform income level in 1960, as well as the according income classification.

This analysis is complicated by the fact that Sweden's municipalities went through several reforms between 1953 and 1986 that changed the local government district division and the numerical codes used in administrative data. In our data we have 1046 different municipality codes in 1952. By 1986 Sweden's amount of municipalities was reduced to 286.[32].

The reduction of municipalities was mainly done through merges of several municipalities. More specifically, 965 municipalities were merged with neighboring municipalities to build municipalities with one code or in some cases remained the same. In these cases we are able to assign

[31] Unfortunately, we do not have crime records on municipality level before 1981.

[32] All municipality code changes are taken from the report of Statistics Sweden that lists all municipality code and administrative division changes between 1952-1986: Sveriges kommuner åren 1952-1986 Förändringar i kommunindelning och kommunkoder, SCB Meddelanden i samordningsfrågor, Sverige (1986): 5; most changes were finalized already before 1976

unique new post-municipality reform codes that correspond to the previous municipality codes.

However, in a few cases municipalities were split up into several other communities: 76 of the original 1046 municipalities were split up into 2 different municipalities, and 8 original ones were split up into 3 different municipalities. For those 84 cases of split ups we cannot determine new post-municipality reform codes that uniquely correspond to the before 1952 municipality codes. Due to this ambiguity we decided to assign the municipality code of the municipality with the highest population among those municipalities into which the municipality was divided.[33] All together this process led to the mentioned 298 municipalities by 1986.

For our mobility analysis we use the population weighted average of municipality income levels of 1960 for the new 298 municipalities and whether it is below or above median income. More specifically, the income levels of the new municipality codes are computed using the 1960 income levels and population sizes of the municipalities that will later build the new municipalities. We match this information to each individual to assign a municipality income level according their birth municipality and a municipality income level according to their municipality of residence in 1991 both as of 1960 levels and according to the new municipality codes. All birth municipality codes are thus brought in accordance with the new codes after the municipality reform and those are used for the analysis because one would obtain a mechanical move of individuals by the changes of municipality codes even though individuals did not move.

[33]When matching the data some municipalities where individuals lived in 1991 did not appear in our municipality coding because they were split up municipality cases and the higher population destination was chosen. In these five cases we assign the income level of the municipality that was not chosen by our rule.

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