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Abstract

This paper studies the impact of compulsory schooling on in-school violence using individual-level administrative data matching education and criminal records from Queensland. Exploiting a dropout age reform in 2006, it defines a series of regression-discontinuity specifications. While police records show that property and drug offences decrease, education records indicate that in-school violence increases. Effects concentrate among students with prior criminal records and their classmates, with greater exposure to in-school violence leading to increased criminality at older ages. Dropout age reforms may alter the school environment and prior studies that fail to consider in-school behaviour may over-estimate their short-run crime-reducing impact.

JEL-Codes: I200, K420.

Keywords: youth crime, minimum dropout age, school attendance.

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1. Introduction

In many countries, youth crime is a significant policy issue. Worldwide, half of students aged 13–15, approximately 150 million, suffer from peer-to-peer violence in and around school (UNICEF, 2018). Crime rates commonly peak in the late teens and early twenties (Quetelet, 1831; and Landersø, Nielsen and Simonsen, 2016), and increasing the age of compulsory school attendance is often viewed as a means of delivering societal benefits including reduced crime. Underlying this is the idea that, when juveniles are in school, they are kept busy in a supervised environment and, thus, off the streets and not committing crime.[1] The link between crime and school attendance has been documented for most crime types and in a variety of settings, including the US (Jacob and Lefgren, 2003; Lochner and Moretti, 2004; Luallen, 2006; Anderson, 2014; Bell, Costa and Machin, 2016 and 2021; Cook and Kang, 2016; Cano-Urbina and Lochner, 2019), England and Wales (Machin, Marie and Vujic, 2011) and Sweden (Hjalmarsson, Holmlund and Lindquist, 2015). The consensus is a beneficial crime-reducing effect from school attendance.[2]

What is currently less well-understood is how in-school behaviour of potential early dropouts may respond to legislative change mandating one additional year in school. While one of the main rationales behind compulsory schooling is to improve the human capital and labour market prospects of potential early dropouts, the primary experience of compulsory education for such individuals is one of being forced to attend school surrounded by better-performing peers.[3] Compulsory schooling laws often rest on a somewhat paternalistic view that juveniles wishing to drop out of school early are actually better off staying on (Messacar and Oreopoulos, 2012). However, very little is known about their behaviour in school. If juveniles are kept in school against their will and disengaged from learning, delinquency in school may increase. Fellow students may suffer significant costs including increased bullying, gang activity, threats or a reduced perception of safety in school, which may in turn affect their learning process. Disruptive peers may hinder school performance (Robertson and Symons, 2003; Figlio, 2007; Carrell and Hoekstra, 2010) and decrease future earnings (Carrell, Hoekstra and Kuka, 2018), while increasing the risk of drug use (Gaviria and Raphael, 2001; Kawaguchi, 2004; Lundborg, 2006; Powell, Tauras and Ross, 2005), cheating (Carrell, Malmstrom and West, 2008) and indiscipline in the classroom (Carrell and Hoekstra, 2010). Thus, generating an

[1] The terms juvenile, youth and adolescent are used interchangeably throughout the paper.

[2] This literature exploits various sources of variation in school attendance including idiosyncratic school closures for teacher training (Jacob and Lefgren, 2003), teacher strikes (Luallen, 2006), school eligibility laws (Cook and Kang, 2016) and minimum dropout age reforms (Lochner and Moretti, 2004; and Machin, Marie, and Vujic, 2011). Different forms of incapacitation considered in the literature include conscription (Galiani, Rossi and Schargrodsky, 2011), teen pregnancy (Black, Devereux and Salvanes, 2008) and violent movie screenings (Dahl and DellaVigna, 2009).

[3] Murphy and Weinhardt (2020) show evidence of negative effects of exposure to higher-performing peers in school.

understanding of the impact of minimum dropout age (MDA) laws on delinquency in school is a first order question in labour economics.

This paper studies this question by examining a recent MDA law change known as the *"Earning or Learning"* reform (hereafter EL) enacted in 2006 in the Australian state of Queensland. Through the EL reform, the MDA was raised from 16 to 17. Prior to 2006, students in Queensland were required to attend school up until either completing grade 10 or turning 16, whichever occurred first. The reform mandated that young people participate in a range of activities broadly defined as *"Earning or Learning"* for up to two extra years, or until age 17. Juveniles were forced either to stay in school or to participate in paid employment for at least 25 hours per week until turning 17. This is a policy-relevant age, as crime rates peak in the late teens and then decline. As not all in-school delinquency is reported to the police, the analysis uses administrative data from the entire population of Queensland State School students matched at the individual level for the years 2003 to 2013 across two agencies, the Queensland Department of Education and the Queensland Police Service.[4]

A Regression Discontinuity (RD) approach is adopted to study the impact of the EL reform on in-school delinquency and criminal activity from age 16 to 20. Administrative records show that the EL reform had a significant impact on the dropout behaviour of juveniles. Prior to the reform, the proportion of Queensland juveniles aged 15-19 in school was 72.5%, well below the national average of 77.3% (Australian Bureau of Statistics, 2003). The first-order effect of the EL reform was to increase the likelihood of enrolling in the final grade of secondary schooling by approximately 12% and the average time spent in school by a sizeable 0.3 years.[5]

Estimates also show that the EL reform increased the count and risk of violent school discipline sanctions (SDS) at ages 16 and 17 by roughly 15% and 13% respectively, while it did not affect police records of violent offences at the same age. The reform also reduced both property and drug offences in police records at age 16-17 and age 18-20, while it did not alter the records of property or illicit substances misconduct in school. Thus, the main conclusion of our analysis of the EL reform is that the extended compulsory schooling period had a significant crime-reducing impact, especially for property and drug offences, but this is partially offset by a sizeable upsurge in violence in school mostly unreported to the police. While violent SDS often capture less serious acts of violence compared to those recorded by the police, the upsurge in in-school violence documented here represents an additional social cost linked with MDA laws that echoes the evidence in Jacob and Lefgren (2003) and Luallen (2006) of increased violence in the locality when school is in session.

[4] Studies matching school records to crime records in the US, although with a different research focus, include Deming (2011), Billings, Deming and Rockoff (2014), Cook and Kang (2016) and Bacher-Hicks, Billings and Deming (2019).
[5] A similar effect has been documented when the MDA was raised in Sweden. See Meghir, Palme and Schnabel (2012), and Hjalmarsson, Holmlund, and Lindquist (2015).

Conclusions are robust across alternative specifications, estimation methods and sample restrictions. Placebo tests confirm our conclusions while a number of alternative hypothesised channels, including potential idiosyncratic school-time-specific shocks, fail to leverage support. School-specific estimates show that students who experienced a larger upsurge in in-school violence due to the reform were more likely to commit crime at age 18-20 and less likely to leave secondary school with a certificate, suggesting that violence in school may hinder the process of human capital accumulation of students and place them at greater risk of future crime.

Additional analysis suggests that the compositional change in the student population induced by the EL reform is a key mechanism behind the increased violence in school. The EL reform required potential dropouts to remain in school for an extra year. This group is not directly observable but youth with prior violent criminal records had much higher early dropout rates prior to the reform. Estimates that separate youth with prior criminal records from others show that the former are much more directly affected by the reform, as they became nearly 50% more likely to enrol in the final year of secondary school due to the reform. Estimates also show that the EL reform caused an increase in in-school violence for both groups, but the increase is much larger for youth with prior criminal records and it is entirely concentrated among youth with prior criminal records and their classmates. Thus, one way in which the reform may have increased the likelihood of violence in school may be by disproportionately forcing youth with prior criminal records who face a higher risk of crime to remain in school. Finally, a cost-benefit analysis suggests that the economic value of the crime reduction generated by the EL reform justifies the reform even when greater in-school violence is considered as a potential additional social cost linked to the reform.

This study closely relates to Anderson, Hansen and Walker (2013), Anderson (2014) and Gilpin and Pennig (2015), which use self-reported survey data to show that in-school crime in the US is severely under-reported to the police and it increases when the MDA is raised.[6] While self-reported survey data capture information not usually present in primary source administrative data, they may suffer from measurement error due to differences in respondents reference points, potential recall bias in self-reported victimisation, and non-representative or selective responses (Bindler, Ketel and Hjalmarsson, 2020).

This study attempts to overcome these limitations and makes three significant contributions to the literature. First, it uses a rich administrative dataset for the entire population of Queensland State School students to establish that MDA reforms may lead to increased violence in school and, as a result, alter the school environment. Unlike previous studies, our analysis enables us to follow

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[6] Under-reporting of in-school crime in the US is also documented in Jeffrey (2012), Trump (2012) and Anderson, Hansen and Walker (2013).

individuals both before and after they leave compulsory schooling. Our estimates suggest that MDA laws impose an important social cost, as violence in school appears to be positively linked with future criminality and negatively linked with the learning outcomes of students. This also implies that the exclusion restriction may be violated in prior studies that instrument the dropout behaviour using MDA reforms in the earnings or crime equations, as these reforms do not only imply one additional year of compulsory schooling for affected students.[7] Second, our results suggest that the contemporaneous effect of compulsory schooling on crime may be more complex than the pure incapacitation effect documented to date, and prior studies that fail to consider in-school behaviour may over-estimate the short-run crime-reducing impact of the reforms. Third, by showing that the effects of the EL reform on in-school violence are concentrated among youth with prior criminal records and among their classmates, this study presents novel, policy-relevant insights on the educational experience of potential early dropouts and their behavioural response to MDA reforms.

The rest of the paper is structured as follows. Section 2 describes the institutional setting and the data. Section 3 describes the empirical analysis and Section 4 presents the main results and a set of robustness tests. Section 5 investigates a set of hypothesised mechanisms underlying the results. Section 6 discusses the results and their relevance for post-compulsory schooling outcomes, and it presents a cost-benefit analysis of the EL reform. Section 7 concludes.

2. Institutional Framework and Data Description

2.1 The Queensland Education System and the Earning or Learning Reform

The institutional setting of this study is the state of Queensland in Australia. Offender rates in Queensland are comparable to the Australian national average,[8] and roughly three quarters of students attend the state-run school sector, which is funded by the State and Federal Australian Governments (QGOV, 2018a), while the remainder attend private schools. The school year runs from the third week of January to mid-December, and children are expected to start grade one in the calendar year in which they turn six years of age (QGOV, 2018b). Children attend up to 12 years of education (grades 1 to 12), with primary school consisting of grades 1 to 7 and high school grades 8 to 12.[9] At

[7] Earnings returns to education have been estimated instrumenting education with compulsory school laws in many countries, including Canada (Oreopoulos, 2006), France (Grenet, 2013), Germany (Pischke and von Wachter, 2008), the UK (Harmon and Walker, 1995; Oreopoulos, 2006; Del Bono and Galindo-Rueda, 2007; Devereux and Hart, 2010; Dickson and Smith, 2011; Buscha and Dickson, 2012; Grenet, 2013; Dickson, 2013) and the US (Angrist and Krueger, 1991; Acemoglu and Angrist, 2001; Lleras-Muney, 2005; Oreopoulos, 2006; Carneiro, Heckman and Vytlacil, 2011; Clark and Royer, 2013). See also the discussion in Card (2001).

[8] These statistics can be accessed on the website of the Australian Bureau of Statistics here.

[9] In 2015, i.e., after the end of our study period, the school enrolment window changed. At present, a child enrolling in grade one must turn six between the 1st July in the year prior to enrolment and the 30th June in the year of enrolment. Since 2015, grade 7 also forms part of high school grades.

the end of secondary school, students are expected to sit high-stakes exams to obtain an Overall Position (OP) certificate, which is required for admission into the university system in Australia.

The 2006 Queensland's "*Earning or Learning"* (EL) reform modified the legislation governing dropout behaviour starting from the cohort born in 1990. Prior to 2006, school attendance was mandatory until either completing grade 10 or turning 16 years old, whichever occurred first. The EL reform raised the minimum dropout age (MDA) from 16 to 17. This additional year was to be spent either in school, vocational training, or in a full-time job. The EL reform introduced a compulsory obligation forcing juveniles to participate in a range of activities broadly defined as "earning or learning" for up to an additional two years, or until they turned 17 years old. Juveniles were forced to either stay on at school until obtaining a high school Senior Certificate or a vocational education Certificate III, or to participate in paid employment for at least 25 hours per week until age 17. The intention of the policy was to help the school-to-work transition and boost the labour market prospects of juveniles, with the underlying idea that education and training offer the skills needed to succeed in adult life and widen the employment options available to juveniles.[10]

Although an innovative policy, the EL reform has attracted little attention to date.[11] Analysis of the EL reform is useful to derive policy prescriptions that extend beyond Australia, as it shares many of the same features of US reforms that encourage participation in training or employment (see Oreopoulos, 2009; and Domnisoru, 2015). The Queensland school system and the EL reform display similarities with the legislation in the UK and the US, where similar MDA laws have been recently enacted offering exemptions from school attendance based on proof of employment or volunteering status, thus broadening the scope of legislation to boost participation in training or employment.[12] Thus, study of the EL reform may generate lessons for education policy, training programmes and labour reinsertion policies that extend beyond Australia.

2.2 Data

Our analysis uses administrative data matched at the individual level from two agencies, the Queensland Department of Education and the Queensland Police Service. This linked dataset was prepared in collaboration with these agencies, and it includes individual record data for the entire population of Queensland Government funded school attendees together with matched individual criminal offence data on juveniles and young adults for the period 2003 to 2013. Availability of these

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[10] In a media statement in November 2005, the Queensland Education Minister R. Welford stated: "From 1 January 2006, when the Act comes into effect, the compulsory school age will increase and young people will be required to be 'learning or earning' until they turn 17. We want to help keep young people learning, or to help them return to learning, so they can gain the skills and qualifications they need to succeed in later life." (see http://statements.qld.gov.au/Statement/Id/43712).

[11] See Beatton, Kidd, Machin and Sarkar (2018).

[12] See https://www.gov.uk/know-when-you-can-leave-school.

data for the entire population of students in state schools in Queensland allows us to study in-school delinquency and crime both during and after the compulsory schooling period.

School disciplinary sanctions (SDS), i.e., school suspension records from the Department of Education, are used to measure delinquent behaviour in school. SDS are actions in response to serious breaches of school rules and unacceptable behaviour that are available to Queensland school principals in order to restore discipline. Reasons for SDS include physical misconduct, such as violence against other students or against school personnel; in the empirical analysis, physical misconduct (i.e., violent) SDS are the main outcome of interest. Categories of SDS that are also studied here include property misconduct, such as destruction or theft of school property or the property of others on school premises, and substance misconduct involving illegal substances, such as drugs.[13] In any one year, principals may discipline students with multiple short suspensions of 1 to 5 days or multiple long suspensions of 6 to 20 days.[14] Principals may also expel a student from school in response to extreme and repeated bad behaviour. The allocation of SDS is moderated by the Queensland Education head office in order to ensure consistency in discipline sanctions, independent of the principals' specific attitudes towards discipline.

SDS records often capture minor acts of delinquent behaviour that do not correspond to a criminal offence. However, they may be linked to criminal charges and they complement the evidence from police records on youth delinquency in school. In the United States (US), 1.4 million crimes were recorded in public schools in the 2015/16 school year, with only 449,000 crimes, i.e., less than one in three, reported to the police. In 2017, among students aged 12 to 18, approximately 827,000 victimisations (theft and non-fatal violent victimisation) took place at school, and roughly one in five students reported being bullied in school (US Department of Education).[15] The US School Survey on Crime and Safety shows school administrators fail to report roughly 60% of all physical attacks without a weapon (Gilpin and Pennig, 2015). In the UK, one in four juveniles report assaulting another person at age 16-17, and more than 30,000 school crime offences were recorded in 2015.[16] In Australia, over 42% of school leaders report being exposed to physical violence.[17] Given the scale

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[13] Administrative school records also include information on other less serious types of misconduct that may result in the receipt of an SDS, including truancy, persistent disruptive behaviour, verbal and non-verbal misconduct, refusal to participate and misconduct involving legal substances, i.e., cigarettes or alcohol (QGOV, 2018c). These SDS categories are brought later into the analysis.

[14] The definition of short suspension has been modified in 2015, i.e., after our study period ends, and now it includes suspensions lasting from 1 to 10 days.

[15] See https://nces.ed.gov/fastfacts/display.asp?id=49 or https://nces.ed.gov/pubs2019/2019047.pdf.

[16] See Appendix Figure A.1, https://www.bbc.com/news/education-56001234, https://www.bbc.co.uk/news/education-34268942, and https://www.bbc.co.uk/news/education-47537631.

[17] See Australian Principal Occupational Health, Safety and Wellbeing Survey 2019 Data, Australian Research Council Project (LP160101056) at https://www.healthandwellbeing.org/principal-reports.

of the problem and the systematic tendency of schools to handle youth delinquency without involving the police, SDS records constitute a valuable outcome in the study of youth delinquency in school.

The administrative Queensland schooling data are matched at the individual level to the crime data from the Queensland Police Service (QPS) from 2003 to 2013. QPS records refer to alleged criminal offences in a given year by individuals aged 16 to 20. An alleged offender is a person who has allegedly committed a crime and has been charged by the police by arrest, caution, warrant or apprehension. In the empirical analysis, alleged violent criminal offences by an offender in a given year are another outcome of central interest. Violent offences include violence against the person, sexual offences and robbery. Property and drug-related offences by an offender in a given year are also studied, with property offences including criminal damage and theft and handling of stolen goods.

3. Empirical Strategy

The Earning or Learning (EL) reform was enacted on the 1st January 2006 and the first cohort to be directly affected by the legislative change was born in the year 1990. Data from the Queensland Department of Education indicate that 66% of youth from the 1987-89 cohorts and 86% of youth from the 1990-93 cohorts enrolled in grade 12, i.e., the final year of secondary school. The dropout grade, defined as the highest grade in which an individual is enrolled in state-maintained schools, was on average 11.42 for the 1987-89 cohorts and 11.85 for the 1990-93 cohorts. Thus, a significant increase in the likelihood to enrol in the final year of secondary schooling of 20 percentage points and in the average dropout grade of 0.43 units appears for the cohorts born starting from 1990.[18]

In order to quantify the impact of the EL reform on in-school violence, a series of Regression Discontinuity (RD) specifications is defined whereby the birth cohort defines the Intention-To-Treat (ITT) status. The violent behaviour of ITT juveniles born in 1990-93 is compared with that of control juveniles born in 1987-89. Violent behaviour is modelled as a function of distance in year of birth to the relevant birth cut-off determining EL eligibility, post-cut-off indicator (i.e., born in 1990-93), their interaction term, and a variety of individual and school characteristics.

A series of ITT estimates of the impact of the EL reform on delinquent behaviour are presented. Information on the school leaving grade of individuals is available in the data, and thus the first-order effect of the EL reform on youth dropout behaviour could also be estimated. However, instrumenting the dropout behaviour with the EL reform in the violence equation would require us to assume that the only channel through which the reform affected violence is years of schooling and

[18] This is a similar order of magnitude to the effect on years of schooling of the school reform in Sweden studied by Meghir, Palme and Schnabel (2012) and Hjalmarsson, Holmlund, and Lindquist (2015), and represents a sizeable 3.8% increase in the average dropout grade.

not via other aspects of the reform, such as changing peer group. This does not appear a tenable assumption for our analysis, since behavioural outcomes are measured from age 16, i.e., at a younger age than the minimum dropout age (MDA) introduced by the EL reform, and thus greater exposure to older unruly peers is a potential mechanism through which the EL reform may increase delinquency and victimisation in school. Thus, no instrumental variable (IV) estimates are presented.

Formally, the ITT impact of the EL reform on the dropout behaviour, violence and other behavioural outcomes of juvenile *i* from birth cohort *c* in year *t* is estimated as follows:

$$Y_{ict} = \alpha + \beta_1 * EL_{ic} + \beta_2 * Yob_{ic} + \beta_3 * Yob_{ic} * EL_{ic} + f(a,t) + \delta * X_{ic} + \vartheta_s + \omega_{ict}, \quad (1)$$

where *Y* is the outcome of interest, with SDS calculated at age 16-17 and crime calculated separately at age 16-17 and age 18-20. *EL* is a binary variable that takes the value 1 for juveniles born in 1990-93 who were subject to the EL reform, and value 0 for juveniles born in 1987-89. Yob is a continuous indicator of distance from juvenile *i*'s year of birth to the 1990 cohort included in all specifications together with its interaction with the *EL* variable and with f(a, t), which is a function of the individual's age (a) and year (t). In all estimated specifications, $\mathrm{f(a,t)} = a_{t-c} + a_t$ is modelled by a full set of age (where $\mathrm{a = t - c}$) and year dummies a_{t-c} and a_t. *X* is a vector of time-invariant individual characteristics including gender, native language and day-month of birth. ϑ is a set of school fixed effects measured at age 15, and ω is the error term.

In equation (1) the key parameter of interest is β_1, the estimated coefficient on eligibility for the Earning or Learning reform variable *EL* for birth cohorts ≥ 1990. In equation (1), once fixed effects for gender, native language, day-month of birth, age and year have been taken into account, together with heterogeneous linear trends by year of birth either side of the discontinuity cut-off, estimates of β_1 show the short-run RD impact of the EL reform on delinquent behaviour of juvenile *i* from birth cohort *c* in year *t*. Thus, in equation (1), β_1 identifies the different propensity to receive SDS or commit crime of juveniles marginally separated by eligibility for the EL reform in a given year, at a given age, and with a given set of characteristics.

Since the data contain individual-level information on the exact date of birth of individual *i*, one could define the running variable based on the exact date of birth and conduct the analysis controlling for a continuous indicator of distance from juvenile *i*'s exact date of birth to the 1st January 1990. However, this is not done here because dropout behaviour and youth delinquency exhibit seasonality by month of birth (Cook and Kang, 2016; and Landersø, Nielsen and Simonsen, 2016), and thus this empirical strategy would retrieve estimates that pick up and mix the effect of exposure to the EL reform with the separate effect of the month of birth. While the former is the effect of interest here and it is best isolated comparing delinquency across year-of-birth cohorts, the effect of the month of birth is estimated in a separate test presented below.

Standard errors are clustered at the date of birth level (i.e., at the day-month-year of birth level) since the MDA legislation in Queensland is defined in terms of age and not grade completion. Thus, while youth born on the same date are subject to the same compulsory schooling period, youth born on different days-months in the same year may face up to almost a one-year difference in the timing of dropout eligibility depending on the day-month of birth within a given year. In this regard, the MDA legislation in Queensland resembles the legislation in North Carolina and other US states (see Cook and Kang, 2016).

4. Empirical Estimates

4.1 Descriptive Estimates

Table 1 shows the structure of the administrative panel dataset used in the econometric analysis. Since the schooling data are matched at the individual level to the crime data from 2003 to 2013, complete information on delinquent behaviour is available for a panel of individuals aged 16-20 from the 1987-1993 birth cohorts. Earlier cohorts could not be observed since the age of 16, i.e., the minimum dropout age (MDA) prior to the Earning or Learning (EL) reform, and later cohorts could not be followed up to age 20. Crime and school enrolment data for the 1987-1993 birth cohorts are also available at age 15. However, while crime and school enrolment records exist from 2002, no School Discipline Sanction (SDS) records are available in 2002. Thus, the main analysis is conducted on the 2003-13 years for which complete information is available. All juveniles included in the econometric analysis were in the state school system at least once from age 15-17, and the main econometric analysis comprises 282,702 juveniles aged 16-20 and 1,412,758 juvenile-year observations in total.

Table 2 presents descriptive statistics for the outcomes of interest separately by age for the 1987-89 cohorts, i.e., the cohorts unaffected by the EL reform. Panel A of Table 2 shows that, prior to the EL reform, juveniles faced a greater risk of receiving a violent SDS at age 16, and the same pattern appears in Panel B where violent, property and drug SDS are studied together. Panel C shows that juveniles born in the years 1987-89 faced a greater risk of violent criminal participation at ages 17-18 followed by a reduced risk at subsequent ages, and Panel D shows a similar pattern when violent, property and drug offences are studied together. The comparison of column (1) in Panels A and C is informative on the relationship between violent SDS and police records of violence, as it shows that in-school violence records are twice as frequent as violence in the police records: while roughly 1.6% of juveniles receive a violent SDS, roughly 0.8% of juveniles commit a violent crime according to police records. Since these statistics are derived from the same set of youths unaffected by the EL reform, this suggests that violence in school is twice as likely to occur as violence in the police records.[19] Figure 1 also shows for each secondary state school in Queensland the average count of violent SDS at age 16-17 per individual plotted against the average count of violent crime offences from police records at age 16-20 per individual for the 1987-89 birth cohorts. Figure 1 shows a strong positive correlation, implying that greater violence appears for the same youths in both administrative registries and suggesting that both registries capture similar behavioural traits.

Figure 2 shows that similar individual characteristics correlate with violent behaviour in the school and the police records respectively for the 1987-89 birth cohorts. Enrolment in grade 12, i.e., the final grade of secondary school, appears negatively correlated with the likelihood to receive one or more violent SDS at age 16-17, as well as to commit violent offences at age 16-20. This plausibly reflects the positive selection of students prior to the EL reform in the final grade of secondary school. Males display more violence both in and out of school, while native English speakers appear as likely as others to display violence. Youth born towards the end of the calendar year are more likely to receive violent SDS and as likely to commit violent crime at age 16-20 as peers born earlier in the year. Youth aged 17 appear less likely than youth aged 16 to receive violent SDS, reflecting again the positive selection of students before the reform in school enrolment at age 17. Youth aged 19-20 appear less likely to commit violent crime than their younger peers aged 16-18, reflecting that crime rates peak in the late teens followed by a lasting decline (Quetelet, 1831).

Table 3 shows that individual characteristics appear balanced between treatment and control group juveniles. In particular, Table 3 shows a set of balancing tests of the distribution of individual characteristics across the 1990 cut-off (i.e., the year-of-birth cut-off determining eligibility for the EL reform). Each individual variable was regressed on the EL indicator (i.e., born in 1990-93), a continuous control for distance in year of birth to the 1990 cut-off and their interaction term. Results from OLS in column (1), as well as from Non-Parametric Regression Discontinuity (RD) Estimates using a Uniform Kernel and a Triangular Kernel in columns (2) and (3) respectively show that ITT treatment and control groups are balanced on observable characteristics. On average, ITT treatment and control groups have a similar age and fraction of males, native speakers and male native speakers. “Off time” students, defined as students not attending grade 10 at age 15, i.e., the expected school grade at age 15, appear similarly distributed, and the same applies to “off time” males and “off time” native speakers. ITT treatment and control group youth were born on similar days-months of the year, and this applies also to males and native speakers only.

[19] Section 5 shows that the potential differential detection rate of violence in and out of school does not plausibly affect the conclusions of this study.

4.2 Main Estimates

Table 4 shows our main RD estimates of the causal impact of the EL reform on youth violence. In Table 4, OLS estimates are presented separately for violent SDS in columns (1)-(3) and for police records of violent crime at age 16-17 and age 18-20 in columns (4)-(6) and (7)-(9) respectively. Police records of violent crime include violence against the person, sexual offences, and public order offences by offender in a year. For all outcomes, Panel A shows estimates of the impact of the reform on the count of incidents, i.e., including both extensive and intensive margin effects of the reform on violence, while Panel B shows estimates of the impact of the reform on the likelihood of one or more incidents, i.e., the extensive margin effect of the reform on violence. In all cases, the outcome of interest was regressed on a binary indicator of eligibility for the EL reform (i.e., born in 1990-93), a linear trend for year of birth, their interaction (thus allowing for heterogeneous trends either side of the EL cut-off), age fixed effects and year fixed effects. Fixed effects for gender, native English speaker status, day-month of birth and school at age 15 were progressively added to the specification.

Table 4 shows that the EL reform caused a significant increase in in-school violence. The EL reform led to roughly a 15% increase in the count of violent SDS at age 16-17, and it increased the risk of one or more violent SDS at age 16-17 by 13.2%. This conclusion appears robust to the set of controls and fixed effects included in the specification. The bottom row of Table 4 shows the effect of the EL reform on the likelihood of enrolment in grade 12, i.e., the final grade of secondary school, using the same specifications across columns as in the main analysis. Regardless of the set of controls, a significant increase by more than 12.3% in the likelihood of enrolment in grade 12 is evident.

Figure 3 also provides a visual representation of this result, as it shows a steep jump upwards for treated cohorts in the rate of grade 12 enrolment (Panel A) and in the average dropout grade (Panel B) that increased by a significant 2.3%. Figure 4 does likewise for in-school violence, and it shows a clear increase in violence in school for the 1990 cohort and an even stronger increase for later cohorts. Since the 1990 cohort was only subject to the extended period of compulsory schooling while later cohorts were subject to the extended period of compulsory schooling plus the extended presence in school of the potential early dropouts from earlier cohorts forced into school for longer, later cohorts were subject to a greater dosage of the treatment, and the observed greater rate of in-school violence among later cohorts may reflect this.

The remainder of Table 4 shows estimates of the impact of the EL reform on police records of violence at age 16-17 and age 18-20 in columns (4)-(6) and (7)-(9) respectively. Regardless of the set of controls and fixed effects included in the specification, Table 4 shows that the EL reform did not affect police records of violence at age 16-17, and it reduced the likelihood to commit violent criminal offences at age 18-20 at the extensive margin by nearly 10%. This suggests that the extra year in school mandated by the EL reform reduced violence at age 18-20 for the marginal offender.[20]

Table 5 shows estimates of the impact of the EL reform on property and drug incidents. Columns (1) and (4) show results for in-school property misconduct and illicit substances misconduct at age 16-17, columns (2) and (3) show results for property crime at age 16-17 and 18-20 respectively, and columns (5) and (6) do likewise for drug offences. Property crime includes property damage and theft. In all cases, the same equation specification was estimated as in columns (3), (6) and (9) of Table 4. For each outcome, estimates of the impact of the reform are presented in Panel A for the count of incidents, and in Panel B for the likelihood of one or more incidents.

Results show that the EL reform did not affect the records of property or drug SDS in school. This conclusion emerges whether the extensive or the intensive margin effect of the reform is studied, and it appears robust to the set of controls and fixed effects included in the analysis. In contrast, exposure to the EL reform had a significant effect on property crime and drug offences both at age 16-17 and age 18-20. In particular, the EL reform reduced the count of property crime offences by 26% at age 16-17 and by 21% at age 18-20, and it reduced the likelihood of property crime offences by 11.6% at age 16-17 and by 8% at age 18-20. The EL reform also reduced the count of drug offences by 16% at age 16-17 and by 18% at age 18-20, and it reduced the likelihood of drug offences by 22.7% at age 16-17 and by 14.8% at age 18-20.

Results in Tables 4 and 5 show that the EL reform reduced crime at age 18-20. This conclusion is consistent with the large literature that documents the crime-reducing effect of school attendance. While arrested and incarcerated juveniles are less likely to graduate from high school (Hjalmarsson, 2008) and more likely to become recidivists either in youth or in adulthood (Mendel, 2011; Aizer and Doyle, 2015; Stevenson, 2017; Mueller-Smith and Schnepel, 2020), schools seem to exert a protective factor towards juveniles and reduce the risk of crime. The improved labour market prospects, reduced stigma and reduced criminal capital accumulated by juveniles exposed to the "protective factor" of schooling for longer plausibly explain these results. The EL reform also reduced property and drug offences at age 16-17. Since police records at age 16-17 show a clear reduction in property and drug crime while school records of in-school property and drug SDS remain unaffected, the incapacitation effect of school plausibly explains results for property and drugs crime at age 16-17.

However, Table 4 and Figure 4 also suggest that this crime reduction may carry a social cost in terms of in-school violence, as they show a sizeable increase in records of in-school violence. Violent SDS generally capture less serious acts of violence compared with police records of violence

[20] Bell, Costa and Machin (2021) also present relatively little evidence of violent crime reduction due to MDA laws in the US until after the end of the compulsory schooling period. Machin, Marie, and Vujic (2011) document the insignificant impact of the MDA law in 1972 in the UK on violent crimes.

and thus school and police records of violence should be compared with caution (for example, police records of violence in our analysis include murders, which obviously could not be dealt with by school authorities without appearing also in the police records). However, the lack of significance in the estimated impact of the reform on police records of violence in columns (4)-(6) of Table 4 suggests that violent crime was not affected by the reform at age 16-17, and thus that violence was not merely displaced from outside to inside school. These results rather suggest that the extended compulsory schooling period had a significant crime-reducing impact, especially for property and drug offences, but this is partly offset by a sizeable upsurge in minor acts of violence and victimisation in school mostly unreported to the police. This increase in violence represents an additional social cost linked with MDA laws echoing the findings in Jacob and Lefgren (2003) and Luallen (2006) of increased violence in the locality when school is in session.

4.3 Robustness to Estimation Method and Equation Specification

Appendix Table A.1 shows that the conclusion that the EL reform led to increased in-school violence is robust to estimation method, as consistent results from Non-Parametric RD Estimation using a Uniform Kernel and Triangular Kernel functions are presented in Columns (1)-(3) and (4)-(6) respectively. The same set of control variables are progressively added to the RD specification in columns (1) and (4) that includes heterogeneous trends by year of birth either side of the EL cut-off, age fixed effects and year fixed effects, and estimates appear as a replication of the parametric estimates for violent SDS in Table 4. Appendix Figure A.2 also shows robustness to equation specification, as adding heterogeneous quadratic trends by year of birth either side of the discontinuity cut-off to the specification in Figure 4 does not alter our main conclusion.

4.4 Robustness to Sampling Restrictions

Appendix Table A.2 tests the robustness of results for violent SDS to sampling restrictions, as the impact of the EL reform is re-estimated without "off time" students. As stated above, these are defined as students out of synch, i.e., not attending grade 10 at age 15, the expected school grade at age 15.[21] Thus, the analysis in Appendix Table A.2 includes only "on time" students, who started school in the expected year and did not have to repeat a grade prior to age 16. Since our ITT measure is defined based on the year of birth, these students were fully exposed to "the treatment". The same set of controls is gradually added to the estimated equation in Appendix Table A.2. Estimates appear statistically significant at 1% and larger than in columns (1)-(3) of Table 4, thus providing support for our conclusion that in-school violence increased among youth aged 16-17 due to the reform. Appendix Table A.3 also tests robustness to sampling restrictions, as it replicates the main results for violent SDS limiting the analysis to the 1987-91 cohorts. This ensures that all the cohorts in the

[21] Table 3 shows off time students to be similarly distributed across ITT treatment and control groups.

analysis reached the age 16-17 in 2003-08, i.e., in times of positive GDP growth in Australia and before the Great Recession.[22] Results look like a replication of our main results in Table 4.

4.5 Placebo Estimates

Finally, Appendix Table A.4 presents placebo estimates of the impact of a fake EL reform artificially shifted back to 1989, i.e., by one birth cohort, on violent SDS. Columns (1)-(3) show estimates for the entire sample, whereas columns (4)-(6) show estimates for the 1987-89 cohorts only, i.e., the pre-reform cohorts only. Regardless of whether the analysis is restricted to the pre-EL reform cohorts, estimates appear statistically insignificant and numerically much smaller than those in Table 4 for violent SDS. This confirms the impression from the visual inspection of Figure 4, where no increase in violence in school appears among control cohorts not exposed to the reform.

5. Mechanisms

5.1 Modified Propensity to Punish

Why did the Earning or Learning (EL) reform cause an increase in violence in school? The reform and the resulting school enrolment of one extra cohort might have put pressure on school facilities and personnel, generating a temporary increase in school suspensions. Limited school capacity to absorb the extra enrolment of students actually appears an unlikely mechanism for our findings, given the growing and lasting increase in violence records up until the 1993 cohort in Figure 4. Moreover, if the estimates in Figure 4 reflected an increased propensity of schools to punish, rather than a genuine increase in violence, a general increase in the count of most types of SDS should appear due to the EL reform. However, Table 5 shows no increase in property and drug SDS. The analysis of other, minor infractions is also presented in Appendix Table A.5, and it shows no average change in the count of SDS for non-verbal misconduct, persistent disruptive behaviour or refusal to participate in instruction, and misconduct involving legal substances, with most estimates being statistically indistinguishable from zero.[23] Thus, a systematic increase in the propensity of schools to punish due to the EL reform does not appear to lie behind the increase in in-school violence documented here.

5.2 Unobserved Idiosyncratic Time-School-Specific Shocks

The EL reform may have coincided with some unobserved idiosyncratic time-school-specific shocks, e.g., to school resources, support programmes or school principals, which may have altered the propensity of some schools in particular to punish. For example, some schools may have adopted

[22] See World Bank and OECD national accounts data on Australian GDP per capita growth (%) at: https://data.worldbank.org/indicator/NY.GDP.PCAP.KD.ZG?end=2019&locations=AU&start=1961&view=chart.

[23] Appendix Figure A.3 also shows that SDS records and police crime records at the individual level show the same strong positive correlation before and after the reform, again suggesting that our results are not due to the increased tendency of schools to punish.

a stricter discipline approach towards violence in particular, and Figure 4 may reflect the greater propensity to punish this specific type of misconduct rather than a genuine increase in in-school violence. As stated above, the allocation of SDS is moderated by the Queensland Education head office to ensure consistency in disciplinary actions. However, to further study whether unobserved idiosyncratic time-school-specific shocks may underlie our results, a set of interactions between year fixed effects and the set of time-invariant fixed effects used in Figure 4 are added to the analysis. Appendix Figure A.4 presents the results from this exercise, with estimates including dummies for gender and native English speaker status, day-month of birth fixed effects and school fixed effects, together with interactions between this set of fixed effects and year fixed effects. Age fixed effects are also included in the specification but they are not interacted with year fixed effects, as inclusion of the interactions between age and year fixed effects would be equivalent to adding year of birth fixed effects and our parameter of interest would not be identified. Thus, the only variation left is within school and year coming from the eligibility for the EL reform of juveniles born in 1990-93.

Estimates in Appendix Figure A.4 replicate those in Figure 4, once again showing a sizeable increase in the count and risk of in-school violence due to the EL reform. All the hypothesised confounding factors, e.g., limited school capacity in 2006, changes in school resources, in support programmes, in school principals or in disciplinary policies in response to violence, should be common to all students in a given school in a given year, and thus they should be captured by the set of interactions between school fixed effects and year fixed effects. Thus, results in Figures 4 and A.4 are unlikely to arise from any of these confounding factors.

5.3 Displacement of Violence and In-School Violence at Age 15

The EL reform may have mechanically moved violence from outside to inside school without there being any genuine increase in violence, and results in Figures 4 and A.4 may simply reflect the mechanical effect of the longer time spent in school by ITT juveniles due to the reform. A related concern is that violent crime records from the police and violent SDS from the Education Department may not capture the same types of violence. As there might be differences in the type of violence that leads to an SDS vis a vis an arrest, the EL reform may have increased the detection of minor acts of youth violence, rather than leading the youth in school to become more violent. The same behaviour that used to go unpunished outside of schools may now be punished in school. If this was the case, the increased records of in-school violence may represent a social benefit linked with MDA laws and not a social cost, as they would indicate that more of the underlying violence (that would have taken place anyway outside school) is now actually being detected.

Columns (4)-(6) in Table 4 already showed that no change in violent offences appears from police records, suggesting that a substitution away from violence outside school and towards violence in school is unlikely. However, one additional way to test this is to study the impact of the EL reform on violent behaviour in school of juveniles aged 15. A benefit of restricting the analysis to this group is that it ensures our results are not driven by students who are kept in school due to a change in the MDA law.[24] Juveniles aged 15 were not included in the analysis presented so far because the data start in 2003 and thus the 1987 cohort is only observed since the age of 16. Thus, inclusion of juveniles aged 15 in the analysis would have resulted in an unbalanced dataset with an older ITT control group and a younger ITT treatment group on average. However, to check if Figure 4 merely reflects a shift in the detection of minor acts of violence, a statistical exercise was conducted without the 1987 cohort and focusing on the 1988-93 cohorts at age 15.

Appendix Table A.6 shows OLS estimates of the impact of the EL reform on in-school violence of juveniles aged 15. The Table is organised similarly to Table 4 as control variables and fixed effects are progressively added to the estimation across columns. Results suggest that the EL reform increased the count of violent SDS among juveniles aged 15 by 13.5%, thus to a lesser extent (in % terms) compared with the main effect on in-school violence for juveniles aged 16-17, while it did not significantly increase the likelihood of violent SDS among juveniles aged 15, reflecting that the results in Panel A are driven by a few serial offenders. This is also suggested by the relatively large standard errors in the results for juveniles aged 15. Figure 5 offers a visual representation of the results in column (3). Since juveniles aged 15 were forced to stay in school both before and after the EL reform, these findings indicate that schools experienced a genuine increase in in-school violence and our findings do not simply reflect a shift in the detection of minor acts of violence.

Figure 6 also shows that the same schools experienced the respective increases in in-school violence due to the EL reform documented in Figures 4 and 5, as it shows school-specific heterogeneous estimates of the impact of the EL reform on in-school violence by age for all secondary schools in Queensland. Figure 6 displays a strong and positive correlation between the impact of the EL reform on in-school violence of students aged 16-17 and students aged 15. Even though empirical analysis of whether and how peers affect each other's outcomes would require records of micro-interactions not included in our dataset (see Guryan, Jacob, Klopfer and Groff, 2008), the evidence in Figures 5 and 6 suggests that schools experienced a genuine increase in in-school violence and points towards the presence of a contagion effect of violence across school peers of different ages.

5.4 Compositional Change

MDA laws typically act upon juveniles at the bottom end of the distribution of ability and/or desire to stay in school. One way in which the EL reform may have caused the increased in-school violence in Figure 4 is in fact by altering the characteristics of the student population. In order to

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[24] A similar exercise is presented in Anderson, Hansen and Walker (2013).

study whether the EL reform differentially affected the dropout behaviour of different types of juveniles, the results in Figure 3 were re-estimated separately for juveniles with and without a violent crime record by age 15. Figure 7 shows the results of this exercise, with the left-hand side panels showing the impact of the EL reform on the dropout behaviour of juveniles with no violent crime record at age 15 and the right-hand side panels doing likewise for juveniles with one or more violent crime records at age 15. Figure 7 shows that, while the EL reform raised the likelihood of enrolment in Grade 12 by 12.2% and the average dropout grade by 2.3% for juveniles with no violent criminal record by age 15, it raised the likelihood of enrolment in Grade 12 by 47.4% and the average dropout grade by 5.9% for juveniles with a violent criminal record by age 15.[25] Juveniles with a violent criminal record by age 15 were much more likely than others to drop out of school early, and the EL reform markedly reduced this gap. Thus, results in Figure 7 suggest that the reform led many unruly juveniles who face a disproportionate risk of crime to spend an additional year in school.

To further study whether this may really be a mechanism driving our results, Table 6 replicates the main results for in-school violence at age 16-17 in Table 4 separately for youth with and without violent crime records by age 15. Columns (1)-(3) show the results for youth with no violent crime record at age 15, while columns (4)-(6) do likewise for youth with one or more violent crime records at age 15. The same equation specifications are estimated across columns as in Table 4. Results indicate that the EL reform caused an increase in in-school violence at age 16-17 across all juveniles, although the increase was much larger for youth with a violent crime record at age 15. While other juveniles faced an 11-12% greater likelihood of in-school violence at age 16-17 due to the reform, for juveniles with a violent crime record at age 15 the likelihood of in-school violence at age 16-17 increased by 80-90% (depending on equation specification). Panel B also shows results splitting the sample between classmates of youth with violent crime records at age 15 and others. Estimates show that the increase in in-school violence documented so far is driven by classes in which one or more juveniles with violent crime records at age 15 are enrolled (columns (4)-(6)), while no increase in in-school violence appears in other classes (columns (1)-(3)). In sum, Figure 7 and Table 6 suggest that a key mechanism through which the EL reform led to an increase in in-school violence was indeed by keeping unruly juveniles who face a disproportionate risk of violence in school for an extra year.

6. Discussion

The relevance of the findings presented so far is threefold. First, they show that Minimum Dropout Age (MDA) laws may generate a significant contemporaneous increase in in-school violence that has

[25] In all cases, percent effects are calculated as the estimated coefficient divided by the mean of the dependent variable prior to the EL reform (i.e., ITT = 0).

the potential to alter the school environment and the learning process for students who would have remained in school regardless of the MDA law. This implies that the exclusion restriction may be violated in prior studies that instrument the dropout behaviour in the earnings or crime equations using MDA laws, as these laws may modify the school environment in addition to mandating one extra year of compulsory schooling for target juveniles. Figures 8 and 9 show the potential for in-school violence to affect the trajectories of juveniles in a lasting manner, as they plot school-specific estimates of the impact of the Earning or Learning (EL) reform on in-school violence at age 16-17 against school-specific estimates of the impact of the EL reform on future criminality at age 18-20 (Figure 8), and against school-specific estimates of the impact of the reform on the fraction of youth leaving secondary school with a school certificate (Figure 9). Since individual test scores are not available for the years of our analysis, the analysis in Figure 9 complements our dataset with school-level records since 2005 on the school-level fraction of students who obtained a certificate at the end of secondary school.

Figure 8 shows that youth enrolled in schools where the reform led to a greater increase in in-school violence at age 16-17 were more likely to commit one or more criminal offences at age 18-20. Figure 9 shows that the same juveniles were also less likely to obtain a school certificate. Both Figures include all secondary schools in Queensland, both estimate the same equation specification as in column (3) of Table 4 and both display statistically significant correlations between the impact of the reform on in-school violence of students aged 16-17 and later outcomes. Given the potential negative effects of peers in school (Carrell and Hoekstra, 2010; Carrell, Hoekstra and Kuka, 2018) and exposure to violence (Ang, 2021), these findings point towards the negative impact that MDA laws can have on the school environment and the learning process of juveniles. In turn this unveils one potential mechanism behind the non-significant reduction in violent crimes resulting from MDA laws in some settings (Machin, Marie, and Vujic, 2011) and behind the zero earnings returns to school in some studies that use MDA laws to instrument dropout behaviour in the earnings equation (Pischke and von Wachter, 2008; Clark and Royer, 2013).

Second, our findings show that the contemporaneous effect of compulsory school on crime is more complex than the pure incapacitation effect documented to date. Third, the fact that police records detect no change in violence while school records detect a sizeable increase suggests that the increase in violence caused by the reform was largely unreported to the police. This reflects in part the fact that violent SDS often capture less serious acts of violence compared with police records, but it also shows the importance of combining school and police records in the study of education policy and youth delinquency. To further substantiate this claim, Figure 10 shows a violent crime-age profile that distinguishes in-school violence from police records of violence at all ages from 16-20. A recent literature documents that the timing of school eligibility affects the propensity to commit crime in adolescence (Cook and Kang, 2016; Depew and Eren, 2016; and Landersø, Nielsen and Simonsen, 2016), although none of the existing studies uses both education and police administrative registries to study youth violence. Since in Queensland the formal age of school start is defined by the timing of birth and administrative rules imply that children are expected to start grade one in the calendar year in which they turn six years old (QGOV, 2018b), children born one day apart, i.e. December 31st versus January 1st, are expected to start school one year apart and face a one-year difference in timing of administratively determined school start.[26] At a given 1st January cut-off, children born in the final months of the prior calendar year (i.e., September-December) will likely start school one year earlier than their counterparts born in January-April of the following year. Thus, Figure 10 focuses on juveniles born 100 days either side of the 1st January cutoff, and shows the results of a series of Regression Discontinuity (RD) estimates using OLS and controlling for distance to the 1st January cut-off, pre-cut-off indicator, their interaction term, school fixed effects, year of birth fixed effects, gender fixed effects and native English speaker fixed effects. Robust standard errors were clustered at the level of the running variable, i.e., the day-month of birth.

Compared with juveniles born on or just after the 1st January, Figure 10 shows that early-school-eligible (ESE) juveniles, i.e., born within 100 days before the 1st January cut-off, face a greater risk of committing violent offences from age 18 up until age 20, i.e., after the compulsory schooling age. Prior to age 18, when they must attend school, ESE and control group juveniles appear to be equally likely to commit violent crime. However, use of education records reveals that ESE juveniles are in fact more likely to engage in violence in school at age 16 and 17. This violent crime-age profile is consistent with the notion that schools exert a protective factor keeping juveniles away from crime up until they leave school, but it is also consistent with the notion that most acts of violence and victimisation in school are not reported to the police. Due to the severe under-reporting of in-school crime to the police, prior studies that fail to consider in-school behaviour may over-estimate the crime-reducing impact, and therefore the desirability, of MDA laws in the short run.

Finally, Table 7 reports back of the envelope cost-benefit calculations for the EL reform. Based on our earlier results, the estimated foregone costs of crime were calculated as benefits using the same methodology as Lochner and Moretti (2004), and the costs of both SDS and keeping students in school for one extra year were additionally incorporated. Costs of violent SDS and property SDS are taken from Table 2 in Miller, Cohen and Wiersema (1996) as in Lochner and Moretti (2004), and

[26] A large literature also shows that starting school at a younger age leads to a significant academic disadvantage from childhood and may alter the entire path of skill acquisition (Bedard and Dhuey, 2006; Datar, 2006; Puhani and Weber, 2007; Cunha and Heckman, 2008; McEwan and Shapiro, 2008; Elder and Lubotsky, 2009; Crawford, Dearden, and Meghir, 2010; Black, Devereux, and Salvanes, 2011; McCrary and Royer, 2011; and Fredriksson and Ockert, 2013).

violent SDS are given equal costs as assault with no injury while property SDS are given equal costs as larceny. Thus, in both cases SDS are given the minimum cost across different types of violent and property crimes respectively. Costs of drug crimes are based on the US Department of Justice (2011) victim costs and other crime costs as in Bell, Costa and Machin (2021). As reported in Table 7, by age 20 the benefit-cost ratio shows a return of over 1.2 dollars per dollar spent on the policy. While the increased records of in-school violence generate an additional social cost linked with the reform, this conclusion appears robust to their inclusion in the cost-benefit analysis of the EL reform and in line with cost-benefit estimates of US educational reforms (see Bell, Costa and Machin, 2021). These estimates are only suggestive and they constitute a lower bound of the true cost of increased in-school violence since they omit to quantify the negative impact of exposure to violence and disruptive peers on the wellbeing, human capital accumulation and future productivity of youth. However, they are indicative of the economic cost of the increased in-school violence effect estimated in our study.

7. Conclusion

A large literature has documented the beneficial effects of compulsory school attendance, one of which is reduced crime. This has been a major driver of policy in the United States and elsewhere towards schooling and other youth intervention programmes aiming to support the path of human capital formation and labour market prospects, and discourage criminal participation of at-risk youth (see, e.g., Heller, 2014). However, little is known about the educational experience of juveniles when compulsory schooling laws are enacted to keep them in school for longer. Using administrative data linking education and criminal records at the individual level for all juveniles in the state school system in Queensland from 2003 to 2013, this study examines the impact of a recent minimum dropout age (MDA) reform on the educational experiences and criminal trajectories of juveniles.

The *"Earning or Learning"* (EL) reform raised the MDA from 16 to 17 in Queensland on the 1st January 2006, and a series of regression-discontinuity (RD) estimates are derived to estimate its impact on the dropout behaviour and delinquency of juveniles. The EL policy experiment, combined with our data, provides a rare opportunity to assess the effectiveness of modern MDA laws in reducing youth delinquency both in and outside school. From the perspective of a policy maker designing a policy that would alter the school dropout behaviour, this parameter is directly of interest.

Juveniles exposed to the reform spent longer time in school and experienced a significant and lasting reduction in their propensity to commit criminal offences. However, school records also show that the same juveniles were more likely to display violence in school by age 17. This conclusion appears robust to the set of controls included in the estimated equation, to estimation method and alternative sampling restrictions and falsification tests. Additional tests also show that the increased in-school violence is not the result of the Great Recession, the increased propensity of schools to punish, nor the result of any other unobserved time-school-specific idiosyncratic shock. Evidence of greater in-school violence appears also for students aged 15 who had to attend school both before and after the EL reform, suggesting that the reform generated a genuine increase in in-school violence.

This increase in in-school violence is plausibly explained by the compositional change to the student population induced by the EL reform. The dropout behaviour of juveniles with one or more violent criminal records at age 15 was affected to a much larger extent than the dropout behaviour of other juveniles. Moreover, juveniles with one or more violent criminal records at age 15 display a much greater increase in the likelihood to commit in-school violence at age 16-17 and the results are concentrated among the classes in which they were enrolled at age 15.

The extended compulsory schooling period had a significant crime-reducing impact, especially for property and drug offences, but this is partly offset by a sizeable upsurge in violence and victimisation in school mostly unreported to the police. While violent SDS generally indicate less serious acts of violence compared with police records of violence, their increase represents an additional social cost linked with MDA laws. Analysis of later outcomes shows that the increase documented here in in-school violence is associated with greater likelihood of criminality at age 18-20 and lower likelihood of graduation at the end of secondary school. A cost-benefit analysis reveals that the economic benefit of the crime reduction largely justifies the EL reform even when greater in-school violence is considered as an additional social cost linked with the reform.

From a policy perspective, this study highlights the need to supplement youth justice policies and MDA law changes with services that help keep students interested and engaged in learning and prevent the potential increase in school violence documented here. Modern compulsory schooling laws must go beyond fines and other sanctions for school absenteeism and must include effective approaches to combat disengagement and help students stay in school out of choice, even when dropping out is permitted. If an adolescent at risk is forced to stay at school, this can cause trouble on the school premises, and the school system needs appropriate support to be effective. Generating an understanding of the education experience of adolescents at risk and their delinquent behaviour in school is a first step toward helping them avoid the revolving door between poor school behaviour, juvenile delinquency and future incarceration.

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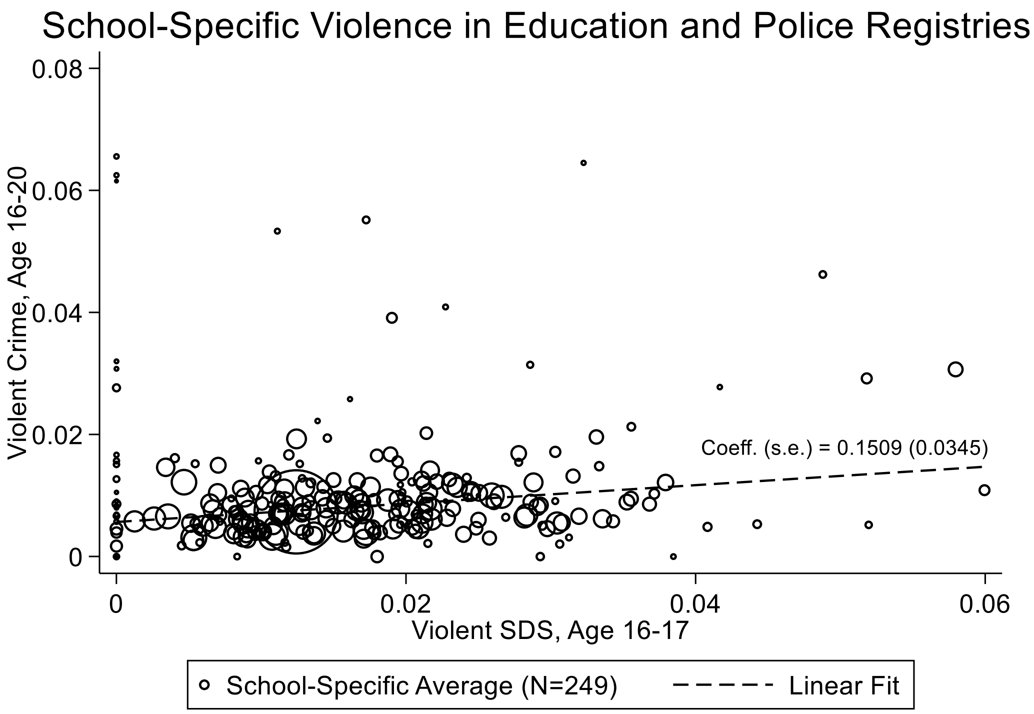
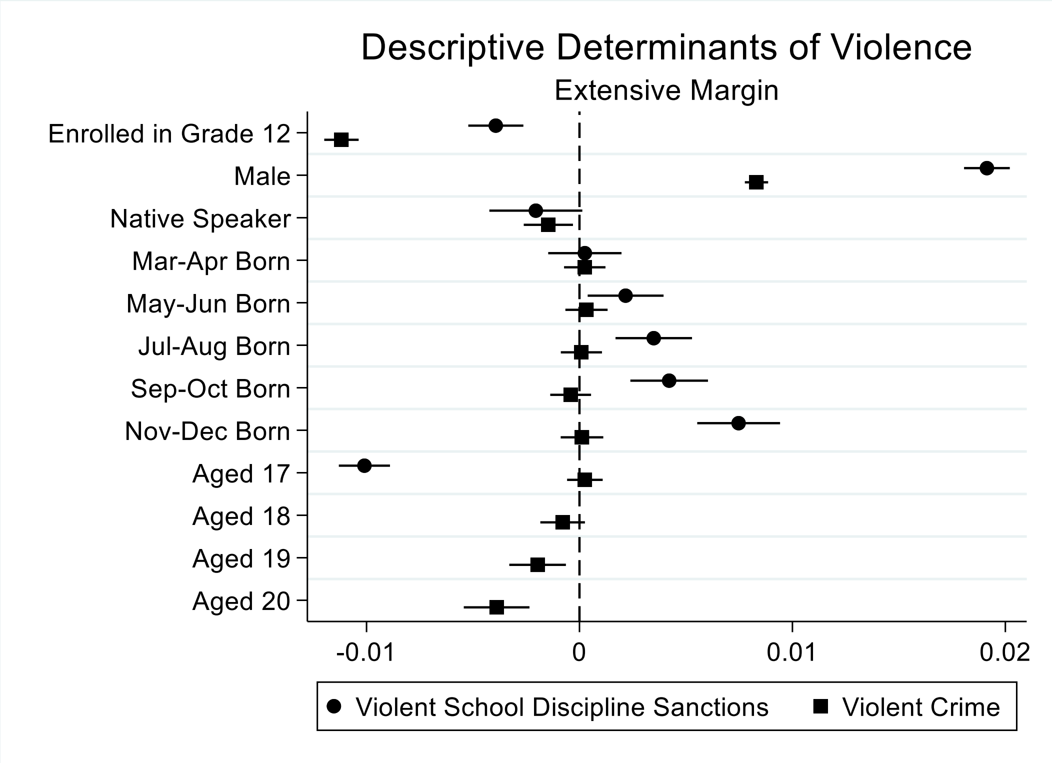
Figure 1. Correlation between School-Specific Violent School Discipline Sanctions (SDS) and Violent Crime in the 1987-89 Birth Cohorts



Notes: Figure shows for each school the average number of violent School Discipline Sanctions (SDS) at age 16-17 plotted against the average number of violent crime offences at age 16-20 per individual in the 1987-89 birth cohorts. Violent crime includes violence against the person, sexual offences, and public order offences by offender in a year. Violent SDS include violent SDS received per youth in a year. School-specific average measured based on school of enrolment at age 15.

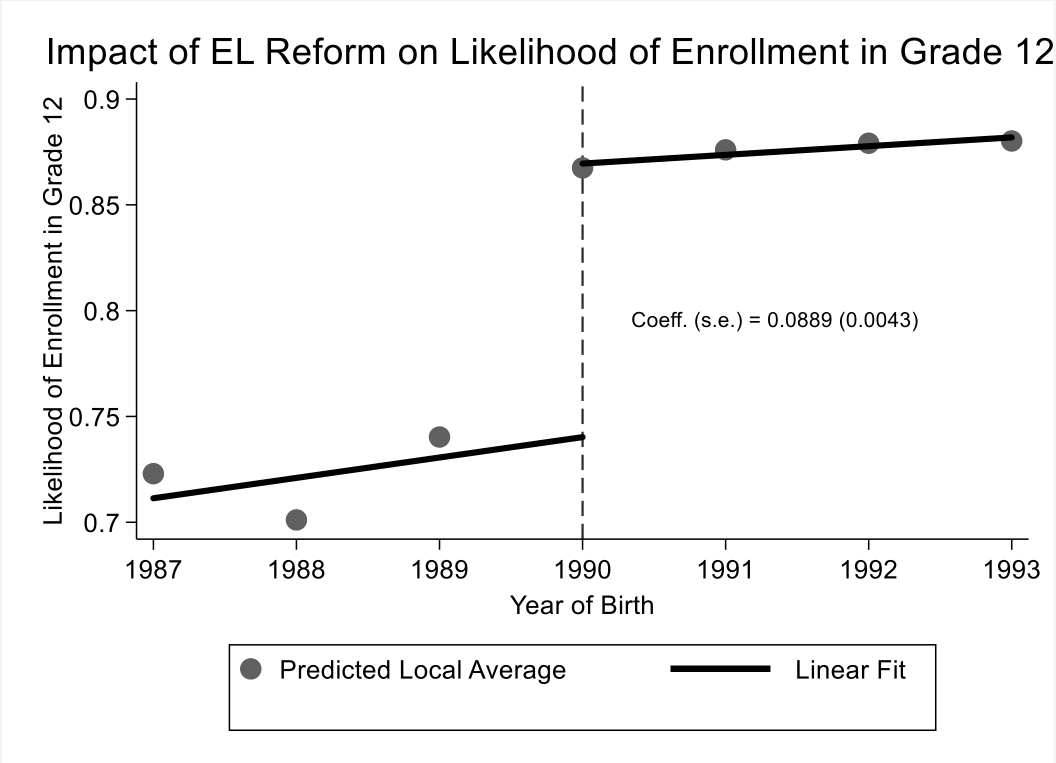
Figure 2. Descriptive Determinants of Violent School Discipline Sanctions (SDS) and Violent Crime in the 1987-89 Birth Cohorts



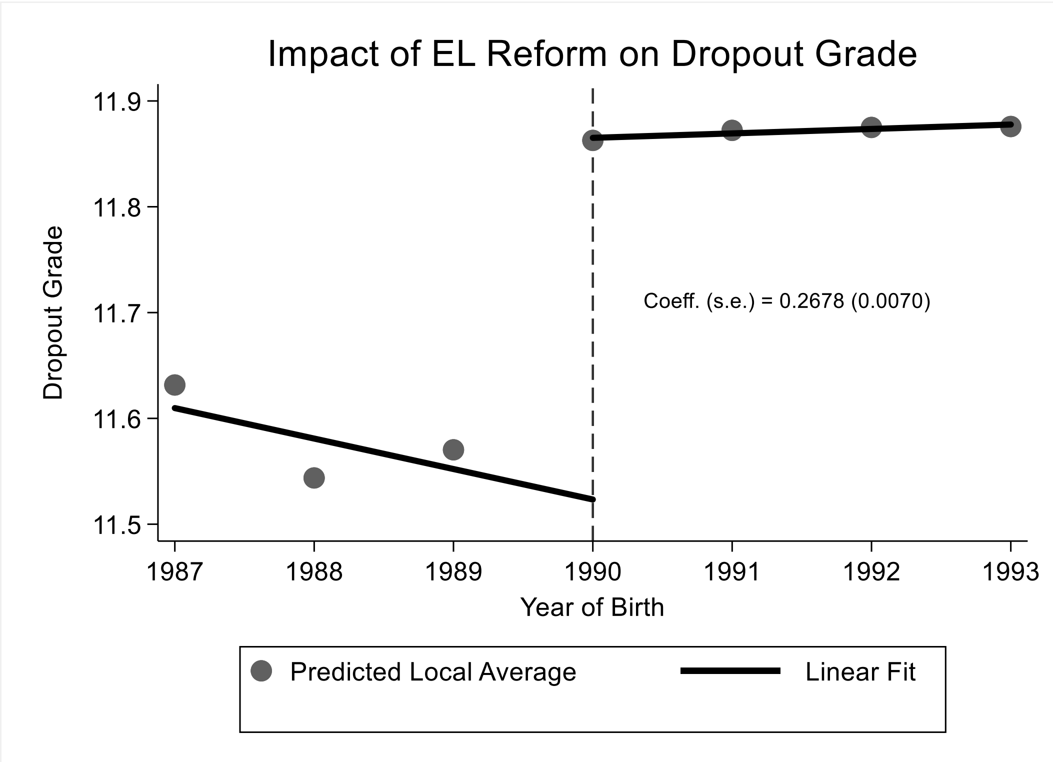
Notes: Figure shows point estimates and 95 percent confidence intervals of the determinants of violent school discipline sanction (SDS, extensive margin) and violent crime offences (extensive margin) in the 1987-89 birth cohorts. All estimates are obtained from OLS regression specifications which include year fixed effects. Standard errors are clustered at the individual level. Violent crime offences include violence against the person, sexual offences, and public order offences by offender in a year. Violent SDS include violent SDS received per youth in a year.

Figure 3. Estimates of Impact of Earning or Learning (EL) Reform on Dropout Behaviour

Panel A



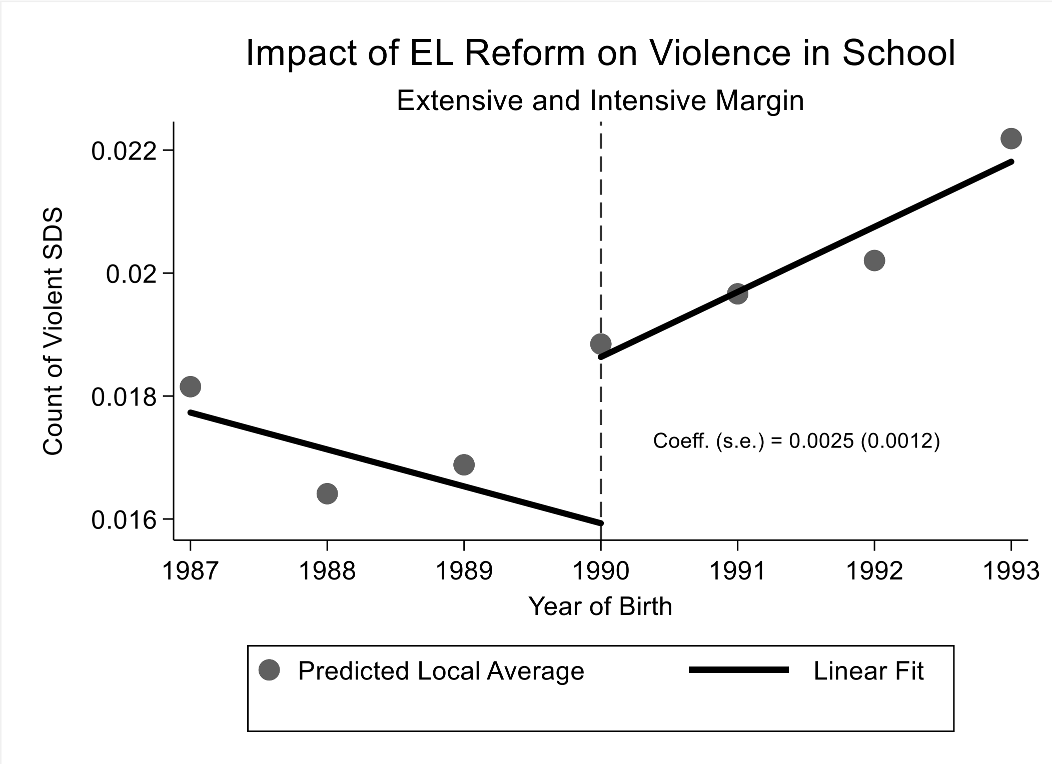
Panel B



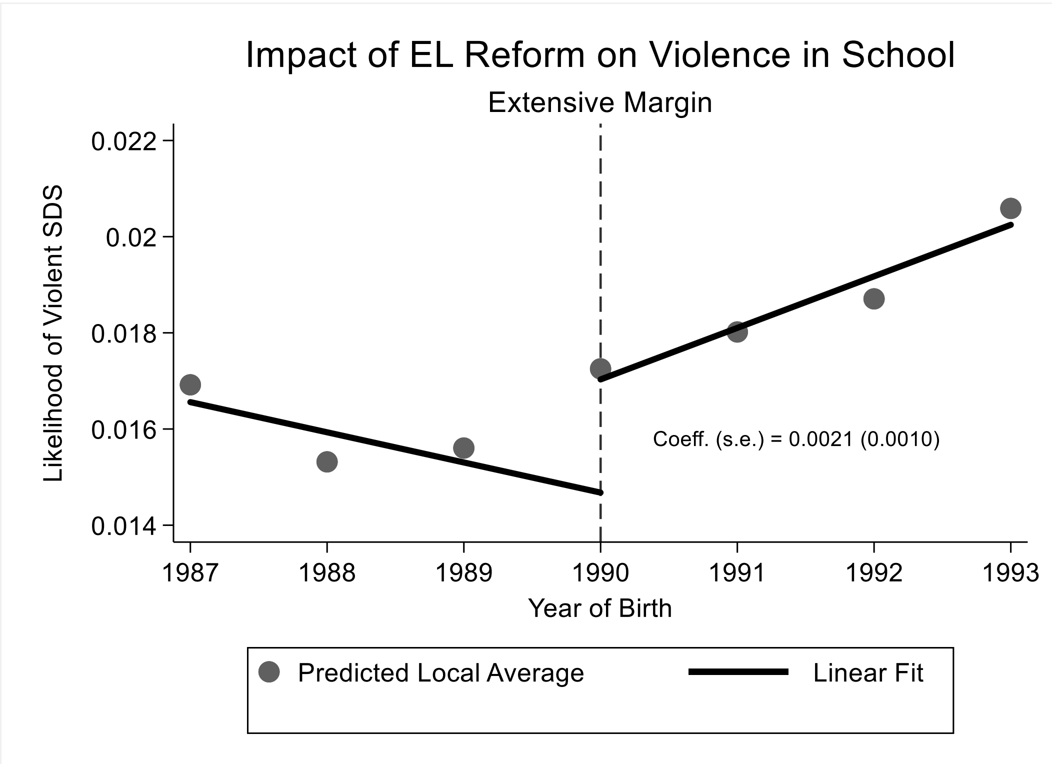
Notes: Figure shows estimates of the causal effect of the Earning or Learning (EL) Reform on likelihood of enrolment in grade 12 and dropout grade. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are age fixed effects, year fixed effects, dummies for whether the youths are male and whether they are native English speakers, day-month of birth fixed effects and school fixed effects, together with heterogeneous linear trends by year of birth either side of the discontinuity window. School fixed effects are measured at age 15.

Figure 4. Estimates of Impact of Earning or Learning (EL) Reform on Violence in School

Panel A



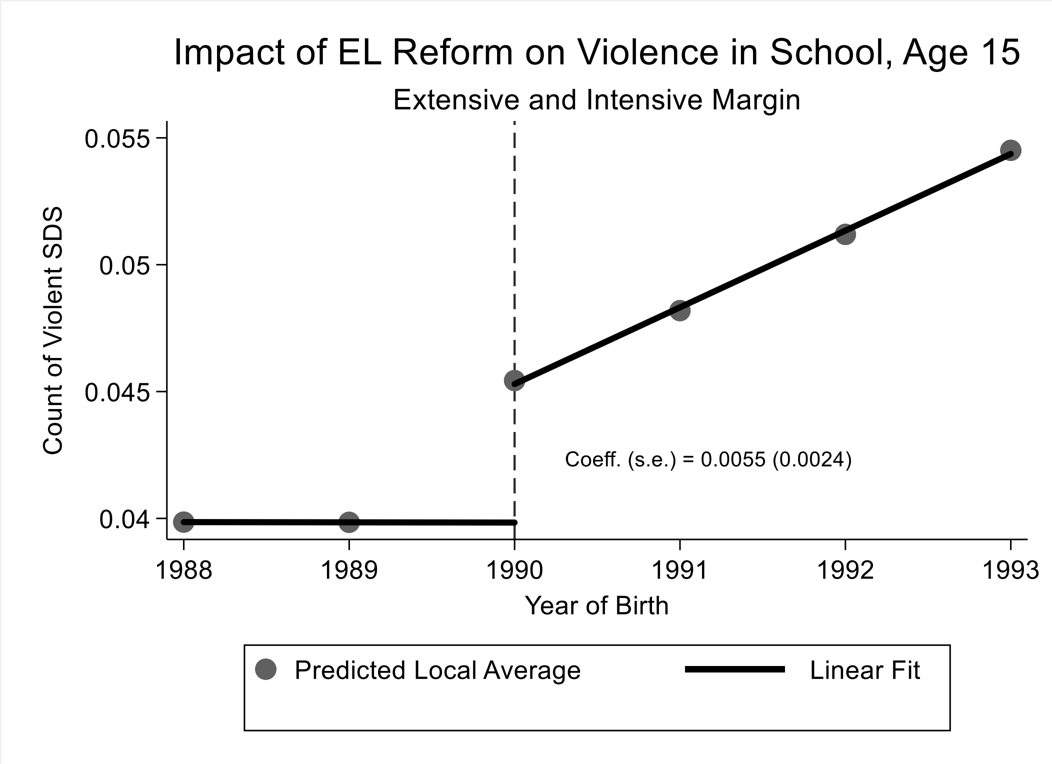
Panel B



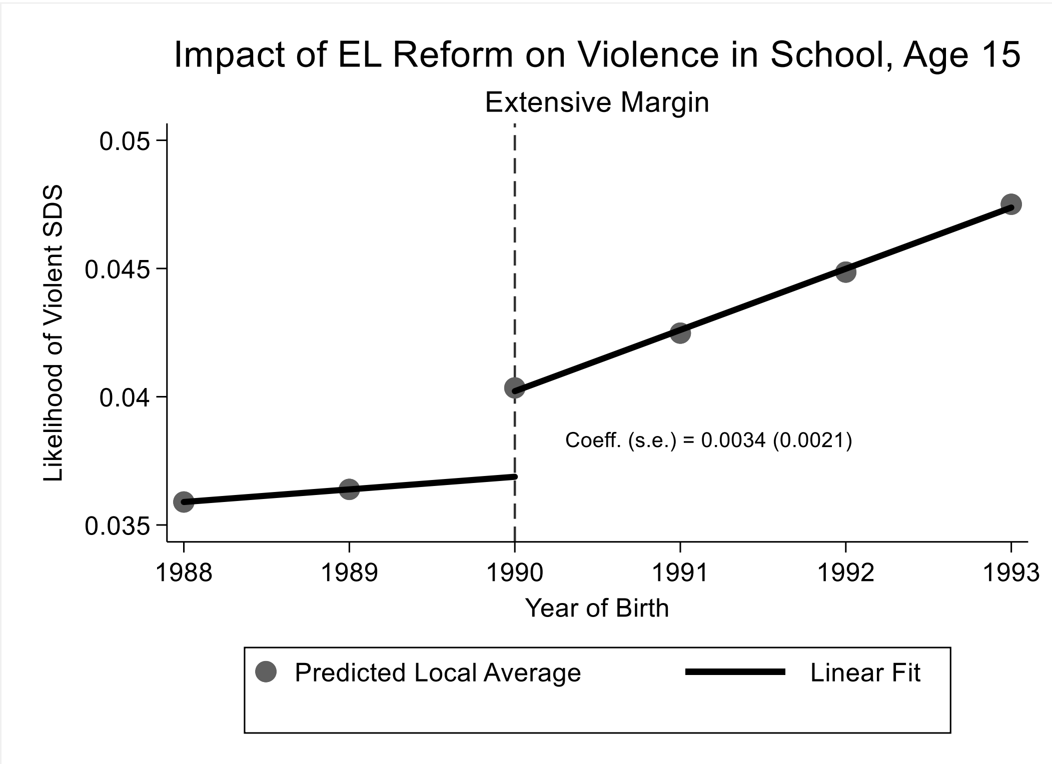
Notes: Figure shows estimates of the causal effect of the Earning or Learning (EL) Reform on violent school discipline sanctions (SDS) at age 16-17. Estimates are shown at the extensive and intensive margin in Panel A, and at the extensive margin in Panel B. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are age fixed effects, year fixed effects, dummies for whether the youths are male and whether they are native English speakers, day-month of birth fixed effects and school fixed effects, together with heterogeneous linear trends by year of birth either side of the discontinuity window. School fixed effects are measured at age 15.

Figure 5. Estimates of Impact of Earning or Learning (EL) Reform on Violence in School, Age 15

Panel A

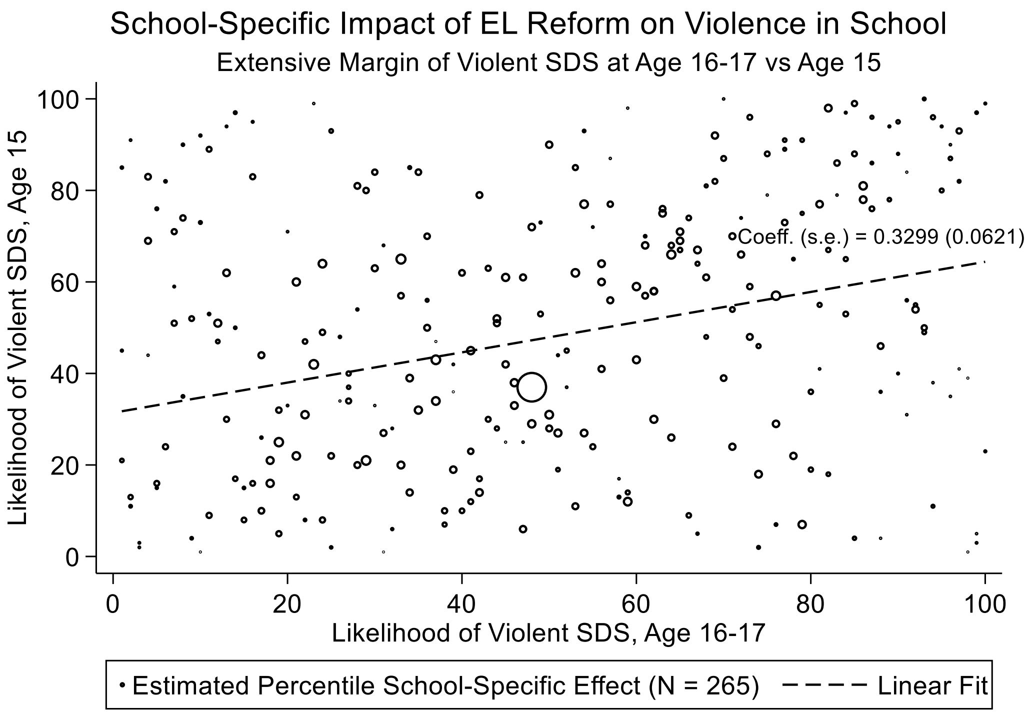


Panel B



Notes: Figure shows estimates of the causal effect of the Earning or Learning (EL) Reform on violent school discipline sanctions (SDS) at age 15. Estimates are shown at the extensive and intensive margin in Panel A, and at the extensive margin in Panel B. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are dummies for whether the youths are male and whether they are native English speakers, day-month of birth fixed effects and school fixed effects, together with heterogeneous linear trends by year of birth either side of the discontinuity window. School fixed effects are measured at age 15.

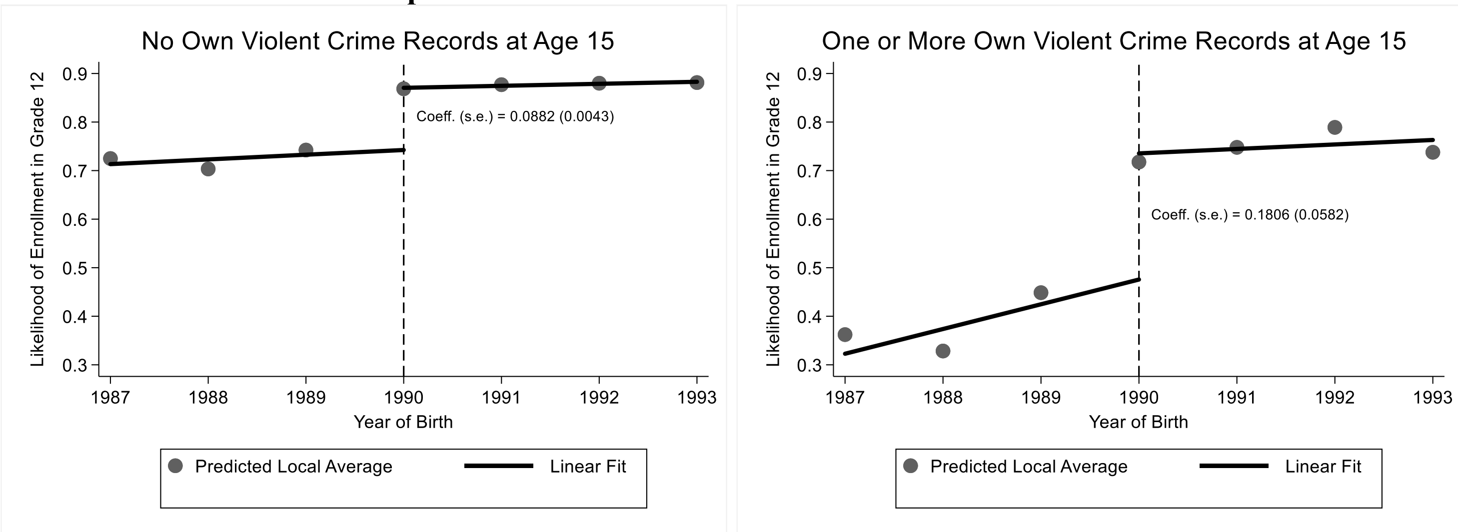
Figure 6. Estimates of School-Specific Impact of Earning or Learning (EL) Reform on Violence in School by Age



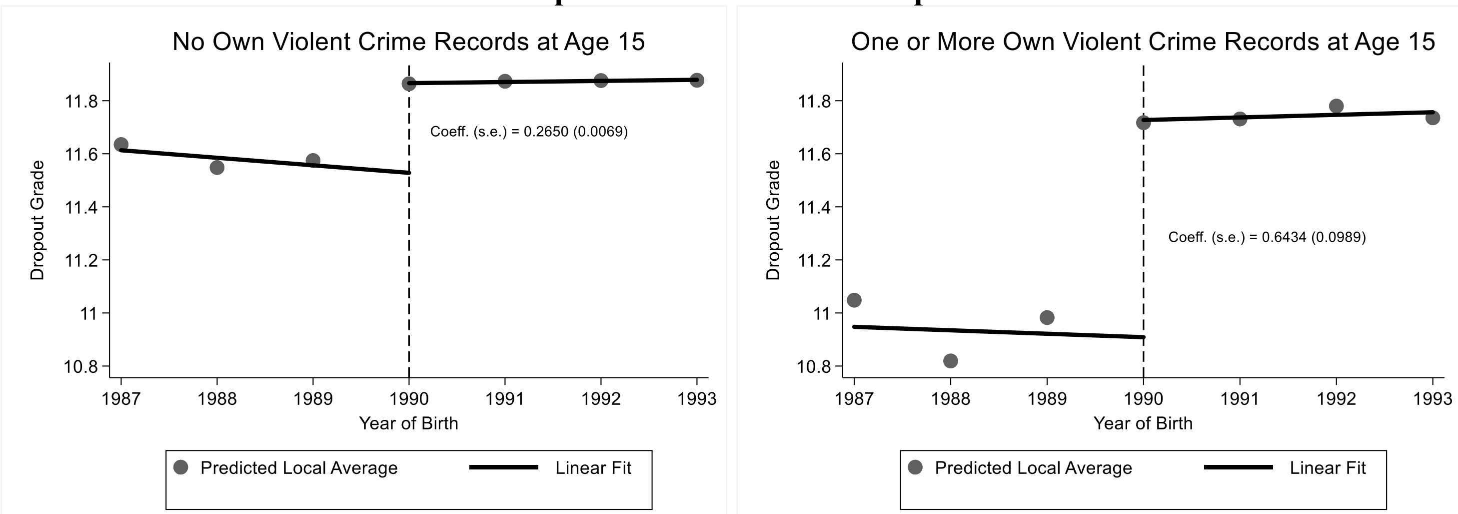
Notes: Figure shows school-specific percentile estimates of the causal effect of the Earning or Learning (EL) Reform on violent school discipline sanctions (SDS) at age 16-17 plotted against school-specific percentile estimates of the causal effect of the EL Reform on violent SDS at age 15. Estimates are shown at the extensive margin. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are age fixed effects, dummies for whether the youths are male and whether they are native English speakers, day-month of birth fixed effects and school fixed effects measured at age 15, together with heterogeneous linear trends by year of birth either side of the discontinuity window.

Figure 7. Estimates of Impact of Earning or Learning (EL) Reform on Dropout Behaviour by Criminal Background

Panel A. Impact of EL Reform on Likelihood of Enrolment in Grade 12

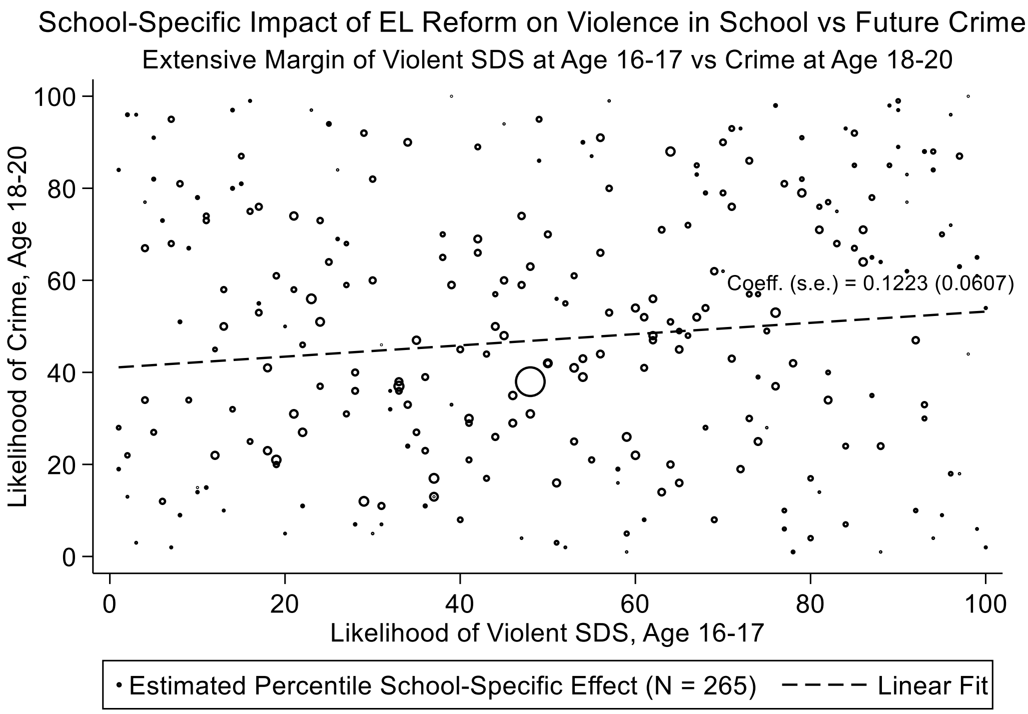


Panel B. Impact of EL Reform on Dropout Grade



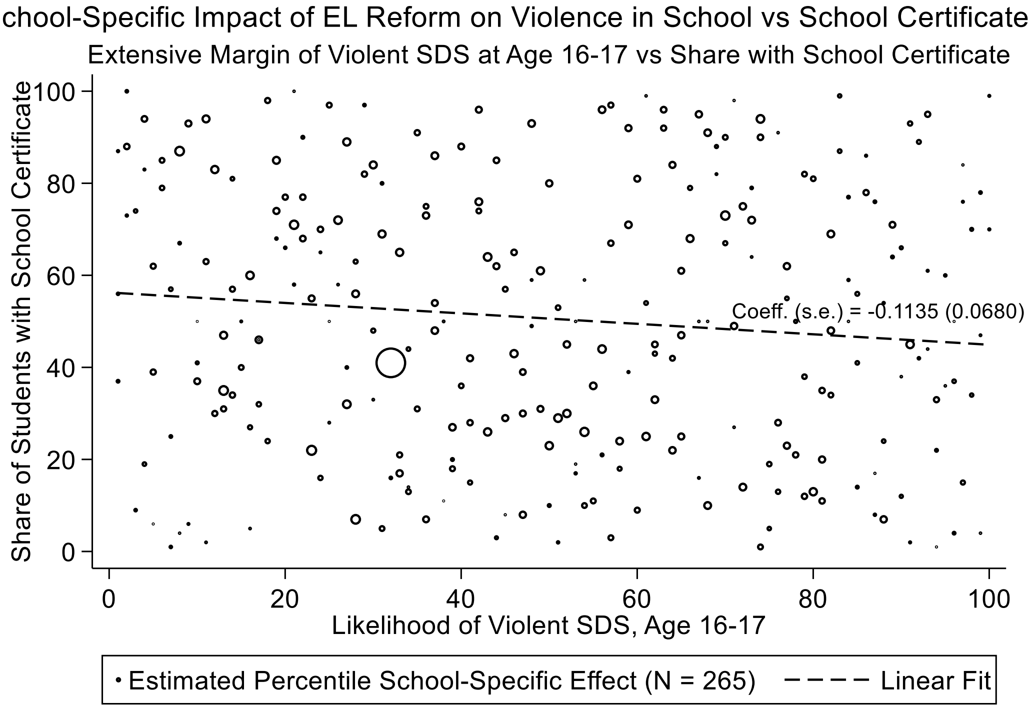
Notes: Figure shows estimates of the causal effect of the Earning or Learning (EL) Reform on likelihood of enrolment in grade 12 (Panel A) and dropout grade at age 16-17 (Panel B) separately for juveniles without violent criminal records at age 15 (left-hand side panels) and with violent criminal records at age 15 (right-hand side panels). Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are age fixed effects, year fixed effects, dummies for whether the youths are male and whether they are native English speakers, day-month of birth fixed effects and school fixed effects, together with heterogeneous linear trends by year of birth either side of the discontinuity window. School fixed effects are measured at age 15.

Figure 8. Estimates of School-Specific Impact of Earning or Learning (EL) Reform on Violence in School at Age 16-17 vs Crime at Age 18-20



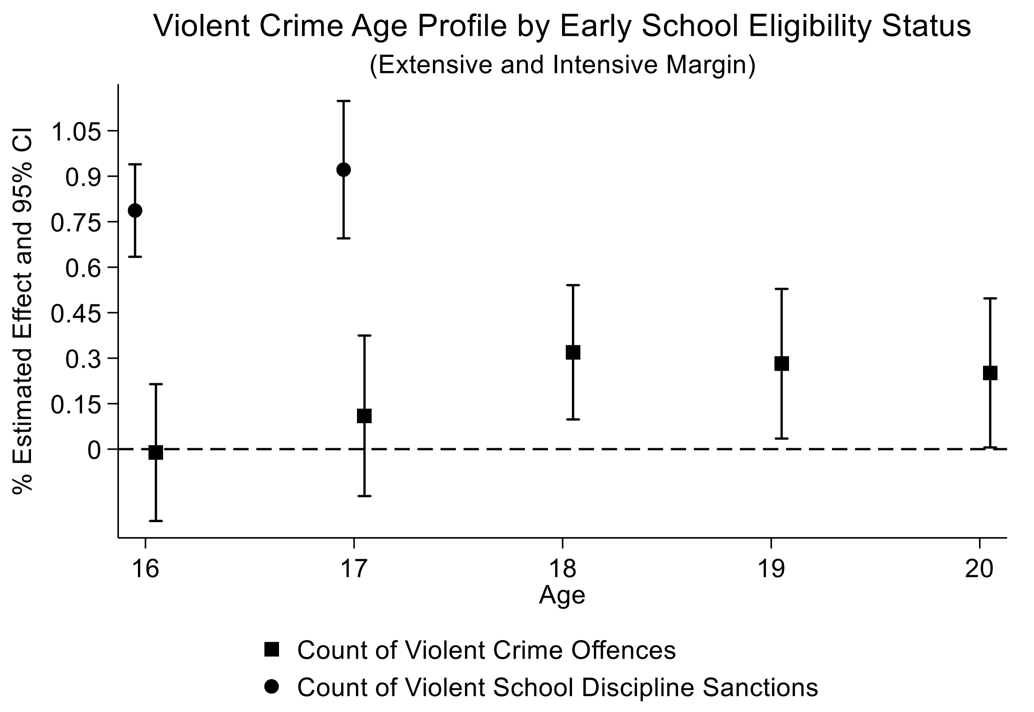
Notes: Figure shows school-specific percentile estimates of the causal effect of the Earning or Learning (EL) Reform on violent school discipline sanctions (SDS) at age 16-17 plotted against school-specific percentile estimates of the causal effect of the EL Reform on crime at age 18-20. Crime offences include property damage and theft, drugs and violence by offender in a year. Violent crime includes violence against the person, sexual offences, and public order offences by offender in a year. Estimates are shown at the extensive margin. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are age fixed effects, dummies for whether the youths are male and whether they are native English speakers, day-month of birth fixed effects and school fixed effects measured at age 15, together with heterogeneous linear trends by year of birth either side of the discontinuity window.

Figure 9. Estimates of School-Specific Impact of Earning or Learning (EL) Reform on Violence in School at Age 16-17 vs End-of-Secondary School Certificate



Notes: Figure shows school-specific percentile estimates of the causal effect of the Earning or Learning (EL) Reform on violent school discipline sanctions (SDS) at age 16-17 plotted against school-specific percentile estimates of the causal effect of the EL Reform on the percentage of students awarded Senior Certificates with OP-eligibility or awarded a VET qualification. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are age fixed effects, dummies for whether the youths are male and whether they are native English speakers, day-month of birth fixed effects and school fixed effects measured at age 15, together with heterogeneous linear trends by year of birth either side of the discontinuity window.

Figure 10. Violent Crime Age Profile by Early School Eligibility Status



Notes: Figure shows point estimates and 95 percent confidence intervals of the % causal effect of early school eligibility (ESE) on the count of violent crimes and violent school discipline sanctions (SDS) by age. The % causal effects of interest were measured as the estimated coefficients deflated by the mean of the dependent variable in the control group (i.e., ESE = 0) separately at all ages. All estimates are obtained from OLS regression specifications on individuals born within 100 birthdates either side of the 1st January cut-off. All specifications include distance to the 1st January cut-off, pre-cut-off indicator (Nov-Dec = 1), their interaction term, school fixed effects and year of birth fixed effects. Robust standard errors were clustered at the day-month of birth level. Control variables included are dummies for whether the youths are male and whether they are native English speakers. School fixed effects are measured at age 15. Violent crime includes violence against the person, sexual offences, and public order offences by offender in a year. SDS include violent SDS received per youth in a year.

Table 1. Structure of Panel Dataset

Age	16	17	18	19	20	Total
Year of Birth						
1987	33,220	33,227	33,224	33,228	33,231	166,130
1988	38,893	38,896	38,900	38,907	38,902	194,498
1989	39,903	39,897	39,915	39,908	39,894	199,517
1990	41,679	41,672	41,676	41,687	41,677	208,391
1991	41,967	41,960	41,961	41,975	41,972	209,835
1992	43,458	43,452	43,459	43,476	43,481	217,326
1993	43,398	43,404	43,416	43,421	43,422	217,061
Total	282,518	282,508	282,551	282,602	282,579	1,412,758

Notes: Table shows the structure of the dataset used for the main analysis. Each unit of observation here is an individual-year. Crime offences per individual are studied at age 16-20 and School Discipline Sanctions (SDS) at age 16-17. Crime offences include property damage and theft, drugs and violence by offender in a year. Violence includes violence against the person, sexual offences, and public order offences by offender in a year. SDS include property misconduct SDS, illicit substance SDS and violent SDS received per youth in a year.

Table 2. School Disciplinary Sanctions (SDS) and Crime Offences in 1987-89 Birth Cohorts

	(1)	(2)	(3)	(4)	(5)	(6)
	Age 16 to 20	Age 16	Age 17	Age 18	Age 19	Age 20
A. Violent SDS Received						
Number	3,561	2,374	1,187			
Share of Age 16 to 17	1	0.6667	0.3333			
Percent Rate	0.0159	0.0212	0.0106			
B. Any SDS Received						
Number	4,927	3,276	1,651			
Share of Age 16 to 17	1	0.6649	0.3351			
Percent Rate	0.0220	0.0292	0.0147			
Juveniles		112,083				
Observations		224,036				
C. Violent Crime Committed						
Number	4,566	828	974	957	956	851
Share of Age 16 to 20	1	0.1813	0.2133	0.2096	0.2094	0.1864
Percent Rate	0.0082	0.0074	0.0087	0.0085	0.0085	0.0076
D. Any Crime Committed						
Number	22,326	4,491	4,827	4,754	4,459	3,795
Share of Age 16 to 20	1	0.2012	0.2162	0.2129	0.1997	0.1700
Percent Rate	0.0399	0.0401	0.0431	0.0424	0.0398	0.0339
Juveniles			112,083			
Observations			560,145			

Notes: Table shows the count of School Discipline Sanctions (SDS) received and crime offences committed by the 1987-89 birth cohorts at all ages and separately by age. The share of SDS received and crime offences committed at each age are also shown, together with the fraction of individuals who received SDS or committed crime offences at all ages and at each age separately. SDS in Panel B include property misconduct SDS, illicit substance SDS and violent SDS received per youth in a year. Crime offences in Panel D include property damage and theft, drugs and violence by offender in a year. Violence includes violence against the person, sexual offences, and public order offences by offender in a year.

Table 3. Balancing of Individual Characteristics across the Earning or Learning (EL) Reform, Aged 16-17

	Impact on EL Reform on:	Parametric OLS Estimates	Non-Parametric RD Estimates using Uniform Kernel	Non-Parametric RD Estimates using Triangular Kernel
		(1)	(2)	(3)
(1)	Age	0.0000	0.0000	0.0000
		(0.0001)	(0.0001)	(0.0001)
(2)	Male	0.0033	0.0033	0.0028
		(0.0046)	(0.0046)	(0.0063)
(3)	Native Speaker	-0.0042	-0.0042	-0.0024
		(0.0026)	(0.0026)	(0.0035)
(4)	Male * Native Speaker	0.0002	0.0002	-0.0005
		(0.0046)	(0.0046)	(0.0063)
(5)	Off Time	0.0129	0.0129	0.0066
		(0.0119)	(0.0119)	(0.0164)
(6)	Off Time * Male	0.0072	0.0072	0.0030
		(0.0071)	(0.0071)	(0.0098)
(7)	Off Time * Native Speaker	0.0070	0.0070	0.0082
		(0.0114)	(0.0114)	(0.0157)
(8)	Day-Month of Birth	-2.0490	-2.0490	-2.3118
		(9.6055)	(9.6078)	(13.2200)
(9)	Day-Month of Birth * Male	0.2693	0.2693	0.0796
		(4.9029)	(4.9040)	(6.7166)
(10)	Day-Month of Birth * Native Speaker	-2.6572	-2.6572	-2.6947
		(8.9454)	(8.9475)	(12.3180)
	Year of Birth Trend	Yes	Yes	Yes
	Year of Birth Trend x Born 1990	Yes	Yes	Yes
	Juveniles	282702	282702	282702
	Observations	565026	565026	565026

Notes: Presented in each row are the discontinuity estimates of the given variable at the 1990 year of birth cut-off. Estimates in column (1) are obtained from OLS regression specifications, estimates in column (2) are obtained from local linear regression specifications where a uniform kernel is used and estimates in column (3) are obtained from local linear regression specifications where a triangular kernel is used. Standard errors are clustered at the date of birth level. All specifications include distance to the 1990 cut-off and its interaction term with the post-cut-off indicator (born in 1990-93 = 1). *** indicates significance at 1%, ** indicates significance at 5%, * indicates significance at 10%.

Table 4. Estimates of Impact of Earning or Learning (EL) Reform on Violent Incidents

	Impact of EL Reform on Violent Incidents								
	SDS, Aged 16-17			Crime, Aged 16-17			Crime, Aged 18-20		
	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Crime or SDS Count	0.0026**	0.0025**	0.0025**	-0.0001	-0.0003	-0.0002	-0.0009	-0.0010	-0.0008
	(0.0013)	(0.0012)	(0.0012)	(0.0013)	(0.0012)	(0.0012)	(0.0008)	(0.0008)	(0.0008)
Percent Effect	15.2047%	14.6199%	14.6199%	-0.9709%	-2.9126%	-1.9417%	-8.4906%	-9.4340%	-7.5472%
Mean Dep. Var. Born 1987-89	0.0171	0.0171	0.0171	0.0103	0.0103	0.0103	0.0106	0.0106	0.0106
Panel B. Crime or SDS Occurrence (0/1)	0.0021*	0.0021**	0.0021**	0.0001	0.0000	0.0001	-0.0009	-0.0010*	-0.0008
	(0.0011)	(0.0010)	(0.0010)	(0.0008)	(0.0008)	(0.0008)	(0.0006)	(0.0005)	(0.0005)
Percent Effect	13.2075%	13.2075%	13.2075%	1.25%	0%	1.25%	-10.9756%	-12.1951%	-9.7561%
Mean Dep. Var. Born 1987-89	0.0159	0.0159	0.0159	0.0080	0.0080	0.0080	0.0082	0.0082	0.0082
Year of Birth Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth Trend x Born 1990	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Day-Month of Birth Fixed Effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
School Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Number of Observations	565026	565026	565026	565026	565026	565026	565026	565026	565026
Number of Birthdates	2555	2555	2555	2555	2555	2555	2555	2555	2555
Enrolled in Grade 12	0.0886***	0.0886***	0.0889***	0.0886***	0.0886***	0.0889***	0.0886***	0.0886***	0.0889***
	(0.0059)	(0.0043)	(0.0043)	(0.0059)	(0.0043)	(0.0043)	(0.0059)	(0.0043)	(0.0043)

Notes: Table shows estimates of the causal effect of the Earning or Learning (EL) Reform on violent school discipline sanctions at age 16-17 (SDS, columns (1)-(3)), violent crime offences at age 16-17 (columns (4)-(6)) and violent crime offences at age 18-20 (columns (7)-(9)). Estimates are shown at the extensive and intensive margin in Panel A and at the extensive margin in Panel B. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are dummies for whether the youths are male and whether they are native English speakers. School fixed effects are measured at age 15. *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table 5. Estimates of Impact of Earning or Learning (EL) Reform on Property and Drug Incidents

	Impact of EL Reform on Property and Drug Incidents					
	Property Incidents			Drug Incidents		
	SDS, Aged 16-17	Crime, Aged 16-17	Crime, Aged 18-20	SDS, Aged 16-17	Crime, Aged 16-17	Crime, Aged 18-20
	ITT	ITT	ITT	ITT	ITT	ITT
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Crime or SDS Count	-0.0008	-0.0209**	-0.0110*	0.0001	-0.0033*	-0.0058***
	(0.0006)	(0.0101)	(0.0058)	(0.0003)	(0.0017)	(0.0017)
Percent Effect	-16%	-26.4557%	-21.1538%	5.2632%	-16.2562%	-18.1818%
Mean Dep. Var. Born 1987-89	0.0050	0.0790	0.0520	0.0019	0.0203	0.0319
Panel B. Crime or SDS Occurrence (0/1)	-0.0009	-0.0034**	-0.0017**	0.0000	-0.0027***	-0.0026***
	(0.0006)	(0.0015)	(0.0008)	(0.0003)	(0.0009)	(0.0008)
Percent Effect	-18.3673%	-11.6438%	-8.3744%	0%	-22.6891%	-14.7727%
Mean Dep. Var. Born 1987-89	0.0049	0.0292	0.0203	0.0019	0.0119	0.0176
Year of Birth Trend	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth Trend x Born 1990	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Day-Month of Birth Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	565026	565026	847732	565026	565026	565026
Number of Birthdates	2555	2555	2555	2555	2555	2555

Notes: Table shows estimates of the causal effect of the Earning or Learning (EL) Reform on property misconduct school discipline sanctions at age 16-17 (SDS, column (1)), property crime offences at age 16-17 (column (2)), property crime offences at age 18-20 (column (3)), illicit substance SDS at age 16-17 (column (4)), drug crime offences at age 16-17 (column (5)) and drug crime offences at age 18-20 (column (6)). Estimates are shown at the extensive and intensive margin in Panel A, and at the extensive margin in Panel B. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are dummies for whether the youths are male and whether they are native English speakers. School fixed effects are measured at age 15. Property crime offences include property damage and theft. *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table 6. Estimates of Impact of Earning or Learning (EL) Reform on Violent School Disciplinary Sanctions (SDS) by Criminal Background

	Impact of EL Reform on Violent SDS, Aged 16-17					
	ITT	ITT	ITT	ITT	ITT	ITT
	(1)	(2)	(3)	(4)	(5)	(6)
	No Own Violent Crime Records at Age 15			One or More Own Violent Crime Records at Age 15		
Panel A.	0.0019*	0.0018*	0.0018*	0.0404*	0.0464**	0.0455*
	(0.0011)	(0.0010)	(0.0010)	(0.0208)	(0.0223)	(0.0237)
Percent Effect	12.1019%	11.4650%	11.4650%	79.0607%	90.8023%	89.0411%
Mean Dep. Var. Born 1987-89	0.0157	0.0157	0.0157	0.0511	0.0511	0.0511
Number of Observations	560919	560919	560919	4107	4107	4107
Number of Birthdates	2555	2555	2555	1379	1379	1379
	No Classmates with Violent Crime Records at Age 15			One or More Classmates with Violent Crime Records at Age 15		
Panel B.	0.0007	0.0008	0.0008	0.0041**	0.0038**	0.0038**
	(0.0015)	(0.0014)	(0.0014)	(0.0016)	(0.0015)	(0.0015)
Percent Effect	4.1420%	4.7337%	4.7337%	28.2759%	26.2069%	26.2069%
Mean Dep. Var. Born 1987-89	0.0169	0.0169	0.0169	0.0145	0.0145	0.0145
Number of Observations	319476	319476	319476	245550	245550	245550
Number of Birthdates	2555	2555	2555	2555	2555	2555
Year of Birth Trend	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth Trend x Born 1990	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	No	Yes	Yes	No	Yes	Yes
Day-Month of Birth Fixed Effects	No	Yes	Yes	No	Yes	Yes
School Fixed Effects	No	No	Yes	No	No	Yes

Notes: Table shows estimates of the causal effect of the Earning or Learning (EL) Reform on violent school discipline sanctions at age 16-17 separately for juveniles without violent criminal records at age 15 (Panel A, columns (1)-(3)), with violent criminal records at age 15 (Panel A, columns (4)-(6)), without classmates with violent criminal records at age 15 (Panel B, columns (1)-(3)) and with classmates with violent criminal records at age 15 (Panel B, columns (4)-(6)). Estimates are shown at the extensive margin, they are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are dummies for whether the youths are male and whether they are native English speakers. School fixed effects are measured at age 15. *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table 7. Cost-Benefit Analysis of Earning or Learning (EL) Reform

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Victim Costs per Incident	Property Loss per Incident	Incarceration Costs per Incident	Total Costs per Incident	Estimated Change in Arrests, Cautions, Warrants, Apprehensions or SDS	Estimated Change in Incidents	Estimated Change in Incarcerations	Benefits = -(6)*(4)	Estimated Change in School Enrolment	Costs (9)*$3910	Benefit/Cost Ratio = (8)/(10)
Crime at Age 18-20											
Violent Crime	47,759	113	8,997	56,665	-269	-571	-136	32,355,715			
Property Crime	1,890	1,599	171	782	-3,695	-19,866	-1,857	15,535,212			
Drugs Crime	1,004	N/A	6,431	7,435	-1,950	-2,420	-914	17,992,700			
Crime at Age 16-17											
Violent Crime	33,965	139	5,005	38,858	-45	-96	-23	3,730,368			
Property Crime	1,897	1,608	168	778	-4,681	-25,167	-2,352	19,579,926			
Drugs Crime	1,004	N/A	6,431	7,435	-741	-919	-347	6,832,765			
Total Crime								96,026,686	19,915	77,867,650	**1.2332**
SDS at Age 16-17											
Violent SDS	2,000	15	N/A	1,988	560	595	N/A	-1,182,860			
Property SDS	370	270	N/A	154	-180	-484	N/A	74,536			
Drugs SDS	1,004	N/A	N/A	1,004	23	23	N/A	-23,092			
Total SDS								-1,131,416	19,915	77,867,650	**-0.0145**
Total Crime + SDS								94,895,270	19,915	77,867,650	**1.2187**

Notes: Costs of violent and property crimes are weighted averages of the breakdown costs from Lochner and Moretti (2004) using average share of crimes composing each of the categories as weights. Costs of drug crimes are based on the US Department of Justice (2011) victim costs and other crime costs, and incarceration costs are scaled in the same way as Lochner and Moretti (2004). Costs of violent school discipline sanctions (SDS) and property SDS are taken from Table 2 in Miller et al. (1996) as in Lochner and Moretti (2004), and violent SDS are given equal costs as assault with no injury while property SDS are given equal costs as larceny. Estimated change in arrests, cautions, warrants, apprehensions or SDS are calculated based on the results from Table 4, Panel A of Columns (3), (6) and (9), and Table 5, Panel A. Estimated crimes and incarcerations are calculated using 2009 clearances rates and conviction to incarceration rates respectively for each type of crime in the US. Clearances rates for each type of SDS are estimated to be twice as large as clearance rates for the same type of crime. Estimated change in enrolment is calculated estimating the specification in Figure 5 with enrolment in grade 12 modelled as dependent variable. Yearly costs per pupil (US $3,910) correspond to the average pupil costs (U.S. Department of Education, 2016) from 1974 to 2014. All figures are deflated to 1993 US dollars.

APPENDIX

Figure A.1. UK Media Coverage on the Potential for Compulsory Schooling to Increase Violence in School.


Exclusive: Keeping pupils in educ
telegraph.co.uk/news/2020/02/07/exclusive-keeping-pupils-education-age-18-iscontributing-rising/
App Impostazioni Outlook.com - Post... Nuova scheda

The Telegraph News Politics Sport Business Money Opinion Tech Life Style Travel

UK news World news Royals Health Defence Science Education Investigations

Exclusive: Keeping pupils in education until age 18 is contributing to rising crime, police chief says

Chief Constable calls for a review into the policy as he urges minister to consider link with explosion in violent crime among youngsters

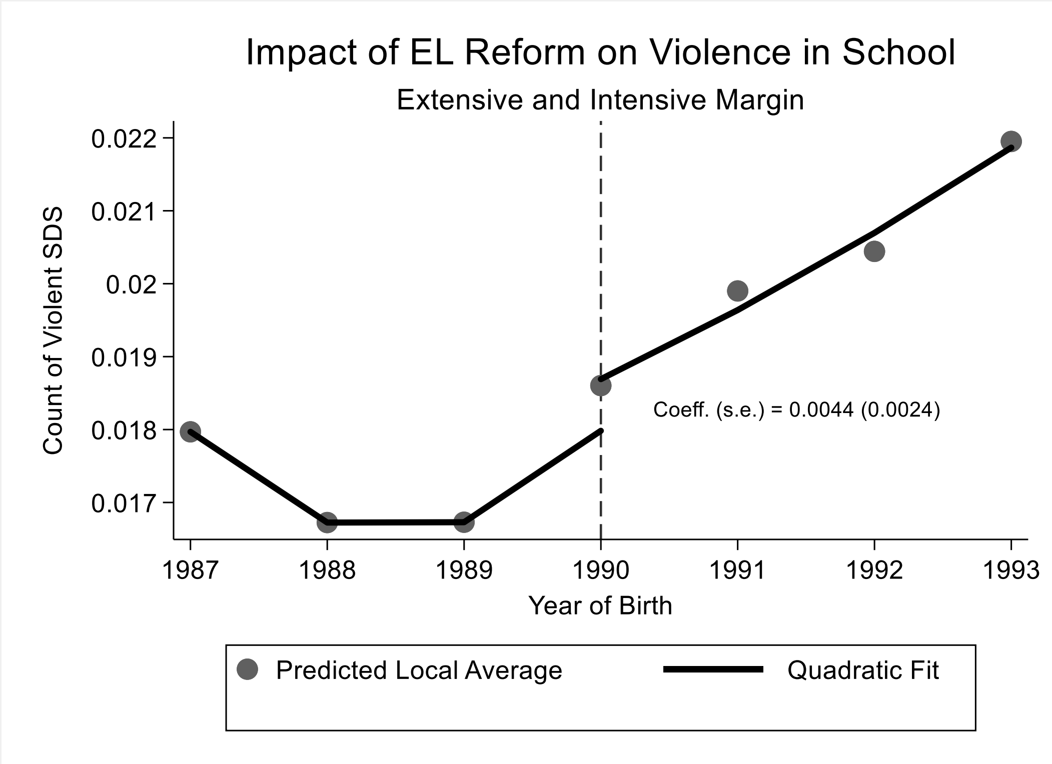
Premium

By Camilla Turner, EDUCATION EDITOR
7 February 2020 • 10:00pm

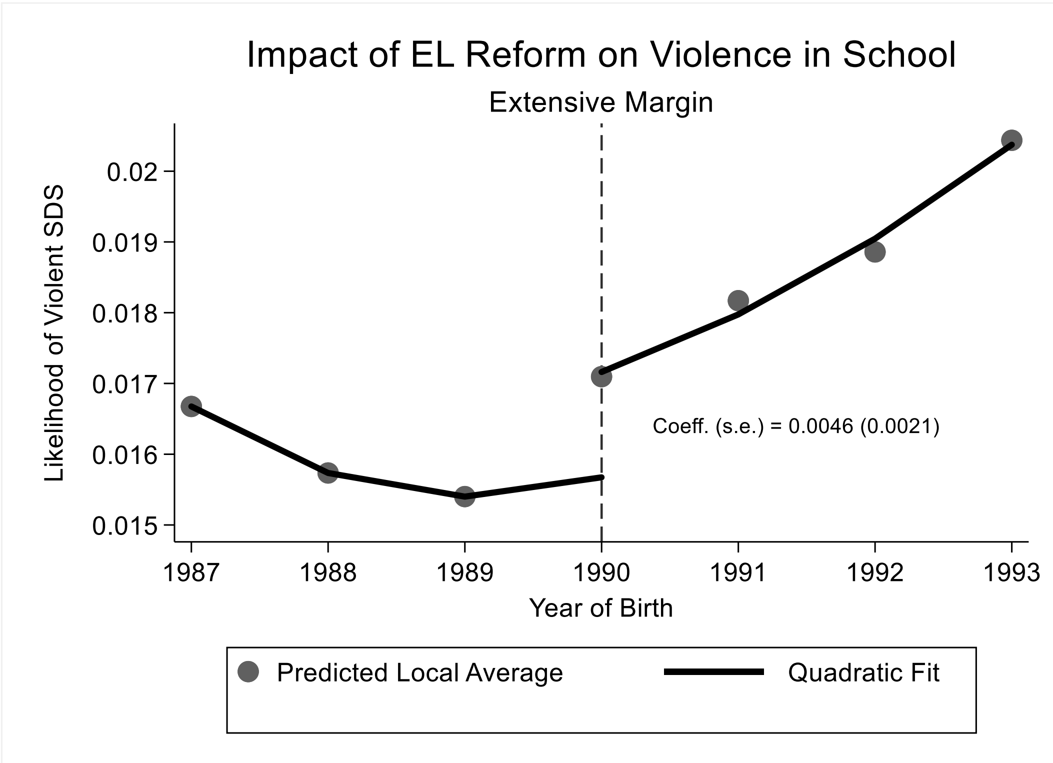
Related Topics
Education News

Figure A.2. Robustness Estimates of Impact of Earning or Learning (EL) Reform on Violence in School with Quadratic Trends by Year of Birth

Panel A

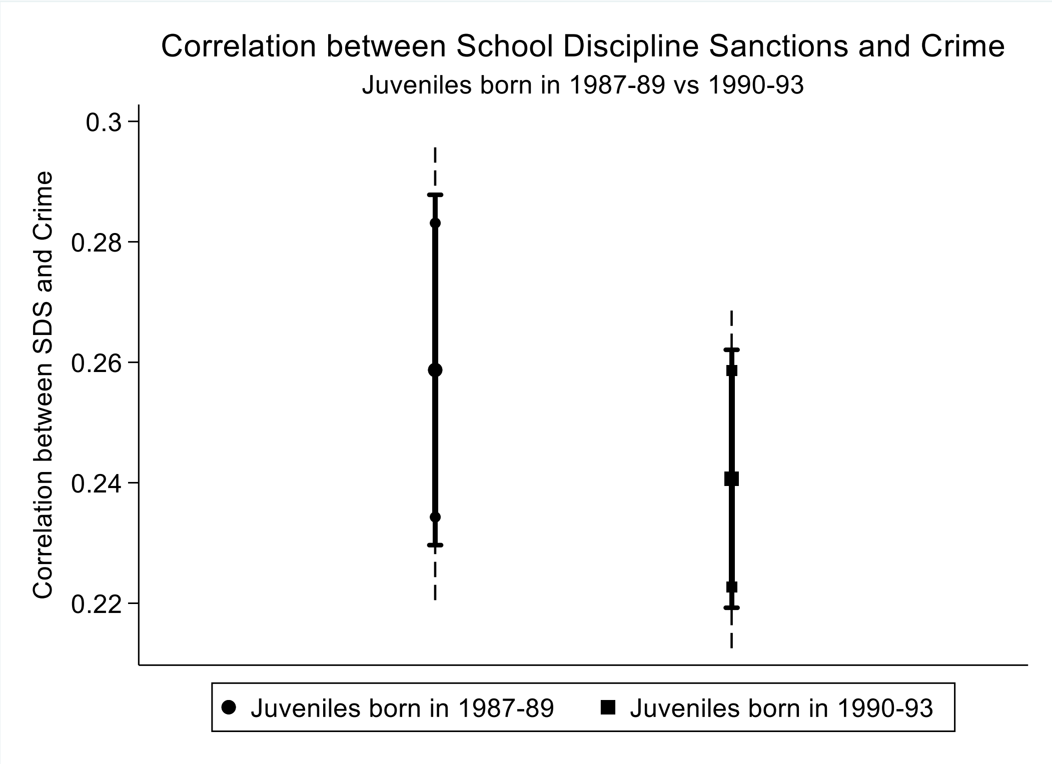


Panel B



Notes: Figure shows estimates of the causal effect of the Earning or Learning (EL) Reform on violent school discipline sanctions (SDS) at age 16-17. Estimates are shown at the extensive and intensive margin in Panel A, and at the extensive margin in Panel B. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are age fixed effects, year fixed effects, dummies for whether the youths are male and whether they are native English speakers, day-month of birth fixed effects and school fixed effects, together with heterogeneous quadratic trends by year of birth either side of the discontinuity window. School fixed effects are measured at age 15.

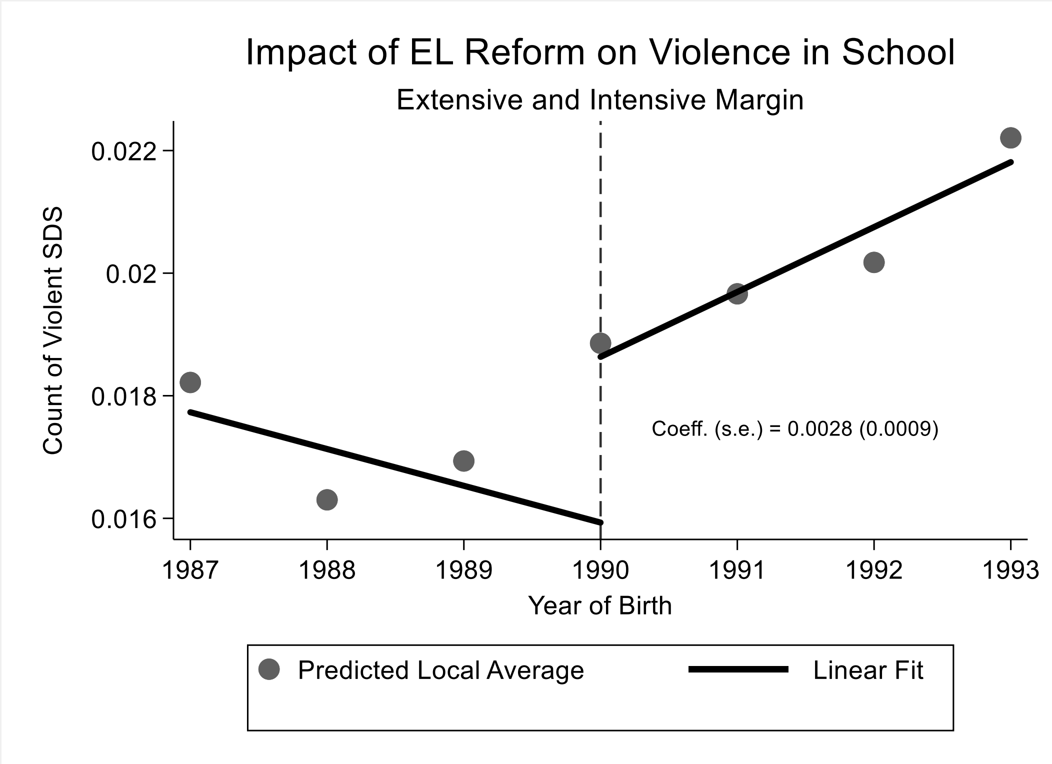
Figure A.3. Correlation between School Discipline Sanctions (SDS) and Crime for ITT Control (Born in 1987-89) and ITT Treatment (Born in 1990-93) Group Juveniles



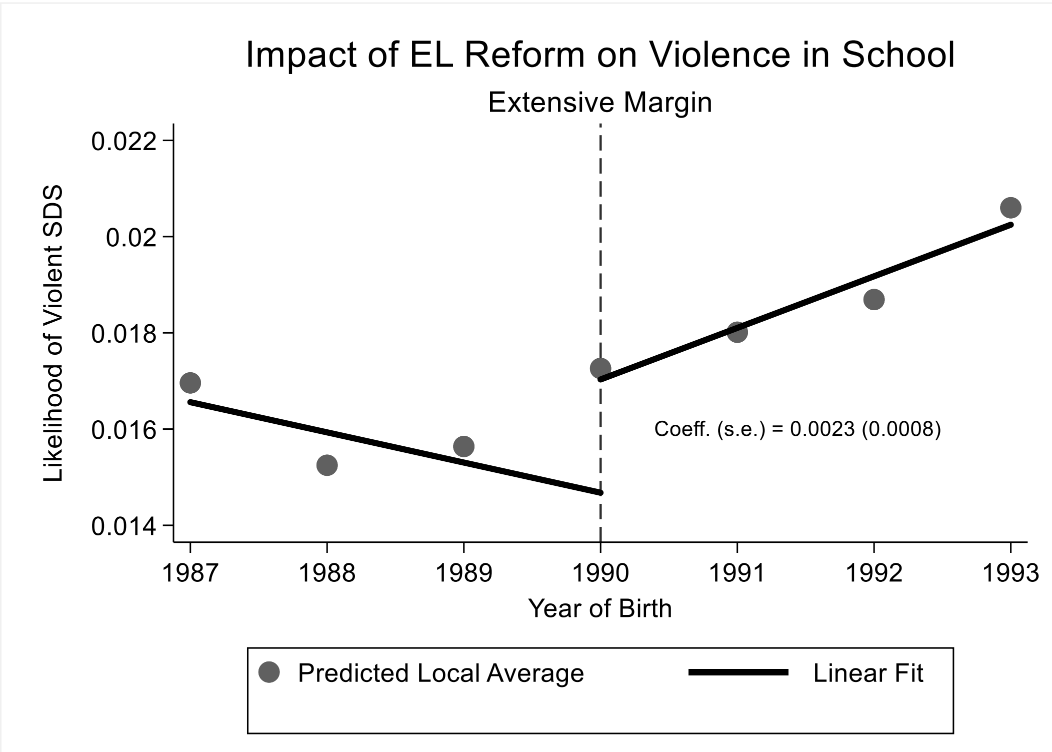
Notes: Figure shows the point estimates and 90%, 95% and 99% confidence intervals of the correlation between the number of crime offences per individual at age 16-20 and the number of School Discipline Sanctions (SDS) per individual at age 16-17 separately for individuals born from 1987-89 and from 1990-93. Crime offences include property damage and theft, drugs and violence by offender in a year. Violence includes violence against the person, sexual offences, and public order offences by offender in a year. SDS include property misconduct SDS, illicit substance SDS and violent SDS received per youth in a year.

Figure A.4. Robustness Estimates of Impact of Earning or Learning (EL) Reform on Violence in School

Panel A



Panel B



Notes: Figure shows estimates of the causal effect of the Earning or Learning (EL) Reform on violent school discipline sanctions (SDS) at age 16-17. Estimates are shown at the extensive and intensive margin in Panel A, and at the extensive margin in Panel B. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are age fixed effects, dummies for whether the youths are male and whether they are native English speakers, day-month of birth fixed effects and school fixed effects. School fixed effects are measured at age 15. Interactions between year fixed effects and dummies for whether the youths are male and whether they are native English speakers, day-month of birth fixed effects and school fixed effects were also included, together with heterogeneous linear trends by year of birth either side of the discontinuity window.

Table A.1. Robustness Estimates of Impact of Earning or Learning (EL) Reform on Violent School Disciplinary Sanctions (SDS)

	Impact of EL Reform on Violent SDS, Aged 16 to 17					
	Non-Parametric RD Estimates, Uniform Kernel			Non-Parametric RD Estimates, Triangular Kernel		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. SDS Count	0.0026***	0.0025***	0.0025***	0.0030**	0.0030***	0.0031***
	(0.0010)	(0.0008)	(0.0008)	(0.0013)	(0.0011)	(0.0011)
Percent Effect	15.2047%	14.6199%	14.6199%	17.5439%	17.5439%	18.1287%
Mean Dep. Var. Born 1987-89	0.0171	0.0171	0.0171	0.0171	0.0171	0.0171
Panel B. SDS Received (0/1)	0.0021**	0.0021***	0.0021***	0.0028**	0.0028***	0.0028***
	(0.0009)	(0.0007)	(0.0007)	(0.0012)	(0.0010)	(0.0010)
Percent Effect	13.2075%	13.2075%	13.2075%	17.6101%	17.6101%	17.6101%
Mean Dep. Var. Born 1987-89	0.0159	0.0159	0.0159	0.0159	0.0159	0.0159
Year of Birth Trend	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth Trend x Born 1990	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	No	Yes	Yes	No	Yes	Yes
Day-Month of Birth Fixed Effects	No	Yes	Yes	No	Yes	Yes
School Fixed Effects	No	No	Yes	No	No	Yes
Number of Observations	565026	565026	565026	565026	565026	565026
Number of Birthdates	2555	2555	2555	2555	2555	2555

Notes: Table shows estimates of the causal effect of the Earning or Learning (EL) Reform on violent school discipline sanction (SDS) at the extensive and intensive margin in Panel A, and at the extensive margin in Panel B. Estimates in columns (1)-(3) are obtained from Non-Parametric RD Estimation using a Uniform Kernel, and estimates in columns (4)-(6) from Non-Parametric RD Estimation using a Triangular Kernel. Standard errors were clustered at the date of birth level. Control variables included are dummies for whether the youths are male and whether they are native English speakers. School fixed effects are measured at age 15. *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table A.2. Robustness Estimates of Impact of Earning or Learning (EL) Reform on Violent School Disciplinary Sanctions (SDS) Without "Off Time" Students

	Impact of EL Reform on Violent SDS, Aged 16 to 17		
	(1)	(2)	(3)
Panel A. SDS Count	0.0044***	0.0043***	0.0043***
	(0.0013)	(0.0012)	(0.0012)
Percent Effect	33.5878%	32.8244%	32.8244%
Mean Dep. Var. Born 1987-89	0.0131	0.0131	0.0131
Panel B. SDS Received (0/1)	0.0036***	0.0036***	0.0035***
	(0.0011)	(0.0011)	(0.0011)
Percent Effect	28.8%	28.8%	28%
Mean Dep. Var. Born 1987-89	0.0125	0.0125	0.0125
Year of Birth Trend	Yes	Yes	Yes
Year of Birth Trend x Born 1990	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes
Control Variables	No	Yes	Yes
Day-Month of Birth Fixed Effects	No	Yes	Yes
School Fixed Effects	No	No	Yes
Number of Observations	398405	398405	398405
Number of Birthdates	2555	2555	2555
Enrolled in Grade 12	0.1324***	0.1326***	0.1302***
	(0.0047)	(0.0044)	(0.0043)

Notes: Table shows estimates of the causal effect of the Earning or Learning (EL) Reform on violent school discipline sanctions (SDS) at the extensive and intensive margin in Panel A, and at the extensive margin in Panel B without "off time" students. "Off time" students defined as students not attending grade 10 at age 15, i.e., the expected school grade at age 15. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are dummies for whether the youths are male and whether they are native English speakers. School fixed effects are measured at age 15. *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table A.3. Estimates of Impact of Earning or Learning (EL) Reform on Violent School Discipline Sanctions (SDS) in 1987-91 Birth Cohorts

	SDS, Aged 16-17		
	ITT	ITT	ITT
	(1)	(2)	(3)
Panel A. SDS Count	0.0026**	0.0026**	0.0025**
	(0.0013)	(0.0012)	(0.0011)
Percent Effect	15.2047%	15.2047%	14.6199%
Mean Dep. Var. Born 1987-89	0.0171	0.0171	0.0171
Panel B. SDS Occurrence (0/1)	0.0021*	0.0021**	0.0020**
	(0.0011)	(0.0010)	(0.0010)
Percent Effect	13.2075%	13.2075%	12.5786%
Mean Dep. Var. Born 1987-89	0.0159	0.0159	0.0159
Year of Birth Trend	Yes	Yes	Yes
Year of Birth Trend x Born 1990	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes
Control Variables	No	Yes	Yes
Day-Month of Birth Fixed Effects	No	Yes	Yes
School Fixed Effects	No	No	Yes
Number of Observations	391314	391314	391314
Number of Birthdates	1825	1825	1825
Enrolled in Grade 12	0.0886***	0.0887***	0.0887***
	(0.0059)	(0.0042)	(0.0042)

Notes: Table shows estimates of the causal effect of the Earning or Learning (EL) Reform on violent school discipline sanctions (SDS) at age 16-17 in 1987-91 birth cohorts. Estimates are shown at the extensive and intensive margin in Panel A and at the extensive margin in Panel B. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are dummies for whether the youths are male and whether they are native English speakers. School fixed effects are measured at age 15. *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table A.4. Placebo Estimates of Impact of Fake Earning or Learning (EL) Reform on Violent School Disciplinary Sanctions (SDS)

	Placebo Estimates of Impact of Fake EL Reform on Violent SDS, Aged 16 to 17					
	ITT	ITT	ITT	ITT	ITT	ITT
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. SDS Count	-0.0010	-0.0011	-0.0013	-0.0010	-0.0011	-0.0015
	(0.0015)	(0.0014)	(0.0014)	(0.0015)	(0.0013)	(0.0013)
Percent Effect	-5.7803%	-6.3584%	-7.5144%	-5.7803%	-6.3584%	-8.6705%
Mean Dep. Var. Born 1987-88	0.0173	0.0173	0.0173	0.0173	0.0173	0.0173
Panel B. SDS Received (0/1)	-0.0014	-0.0015	-0.0017	-0.0014	-0.0015	-0.0019
	(0.0014)	(0.0013)	(0.0013)	(0.0014)	(0.0011)	(0.0012)
Percent Effect	-8.6420%	-9.2593%	-10.4938%	-8.6420%	-9.2593%	-11.7284%
Mean Dep. Var. Born 1987-88	0.0162	0.0162	0.0162	0.0162	0.0162	0.0162
Year of Birth Trend	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth Trend x Born 1989	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	No	Yes	Yes	No	Yes	Yes
Day-Month of Birth Fixed Effects	No	Yes	Yes	No	Yes	Yes
School Fixed Effects	No	No	Yes	No	No	Yes
Number of Observations	565026	565026	565026	224036	224036	224036
Number of Birthdates	2555	2555	2555	1095	1095	1095

Notes: Table shows Placebo estimates of the fake causal effect of the Earning or Learning (EL) Reform shifted back to 1989, i.e., by one birth cohort, on violent school discipline sanctions (SDS) at the extensive and intensive margin in Panel A, and at the extensive margin in Panel B. Columns (1)-(3) show estimates for the entire sample, whereas columns (4)-(6) show estimates for the 1987-89 birth cohorts only. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are dummies for whether the youths are male and whether they are native English speakers. School fixed effects are measured at age 15. *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table A.5. Estimates of Impact of Earning or Learning (EL) Reform on Additional Categories of School Disciplinary Sanctions (SDS)

	Verbal Abuse SDS	Misconduct SDS	Disruption or Refusal to Participate SDS	Legal Substances SDS
	(1)	(2)	(3)	(4)
EL Reform	0.0025**	-0.0002	0.0004	-0.0003
	(0.0012)	(0.0008)	(0.0013)	(0.0008)
Percent Effect	14.5349%	-2.4096%	-2.0408%	-3.7975%
Mean Dep. Var. Born 1987-89	0.0172	0.0083	0.0196	0.0079
Year of Birth Trend	Yes	Yes	Yes	Yes
Year of Birth Trend x Born 1990	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes
Day-Month of Birth Fixed Effects	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes
Number of Observations	565026	565026	565026	565026
Number of Birthdates	2555	2555	2555	2555

Notes: Table shows estimates of the causal effect of the Earning or Learning (EL) Reform on other categories of school discipline sanctions (SDS) at the extensive and intensive margin not included in the main analysis. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are dummies for whether the youths are male and whether they are native English speakers. School fixed effects are measured at age 15. *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table A.6. Estimates of Impact of Earning or Learning (EL) Reform on Violent School Disciplinary Sanctions (SDS) of 15-Year-Old Students

	Impact of EL Reform on Violent SDS, 1988-93 birth cohorts Aged 15		
	(1)	(2)	(3)
Panel A. SDS Count	0.0054**	0.0054**	0.0055**
	(0.0027)	(0.0024)	(0.0024)
Percent Effect	13.5%	13.5%	13.75%
Mean Dep. Var. Born 1988-89	0.0400	0.0400	0.0400
Panel B. SDS Received (0/1)	0.0033	0.0033	0.0034
	(0.0023)	(0.0021)	(0.0021)
Percent Effect	9.1413%	9.1413%	9.4183%
Mean Dep. Var. Born 1988-89	0.0361	0.0361	0.0361
Year of Birth Trend	Yes	Yes	Yes
Year of Birth Trend x Born 1990	Yes	Yes	Yes
Year Fixed Effects	No	No	No
Age Fixed Effects	No	No	No
Control Variables	No	Yes	Yes
Day-Month of Birth Fixed Effects	No	Yes	Yes
School Fixed Effects	No	No	Yes
Number of Observations	249289	249289	249289
Number of Birthdates	2190	2190	2190

Notes: Table shows estimates of the causal effect of the Earning or Learning (EL) Reform on violent school discipline sanctions (SDS) at the extensive and intensive margin in Panel A, and at the extensive margin in Panel B. The analysis is conducted on students aged 15. Estimates are obtained from OLS regression specifications and standard errors were clustered at the date of birth level. Control variables included are dummies for whether the youths are male and whether they are native English speakers. School fixed effects are measured at age 15. *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.