The Effect of Education Policy on Crime: An Intergenerational Perspective[*]

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September 24, 2013

Abstract

The intergenerational transmission of human capital and the extent to which policy interventions can affect it is an issue of importance. Policies are often evaluated on either short term outcomes or just in terms of their effect on individuals directly targeted. If such policies shift outcomes across generations their benefits may be much larger than originally thought. We provide evidence on the intergenerational impact of policy by showing that educational reform in Sweden reduced crime rates of the targeted generation and their children by comparable amounts. We attribute these outcomes to improved family resources and to better parenting.

Keywords: Economics of crime; compulsory education reform; intergenerational transmission; returns to education; returns to human capital; comprehensive school

JEL Codes: I20; I21; I28; J62; K42

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1 Introduction

Crime imposes huge costs on society. Earlier papers have demonstrated that crime and education are related and that policies that increase education can reduce crime (see Lochner and Moretti (2004)). There are strong theoretical reasons why this should be the case since increased education improves economic opportunity and can also increase the psychic costs of committing crimes. Becker (1981), Freeman (1999) and Lochner (2004) amongst others have developed theoretical models with these predictions. A number of papers have demonstrated the empirical relevance of these models and documented the strong association between education and crime.[1]

An outstanding question, however, is to what extent education policies can have intergenerational effects on crime. 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.

Intergenerational associations of criminal behavior have been documented in the criminology literature. 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 Swedish study the same authors (see Hjalmarsson and Lindquist (2010)) following the approach of Björklund, Lindahl, and Plug (2006), focus on parent-child correlations in crime using adoption data, to

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[1]Examples include Grogger (1998), Gould, Weinberg, and Mustard (2002) Machin and Meghir (2004) and Edmark (2006). For Sweden Edmark (2006) shows the relationship between unemployment rates and property crimes on county level. Williams and Sickles (2002) finds that years of schooling reduces crime in adulthood. Freeman (1996), based on the 1991 US census, documents that 12 percent of 24-35 year old high school drop outs were incarcerated in 1993; The criminology and sociology literature presents similar evidence, see e.g. Sabates and Feinstein (2008a) and Sabates and Feinstein (2008b). Finally, Gallipoli and Fella (2006)) develop an empirical model that allows for general equilibrium policies that can offset direct effects.

determine the factors through which mothers and fathers influence child criminality.

In general, child outcomes will be driven by predetermined parental characteristics and by the investments parents (and possibly the state) undertake to promote the child's human capital.[2] Cunha, Heckman, and Schennach (2010) formalize the intergenerational links and show the importance of parental background and investments for child cognitive and social skill outcomes. An implication of their results is that improving parental skills will have a direct impact on their children, while the increased parental resources may increase investments leading to further intergenerational improvements (see also Cunha (2007) Caucutt and Lochner (2012)).

Several theories originating from sociology and social behavior also predict a causal relationship between family resources and criminal behavior of the offspring. Merton (1938) suggests that lack of means to fulfill culturally defined societal goals may cause some individuals to reject legitimate means of achieving these goals. Coleman (1988) stresses the importance of interaction between parental human capital and other family resources - such as parental attention, control and quality of parent-child relations - in the formation of child human capital.

There is direct evidence that better childhood environments and early education can reduce crime rates as shown by the Perry pre-school experiment presented in Schweinhart, Montie, Xiang, Barnett, Belfield, and Nores (2005) and Cunha and Heckman (2007). Our focus here is estimating the impact of educational interventions received by the parents on child crime outcomes. Specifically, we show that the Swedish comprehen-

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[2]see Becker (1981). For some evidence on the importance of mother's education on child outcomes see for example Carneiro, Meghir, and Parey (forthcoming); Deming (forthcoming) highlights the importance of school quality and it's potential impact on crime.

sive school reform, (originally studied by Meghir and Palme (2005) for its effects on education and earnings) substantially decreased crime rates of the target generation and that of their children. The reason we may expect this intergenerational effect is because men affected by the reform attain higher education levels, have improved cognitive and social skills, earn substantially more, engage less in criminal activity, and marry higher earning wives.

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 background characteristics covering the entire population. The dataset also links information on three generations. This allows us to estimate the effect of the reform on both the parent generation (confirming results from earlier studies) and on the child generation. Our empirical approach is based on comparing changes in the crime rate across cohorts in municipalities that implemented the reforms at different times.

The paper is organized as follows. We first provide a brief description of the reform followed by a data section outlining our administrative data, documenting the crime rates and presenting descriptive evidence on inter-generational associations in crime. We then discuss our empirical strategy followed by the main results, first on the parent generation and then for the child generation (which is our main focus). We then close by a discussion of the results and a concluding summary.

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2 The 1950 Swedish Education Reform

2.1 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 continue one or (mainly in 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 there was increasing pressure to reform the educational system so as to respond to the increasing amounts of education in countries such as the US (see Goldin (1999)) and to offer a unified path to higher education.[3]

In 1948 a parliamentary school committee proposed a school reform that implemented a new nine-year compulsory comprehensive school, abolished early tracking and introduced a national curriculum. Until the age of 16 all children would henceforth attend a comprehensive school with a national curriculum. The extension to nine years of compulsory schooling meant that in many parts of the country the compulsory increase was two years, while in others it was one.[4]

2.2 The Social Experiment

The proposed new school system, as described above, was introduced gradually from 1949 to 1962 in municipalities or parts of city communities,

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[3]See Erikson and Jonsson (1993) for an analysis of the educational trends at the time.

[4]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).

which in 1952 numbered 1,055 (including 18 city communities).[5] The se-
lection of municipalities was not random. However, the selection of areas
was guided by an attempt to ensure the implementing municipalities were
representative of the whole country, both in terms of demographics as well
as geographically.

Figure 1: Proportion of individuals in sample assigned to the reform
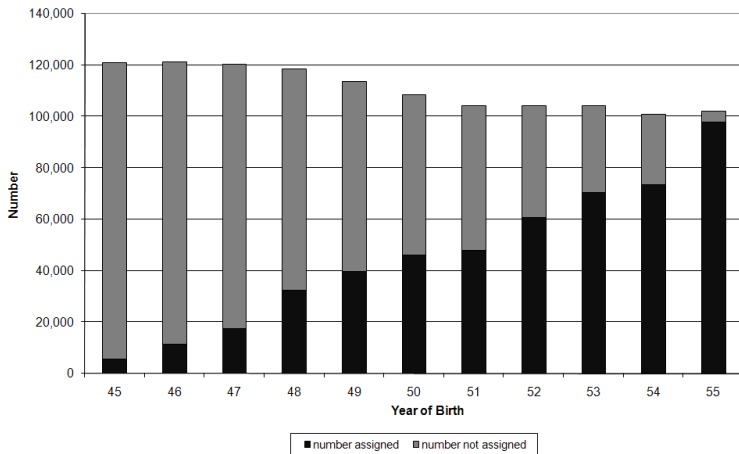
When a municipality introduced the new school system it implemented
it either for the cohort of pupils who where in fifth grade at the time of
the decision or for those who were currently in the first grade, effectively
delaying the start of the program. In our analysis we consider cohorts born
between 1945 and 1955. Figure 1 shows the number of observations in each
one-year birth cohort and the proportion assigned to the reform.

3   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,

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[5]This was done for evaluation purposes as well as a way of resolving the political
differences relating to the reforms. The official evaluation National School Board (1959)
was mainly of administrative nature. Details on this evaluation are also described in
Marklund (1981).

provided by Statistics Sweden.[6] We are able to link and use three generations in our analysis: the *parent generation* consisting of all individuals born in Sweden between 1945 and 1955, which attended school during the social experiment described above; their parents labeled as the *grandparent generation*; and their children referred to as the *children generation*.[7]

We do not have direct information on individual assignment to the reform. Our reform assignment variable is based on information on parish of birth from the population census. Using information on year of birth and when the individual's municipality of birth implemented the reform we then use an algorithm provided by Helena Holmlund (see Holmlund, 2007) to decide whether or not the individual went through the pre of post reform school system.

The advantage with using this variable for reform assignment, rather than one based on direct information on type of school attended, is that it is not susceptible to endogeneity caused by parents moving to municipalities on the basis of preferences for school system for their children. The disadvantage is that it might lead to some attenuation of the effects of the reform because some individuals may have moved leading to some measurement error with respect to actual assignment. As a sensitivity analysis we will therefore present results on the basis of information on municipality of living in the 1960 population census (which is also not perfect).[8]

Information on the education level for the *parent generation* and *child generation* was obtained and matched onto our sample from the Swedish National Education Register. For the *grandparent generation* we obtain information from the 1970 census.

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

[7] 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.

[8] Information on mobility for the 1948 and 1953 cohorts is reported in Meghir and Palme (2005)

Data on all convictions in Sweden covering the time period between 1973 and 2010 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 of the effects of education reform on criminal behavior (Lochner and Moretti (2004) and Machin, Marie, and Vujić (2011)). For each conviction we have detailed information on the type of crime for the main violation within the conviction and the age when it was committed. We categorize crimes into seven types: violent crimes, property crimes, fraud and tax evasion, traffic crimes (excluding parking and speeding tickets), drug and trafficking violations, sex crimes and others containing crimes that cannot be categorized as any of the latter six categories.[9]

We select the sample of men born 1945-1955 who were alive in 1973 (when the crime records begin) and who had not migrated out of Sweden permanently. From the education census we link in the education of their fathers (the grandparent generation), which is available if they were younger than 60 in 1970, i.e. for 71.6% of the cases. The sample of sons is restricted to those who have reached the age of criminal responsibility (age 15) in 2008, and have not migrated permanently out of Sweden. For this sample of sons we are able to match education information of paternal grandfathers for 61.53%.

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[9]Types of crimes are detailed in several variables that specify the chapter, paragraph, moment, piece and point in the section of the relevant penal code (law-book). Details of the types-of-crime variables in the conviction data are in brå Variabelbeskrivning Lagföringsregistret (2009) and the documentation of coding crime types can be found in Brå Kodning av brott (2010). The crime register also contains information on the number of crimes within each individual's conviction, the date of conviction, the age of the offender, as well as the penalty for each crime.

Table 1: Percent convicted by age and type of crimes

Percent convicted - by crime types	total	violent	property	fraud	traffic	drugs	sex	others	prison
Men born 45-55, N= 447,382									
All	33.94	5.081	7.132	6.165	19.67	5.784	0.551	11.28	6.668
ages 20-24 (N=133,200)	19.11	1.933	4.905	2.259	8.256	3.392	0.0781	5.901	3.114
ages 25-29 (N=339,888)	11.53	1.215	2.523	1.701	4.536	2.371	0.0771	3.297	2.399
ages 30-39 (N=447,382)	12.68	1.766	2.417	2.515	5.558	2.020	0.159	3.658	2.718
ages 40-49 (N=447,382)	11.21	1.498	1.513	1.526	6.156	1.048	0.171	2.710	
Men born 45-55 low education background, N=283,841									
All	34.73	5.623	7.514	6.376	20.35	5.793	0.613	11.71	7.140
ages 20-24 (N=80,835)	20.32	2.292	5.366	2.484	9.008	3.424	0.0829	6.314	3.502
ages 25-29 (N=212,906)	12.09	1.397	2.694	1.827	4.948	2.399	0.0930	3.371	2.577
ages 30-39 (N=283,841)	13.27	1.982	2.598	2.612	5.968	1.999	0.181	3.832	2.978
ages 40-49 (N=283,841)	11.51	1.653	1.646	1.594	6.182	1.075	0.187	2.883	
Sons of men born 45-55									
All 15-29 (N=426,721)	23.69	4.555	7.833	2.908	10.90	3.229	0.209	7.386	2.429
ages 15-19 (N=426,721)	15.83	2.525	6.301	1.804	5.620	1.191	0.0820	3.906	0.431
ages 20-24 (N=380,249)	11.80	2.089	2.429	0.980	4.890	1.826	0.0868	3.542	1.805
ages 25-29 (N=294,749)	7.948	1.293	1.136	0.686	3.886	1.501	0.0777	1.965	1.365
Sons of men born 45-55, low education background									
All 15-29 (N=271,971)	25.23	5.019	8.511	3.148	11.94	3.256	0.232	7.914	2.739
ages 15-19 (N=271,971)	16.95	2.761	6.846	1.922	6.253	1.185	0.0952	4.125	0.493
ages 20-24 (N=245,342)	12.54	2.297	2.662	1.080	5.275	1.820	0.0937	3.796	2.026
ages 25-29 (N=194,854)	8.344	1.424	1.249	0.741	4.040	1.530	0.0831	2.109	1.467

Table 1 shows conviction rates for men in the parent generation born between 1945-1955 and their sons. We report conviction rates in total and by all seven crime categories. The overall conviction rates state the percent of the sample that has ever been convicted for any crime, or for a certain type of crime within the observation period 1973-2010. Age specific conviction rates are defined as the percent of individuals convicted at least once within the age bracket among those who were fully observed throughout the considered ages. Furthermore, we separately report conviction rates for low educated backgrounds, which refers to the lowest level of education (pre-reform statutory level) of the grandparent generation.

During the entire observation period 1973-2010, 34 percent of men in the parent generation have been convicted at some point of a crime, 5 percent of all men have been convicted for a violent crime, 7 percent for property crimes, 6 percent for fraud, almost 20 percent for a traffic violation, 6 percent for a drug offence, 0.5 percent for sex crime, and 11 percent for a crime in the others category. The oldest cohort, born in 1945, was aged 28 when we start observing convictions in 1973, whereas the youngest cohort, born in 1955, was observed from the age of 18 and onwards. This restricts our ability to fully observe the parent generation at very young ages. Though for the 1953-1955 cohorts we are able to observe ages 20-24, for those born between 1948-1955 we are able to fully observe ages 25-29, and for ages 30 and above we are able to observe the full parent generation sample.

Age specific conviction rates reveal that the highest conviction rates are at younger ages with 19 percent of males convicted between the ages 20-24 but only 11.5 percent at ages 25-29. For the ages 30-39 conviction rates are slightly higher at 12.63 which should not be surprising considering that this relates to a 10-year compared to a 5-year age bracket. The age pattern for different types of crimes is similar to the overall rates. Conviction rates

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in the low educated background sample are slightly higher throughout for all types of crimes and all ages (with the exception for drugs category).

We report convictions for sons in the 15-29 age bracket. Over this age range 24% of all sons have at least one conviction, with 4.5 being violent offenders, almost 8 percent have been convicted for a property crime, 3 percent for fraud, 10 percent violated traffic laws, 3.2 percent were found guilty of drug offenses, 0.2 percent were convicted for sex crimes and 7.3 percent have been convicted for other crimes. All of the sons in our child generation sample are observed already in the age group 15-19 and 15.8 percent have been convicted in that age bracket. Among those fully observed at ages 20-24, 11.8 percent have been convicted in their early twenties.[10] The conviction rate at older ages 25-29 decreases further to 7.9 percent. This pattern of decreasing crime rates at older ages is observed for all types of crimes. Similarly, to the parent generation, conviction rates for the low educated background sample are higher at all ages and for all types of crimes (again with the exception for drug crimes).

The stated conviction rates for men of roughly 34 percent for the parent generation and 24 percent for sons is a surprisingly high proportion of the population, which prompted us to look into this in greater detail by types of crimes. One concern is that traffic crimes account for a large proportion of the convictions. However, traffic crimes are serious violations and they exclude parking and speeding tickets, and only about 5-10 percent of the sample has only a traffic violation such that the overall conviction rate excluding traffic crimes remains at 23.3 percent for the parent generation and at 18.8 percent for their sons. 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).

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[10]Sons born before 1986 who are 24 years old in our last observation year 2010.

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3.1 Parental Background, Education and Crime

Table 2 shows the results from regressing conviction (columns 1 and 3) and incarceration (cols 2 and 4) on ' and mother's education based on a a Linear Probability Model. All regressions include dummies for the municipality of north of the father and cohort effects.

One year of own schooling for men in the parent generation is associated with a decrease of the probability of a conviction by 2.5 percentage points corresponding to a 7.4% reduction in conviction rates. Including parental education increases the coefficient of own schooling for conviction but not on incarceration. Both father's and mother's education are significant but the impact of the former is larger.

Finally, Table 3 illustrates the intergenerational associations of crime. The probability of ever being convicted increases by over 13 percentage points if the father has been convicted. The effect association remains unchanged when we focus on those with a low educated grandfather. The father haven been jailed is associated with a 6-7 percentage point increase in the probability that the son will go to prison.

Table 2: Association between own and parental education and crime

	(1)	(2)	(3)	(4)
	Men born 45-55		Sons of parents born 45-55	
	Conviction	Prison	Conviction	Prison
Own Schooling	-2.496***	-0.998***	-4.246***	-0.754***
	(0.114)	(0.059)	(0.036)	(0.020)
Father's Schooling			-0.218***	-0.038***
			(0.033)	(0.012)
Mother's Schooling			-0.087**	-0.029***
			(0.040)	(0.010)
$\bar{y}$ %	33.88	6.597	24.48	2.371
Observations	444,272	444,272	273,093	273,093

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Effects scaled by 100. Standard errors in parentheses clustered by own birth municipality and for son estimations by father's birth municipality. Includes own birth cohort and birth municipality indicator variables, and for sons father's cohort and birth municipality indicator variables. The sample of sons are those whose father and mother were born 45-55 and for whom we have education levels available.

Table 3: The Association between son's and father's crime

	(1)	(2)	(3)	(4)
	All education		Low education	
	Convict	Prison	Convict	Prison
Father convicted/prison	13.063***	6.504***	13.337***	6.952***
	(0.192)	(0.173)	(0.215)	(0.242)
Observations	410,475	410,475	261,918	261,918
mean dependent var	23.54	2.380	25.09	2.682

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Effects scaled by 100. Standard errors in parentheses clustered by father's birth municipality. Includes father's cohort and birth municipality indicator variables.

4 Empirical Strategy

The main outcome variables we use is whether an individual was ever convicted during the observation window 1973-2010 for any crime; by types of crimes: violent, property, fraud, traffic, and drugs crimes; and whether and individual was convicted at cetain ages: 15-19 (for children generation), 20-24, 25-29, and 30-39 (for parent generation only). All the analysis we present is for males only because the female crime rate is very low - about a quarter of the male one, and was not been affected by the reform. We present results for the whole sample and separately for those with a low SES background.[11] Our main focus is on the child generation. However, we summarize some results for the parent generation for completeness. A detailed analysis for the parent generation can be found in an earlier version of our paper Meghir, Palme, and Schnabel (2012) and in Hjalmarsson, Holmlund, and Lindquist (2011).

The crime records start in 1973 and our sample in the parent generation covers the 1945-1955 cohorts. Hence the youngest person it is possible to observe is 18, thus covering most of the relevant part of the lifecycle. As a result of this (inevitable) lower age cutoff the estimated effect of the

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[11]We refer to low SES background those families where the male in the grandparent generation had at most statutory education. This is the largest group since schooling at that time was relatively low.

reform on the parent generation does not include the mechanical impact due to keeping the treated children off the streets by increasing compulsory schooling them.

For the child generation we observe the criminal history from the age 15, when criminal responsibility begins and crimes are recorded according to Swedish law. We follow them until the age of 29. This allows us to measure the effects on the most important part of the criminal lifecycle. The child generation all attend the same schooling system because the reform had been rolled out nationally at that point. The only difference is that for some the fathers also attended the new system, while for others they did not. The children of both treated and untreated fathers live in the same labor markets.

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-

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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 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.[12] The general assumptions underlying the method of Difference in Differences are discussed in Heckman and Robb (1985), **?**Athey and Imbens (2006) and Altonji and Blank (1999).

5 Results

5.1 The Reform, Educational Attainment and Crime in the Parent Generation

In what follows all regressions include a full set of fixed effects for the birth municipality and the cohort of the parent generation, as well as the education level of the previous (grandparent) generation.

5.1.1 Education

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

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[12]In a Monte Carlo experiment we generated data based on the normal distribution assuming impacts of the reform of a about the size we find here. We then estimated the impacts using the linear probability model and found results that were indistinguishable from the true outcomes.

Table 4: Reform effects on years of schooling for the generation directly affected by the reform, including father's education

Dependent variable: Own years of schooling			
	(1)	(2)	(3)
Sample:	All	Low educ	High educ
Panel A: Men born 45-55			
Reform birth	0.174***	0.267***	0.052*
	(0.038)	(0.038)	(0.030)
Obs	444,272	282,080	162,192
mean years of schooling	11.62	10.91	12.85

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered by municipality of birth, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of individual. Column (1) includes all men born 1945-1955 and controls for father's education level, column (2) restricts the estimation to men of 45-55 cohorts and whose father was low educated, and column (3) restricts the estimation to men from high education backgrounds.

low educated fathers, as reported in Meghir and Palme (2005). However, in this broader and larger sample we find a small but significant effect (at the 10% level) on those with higher educated fathers. When we control for municipality specific trends the results become slightly stronger but not significantly so (see Appendix Table 14). The effect for women is similar and shows a strong and significant increase in schooling for those with a low educated father, though smaller in magnitude than for males. We find no effect on those women with a father who has higher than statutory level of education.

5.1.2 Crime

Our focus is on crime in the child generation, but we first report results for the parents, providing a link with the existing literature.[13]

The first column in Table 5 shows the impact of the reform on the probability of being convicted any time in the observation period 1973-2010 for those born in the 1945-1955 period. This estimate includes the

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[13]See Lochner and Moretti (2004)

impact on older cohorts for whom the available criminal records (which start in 1973) do not include the ages of peak criminal activity (around 20). The crimes we observe are always after the end of statutory schooling and hence do not include the more mechanical effect of keeping children off the streets. Thus, Table 5 also shows results for sub-samples of younger cohorts. The impacts increase as we successively exclude the older cohorts and become significant for those born after 1951: the reform leads to a 1.5 percentage point decline in crime for the 1952-55 cohort in the observation window. We believe that this is mainly driven by the increased presence in the data of younger individuals. Indeed, when we break up the impact by age, keeping cohort fixed to 1952-55, we find that the largest effect by far is for those aged 20-24, where we obtain a decline within that age group of 1.47 (st. error 0.63). Finally, the effects are larger for the younger cohorts where the grandparent education is lower but the impacts are not statistically distinguishable (see Panel B in Table 5).

Next, in Table 6, we split up the effect by type of crime committed. Here it becomes clear that the impact is driven by property and traffic crime.[14]

Our results confirm earlier findings of the impact of compulsory schooling reforms on crime in the US Lochner and Moretti (2004) and in the UK (Machin, Marie and Vijic, 2011). Not surprisingly they are also consistent with those of Hjalmarsson et al. (2013) for Sweden, who followed up on our working paper results by extending the data to earlier periods.[15] We now move on to the impact on the child generation, which is the focus of this work.

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[14]Note that these are the more serious traffic crimes that lead to a court appearance and do not include speeding or parking tickets.

[15]For completeness we now have extended our data to include the entire period where crime records exist. Our results use a more detailed division of type of crime, we examine heterogeneity of impacts based on the education level of the previous (grandparent) generation and we investigate impacts by age.

Table 5: Effects of the reform on own crime for men - split up by birth cohorts, including father's education level

	(1)	(2)	(3)	(4)	(5)	(6)
cohorts	45-55	50-55	51-55	52-55	53-55	54-55
Panel A: All education levels, including father's education
Reform birth	-0.006	-0.381	-0.551	-1.464***	-1.626**	-2.987***
	(0.279)	(0.411)	(0.435)	(0.556)	(0.822)	(1.133)
Obs	447,382	258,917	217,407	176,232	133,200	88,588
$\bar{y}$ %	33.94	37.00	37.80	38.62	39.44	40.35
Panel B: Low educated fathers
Reform birth	0.052	-0.825*	-0.781	-1.693**	-1.843*	-3.791***
	(0.351)	(0.493)	(0.552)	(0.680)	(1.004)	(1.366)
Obs	283,841	160,430	133,907	107,557	80,835	53,224
$\bar{y}$%	34.73	38.14	39.05	40.00	40.95	42.04

Notes: Results are scaled by 100. Robust standard error in parentheses, clustered by municipality of birth.

Table 6: Effects of the reform on own crime by types of crimes for men of cohorts 52-55, including father's education

	(1)	(2)	(3)	(4)	(5)	(6)
	Any	Violent	Property	Drugs	Traffic	Fraud
Panel A: All education levels, including father's education, Obs: 176,232
Reform birth	-1.464***	-0.364	-0.783**	-0.427	-1.391***	-0.334
	(0.556)	(0.297)	(0.347)	(0.294)	(0.473)	(0.269)
$\bar{y}$ %	38.62	6.156	9.366	7.233	22.59	7.024

Notes: Results are scaled by 100. Robust standard errors in parentheses, clustered by municipality of birth.

5.2 The Reform and Crime in the Child Generation

In Table 7 we show the impact of the reform on the probability of conviction in the child generation for any age between the ages of 15-29 inclusive. The first column shows the results for the entire sample and columns 2 through 4 show the effect in different age groups, separately. Panel B in the table shows the results for those whose fathers were born in low SES homes, i.e. those where the grandfather had only statutory schooling.

The overall result is a highly significant reduction in criminality of 0.78 percentage points (pp) in the child generation. The point estimate is similar, and also significant, in the group originating from low SES families. The division of the sample by age groups shows that the effect is largest for the younger (15-19) age group and declines for older groups.

Table 8 splits up the effect by type of crime. Such analysis is important because different types of crime have a different social cost and may have different underlying motivations, which in turn is suggestive about the way the reform affected crime outcomes. We see that the effects that dominate are the reduction of violent crime, traffic and fraud each by about 0.2-0.3pp. Interestingly, property and drugs crime seem unaffected with the estimates being effectively zero.

The results for those from a lower education background are very similar although less precise because of the smaller sample size. This is perhaps surprising because the reform increased the education levels in the parent education most for this group. This points either to a large intergenerational crime elasticity with respect to parental education in the higher socio-economic group and/or to other effects of the reform on the parent generation, such as the impact of abolishing tracking and introducing comprehensive schools where students from all socio-economic backgrounds mix. This aspect of the reform may have affected social skills of individuals in the higher socio-economic group. Indeed, evidence of this is presented in Meghir, Palme, and Simeonova (2011), where it is shown that the social skills measure for this group increased substantially as a result of the reform.

5.3   Sensitivity analysis

The estimates point to a significant reduction of crime for the children of those affected by the reform. In the course of estimation we have used certain specification choices and assumptions as reflected in the linear Dif-in-Dif specification; we now examine the sensitivity of the estimates to some of these choices.

Table 7: Age specific effects of father's reform assignment on son's crime including grandfather's education levels

	(1)	(2)	(3)	(3)
Convicted at age:	15-29	15-19	20-24	25-29
Cohorts observed:	1960-1993	1960-1993	1960-1988	1960-1983
Panel A: All education levels, including grandfather's education				
Reform father birth	-0.779***	-0.589***	-0.314	-0.107
	(0.257)	(0.210)	(0.203)	(0.186)
Obs		410,476	365,782	283,297
$\bar{y}$ %	23.54	15.70	11.69	7.861
Panel B: Low educated grandfather				
Reform father birth	-0.667**	-0.567**	-0.196	0.210
	(0.326)	(0.267)	(0.249)	(0.238)
Obs		261,918	236,289	187,515
$\bar{y}$ %	25.09	16.81	12.44	8.255

Notes: Results are scaled by 100. Robust standard error in parentheses, clustered by father's municipality of birth.

Table 8: Effects of father's reform assignment on son's crime by type of crimes including grandfather's education level

	(1)	(2)	(3)	(4)	(5)
Convicted at age 15-29:	Violent	Prop	Drugs	Traffic	Fraud
Panel A: All education levels, Obs 410,476, incl. grandfather's education					
Reform father birth	-0.243**	-0.019	0.095	-0.446**	-0.224**
	(0.121)	(0.158)	(0.110)	(0.178)	(0.097)
$\bar{y}$ %	4.485	7.736	3.182	10.82	2.877
Panel B: Low educated grandfathers, Obs 261,918					
Reform father birth	-0.108	0.022	0.081	-0.444*	-0.233*
	(0.163)	(0.203)	(0.139)	(0.238)	(0.120)
$\bar{y}$ %	4.946	8.406	3.199	11.86	3.109

Notes: Results are scaled by 100. Robust standard error in parantheses, clustered by father's municipality of birth.

5.3.1 Municipality of residence versus birth municipality reform assignment

Our reform assignment in the previous section is based on the municipality of birth for those in the parent generation. The alternative would be to assign the reform indicator based on the municipality of residence in 1960. By using the birth municipality we avoid any biases caused by the grandparent generation choosing the municipality based on their schooling preferences and the ability of the child. On the other hand, this means that the effect of the reform may be attenuated to the extent that people

move. As an (imperfect) alternative, we also show results with a reform assignment indicator, which is based on the region of residence in 1960. This does not necessarily reflect the actual reform assignment but it may be closer if families move about less once the child has started school.

Table 9 compares the results for the overall effect in the two samples used above with the two different reform assignment variables. For the overall sample we now obtain a smaller impact (-0.59pp compared to -0.78pp); however for the low education group the effect increases. None of these measures are perfect for the reasons we discussed and because neither necessarily assigns individuals to the precise school system they attended. Nevertheless, they do not change the main thrust of the results and the differences are not significant. Based on the intention to treat logic we believe the ones using the municipality of birth of the parent generation is a cleaner way of approaching the problem.

Table 9: Effects of father's reform on son's crime by type of crimes at ages 15-29 for two reform assignments - including grandfather's education levels

	(1)	(2)	(3)	(4)
	All		Low Education	
Reform Assignment based on:	Birth	Residence	Birth	Residence
Impact	-0.779***	-0.590**	-0.667**	-0.783**
	(0.257)	(0.261)	(0.326)	(0.320)
mean in %	23.54	23.60	25.09	25.15
Observations	410,476	416,975	261,918	265,577

Notes: Results are scaled by 100. Robust standard error in parentheses, clustered by father's municipality of birth or father's municipality of residence in 1960.

5.3.2 Common trends assumption

One of the key identifying assumptions of our approach is that the underlying trends in crime are the same irrespective of the birth municipality of the parent generation. We now bring to bear evidence for this assumption in two different ways: first we repeat our estimation assuming that the

reform took place at a different date than it actually did (placebo estimations); second we explicitly include municipality specific trends to evaluate whether they are significant (akin to a test of overidentifying restrictions) and whether our results are sensitive to their inclusion. For both tests we group municipalities by the earliest cohort for which they implemented the reform. Hence we look at trends specific to each of these groups.

For the placebo estimations, where we pretend that the reform was implemented later, we only use the sample of sons whose fathers were treated by the reform. To construct placebo treatment and control groups we pretend that the reform was implemented one year later, two years, three years, etc. We (falsely) assign the first treated cohort (the first two treated cohorts, the first three treated cohorts, etc.) in each municipality group to be untreated and the remaining ones to the treated group.

Similarly, for the placebo estimations where we pretend that the reform was implemented earlier, we restrict the sample to sons whose fathers were not treated by the reform.[16] The placebo treatment groups are defined by (falsely) assigning the two last untreated cohorts (the three last untreated cohorts, the four last untreated cohorts, etc.) to the treated group and the remaining cohorts stay in the control group.

The results are all brought together in Figure 2. Each dot represents the DD estimate assuming the reform took place at the specified period on the x-axis (relative to when it actually took place). The vertical line around the dot represents the 95 percent confidence interval. The graph shows that the largest (in absolute value) and only significant effect is obtained when we use the correct timing for the reform assignment (that is at zero). In all other cases we estimate insignificant effects and no particular pattern

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[16]We require at least two treated cohorts and one untreated cohort in each municipality group to implement the estimator. This means that we start our first placebo estimation pretending the reform was implemented two years earlier than it actually was.

shows up implying there is nothing systematic taking place biasing the results towards an effect on crime.

Figure 2: Placebo estimations sons
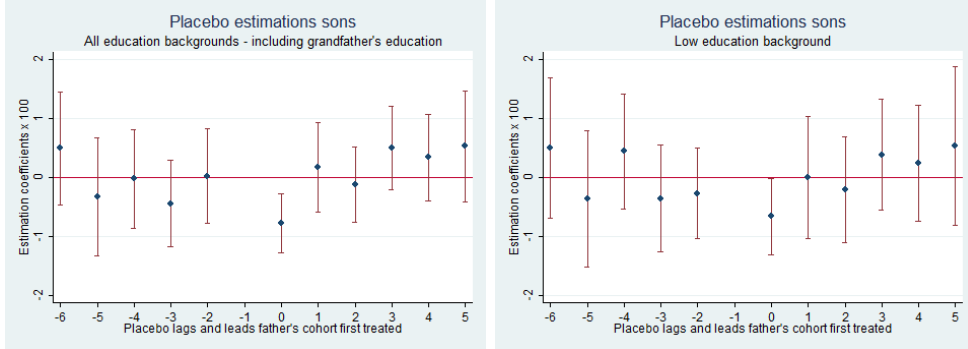
For our second approach, Table 10 shows F-statistics and p-values for two tests: that the trends are not specific to groups of municipalities (sorted by cohort of first implementation) and that the impacts remain unaffected by the inclusion of such trends. For the parent generation as well as for the child generation overall we find no evidence that such specific trends are present. The trends for the child generation of those with lower educated grandparents are however marginally significant (p-value 4.4%). Nevertheless, as is clear from the lower panel of this table, this marginal significance does not translate to a significant effect on the impacts. For example, the overall effect without any municipality specific trends is -0.779 (se 0.257) while when we include trends this becomes -0.800 (se 0.276). For the lower education group the effect drops a bit from -0.667 (se 0.326) to -0.571 (se 0.356). None of these changes are the least bit significant. Both these result and the placebo estimations provide strong evidence that the results are not driven by municipality specific trends. The parameter estimates obtained when the trends are included are shown in the appendix. For the parent generation results where we show estimates for the 1952-55 cohort, there is substantial loss in precision when the municipality specific trends

are added. However for the child generation not much changes either in terms of estimates or in terms of precision, probably because we cover the children of all parental cohorts and the sample size is much larger.

Table 10: Trends tests including father's or grandfather's education

	(1)	(2)	(3)	(4)
	Parent genertion		Children generation	
	All	Low	All	Low
Test 1: joint test of trends=0				
F statistic	1.210	1.505	1.235	1.665
Prob>F	0.249	0.0853	0.230	0.0436
Test 2: test of reform parameter across models				
chi2 statistic	0.0520	0.00073	0.0338	0.320
Prob>chi2	0.820	0.979	0.854	0.571

Notes: Test 1 jointly tests the hypothesis that trends are common across municipalities. Test 2 tests the hypothesis that the impacts are the same when comparing the specification with and without trends.

5.3.3 Graphical analysis of preexisting municipality specific trends

We complete our analysis with a simple graphical representation of the difference in differences result. The sample relates to everyone in the child generation. In Figure 3 we plot the residuals from the Dif-in-Dif regressions (with the estimated average impact added back in) grouped by years to implementation. Each point corresponds to an average residual crime rate across municipalities grouped by their years to implementation.[17] These residuals should display no trend.

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[17]For example if municipality 1 implemented the reform for the 1955 cohort the 1954 cohort would correspond to implementation time -1 the 1953 to -2 etc. Now take municipality 2 which implemented it for say the 1956 cohort on. Then cohort 1955 corresponds to implementation period -1, 1954 to -2, etc. Also for municipality 1 1955 corresponds to zero 1956 to 1, 1957 to 2 and so on, while for municipality 2 implementation time = 0 corresponds to cohort 1956, +1 to 1957, +2 to 1958 etc. The residual crime rates, which for this graph have been purged fixed implementation group fixed effects as well as aggregate trends, are grouped by this time to implementation value and averaged. If differential trends were responsible they would show up as pre or post implementation trends.

Indeed, they do not: the pre-implementation trend is -0.0025 and the post implementation one is zero to 4 decimal points. This completes what we view as conclusive evidence that the results we present on the intergenerational impacts of the reform are robust and not a spurious artifact of other events in the data.

Figure 3: Trend graphs
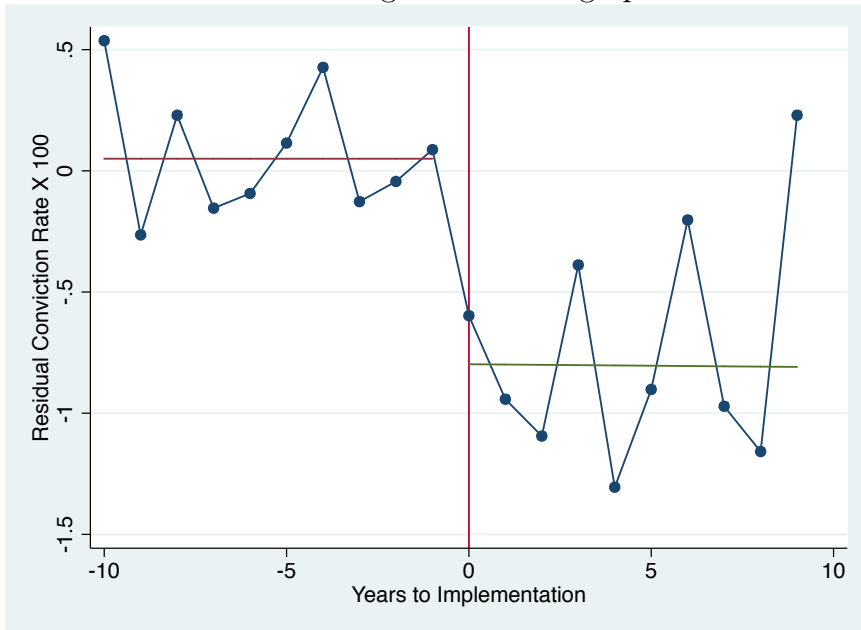
5.3.4 Reform assignment and municipality characteristics

As we discussed in Section 3, the reform was not implemented randomly across municipalities. Both the central government and the local authority had a say on whether and when the reform would be implemented. By controlling for municipality fixed effects we control for permanent and potentially confounding characteristics that may differ across early and late implementers. Nevertheless, it is interesting to document how these differed. Thus we run a regression of the earliest cohort for which a municipality implemented the reform on three municipality characteristics that

are potentially correlated with the municipality crime rate: population size, average income and tax rate in 1960, when the reform could have any effects on outcomes. The results shown in Table 11 imply that early implementers were higher income and had a higher local tax rate. The municipality population size had no effect.

Table 11: Timing of implementation and municipality characteristics

Dependent variable: first cohort implemented		
	(1)	(2)
	all municipalities	excluding Stockholm
Population in 1960	0.036	0.083
	(0.039)	(0.074)
Income level in 1960	-0.072***	-0.074***
	(0.012)	(0.012)
Tax rate in 1960	-0.654***	-0.662***
	(0.066)	(0.067)
Observations	984	983

Notes: Dependent variable is the cohort when the reform was first implemented in the municipality, the regressors are municipality population size, average income and tax rate in 1960..

To the extent that the differences above reflect permanent characteristics they do not pose a threat to identification. However if there are important time varying characteristics that are correlated with crime rates this could lead to biases. In our empirical work we already control for one such variable namely the education level of the grandparent generation which can be correlated with the crime rates of the child generation through intergenerational persistence in human capital. Indeed this variable is significant and when we include it the impacts increase (from -0.5 to -0.78). Moreover, the test for differential trends across municipalities partially addresses this issue as well. We are thus confident that conditional on the fixed effects, the cohort effects and the grandparents' education the reform can be taken as exogenous.

5.4 Incarceration

In the Appendix to this paper we present evidence on the impact of the reform on incarceration, which is an indicator for the more serious offenses that lead to prison sentences. For the parent generation we take the 1952-55 cohorts who are observed from a sufficiently young age. For the child generation we take children of fathers born between 1945-55 in the age range of 15-29, as before. Note that the proportion of convictions that end up in prison sentences are about 30% (incarceration rate 7.5%) for the parent generation and only about 10% for the child generation (incarceration rate 2.4%) over the age window we observe.

For the parent generation the estimates are negative and quite large implying a reduction in prison by about 0.49-0.58 percentage points (st. error 0.32-0.37). For those from a low education background the effects are significant at the 10% level. For the child generation, although the estimates are much more precise the coefficients are much smaller and not significant at all. Perhaps this is not surprising since the baseline incarceration rate is so much lower. Finally including municipality specific trends does not change these results. The full set of results are included in Table 13 in the appendix.

5.5 Discussion

The Swedish educational reform reduced the crime rates of both the direct subjects of the reform (the parent generation) as well as that of their children. For the parent generation the impacts are driven by a reduction in property crimes and those traffic crimes serious enough to lead to a court appearance. Violent and drug-related crimes remained unaffected. For the child generation the impacts are driven by declines in fraud as well as violent crime and traffic offenses. Thus the impact relates both to crimes with

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a clear economic motivation (fraud, property) as well as to crimes relating more to anti-social behavior (traffic).

The persistence of the effects of this educational policy across generations puts a different perspective on the value of such reforms. However, understanding the mechanisms through which the reform achieved these effects is complicated by the multiple possible channels. We present impacts on a number of outcomes as information to help understand the channels that operated. We do not, however, claim to offer conclusive evidence on mechanisms. After all we only have one discrete source of variation.

For the parent generation, who were the direct subjects of the reform, theory points to the improved economic opportunities in the legal labor market resulting from increased education as a key factor leading to a reduction in crime participation (see e.g. Freeman, 1999). In fact, human capital and economic opportunity did improve as a result of the reform: educational attainment increased and as reported in Meghir, Palme, and Simeonova (2011) the reform led to a 0.12 of a standard deviation (se 0.044) increase in cognitive skills for those with low education fathers. Moreover, as reported in Meghir and Palme (2005) and as also reported in Table 12 the reform translated to higher earnings for the parent generation. This in itself increases the opportunity cost of crime. Meghir, Palme, and Simeonova (2011) also report an increase in the armed forces social skills indicator of 17% of a standard deviation (se 0.077) as a result of this reform;[18] interestingly this increase in social skills is driven mainly by those from a higher SES background, demonstrating that the reform affected all groups.[19]

A decline in crime in the parent generation may induce directly a decline in crime in the child generation (through improved role models). In addition, as we discussed in the Introduction, there are several reasons why

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[18]The test is administered to army conscripts. Military service was compulsory in Sweden at that time

[19]0.53 of a st. deviation with a standard error of 0.198.

Table 12: Impact of the Reform on further outcome variables in the parent and child generation

| | Education of Grandparent | |
	All	Low educ
Parent Generation: Men born 45-55		
log annual Earnings $\times$ 100	6.4	6.9
	(3.0)	(2.4)
Ever had a child $\times$ 100	-0.093	-0.096
	(0.185)	(0.273)
Number of children	-0.004	0.001
	(0.007)	(0.010)
Age at birth first child	0.106	0.064
	(0.075)	(0.048)
Child born while a teen $\times$ 100	-0.263**	-0.210**
	(0.106)	(0.100)
Spouse education	0.0499	0.0274
	(0.061)	(0.0274)
Spouse annual earnings in SEK	5,462**	4,829
	(2,672)	(3,361)
Spouse unemployed	-0.003***	-0.0006
	(0.001)	(0.001)
Child Genration: Sons of men born 45-55		
Years of schooling (measured at age 25)	-0.002	-0.0001
	(0.017)	(0.021)

Notes: Standard errors clustered at the municipality. Significance levels *** p<0.01, ** p<0.05, * p<0.1.

improvements in parental human capital and resources may translate into a reduction in the probability that the children engage in criminal activities.

Table 12 shows a number of outcomes that are consistent with improved environments and resources for the children. First, fathers' earnings increased. In addition fathers subject to the reform marry women who earn more (about $820 per annum) and who are marginally less likely to be unemployed.[20] Moreover, fertility does not increase and hence these in-

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[20]This can be partly a mechanical effect: if men tend to marry locally and they marry younger women, then these women will have been treated by the reform, increasing their schooling and earning. Nevertheless, this does not abstract from the fact that the family

creased resources do not get diluted. There is some evidence of having children later, although the decline in teenage fatherhood comes from a very low base.[21] Finally both the cognitive skills and the social skills of parents increased as already discussed.

Taken together, the evidence points towards improved home environments and increased parental quality. This is consistent with reductions in criminal activity of the child generation.

If the reduction in crime reflected an underlying increase in general human capital then we would expect this to show in other outcomes, including an increase in educational attainment. However, as can be seen in Table 12, the children of those who went through the reform did not attain higher levels of education relative to those whose fathers did not go through the reform. This result is in general confirmed by those obtained by Holmlund, Lindhal, and Plug (2011). Also, Lundborg, Nilsson, and Rooth (forthcoming) looks at second generation effects of the reform on cognitive, non-cognitive and different health measures obtained at the conscription. They only find significant effects on health measured as physical strength. The improvements in education and resources do not seem to reflect themselves in improved schooling for the next generation, but they certainly affect crime outcomes.

To gain a better understanding of possible underlying mechanism for these results we use quantile regression to estimate the way that the reform affected the intergenerational persistence of education across the distribution.

The intergenerational persistence between the parent and the grandparent generation at the 25th percentile declines to 0.25 as a result of the treatment, compared to 0.52 (with a standard error of 0.008) for the

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resources of treated fathers have increased as a result.

[21]The table shows a 0.263 percentage point decline from a baseline of 1.7% of fathers having a child while being teens.

untreated. It remains unaffected at the 75th percentile. For the child generation persistence is overall much lower. However, we get the opposite effect of treatment on the persistence coefficient: those with treated fathers display more persistence in the 25th percentile (0.25 with a se of 0.0005) than those with untreated fathers (0.19 with a se of 0.003). The persistence at the 75th percentile remains unchanged at 0.46.

These results are consistent with the reform directly raising education at the low end once implemented nationally for the child generation (as it did with the parent generation) but not having a further effect through the increased education of the treated fathers. Our interpretation is that the children of treated fathers are not on the margin of increasing their education by one year or more, which is a lumpy investment. However, these same children may be on the margin of crime and the improvement in the home environments we documented above may have been enough to reduce crime without strong enough improvements in other dimensions.

6   Conclusions

An earlier literature has established that educational reform increasing compulsory schooling improves outcomes in a number of dimensions, including cognitive skills, social skills, earnings and crime. This has been shown for the US and the UK. We first confirm that this is also the case in Sweden for a far reaching reform that increased compulsory schooling and abolished tracking.

The new question we address here is whether exposing fathers to the reform has an impact on the crime rates of the next generation, given that all the children are in any case educated under the new reformed system. The reason we may expect this to happen is because of improved family environments, which may translate to better parenting and greater

availability of resources as indeed is the case.

Our results establish substantial impacts of father's exposure to the reform on the child generation crime rates: it resulted in an overall decline in the crime rate by about 0.8 percentage points, mostly driven by a decline in convictions among the 15-19 year olds. The reductions are mainly concentrated among property crime, traffic crime (serious enough to lead to a court case) and fraud. We are not able to conclusively establish the mechanisms that led to such a reduction. We are, however, able to establish that home environments for children in families where the father was exposed to the reform improved in a number of dimensions. That these improvements led to a reduction in criminality of their children is consistent with both theories of intergenerational transmission of human capital (see e.g. Becker and Tomes, 1979, or Cuhna and Heckman, 2007) as well as sociological theories on the effect of strains (see Merton (1938)) and formation of social capital (see Coleman (1988)).

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7 APPENDIX

7.1 Impact on incarceration

Table 13: Reform Impact on Prison sentences for both generations

Dependent variable: Prison conviction				
	(1)	(2)	(3)	(4)
Sample:	Men born 52-55		Sons of men 45-55	
Education background:	All	Low	All	Low
Impact	-0.488	-0.580	-0.087	0.009
	(0.316)	(0.366)	(0.092)	(0.124)
$\bar{y}$ %	7.507	8.211	2.380	2.682
Obs	176,232	107,557	410,286	261,918
Including trends				
Impact	-0.495	-0.670	-0.015	0.108
	(0.447)	(0.527)	(0.099)	(0.138)
$\bar{y}$ %	7.505	8.206	2.385	2.683
Obs	175,554	107,210	408,021	261,014

Notes: Dependent variable for sons is one if they have ever been convicted to a prison sentence between the ages 15-29.

7.2 Parent estimations including trends

Table 14: Reform effects on years of schooling for the generation directly affected by the reform, including trends

Dependent variable: Own years of schooling			
	(1)	(2)	(3)
Sample:	All	Low educ	High educ
Reform birth	0.216***	0.309***	0.080**
	(0.033)	(0.034)	(0.036)
Obs	437,921	278,074	159,847
mean years	11.61	10.90	12.85

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered by municipality of birth, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of individual, father's education level indicator variables, and municipality group specific trends. Column (1) includes all men born 1945-1955, column (2) restricts the estimation to men of 45-55 cohorts, whose father was low educated, and column (3) restricts the estimation to men from high education backgrounds.

Table 15: Effects of the reform on crimes split up by types of crimes for men of cohorts 52-55

	(1) Any	(2) Violent	(3) Property	(4) Drugs	(5) Traffic	(6) Fraud
Panel A: All education levels, Cohorts 52-55, including education						
Reform birth	-1.292	-0.525	-1.044*	-0.943**	-0.559	-1.139***
	(0.921)	(0.457)	(0.558)	(0.469)	(0.793)	(0.383)
Obs	175,681	175,538	175,679	175,681	175,681	175,681
$\bar{y}$ %	38.62	6.160	9.364	7.223	22.58	7.022
Panel B: Low educated fathers, Cohorts 52-55, Obs: 107,210						
Reform birth	-1.495	-0.825	-0.908	-1.309**	-0.844	-1.269***
	(1.053)	(0.865)	(0.648)	(0.580)	(0.950)	(0.470)
$\bar{y}$ %	40.00	6.949	10.10	7.282	23.65	7.433

Notes: Results are scaled by 100. Robust standard errors in parentheses, clustered by municipality of birth. All regressions include a full set of birth municipality fixed effects and birth cohort indicator variables. In addition all estimations include municipality of birth group specific cohort trends. Dependent variable: ever convicted between 1973-2012 column (1), and split up by type of crimes for columns (2)-(7). Sample are men born between 1952-1955.

7.3 Child estimations including trends

Table 16: Effects of father's reform assignment on son's crime by type of crimes for two reform assignments including trends

Convicted at age 15-29	(1) Any	(2) Violent	(3) Prop	(4) Drugs	(5) Traffic	(6) Fraud
Panel A: All education levels, reform birth, incl. grandfather's educ, Obs 409,083						
Reform father birth	-0.800***	-0.202	-0.015	0.118	-0.434**	-0.166
	(0.276)	(0.134)	(0.187)	(0.128)	(0.192)	(0.104)
$\bar{y}$ %	23.53	4.483	7.733	3.181	10.82	2.880
Panel B: All education levels, reform residence, incl. grandfather's educ, Obs 416,639						
Reform father residence	-0.743***	-0.092	-0.109	-0.064	-0.187	-0.297***
	(0.285)	(0.141)	(0.173)	(0.121)	(0.213)	(0.112)
$\bar{y}$ %	23.60	4.519	7.782	3.205	10.86	2.886
Panel C: Low educated grandfathers, reform birth, Obs 261,014						
Reform father birth	-0.571	0.020	0.046	0.119	-0.448*	-0.168
	(0.356)	(0.181)	(0.240)	(0.169)	(0.266)	(0.131)
$\bar{y}$ %	25.09	4.947	8.405	3.199	11.86	3.113
Panel D: Low educated grandfathers, reform residence, Obs 265,316						
Reform father residence	-0.934**	0.021	-0.074	-0.124	-0.387	-0.311**
	(0.363)	(0.197)	(0.231)	(0.158)	(0.279)	(0.135)
mean in %	25.15	4.978	8.454	3.229	11.91	3.126

Notes: Results are scaled by 100. Robust standard error in parentheses, clustered by father's municipality of birth (Panel A and C) or father's municipality of residence in 1960 (Panel B and D). All regressions include full set of birth municipality of father fixed effects (Panel A and C) or municipality of residence of father in 1960 (Panel B and D) and birth cohort of father fixed effects. In addition all estimations include father's cohort trends that are specific to birth municipality groups of father (Panel A and C) or specific to municipality of residence in 1960 of father (Panel B and D). Sample are sons of fathers born between 1945-1955 and the sons are themselves born before 1994.

Table 17: Age specific effects of father's reform assignment on son's crime including trends and grandfather's education level

	(1)	(2)	(3)	(4)
Convicted at age:	15-29	15-19	20-24	25-29
Cohorts observed:	1960-1993	1960-1993	1960-1988	1960-1983
Panel A: All education levels, including grandfather's education				
Reform father birth	-0.800***	-0.632***	-0.345*	-0.196
	(0.276)	(0.221)	(0.197)	(0.208)
$\bar{y}$ %	23.53	15.69	11.69	7.863
Obs	409,083	409,083	364,521	282,305
Panel B: Low educated grandfather				
Reform father birth	-0.571	-0.481*	-0.244	0.104
	(0.356)	(0.289)	(0.263)	(0.269)
$\bar{y}$ %	25.09	16.81	12.44	8.259
Obs	261,014	261,014	235,478	186,858

Notes: Results are scaled by 100. Robust standard error in parantheses, clustered by father's municipality of birth. All regressions include a full set of father's birth municipality fixed effects and father's birth cohort fixed effects. In addition all estimations include father's cohort trends that are specific to father's birth municipality group. Dependent variable: ever convicted between the ages 15-19, 20-24, 25-29, 30-39 using only the cohorts that are fully observed for those ages. Sample are sons of fathers born between 1945-1955 and the sons themselves belong to the cohorts mentioned for each column.