

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

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Abstract

In this paper we consider the intergenerational effect of education policy on crime. 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.

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.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 (2012) 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 (2013)) following the approach of Björklund, Lindahl, and Plug (2006), focus on parent-child correlations in crime using adoption data, to

¹Examples 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 (2008) develop an empirical model that allows for the evaluation of policies designed to reduce crime allowing for general equilibrium 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 (see Becker (1981)).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) and 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 in this paper is estimating the impact of educational interventions received by the parents on child crime outcomes. Specifically, we show that the Swedish comprehensive school reform, (originally studied by

²For some evidence on the importance of mother's education on child outcomes see for example Carneiro, Meghir, and Parey (2013); Deming (forthcoming) highlights the importance of school quality and its potential impact on crime.

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 in Sweden between 1973 to 2010, including details on the types of crimes committed. 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, by age and by type of crime, while conditioning on the education level of the grandparent generation. Our empirical approach is based on comparing changes in the crime rate across cohorts in municipalities that implemented the reforms at different times.

In a recent paper Hjalmarsson, Holmlund, and Lindquist (2011) followed up on our working paper (Meghir, Palme, and Schnabel (2011)) by extending the observation window for convictions back to 1973 and obtaining data on crime by type; they then focus on using the reform as an instrument for the impact of education on crime for the parent generation only, i.e. for those who were directly affected by the reform - they do not consider intergenerational effects. Their approach requires the strong as-

sumption that all the impact of the reform is mediated by the change in years of education. Below we argue that this assumption is unlikely to hold, given the nature of the reform. Since our original publication we have also extended our data back to 1973 and forward to 2010 obtaining also data on type of crime for *both* generations we consider. The focus of our study is the intergenerational impact of the reform itself and we are also able to investigate heterogeneity of effects with respect to socioeconomic status of the previous (grandparent) generation.³

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 intergenerational 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.

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*). 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.

³We argue in the paper that the reform cannot be used as an instrument for education in either generation and as a result we focus on the direct effect of the reform on both generations.

The junior secondary school was a prerequisite for the subsequent upper secondary school, which, in turn, was required for higher education.

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

The reform is a combination of various components, all of which have been elements of reforms in other countries. For example, England increased compulsory school leaving age in 1973 from 15 to 16 and abolished selection at 11, gradually creating comprehensive schools starting in the mid-sixties.⁵ In the early eighties England also adopted a common curriculum. Thus the effect of these reforms is of general interest in itself and showing an impact on crime, even as a package, can be of broad interest.

If we could disentangle the impact of each component of the reform we could learn more. Unfortunately, with the current design this is not possible as they were all implemented together. And while different groups based on socioeconomic status may be affected differently by the various components, spillover effects will ensure that all are impacted as a result of the different components. For example increasing compulsory schooling and abolishing tracking would change the social mix in schools and dilute the resources available per child. Indeed this is a key reason why the reform cannot be safely used as an instrument for years of education.

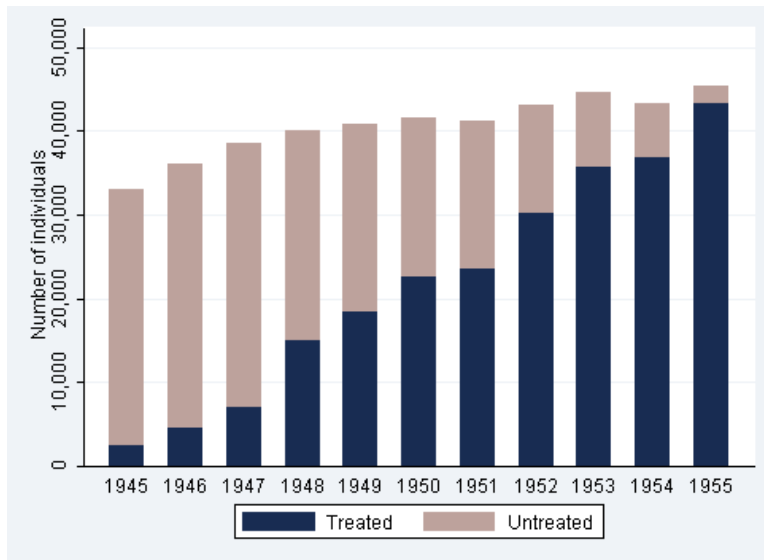
⁴The school reform and its development are described in Meghir and Palme (2003), Meghir and Palme (2005), and Holmlund (2007). For more detailed reference on the reform, see Marklund (1980) and Marklund (1981).

⁵Some parts of England still have selection, e.g. Kent.

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, which in 1952 numbered 1,055 (including 18 city communities).⁶ The selection 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. Given the design of the social experiment our approach will be based on a difference-in-differences estimation strategy.

Figure 1: Number 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 were 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

⁶This 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).

in our sample in each year birth cohort and the proportion of the parent generation assigned to the reform.

As mentioned above, 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. In the empirical analysis that will follow we will be controlling for municipality fixed effects and other characteristics that vary over time to allow for permanent and potentially confounding characteristics that may differ across early and late implementers. Nevertheless, it is interesting to document here how these municipalities 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 1 imply that early implementers were higher income and had a higher local tax rate. The municipality population size had no effect.

Table 1: Timing of implementation and municipality characteristics

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

Notes: Significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The 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.

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, provided by Statistics Sweden.⁷ 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, who attended school during the social experiment described above; their parents labeled as the *grandparent generation*; and their children referred to as the *children generation*.⁸

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 or 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.

Fortunately, we can investigate this by deriving a reliability ratio (see Aigner (1973)); For a subset of the data set - those born the 5th, 15th or 25th each month in 1948 or 1953⁹ we have register-based information on

⁷See Statistics Sweden (2003).

⁸Even 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.

⁹These are included in the so called UGU-data set, collected by the Department of Education, Gothenburg University - see Meghir and Palme, 2005.

which school system they actually were assigned to. For 87.3 percent of this sample we were able to match information on actual reform assignment and the one predicted by the municipality of birth - which is what we use in this study. The discrepancy between the two measures is only 9.9 percent: 5.3 percent moved to a non-reform municipality and 4.6 percent moved in the other direction. This implies that on average our estimates will be attenuated by a factor of 0.901 with respect to correct assignment to the reform.¹⁰

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, drug and trafficking violations, sex crimes and others containing crimes that cannot be categorized as any of the latter six categories. The traffic crimes need to be serious enough to lead to a court case and do not include speeding and parking offenses.¹¹

We select the sample of men born 1945-1955 who were alive in 1973

¹⁰The attenuation coefficient is $Pr(R = 1 | R^B = 1) - Pr(R = 1 | R^B = 0) = 0.947 - 0.046$ where R denotes actual reform assignment and R^B reform assignment based on municipality of birth.

¹¹Types 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.

(when the crime records begin) and who had not migrated out of Sweden permanently. 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. From the education census we also 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 percent 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 percent.

Under the column "Total", Table 2 shows the overall conviction rate for men in the parent generation and below for their sons. We also report the conviction rate by type of crime *conditional* on being convicted in the remaining columns. Since individuals can have multiple convictions these columns add up to more than 100. The conviction rates corresponds to the observation period 1973-2010.

During the entire observation period 1973-2010, 34 percent of men in the parent generation have been convicted at some point of a crime. Of those convicted 58 percent were convicted of a traffic crime (serious enough to lead to a court case), 21 percent of a property crime, 18 percent for fraud (including tax evasion) and 33 percent of "other" unspecified crimes. One in five convictions led to a prison sentence. 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 (2012), Hjalmarsson and Lindquist (2013), and Grönqvist (2011). In Appendix Table 14 we separately report conviction rates for individuals from a lower socio-economic status (SES) background, which in the entire paper refers to those individuals for whom the grandparent generation had just pre-reform statutory level of education. Their conviction rates are slightly

higher. This surprising result might be due to the fact that the low SES group represents 63 percent of the population.

Table 2: Conviction rates by age and types of crimes for all SES

	Percent convicted by crime types conditional on being convicted								
	Total	Violent	Property	Fraud	Traffic	Drugs	Sex	Others	Prison
Panel A: Men in Parent Generation: at least one conviction									
All	33.94	14.97	21.02	18.16	57.96	17.04	1.623	33.23	19.65
ages 20-24	19.11	10.12	25.66	11.82	43.20	17.75	0.409	30.88	16.29
ages 25-29	11.53	10.54	21.89	14.75	39.35	20.57	0.669	28.60	20.82
ages 30-39	12.68	13.93	19.06	19.84	43.84	15.93	1.252	28.85	21.44
ages 40-49	11.21	13.36	13.49	13.62	54.92	9.350	1.523	24.17	
Panel B: Men in Parent Generation: multiple convictions									
All	15.75	25.40	34.79	30.03	71.06	24.98	2.518	49.17	35.54
ages 20-24	6.399	18.66	48.30	24.26	52.82	26.13	0.751	48.04	35.43
ages 25-29	3.620	19.99	42.35	29.79	49.89	29.29	1.097	43.41	43.71
ages 30-39	4.313	26.12	37.74	35.04	56.54	24.55	1.902	43.65	44.26
ages 40-49	3.309	25.00	29.65	24.56	64.23	18.84	2.175	38.65	
Panel C: Sons in Child Generation: at least one conviction									
All 15-29	23.69	19.23	33.07	12.28	46.03	13.63	0.881	31.18	10.26
ages 15-19	15.83	15.95	39.81	11.40	35.51	7.527	0.518	24.68	2.726
ages 20-24	11.80	17.71	20.59	8.303	41.44	15.48	0.736	30.02	15.30
ages 25-29	7.948	16.27	14.29	8.627	48.89	18.88	0.978	24.73	17.17
Panel D: Sons in Child Generation: multiple convictions									
All 15-29	7.456	36.48	57.48	23.06	58.86	27.26	1.264	50.55	24.46
ages 15-19	5.107	30.04	62.42	18.65	45.55	12.84	0.776	39.42	6.943
ages 20-24	3.636	29.90	40.59	16.63	49.04	30.55	0.825	43.73	33.75
ages 25-29	2.387	25	32.70	16.98	56.78	38.89	1.109	37.28	37.98

Notes: Table shows overall conviction rates (Total) and type of crimes rates conditional on having been convicted at least once or twice. The sample are men with all SES. For the age specific conviction rates only men who are fully observed for the relevant age bracket are included. For Panel A and B: All includes the whole sample of men born 45-55 (N=447,382) and the conviction rates refers to having ever been convicted or having been convicted at least twice between 1973-2010. Age specific conviction rates for men in Parent Generation includes: for ages 20-24 cohorts 53-55 (N=133,200), for ages 25-29 cohorts 48-55 (N=339,888), and for ages 30-39, and 40-49 the whole sample of cohorts 45-55 (N=447,382). For Panel C and D: All 15-29 includes the whole sample of sons born in or before 1993 (N=426,721), and the conviction rates refers to having ever been convicted or having been convicted at least twice between the age 15-29. Sons' conviction rates by age includes: for ages 15-19 the whole sample of sons (N=426,721), for ages 20-24 cohorts born in or before 1988 (N=380,249), and for ages 25-29 cohorts born in or before 1983 (N=294,749).

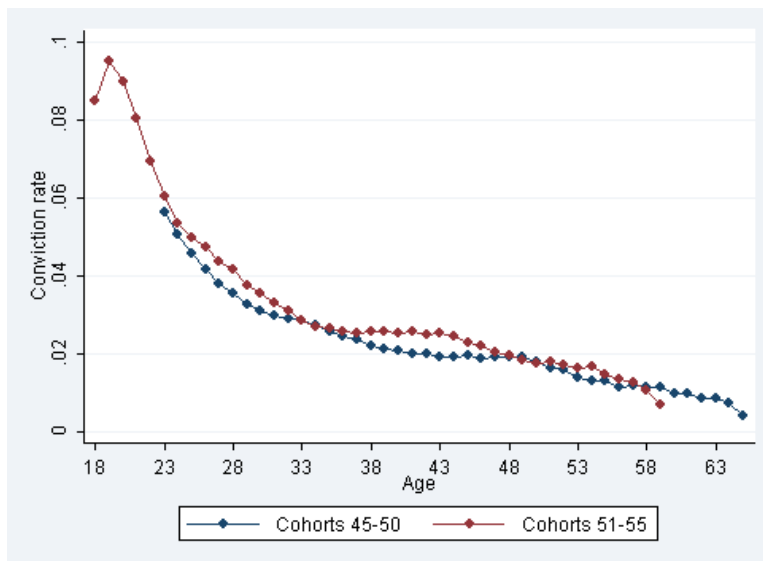
The crime rate for the child generation is reported at the lower panel of the table: about 24 percent had a conviction in our observation age window of 15-29. Conditional on a conviction there are substantial numbers convicted of violent and property crimes as well as traffic crimes.

The Table also reports conviction rates by age. Comparing the crime rates at the same ages with that of the parent generation we see a very large decline in crime across the generations. For both generations most offenses

are committed by the younger individuals. This is best seen in Figure 2.1 for the parent generation and Figure 2.2 for the child generation. Both show a pronounced decline of crime with age. Interestingly, there are very strong cohort effects on crime for the child generation, showing a marked decline over time; no similar decline is apparent among the two broad cohorts we show for the parents.

Figure 2: Crime age profiles

2.1: Age profile of crime rate for the Parent Generation by cohort



2.2: Age profile of crime rate for the Child Generation by cohort

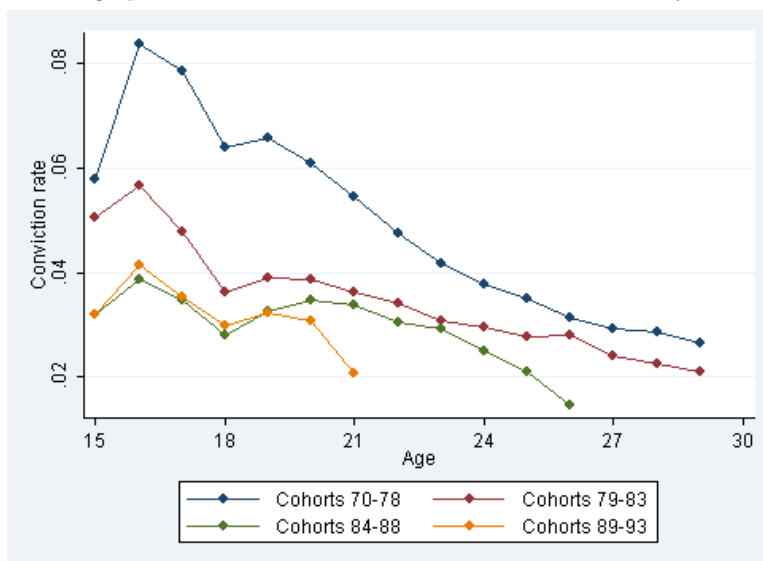


Table 2 also provides information on more serious criminal activity, namely convictions leading to prison and multiple (2+) convictions, as well as the percent convicted of each type of crime conditional on having 2+ convictions. The distribution by type of crime involves more violent, property and fraud crime, although traffic related crimes are also prevalent. About 20 percent of those convicted in the parent generation were incarcerated. In the child generation this number is 10 percent (over the 15-29 age range). Moreover, when we consider multiple convictions the prevalence of violent, fraud and property crimes increases.

In Appendix Table 16 we report the crime rates for women. These are almost a quarter of the male rates. As with men there is no difference in the crime rates when we focus on the low SES background. The reform has no discernible effect on these rates and we present results in the Appendix for completeness. However, we do examine whether exposing the mother to the reform affects the crime rate of sons.

Table 3: 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*** (0.114)	-0.998*** (0.059)	-4.246*** (0.036)	-0.754*** (0.020)
Father's Schooling			-0.218*** (0.033)	-0.038*** (0.012)
Mother's Schooling			-0.087** (0.040)	-0.029*** (0.010)
Obs	444,272	444,272	273,093	273,093
\bar{y} %	33.88	6.597	24.48	2.371

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Effects scaled by 100. Dependent variables for men born 45-55: indicator variables for having ever been convicted or having received a prison sentence between 1973-2010. Dependent variables for sons: indicator variables for having ever been convicted or having received a prison sentence between the ages 15-29. Robust standard errors in parentheses clustered by own birth municipality or by father's birth municipality. Includes own birth cohort and birth municipality indicator variables, or father's cohort and father's birth municipality indicator variables.

3.1 Parental Background, Education and Crime

Table 3 shows the results from regressing conviction (columns 1 and 3) and incarceration (cols 2 and 4) on father’s and mother’s education based on a Linear Probability Model. All regressions include dummies for the municipality of birth 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 percent reduction in conviction rates. For the child generation (and including parental education) the coefficient on own education increases substantially for conviction, but for incarceration. Both father’s and mother’s education are significant but the impact of the former is larger.

Table 4: Association between son’s and father’s crime

	(1)	(2)	(3)	(4)
	All SES		Low SES	
	Convict	Prison	Convict	Prison
Panel A: Fathers born 45-55 and their sons born before 1994				
Father convict/prison	12.503*** (0.187)	6.205*** (0.168)	12.895*** (0.215)	6.699*** (0.235)
Obs	410,475	410,475	261,918	261,918
\bar{y} %	23.54	2.380	25.09	2.682
Panel B: Fathers born 53-55 (ages 20-29), sons born before 1982 (ages 20-29)				
Father convict/prison at ages 20-29	12.000*** (0.627)	9.073*** (0.772)	12.457*** (0.635)	10.086*** (0.998)
Obs	37,006	37,006	24,956	24,956
\bar{y} %	18.42	3.437	18.94	3.614

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Effects scaled by 100. The dependent variables are indicator variables for sons having been convicted or sentenced to prison between the ages 15-29 in Panel A, and between the ages 20-29 in Panel B. Robust standard errors in parentheses clustered by father’s birth municipality. Includes father’s cohort, sons’ cohort and father’s birth municipality indicator variables.

Finally, Table 4 illustrates the intergenerational associations of crime. The probability of ever being convicted increases by over 12 percentage points if the father has been convicted. The father having been jailed is

associated with a 6 percentage point increase in the probability that the son will go to prison. These associations do not change much when we take just low SES individuals.

In interpreting these results there is an issue with the alignment of ages across generations as pointed out by Haider and Solon (2006) in the context of intergenerational mobility of income. So in Panel B we present the intergenerational association for ages 20-29 for both generations, controlling for cohort effects for both. For the conviction rates the intergenerational associations are essentially the same when we align ages in this way. However for prison they increase by 50 percent. The inescapable conclusion however from these results is that there is a very strong intergenerational association of crime and incarceration.

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 and by type of crime; and whether an individual was convicted at certain ages: 15-19 for the children generation only, 20-24 and 25-29 for both generations and 30-39 for the parent generation only. We present results for the whole sample and separately for those with a low SES background.

Our outcome variable is based on convictions and incarcerations, rather than offending as such. By the administrative nature of our data this is what we observe. The interpretation of our results presumes that the impact on convictions reflects a real reduction in crime and not simply an improved ability by criminals to evade arrest and convictions. So a key assumption in this approach is that the reform did not affect the relationship between offending and convictions.

The crime records start in 1973 and the gradual transition to the new

system covers the cohorts born between 1945-1955. As documented above, most crimes are committed by young people and it is this age group that is most likely to be affected by the reform. Thus, for the parent generation we estimate impacts on crime for the 1952-55 cohort who were 18-21 years old in 1973 when our records begin. They are followed up until 2010 when they are 55-58.

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 market areas and their fathers belong to all the cohorts 1945-55 of the transitional period.

The crime rate of females is very low - about a quarter of the male one, and was not affected by the reform; so we include the results for women in an Appendix for completeness but do not discuss the results at depth. We also explore the impact of exposing females to the reform on the crime rate of their sons.

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 11 cohorts in our window and all 1000 or so 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 indicating in which municipality individual i was born. $\epsilon_{i,m,t}$ is conditionally independent of $R_{i,m,t}$. The general assumptions underlying the method of Difference in Differences whether unconditional or conditional on explanatory variables (like here) are discussed in Heckman and Robb (1985), Heckman, Ichimura, Smith, and Todd (1999) and Athey and Imbens (2006).

Based on the latent equation above we use the linear probability model (LPM), 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. To check whether using an LPM biases the results we ran a Monte Carlo experiment replicating the crime rates across municipalities and imposing an average effect of the reform equal to what we obtain. Assuming the data was generated by a normal probability model (probit) and then using an LPM only biased the results upwards by 5 percent with respect to the true average effect - a difference that is statistically indistinguishable in our data.

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. All standard errors are clustered at the municipality level.

5.1.1 Education

Table 5 shows the estimates of the effects of the education reform on years of schooling for the parent generation.¹² The results are presented for all men and all women born between 1945 and 1955, as well as separately by SES background.

The reform significantly increased years of schooling of both men and women and substantially more so for the former. The overall effect is larger for the low SES group. We also find a small but significant effect (at the 10 percent level) on the men with higher educated fathers. We find no effect on those women from a higher SES background.¹³

In our analysis of crime that follows we show results for the whole sample and for the low SES group separately. We do not show results for the high SES group separately because there is insufficient precision to draw clear conclusions.

¹²Years of schooling are inferred from the level of schooling attainment obtained from the registers.

¹³In Appendix Table 21 we also show results with municipality specific trends - this does not lead to any large or significant changes. Comparing with Meghir and Palme (2005) the effects are overall slightly different. However, these estimates relate to a larger group of cohorts - not 1948 and 1953, and the results might be attenuated by a factor of 0.9 because we use municipality of birth instead of actual reform assignment.

Table 5: Reform effects on years of schooling for parent generation

Dependent variable: Own years of schooling			
	(1)	(2)	(3)
Sample:	All SES	Low SES	High SES
Panel A: Men born 45-55			
Reform	0.174*** (0.038)	0.267*** (0.038)	0.052* (0.030)
Obs	444,272	282,080	162,192
mean years of schooling	11.62	10.91	12.85
Panel B: Women born 45-55			
Reform	0.108*** (0.033)	0.161*** (0.029)	0.051 (0.032)
Obs	423,781	268,567	155,214
mean years of schooling	11.75	11.14	12.80

Notes: Significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is number of years of own schooling. 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 father's education levels.

5.1.2 Crime

While our focus is on crime in the child generation, we first report results for the parents, providing a link with the existing literature and establishing that the reform did indeed have a direct effect on crime.¹⁴

In Table 6 we present the effects of crime on the parent generation for cohorts 1952-55, who are aged 18-21 at the start of our observation year in 1973. Descriptive statistics for these cohorts are presented in Appendix Table 15. The crimes we observe are always after the end of statutory schooling and hence the effects do not include the more mechanical effect of keeping children off the streets. The overall effect of the reform is to reduce by 1.46 percentage points the probability of a conviction over the entire observation period - the effect is highly significant. When we keep only the low SES individuals the impact increases to 1.69 percentage points

¹⁴See Lochner and Moretti (2004).

Table 6: Reform effects on crime by types of crimes for parent generation

	(1)	(2)	(3)	(4)	(5)	(6)
	Any	Violent	Property	Drugs	Traffic	Fraud
Panel A: Men born 52-55, All SES, Obs: 176,232						
Reform	-1.464*** (0.556)	-0.364 (0.297)	-0.783** (0.347)	-0.427 (0.294)	-1.391*** (0.473)	-0.334 (0.269)
\bar{y} %	38.62	6.156	9.366	7.233	22.59	7.024
Panel B: Men born 52-55, Low SES, Obs: 107,557						
Reform	-1.693** (0.680)	-0.344 (0.357)	-0.757* (0.421)	-0.409 (0.361)	-1.899*** (0.604)	-0.490 (0.355)
\bar{y} %	40.00	6.946	10.09	7.291	23.66	7.438

Notes: Significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between 1973-2010 for any crime in column (1), or one of the crime types specified in columns (2)-(6). 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. Panel A also includes father's education levels.

but the difference is not significant.

Table 7: Age specific reform effects on crime for the parent generation

	(1)	(2)	(3)
Convicted at age:	30-39	25-29	20-24
Panel A: Men born 52-55, All SES, Obs. 176,232			
Reform	-0.347 (0.392)	-0.890** (0.386)	-0.988** (0.448)
\bar{y}^c %	12.38	11.61	18.65
Panel B: Men born 52-55, Low SES, Obs. 107,557			
Reform	-0.508 (0.475)	-0.657 (0.485)	-1.045* (0.565)
\bar{y}^c %	13.19	12.28	19.79

Notes: Significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 30-39, 25-29, and 20-24. The sample are men from the parent generation born 52-55. Robust standard errors, clustered by birth municipality, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of the individual. Panel A also includes father's education levels.

In the remaining columns we split up the effect by type of crime committed. Here it becomes clear that the impact is driven by property crime, which decline by .78 percentage points and traffic crimes, which decline by

1.39 percentage points. Note that the impacts by type will typically add up to more than the total effect, because many individuals commit more than one type of offense.

In the descriptive statistics it became obvious that younger people have much higher crime rates; it is thus reasonable to expect the impact of the reform to be concentrated at younger ages. Indeed this is the case as we show in Table 7: the effect for the 20-24 age group is -0.988, declines slightly for the 25-29 age group and becomes much smaller and insignificant for the 30-39 year olds of the same cohort (although the estimates are not significantly different from each other).

We also estimated the effect of the reform on women. We found no discernible effects. The complete set of estimates are presented in Appendix Tables 17 and 18.

It is interesting to estimate the effect of the reform on more serious criminal activity, since this will be associated with larger social costs. To get at this we consider impacts on recidivism, i.e. on the probability of having two or more (2+) separate convictions as well as convictions that lead to a prison sentence. Table 8 shows that the impact of the reform on having 2+ convictions is very strong, reducing them by 1.48 percentage points overall and by 1.77 percentage points for the low SES group. When we consider the effects by age (in the next three columns) we get a strong effect for the youngest group, which is higher for the low SES individuals. We also seem to get a marginally significant effect for the older 30-39 age group.

Only 30 percent of convictions end up in prison sentences for the parent generation (incarceration rate 7.5 percent). At the tails of the distribution the linear probability model may not be a very good approximation. So we estimated the effects in two different ways; first we use the LPM. Second we

Table 8: Reform effects on recidivism and incarceration for parent generation

Dependent variables:	Multiple convictions/Recidivism				Incarceration
	(1)	(2)	(3)	(4)	(5)
Convicted at age:	All	30-39	25-29	20-24	All
<hr/>					
Panel A: Men born 52-55, All SES, Obs. 176,232					
Reform	-1.476*** (0.491)	-0.422* (0.227)	-0.344 (0.233)	-0.896*** (0.290)	-0.488 (0.316)
$\bar{y}\%$	18.88	4.422	3.824	6.206	7.507
<hr/>					
Panel B: Men born 52-55, Low SES, Obs. 107,557					
Reform	-1.767*** (0.566)	-0.409 (0.293)	-0.284 (0.283)	-1.053*** (0.339)	-0.580 (0.366)
$\bar{y}\%$	20.07	4.849	4.192	6.889	8.211

Notes: Significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Results are scaled by 100. The dependent variables are indicator variables for having been convicted at least twice between 1973-2010 (column (1)) or between the ages of 30-39, 25-29, and 20-24 (columns (2)-(4)), or an indicator variable for having ever been convicted for a prison sentence between 1973-2010 in column (5). The sample are men from the parent generation born 52-55. Robust standard errors, clustered by birth municipality, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of the individual. Panel A includes father's education levels.

use a probit, using a reduced set of fixed effects: we group the municipalities by the first cohort for which they implemented the reform and define a fixed effect for each of these groups. This probit gives almost identical results to the LPM and hence we report results from the latter.¹⁵

The LPM results are presented in Table 8. 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 percent level.

Overall, 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 Vujić (2011)). We now move on to the impact on the child generation, which is the focus of this work.

¹⁵For example for the 52-55 cohort of the adult generation the impact with the probit is -0.497 (se 0.329), while with the LPM and a full set of municipality fixed effects we get -0.488 (se 0.316).

5.2 The Reform and Crime in the Child Generation

The reform can only have an effect through the parents having been affected because at this time all children were attending the new school system. For these estimations we take all the sons of fathers born between 1945-55, some of whom will have been exposed to the reform and others not. This is a broader group than the one we used to estimate the impacts on the parent generation: while we do not observe the older cohorts at a young enough age to estimate impacts of the reform on their crime rates we can certainly use their exposure to the reform to measure the impact on children.

Table 9: Age specific reform effects on son's crime

	(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 SES				
Reform father	-0.779*** (0.257)	-0.589*** (0.210)	-0.314 (0.203)	-0.107 (0.186)
Obs	410,476	410,476	365,782	283,297
\bar{y} %	23.54	15.70	11.69	7.861
Panel B: Low SES				
Reform father	-0.667** (0.326)	-0.567** (0.267)	-0.196 (0.249)	0.210 (0.238)
Obs	261,918	261,918	236,289	187,515
\bar{y} %	25.09	16.81	12.44	8.255

Notes: Significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 15-29, 15-19, 20-24 or 25-29. The sample are sons of men born 45-55 who are fully observed for the relevant age bracket of the dependent variable. Robust standard errors, clustered by father's municipality of birth, in parentheses; all regressions include a full set of father's birth municipality and father's birth cohort indicator variables. Panel A includes grandfather's education levels.

In Table 9 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.

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 10: Reform effects on son's crime by type of crimes

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

Notes: Significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 15-29 for one of the specified crime types. Robust standard errors, clustered by father's municipality of birth, in parentheses; all regressions include a full set of father's birth municipality and father's birth cohort indicator variables. Panel A includes grandfather's education levels.

Table 10 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 0.24-0.45pp. Interestingly, property and drugs crime seem unaffected with the estimates being effectively zero. Focussing on the low SES group does not change these conclusions.

The reform also increased schooling for women, albeit a bit less. However, as we show in Appendix Tables 19 and 20 there is no effect of exposing

Table 11: Reform effects on son's recidivism and incarceration

Dependent variables:	Multiple convictions/Recidivism				Incarceration
	(1)	(2)	(3)	(4)	(5)
Convicted at age:	15-29	15-19	20-24	25-29	15-29
Cohorts observed:	1960-1993	1960-1993	1960-1988	1960-1983	1960-1993
Panel A: All SES					
Reform father	-0.267 (0.172)	-0.183 (0.129)	-0.134 (0.120)	-0.154 (0.108)	-0.087 (0.092)
Obs	410,476	410,476	365,782	283,297	410,286
\bar{y} %	7.343	5.033	3.570	2.337	2.380
Panel B: Low SES					
Reform father	-0.166 (0.225)	-0.186 (0.172)	-0.056 (0.148)	0.001 (0.163)	0.009 (0.124)
Obs	261,918	261,918	236,289	187,515	261,918
\bar{y} %	8.077	5.542	3.924	2.505	2.682

Notes: Significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Results are scaled by 100. The dependent variables are indicator variables for having been convicted at least twice between the ages of 15-29, 15-19, 20-24 or 25-29, or an indicator variable for having ever been convicted for a prison sentence between the ages 15-29. The sample are sons of men born 45-55 who are fully observed for the relevant age bracket of the dependent variable. Robust standard errors, clustered by father's municipality of birth, in parentheses; all regressions include a full set of father's birth municipality and father's birth cohort indicator variables. Panel A includes grandfather's education levels.

the mother to the reform on the criminal activity of male children.

We now turn to the impacts on recidivism and incarceration. For recidivism, Table 11 shows the effects are small and overall they are significant only at the 12 percent level. For the child generation the incarceration rate for the 15-29 age group is 2.4 percent over the age window we observe. We again experimented with a probit using the fixed effects based on the first cohort for which the reform was implemented in the municipality; as for the parents this probit gave identical results to the LPM, which we report here. The results are presented in the last column of Table 11 and show that the coefficients are small and not significantly different from zero, although they are quite precisely estimated. Both recidivism and incarceration are very low in the child generation and perhaps it is not surprising that we do not observe any effects, since these may be really the hardened offenders.

5.3 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 three 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; third we plot residuals to show that they do not display a trend. For these tests we group municipalities by the earliest cohort for which they implemented the reform and we look for omitted trends specific to each of these groups.

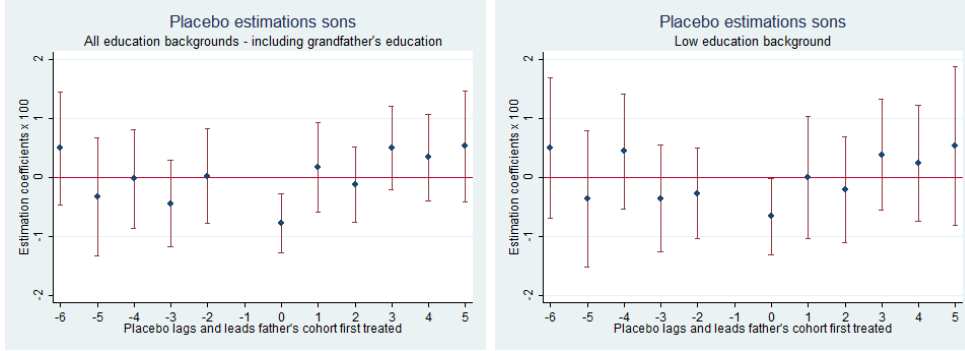
For the placebo estimations, where we pretend that the reform was implemented later, we only use the sample of sons whose fathers were treated by the reform. To construct placebo treatment and control groups we then pretend that the reform was implemented successively one year later, two years, three 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. This provides five placebo estimates.

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. This provides an additional

¹⁶We 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.

five placebo estimates.

Figure 3: Placebo estimations sons



The results are all brought together in Figure 3. Each dot represents the estimate assuming the reform took place at the specified period on the x-axis (relative to when it actually took place, which is the zero point). 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 shows up implying there is nothing systematic taking place biasing the results towards an effect on crime.

For our second approach, Table 12 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 from lower SES backgrounds are however marginally significant (p-value 4.4 percent). 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 SES 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.

For completeness the parameter estimates obtained when these trends are included are shown in the Appendix in Tables 21 - 26. For the parent generation, there is loss in precision when the municipality specific trends are added. Some of the effects become larger and even significant when they were not before (e.g. the impact on drugs crime and the impact on crime between ages 25-29). Nevertheless, as shown, the differences are not significant and the overall conclusions do not change. 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 relevant parental cohorts (1945-55) and the sample size is much larger.

Table 12: Trends tests for both generations

	(1)	(2)	(3)	(4)
	52-55 cohort		Children generation	
	All SES	Low SES	All SES	Low SES
Test 1: joint test of trends=0				
F statistic	0.804	0.803	1.235	1.665
P-value	0.690	0.691	0.230	0.0436
Test 2: test of reform parameter across models				
chi2 statistic	0.066	0.062	0.0338	0.320
P-value	0.797	0.804	0.854	0.571

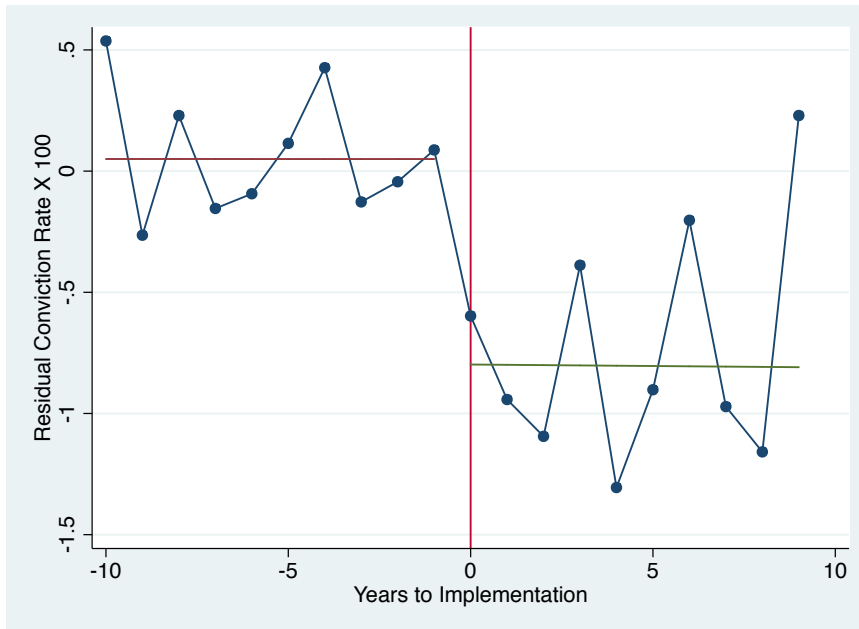
Notes: Test 1 jointly tests the hypothesis that trends are common across municipalities. Test 2 tests the hypothesis that the impacts are the same when comparing the specification with and without trends.

Turning now to a graphical representation, in Figure 4 we plot the residuals from the difference-in-differences regressions (with the estimated average impact added back in) grouped by years to implementation. Each point corresponds to an average residual across cohorts in different municipalities grouped by their years to implementation.¹⁷ If there are systematic

¹⁷For example if municipality 1 implemented the reform for the 1948 cohort, this cohort would contribute to the zero point on the graph, the 1947 cohort contributes

trends related to early or late implementing municipalities these would show up as a trend in these residuals because the composition of municipalities changes as we move along the x-axis to different times to implementation. However these residuals display no trend: 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 4: Trend graphs



5.4 Municipality Characteristics and exogeneity of the reform

The fixed effects approach controls for permanent confounding differences in the municipalities that implemented the reform for different cohorts.

to -1 and so on. Going forward 1949 would contribute to +1, 1950 to +2 etc. This is repeated for all municipalities by time to implementation. The residuals are then averaged by this time to implementation because presenting these trends one by one is too noisy to be visually informative.

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. Indeed this variable is significant and when we include it the overall estimated impact of the reform on crime increases (from -0.5 to -0.78). The various tests of differential trends should have power against other omitted time varying characteristics. The fact that these tests have supported the robustness of our results corroborates our assumption that the reform can be taken to be conditionally exogenous.

5.5 Discussion

The Swedish educational reform reduced the crime rates of men of both the direct subjects of the reform (the parent generation) as well as that of their sons. 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. The reform also had a large impact on repeat crime for men. The crime rate of women, which was already much lower was not affected.

For the child generation the impacts are driven by declines in fraud (including tax evasion) as well as violent crime and traffic offenses. Thus the impact relates both to crimes with a clear economic motivation (fraud) as well as to crimes relating more to anti-social behavior (violent, traffic). These results relate to the case where the father was exposed to the reform. When instead we consider the impact of treating the mother we find no impact on the child generation, despite the fact that the years of education increased for women as well.

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. In Table 13 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 men of 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. Becker (1981), 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 (2013) 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 shown in the Table, the reform translated to higher earnings for the parent generation.¹⁸ This in itself increases the opportunity cost of crime. Meghir, Palme, and Simeonova (2013) also report an increase in the armed forces social skills indicator of 17 percent of a standard deviation (se 0.077) as a result of this reform;¹⁹ interestingly this increase in social skills is driven mainly by those from a higher SES background for whom the social skills indicator increased by 0.53 of a standard deviation (se 0.198). This demonstrates that the reform affected all groups. We know from Heckman, Stixrud, and Urzua (2006) that an improvement in cognitive and social skills reduces antisocial and risky behavior and improves earnings. The reduction in criminality we observe here is consistent with this.

A decline in crime and an improvement in the cognitive and social skills

¹⁸This is consistent with Meghir and Palme (2005).

¹⁹The test is administered to army conscripts. Military service was compulsory in Sweden at that time

Table 13: Impact of the Reform on further outcome variables in the parent and child generation

	(1)	(2)
	All SES	Low SES
Panel A: Men born 45-55		
Log annual earnings \times 100	6.4** (3.0)	6.9*** (2.4)
Ever had a child \times 100	-0.093 (0.185)	-0.096 (0.273)
Number of children	-0.004 (0.007)	0.001 (0.010)
Age at birth first child	0.106 (0.075)	0.064 (0.048)
Child born while a teen \times 100	-0.263** (0.106)	-0.210** (0.100)
Spouse education	0.0499 (0.061)	0.0274 (0.0274)
Spouse annual earnings in SEK	5,462** (2,672)	4,829 (3,361)
Spouse unemployed	-0.003*** (0.001)	-0.0006 (0.001)
Panel B: Sons of men born 45-55		
Years of schooling (measured at age 25)	-0.002 (0.017)	-0.0001 (0.021)
Disposable income (measured at age 25) \times 100	0.084 (0.401)	-0.472 (0.503)

Notes: Significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Each row corresponds to a separate specification, estimating the reform impacts on the dependent variables specified. We use the level of spousal earnings instead of the log to accommodate zeros when the spouse is not working. Disposable income is defined as the sum of incomes from capital, labor, income security programs and allowances minus all income taxes. Robust standard errors clustered at the birth municipality of individual (Panel A) or birth municipality of father (Panel B). All estimations include birth cohort and birth municipality indicator variables (Panel A), or father's birth cohort and father's birth municipality indicator variables (Panel B).

in the parent generation can induce directly a reduction in crime in the child generation through better parenting practices. Moreover, there was an increase in parental resources both because fathers' earnings increased and because fathers exposed to the reform had children with women who earn

more (about \$820 per annum measured in 2004) and who are marginally less likely to be unemployed.²⁰ Taken together and assuming that child investments are driven by life-time income these results imply an increase in resources available for child investments. Moreover, fertility does not increase and hence these increased resources do not get diluted. Having children as a teen also declined but from a very low base: the table shows a 0.263 percentage point decline from a baseline of 1.7 percent of fathers having a child while being teens.

Taken together, the evidence points towards increased resources at the home and improved parental quality, which should lead to better upbringing for the children. This is consistent with reductions in criminal activity of the child generation.

A possible puzzle in these results is that the reform does not lead to improvements in other outcomes in the child generation: as can be seen in Table 13, the children of those who went through the reform did not attain higher levels of education relative to those with untreated fathers. Moreover, the effect on earnings at 25 is also zero. The educational result is in general confirmed by those obtained by Holmlund, Lindhal, and Plug (2011).²¹

In interpreting these results it is important to note that low ability individuals are likely to be constrained by the compulsory schooling laws and may not be willing to invest one whole extra year of education. Indeed,

²⁰For this impact we use levels since some women have zero earnings. We do not condition on whether the couple is married or not; we just use information on who is the mother of the child. It is interesting to note that we have not found any direct evidence that female earnings increased as a result of the reform. So this result indicates an improvement in matches for men treated by the reform and not just a mechanical effect that men are having children with younger women in the locality and who are treated as a result.

²¹Lundborg, Nilsson, and Rooth (2012) use an IV strategy to estimate the effect of years of schooling on a number of outcomes for the child generation. They do find a number of significant effects, particularly of mother's schooling. However, their results depend on the validity of using the reform as an exclusion restriction. They do not report the reduced form effect of the reform other than on schooling.

when we estimated a simple quantile regression for the 25th percentile of schooling we found that the intergenerational persistence between the child and the parent generation was significantly higher when the father had gone through the reform. This reflects the fact that the children were constrained by the compulsory schooling laws. At the 75th percentile there was no effect of the reform on intergenerational persistence. Still we could observe an effect on earnings. However, the negotiated minimum wages prevalent in Sweden may be masking any subtle improvements at the bottom of the distribution.²²

Thus despite the lack of effects in these other dimensions, human capital may have increased sufficiently at the lower part of the distribution to induce a reduction in crime. In addition, the propensity to commit crimes does not only depend on human capital, but also relates broadly to the psychic costs of crime, such as moral values and other personal preferences, including attitudes to risk. These characteristics are hard to measure directly, but are likely to be affected by home environments and parental resources.

6 Conclusions

This paper considers the intergenerational effects of educational policy on crime. We first confirm that a Swedish educational reform of the 1950s which increased compulsory schooling and abolished tracking reduced crime substantially. This is consistent with results found for the US and the UK.

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.

²²We do not have at our disposal measures of cognition for this cohort yet although we hope to obtain them.

The reason we may expect this to happen is because of improved parental human capital, 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 violent crime, traffic crime (serious enough to lead to a court case) and fraud - including tax evasion. 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 Cunha 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 Descriptive Statistics

Table 14: Conviction rates by age and type of crimes conditional on being convicted for low SES

	Percent convicted by crime types conditional on being convicted								
	Total	Violent	Property	Fraud	Traffic	Drugs	Sex	Others	Prison
Panel A: Men of Parent Generation, low SES: at least one conviction									
All	34.73	16.19	21.63	18.36	58.59	16.68	1.765	33.71	20.56
ages 20-24	20.32	11.28	26.40	12.22	44.32	16.85	0.408	31.07	17.23
ages 25-29	12.09	11.55	22.28	15.11	40.93	19.85	0.769	27.88	21.32
ages 30-39	13.27	14.94	19.57	19.68	44.97	15.06	1.362	28.87	22.44
ages 40-49	11.51	14.37	14.31	13.85	53.72	9.343	1.623	25.05	
Panel B: Men of Parent Generation, low SES: multiple convictions									
All	16.44	27.22	35.71	30.28	71.64	23.99	2.738	49.78	36.90
ages 20-24	7.143	20.00	48.98	24.66	54.49	24.28	0.693	47.97	36.80
ages 25-29	3.929	21.84	43.04	30.09	51.29	27.76	1.243	42.80	45.02
ages 30-39	4.653	27.50	38.52	34.47	57.46	22.95	2.029	43.87	45.57
ages 40-49	3.495	26.47	30.69	24.83	63.60	18.73	2.228	39.66	0
Panel C: Sons of Child Generation, low SES: at least one conviction									
All 15-29	25.23	19.90	33.74	12.48	47.32	12.91	0.918	31.37	10.86
ages 15-19	16.95	16.29	40.40	11.34	36.90	6.993	0.562	24.34	2.907
ages 20-24	12.54	18.31	21.22	8.607	42.05	14.50	0.747	30.26	16.15
ages 25-29	8.344	17.06	14.96	8.875	48.42	18.33	0.996	25.27	17.58
Panel D: Sons of Child Generation, low SES: multiple convictions									
All 15-29	8.203	37.16	58.13	23.57	60.01	25.99	1.358	50.63	25.20
ages 15-19	5.625	30.21	63.11	18.84	46.85	12.18	0.837	38.90	7.275
ages 20-24	4.002	30.29	41.21	17.11	49.69	28.72	0.937	43.94	34.26
ages 25-29	2.556	25.96	33.76	17.63	56.35	38.03	1.145	37.87	38.67

Notes: Table shows overall conviction rates (Total) and type of crimes rates conditional on having been convicted at least once or twice. The sample are men with low SES, and for the age specific conviction rates only low SES men who are fully observed for the relevant age bracket are included. Panel A and B: All includes the sample low SES men born 45-55 (N=283,841) and the conviction rates refers to having ever been convicted or having been convicted at least twice between 1973-2010. Age specific conviction rates for men in Parent Generation includes: for ages 20-24 cohorts 53-55 (N=80,835), for ages 25-29 cohorts 48-55 (N=212,906), and for ages 30-39, and 40-49 the low SES sample of cohorts 45-55 (N=283,841). For Panel C and C: All 15-29 includes the low SES sample of sons born in or before 1993 (N=271,971), and the conviction rates refers to having ever been convicted or having been convicted at least twice between the age 15-29. Sons' conviction rates by age includes low SES sons: for ages 15-19 the whole low SES sample of sons (N=271,971), for ages 20-24 cohorts born in or before 1988 (N=245,342), and for ages 25-29 cohorts born in or before 1983 (N=194,854).

Table 15: Conviction rates by age and types of crimes for Men born 52-55

	Percent convicted by crime types conditional on being convicted								
	Total	Violent	Property	Fraud	Traffic	Drugs	Sex	Others	Prison
Panel A: Men born 52-55, all SES, Obs 176,232: at least one conviction									
All	38.62	15.94	24.25	18.19	58.49	18.73	1.465	35.36	19.44
ages 20-24	18.65	10.00	25.17	11.77	42.83	17.41	0.432	31.05	16.42
ages 25-29	11.61	11.31	22.87	16.22	38.24	21.35	0.660	28.97	21.32
ages 30-39	12.38	15.83	20.19	19.52	44.28	16.01	1.311	30.17	22.65
ages 40-49	11.30	14.27	13.13	12.50	56.54	9.996	1.486	24.85	
Panel B: Men born 52-55, all SES, Obs 176,232: multiple convictions									
All	18.88	26.63	38.94	29.72	71.11	27.90	2.278	51.59	34.66
ages 20-24	6.206	18.48	47.47	23.96	51.83	25.90	0.713	47.81	35.51
ages 25-29	3.824	20.92	43.97	31.38	47.97	31.35	0.920	43.78	44.84
ages 30-39	4.422	28.11	40.19	34.09	56.23	28.06	2.027	43.77	45.84
ages 40-49	3.528	25.24	28.95	21.97	64.95	21.83	1.914	40.16	
Panel C: Men born 52-55 low SES, Obs. 107,557: at least one conviction									
All	40.00	17.36	25.23	18.59	59.14	18.23	1.562	36.30	20.53
ages 20-24	19.79	11.05	25.89	12.20	44.11	16.39	0.456	31.04	17.12
ages 25-29	12.28	12.70	23.29	16.77	39.31	20.55	0.750	29.04	22.46
ages 30-39	13.19	16.92	21.09	19.49	45.00	15.12	1.417	30.42	23.67
ages 40-49	11.56	15.27	14.27	12.86	54.63	10.31	1.496	26.32	
Panel D: Men born 52-55, low SES, Obs. 107,557: multiple convictions									
All	20.07	28.61	40.10	30.16	71.73	26.66	2.413	52.60	35.95
ages 20-24	6.889	19.77	48.31	24.56	53.62	23.74	0.675	47.46	36.52
ages 25-29	4.192	23.38	44.82	31.96	49.41	29.94	1.042	43.65	46.66
ages 30-39	4.849	29.57	41.30	33.79	56.63	26.63	2.090	44.51	47.40
ages 40-49	3.734	26.54	30.55	22.29	63.99	22.24	1.718	41.46	

Notes: Table shows overall conviction rates (Total) and type of crimes rates conditional on having been convicted at least once or twice. The sample are all men of the Parent Generation born 52-55 with all SES (Panel A and B, N=176,232) or with low SES (Panel C and D, N=107,557).

Table 16: Conviction rates by age and types of crimes for women of Parent Generation

	Percent convicted by crime types conditional on being convicted								
	Total	Violent	Property	Fraud	Traffic	Drugs	Sex	Others	Prison
Panel A: Women of Parent Generation, all SES: at least one conviction									
All	9.084	5.706	26.94	17.81	45.34	13.71	0.0594	15.77	5.766
ages 20-24	3.300	2.608	26.01	16.39	34.19	23.79	0	13.07	2.943
ages 25-29	2.272	3.151	25.61	19.23	32.13	20.10	0.0407	15.61	4.631
ages 30-39	3.070	5.060	28.36	20.32	39.84	10.80	0.0611	14.41	5.457
ages 40-49	2.623	6.469	27.69	15.25	45.40	6.970	0.0626	14.39	
Panel B: Women of Parent Generation, low SES: at least one conviction									
All	9.008	5.968	27.31	18.93	45.05	13.29	0.0576	15.52	6.149
ages 20-24	3.368	2.747	25.11	16.71	35.13	21.82	0	13.11	2.631
ages 25-29	2.229	3.567	25.55	20.47	32.86	19.39	0.0665	15.02	4.919
ages 30-39	3.118	5.256	28.70	21.72	40.12	10.40	0.0476	13.78	6.029
ages 40-49	2.628	6.674	29.19	16.27	43.35	6.914	0.0564	14.56	
Panel C: Women born 52-55 all SES, Obs. 167,588: at least one conviction									
All	10.22	5.996	27.42	18.80	45.37	15.99	0.0409	16.21	6.159
ages 20-24	3.210	2.603	25.77	16.38	33.87	23.72	0.0186	13.42	2.956
ages 25-29	2.359	3.542	26.21	22.54	31.93	19.78	0.0253	14.70	5.490
ages 30-39	2.936	6.016	30.55	21.48	41.91	10.30	0.0610	14.47	7.114
ages 40-49	2.604	7.333	26.81	15.03	45.14	9.120	0.0458	16.77	
Panel D: Women born 52-55 low SES, Obs. 102,223: at least one conviction									
All	10.21	6.350	27.65	19.85	45.10	15.38	0.0479	15.96	6.656
ages 20-24	3.259	2.852	24.95	16.69	34.97	22.16	0.0300	13.12	2.852
ages 25-29	2.300	3.828	26.16	23.99	31.82	19.44	0.0425	14.16	5.700
ages 30-39	3.022	6.280	30.79	23.28	41.73	9.938	0.0324	13.79	8.158
ages 40-49	2.592	7.623	29.02	15.32	42.72	9.585	0.0755	17.28	

Notes: Table shows overall conviction rates (Total) and type of crimes rates conditional on having been convicted at least once or twice for women in the Parent Generation, with all SES or low SES. Panel A and B: only women who are fully observed for the relevant age bracket are included. All includes the whole sample of women born 45-55 (all SES N=426,133, low SES N=269,701) and the conviction rates refers to having ever been convicted or having been convicted at least twice between 1973-2010. Age specific conviction rates for women in Parent Generation includes: for ages 20-24 cohorts 53-55 (all SES: N=126,625, low SES: 76,749), for ages 25-29 cohorts 48-55 (all SES: N=324,147, low SES: 202,430), and for ages 30-39, and 40-49 the whole sample of cohorts 45-55 (all SES: N=426,133, low SES: 269,701). Panel C and D: all conviction rates include only women born 52-55 (all SES: N=167,588, low SES: N=102,223).

7.2 Women and the reform

Table 17: Reform effects on crime by types of crimes for women of parent generation

	(1)	(2)	(3)	(4)	(5)	(6)
	Any	Violent	Property	Drugs	Traffic	Fraud
Women born 52-55: All SES, Obs: 167,588						
Reform	0.389	0.039	-0.209	0.190	0.414*	-0.120
	(0.315)	(0.084)	(0.214)	(0.135)	(0.238)	(0.135)
\bar{y} %	10.22	0.613	2.803	1.634	4.638	1.922

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between 1973-2010 for any crime in column (1), or one of the crime types specified in columns (2)-(6). 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 and father's education levels.

Table 18: Age specific reform effects on crime for women of the parent generation

	(1)	(2)	(3)
Convicted at age:	30-39	25-29	20-24
Women born 52-55, All SES, Obs. 167,588			
Reform	-0.013	-0.043	0.262
	(0.201)	(0.166)	(0.194)
\bar{y} %	2.936	2.359	3.210

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 30-39, 25-29, and 20-24. The sample are women from the parent generation born 52-55. Robust standard errors, clustered by birth municipality, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of the individual, and father's education levels.

Table 19: Age specific reform effects of mothers on son's crime

	(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 SES				
Reform mother	-0.078 (0.337)	0.123 (0.247)	0.029 (0.200)	0.129 (0.190)
Obs	429,114	429,114	406,408	347,811
\bar{y} %	25.65	16.81	12.41	8.053
Panel B: Low SES				
Reform mother	0.092 (0.358)	0.259 (0.277)	0.202 (0.265)	0.319 (0.227)
Obs	275,501	275,501	263,373	230,812
\bar{y} %	27.18	17.79	13.28	8.481

Notes: Significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 15-29, 15-19, 20-24 or 25-29. The sample are sons of women born 45-55 who are fully observed for the relevant age bracket of the dependent variable. Robust standard errors, clustered by mother's municipality of birth, in parentheses; all regressions include a full set of mother's birth municipality and mother's birth cohort indicator variables. Panel A includes grandfather's education levels.

Table 20: Reform effects of mothers on son's crime by type of crimes

	(1)	(2)	(3)	(4)	(5)
Convicted at age 15-29:	Violent	Property	Drugs	Traffic	Fraud
Panel A: All SES, Obs 429,114					
Reform mother	0.124 (0.110)	0.288 (0.191)	0.016 (0.100)	-0.049 (0.203)	-0.012 (0.097)
\bar{y} %	4.628	8.631	3.122	12.29	3.352
Panel B: Low SES, Obs 275,501					
Reform mother	0.117 (0.144)	0.539** (0.228)	0.095 (0.130)	-0.092 (0.247)	0.124 (0.112)
\bar{y} %	5.053	9.230	3.111	13.42	3.572

Notes: Significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 15-29 for one of the specified crime types. Robust standard errors, clustered by mother's municipality of birth, in parentheses; all regressions include a full set of mother's birth municipality and mother's birth cohort indicator variables. Panel A includes grandfather's education levels.

7.3 Parent estimations including trends

Table 21: Reform effects on years of schooling for parent generation

Dependent variable: Own years of schooling			
	(1)	(2)	(3)
Sample:	All SES	Low SES	High SES
Reform	0.216*** (0.033)	0.309*** (0.034)	0.080** (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. The dependent variable is number of years of own schooling. 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 and municipality group specific cohort trends. Column (1) includes father's education levels.

Table 22: Reform effects on crime by types of crimes for parent generation

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

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between 1973-2010 for any crime in column (1), or one of the crime types specified in columns (2)-(6). 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, and municipality of birth group specific cohort trends. Panel A includes father's education levels.

Table 23: Age specific reform effects on crime for the parent generation

	(1)	(2)	(3)
Convicted at age:	30-39	25-29	20-24
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Panel A: Men born 52-55, All SES, Obs.	175,681		
Reform	-0.291 (0.573)	-1.261** (0.630)	-1.594** (0.711)
\bar{y} %	12.37	11.61	18.65
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Panel B: Men born 52-55, Low SES, Obs.	107,210		
Reform	-0.691 (0.695)	-1.403* (0.739)	-1.537* (0.849)
\bar{y} %	13.18	12.27	19.79

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 30-39, 25-29, and 20-24. The sample are men from the parent generation born 52-55. Robust standard errors, clustered by birth municipality, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of the individual, and municipality of birth group specific cohort trends. Panel A also includes father's education levels.

Table 24: Reform impact on prison sentences for parent generations

	(1)	(2)
Men born 52-55	All SES	Low SES
Reform	-0.495 (0.447)	-0.670 (0.527)
Obs	175,554	107,210
\bar{y} %	7.505	8.206

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1. Effects scaled by 100. The dependent variable is an indicator variable for having ever been convicted for a prison sentence between 1973-2010. Robust standard errors in parentheses clustered by own birth municipality. Includes own birth cohort and birth municipality indicator variables, and municipality of birth specific cohort trends. Column (1) includes father's education levels.

7.4 Child estimations including trends

Table 25: Age Specific Reform Effects on Son's Crime

	(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 SES				
Reform father	-0.800*** (0.276)	-0.632*** (0.221)	-0.345* (0.197)	-0.196 (0.208)
Obs	409,083	409,083	364,521	282,305
\bar{y} %	23.53	15.69	11.69	7.863
Panel B: Low SES				
Reform father	-0.571 (0.356)	-0.481* (0.289)	-0.244 (0.263)	0.104 (0.269)
Obs	261,014	261,014	235,478	186,858
\bar{y} %	25.09	16.81	12.44	8.259

Notes: Significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 15-29, 15-19, 20-24 or 25-29. The sample are sons of men born 45-55 who are fully observed for the relevant age bracket of the dependent variable. Robust standard errors, clustered by father's municipality of birth, in parentheses; all regressions include a full set of father's birth municipality and father's birth cohort indicator variables. In addition all estimations include father's cohort trends that are specific to father's birth municipality group. Panel A includes grandfather's education levels.

Table 26: Reform Effects on prison sentences for sons

	(1)	(2)
Sons of men 45-55:	All SES	Low SES
Reform father	-0.015 (0.099)	0.108 (0.138)
Obs	408,021	261,014
\bar{y} %	2.385	2.683

Notes: Significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Effects scaled by 100. The dependent variable for is an indicator variable for having ever been convicted for a prison sentence between the ages 15-29. Robust standard errors in parentheses clustered by father's birth municipality. Includes father's cohort and father's birth municipality indicator variables, and father's birth municipality specific cohort trends. Column (1) includes grandfather's education levels.