THE EFFECT OF EDUCATION POLICY ON CRIME: AN INTERGENERATIONAL PERSPECTIVE

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

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Abstract

We examine the intergenerational effect of education policy on crime. Using administrative data that links outcomes across generations with crime records, we show that the Swedish comprehensive school reform, gradually implemented between 1949 and 1962, reduced conviction rates for both the generation directly affected by the reform and their sons. The reduction in conviction rates occurred in several types of crime. Mediation analysis suggests that key channels include increased parental educational attainment and household income, as well as reduced criminal behavior among fathers.

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

Policies that increase educational attainment have long-term effects on earnings, employment, and many other life outcomes. A growing body of evidence also shows that more education reduces participation in crime (Lochner and Moretti, 2004; Machin, Marie, and Vujić, 2011), consistent with theoretical predictions that education raises the opportunity cost of criminal activity (Becker, 1981; Freeman, 1999; Lochner, 2004).[1]

Whether these effects extend to the next generation remains an open empirical question. This paper investigates the intergenerational consequences of a comprehensive school reform in Sweden, focusing on criminal behavior among the children of those directly exposed.

Between the late 1940s and early 1960s, Sweden gradually implemented a nationwide reform that increased compulsory schooling, unified the curriculum, and delayed academic tracking. The reform was rolled out across municipalities over fifteen years, generating quasi-experimental variation in exposure. Previous research shows that the reform increased educational attainment and earnings (Meghir and Palme, 2005), and reduced criminal convictions in treated cohorts (Hjalmarsson, Holmlund, and Lindquist, 2015). We build on this work by examining whether the reform also improved outcomes in the next generation.

We use administrative data on all individuals born in Sweden from 1945 to 1955 and their children, linked across generations. This includes detailed information on education, income, and criminal records. Our empirical strategy follows a difference-in-differences design, exploiting the staggered rollout of the reform across municipalities. Reform exposure is assigned based on birth year and municipality in the parent generation.

Our central finding is that the reform reduced criminal convictions among the sons of treated fathers. The conviction rate declined by 0.8 percentage points – roughly 3 percent of the baseline rate – with effects spanning violent, financial, and miscellaneous crimes. In

[1] See also Freeman (1996), Grogger (1998), Gould, Weinberg, and Mustard (2002), Williams and Sickles (2002), Machin and Meghir (2004), Edmark (2006), Gallipoli and Fella (2008), and related work in sociology and criminology such as Sabates (2008) and Sabates and Feinstein (2008).

contrast, we find no effect on sons of treated mothers, and no comparable effect among daughters.

To understand these patterns, we examine theoretical and empirical channels linking parental education to child outcomes. There are several reasons why one can expect an intergenerational impact. The hypothesis is based on Becker and Tomes (1979), who developed the theory of intergenerational transmission of human capital, and on Cunha and Heckman (2007) and Cunha, Heckman, and Schennach (2010), who characterize theoretically and empirically a process of skill formation that depends on parental skills and investments in their children. Consistent with these ideas, we expect the improved human capital of those directly treated by the reform to entail better investments in children (Cunha, 2007; Caucutt and Lochner, 2020) and increased productivity of these investments, due to complementarity with parental human capital in the production function for child skills (see Heckman, 2007; Cunha, Heckman, and Schennach, 2010).

The empirical literature on intergenerational transmission and investments in children can be broadly classified into three strands. The first examines early childhood interventions, including experimental programs such as the Perry Preschool Project and the Abecedarian study (Schweinhart, Montie, Xiang *et al.*, 2005; Heckman, Moon, Pinto *et al.*, 2010; Gertler, Heckman, Pinto *et al.*, 2014; Campbell, Conti, Heckman *et al.*, 2014), as well as observational studies of early-life conditions and parental investments (e.g., Currie and Moretti, 2003; Cunha and Heckman, 2008; Cunha, Heckman, and Schennach, 2010; Attanasio, Meghir, and Nix, 2020). The second focuses on long-run effects of targeted income support programs, including food stamps and tax credits for low-income households (see Page, 2024). The third investigates the impacts of education policy reforms – typically those that raise compulsory schooling or expand access—on both the directly affected generation and their children (e.g., Currie and Moretti, 2003; Black, Devereux, and Salvanes, 2005; Holmlund, Lindahl, and Plug, 2011; Lundborg, Nilsson, and Rooth, 2014).

Despite extensive evidence on education and income interventions, relatively few studies

have examined intergenerational effects on criminal behavior – a notable gap given the substantial social costs of crime. Exceptions include Akee, Copeland, Keeler *et al.* (2010), Jacob, Kapustin, and Ludwig (2014), Cesarini, Lindqvist, Östling *et al.* (2023), and Bailey, Hoynes, Rossin-Slater *et al.* (2024), who study the effects of parental income or subsidies on children's criminal outcomes, and Heckman and Karapakula (2019), who evaluate second-generation effects of the Perry Preschool Project.

The sociology literature has long emphasized role models and intra-family social interactions as channels for the intergenerational transmission of behavior and norms (Merton, 1938; Coleman, 1988). Dahl, Kostøl, and Mogstad (2014) provide empirical evidence of this mechanism in the context of disability insurance, showing that children whose parents participate in the program are significantly more likely to claim benefits themselves – a pattern consistent with the transmission of information and behavioral norms across generations. The strong intergenerational correlation in criminal behavior (Hjalmarsson and Lindquist, 2012, 2013), combined with the decline in parental crime induced by the reform (see also Hjalmarsson, Holmlund, and Lindquist, 2015), points to parental criminality as a plausible transmission channel. A complementary mechanism operates through peer effects: growing up in neighborhoods or attending schools with higher exposure to criminal behavior has been shown to affect child outcomes (Chetty, Hendren, and Katz, 2016; Billings, Deming, and Ross, 2019; Chyn and Katz, 2021).

We empirically examine these mechanisms by first documenting that the reform improved several key dimensions of human capital among men in the parental generation. Treated fathers experienced significant gains in schooling, earnings, occupational status (measured by the likelihood of entering white-collar jobs) as well as in cognitive and social skills.[2] They also partnered with women with higher educational attainments and earnings, contributing to higher household income. In addition, families of treated fathers were more likely to relocate to lower-crime neighborhoods and enroll their children in schools with fewer criminally

[2]See also Lager, Seblova, Falkstedt *et al.* (2017).

involved peers.

The reform had only weak effects on schooling (and partners' schooling) for women, presumably because women in the 1945-55 cohorts were much more likely than men to already reach the new compulsory schooling threshold in the pre-reform period. In turn, the reform did not affect their earnings, household income, or criminal activity, and they did not move to lower crime neighborhoods or place their children in schools with peers less involved in crime, as did families with treated fathers. Consistently, we find significantly stronger effects on the participation in crime for sons whose fathers were treated by the reform, compared to sons whose mothers were treated.

We then show that outcomes other than crime improved for the children of fathers exposed to the reform. For sons, these include significant increases in school GPA and employment, as well as better health. For daughters, we find a reduction in welfare dependency. The results are all consistent with improvements in the parent generation driving growth in the human capital of their children, which in turn led to a reduction in crime participation for boys.

In relation to the previous literature on second-generation effects of policies that increase parents' resources, our findings align with evidence that interventions such as food stamps (Hoynes, Schanzenbach, and Almond, 2016; Bailey, Hoynes, Rossin-Slater *et al.*, 2024), tax credits (Dahl and Lochner, 2012; Hoynes, Miller, and Simon, 2015), and education policies (Currie and Moretti, 2003; Lundborg, Nilsson, and Rooth, 2014) can improve children's outcomes. Most of that research, however, examines outcomes early in childhood, whereas we look later in the life cycle.

The Swedish schooling reform increased parental schooling and household resources, and reduced crime among men. To summarize the underlying mechanisms for intergenerational effects between fathers and sons, we conduct a simple mediation analysis, similar to Heckman, Pinto, and Savelyev (2013). The results suggest that beyond resources, "parental quality" as proxied by education, alongside a direct effect of parental crime, play important roles in

determining child participation in crime. While this type of analysis must be interpreted with caution – given the potential endogeneity of the mediators and possible measurement issues – the findings are consistent with an extension of the Becker–Tomes model that incorporates parental quality, as emphasized by Heckman (2007) and Cunha, Heckman, and Schennach (2010).

The remainder of the paper proceeds as follows. Section 2 describes the institutional background. Section 3 outlines the data and key variables. Section 4 discusses theories of how parental education can affect their children's criminal participation, and Section 5 presents the empirical strategy. Section 6 reports the main results. Section 7 investigates the mechanisms and reports the mediation analysis. Section 8 concludes. An online appendix contains a stylized model, robustness checks, and additional results.

2 The Comprehensive School Reform

Before the implementation of the comprehensive school reform, pupils attended a common basic compulsory school (*folkskolan*) until grade 6. After sixth grade, students were selected to continue one or (mainly in urban areas) two years in basic compulsory school or to attend a three-year junior secondary school (*realskolan*). The selection of students in the two different school tracks was based on their previous grades. In most cases, compulsory pre-reform school was administered at the municipality level. The junior high school was a prerequisite for the subsequent high school, which, in turn, was required for higher education.

In 1948, a parliamentary school committee proposed a reform that increased compulsory schooling to nine years, abolished tracking, and instituted a new comprehensive school teaching a national curriculum. As a result, compulsory schooling increased by one to two years, depending on the area. The new system only affected schooling after the 6th grade and not before.[3]

[3]For further details, see Meghir and Palme (2003), Meghir and Palme (2005), Holmlund (2007), Marklund (1980), and Marklund (1981). Similar reforms have been adopted in many other countries, which makes the study of their effects of wide interest. England introduced very similar reforms in the mid-60s and in 1973.

Table 1: Timing of Reform Implementation and Municipality Characteristics

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

Notes: Significance levels *** p<0.01. The dependent variable is the cohort for which the reform was first implemented in the municipality.

The proposed new school system was gradually introduced into the 1,055 Swedish municipalities and parts of city communities from 1949 to 1962 and was implemented for the cohort of students who were in fifth grade at the time or for those who were currently in the first grade, effectively delaying the start of the program.[4] In our analysis, we include people born between 1945 and 1955 and their children. Appendix Figure 1 shows the number of observations in our sample in each year's birth cohort and the proportion of the parent generation assigned to the reform. The rollout was not random and was decided by the government in consultation with the local authority. Based on a regression of the implementation cohort for each municipality, we find that early implementers had higher income and a higher local tax rate. The size of the municipality population had no effect (Table 1).[5]

3 Data

We use data from the Swedish population census. The multigenerational register from Statistics Sweden (2003) allows us to link two generations: the *parent generation* consisting of all

[4]The purpose of gradual implementation was to evaluate the reforms from an administrative perspective (National School Board, 1959; Marklund, 1981).

[5]We do not have crime rates by municipality early enough to check whether implementation was correlated with the crime rate.

individuals born in Sweden between 1945 and 1955 and who, as a result, attended school during the gradual implementation of the new education system; and their children, called the *children generation*. Information on educational achievement is obtained by linking these data to the education register (see Appendix Section A.5 for more details on the various data sources).

To obtain convictions, which is our measure of crime, we link these data with records from the Swedish National Council for Crime Prevention.[6] This provides all convictions at the individual level, as well as the type of crime and the date it was committed.[7] Crime records start in 1973, and the gradual transition to the new educational system covers the cohorts born between 1945 and 1955. However, when estimating reform effects on crime for the parent generation, we use data for the 1952-55 cohort, who were 18-21 years old in 1973 when records began. They are followed until 2010 when they are 55-58.

For the child generation, we observe convictions from age 15, when legal criminal responsibility begins. We follow them until age 29, estimating the effects on the most important part of the criminal life cycle. Their fathers may have been born any time in the 1945-55 period of gradual reform implementation since we only need information on the type of school system attended by the parent (and not their crime history).[8]

Table 2 shows the conviction rates in general and by type of crime, conditional on conviction. We categorize crimes into seven types: violent crimes, property crimes, fraud and tax evasion, traffic crimes, drug and trafficking violations, sex crimes, and others, which do not fit the above definitions.[9] Since individuals can have multiple convictions, these columns add up to more than 100.

[6]Convictions, an important outcome in their own right, closely track trends in reported crimes as shown in Appendix Figure 6, indicating that the relationship between crime participation and convictions is stable over time.

[7]There are five types of punishments in the Swedish judicial system: fines, prisons, parole, probation, and special medical treatment. Pecuniary fines that are decided in courts and included in our data are related to the convicted person's income and, in most cases, paid in smaller installments.

[8]For this sample of sons we match education information of paternal grandfathers, used in the X-vector of controls in the regressions, from the education census of 1970. This is available for 61.5 percent of our sample.

[9]Table 10 in the Appendix provides a detailed description of each category.

During the observed period, 34 percent of men in the parent generation have been convicted at some point. Of these, 21 percent have a conviction for a property crime, 18 percent for fraud (including tax evasion), and 33 percent for "other" crimes (defined in the appendix and below). One in five convictions led to a prison sentence. Finally, 58 percent of convictions relate to serious traffic offenses that lead to a court appearance, including drunk driving, causing a serious accident *et cetera*.[10] Minor offenses that lead to a direct fine without a court appearance are not included.

The child generation, shown in the two lower panels, displays a lower, but still substantial, conviction rate of 24 percent (by age 29). The distribution is slightly different, with a higher share of property crime. The child generation has half the incarceration rate (conditional on a conviction) over the age range when we observe them. Women in both generations have about a third of the conviction rate of men, with some small changes in the distribution by types of crime. As we show in the Appendix Figure 2, crime decreases drastically with age, with the highest incidence from adolescence to around age 30. Moreover, crime rates have fallen substantially across cohorts in the generation of children.

We assign people in the 1945-1955 cohort to the reform based on whether their birth municipality implemented the new system for their cohort, avoiding the potential bias induced by parents moving municipalities as a result of the reform.[11] We then assign people in the child generation to the reform depending on whether their parent was assigned.

We complete our description by showing the association between schooling and crime, as well as the intergenerational association in crime rates, in Table 3. All regressions include dummies for the birth municipality of the father and cohort effects. One year of schooling for men in the parent generation is associated with a decrease in the probability of a conviction of 2.2 percentage points (pp). In the generation of children, an extra year of education for

[10] Additional support for such high conviction rates in Sweden is provided by Hjalmarsson and Lindquist (2012, 2013), Grönqvist (2011) and von Hofer (2014).

[11] We thank Helena Holmlund for providing the matching algorithm - see Holmlund (2007). Based on survey data in which we observe both the birth municipality and the schooling, the discrepancy is only 9.9 percent. 5.3 percent moved to a non-reform municipality, and 4.6 percent moved in the other direction. This leads to a potential attenuation factor of 0.901.

Table 2: Conviction Rates for the Parent and Child Generations, by Crime Type

		Conditional on conviction						
	Total (1)	Violent (2)	Property (3)	Fraud (4)	Traffic (5)	Drugs (6)	Other (7)	Prison (8)
Parent Generation (age 18-65)								
Men (%)	33.85	14.86	20.82	18.03	57.92	17.01	33.09	19.48
Women (%)	9.09	5.69	26.87	17.80	45.31	13.64	15.72	5.72
Child Generation (age 15-29)								
Men (%)	23.69	19.23	33.07	12.28	46.03	13.63	31.18	10.26
Women (%)	7.92	8.64	53.17	18.49	17.60	18.49	18.49	1.40

Notes: This table shows the proportion ever convicted (at least one conviction) over the observed age range. Column 1 shows the proportion ever convicted. Columns 2-7 show the proportion of convictions in that category, conditional on having been convicted at least once. Column 8 shows the proportion of prison sentences, conditional on having been convicted at least once. The parent generation includes the whole sample of men ($N = 451,158$) and women ($N = 448,167$) born 1945-55, with convictions measured in 1973-2010. The child generation includes the sample of sons ($N = 426,721$) and daughters ($N = 403,375$) with parents in the reform sample, born in or before 1993, observed between ages 15 and 29. "Other" crimes include defamation, family law, vandalism, hazardous general crimes, crimes against public order, violation of general business, Crimes against national security, misconduct, Environmental law, Alcohol law, Weapon and knife law, Immigration law, Copyright law, Working environment law, visiting rights law - harassment of ex-spouse, etc., privacy and data protection. See Appendix for all crime definitions.

the father is associated with a 0.9pp reduction in crime for sons and 0.21pp for daughters. A similar association is true with respect to the mother's education. The strong association with education is also true for prison sentences.

Moreover, there is a strong intergenerational correlation in crime, as illustrated in the lower panel of Table 3, although less so for daughters.[12]

4 Intergenerational Transmission of the Reform

This section examines the mechanisms through which the education reform may have influenced criminal behavior in the next generation. We focus on three conceptual channels emphasized in the literature: (i) a *human capital channel*, through which more educated

[12]See Haider and Solon (2006) on the importance of aligning ages when carrying out intergenerational regressions. In our cases, the alignment of the ages only matters when looking at prison sentences, where the alignment increases the coefficient from 6.2 to 9.1.

Table 3: Education, Crime and Intergenerational Persistence

Panel A: Association between Crime and Education

	Parent Generation				Child Generation			
	Men, $N = 451,158$		Women, $N = 430,949$		Sons, $N = 460,770$		Daughters, $N = 436,004$	
	Convicted	Prison	Convicted	Prison	Convicted	Prison	Convicted	Prison
Own education	-2.487	-0.996	-0.520	-0.094	-	-	-	-
	(0.112)	(0.058)	(0.049)	(0.009)				
Fathers' education	-	-	-	-	-0.944	-0.186	-0.209	-0.012
					(0.024)	(0.008)	(0.015)	(0.002)
Mothers' education	-	-	-	-	-0.837	-0.205	-0.282	-0.012
					(0.026)	(0.010)	(0.017)	(0.002)
$\overline{y}$	33.1%	7.2%	9.1%	0.5%	27.1%	3.4%	8.7%	0.2%

Panel B: Intergenerational Persistence in Crime

	Sons, $N = 488,194$		Daughters $N = 461,461$	
	Convicted	Prison	Convicted	Prison
Father conviction/prison	0.133	0.116	0.046	0.024
	(0.002)	(0.007)	(0.001)	(0.003)
Mother conviction/prison	0.126	0.070	0.075	0.006
	(0.001)	(0.002)	(0.002)	(0.001)
$\overline{y}$	27.1%	3.4%	8.7%	0.2%

Notes: This table shows associations between years of schooling and probabilities of conviction in the child generation, scaled by 100 so they are interpreted as percentage points. The parent generation includes the whole sample of men and women born 1945-55, with convictions measured in 1973-2010. The child generation includes the sample of sons and daughters with parents in the reform sample, born in or before 1993, observed between ages 15 and 29. The dependent variables are indicators for ever having been convicted or having received a prison sentence. 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.

parents invest more effectively in their children's development; (ii) a *role model channel*, where parental behavior shapes child norms and choices; and (iii) a *neighborhood channel*, through which changes in parental education and income affect residential sorting and peer environments.

The Human Capital Channel Exposure to the reform increased educational attainment and earnings in the parental generation, as we document below. In standard models of altruistic parental behavior (Becker and Tomes, 1979), increased parental resources increase investment in child utility, including through expenditures and time devoted to the formation of human capital. Empirically, more educated parents are found to spend more time and money on child development (Guryan, Hurst, and Kearney, 2008), and income gains through increased parental labor supply, such as those induced by the EITC, do not appear to

crowd out parental investment time (Bastian and Lochner, 2020). Several studies show that exogenous increases in parental income improve child outcomes, including cognitive and noncognitive skills (Cunha and Heckman, 2007; Dahl and Lochner, 2012; Akee, Copeland, Keeler *et al.*, 2010; Hoynes, Miller, and Simon, 2015; Milligan and Stabile, 2011; Bastian and Michelmore, 2018; Caucutt and Lochner, 2020; Carneiro, García, Salvanes *et al.*, 2021).

There is also evidence that increased household resources can reduce children's involvement in crime.Akee, Copeland, Keeler *et al.* (2010) document a large decline in minor crimes committed by children following substantial annual income gains to their families, while Bailey, Hoynes, Rossin-Slater *et al.* (2024) find second-generation long-term reductions in incarceration associated with the implementation of the Food Stamp program in the United States between 1961 and 1975. However, not all studies identify such effects. For example, Cesarini, Lindqvist, Östling *et al.* (2023), exploiting the random variation in wealth from Swedish lottery winnings, found no significant impacts of large income gains on crime for parents or children. Likewise, studies of the Chicago Housing Authority voucher lottery report substantial increases in household consumption but limited long-term effects on child education or criminal behavior (Heller, Jacob, and Ludwig, 2011; Jacob, Kapustin, and Ludwig, 2014).

Labor market shocks have also been used to study the causal impact of income on family outcomes (Page, 2024). Although most studies focus on contemporaneous impacts in the generation directly affected, Britto, Pinotti, and Sampaio (2022) documents that paternal job loss increases short-term criminal involvement among sons, although these effects reflect immediate stress rather than longer-term channels related to human capital accumulation.

Beyond financial resources, parental education may increase the productivity of investments in children. The framework developed in Heckman (2007) and extended in Cunha, Heckman, and Schennach (2010) augments the Becker–Tomes model to include parental human capital as an input in the production of child skills. In this setting, educated parents are not only better resourced, but also more effective at transmitting skills. This mecha-

nism is central in our context: unlike lottery or voucher shocks, the reform increased both parental education and income. In the online appendix (Section A.1), we present a model formalizing this interaction, allowing for heterogeneity in parental productivity in shaping child behavior.

Finally, *human capital channel* is based on the idea that education increases the expected returns to legal employment, thereby increasing the opportunity cost of engaging in criminal activity. Machin and Meghir (2004) provide empirical support for this mechanism, showing that higher wages and educational attainment are associated with lower rates of criminal participation.

The Role Model Channel Parental behavior may directly shape child behavior. Hjalmarsson and Lindquist (2012) document strong intergenerational persistence in criminal convictions: Sons and daughters of convicted fathers are more than twice as likely to be convicted themselves. Although some of this correlation may reflect unobserved heterogeneity, behavioral models suggest a causal role for parental influence (e.g., Bethencourt and Kunze, 2022). Evidence of behavioral transmission across generations also emerges in domains such as disability insurance. Dahl, Kostøl, and Mogstad (2014) show that parents' program participation increases children's likelihood of claiming benefits, conditional on income.

Behavioral adjustments by parents may also reflect concern about their children's future. For example, Dustmann and Landersø (2021) find that young men reduce criminal activity after becoming fathers, particularly when they have sons, which is consistent with an effort to avoid transmitting undesirable behaviors.

The importance of role modeling has long been emphasized in sociology. Merton (1949) argues that access to prosocial role models is a key determinant of behavior, while Coleman (1988) links the formation of social and human capital to familial and community-level role structures. In our setting, increased education may shift parental norms and behaviors in ways that reduce intergenerational transmission of crime.

The Neighborhood Channel Education and income gains induced by the reform may also have changed where families live and which schools their children attend. Higher-educated parents may be more likely to move to neighborhoods with better schools, lower crime, and higher socioeconomic status. These changes can alter peer influences and institutional exposure during formative years.

Models of social interactions suggest that behavior is shaped by local peer environments and norms (Manski, 1993; Glaeser, Sacerdote, and Scheinkman, 1996; Brock and Durlauf, 2001). Empirical studies confirm that neighborhood quality affects child outcomes, including criminal behavior. For example, Chetty, Friedman, Hendren *et al.* (2018) show that children moving to lower-poverty neighborhoods experience improved long-run outcomes. Ye (2024) also find that peer characteristics shape children's behavior in important ways.

More broadly, Chyn and Katz (2021) reviews the literature and concludes that exposure to high-quality neighborhoods and schools reduces engagement in risky behaviors, including crime and teenage pregnancy. Related studies show that school and neighborhood characteristics strongly predict child criminal outcomes (Hastings and Weinstein, 2008; Deming, 2011; Dobbie and Fryer Jr, 2015), and that exposure to neighborhood or peer violence has persistent effects (Damm and Dustmann, 2014; Billings, Deming, and Ross, 2019).

5 Empirical Strategy

Our main outcome variable is the overall conviction rate, which we also break down by type of crime. Individuals in the parent generation are aged 18-65, while those in the child generation are aged 15-29. Since we observe convictions rather than offending, a key assumption for interpreting our results is that the reform did not affect the relationship between the two.

The reform was not randomized, so we control for possible differences between the treatment and comparison groups using difference-in-differences. We compare the change in convictions between cohorts in municipalities that implemented the reform for the younger

cohort but not the older one, to the change in convictions across the same cohorts living in municipalities where they were not affected by the change in policy.

There are more than 1,000 municipalities and 11 cohorts, making a probit or a logit with fixed effects impractical. Thus, we estimate the impacts using OLS in the linear probability model (LPM).[13] In all results presented below, we use the following regression:

$$y_i = \beta_1 R_i + \gamma_1' t_i + \gamma_2' m_i + \gamma_3' X_i + \epsilon_i, \tag{1}$$

where i denotes individual, y_i is the outcome variable under consideration in the parent or child generation, R_i is the reform indicator, which equals one if the individual belongs to a municipality and cohort that has been assigned to the new school system; t_i and m_i are indicators denoting the cohort and the birth municipality of individual i (for children, these refer to those of their parents). X_i is the grandfather's education, which we include to improve precision since there is such a strong intergenerational association between education and crime. Finally, ϵ_i is assumed conditionally independent of R_i.[14] All standard errors are clustered at the level of the municipality of the parent generation. Following the discussion of our main results, we present a set of robustness checks, with details in the appendix.

5.1 Definition of Treatment

In the parent generation (which is the one directly treated), treatment is defined as having been born in a municipality where the birth cohort of the individual concerned attended the reformed school system. Thus, the treatment unit is the municipality, of which there were about 1,000 in Sweden at the time. The impacts are interpreted as an intention to treat effect, because some people may have moved to a different municipality at the time of their

[13] 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 were generated by a normal probability model (probit), then using an LPM only biased the results upward by 5 percent with respect to the true average effect - a difference that is statistically indistinguishable in our data.

[14] The general assumptions underlying the method of difference-in-differences are discussed in Heckman and Robb (1985), Heckman, Ichimura, Smith *et al.* (1999), and Athey and Imbens (2006).

secondary education, although mobility was limited.[15]

For the child generation, treatment status is potentially more complex because either parent could have been exposed to the reform. Women are on average about two years younger than their husbands, making them more likely to have been exposed within a married couple. Many people marry across municipalities, which means that a treated father does not automatically imply a treated mother, and vice versa. Whether both parents are treated is an endogenous outcome of the marriage market. We therefore present results for two treatment definitions: (1) whether the father was exposed to the reform, and (2) whether the mother was exposed. Appendix Table 14 summarizes the incidence of fathers, mothers, and both parents being treated, from the perspective of the child generation.

6 Results

6.1 The Reform and Crime in the Child Generation

Table 4 shows the impact of fathers' and mothers' exposure to the reform on convictions in the *child generation*.[16] For sons, the reduction in conviction rates is approximately 0.79 percentage points (pp) (p-value$<$0.002), which represents a reduction of 3.4 percent.[17] In terms of percentage reduction, this is similar to the decline in crime attributed to the reform among fathers. The incidence of multiple convictions shows a large decrease of 0.43pp (p-value $<$0.002), representing a reduction of 5.8 percent. Results for the effect on crime among sons when the mother was exposed to the reform are provided in the bottom panel of the table, and show that the impact of the mothers' reform exposure on child conviction rates is an order of magnitude smaller and insignificant at any reasonable significance level. Importantly, the difference in the impact of the father being exposed to the reform as opposed

[15] See Meghir and Palme (2005).

[16] Detailed results on criminal convictions among the parents are presented in the Appendix and discussed below.

[17] The reduction is equally significant if we exclude traffic crimes.

to the mother is significant (p-value 0.048 for "any" and 0.009 for "multiple"). Hence, the second-generation effects are entirely driven by fathers being exposed to the reform – a point we return to below.

Table 4: Impact of the Reform on Crime Among Sons and Daughters in the Child Generation

	Convictions		Breakdown by type of crime					
	Any†	Multiple††	Violent	Property	Drugs	Traffic	Fraud	Other†††
Father treated								
Sons, $N = 410,476$								
Reform	-0.791***	-0.428**	-0.265*	-0.026	0.102	-0.446*	-0.223*	-0.408**
	(0.253) [0.0018]	(0.196) [0.019]	(0.117) [0.09]	(0.154) [>0.5]	(0.109) [>0.5]	(0.173) [0.057]	(0.096) [0.090]	(0.152) [0.047]
Mean of dep var, %	23.54	7.34	4.49	7.74	3.18	10.8	7.02	7.32
Daughters, $N = 388,135$								
Reform	0.156	0.004	0.091	0.111	0.004	0.108	-0.049	-0.129*
	(0.158) [0.32]	(0.072) [>0.5]	(0.049) [0.36]	(0.119) [>0.5]	(0.044) [>0.5]	(0.075) [0.40]	(0.073) [>0.5]	(0.047) [0.052]
Mean of dep var, %	7.92	1.51	0.68	4.21	0.77	1.39	1.46	0.83
Mother treated								
Sons, $N = 429,114$								
Reform	-0.092	0.232	0.122	0.255	0.023	-0.062	-0.021	0.030
	(0.337)	(0.198)	(0.107)	(0.188)	(0.100)	(0.201)	(0.095)	(0.168)
Mean of dep var, %	25.65	10.77	4.63	8.63	3.12	12.29	3.35	8.08
Daughters, $N = 405,710$								
Reform	-0.067	0.089	0.048	-0.045	0.082	-0.024	0.050	-0.055
	(0.147)	(0.066)	(0.040)	(0.102)	(0.051)	(0.063)	(0.065)	(0.050)
Mean of dep var, %	8.11	1.55	0.63	4.13	0.75	1.63	1.54	0.91

Notes: This table shows results from the difference-in-difference model in eq. 1, for sons and daughters of the child generation (with parents born 1945-55) born 1960-93, on convictions at ages 15-29. Results are in percentage points. †**Any convictions**: having ever been convicted. ††**Multiple Convictions**: two or more convictions, at different times. †††**Other** see footnote 19. Appendix Table 10 gives all crime definitions. For crime categories, the counterfactual is "not that crime", whether that is no conviction or some other conviction. All regressions include a full set of father's or mother's birth municipality and birth cohort indicator variables, and grandfather's education levels. **Robust standard errors**, clustered by municipality of father's/mother's birth ($N = 981$), in parentheses. In the top panel, **p-values** are in square brackets. In columns 3-8, these are **adjusted for multiple testing** for 6 hypotheses using the Romano and Wolf (2005) step-down approach, separately for sons and daughters of treated fathers. When mothers are treated, no impacts are significant, and therefore we don't report p-values. *, ** and ***: p-value <0.1, <0.05 and < 0.01 respectively reflecting the p-values given in the square brackets for fathers, and standard p-values for mothers.

Next, we break down the overall conviction rate into the six types of crime reported in Table 2. To adjust for multiple testing, we report stepdown p-values as proposed by Romano and Wolf (2005) (RW).[18] The decline in crime among sons is present and significant across the board, with the exceptions of property crimes and drug crimes (use and distribution). Specifically, violent crime declined by 0.27pp (RW p-value 0.09), traffic-related crimes by 0.45pp (RW p-value 0.057), fraud by 0.22pp (RW p-value 0.09) and "other" crimes by

[18]In each case, the outcome is one if a conviction for that type of crime took place and zero in all other cases.

0.41pp (RW p-value 0.047).[19] The percentage reductions relative to the mean incidence of these four types of crime are similar and lie between three and six percent. The 95 percent confidence intervals for "Property" and "Drugs" do not exclude an equivalent drop. Finally, it is worth pointing out that, on the basis of conventional single-hypotheses testing, all effects are highly significant, other than property crimes and drug-related crimes.

For women in the child generation, the impact of fathers' reform exposure is much smaller, which is not surprising given that their baseline crime rate is one-third of the male average. However, we find a reduction in the category "Other crimes" by 0.129 pp, representing a 15.5 percent reduction, with an RW stepdown p-value of 0.052 (and a conventional single hypothesis p-value of 0.006). Again, we find no effects on criminal participation among daughters from mothers' exposure to the schooling reform.

In addition, we have estimated the effect of parental exposure to the reform on prison sentences but find a small and insignificant effect for sons of fathers exposed to the reform (-0.015 pp, st. error 0.093).[20] Prison in Sweden is reserved for the more serious cases. As we documented in Table 2, only 10.26 percent of the convictions for men and 1.40 percent for women carry a prison sentence in the child generation[21] One interpretation of this finding is that the reform affected mainly offenses not classified as meriting incarceration. However, it may also be an issue with power to detect very small effects.[22]

In summary, our central finding is that exposure of fathers to the educational reform led to large and significant reductions in their sons' crime rates. Next, we examine the robustness of these results and then present evidence on the mechanisms driving these improvements.

[19] "Other crimes" include defamation, family law, vandalism, hazardous general crimes, crimes against public order, violation of general business, crimes against national security, misconduct, environmental law, alcohol law, weapon and knife law, immigration law, copyright law, working environment law, visiting rights law (harassment of ex-spouse, etc), privacy and data protection.

[20] There is also no effect on daughters. In the parent generation, the effect is negligible: -0.149 percentage points, st. error 0.160.

[21] This was higher in the parent generation, where 19.65 percent of convictions for men and 5.77 percent for women carried a prison sentence.

[22] The impact of the reform on incarceration conditional on conviction is small and statistically insignificant (–0.0658 pp, st. error 0.346). We cannot exclude the possibility that the lack of significant effects of the reform on incarceration is due to the fact that the effects are too small to detect with our sample size and research design.

Finally, we use our empirical evidence to shed light on why exposure of mothers to the reform did not yield similar reductions in child crime.

6.2 Robustness Checks

The key assumption underlying our empirical approach is that the growth in crime rates between cohorts would have been the same across all municipalities in the absence of the reform. In online Appendix section A.4, we present in detail three alternative tests that strongly support this parallel trends assumption. We summarize the conclusions here. First, the impacts are unchanged when including municipality-specific linear trends (p-value 0.85), and the linear trends themselves are not significant (p-value 0.23). This is also true for the parent generation directly exposed to the reform. Second, we carry out Placebo tests and show that the impacts are all indistinguishable from zero unless we use the correct date of the reform in each municipality. Finally, we show graphically that the residuals from the crime regressions are not correlated with municipality-specific trends. All approaches lead to the same conclusion and provide strong support for our empirical approach.

Finally, in online Appendix Table 15 we present alternative estimates of Table 4 for men, based on the Callaway and Sant'Anna (2021) approach that allows for heterogeneous impacts in the staggered Difference-in-Differences method. The key difference is that these alternative results are less precisely estimated, but none of the substantive conclusions change, and there are no significant differences between the two sets of results.

7 Mechanisms

Section 4 identified three interrelated channels through which the reform could influence criminal behavior in the next generation: the "human capital channel", the "role model channel", and the "neighborhood and peer effect channel". To provide evidence on these, we first show that parental, and in particular father, human capital indicators, labor market

outcomes, and household income improved. We then show that indicators of child human capital also improved, consistent with the idea that parents with higher human capital invest more in their children, as illustrated by the model in the online Appendix Section A.1. We complete the argument by showing that children of fathers treated by the reform were raised in lower-crime neighborhoods and schools, which, added to the fact that parental crime declined, is evidence that the children had better role models and peers. Finally, we address the question of why we do not see similar impacts when we consider mothers exposed to the reform. We complete our discussion with a mediation analysis that, subject to key assumptions, quantifies the relative importance of the main factors: parental education, parental participation in crime, and parental household income.

7.1 Intergenerational Transmission of Human Capital

7.1.1 Reform Effects on Parent Human Capital

The schooling reform increased educational attainment and earnings, as documented for the 1948 and 1953 birth cohorts in Meghir and Palme (2005). In this section, we expand the set of results that reflect human capital and consider the impact on all affected cohorts. We also look at marriage market outcomes, since better matches represent a channel for improving the home environment.

Table 5 shows the estimated effects on human capital outcomes in the parental generation. For men, education increased by 0.32 years (p-value 0), earnings increased by 0.8 percent (p-value 0.013), and the probability of becoming a white collar worker increased by 1.2 percent (p-value 0). This is important because white collar jobs imply higher income, more stable employment (Medlingsinstitutet, 2023), and better amenities, including more generous income insurance programs, which affect economic resources throughout the life cycle.

For men we can also consider impacts on cognitive and noncognitive skills, from military enlistment tests. The cognitive skills score is the average of four separate tests: synonyms,

Table 5: Reform Effect on Parent Human Capital Outcomes

	(1) Fathers	(2) Mothers	p-value (1)-(2), %
Own Education, years	0.319***	0.206***	< 1
	(0.024)	(0.022)	
Spouse's Education, years	0.045**	0.044*	97.9
	(0.022)	(0.023)	
Household Average Education, years	0.211***	0.110***	< 1
	(0.024)	(0.021)	
Log Earnings age 40-50 (%)	0.801**	0.027	< 1
	(0.321)	(0.295)	
Work age 40-50 (%)	0.128	-0.171	< 1
	(0.112)	(0.123)	
Spouse's Log Earnings age 40-50 (%)	0.939**	0.420	54.1
	(0.402)	(0.393)	
Log Household Income age 40-50 (%)	0.693**	-0.387	< 1
	(0.306)	(0.294)	
Log Household Income (%) child age 0-5	1.021***	-0.323	< 1
	(0.344)	(0.332)	
Log Household Income (%) child age 12-18	0.748**	0.113	22.2
	(0.381)	(0.352)	
Married	0.002	-0.005*	9.9
	(0.003)	(0.003)	
White Collar Worker	0.012***	0.002	1.8
	(0.003)	(0.003)	
Spouse White Collar Worker	0.004	-0.002	23.0
	(0.004)	(0.003)	
Cognitive Skills (prop of STD)	0.144**	-	
	(0.054)		
Non-Cognitive Skills (prop of STD)	0.171***	-	
	(0.077)		

Notes: This table shows results from the difference-in-difference model in eq. 1, for men and women of the parent generation (born 1945-1955) with children born 1960-93, i.e. *parents* only. Earnings for parents and their spouses are estimated on income panels for ages 40 to 50. Baseline earnings (SEK 2010): 244,338 for fathers, 182,695 for mothers, 182,695 for fathers' spouses, and 244,338 for mothers' spouses. Work is an indicator for non-zero earnings at ages 40 to 50 (mean is 88% for fathers, 85% for mothers). The three measures of household income are calculated as the mean total income of the parent and their spouse over age 40 to 50, child age 0 to 5, and child age 12 to 18, respectively. This includes earnings and transfers. Married is an indicator for being married to the other biological parent (the mother for fathers and *vice versa*) when the child is 5 years old. White collar worker is an indicator variable for white-collar professions (including business owners but excluding farmers) in 1985 or 1990 (whichever is closest to age 40). Cognitive and non-cognitive skills are only available for men through military enlistment tests. Standard Errors clustered at the municipality level ($N = 981$). *** Significance at 1% , ** Significance at 5% , * Significance at 10%.

induction, mental folding, and technical comprehension. The noncognitive skills score is assigned by a certified psychologist based on a 25-minute interview. The objective of the score is to measure the ability of the conscript to meet the psychological requirements for military service and, ultimately, to serve in war (see, for example, Lindqvist and Vestman, 2011, for more details).[23] Our results show that there was a substantial increase in cognitive and non-cognitive skills, measured at military enlistment: 14 percent of a standard deviation (SD) for cognitive (p-value 0.008) and 17 percent of a SD for non-cognitive (p-value 0.03).

Finally, the marriage market for men exposed to the reform improved, with their spouses earning significantly more (0.9 percent per year, p-value 0.02).[24] This is consistent with positive assortative matching and an improvement in the prospects of the marriage market of men exposed to the reform, resulting from improved human capital (Chiappori, Costa-Dias, and Meghir, 2018). All this adds up to a highly significant increase in household income for men exposed to the reform, at the prime working age of 40 to 50 (Haider and Solon, 2006), as well as in key formative periods of their children's lives: ages 0-5, when higher income has been shown to improve child outcomes (Dahl and Lochner, 2012; Carneiro, García, Salvanes *et al.*, 2021), and 12-18, when children first run the risk of involvement in criminal activity. The strongest effect is found when their children are aged 0 to 5, at approximately 1 percent (p-value 0.003).

For mothers exposed to the reform, the effects are weaker. Education increased, albeit by one-third less than the estimated effect for fathers (0.21 of a year, p-value 0). Their marriage market matches in terms of schooling also improved, with their husbands being *slightly* better educated (by 0.044 of a year, p-value 0.057). However, earnings, spouse's

[23] Reform effects on cognitive and non-cognitive skills are also reported in Lager, Seblova, Falkstedt *et al.* (2017). The results are restricted to men because the data come from enlistment tests and compulsory military service applied only to men in the birth cohorts under study. During our sample period, almost all men without obvious physical disabilities were conscripted. The Swedish conscription test takes place when men turn 18 or 19 years old.

[24] Zero earnings make up only 0.4% of the income panel at ages 40-50 for spouses of fathers in the reform cohorts. If we instead calculate mean earnings at 40 to 50 and estimate the DiD model on the log of the mean, the point estimates are very similar to those from the income panel.

earnings, and household income did not improve.[25] Consistently, mothers were not more likely to become white-collar workers. Finally, there is some evidence that they were less likely to be married to the father of their children when the child was five, although the effect is not strongly significant (-0.5 percent, p-value 0.1). Thus, although we see effects on women exposed to the reform, they are muted and do not lead to increased household resources.

One contributing factor is that pre-reform compulsory schooling was less binding for women. For women in the 1945-55 cohort, only 13% were below the compulsory level, compared to 20% of men. The overall average level of education for women was 0.34 years higher than that of men, and on a stronger trend: the average growth in education for women for these birth cohorts was 0.56 years, compared to 0.36 years for men (in the untreated municipalities between cohorts 1945 and 1955). In addition, growth stalled to zero for men born after 1949, while rapid growth continued for women for at least another two cohorts. Thus, the scope for reform-induced gains in both education and earnings was limited, consistent with smaller observed effects.

All these results point to a strong effect of the reform on fathers' human capital and a clear indication that home environments improved and household resources increased for children of exposed fathers, which can lead to higher investments in children. The implication is that child human capital should also have increased. We now turn to this.

7.1.2 Effects of Fathers' Exposure to the Reform on Children's Human Capital

In Table 6 we begin by examining the educational performance, health, and skills of the sons and daughters with fathers in the reform cohorts. Panel A shows that fathers' exposure to the reform increased the GPA of sons at the end of compulsory schooling by 1.2 percentiles (RW p value 0.05), or roughly 0.04 standard deviations. There is no statistically discernible

[25] In the income panel at ages 40 to 50 that we use to estimate these results, zero earnings among mothers only make up about 0.7 percent. If we instead calculate mean earnings at 40 to 50 and estimate the DiD model on the log of the mean, the point estimates are very similar to those from the income panel.

effect for daughters and we find no evidence of increased years of schooling for either gender.

Panel B shows significant impacts on health among sons, with a reduction in hospitalization by 2.4 days (RW p-value 0.02) or 17 percent, and in prescribed drugs by 31 doses (RW p-value 0.09), representing a 2.8 percent decline. These health gains are consistent with the intergenerational impacts of the Perry Pre-School Project documented by Heckman and Karapakula (2019), suggesting that educational interventions affecting parents can have durable health benefits in the next generation. Like us, they find that effects are concentrated among the sons of exposed fathers.

In online Appendix Table 12 we break down the prescription drugs into various categories and find that the overall reduction among sons is driven by a decline in the use of nervous system drugs (RW p-value 0.055) and specifically painkillers (RW p-value 0.014), pointing to a possible improvement in mental health. For daughters, we also find a reduction in prescribed drugs driven by a decrease in drugs for the respiratory system (RW p-value 0.012), which could be pointing to a decline in smoking, and to drugs that have a "calming effect" (RW p-value 0.038), again pointing to improved mental health.

Finally, in Panel C of Table 6 we present the results of cognitive and non-cognitive skills in the generation of children, which are available only for men.[26] Cognitive skills improve by about 14 percent of a standard deviation with a p-value for a single hypothesis test of 0.067, which is consistent with the other improvements. However, based on Romano-Wolf step-down p-values for the entire Panel C, the impact is not significant.[27]

We now turn to longer-term labor market outcomes for the child generation, measured at 40 years of age and presented in Table 7.[28] We find that men's employment, defined as positive labor income, increased by 2.3 percentage points (RW stepdown p-value 0.04), and unemployment, measured as receipt of unemployment benefits, declined by approximately

[26] For details on how these test scores were obtained, see Section 7.1.1.

[27] Lundborg, Nilsson, and Rooth (2014) presents similar evidence for children of cohorts of parents different from ours, including early cohorts, barely affected by the reform.

[28] See Haider and Solon (2006) for a motivation of our choice of age group for evaluating labor market outcomes.

Table 6: Effects of Fathers' Reform Exposure on Children's Skills, Education, and Health

	Sons	Daughters
Panel A: *Education*		
GPA‡‡ last year of compulsory schooling (percentile)	0.012**	0.002
	(0.0055), [0.050]	(0.0058), [>0.5]
Years of schooling	0.015	0.019
	(0.012), [0.198]	(0.013), [0.178]
Panel B: *Health*		
Hospitalization, days over 1987-2015	-2.40**	-0.21
	(0.81), [0.02]	(0.78), [>0.5]
Average hospitalization days	14.22	24.04
All prescribed drugs, total daily doses in 2006-2015	-30.711*	-7.623
	(14.417), [0.089]	(20.31), [>0.5]
Average doses 2006-2015	1,079.9	2,581.3
	Sons	
Panel C: *Cognitive and noncognitive skills*‡		
IQ, verbal test	0.139†	
	(0.076), [0.396]	
IQ, number series test	0.020	
	(0.066), [>0.5]	
IQ, spatial	0.141	
	(0.105), [0.446]	
IQ, mechanical	-0.001	
	(0.083), [>0.5]	
Noncognitive test	0.0065	
	(0.018), [>0.5]	

Notes: This table shows estimated effects from the difference-in-difference model in eq. 1, for sons and daughters of the child generation born 1960-93 and with fathers born 1945-55, on outcomes listed in each row. Only fathers' reform exposure is considered. Standard errors clustered at the municipality level in round brackets ($N = 981$). Romano Wolf step down p-values adjusting for multiple testing, separately for sons and daughters and for each group (Cognitive and noncognitive skills (5), Education (2), Health (2)) in square brackets. *** significance at 1% ** Significance at 5% and * Significance at 10% all based on the RW p-values. †Significant at 10% with single hypothesis test but not with multiple testing. ‡Cognitive and noncognitive tests are administered to conscripts, who are all male. Here they are measured in standard deviation units. The cognitive scores are from IQ-type tests; the noncognitive scores are from psychological assessments. No such tests are available for women ‡‡GPA: grade point average awarded centrally.

Table 7: Effects of Fathers' Reform Exposure on Children's Labor Market Outcomes

	(1) Sons	(2) Daughters
Annual Earnings, SEK	11,494	5,240
	(6,554), [0.287]	(4,769), [0.495]
Employment (pp)	2.28**	1.22
	(0.89), [0.040]	(1.37), [0.960]
Unemployment (pp)	-1.87**	0.01
	(0.72), [0.040]	(0.83), [0.980]
Welfare receipt (pp)	-2.22	-2.57**
	(1.10), [0.190]	(1.01), [0.050]
N	23,353	22,668

Notes: This table shows estimated effects from the difference-in-difference model in eq. 1, for sons and daughters of the child generation born 1960-93 and with fathers born 1945-55, on outcomes listed in each row. All outcomes are measured at age 40 of the child. The independent variable is fathers' reform exposure. **pp** is percentage points. Data for the period 2000-2010. Fixed effects for *Year*, *Year of birth* (of the father and child) and *Municipality* included in the specification. SEK measured in 2000 prices. SEK 11,494 and SEK 5,240 correspond to 1,546 and 705 2023 US $, respectively. Standard errors are clustered at the municipality level in round brackets ($N = 981$). Romano Wolf stepdown p-values for four hypotheses for Sons and Daughters separately in square brackets. ** Significance at 5% and based on the RW stepdown p-values.

the same amount (RW p-value 0.04). In the same direction, there is an increase in annual post-tax earnings of SEK 11,500 and a decline in welfare dependency. Both are individually significant at the 10% and the 5% levels, respectively, but not when we adjust for multiple testing. For women, only the decline in welfare dependency is significant.

Taken together, exposure of the father to the reform caused a significant improvement in test scores, health, and labor market outcomes of their sons. This pattern aligns closely with the reductions in crime documented in Table 4 and with the predictions of our model in Online Appendix Section A.1. We now turn to additional channels through which these intergenerational effects may operate.

7.2 Role Models and Peer Effects

Parental Crime and Role Models. Parental involvement in crime can encourage similar behavior in their children through a role model channel. We therefore consider a potential causal impact of the reform through reduced criminal behavior of fathers, which in turn influences children.[29]

In online Appendix Table 11, we show the effects of the reform on crime in the *parent generation*. The analysis is carried out for the subset of people born in the period 1952-55 for whom we have criminal records at the age when most crimes are committed. For these results, we include all adults, regardless of whether they have children. For men, the reform significantly reduced the incidence of a criminal conviction by 1.5pp, and, importantly, it also significantly reduced the incidence of repeat convictions by 1.5pp from a lower base, representing a decline of about 8 percent. For women, we find no impacts, but this is not surprising, given that their conviction rate at baseline is about a third of that of men. These results confirm earlier findings on the impact of compulsory schooling reforms on crime in the US (Lochner and Moretti, 2004), in the UK (Machin, Marie, and Vujić, 2011), and in Sweden using the same reform (Hjalmarsson, Holmlund, and Lindquist, 2015) and offer a

[29]Strong intergenerational associations in criminality have been documented in Table 3 and Hjalmarsson and Lindquist (2012).

potential mechanism through the direct influence of parental behavior on children.

We also investigated the impact of the reform on other potential mediators associated with worse child outcomes, including drug and alcohol abuse by the parent (as recorded in hospital registers), as well as on teen births, the number of children, and the age at first birth. These outcomes were not affected by the reform (see online Appendix Table 13).

Peer Group and Neighborhood Effects. The improved educational and income status of fathers may have affected the residential location and, in turn, the quality of the neighborhood and school, and thus the peer group of the children during the formative years of upbringing.

Table 8: Effects of Fathers' Reform Exposure on Children's Peer Crime Rate

	(1) Fathers	(2) Mothers
Panel A: Crime rate in neighborhood		
Reform effect (%)	-0.0869**	0.0809*
	(0.0360)	(0.0432)
Constant (conditional mean untreated group, %)	17.08	17.69
Panel B: Crime rate in schools		
Reform effect (%)	-0.0771**	0.0851**
	(0.0333)	(0.0335)
Constant (conditional mean untreated group, %)	16.52	17.28
Number of observations	666,054	465,543

Note: This table shows estimated effects from the difference-in-difference model in eq. 1, for sons and daughters of the child generation born 1960-93 and with fathers born 1945-55. In Panel A, the outcome variable is the crime rate among youth aged 15-20 in the neighborhood, defined as the same type of housing in the same church parish (SAMS area), where the family resides when the child is aged 16. In Panel B, the outcome variable is the crime rate, measured between age 15 and 29, among same-aged schoolmates of the child during the year when the child attends 9th grade (age 15). For both measures, we use the leave-one-out means when calculating the conviction rates. Column 1 shows the effect of having a father exposed to the reform and column 2 shows the effect of mothers' reform exposure. Clustered standard errors at the parents' municipality of birth ($N = 981$) in parentheses. ** Significance at 5% , * Significance at 10%.

Although there is some overlap between these two measures of peer crime exposure,[30]

[30]The correlation coefficient in the full sample is 0.49. The note to Table 8 describes in detail how these

peer exposure in the residential area and in school capture distinct dimensions. Crime in the residential neighborhood can affect the entire family, potentially generating both direct and indirect effects on the child's criminal participation, while exposure in school affects only the child directly. The neighborhood crime measure reflects the conviction rate among people aged 15–20 in the year the child turns 15 – predominantly older peers – while the school crime measure reflects the conviction rate among grade 9 classmates for offenses committed between ages 15 and 29.

The results, presented in Table 8, show that the children of fathers affected by the reform were exposed to improved peer environments, measured by lower youth crime rates in both their neighborhoods of residence and their schools of attendance. This suggests that a secondary effect of the school reform was that treated fathers moved to neighborhoods where their children attended schools with lower crime rates.

The picture for the children of mothers exposed to the reform is different: The estimated effects show higher crime rates in the neighborhoods where the mothers treated by the reform live (p-value 0.06), as well as in the schools their children attend (p-value 0.01).

In Summary, the exposure of fathers to the reform improved the environment in which children were raised in various ways and made fathers better role models for their sons. The fathers were less involved in crime, and the probability that they raised their children in better neighborhoods and placed them in schools with lower rates of youth crime increased. These impacts add to the potential channels through which the educational reform reduced crime in the child generation.

7.3 Exposure to the reform of Fathers versus Mothers

We find that exposure of fathers to the education reform led to a measurable reduction in criminal participation among their sons (see Table 4). In contrast, maternal exposure had

measures were obtained.

no discernible effect, despite the fact that both maternal and paternal education levels are associated with child crime outcomes in the descriptive analysis (see Table 3).[31]

There are two broad explanations for this asymmetry. First, the reform may have had quantitatively and qualitatively different effects on men and women. Second, even when educational gains are similar, downstream consequences – such as occupational sorting, income trajectories, and neighborhood choices – can diverge, producing heterogeneous impacts on children. These mechanisms may operate jointly and reinforce each other.

We illustrate the above with four sets of empirical findings. (i) As shown in Table 5, men exposed to the reform experienced a higher increase in schooling than women (0.319 vs 0.206 years), and average household education increased more when fathers were treated (0.211 vs 0.110). (ii) Treated fathers experienced significant increases in labor earnings and household disposable income during key developmental stages of their children (ages 0–5 and 12–17). No comparable effects are observed for treated mothers. (iii) Treated fathers – but not mothers – were more likely to enter white collar occupations. (iv) Treated fathers were more likely to raise their children in lower-crime neighborhoods and send them to schools with fewer criminally involved peers; the opposite pattern holds for treated mothers (Table 8).

As discussed in Section 4, the education reform can affect child crime through three broad channels: human capital transmission, role modeling, and neighborhood effects.

Human Capital Channel. The Becker–Tomes model predicts that income gains for parents increase investments in children's human capital, thereby raising the opportunity cost of criminal activity. Several studies provide direct evidence consistent with this mechanism (Akee, Copeland, Keeler *et al.*, 2010; Bailey, Hoynes, Rossin-Slater *et al.*, 2024), and a large literature documents substantial effects of conditionally exogenous income increases

[31]Since the lower limit for the 95-percent confidence interval for sons of treated mothers is at -0.753, almost at the point estimate of the estimate for sons of treated fathers, sizable effects could, however, not be excluded in this group. Equal effects in the two groups could only be rejected at the 5 percent level (p-value 0.037).

on the development of child skills (see Section 4). At the same time, two studies exploiting lottery-based variation in parental income do not detect effects on crime (Jacob, Kapustin, and Ludwig, 2014; Cesarini, Lindqvist, Östling *et al.*, 2023). Taken together, the evidence suggests that differences in household resources can shape intergenerational human capital formation, and may help account for the asymmetries we document.

Beyond income, parental education can affect child outcomes through productivity-enhancing effects on parenting, as emphasized in extensions of the Becker–Tomes model (Heckman, 2007; Cunha, Heckman, and Schennach, 2010). Our theoretical model in online Appendix A.1 formalizes this idea by incorporating an investment channel, whose productivity depends on parental education. In this framework, the larger educational and household income gains observed among treated fathers, relative to treated mothers, are consistent with stronger intergenerational effects through both the resource and productivity channels. Similar results are found in Heckman and Karapakula (2019).

Role Model Channel. Parental behavior can shape child outcomes through normative and aspirational pathways. Previous work documents substantial intergenerational persistence in crime (Hjalmarsson and Lindquist, 2012), and studies in other domains find causal evidence of behavioral transmission across generations (e.g., Dahl, Kostøl, and Mogstad, 2014). In our setting, the reform increased the likelihood that fathers entered white-collar jobs, altering not only their opportunity cost of criminal behavior but also the peer groups and work environments to which they were exposed. These changes likely shifted paternal norms and behaviors in ways that were observable and influential for children, particularly sons. This effect is absent when mothers were exposed to the reform.

Neighborhood Channel. Treated fathers were significantly more likely to relocate to lower crime neighborhoods and send their children to schools with lower peer exposure to crime. These changes were not observed for treated mothers. As reviewed in Section 4, a growing literature has identified robust causal effects of neighborhood and school quality on

youth behavior, including crime (see discussion and references in Section 4). Our results suggest that these locational and institutional differences, induced when the father was treated, are likely to be an important mediating mechanism. They are absent when the mother was exposed to the reform, and indeed, the results show a worsening of neighborhoods for treated mothers.

In summary, our results suggest that paternal exposure to the reform translated into improved outcomes for sons through multiple and complementary pathways: higher educational gains, increased household income, occupational enhancement, and improved neighborhood environments. These changes are consistent with the magnitude of the effects reported in the existing literature. In contrast, maternal exposure generated smaller shifts along each of these margins. The differential effect of the reform on child crime is thus best understood as the result of a bundle of interrelated changes – most of which were triggered when fathers, rather than mothers, were treated.

7.4 Mediation Analysis

To quantify the relative importance of the mechanisms underlying the intergenerational effects of the reform, we perform a mediation analysis in the spirit of Heckman, Pinto, and Savelyev (2013).[32]

We consider four mediating variables corresponding to alternative theoretical mechanisms: (i) a direct effect of the father's years of schooling; (ii) a role model channel, proxied by the father's criminal convictions; (iii) a resource channel, proxied by household disposable income when the father is 45-49 years old; and (iv) the years of schooling of the mother, which reflects the improved marriage market of the fathers and could have contributed to better quality parenting and home environment.[33]

[32]See also Heckman and Pinto (2015).

[33]We exclude child human capital as a mediator, since it is jointly determined with criminal behavior. We also exclude mothers' crimes from the mediation analysis: There is no statistically significant effect of fathers' reform exposure on mothers' crime (point estimate 0.346, se 0.819), i.e., fathers exposure to the

The sample for this analysis is restricted to fathers born between 1952 and 1955, with sons observed from ages 15 to 29 (that is, born in or before 1981). The observation window runs from 1973, the first year of data on criminal convictions, to 2010. This ensures a near-complete measurement of criminal outcomes for both generations and retains variation in reform exposure.

We first estimate the effect of reform exposure in this sample on each mediator based on equation 1. For convenience, we rewrite it here, replacing the generic notation for the dependent variable y_i with M_i^k denoting the kth mediator:

$$M_i^k = \beta^k R_i + \gamma_1' t_i + \gamma_2' m_i + \theta' X_i + \varepsilon_i^k, \quad k \in \{\text{FS, MS, crime and household income}\}, \quad (2)$$

where R_i is an indicator of *father's* exposure to the reform, FS and MS represent father's and mother's schooling, respectively, X_i includes controls for the father's parental background (specifically, grandparent's education), and t_i and m_i denote fixed effects for the birth cohort and municipality, respectively.

Next, we estimate the reduced-form effect of the reform and the conditional effects of the mediators on child crime outcomes using the following model:

$$y_i^{cr} = \delta_0 + \delta_1 R_i + \delta_2 M_i^{\text{FS}} + \delta_3 M_i^{\text{MS}} + \delta_4 M_i^{\text{crime}} + \delta_5 M_i^{\text{log income}} + \theta' X_i + \gamma_1' t_i + \gamma_2' m_i + \nu_i, \quad (3)$$

where y_i^{cr} is an indicator equal to one if the child is convicted of a crime between ages 15 and 29.

A causal interpretation of this mediation analysis requires an assumption that the mediators M_i^k are conditionally exogenous given the included controls. Further, the interpretation of each of the variables we include as representing the channels we posit requires a tight link between the measure and the underlying latent concept (role model, human capital, etc.). Otherwise, we have an errors in variables problem (Cunha, Heckman, and Schennach, 2010),

reform does not affect marital sorting on criminal behavior.

whereby the results need to be interpreted with caution. With these caveats in mind, we proceed with the analysis.

Table 9: Mediation Analysis Based on Fathers' Reform Exposure

Panel A. Reform Effect on Mediators

	Fathers' Education (1)	Mothers' Education (2)	Fathers' Crime (3)	Log Household Income (4)
Father's Exposure to School Reform	0.316***	0.123*	-1.269	1.177*
	(0.046)	(0.061)	(0.783)	(0.689)
Number of observations	152,662	70,896	153,311	149,827

Panel B. Dependent Variable: Child Conviction (Sons)

	(1)	(2)	(3)	(4)	(5)
Father's Exposure to School Reform	-1.090**	-0.433	-0.306	-0.186	-0.487
	(0.522)	(0.527)	(0.778)	(0.789)	(0.808)
Father's Years of Schooling		-2.019***	-1.393***	-1.132***	-0.801***
		(0.052)	(0.068)	(0.063)	(0.064)
Mother's Years of Schooling			-1.273***	-1.175***	-0.998***
			(0.072)	(0.070)	(0.070)
Father's Crime				10.4***	8.9***
				(0.359)	(0.366)
Log Household Income					-6.27***
					(0.395)
Number of observations	153,311	153,311	70,728	70,728	69,205

Notes: This table presents the results of the mediation analysis. Panel A shows estimated effects of the regression model in eq. (2) on outcomes listed in each column. Panel B shows estimates from eq. (3), where the dependent variable is a binary indicator equal to one if the child is convicted of any crime at age 15-29. Sample restricted to fathers born 1952–1955 and their sons born 1960-1993. Father's and mother's years of schooling refer to the highest observed level of education. Father's crime is an indicator for any conviction at age 15-65. Household income is log disposable income measured when the father is between 40 and 50 years old. All regressions include fixed effects for grandfather's education, and father's cohort and municipality of birth. Standard errors clustered at the municipality level ($N = 981$) in parentheses. All coefficients for binary outcomes or logged variables are scaled by 100 and interpreted as percentage point effects. *** Significance at 1%, ** Significance at 5% and * Significance at 10%.

Table 9 presents the results. Panel A shows that exposure to the reform increased the father's years of schooling by 0.32 years (column 1), and, through assortative matching,

increased the mother's schooling by 0.12 years (column 2).[34], [35] Column 3 shows a reduction in paternal criminal convictions of 1.3 percentage points, while column 4 indicates an increase in disposable household income of 1.2 percent.

Panel B examines the extent to which these mediators account for the effect of the reform on child crime. In column 1, the reduced-form estimate implies a 1.1 percentage point decline in criminal convictions. Adding the father's years of schooling (column 2) reduces the reform coefficient to less than half (-0.43) and makes it statistically insignificant. Each additional year of schooling for the father is associated with a 2 percentage point decline in child crime. In column 3, including the schooling of both parents reduces the impact of the father's education alone and shows an approximately equal influence of mother's education. Column 4 shows that paternal crime is positively associated with child crime, with a magnitude of approximately 10 percentage points. Interestingly, it has only a small impact on the size of the education coefficients. Finally, in column 5, we add household income. This has a strong impact and reduces the two education coefficients further. However, importantly, this final column shows that there is a role for all mediators we have included. Resources matter, but so do paternal crime and the education of both parents.

Using the estimates from column 5, we decompose the total effect as follows:

$$\text{Total effect} = 1.06 = \underbrace{-0.487}_{\text{Direct}} - \underbrace{0.801 \times 0.316}_{\text{FS}} - \underbrace{0.998 \times 0.123}_{\text{MS}} - \underbrace{8.9 \times 0.01269}_{\text{Crime}} - \underbrace{6.27 \times 0.0118}_{\text{Income}},$$

[34] This effect reflects an improved marriage market for men exposed to the reform, presumably because they are now more attractive to better educated women and because the reform increased the proportion of such women. The effect is bigger than the one recorded in Table 5 because the sample is different as described above. For these later cohorts, the abundance of better educated women increased, which improved the chances of an improved match for men. However, the differences are not significant.

[35] A comparison of columns 1 and 2 reveals a substantially smaller number of observations in column 2. This discrepancy arises from differences in sample construction. The main analysis is based on a data set comprising all men and women born between 1945 and 1955. In contrast, the mediation analysis is restricted to men born between 1952 and 1955. Consequently, if a spouse was born after 1955 – as is the case for a sizable proportion – she is not included in the main dataset and is therefore unobserved in the mediation sample. To assess the sensitivity of our findings to this restriction, we replicate all analyses in Table 9 using the reduced sample. The corresponding estimates are reported in online Appendix Table 16. Reassuringly, the results are highly consistent with the main findings, although, as expected, the estimates are somewhat less precise due to the smaller sample size.

The decomposition implies that 46 percent of the total predicted effect in column 5 (1.06) is direct (although it is very imprecisely estimated and not significantly different from zero), while the remaining 56 percent is mediated. Among the indirect components, father's education accounts for 45 percent of the mediated effect, the increase in spousal education (mother's) accounts for 21 percent, the decline in father's crime participation accounts for 20 percent, and improved household resources account for 13 percent.

Returning to the asymmetry between paternal and maternal exposure, the mediation results are fully consistent with our broader findings: maternal education is indeed an important channel in principle, but only paternal exposure to the reform generated sufficiently large and complementary changes, in parental education, parental household income, and parental crime, to produce detectable intergenerational effects. Moreover, the resulting increase in average parental education is only about half as large when the mother is exposed as when the father is (0.110 vs. 0.211 years).

Together, the results indicate that the intergenerational impact of the reform on criminal behavior operates through a variety of channels. Although resources play an important role, mediators linked to improved parenting quality and role model effects also play a central role, as implied by the recent literature on child development, referenced in Section 4. Of course, these interpretations require some caution, given the assumptions underlying the mediation analysis.

8 Concluding Remarks

The comprehensive school reform implemented in Sweden during the 1950s and 1960s increased educational attainment, enhanced a broad range of human capital measures, and reduced criminal participation among those exposed, particularly men. In this paper, we show that the reform also led to a large and statistically significant reduction in crime among the male children of fathers who were exposed. Given the substantial social costs of crime,

this represents a significant and policy-relevant benefit of the reform. The decline in crime is consistent with several other positive second-generation effects that we document, including improved test scores, employment rates, and adult health outcomes, demonstrating that the reform had lasting intergenerational benefits.

We then explore the mechanisms through which the reform led to intergenerational improvements. We identify theoretically three channels: improved resources in the households where children were being brought up, direct effects from having fathers with higher human capital – possibly because of the complementarity of education and child investments – and a role-model effect, whereby lower criminal behavior among fathers leads to a better example for the children. We find strong evidence for all channels: household resources improved, fathers acquired more education and showed improved cognitive and noncognitive skills, were more likely to become white collar workers and less likely to be involved in crime, were more likely to live in neighborhoods with lower youth crime, and moved their children to schools with better peers.

Our mediation analysis also suggests that each of these three channels is important in its own right when fathers are concerned. In contrast, the results for mothers' exposure to the reform show no human capital improvements beyond a small effect on schooling for mothers themselves and no second-generation effects. Together, these facts suggest that multiple channels affect child outcomes and specifically child criminal behavior: household resources, parental education (proxying for parenting quality), and paternal criminal behavior (a role model channel). In this sense, our findings are consistent with extensions of the seminal framework of Becker and Tomes (1979), such as Heckman (2007) and Cunha, Heckman, and Schennach (2010), which allow for a distinct role of parental human capital, directly or in interaction with economic resources, in shaping child human capital and influencing second-generation participation in crime.

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A APPENDIX FOR ONLINE PUBLICATION

A.1 Theoretical Background

We consider two channels through which the educational reform could have reduced crime in the subsequent generation: one is through improvements in human capital; the other is through a direct influence of father's criminal behavior on the child.

We start by discussing the human capital channel. We present the chain of events backwards, by showing how an increase in human capital can reduce crime. We then consider the problem of the parent generation and show how the policy reform can lead to an increase in child human capital through increased parental investments. The key ideas draw on Becker and Tomes (1979) and on **?**. To preserve simplicity we use explicit specifications for preferences and the child human capital production function that can reflect key empirical findings from the literature.

A.1.1 Crime in the Child Generation

Denote child human capital by h, which we measure in monetary units, having normalized its price to one, without loss of generality. If a person does not participate in crime they obtain utility $V_h = F(h)$, where $F(h)$, is an increasing and concave function of h. To participate in crime an individual gives up $1-\alpha$ fraction of their lawfully earned income and if they are not caught, earn overall $y^c = \alpha h + r(h)$, where $r(h)$ is income from crime. If caught, they get no return from crime and incur utility cost $g(h)$. The probability of being caught is $p(h)$. The expected utility of crime participation with an uncertain outcome is $\tilde{V}_c = (1-p(h))F[\alpha h + r(h)] + p(h)(F[\alpha h] - g(h)) + e \equiv V_c + e$, where e is a random preference component, known to the individual, and drawn from a logistic distribution with

parameter $\mu > 0$. Hence the probability of crime participation P_c is given by

$$P_c \equiv Pr(e > V_h - V_c) = \frac{1}{1 + e^{\mu(V_h - V_c)}}. \tag{4}$$

In what follows prime denotes a derivative. Then the effect of increased human capital on participation in crime is given by

$$\frac{\partial P_c}{\partial h} = -P_c(1 - P_c)\mu \times$$
$$\left\{F'(h) + p'(h)\left[F^s - (F^f - g)\right] - \left[(1 - p(h))\left[\alpha + r'(h)\right]F^{s\prime} + p(h)[\alpha F^{f\prime} - g']\right]\right\} \tag{5}$$

where $F^s \equiv F[\alpha h + r(h)]$ and $F^f \equiv F[\alpha h] > 0$. A sufficient condition for crime participation to decline with human capital is that the probability of being caught ($p(h)$) remains constant (or indeed increases, $p'(h) \geq 0$) with respect to human capital and that the expected marginal psychic cost of crime is larger than the expected marginal utility of h when participating in crime $p(h)g' \geq (1-p(h))(\alpha + r'(h))F^{s'} + p(h)\alpha F^{f'}$. This ensures that the expected marginal utility of income from crime declines with human capital.

We now show how improving the human capital of the parent could improve that of the child. This provides the link we require from the original policy, which has been shown empirically to improve parental human capital.

A.1.2 Child Human Capital

Parents are linked to their children altruistically. Parental preferences over their own consumption (c_p) and child human capital (h) are assumed to be

$$V^p(c_p, h) = log(c_p) + \lambda U^k(h), \tag{6}$$

where λ is the altruism parameter and $U^k(h) = E_e max\{V_h, V_c + e\}$ is the expected child utility with respect to the random preference term e and with V_h, V_c defined above. The expectation

is taken with respect to the distribution of child random preferences, e. $V^p(c_p, h)$ is assumed increasing in child human capital, and with sufficient uncertainty on child preferences e it is differentiable in both c_p and h. We simplify the analysis by assuming that parents can only influence child utility by investing in their human capital and not by direct monetary transfers. Since we measure h in monetary units, it effectively represents lifetime income and is equal to lifetime consumption.

The production function of human capital is assumed to be $h = (\gamma_0 + \gamma_1 \varsigma_p)\, \iota_p \equiv \Gamma(\varsigma)\iota_p$ where ι_p are parental investments and ς_p is parental human capital. These may be complements or substitutes in the production of child human capital depending on the sign of γ_1.[36] The production function introduces two separate channels through which parental schooling can affect child skills: through the productivity of investments controlled by γ_1 and through the level of investments, which are a parental choice.

Given the above, parents choose c_p and ι_p to maximize utility, subject to the production function and the budget constraint $c_p + \iota_p = y_p(\varsigma_p)$, where $y_p(\varsigma_p)$ is parental income, which is increasing in human capital ς_p. The solution to the utility maximization problem implies the following investment relationship

$$\iota_p = y_p(\varsigma_p) - \frac{1}{\lambda U^k(h)'(\gamma_0 + \gamma_1 \varsigma_p)} \tag{7}$$

The effect of increasing parental human capital ς_p on parental investments in children is then given by

$$\frac{\partial \iota_p}{\partial \varsigma_p} = \frac{y_p' + \gamma_1 \frac{\lambda}{x^2} U^k(h)'\,(1+R)}{1 - \frac{\lambda}{x^2}\Gamma^2 U^k(h)''} \tag{8}$$

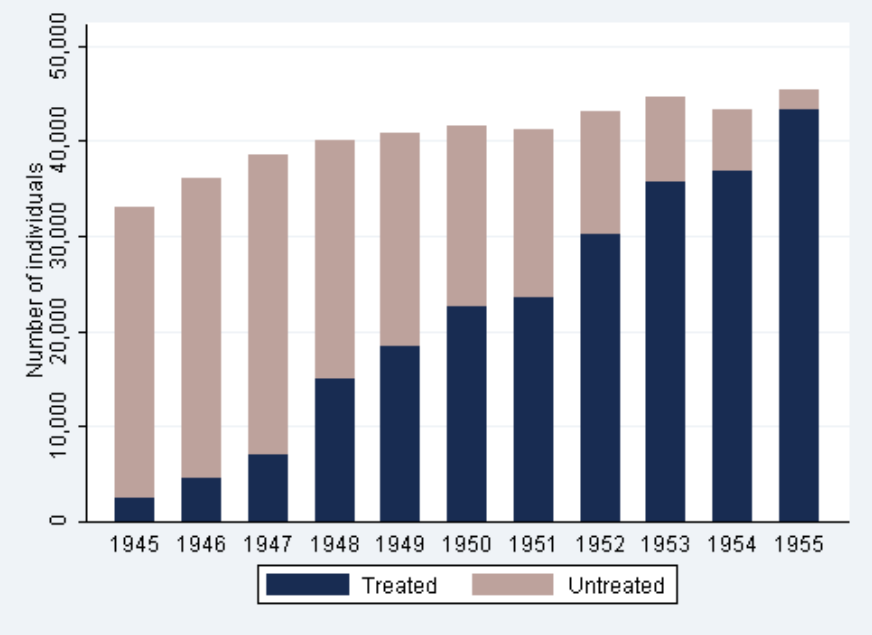
where $R = hU^k(h)''/U^k(h)'$ is the coefficient of relative risk aversion and $x = \lambda U^k(h)'(\gamma_0 + \gamma_1 \varsigma_p)$. The denominator is positive assuming concavity of $U^k(h)$. For $1+R \geq 0$ this expression

[36] We assume throughout that $\gamma_0 + \gamma_1 \varsigma_p \geq 0$. We ignore time inputs for simplicity. They can cause a trade-off between investing time in children and earning more. Still, empirically higher parental human capital is associated with improved human capital for children (**?**).

in positive if parental human capital (ς_p) and child investments are complements in the production function for human capital ($\gamma_1 \geq 0$). If they are substitutes ($\gamma_1 < 0$) the sign of the impact is ambiguous and depends on the return to parental human capital (y'_p). Given the literature on human capital production functions the most empirical relevant case is that of $\gamma_1 \geq 0$.[37] In turn improved investments raise child human capital as shown empirically by both experimental and observational studies.[38] Putting these arguments together, defines one of the channels through which the educational reform in Sweden reduced crime in both the exposed and the child generation.

A.2 Definitions and Implementation of the Reform

Figure 1: Number of Individuals in the Sample Assigned to the Reform by Year of Birth



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.[39] We categorize crimes

[37] See Cunha, Heckman, and Schennach (2010), and Attanasio, Cattan, Fitzsimons *et al.* (2020) amongst others.

[38] See Cunha and Heckman (2008); Cunha, Heckman, and Schennach (2010); Gertler, Heckman, Pinto *et al.* (2014) and Attanasio, Cattan, Fitzsimons *et al.* (2020) amongst others.

[39] Types of crimes are detailed in several variables that specify the chapter, paragraph, moment, piece

Table 10: Description of the Different Types of Crimes Included in the Study

Type	Description
Violent Crimes	Crime against Chapter 3 or 4 in the Swedish Criminal Code. Includes murder, assault, battery as well crime against liberty and peace.
Property Crimes	Crime against Chapter 8 in the Swedish Criminal Code. Includes theft and robbery and stealing.
Traffic Crimes	Crime against the Road traffic regulation (The Highway Code in the US). Includes traffic crimes serious enough to lead to a court appearance such as driving under the influence of drink or drugs, causing serious accidents or serious speeding violations. It *excludes* minor traffic offenses, punishable by a fine without a court appearance
Fraud Crimes	Crime against Chapter 9, 10, 11, 14 and 15 in the Swedish Criminal Code. Includes embezzlement, breach of trust, dishonesty against creditors, falsification, tax fraud and perjury.
Other Crimes	Crimes against the following chapters of the Swedish Criminal Code: Chapters 5 (Defamation), 6 (Sexual Crimes), 7 (Crimes against the Family), 12 (Crimes Inflicting Damage), 13 (Crimes Involving Public Danger), 16 (Crime against Public Order), 17 (Crime against Public Activity), 19 (Crime against the Security of the Realm), and 20 (Misuse of Office)

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.[40]

The traffic crimes need to be serious enough to lead to a court case and do not include speeding and parking offenses. Table 10 shows our categorization.

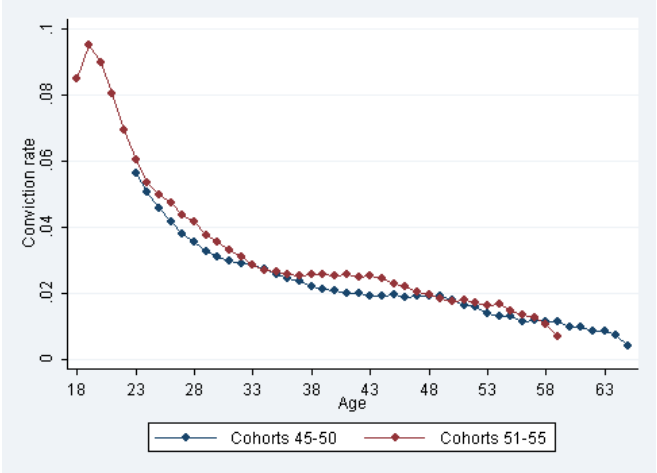
and point in the section of the relevant penal code (law-book). Details of the types-of-crime variables in the conviction data are in Brå Variabelbeskrivning Lagföringsregistret (2009) and the documentation of coding crime types can be found in Brå Kodning av brott (2010). The crime register also contains 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.

[40] Other crimes include defamation, family law, vandalism, hazardous general crimes, crimes against public order, violation of general business, Crimes against national security, misconduct, Environmental law, Alcohol law, Weapon and knife law, Immigration law, Copyright law, Working environment law, visiting rights law - harassment of ex-spouse etc., privacy and data protection.

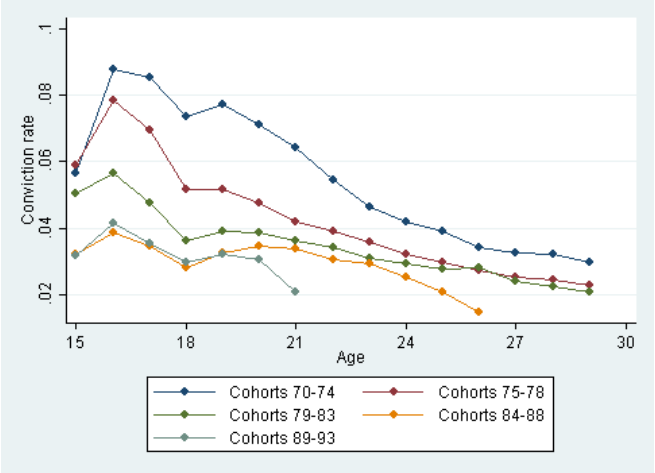
Figure 1 shows the number of observations in our sample in each year birth cohort and the proportion of the parent generation assigned to the reform.

Figure 2: Crime Age Profiles

(a) Parent Generation, by cohort



(b) Child Generation, by cohort



A.3 Education and Crime in the Parent Generation

We use the difference-in-differences regression (1) to estimate the impact of the reform on educational attainment for the parent generation for all potentially affected cohorts (1945-55). As in all results we use a reform assignment based on the municipality of birth. The reform increased years of schooling by of 0.319 for men and 0.206 for women (see Table 5). Both effects are highly significant and confirm earlier results by Meghir and Palme (2005) obtained on just two of the cohorts (1948 and 1952).

In Table 11 we show the effects of the reform on crime in the parent generation. The analysis is carried out for the subset of people born in the period 1952-55 for whom we have criminal records at a young enough age. Here we include all, whether they have children or not. For men the reform significantly reduced the incidence of having any conviction by 1.5 percentage points (pp) and importantly, it also reduced significantly the incidence of repeat convictions by 1.5 percentage points from a lower base, representing a decline of about 8 percent. For women, we find no impacts. These results confirm earlier findings

of the impact of compulsory schooling reforms on crime in the US (Lochner and Moretti, 2004), in the UK (Machin, Marie, and Vujić, 2011), and in Sweden using the same reform (Hjalmarsson, Holmlund, and Lindquist, 2015).

Table 11: Impact of the Reform on Crime in the Parent Generation

	Convictions		Breakdown by type of crime					
	Any^{+}	Multiple^{++}	Violent	Property	Drugs	Traffic	Fraud	Other
Men born 52-55, Convicted at age 18-58, $N = 176,232$								
Reform	-1.464	-1.476	-0.364	-0.783	-0.427	-1.391	-0.334	-0.149
	(0.556)	(0.491)	(0.297) [>0.5]	(0.347) [0.12]	(0.294) [0.44]	(0.473) [0.018]	(0.269) [>0.5]	(0.419) [>0.5]
Mean of dep var %	38.62	18.90	6.16	9.37	7.23	22.59	7.02	13.66
Women born 52-55, Convicted at age 18-58, $N = 167,588$								
Reform	0.389	-0.033	0.039	-0.209	0.190	0.414	-0.120	-0.062
	(0.315)	(0.202)	(0.084)	(0.214)	(0.135)	(0.238)	(0.135)	(0.137)
Mean of dep var %	10.22	3.74	0.61	2.80	1.63	4.64	1.92	1.66

Notes: The impact is measured in percentage points and the dependent variable is percent of the population. Sample includes people from the 1952-55 cohorts, whether they have children or not. $^{+}$Any convictions: having ever been convicted. $^{++}$Multiple Convictions: two or more convictions at different times. Results are percentage points. Robust standard errors, clustered by birth municipality, in parentheses; Romano-Wolf step down p-values in square brackets for the six different types of crime. No RW p-values for women since no impact is significant at the individual significance level. All regressions include a full set of birth municipality, birth cohort indicator variables, and father's education levels.

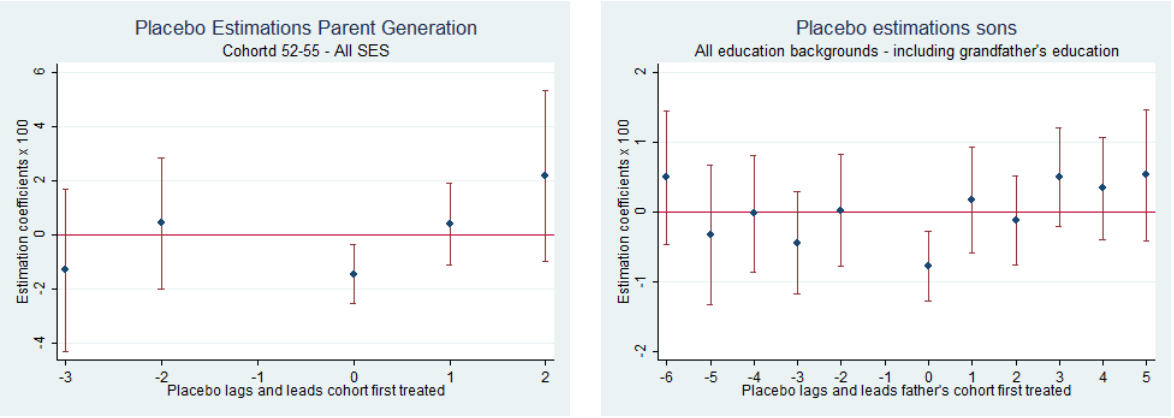
A.4 Robustness Tests: Common Trends Assumption

A key assumption underlying our empirical approach is that cohort effects are common across municipalities. The assumption may be violated if there are changes in the municipalities affecting cohorts in ways that are relevant for crime. We now bring to bear evidence on this issue using three different approaches. 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. Third, we plot residuals to show that they do not display a trend. In all the above 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.

Placebo Tests 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 later, 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.[41] 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 comparison group. This provides an additional five placebo estimates.

Figure 3: Placebo Estimations Sons



The results are all brought together in the right panel of 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 outcome variable is the summary measure

[41] 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.

of any conviction. 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. Finally, we tested the joint hypothesis that each placebo effect is equal to the true effect. The p-value of this test, which was carried out using the bootstrap, is zero implying that the placebo and the real effects are indeed significantly different.

In the left panel of the Figure we show the results from the corresponding placebo tests for the fathers in the parental generation.

Including Differential Trends For our second approach, the inclusion of heterogeneous trends in the empirical specification, has a p-value of 0.23 for the child generation and 0.69 for the parent generation. Moreover, including them does not change the parameters either: the p-value for parameter equality between the model that allows for heterogeneous trends and the one that does not is 0.854 for the child generation and 0.797 for the parent generation.[42]

Residual Plots Our third approach to evaluate the common trends assumption is to plot residuals to show that they do not display a trend. In Figure 4 we plot the residuals from the difference-in-differences regressions for all convictions for sons (with the estimated average impact added back in). Each point corresponds to an average residual across cohorts in different municipalities grouped by years to implementation.[43] The straight line on each graph is fitted by weighted least squares across the grouped residuals, with the weight being the inverse of the variance in each group. In Figure 5 we plot the residuals from the same

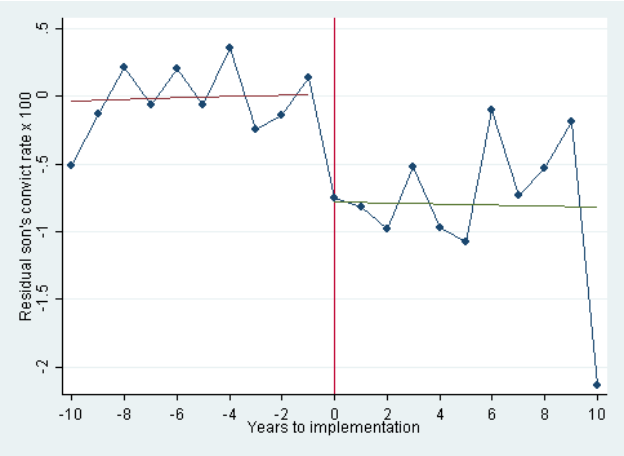
[42]Parameter values with differential trends available upon request.

[43]For 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 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. Groups closer to zero include many more observations and are thus more precise.

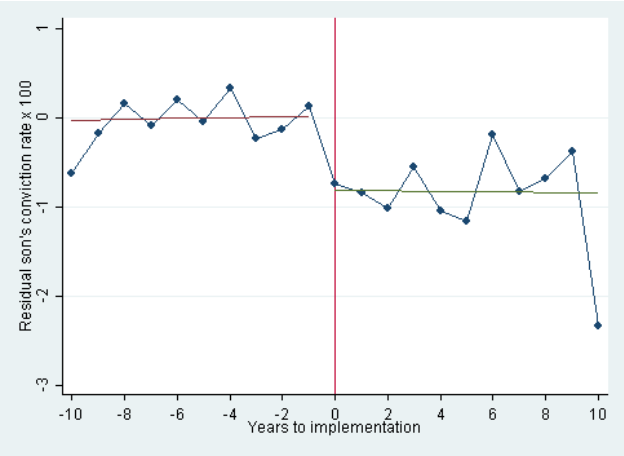
exercise for the type of crimes where the effects were significant.

Figure 4: Residual Graph for Overall Conviction Rate (age 15-29). Sons

(a) Including grandfather's education



(b) Excluding grandfather's education

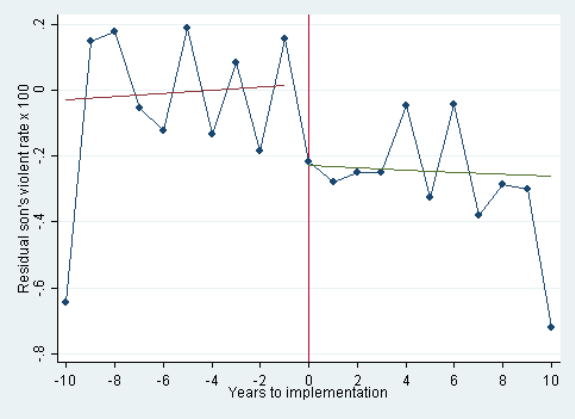


Note: Regression lines fitted to the grouped data using weighted least squares with the weights being the inverse of the variances of each group.

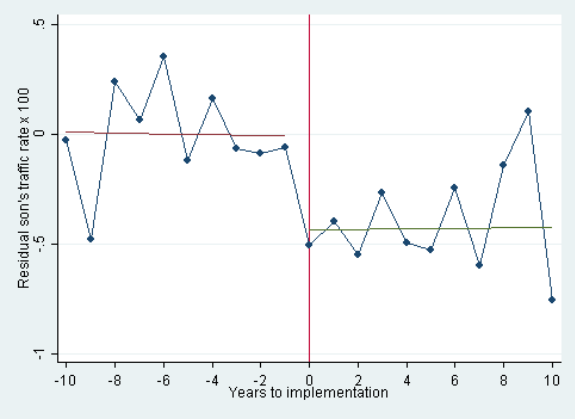
If there are systematic trends related to early or late implementing municipalities these would show up as a trend in these residuals because the composition of municipalities changes as we move along the x-axis to different times to implementation. However, these residuals display no significant trend either for the overall conviction rate or for each type of crime separately. This is true whether we condition on grandfathers education or not. We also tested formally for the null of zero slopes using a bootstrap-based test and the p-values are all above 0.2. Finally, we reach the same conclusion for daughters as well as for the parents - not shown here for brevity. 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 5: Correlation of Residuals with Omitted Trends

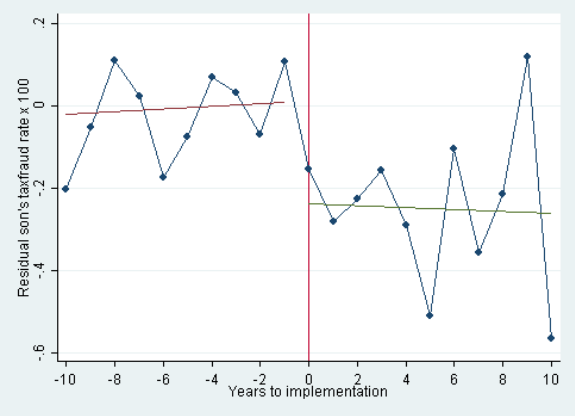
(a) Violent Crimes



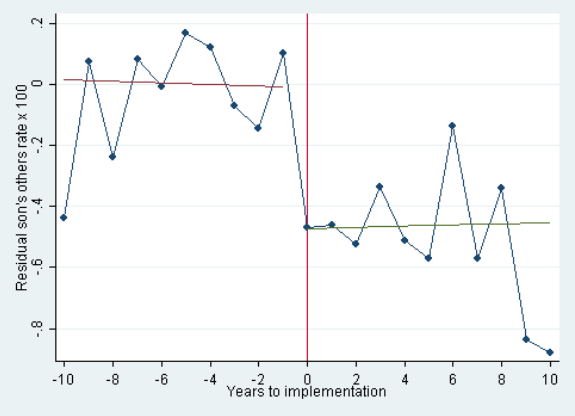
(b) Traffic Crimes



(c) Fraud Crimes



(d) Other Crimes



Note: Regression lines fitted to the grouped data using weighted least squares with the weights being the inverse of the variances of each group.

A.5 Data Appendix

This section describes the data sources used in this paper and the variable construction for all outcomes. All data registers were processed and delivered by Statistics Sweden (SCB). Raw data files used to create the tables and figures presented in the paper were accessed from Statistics Sweden in 2011.

Sample The analysis sample consists of children of men and women born in Sweden in years 1945-1955. The children were born in or before the year 1993, and must be alive and living in Sweden in 1973. Year of birth, gender, and municipality of birth are from the population census. Residence in Sweden is measured in the annual *Registry of the total population*. Family linkages between the parent and child generations are taken from the *Multigenerational register*.

Sources:
Statistics Sweden. 1968-2014. "*Registret över totalbefolkningen* (population census) [database]", accessed 2011.
Statistics Sweden. 2011. "*Flergenerationsregistret* (biological parents and biological siblings) [database]", Microdata Online Access At Statistics Sweden (MONA), accessed 2011.

Education Educational attainments feature in our analysis in three generations. For the children's sample, we measure years of schooling from the education register ("Utbildningsregistret"), and high school GPA from the annual educational registers on grade 9 completion (*Grundskolan: åk 9*). For mothers and fathers, we measure years of schooling in the 1990 education register, or if this is missing, in the 1970 Census (*Folk- och bostadsräkningen 1970*).

As a control variable, we also use the highest attained level of education among the maternal and paternal grandfathers (from the child generation's point of view). This is measured in the 1970 Census, and thus only available for the subset of children whose grandfathers were still alive in 1970.

Additionally, we use the annual grade 9 registers to construct *school peer groups* for the children's sample, as all children born in the same year and attending the same school in grade 9.

Sources:
Statistics Sweden. 1990-2010. "*Utbildningsregistret*" [database], Microdata Online Access At Statistics Sweden (MONA), accessed 2011.
Statistics Sweden. 2011. "*Folk- och Bostadsräkningen 1970*" [database], Microdata Online Access At Statistics Sweden (MONA), accessed 2011.
Statistics Sweden. 1987-2010. "*Grundskolan: åk 9.*" [database], Microdata Online Access At Statistics Sweden (MONA), accessed 2011.

Income and employment Information on income in the parent and child generations comes from two sources: the annual income taxation registers (IoT), which feature individual income from employment, self-employment, and social welfare payments from 1968 to 2014, and the *LISA* register (see below), where SCB have processed the information from IoT.

We construct the following variables for the parents' sample:

- **Log Earnings age 40-50** The log of own disposable income (income plus net social transfers minus tax) from LISA at ages 40 to 50, in panel format (zeroes excluded).
- **Work age 40-50** An indicator for non-zero earnings (income from self-employment or employment) at age 40-50, in panel format.
- **Spouse's Log Earings age 40-50** For spouses, i.e. the co-parent of the focal parent, the log of own disposable income from LISA at ages 40 to 50, in panel format (zeroes excluded).
- **Log Household income age 40-50.** The log of family disposable income for the focal parent at ages 40 to 50, in panel format. Disposable income is constructed by SCB and pertains to the current household in each year.
- **Log Household Income Child Age 0-5.** The log of the average of father's and mother's total household income, across child age 0 to 5.[44] The income measure used is *total income* at the individual level: all earnings-based and self-employment income plus net transfers and capital income, taken from IoT.
- **Log Household Income Child Age 12-18.** Same as above, but calculated across ages 12 to 18 of the child.

[44] $\frac{1}{6}\sum_{t=0}^{5} \frac{(\text{Mother income}_t+\text{Mother partner income}_t)+(\text{Father income}_t+\text{Father partner income}_t)}{2}$, for t = age of child.

- **White Collar Worker.** An indicator for working in a white-collar profession (including business owners but excluding farmers) in 1985 or 1990, whichever is closest to age 40. Constructed from the variable *socio-economic classification* ("socioekonomisk indelning (SEI) av förvärvsarbetande") in the population census (FoB).
- **Spouse White Collar Worker.** Same as above, for the co-parent of the mother or father.

Additionally, we construct the following variables for the children's sample:

- **Annual Earnings, SEK.** Individual disposable income (in SEK 2010) as calculated by SCB, in the years 2000-2010, in panel format.
- **Welfare receipt.** Net annual social welfare (in SEK 2010) received by an individual or their household, calculated by SCB from the taxation registers.
- **Employment** and **Unemployment**. Indicator variables taking the value one if an individual is listed as employed or unemployed (excl. non-employment) in the LISA register. Measured in the fall of each year by SCB from income statements.

Sources:
Statistics Sweden. 1990-2014. "*Inkomst- och taxeringsregistret (IoT)*, 1968-2014" [database], accessed 2017.
Statistics Sweden. 2011. "*Folk- och Bostadsräkningen 1970, 1985, and 1990*" [database], Microdata Online Access At Statistics Sweden (MONA), accessed 2011.
Statistics Sweden. 1990-2010. "*Longitudinal integrated database for health insurance (LISA)*" [database], Microdata Online Access At Statistics Sweden (MONA), accessed 2011.

Family formation We use two data sources to study family formation From the annual population census, we collect information on marriages for the parent generation. For each person in the children's sample, we construct an indicator variable for "marital stability", taking the value one if the mother (father) is married to the father (mother) when the child is five years old. From the multigenerational register, we collect information on the age of the parent when their first child is born and the number of children (shown in Appendix results).

Sources:
Statistics Sweden. 1968-2014. "*Registret över totalbefolkningen* (population census)" [database], accessed 2017.
Statistics Sweden. 2011. "*Flergenerationsregistret* (biological parents and biological siblings)" [database], Microdata Online Access At Statistics Sweden (MONA), accessed 2011.

Criminal convictions Our measure of criminal activity comes from a database consisting of all criminal convictions (in first-level courts, summary impositions of fines, and waiver of prosecution) in Sweden in years 1973-2010 (*Lagföringsregistret*). We use data on the ID of the person charged, date of conviction, verdict, sentencing outcomes, and the crime a person is charged with. We remove any observations where the verdict is acquittal (only pertinent to court cases).

We create the following variables:

- **Any conviction.** An indicator for whether a person was ever convicted of a crime.
- **Multiple convictions.** An indicator for whether a person was convicted at least twice.
- **Type of crime** (violent, property, drugs, traffic, fraud, other). Indicators taking the value one if a person was ever convicted of *that type of crime*, zero if never convicted *or* never convicted of that type of crime. Derived from the first listed (most severe) crime in the charge. See Appendix Table 10 for a full description of how crimes are classified in our data.
- **Prison.** An indicator for whether a person was ever given a prison sentence, zero if never convicted *or* never given a prison sentence.

Source: BRÅ (The Swedish National Council for Crime Prevention). 1973-2010. "*Lagföringsregistret*" (Convictions register) [database]. Microdata Online Access At Statistics Sweden (MONA), accessed 2011.

Military enlistment tests We construct variables reflecting the cognitive and non-cognitive skills of all men (fathers and sons) in our sample, using information from the military enlistment tests. The cognitive skills score is the average of four separate tests: synonyms, induction, mental folding, and technical comprehension. The non-cognitive skills score is

assigned by a certified psychologist based on a 25-minute interview. Before the interview, the psychologist is informed of the conscript's results on the cognitive test, a physical endurance test, school grades, and answers to 70–80 questions about friends, family, hobbies, and related topics. The objective of the score is to measure the conscript's ability to meet the psychological requirements for military service and, ultimately, to serve in war. Both scores are population standardized by cohort, following a Stanine scale.

Source: Swedish National Archives. 1969-2010. "*INSARK* (Draft Register)" [database]. www.riksarkivet.se.

Health data We use two registers with health information covering the entire population. From the *Prescriptions register*, we access all prescription drug outtakes made by individuals in the years 2005 to 2015. We use this to construct total prescription drug usage, and the Appendix results on prescription drug usage by drug category (in Table 12) among the child generation. All variables are constructed as the number of doses, total and in each drug category, that a person collects in 2005-2015.

From the *Inpatient register*, we source information on the number of inpatient hospital visits in the years 1987 to 2015, and the medical diagnosis causing the visit (ICD codes). We use this to construct total hospitalization days for the child generation, and the Appendix results on alcohol and drug misuse among the parent generation (indicator variables for any hospital visit induced by alcohol- or drug-related injuries) – see Appendix Table 13.

Sources:
Socialstyrelsen (The National Board of Health and Welfare). 1987-2015. "*National Patient Register*" [database]. www.socialstyrelsen.se. Accessed 2017.
Socialstyrelsen (The National Board of Health and Welfare). 2005-2015. "*National Prescribed Drug Register*" [database]. www.socialstyrelsen.se. Accessed 2017.

Residential neighborhoods We construct the residential neighborhood at age 16 of the child generation, using information from the annual geographical database on the residential area (*SAMS* area) where a person lives according to official registers. These areas are constructed by SCB to be internally homogeneous housing units with about 1,000 inhabitants.

Source: Statistics Sweden. 1985-2012. "*Geografidatabasen*" [database], Microdata Online Access At Statistics Sweden (MONA), accessed 2013.

A.6 Additional Results

Table 12: Reform Effects on Prescribed Drugs Usage in the Child Generation, by Main ATC Code

	(1) Sons	(2) Daughters
Panel A: Breakdown of Prescribed Drugs		
Metabolism	-2.66	2.13
	(4.22) [>0.5]	(3.49) [>0.50])
Average doses over the period	165.5	190.8
Cardiovascular	3.91	1.25
	(4.84) [>0.5]	(3.05) [>0.50]
Average doses over the period	154.7	95.1
Nervous system (N)	-15.592*	-8.617
	(6.41), [0.055]	(8.36), [>0.50]
Average doses over the period	316.6	511.0
Respiratory	-3.79	-11.84**
	(3.71) [>0.5]	(4.38) [0.038]
Average doses over the period	197.9	263.9
Panel B: Breakdown of Nervous System Drugs		
Prescribed drugs, pain killers (N2)	-3.788**	0.261
	(1.35), [0.014]	(2.56), [>0.5]
Average doses over the period	41.8	80.9
Prescribed drugs, calming effect (N5)	-4.690	-10.084**
	(3.55), [0.19]	(3.63), [0.012]
Average doses over the period	96.2	122.9
Prescribed drugs, anti-depressive (N6)	-7.11	1.206
	(3.94), [0.19]	(5.28), [>0.5]
Average doses over the period	178.6	307.2

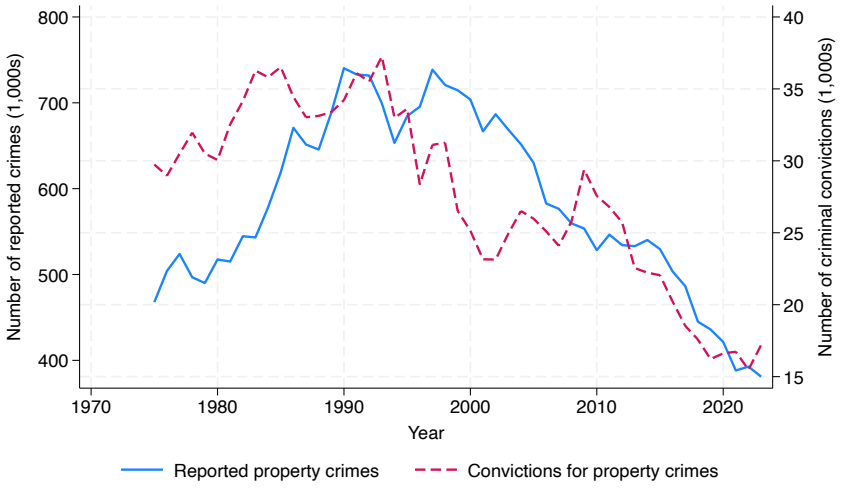
Notes: Standard errors clustered at the municipality level in round brackets. Romano Wolf step down p-values for each group (Cognitive and non-cognitive skills, Education, Health, drugs and breakdown of drugs) in square brackets. In this, men and women are treated separately. *** Significance at 1% ** Significance at 5% and * Significance at 10%, all based on the RW p-values.

Table 13: Reform Effects on Parental Role Models and Parenting Quality

	(1) Fathers	(2) Mothers
Panel A: Diagnoses associated with criminality		
Alcohol-related diagnosis	0.001*	0.000
	(0.001)	(0.001)
Spouse alcohol-related diagnosis	-0.001	-0.001
	(0.001)	(0.001)
Drug-related diagnosis	0.000	-0.000
	(0.001)	(0.001)
Spouse drug-related diagnosis	-0.000	-0.000
	(0.001)	(0.001)
Panel B: Outcomes related to parenting quality		
Child born when teenager (%)	0.096	-0.013
	(0.074)	(0.217)
Number of children	0.008	0.002
	(0.006)	(0.005)
Age at childbirth	0.025	0.011
	(0.034)	(0.037)

Notes: Column 1 shows effects of father exposure to the reform, column 2 shows effects of mother exposure. Outcome variables in Panel A are constructed as having any inpatient hospital visit related to alcohol or drug abuse in years 1987-2013 (child ages 15-20). Standard errors clustered at the municipality level in round brackets. *** significance at 1% ** Significance at 5% and * Significance at 10% both based on the RW p-values

Figure 6: Criminal Convictions and Reported Crimes in Sweden 1973-2023



Note: The figure shows the evolution of reported crimes and court convictions in years 1973-2s023. Blue solid graph: annual number of property crimes reported to the police. Red dashed graph: annual number of criminal convictions for property crimes. Property crimes are chosen for their tendency to be always reported to the police for insurance purposes. Source: BRÅ (National Council for Crime Prevention), Crime Statistics. Date of access: 2025-01-16.

Table 14: Incidence of the Schooling Reform among the Child Generation

	(1)	(2)
		Father exposed to the reform
Sample of children with a father born in 1945-1955 (mother born any year)	798,265	413,894 (51.8%)
		Mother exposed to the reform
Sample of children with a mother born in 1945-1955 (father born any year)	834,824	440,760 (52.8%)
		Mother and father exposed
Sample of children with both father and mother born 1945-1955	407,991	145,498 (35.7%)

Notes: This table shows the number of children whose mother, father, or both parents were affected by the reform. Column 1 reports the number of children in the sample (father, mother, or both parents born 1945–1955). Column 2 reports the number of children whose father, mother, or both parents, respectively, were subject to the comprehensive schooling reform (the "treatment group"). Percentages in Column (2) are relative to Column (1).

Table 15: Heterogeneity-robust (Callaway–Sant'Anna, 2020) estimates of reform effect on crime in child generation (sons of fathers treated by the reform)

	Any	Multiple	Violent	Property	Drugs	Traffic	Fraud	Other
Reform	-1.068***	-0.410	-0.592***	-0.172	0.327**	-0.433	-0.352*	-0.363
	(0.386)	(0.258)	(0.188)	(0.249)	(0.156)	(0.276)	(0.183)	(0.230)
p-value	0.006	0.112	0.002	0.490	0.036	0.117	0.054	0.115

Notes: Results are percentage point estimates from the csdid package in Stata. †Any convictions: having ever been convicted. ††Multiple Convictions: two or more convictions at different times. †††Other see footnote 19. Appendix Table 10 gives all crime definitions. For the types of crime the counterfactual is "not that crime", whether that is no conviction or some other conviction. The sample consists of sons and daughters of men born 1945-55 who are fully observed from age 15-29. All regressions include a full set of father's birth municipality and father's birth cohort indicator variables. Robust standard errors, clustered by municipality of father's birth ($N = 919$), in parentheses. *, ** and ***: p-value <0.1, <0.05 and < 0.01 respectively reflecting the p-values given in the bottom row. No corrections for multiple testing.

Table 16: Mediation Analysis, Restricted Sample

Panel A: Reform Effect on Mediators

	Fathers' Education (1)	Mothers' Education (2)	Fathers' Crime (3)	Log Household Income (4)
Father's exposure to school reform	0.352***	0.115*	-2.042*	1.073
	(0.0675)	(0.0697)	(1.138)	(0.989)
Observations	69,205	69,205	69,205	69,205

Panel B: Dependent Variable: Child Conviction

	(1)	(2)	(3)	(4)	(5)
Father's exposure to school reform	-1.146	-0.469	-0.519	-0.411	-0.487
	(0.774)	(0.782)	(0.791)	(0.798)	(0.803)
Father's years of schooling		-1.924***	-1.370***	-1.115***	-0.800***
		(0.0683)	(0.0693)	(0.0640)	(0.0630)
Mother's years of schooling			-1.264***	-1.172***	-0.998***
			(0.0686)	(0.0669)	(0.0696)
Father's crime				10.20***	8.926***
				(0.366)	(0.363)
Log household income					-7.510***
					(0.477)
Observations	69,205	69,205	69,205	69,205	69,205

Notes: This table shows the same analysis as in Table 9 (mediation analysis), but restricts the sample to only observations with non-missing information in all included variables. The dependent variable in Panel B is a binary indicator equal to one if the child is convicted of any crime. Sample restricted to fathers born 1952–1955 and their children, who are observed from ages 15 to 29. Father's and mother's education is measured in years. Household income is log disposable income measured when the father is aged 40 to 50. All regressions control for grandfather's education, birth cohort, and municipality fixed effects. Standard errors clustered at the municipality level ($N = 981$) in parentheses. All coefficients for binary outcomes or logged variables are scaled by 100 and interpreted as percentage point effects.