School Indiscipline and Crime

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Abstract

This paper establishes a novel fact about educational production: the timing of school eligibility affects behaviour in school, with disciplinary sanctions predicting but not affecting future criminality. Using administrative data matching education and criminal records from Queensland, this paper establishes that early school eligible juveniles who receive more school disciplinary sanctions during compulsory schooling also commit more crime once they can leave school. Analysis of a school-leaving-age reform also demonstrates that schools internalise misbehaviour during compulsory schooling. Thus, studies that failed to consider in-school behaviour may overestimate the short-run crime-reducing impact of education. This is particularly true of violent crime.

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1. Introduction

Juvenile crime is a significant policy issue. As crime rates increase steadily until around age 18–20 and then decline later in life (Quetelet, 1831; and Landersø, Nielsen and Simonsen, 2016), the educational experience may play a critical role in determining participation in criminal activity over the lifetime of an individual.[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; Bell, Costa and Machin, 2018), England and Wales (Machin, Marie and Vujic, 2011), Sweden (Hjalmarsson, Holmlund and Lindquist, 2015) and, more recently, Australia (Beatton, Kidd, Machin and Sarkar, 2017). This literature employs a variety of sources of exogenous variation in school attendance including idiosyncratic school closure dates for teacher training (Jacob and Lefgren, 2003) or teacher strikes (Luallen, 2006), as well as changes in the minimum school leaving age (Lochner and Moretti, 2004; and Machin et al., 2011). The consensus is a beneficial crime-reducing effect of school attendance.

Recent literature suggests that starting school at a younger age may place juveniles at a greater risk of committing crime (Cook and Kang, 2016; Depew and Eren, 2016; and Landersø et al., 2016). This is consistent with the finding that starting school at a younger age leads to a significant academic disadvantage from childhood (Bedard and Dhuey, 2006; Datar, 2006; Puhani and Weber, 2007; McEwan and Shapiro, 2008; Elder and Lubotsky, 2009; Crawford, Dearden, and Meghir, 2010; Black, Devereux, and Salvanes, 2011; and McCrary and Royer, 2011). Cunha and Heckman (2008) suggest that starting school at an earlier age may have significant lifetime effects, as it has the potential to alter the entire path of skill acquisition.

What is currently much less well understood is how early school eligibility and the school-starting age affect the educational experience of juveniles and, in turn, how the initial disadvantage

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

in school performance leads to greater future criminal participation. In the United States (US), 1.4 million school 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 (US Department of Education).[2] In England, more than 30,000 school crime offences were recorded in 2015,[3] and the education system was deemed potentially responsible for the nationwide spate of fatal juvenile stabbings in 2019.[4] In Australia, from July 2015 to June 2016 the police proceeded against 55,000 young offenders (Australian Bureau of Statistics, 2017).

While the main rationale behind compulsory schooling is to lift the human capital and labour market prospects of potential early dropouts, the primary experience of compulsory education for low-performing individuals is one of being forced to attend school surrounded by better-performing peers.[5] If juveniles are at school against their will, the compulsory schooling period may turn out to be actively formative for their crime capital accumulation. On the other hand, the adoption of behavioural rules and the use of discipline sanctions in school may contribute to the education production function (Lazear, 2001) and reduce the criminal participation of juveniles. Thus, generating an understanding of the educational experience of juveniles and how it affects their future criminal behaviour is a first order research question in the economics of crime.

This paper studies the impact of early school eligibility on the educational experience and criminal participation of juveniles in the institutional context of the state of Queensland in Australia. Focusing on children born around the 1st January, it presents a regression discontinuity design (RDD) analysis of the impact of early school eligibility on crime and school suspensions. Administrative rules governing school entry eligibility state that a child is expected to enrol in the school year in which the child turns six. Thus, children born around the 1st January are subject to a discontinuity in school entry eligibility. Using matched administrative data on education and crime for the state of

[2] See https://nces.ed.gov/fastfacts/display.asp?id=49.
[3] See https://www.bbc.co.uk/news/education-34268942.
[4] See https://www.bbc.co.uk/news/education-47537631.
[5] A growing literature has started to document the negative effects of exposure to higher-performing peers in school (Murphy and Weinhardt, 2018).

Queensland, the analysis compares the propensity to commit crime and receive school suspensions of juveniles born 60 days either side of the 1st January cut-off.[6] Availability of information on the actual secondary school starting age also enables us to define a dichotomous variable that indicates whether a juvenile is young in cohort (YINC) in secondary school. In the econometric analysis, this variable is then instrumented with an indicator of whether the juvenile is early school eligible (ESE) (i.e., whether s/he was born within 60 days before the 1st January cut-off), thus accounting for the potential parental over-riding of administrative rules.

The empirical findings show that ESE juveniles aged 15 to 24 are more likely to commit criminal offences as well as to receive School Disciplinary Absences (SDAs), i.e. school suspensions.[7] This conclusion is robust to equation specification and bandwidth selection, as results appear consistent when either 90-day, 60-day or 30-day bandwidths either side of the 1st January cut-off are adopted. The estimated effects also appear robust to potential parental over-riding of administrative rules, as the IV estimates lead to the conclusion that YINC juveniles are more likely to commit crime and be suspended from school. Finally, results are also robust across parametric and non-parametric local linear regression estimation.

When the impact of ESE status on crime and school suspensions is estimated separately by age, the analysis reveals that ESE juveniles face a greater risk of committing criminal offences from age 18 up until age 24. As the minimum school leaving age in Queensland is 17, juveniles aged 18 or older are no longer subject to compulsory schooling laws.[8] Prior to age 18, when juveniles in Queensland must attend school, ESE and control group juveniles are equally likely to commit crime. However, while no discrepancy appears at age 15, ESE juveniles appear more likely to receive school suspensions at age 16 and 17. This crime-age profile is consistent with an incapacitation effect of school on crime. It suggests that schools internalise the bad behaviour of ESE juveniles prior to them

[6] Robustness of the results to alternative bandwidth choices is tested and presented below.
[7] SDAs in Australia are equivalent to suspensions in the US and temporary exclusions in the UK.
[8] The minimum school leaving age was raised from 16 to 17 in 2006 in Queensland. Further details of this reform are provided in Section 3.

leaving school and gravitating towards crime. The same conclusion emerges for both the reduced-form OLS effect of ESE status and the structural IV effect of YINC status on crime and school suspensions. While arrested and incarcerated juveniles are significantly less likely to graduate from high school (Hjalmarsson, 2008) and significantly more likely to become recidivists either in youth or in adulthood (Mendel, 2011; Aizer and Doyle, 2015; and Stevenson, 2017), schools seem to internalise the bad behaviour of ESE juveniles and delay the onset of crime.

The notion that schools internalise the bad behaviour of juveniles prior to them leaving school and gravitating towards crime is a novel and important finding, as it suggests that behavioural records in school may be used as valuable flags to predict and prevent future criminal participation. The remainder of the paper empirically tests whether this interpretation of the crime-age profile is warranted. First, individual-specific estimates are derived to test whether the same ESE individuals who receive suspensions during secondary school also commit crime after leaving school. Average percentile youth-specific effects of ESE status on crime are plotted against percentile youth-specific effects of ESE status on school suspensions. This analysis shows that there is a close correspondence between the two groups, with school suspensions being strongly predictive of future criminal participation.

Second, a recent school-leaving age reform is exploited to test empirically whether compulsory schooling causes the transition from SDAs to crime at age 18, and thus affects the observed crime-age profile. Interest lies in quantifying the extent to which ESE juveniles receive more school suspensions at age 16 and 17 and start to commit more crime at age 18 because of compulsory school attendance. Evidence of the causal impact of compulsory school attendance on crime and school suspensions in the short-run would suggest that policies governing school-leaving behaviour may be used to affect this crime-age profile.

To quantify the causal effect of school attendance on crime and school suspensions, the introduction of the Earning or Learning (EL) education reform in Queensland in 2006 is exploited. This reform increased the minimum school leaving age from 16 to 17, and the effects of compulsory

schooling on crime and school suspensions by age 17 are therefore studied.[9] Juveniles forced to stay on at school for one more year committed less crime and received more school suspensions overall. This suggests that the crime-age profile derived in this study is affected by the compulsory schooling legislation. Juveniles forced to stay in school for longer committed less property crime and less drugs crime offences than the control group juveniles who reached the minimum school leaving age prior to 2006. However, they received more school suspensions for property and physical misconduct in school by age 17.[10] A simulation exercise in which similar criminal offences and school suspensions are added and modelled together suggests that forcing juveniles to remain in school has a net crime-reducing effect for both property and drugs offences, i.e., even after accounting for the increase in school suspensions for property misconduct. Once behaviour in school is considered, the property crime-reducing effect of school is smaller than police records alone would suggest. Thus, while these results show that the crime-age profile of juveniles is affected by compulsory school attendance, they also suggest that omission of in-school discipline records in existing studies may have led to an over-estimate of the magnitude of the crime-reducing effect of school in the short run.[11]

Third, the link between school suspensions and future juvenile criminality is quantified. Here the panel dimension of the data and the within-individual variation in categories of crime and school suspensions are exploited to test whether school suspensions at age 15 to 17 of a particular category predict crime at age 18 to 24 of the same category after controlling for individual fixed effects. Since information is available for each individual on three categories of crime and school indiscipline (i.e., property crime and property misconduct in school; drugs crime and illicit substances misconduct in

[9] This analysis also relates to the notion that compulsory school attendance may shift juvenile crime from outside to inside the school premises. School leaving age (SLA) reforms typically mandate school attendance at a critical age when the likelihood of committing crime is highest (Quetelet, 1831; Landersø et al., 2016), and impact the subpopulation of compliers with the lowest ability or desire to stay on at school. Insofar as potential criminals plausibly originate from the bottom of the skill distribution (Machin and Meghir, 2004), the concern emerges that the crime-reducing effect of school documented to date may be over-estimated due to under-reporting of in-school crime.

[10] The notion that compulsory schooling may generate an increase in violence among juveniles has been documented in Jacob and Lefgren (2003).

[11] Gilpin and Pennig (2015) constitutes an early attempt to study the potential displacement of crime induced by school leaving age reforms. However, these authors use school-level survey data and, therefore, are unable to investigate and indeed exploit any within-school variation at the individual level. Their event-study estimates do not retrieve any effect of compulsory school attendance on in-school crime.

school; violent crime and violent misconduct in school), a dataset with three observations for each individual was constructed, one for each category of crime and school indiscipline. Individual fixed effects were also added to the regression analysis, and the link between school suspensions and future crime of a given category estimated conditional on individual fixed effects. The results show that, when individual fixed effects are omitted from the analysis, school suspensions at age 15 to 17 and crime at age 18 to 24 of the same category are positively correlated. This indicates, for example, that the likelihood of receiving a school suspension for violent misconduct at age 15 to 17 and the likelihood of committing violent crime at age 18 to 24 are positively correlated. However, when individual fixed effects are included in the analysis, school suspensions at age 15 to 17 no longer predict future crime of the same category. When the unobservable inherent propensity of an individual to misbehave is controlled for via inclusion of individual fixed effects, school disciplinary sanctions do not appear to affect the future likelihood of committing crime.[12]

The main takeaway from this paper is that ESE juveniles receive more disciplinary sanctions while they are forced to attend school, and they start to commit more crime once they are allowed to leave school. School disciplinary sanctions do not affect future criminality but they strongly predict it, thus providing a valuable flag to identify individuals at risk. Cook and Kang's (2016) and Landersø et al.'s (2016) studies are closest to ours as they analyse the impact of school starting age on crime and present crime-age profiles for the US and Denmark respectively. Our analysis adds to these studies in three ways. First, our analysis demonstrates that ESE juveniles exhibit behavioural issues in school prior to starting to commit more crime. Second, the introduction of a policy change influencing entire cohorts of juveniles is exploited to quantify the impact of compulsory schooling on the crime age profile. Third, our analysis demonstrates that school disciplinary sanctions do not affect future criminality but strongly predict it, thus providing an informative metric to detect

[12] Bayer, Hjalmarsson and Pozen (2009) show that incarcerated juveniles who are exposed to peers with experience in a specific type of crime are more likely to commit the same type of crime after release from prison, showing that specific crime skills can follow their own trajectories that need not co-evolve with those of other crime skills.

juveniles at risk prior to engaging in criminal activity. The remainder of the paper is structured as follows. Section 2 describes the institutional setting of this study and presents the RDD analysis of the impact of ESE status on crime and suspensions in secondary school. Section 3 presents three sets of statistical tests to develop an understanding of the crime age profile and derive useful policy prescriptions. Section 4 concludes.

2. Early School Eligibility, School Indiscipline and Crime

The institutional setting of this study is the state of Queensland in Australia. In the last two decades, offender rates in Queensland have closely resembled the Australian national average, thus making the state representative of Australia as a whole (Australian Bureau of Statistics).[13] In Queensland, approximately three quarters of students attend the state-run school sector, which is funded by the Queensland State and Federal Australian Governments (QGOV, 2018a). The remainder attend private schools. Children attend up to 12 years of education (grades 1 to 12), with primary school consisting of grades 1 to 7 and high school of grades 8 to 12. At the end of secondary school, students are expected to sit high-stakes exams to obtain an Overall Position (OP) certificate.

The school year in Queensland runs from the third week of January to mid-December. Administrative rules imply that children are expected to start grade one in the calendar year in which they turn six years old (QGOV, 2018b). This is crucial in the ensuing empirical analysis as it implies that children born one day apart, i.e. December 31st versus January 1st, are expected to start school one year apart. This institutional feature of the Queensland education system is exploited here to define our treatment and control groups and compare their respective behavioural outcomes.

Data

Queensland State Government administrative data matched at the individual level from two agencies, the Queensland Education Department (QED) and the Queensland Police Service (QPS),

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

are used for the empirical analysis. This dataset includes individual record data for the population of Queensland Government funded school attendees, together with matched individual criminal offence data on juveniles and young adults for the period 2003 to 2014.

Queensland Police Service (QPS) records refer to alleged criminal offences in a given year by individuals aged 15 to 24. An alleged offender is a person who has allegedly committed a crime and has been charged by the police by either arrest, caution or warrant of apprehension. An array of crime measures is considered in the forgoing empirical analysis. The main outcome of interest is a general measure of criminal offences, which include property, drugs, violence and other offences by offender in a given year. Property offences include criminal damage and theft and handling of stolen goods. Violent offences include violence against the person, sexual offences and robbery. Other offences include burglary, dangerous or negligent acts endangering persons, blackmail, fraud, deception and related offences, weapons offences, public order offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations, and miscellaneous offences.

The crime data were matched at the individual level to the Queensland schooling data from 2003 to 2014. A major advantage of the data is that education and crime records could be matched at the individual level. School disciplinary absences (SDAs), i.e., school suspension records from the QED, are used to measure behaviour in school. SDAs are a set of actions that are available to schools in Queensland to restore discipline. Queensland state school principals may use SDAs in response to serious breaches of school rules and unacceptable school behaviour. Reasons for SDAs include property misconduct, such as destruction or theft of school property or the property of others on the school premises; substance misconduct involving illegal substances, such as drugs; and physical misconduct, including violence against other students or against school personnel. Other less serious types of misconduct that may result in the receipt of an SDA include truancy, persistent disruptive behaviour, verbal and non-verbal misconduct, refusal to participate and misconduct involving legal substances, i.e., cigarettes or alcohol (QGOV, 2018c). 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.

Empirical Strategy

In Queensland, the formal age at school start is defined by the timing of birth. Children born either side of the 1st January cut-off are subject to a one-year difference in timing of administratively determined school start. There is some parental scope to deviate from the administrative rules governing school starting age of a child. However in the main, at a given 1st January cut-off, children born in the final months of the prior calendar year (i.e., November-December) will likely start school one year earlier than their counterparts born in January-February of the following year. Juveniles born in November-December are referred to as early school eligible (ESE), and their behavioural outcomes are compared with those of juveniles born in January-February.

In practice, only youth born +/- 60 days either side of the 1st January cut-off who are observed at the age of 15 in state-maintained schools in Queensland are included in the analysis. ESE is modeled as a binary variable indicating whether youth i was born in November-December (i.e., $\mathrm{ESE} = 1$) or in January-February (i.e., $\mathrm{ESE} = 0$). Alternative bandwidths to test robustness of the results involve birth-dates up to 90 and 30 days either side of the cut-off respectively. In contrast, juveniles born in the remainder of the calendar year are excluded from this RDD analysis and only brought into the later analyses in Section 3. Formally, the OLS equation of interest can be expressed as follows:

$$Y_{it} = \alpha + \beta_1 * ESE_i + X_i\delta + \gamma + \theta_t + u_i, \quad (1)$$

where Y is the behavioural outcome of juvenile i at age t, ESE as defined above, γ is an age fixed effect, θ is a set of discontinuity fixed effects (i.e., indicator variables for being born in November-December 1987 - January-February 1988, November-December 1988 - January-February 1989, November-December 1989 - January-February 1990, etc.) and u is the error term. X is a vector of

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

observable characteristics which includes individual and primary school characteristics of juvenile *i*, a control for distance in birthday to the 1st January cut-off and its interaction term with ESE_i. Since some autonomy is granted to parents and administrators in the decision to hold children back one year and postpone school entry, the actual relative age in cohort in secondary school of juvenile *i* is likely to be correlated with the error term in (1). Since the actual age of juveniles in secondary school is observed in the data, the structural IV analogue to (1) is also estimated where the actual young in cohort (YINC) status at the beginning of secondary school is instrumented using ESE status. In this case, ESE is used as an instrument for the endogenous YINC status that takes value 1 for juveniles who started secondary school almost one year prior to their 13th birthday (i.e., $\text{YINC} = 1$), and value 0 for those who started in close proximity to their 13th birthday (i.e., $\text{YINC} = 0$).

Table 1 shows the means of our selected behavioural outcomes for the full sample of juveniles and the subsamples of juveniles born within 90, 60 and 30 days respectively either side of the 1st January cut-off. The proportion of youth charged with a criminal offence or in receipt of a school suspension are shown in columns (1) and (2) respectively, while the average count of criminal offences and school suspensions are shown in columns (3) and (4). The descriptive statistics from the full sample and from the discontinuity samples look very similar, suggesting that the latter are a representative subsample of the former. Approximately 3 percent of juveniles educated in state-maintained schools in Queensland committed at least one criminal offence from age 15 to 24, while almost 7 percent of them received at least one suspension in secondary school. Approximately 8 criminal offences from age 15 to 24 and 10 suspensions in secondary school were recorded for every 100 juveniles in the data.

Tables A.1 and A.2 in the Appendix also show the means of our selected behavioural outcomes by age and type of crime and SDA for the sample of 203,355 youths born within 60 days either side of the 1st January cut-off. Both our measures of crime and SDAs are memoryless outcomes that inform the tendency to engage in crime or receive school suspensions at any given age. Thus, for each crime category, Table A.1 shows both the proportion of youth charged with a criminal offence

in columns (1) to (5) and the average count of criminal offences in columns (6) to (10) by crime type at a given age. Table A.2 follows the same structure for school suspensions. Table A.1 shows that, for youth in state-maintained schools in Queensland, the likelihood of being charged with a criminal offence displays a quadratic form, increasing up until age 18, falling thereafter and almost halving by age 24. The average count of criminal offences per individual falls monotonically from the late teens onwards, with the pattern appearing most pronounced for property crime.[15] Table A.2 suggests that both the incidence and the average count of school suspensions are highest at age 15 and then decrease with age.

Results

The starting point in the econometric analysis is to show that juveniles born in January-February provide a suitable control group for ESE juveniles born in November-December of the previous year. Table 2 shows a set of balancing tests of the distribution of individual and primary school characteristics across the 1st January cut-off (i.e., the ESE cut-off). All the primary school characteristics considered here were measured in grade 7, i.e., the last year of primary school.[16] Each individual variable was regressed on the ESE indicator, a control for distance of birthday to the 1st January cut-off, their interaction term and discontinuity-specific fixed effects.

Results from OLS and local linear regression specifications are shown in columns (1) and (2) respectively. Both estimation methods suggest males and native speakers to be uniformly distributed across the 1st January discontinuity. The same conclusion applies to the primary school characteristics considered here, i.e., the ICSEA Index and the School Deprivation Index, which proxy for the primary schools' financial resources, the fraction of males and native speakers in class, and the number of classmates in primary school. This suggests that juveniles from the treatment and control groups come from similar socio-economic backgrounds on average. The ESE indicator does not predict the primary

[15] Similar patterns have been found in France (Quetelet, 1831) and Denmark (Landersø et al., 2016).
[16] When information from primary school was not available, binary variables were created to indicate juveniles for which the information was not available, and missing values were then replaced with zeros.

school fixed effects either, suggesting that ESE pupils and control pupils do not systematically sort into different schools. Finally, the ESE dummy does not predict whether any of the considered individual and primary school characteristics are missing in the data.

Figure 1 shows a density test of the distribution of juveniles across the ESE cut-off. If parents planned fertility in order to have children just after the cut-off, a disproportionate number of individuals born just after the 1st January would be observed. This would invalidate our experiment. However, our sample exhibits similar birth densities either side of the 1st January cut-off. The null hypothesis of equality in the densities either side of the 1st January cut-off could not be rejected at the 10 percent level, based on the main local linear regression specification with a 60-day bandwidth, a triangular kernel and for both conventional and bias-robust confidence intervals.

Tables 3 and 4 show estimates of the causal effect of ESE status on crime and school suspensions respectively. Results are presented for the extensive margin of crime and school suspensions in columns (1) to (3), and for the combined extensive and intensive margin of crime and school suspensions respectively in columns (4) to (6). In the former case, the dependent variable is a binary indicator (i.e., 0/1) reflecting whether juvenile *i* committed a criminal offence or received an SDA, while in the latter case the dependent variable is the count of criminal offences or SDAs of juvenile *i*. All estimates are obtained from OLS regression specification (1), which include individual-level characteristics, a control for distance of birthday from the 1st January cut-off, its interaction term with ESE_i, age fixed effects and discontinuity fixed effects. Specifications in columns (2) and (5) additionally control for the primary school characteristics listed in Table 2, and columns (3) and (6) report results when primary school-discontinuity fixed effects are also added to the estimating equation.[17] In Tables 3 and 4, results are reported for our general measures of crime offences and school suspensions, which include all crime offences and SDAs by juvenile *i*. For all outcomes,

[17] A similar set of fixed effects has been used to identify peer effects in school in, e.g., Black, Devereux and Salvanes (2013), Anelli and Peri (2017) and Carrell, Hoekstra and Kuka (2018).

results are shown using our preferred 60-day bandwidth, as well as 90- and 30-day bandwidths either side of the discontinuity. Robust standard errors are clustered at the birthday level in all cases.

Tables 3 and 4 clearly show that ESE juveniles are at greater risk of committing crime by age 24 and being suspended during secondary school. This holds true whether the extensive margin or both the extensive and intensive margin of crime and school suspensions are modelled, suggesting that these effects are not driven by a few unruly juveniles. The comparison of these point estimates with the overall delinquency prevalence shown in Table 1 reveals that ESE juveniles are roughly 10 to 19 percent more likely to engage in crime at the extensive margin and commit 22 to 27 percent more criminal offences by the age of 24. ESE juveniles also seem roughly 10 to 16 percent more likely to receive SDAs in school at the extensive margin and receive 16 to 26 percent more SDAs by the age of 17. Reflecting the fact that sociodemographic characteristics are independent of the date of birth near the 1st January cut-off, the point estimates vary only marginally across specifications and bandwidth selections, and the conclusion that ESE juveniles face a greater risk of committing crime and receiving school suspensions appears robust.

Results are also obtained using an instrumental variable approach to account for the practice of "academic redshirting", whereby parents or educators may delay enrolling a child into mainstream schooling. Figure A.1 and Table A.3 in the Appendix show that the ESE indicator is a strong predictor of whether a juvenile is young in cohort (YINC) at the beginning of secondary school. The magnitude of the discontinuous jump on the 1st January in the likelihood of being YINC in secondary school in Figure A.1 is very similar to the estimated coefficients in Table A.3. The robust standard errors reported in Table A.3, clustered at the birthday level in all specifications, confirm that ESE status is a strong predictor of YINC status. Although a considerable fraction of ESE juveniles are held back

one year, ESE juveniles are much more likely to be YINC in secondary school than control group juveniles.[18]

Tables A.4 and A.5 in the Appendix show the IV results for criminal offences and SDAs respectively and follow the same structure as Tables 3 and 4, with the same set of control variables progressively added to the specification. Results are shown for the extensive margin of crime and school suspensions, as well as the combined extensive and intensive margins. Regardless of the equation specification and bandwidth selection, YINC juveniles are much more likely to commit crime by age 24 and receive suspensions during secondary school. This is consistent with the results in Tables 3 and 4, once discretion to defer school enrolment is considered. YINC juveniles are at least 40 percent more likely to engage in crime at the extensive margin, and they commit 64 percent more criminal offences by age 24. YINC juveniles have more than a 40 percent greater chance of receiving SDAs in school at the extensive margin, and they receive roughly 68 percent more SDAs by age 17. These results are large and they appear comparable in magnitude with Cook and Kang (2016), who report a 0.53 discontinuity in the grade level at age 15 and roughly a 0.33 discontinuity in the probability of a delinquent complaint for ESE juveniles by age 15 in North Carolina (thus, the implicit Wald estimator in their case would retrieve a statistically significant 0.33/0.53 = 0.62 effect).

Results are also estimated using a local linear regression with a triangular kernel. Table A.6 in the Appendix shows that the conclusion that ESE and YINC juveniles face a greater risk of committing crime and receiving SDAs is robust to estimation method, as results are consistent when using either parametric or non-parametric estimation methods. Table A.7 in the Appendix also shows robustness of this conclusion to sample attrition. In fact, Table A.1 in the Appendix shows that not all juveniles are observed from age 15 to 24: while some individuals may have genuinely migrated out of Queensland or died, in most cases individuals were simply too young to be observed until age

[18] Since no youths in the data start secondary school more than one year before or after the date at which they are supposed to start, the ESE instrument monotonically increases the likelihood of being YINC in secondary school. Thus, the monotonicity assumption is satisfied.

24. Both the ESE and YINC impact on crime and SDAs were re-estimated using the sample of 57,330 juveniles that could be observed continuously from age 15 up until age 24. A greater likelihood to commit crime by age 24 still appears here, suggesting that our results are not driven by the potential non-random attrition of ESE juveniles.

Discipline Sanctions and the Crime Age Profile

ESE juveniles are at greater risk of committing crime by age 24 and at greater risk of receiving SDAs up until age 17. One way to derive insights on the educational experience of ESE juveniles is to recalculate results separately by age. Figures 2 and 3 break down crime offences and SDAs by age, with Figure 2 showing the results for the extensive margin of crime and SDAs and Figure 3 showing the results for the combined extensive and intensive margins of crime and SDAs respectively. Equation (1) was estimated separately at each age and for each outcome of interest, and again all specifications include distance from the 1st January cut-off, a pre-cut-off indicator (Nov-Dec = 1), their interaction term, discontinuity fixed effects and controls for individual characteristics. Robust standard errors are clustered again at the date of birth in all cases.

At age 15, no discrepancy appears between ESE and control group juveniles in their propensities to commit crime and be suspended from school. However, a steep increase in the likelihood of being suspended from school for ESE juveniles appears at the ages of 16 and 17. In contrast, ESE juveniles are no more likely to commit crime until they turn 18, i.e., above the minimum school leaving age. At age 18, ESE juveniles start to commit more crime and remain significantly more likely to commit crime at all ages until age 24, when the data terminate. Very similar results emerging from IV regression specifications are shown in Figures A.2 and A.3 in the Appendix. While ESE (YINC) juveniles do not appear to face a greater risk of committing crime until they turn 18, they are significantly more likely to receive SDAs from age 16 in secondary school. As soon as they turn 18, when the legislation governing compulsory school attendance is no longer binding, they face a significantly greater risk of committing crime, which persists until the end of the crime-age profile.

3. Disentangling the Crime Age Profile

The educational experience of juveniles and the link with future criminality are of central interest to this paper. The results in Section 2 clearly show that ESE juveniles are more likely to engage in crime and to be disciplined at school. The derivation of a crime-age profile combining crime and school discipline records also shows ESE juveniles to receive more school suspensions at ages 16 and 17. At age 18, when compulsory schooling laws are no longer binding, ESE juveniles appear to switch from misbehaving in school to committing crime, with a greater likelihood observed at all ages 18-24 when the data terminate. This crime-age profile is consistent with an incapacitation effect of school on crime, as schools seem to internalise the bad behaviour of ESE juveniles and reduce their likelihood of committing crime until age 17. Once juveniles are no longer incapacitated, they start misbehaving outside school as reflected by their greater criminality from age 18 onwards.

The crime-age profile of ESE juveniles suggests a link exists between school suspensions and future crime, and thus raises some related questions. Are the ESE juveniles who receive school suspensions the same set of individuals committing crime after leaving school? Do juveniles switch from bad behaviour in school to crime at age 18 because of the existing compulsory schooling laws in Queensland? Finally, do school suspensions affect future criminality, or do they simply reflect similar unobservable individual-specific traits? The remainder of the paper presents three sets of statistical tests to answer these questions, develop a robust understanding of the crime-age profile and thus derive education policy prescriptions.

Youth-Specific ESE Impact on Crime and School Suspensions

Are the ESE juveniles who receive school suspensions the same individuals who commit crime? To address this question, the results in Tables 3 and 4 are recalculated separately for each ESE juvenile. A separate OLS regression comparing each ESE juvenile with the control group was estimated for school suspension and crime at the extensive margin, and the estimated youth-specific effects were then grouped in percentiles. Figure 4 shows the average percentile youth-specific effects of ESE status on crime by percentile youth-specific effects of ESE status on school suspension. A

strong positive correlation appears between percentile effects of ESE status on crime and school suspension, with the linear fit of the scatterplot being 0.24 and strongly statistically significant. This shows that juveniles born at the end of the calendar year who commit more crime are largely the same juveniles more likely to experience school suspension by age 17.

Compulsory School Attendance and the Crime Age Profile

The positive correlation in Figure 4 is consistent with the notion that schools internalise the bad behaviour of juveniles at risk, as the same juveniles seem to receive school suspensions as to commit crime. However, this is not informative on whether the crime-age profile in Figures 2 and 3 is the result of the legislation governing the school-leaving behaviour of juveniles in Queensland. In particular, this does not explain whether the compulsory schooling law causes the transition from SDAs to crime at age 18. One way to test this is to quantify the short-run impact on crime and school suspensions of a recent reform, i.e., Queensland's Earning or Learning (EL) Reform, which modified the legislation governing school leaving behaviour in 2006. 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 school leaving age from 16 to 17. This additional year of compulsory education was to be spent either in school, vocational training, or in a full-time job. The EL reform introduced a compulsory participation 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 first cohort to be affected by the EL reform was born in 1990, and thus the full sample of juveniles born at any point in time from 1987 to 1992 was used for this exercise. Cohorts born before 1987 could not be used here because the data starts in 2003, and thus the 1987 cohort is the earliest cohort that can be observed at the age of 16. For purposes of symmetry we also focus on three years post EL reform. The same exercise was also restricted to the subsample of juveniles that were born

from 1987 to 1992 within 60 days either side of the 1st January cut-off. Formally, the impact of the EL reform on school leaving behaviour, crime and school discipline sanctions was estimated using OLS as follows:

$$Y_i = \alpha + \beta_2 * EL + X_i\delta + \vartheta + \omega_i, \quad (2)$$

where *Y* is school leaving grade, crime or SDA of juvenile *i*, with crime and SDAs calculated until age 17. *EL* is a binary variable that takes the value 1 for juveniles born in 1990-92 who were subject to the EL reform, and value 0 for juveniles born 1987-89. In (2), estimates of β_2 show the short-run impact of the EL reform on school leaving behaviour, crime and SDA of juvenile *i*; *X* is a vector of individual characteristics, ϑ is a set of age fixed effects and ω is the error term. Standard errors are clustered at the year of birth level and, due to the small number of clusters, the Wild Cluster Bootstrap approach suggested in Cameron, Gelbach and Miller (2008) is used for inference.

Table 5 shows the result of the analysis for all types of crime and SDA, while Tables 6, 7 and 8 present results separately for property, drugs and violent offences and SDAs respectively. In Tables 5 to 8, the upper panel shows results for the full sample of juveniles born from 1987 to 1992, while the lower panel shows results for juveniles born 60 days either side of the 1st January cut-off in the same years. In all Tables, for convenience column (1) repeats the first order effect of the EL reform on the school leaving behaviour of juveniles. The result demonstrates that the EL reform increased school attendance by roughly half a year on average. These results are consistent between the full sample, shown in the upper panel of the Tables, and the sample born 60 days either side of the 1st January cut-off, shown in the lower panel. Columns (2) and (3) show the unintended impact of the EL reform on the extensive margin of crime and school suspension, while columns (4) and (5) show its impact on both the extensive and intensive margins of crime and school suspension. In columns (6) and (7), the dependent variables are the sum of crime records and SDAs in school. This simulation exercise is conducted to study the extent to which the potential increase in indiscipline in school may offset any crime-reducing effect of school. In each Table, column (6) shows the net effect of the EL reform on crime at the extensive margin, while column (7) shows the net effect of the EL reform on

the count of crimes at the extensive and intensive margins. In Tables 5 to 8, p-values from Wild Cluster Bootstrap estimation are reported in parentheses.

Estimates in Table 5 show that juveniles forced to stay at school for one more year by the EL reform reduced criminal participation both at the extensive and intensive margin. However, an increase in the count of school suspensions is also observed suggesting that the EL reform moved the bad behaviour of unruly juveniles from outside to inside school. These results appear consistent between the full sample, shown in the upper panel, and the sample born 60 days either side of the 1st January cut-off, shown in the lower panel. The EL reform reduced the likelihood of committing crime by 21 percent in the full sample and by 13 percent in the sample born 60 days either side of the 1st January cut-off, while the likelihood of receiving a school suspension increased by roughly 30 percent in both samples. While the count of crimes decreased by 20 to 25 percent for the cohorts exposed to the EL reform, the count of school suspensions increased by 37 to 38 percent. Although ESE and their control juveniles are not compared in this analysis, these results confirm the visual impression from Section 2 that the crime-age profile of ESE juveniles is affected by the compulsory schooling legislation in Queensland.

Columns (6) and (7) also show that, when school suspensions are also considered, no significant crime-reducing effect of the EL reform can be detected. Compared with the estimates in columns (2) and (3), the coefficients in columns (6) and (7) are also much smaller fractions of the means of the dependent variables prior to the reform. This of course is a worst-case scenario where in-school crime is overestimated, as not all school suspensions considered here are actually crimes. However, this is a useful exercise to address the concern that the omission of in-school crime in existing studies may have led to overestimate the crime-reducing effect of compulsory education. This concern cannot be ruled out based on these results.

Table 6 shows the results for property crime and school suspensions for property misconduct. Columns (2) and (4) respectively show that the EL reform reduced participation in property crime at the extensive margin by 18 percent and the count of property crime incidents by 27 percent. Columns

(3) and (5) show that the EL reform resulted in a 60 percent increase in school suspensions for property misconduct among juveniles who were affected by the EL reform. Columns (6) and (7) show that this increase in in-school indiscipline does not wipe out the net property crime-reducing effect of school, but it reduces the magnitude of the effect. Column (7) suggests that, when in-school property misconduct is also included, the EL reform does not reduce the count of property crime incidents by 27 percent, as suggested by column (4), but rather by a smaller 20 percent.

Results for drugs offences and school suspensions for illicit substances are presented in Table 7. Here a crime-reducing effect of the EL reform appears again, while no increase in school suspensions for illicit substances could be detected. Both the likelihood of committing drug offences as well as the count of drug offences committed by juveniles up to age 17 are reduced by 45 percent. As columns (6) and (7) show, when drugs offences and school suspensions for illicit substances are considered together, a 40 percent net crime-reducing effect of the EL reform appears both at the extensive and at the intensive margin.

A different conclusion emerges from Table 8, where estimates for violent crime and school suspensions for physical misconduct are presented. Police records suggest that exposure to the provisions of the EL reform did not affect violent crime. However, as Columns (3) and (5) of Table 8 show, the reform increased school suspensions for violent misconduct by roughly 40 percent both at the extensive and intensive margin. Columns (6) and (7) show that, when violence both outside and inside school are considered together, a net 28 percent increase in violent behaviour is found at the extensive and intensive margin. This is consistent with the increase in violence induced by compulsory school attendance in the US documented in Jacob and Lefgren (2003).

Tables A.8, A.9, A.10 and A.11 in the Appendix show the instrumental variable (IV) results when the EL reform is used to instrument the actual school leaving grade of juveniles born from 1987 to 1992. Results are presented for criminal offences and SDAs respectively and the Tables follow the same structure as Tables 5, 6, 7 and 8, with results being shown for the extensive margin of crime and school suspensions, as well as the combined extensive and intensive margins. In line with the

reduced form OLS estimates of the impact of the EL reform on crime and SDAs, the IV estimates show that the juveniles induced by the reform to stay in school for one extra year committed fewer property crime offences, even though they received more school suspensions for property misconduct by age 17. This is shown in the Appendix Table A.9. By the same age, juveniles induced to stay in school for longer also committed fewer drugs crime offences and received more school suspensions for physical misconduct. These results are shown in the Appendix Tables A.10 and A.11 respectively.

Impact of School Suspensions on Crime

The analysis of the EL reform confirms that compulsory school attendance reduces crime in the short run. However, this analysis also suggests that schools internalise bad behaviour, with juveniles unable to commit crime outside school when they are forced to stay in school. When in-school behaviour is considered, evidence of net reductions in property offences and drug offences still appear, while the incidence of violent offences increases. While an incapacitation effect of school on property crime and drugs crime is the likely driver of the net crime reductions in Tables 6, 7, A.9 and A.10, a concentration effect resulting in a net crime increase seems to be at play for violent crime (see Jacob and Lefgren, 2003). These findings confirm the visual impression that the laws governing mandatory school attendance affect the crime-age profiles and the transition from SDAs to crime in Figures 2 and 3. Schools internalise the bad behaviour of ESE juveniles, but as soon as they reach the minimum school leaving age, ESE juveniles become more likely to commit crime up until age 24 when the data terminate.

The final question addressed in this paper is whether discipline sanctions in secondary school have a direct effect on the future criminal activity of juveniles. Suspensions in secondary school and crime may simply reflect similar unobservable predetermined characteristics of juveniles. Thus, the crime-age profiles in Figures 2 and 3 may simply reflect an individual fixed effect. Alternatively, the adoption of behavioural rules and the use of discipline sanctions in school may actively contribute to the education production function and thus reduce the future criminal behaviour of juveniles (Lazear, 2001). The importance of behavioural norms and work ethics, strict behavioural expectations and

disciplinary codes in school is appraised in some recent empirical studies (Abdulkadiroglu et al., 2011; Dobbie and Fryer, 2011; Angrist et al., 2016; Dobbie and Fryer, 2015, 2016; and Machin and Sandi, 2019).

To explore this question, within-juvenile variation in categories of crime offences committed and categories of school suspensions received is exploited. Three categories of crime and school suspensions are observable in the data, namely property crime and SDAs, drugs crime and SDAs, and violent crime and SDAs. Thus, criminal activity at age 18-24 of category *c* can be modelled on SDAs at age 15-17 of category *c*. For this analysis, a dataset with three observations per individual was constructed, i.e., one observation for each crime/SDA category by individual. As within-individual variation in crime and school suspension records by category is modelled here, the coefficient of interest is identified when individual fixed effects are also included in the specification. Once the inherent individual-specific propensity to commit crime is controlled for via inclusion of individual fixed effects, the estimated coefficient for school suspensions at age 15-17 is identified with the remaining within-individual crime-/SDA-category specific variation. This analysis should reveal whether school suspensions of a certain category (e.g., property crime) during compulsory schooling age have an impact on future criminal activity of the same category (e.g., property crime).[19] Our estimating equation can be expressed as follows:

$$Y_{ic18-24} = \alpha + \beta_3 * SDA_{ic15-17} + i + c + \epsilon_i, \quad (3)$$

where $Y_{ic18-24}$ measures participation in criminal activity of category c by juvenile i from age 18 to 24 at the extensive margin. In (3), $SDA_{ic15-17}$ captures school suspensions of category c by juvenile i from age 15 to 17, i is a set of individual fixed effects, c is a set of crime/suspension category fixed effects, and ϵ is the error term. The sample of this analysis is all juveniles observed continuously from

[19] A similar approach is used in Murphy and Weinhardt (2018) to study the impact of relative ranking in primary school on future school performance. In their setting, since grades in three subjects (English, Maths and Science) are available for each student in primary and secondary school, within-individual variation in primary school ranking and future school performance by subject can be exploited.

age 15 in state-maintained schools in Queensland up until age 18 or older. Standard errors are clustered at the individual level for inference.

Table 9 shows the results of this exercise. In columns (1) to (3), criminal activity from age 18 to 24 is modelled on a binary variable (i.e., 0/1) indicating whether individual *i* received any school suspensions of category *c* from age 15 to 17, while Columns (4) to (6) display the results for the count of school suspensions of category *c* from age 15 to 17. The upper panel shows the results for the full sample and the lower panel shows results for the subsample of individuals born within 60 days either side of the 1st January cut-off. Columns (1) and (4) show results without controlling for individual fixed effects, as controls are only included for individual characteristics, year of birth fixed effects and crime/suspension category fixed effects. All other columns additionally include individual fixed effects, with Columns (5) and (6) also controlling for count of crime records of category *c* from age 15 to 17.

Columns (1) and (4) show a strong positive correlation between criminal activity at age 18 to 24 and school suspensions at age 15 to 17. This is consistent with the results in Figure 4 for ESE juveniles. However, when individual fixed effects are included in the estimating equation, no significance appears in the relationship between discipline sanctions in school and future crime. This is shown in columns (2), (3), (5) and (6). This suggests that school suspensions do not affect future criminal participation. The strong positive correlations between SDAs and future crime observed in columns (1) and (4) appear entirely driven by unobservable time-invariant characteristics of individuals, i.e. by the individual fixed effects. The logical corollary of these results is that discipline sanctions do not decrease future criminality of individuals. Arguably, more support and assistance are needed to address the behavioural issues of juveniles to prevent their future criminal participation. These results also suggest that school discipline sanctions and crime reflect similar individual characteristics and the former can therefore be used as flags to predict future criminal activity before crime has taken place.

4. Conclusion

A large literature has documented the disadvantage stemming from early school eligibility. This has been shown to affect the path of skills acquisitions, school grades since childhood and, more recently, criminal participation. However, very little is known about the educational experience of early school eligible juveniles and how this may affect their criminal trajectories. Using administrative data linking education and criminal records of individuals in the state of Queensland in Australia from 2003 to 2014, this study examines the educational experience of these juveniles and its effect on their criminal trajectories.

Juveniles born at the end of the calendar year are more likely to commit crime as well as to receive discipline sanctions in school. A very clear profile emerges from the analysis, suggesting that juveniles born at the end of the calendar year are more likely to receive school suspensions by age 17 and to commit more crime post-compulsory schooling starting from age 18. This crime-age profile is consistent with an incapacitation effect of school on crime, as schools appear to internalise the bad behaviour of ESE juveniles and decrease their likelihood of committing crime until they leave school.

A series of statistical tests is also conducted to develop an understanding of the link between school suspensions and future crime and derive policy prescriptions. Individual-specific estimates show that, largely, the same juveniles who receive school suspensions in secondary school also commit criminal offences by age 24. Availability of a recent school leaving age reform allows us to show that the crime age profile is causally affected by the legislation governing compulsory schooling in Queensland. When juveniles are forced to stay at school for one additional year, a reduction in crime and an increase in school disciplinary sanctions are both observed. The crime reduction is driven by reductions in property crime and drugs crime, while violent crime remains unaffected. The increase in school suspensions is driven by the increase in disciplinary sanctions for property and physical misconduct. When crime records and school suspensions are considered together, a net reduction in property crime and drugs offences still appears, while violence increases as a result of the lengthened duration of compulsory schooling. By quantifying the extent to which schools

internalise the bad behaviour of unruly juveniles when forced to stay on at school, this analysis also mitigates the concern that the crime-reducing effect of school documented in the prior literature may be a statistical artefact, i.e., due to systematic under-reporting of in-school crime to the police. Additional statistical tests reveal that, once unobservable time-invariant individual-specific characteristics are considered, the receipt of discipline sanctions in school for a certain type of misconduct has no effect on future criminal participation of the same type.

The overall conclusion of this study is that ESE juveniles receive more disciplinary sanctions while they are forced to attend school and start to commit more crime once they are allowed to leave school. School disciplinary sanctions do not affect future criminality but they strongly predict it, thus providing a valuable flag to identify individuals at risk. From a policy perspective, this underlines the fact that education policy may reduce youth crime participation not just by keeping juveniles off the streets, but also by observing their behaviour in school and thus helping identify juveniles at risk. If an adolescent at risk is forced to stay at school, this shifts trouble from outside to inside the school premises, and compulsory education can play an active role in reducing crime. Generating an understanding of the education experience of adolescents at risk and their criminal behaviour is a step toward helping them avoid the revolving door between poor school behaviour, juvenile delinquency and future incarceration.

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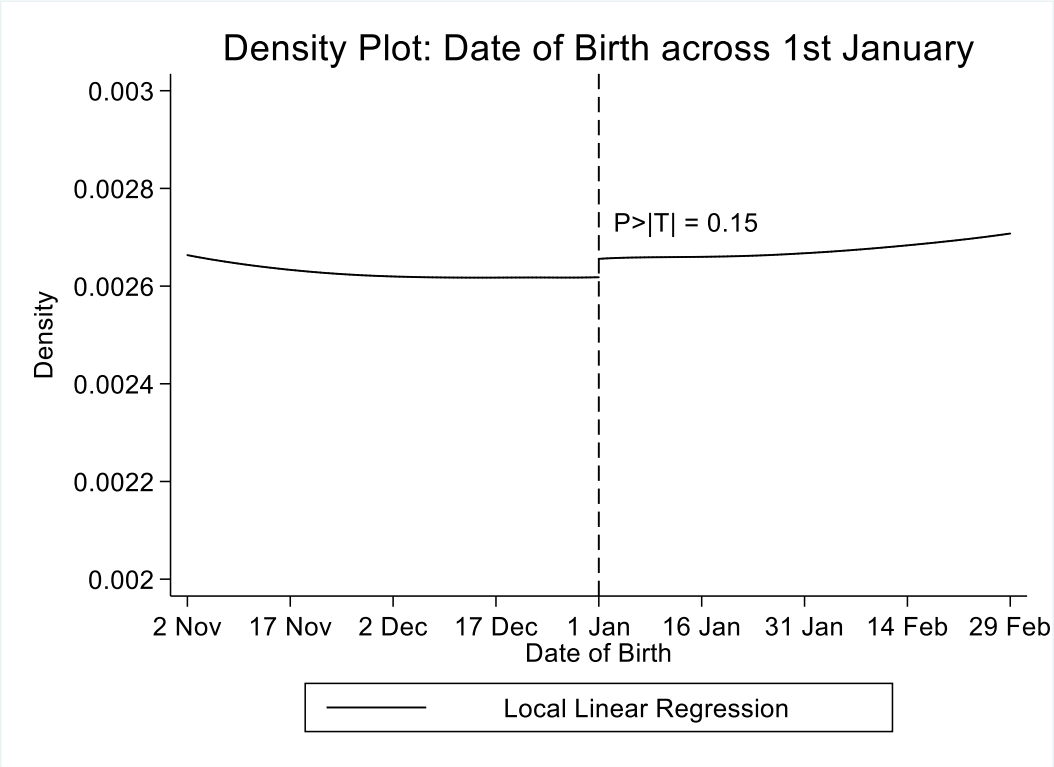
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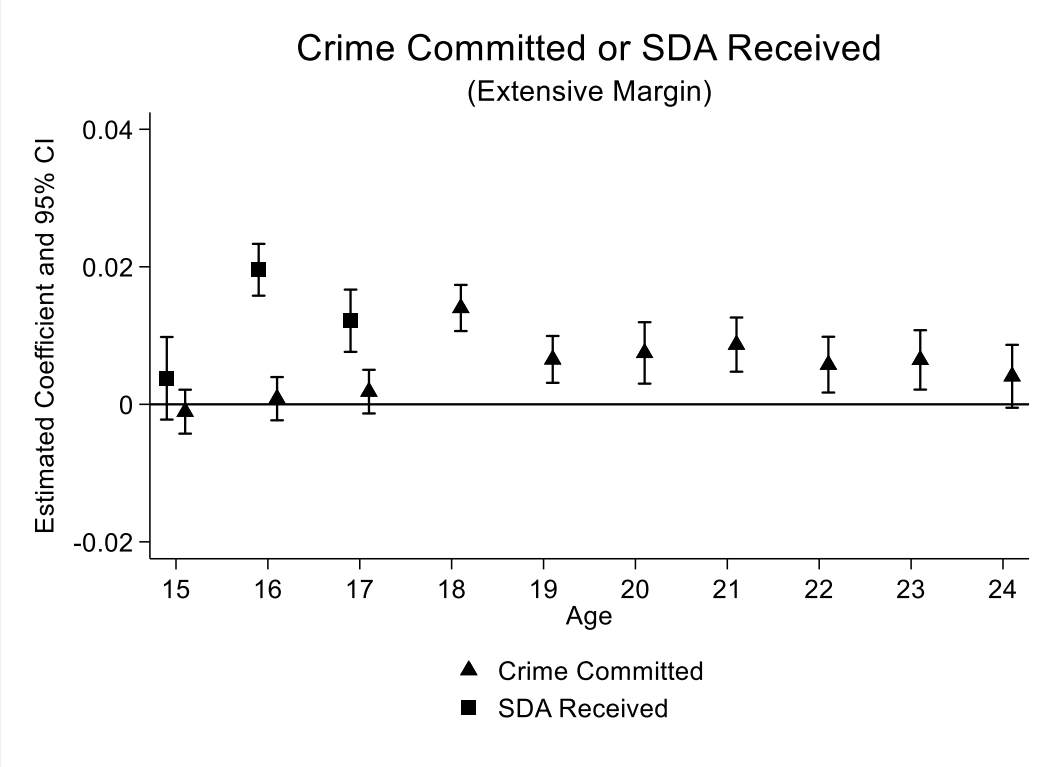
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Figure 1. Density Test of Births Across 1st January Cut-off.



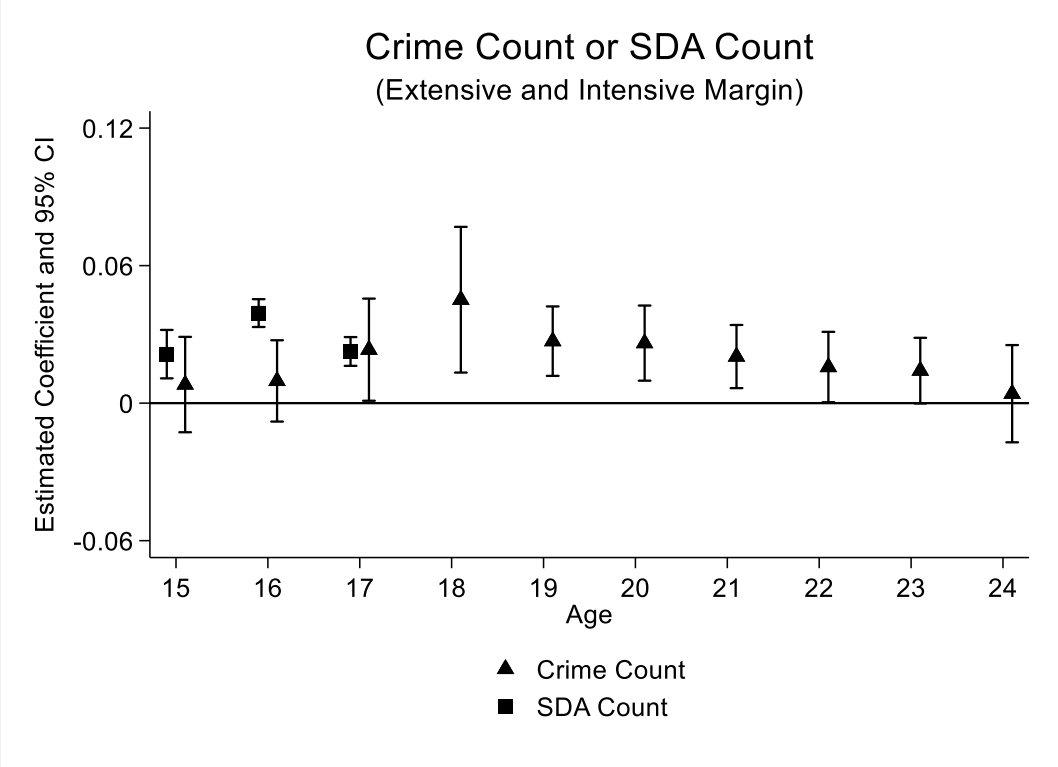
Notes: The horizontal axis represents the date of birth for the sample of youths born within 60 days either side of the 1st January cut-off. Local linear regression estimates using a triangle kernel and a 60-day bandwidth are shown either side of the 1st January cut-off. The null hypothesis of equality in the densities either side of the 1st January cut-off could not be rejected at the 10 percent level based on the local linear regression specification with a 60-day bandwidth and triangle kernel that is shown here. The null hypothesis of equality in the densities either side of the 1st January cut-off could not be rejected at the 10 percent level also when using a local quadratic regression specification with a 60-day bandwidth and triangle kernel (P> |T| = 0.34).

Figure 2. OLS Estimates of ESE Impact on Crime and SDAs by Age at the Extensive Margin.



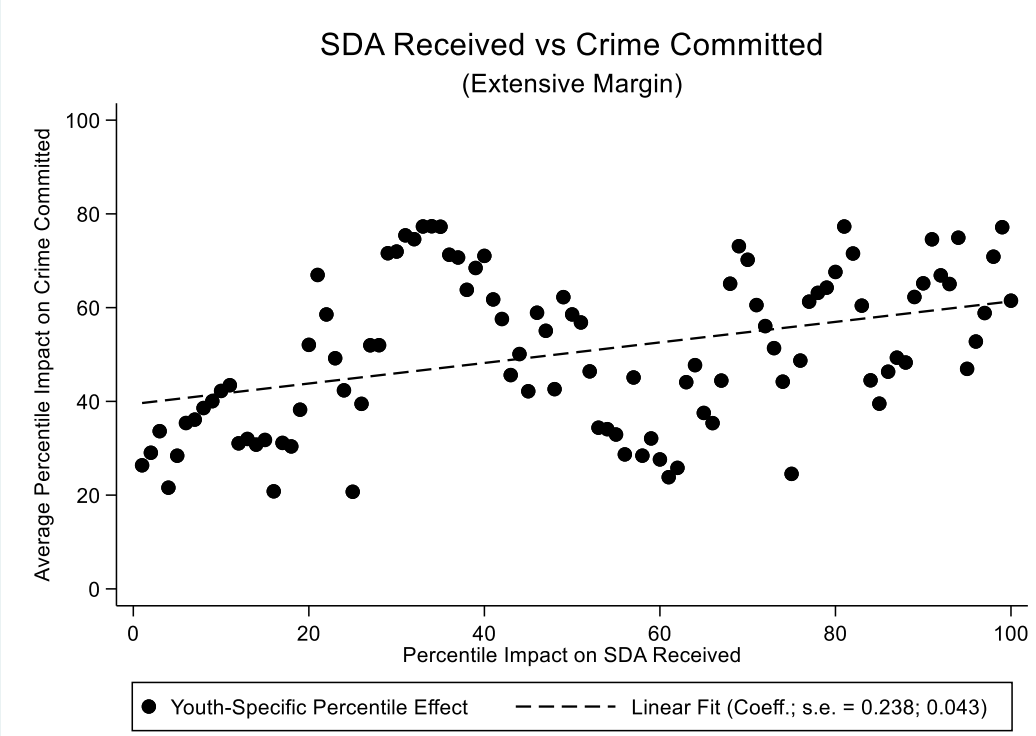
Notes: Figure 2 shows point estimates and 95 percent confidence intervals of the causal effect of early school eligibility (ESE) on the likelihood to commit crime (extensive margin) and on the likelihood to receive a school disciplinary absence (SDA, extensive margin) by age. All estimates are obtained from OLS regression specifications, which include distance to the 1st January cut-off, pre-cut-off indicator (Nov-Dec = 1), their interaction term and discontinuity fixed effects. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the youths are male, whether they are native English speakers and a binary indicator for whether the native language is unknown. Crime offences include property damage and theft, drugs, violence and other crimes by offender in a year. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. Other crimes include burglary, dangerous or negligent acts endangering persons, blackmail, fraud, deception and related offences, robbery, weapon offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations offences, and miscellaneous offences by offender in a year. SDAs include property misconduct SDAs, illicit substance SDAs, physical misconduct SDAs and other SDAs received per youth in a year. Other SDAs include absence SDAs, licit substance SDAs, persistently disruptive behaviour SDAs, refusal to participate SDAs, verbal SDAs and other misconduct SDAs.

Figure 3. OLS Estimates of ESE Impact on Crime and SDAs by Age at the Extensive and Intensive Margin.



Notes: Figure 3 shows point estimates and 95 percent confidence intervals of the causal effect of early school eligibility (ESE) on the count of crime offences (extensive and intensive margins) and on the count of school disciplinary absences (SDA, extensive and intensive margins) by age. All estimates are obtained from OLS regression specifications, which include distance to the 1st January cut-off, pre-cut-off indicator (Nov-Dec = 1), their interaction term and discontinuity fixed effects. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the youths are male, whether they are native English speakers and a binary indicator for whether the native language is unknown. Crime offences include property damage and theft, drugs, violence and other crimes by offender in a year. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. Other crimes include burglary, dangerous or negligent acts endangering persons, blackmail, fraud, deception and related offences, robbery, weapon offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations offences, and miscellaneous offences by offender in a year. SDAs include property misconduct SDAs, illicit substance SDAs, physical misconduct SDAs and other SDAs received per youth in a year. Other SDAs includes absence SDAs, licit substance SDAs, persistently disruptive behaviour SDAs, refusal to participate SDAs, verbal SDAs and other misconduct SDAs.

Figure 4. Average Youth-Specific Percentile Estimates of ESE Impact on Crime by Youth-Specific Percentile Estimates of ESE Impact on SDAs.



Notes: Figure 4 shows average percentile youth-specific OLS estimates of the causal effect of early school eligibility (ESE) on the likelihood to commit crime (extensive margin) by percentile youth-specific OLS estimates of the causal effect of ESE on the likelihood to receive school disciplinary absences in secondary school (SDAs, extensive margin). A linear function fitting the distribution of youth-specific OLS estimates is also shown in Figure 4, and the legend reports its OLS coefficient and standard error. For each youth in the analysis, the horizontal axis shows the percentile OLS estimate of the causal effect of ESE on SDAs in secondary school, whereas the vertical axis shows the average percentile youth-specific OLS estimates of the causal effect of ESE on crime. Crime offences include property damage and theft, drugs, violence and other crimes by offender in a year. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. Other crimes include burglary, dangerous or negligent acts endangering persons, blackmail, fraud, deception and related offences, robbery, weapon offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations offences, and miscellaneous offences by offender in a year. SDAs include property misconduct SDAs, illicit substance SDAs, physical misconduct SDAs and other SDAs received per youth in a year. Other SDAs include absence SDAs, licit substance SDAs, persistently disruptive behaviour SDAs, refusal to participate SDAs, verbal SDAs and other misconduct SDAs.

Table 1. Means of Selected Behavioural Outcomes.

	Crime Committed	SDA Received	Crime Count	SDA Count
	(Extensive Margin)		(Extensive and Intensive Margin)	
	(1)	(2)	(3)	(4)
Mean	0.031	0.068	0.083	0.103
No. Youths (Full Sample)	636061	636061	636061	636061
Mean	0.031	0.068	0.081	0.104
No. Youths (90 days Bandwidth)	308628	308628	308628	308628
Mean	0.031	0.069	0.081	0.105
No. Youths (60 days Bandwidth)	203355	203355	203355	203355
Mean	0.031	0.069	0.080	0.107
No. Youths (30 days Bandwidth)	101285	101285	101285	101285

Notes: Table 1 shows means of selected behavioural outcomes. All crimes includes property damage and theft, drugs, violence and other crimes by offender in a year. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. Other crimes include burglary, dangerous or negligent acts endangering persons, blackmail, fraud, deception and related offences, robbery, weapon offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations offences, and miscellaneous offences by offender in a year. All school disciplinary absences (SDAs) include property misconduct SDAs, illicit substance SDAs, physical misconduct SDAs and other SDAs received per youth in a year. Other SDAs include absence SDAs, licit substance SDAs, persistently disruptive behaviour SDAs, refusal to participate SDAs, verbal SDAs and other misconduct SDAs.

Table 2. Balancing of Individual and Primary School Characteristics across the 1st January Cut-off.

	Impact on ESE status (Nov-Dec = 1) on:	OLS	Local Linear Regression
		(1)	(2)
(1)	Male	-0.001	-0.006
		(0.005)	(0.006)
(2)	Native Speaker	0.025	0.022
		(0.022)	(0.030)
(3)	Not Known Native Speaker	-0.009	-0.002
		(0.012)	(0.017)
(4)	ICSEA in final year of Primary School (X 100)	-1.210	45.641
		(3.607)	(298.68)
(5)	Not Known ICSEA in final year of Primary School	0.001	-0.046
		(0.004)	(0.304)
(6)	SD Index in final year of Primary School (X 100)	-1.305	45.556
		(3.635)	(298.68)
(7)	Not Known SD Index in final year of Primary School	0.001	-0.046
		(0.004)	(0.304)
(8)	Fraction of Male Classmates in final year of Primary School	-0.002	0.023
		(0.002)	(0.156)
(9)	Not Known Fraction of Male Classmates in final year of Primary School	0.001	-0.046
		(0.004)	(0.303)
(10)	Fraction of Native Speaker Classmates in final year of Primary School	0.000	0.044
		(0.003)	(0.283)
(11)	Not Known Fraction of Native Speaker Classmates in final year of Primary School	0.001	-0.046
		(0.004)	(0.303)
(12)	Number of Classmates in final year of Primary School	-0.004	-1.059
		(0.088)	(7.016)
(13)	Not Known Number of Classmates in final year of Primary School	0.001	-0.046
		(0.004)	(0.304)
(14)	Final year of Primary School Fixed Effects	-7.239	46.179
		(8.710)	(323.75)
(15)	Not Known Final year of Primary School Fixed Effects	0.001	-0.046
		(0.004)	(0.304)
	Discontinuity Fixed Effects	Yes	Yes
	Youths	203355	203355

Notes: Presented in each row are the discontinuity estimates of the given variable at the 1st January cut-off date. Estimates in column (1) are obtained from OLS regression specifications where a bandwidth of 60 days either side of the cut-off is used. Estimates in column (2) are obtained from local linear regression specifications where a triangular kernel with bandwidth of 60 days either side of the cut-off is used. Robust standard errors are clustered at the date of birth level in rows (1) to (3), and at the primary school level in rows (4) to (15). All specifications include distance to the 1st January cut-off and its interaction term with the pre-cut-off indicator (Nov-Dec = 1). * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 3. Estimates of ESE Impact on Crime.

	Crime Committed (Extensive Margin)			Crime Count (Extensive and Intensive Margin)		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
ESE	0.003***	0.006***	0.005***	0.014***	0.020***	0.019***
	(0.001)	(0.001)	(0.001)	(0.004)	(0.004)	(0.004)
No. Youths (90 days Bandwidth)	308628	308628	308628	308628	308628	308628
ESE	0.003***	0.005***	0.005***	0.016***	0.022***	0.019***
	(0.001)	(0.001)	(0.001)	(0.005)	(0.005)	(0.005)
No. Youths (60 days Bandwidth)	203355	203355	203355	203355	203355	203355
ESE	0.004**	0.005***	0.004***	0.016**	0.020***	0.016***
	(0.001)	(0.001)	(0.001)	(0.006)	(0.006)	(0.005)
No. Youths (30 days Bandwidth)	101285	101285	101285	101285	101285	101285
Primary School Characteristics	No	Yes	Yes	No	Yes	Yes
Primary School x Discontinuity Fixed Effects	No	No	Yes	No	No	Yes
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Discontinuity Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 3 shows estimates of the causal effect of early school eligibility (ESE) on crime. All estimates are obtained from OLS regression specifications. All specifications include distance to the 1st January cut-off, pre-cut-off indicator (Nov-Dec = 1) and their interaction term. Robust standard errors (clustered at the date of birth level) are reported in parentheses. Individual Characteristics variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Primary School Characteristics variables included are calculated in the final year of primary school and they are the School Deprivation Index, the School ICSEA Index, the share of male classmates, the share of English native speaker classmates, the number of classmates and a series of binary indicators for whether the Primary School Deprivation Index is unknown, for whether the Primary School ICSEA Index is unknown, for whether the share of male classmates in the final year of primary school is unknown, for whether the share of English native speaker classmates in the final year of primary school is unknown, and for whether the number of classmates in the final year of primary school is unknown. Crime offences include property damage and theft, drugs, violence and other crimes by offender in a year. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. Other crimes include burglary, dangerous or negligent acts endangering persons, blackmail, fraud, deception and related offences, robbery, weapon offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations offences, and miscellaneous offences by offender in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 4. Estimates of ESE Impact on SDAs.

	SDA Received (Extensive Margin)			SDA Count (Extensive and Intensive Margin)		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
ESE	0.011***	0.011***	0.011***	0.027***	0.027***	0.026***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
No. Youths (90 days Bandwidth)	308628	308628	308628	308628	308628	308628
ESE	0.011***	0.011***	0.011***	0.026***	0.026***	0.025***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
No. Youths (60 days Bandwidth)	203355	203355	203355	203355	203355	203355
ESE	0.008***	0.008***	0.007***	0.019***	0.019***	0.017***
	(0.002)	(0.003)	(0.002)	(0.004)	(0.004)	(0.004)
No. Youths (30 days Bandwidth)	101285	101285	101285	101285	101285	101285
Primary School Characteristics	No	Yes	Yes	No	Yes	Yes
Primary School x Discontinuity Fixed Effects	No	No	Yes	No	No	Yes
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Discontinuity Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 4 shows estimates of the causal effect of early school eligibility (ESE) on school disciplinary absences (SDAs) in secondary school. All estimates are obtained from OLS regression specifications. All specifications include distance to the 1st January cut-off, pre-cut-off indicator (Nov-Dec = 1) and their interaction term. Robust standard errors (clustered at the date of birth level) are reported in parentheses. Individual Characteristics variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Primary School Characteristics variables included are calculated in the final year of primary school and they are the School Deprivation Index, the School ICSEA Index, the share of male classmates, the share of English native speaker classmates, the number of classmates and a series of binary indicators for whether the Primary School Deprivation Index is unknown, for whether the Primary School ICSEA Index is unknown, for whether the share of male classmates in the final year of primary school is unknown, for whether the share of English native speaker classmates in the final year of primary school is unknown, and for whether the number of classmates in the final year of primary school is unknown. SDAs include property misconduct SDAs, illicit substance SDAs, physical misconduct SDAs and other SDAs received per youth in a year. Other SDAs include absence SDAs, licit substance SDAs, persistently disruptive behaviour SDAs, refusal to participate SDAs, verbal SDAs and other misconduct SDAs. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 5. Estimates of School Leaving Age Reform Impact on School Leaving Grade, Crime and SDAs.

	School Leaving Grade	Crime Committed at age 15 to 17	SDA Received at age 15 to 17	Crime Count at age 15 to 17	SDA Count at age 15 to 17	Crime Count at age 15 to 17 + SDA Count at age 15 to 17	
		(Extensive Margin)		(Extensive and Intensive Margin)		(Extensive Margin)	(Extensive and Intensive Margin)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	OLS	OLS	OLS	OLS
EL Reform	0.509***	-0.009***	0.018*	-0.030***	0.031*	0.007	0.001
	(p = 0.003)	(p = 0.008)	(p = 0.095)	(p = 0.007)	(p = 0.059)	(p = 0.511)	(p = 0.929)
Pre EL Reform Mean Dep. Var.	11.414	0.042	0.058	0.114	0.081	0.091	0.195
No. Youths (Full Sample)	293562	293562	293562	293562	293562	293562	293562
EL Reform	0.503***	-0.006**	0.018	-0.021	0.031*	0.009	0.010
	(p = 0.004)	(p = 0.047)	(p = 0.114)	(p = 0.101)	(p = 0.085)	(p = 0.394)	(p = 0.622)
Pre EL Reform Mean Dep. Var.	11.390	0.040	0.059	0.108	0.084	0.091	0.192
No. Youths (60 days Bandwidth)	93924	93924	93924	93924	93924	93924	93924
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 5 shows estimates of the causal effect of the Earning or Learning (EL) reform on school leaving grade, crime at age 15 to 17 and school disciplinary absences (SDAs) at age 15 to 17. All estimates are obtained from OLS regression specifications. Standard errors were clustered at the year of birth level and the p-values from Wild Cluster Bootstrap estimation are reported in parentheses. Control variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Crime offences include property damage and theft, drugs, violence and other crimes by offender in a year. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. Other crimes include burglary, dangerous or negligent acts endangering persons, blackmail, fraud, deception and related offences, robbery, weapon offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations offences, and miscellaneous offences by offender in a year. SDAs include property misconduct SDAs, illicit substance SDAs, physical misconduct SDAs and other SDAs received per youth in a year. Other SDAs include absence SDAs, licit substance SDAs, persistently disruptive behaviour SDAs, refusal to participate SDAs, verbal SDAs and other misconduct SDAs. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 6. Estimates of School Leaving Age Reform Impact on School Leaving Grade, Property Crime and Property SDAs.

	School Leaving Grade	Property Crime Committed	Property SDA Received	Property Crime Count	Property SDA Count	Property Crime Count + Property SDA Count	
		(Extensive Margin)		(Extensive and Intensive Margin)		(Extensive Margin)	(Extensive and Intensive Margin)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	OLS	OLS	OLS	OLS
EL Reform	0.509***	-0.005***	0.003*	-0.018***	0.003*	-0.003	-0.015**
	(p = 0.003)	(p = 0.008)	(p = 0.072)	(p = 0.008)	(p = 0.073)	(p = 0.159)	(p = 0.019)
Pre EL Reform Mean Dep. Var.	11.414	0.028	0.005	0.067	0.005	0.032	0.072
No. Youths (Full Sample)	293562	293562	293562	293562	293562	293562	293562
EL Reform	0.503***	-0.004	0.003*	-0.012	0.003*	-0.001	-0.009
	(p = 0.004)	(p = 0.144)	(p = 0.073)	(p = 0.174)	(p = 0.054)	(p = 0.529)	(p = 0.283)
Pre EL Reform Mean Dep. Var.	11.390	0.027	0.005	0.063	0.005	0.031	0.068
No. Youths (60 days Bandwidth)	93924	93924	93924	93924	93924	93924	93924
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 6 shows estimates of the causal effect of the Earning or Learning (EL) reform on school leaving grade, property crime at age 15 to 17 and property misconduct school disciplinary absences (SDAs) at age 15 to 17. All estimates are obtained from OLS regression specifications. Standard errors were clustered at the year of birth level and the p-values from Wild Cluster Bootstrap estimation are reported in parentheses. Control variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Crime offences include property damage and theft by offender in a year. SDAs include property misconduct SDAs received per youth in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 7. Estimates of School Leaving Age Reform Impact on School Leaving Grade, Drugs Crime and Illicit Substance SDAs.

	School Leaving Grade	Drugs Crime Committed	Illicit Substance SDA Received	Drugs Crime Count	Illicit Substance SDA Count	Drugs Crime Count + Illicit Substance SDA Count	
		(Extensive Margin)		(Extensive and Intensive Margin)		(Extensive Margin)	(Extensive and Intensive Margin)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	OLS	OLS	OLS	OLS
EL Reform	0.509***	-0.005***	-0.000	-0.008***	-0.000	-0.005***	-0.008***
	(p = 0.003)	(p = 0.009)	(p = 0.449)	(p = 0.008)	(p = 0.492)	(p = 0.009)	(p = 0.008)
Pre EL Reform Mean Dep. Var.	11.414	0.011	0.002	0.018	0.002	0.013	0.020
No. Youths (Full Sample)	293562	293562	293562	293562	293562	293562	293562
EL Reform	0.503***	-0.004***	-0.000	-0.007***	-0.000	-0.005***	-0.008***
	(p = 0.004)	(p = 0.007)	(p = 0.239)	(p = 0.008)	(p = 0.275)	(p = 0.008)	(p = 0.007)
Pre EL Reform Mean Dep. Var.	11.390	0.010	0.002	0.018	0.002	0.012	0.020
No. Youths (60 days Bandwidth)	93924	93924	93924	93924	93924	93924	93924
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 7 shows estimates of the causal effect of the Earning or Learning (EL) reform on school leaving grade, drugs crime at age 15 to 17 and illicit substances school disciplinary absences (SDAs) at age 15 to 17. All estimates are obtained from OLS regression specifications. Standard errors were clustered at the year of birth level and the p-values from Wild Cluster Bootstrap estimation are reported in parentheses. Control variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Crime offences include drugs crimes by offender in a year. SDAs include illicit substances SDAs received per youth in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 8. Estimates of School Leaving Age Reform Impact on School Leaving Grade, Violence Crime and Violence SDAs.

	School Leaving Grade	Violence Crime Committed	Violence SDA Received	Violence Crime Count	Violence SDA Count	Violence Crime Count + Violence SDA Count	
		(Extensive Margin)		(Extensive and Intensive Margin)		(Extensive Margin)	(Extensive and Intensive Margin)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	OLS	OLS	OLS	OLS
EL Reform	0.509***	-0.000	0.007*	-0.000	0.008**	0.006	0.008*
	(p = 0.003)	(p = 0.215)	(p = 0.054)	(p = 0.482)	(p = 0.040)	(p = 0.129)	(p = 0.059)
Pre EL Reform Mean Dep. Var.	11.414	0.008	0.017	0.010	0.018	0.024	0.029
No. Youths (Full Sample)	293562	293562	293562	293562	293562	293562	293562
EL Reform	0.503***	0.000	0.007*	0.001	0.008**	0.006	0.008**
	(p = 0.004)	(p = 0.923)	(p = 0.055)	(p = 0.145)	(p = 0.045)	(p = 0.102)	(p = 0.034)
Pre EL Reform Mean Dep. Var.	11.390	0.008	0.017	0.010	0.019	0.024	0.029
No. Youths (60 days Bandwidth)	93924	93924	93924	93924	93924	93924	93924
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 8 shows estimates of the causal effect of the Earning or Learning (EL) reform on school leaving grade, violence crime at age 15 to 17 and violence school disciplinary absences (SDAs) at age 15 to 17. All estimates are obtained from OLS regression specifications. Standard errors were clustered at the year of birth level and the p-values from Wild Cluster Bootstrap estimation are reported in parentheses. Control variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. SDAs include physical misconduct SDAs received per youth in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

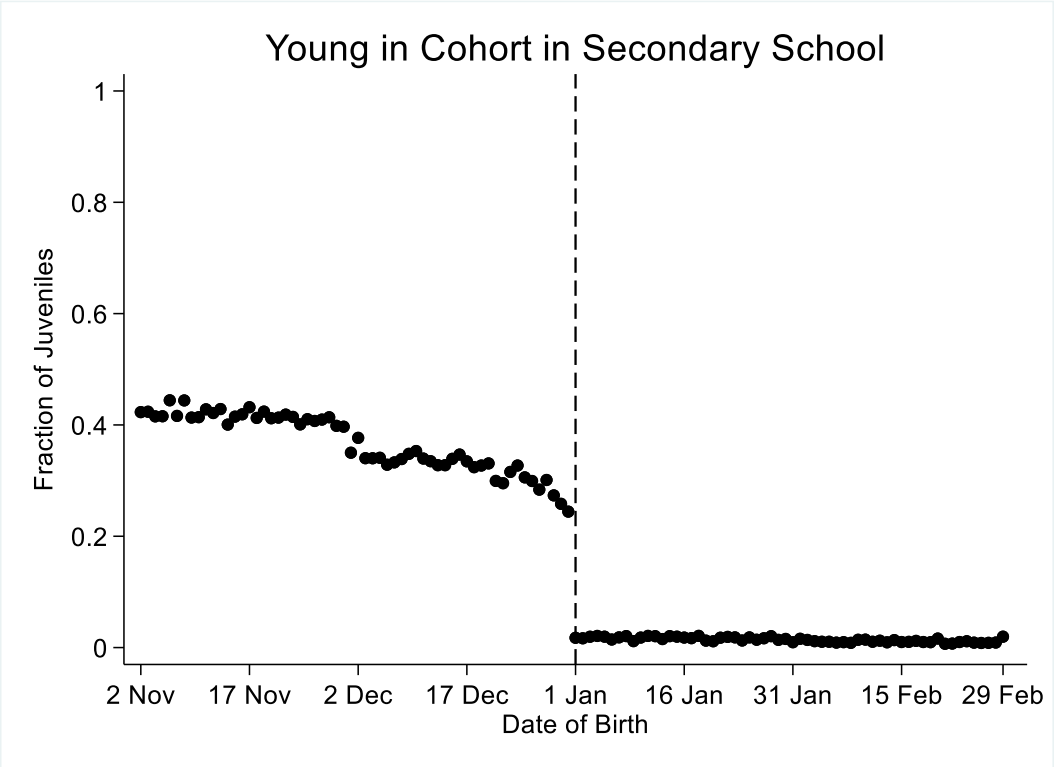
Table 9. Estimates of SDA by Age 17 Impact on Crime at Age 18 to 24.

	Crime Committed Age 18 to 24 (Extensive Margin)					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Any SDA from Age 15 to 17	0.086***	0.001	0.000			
	(0.002)	(0.002)	(0.002)			
Count of SDAs from Age 15 to 17				0.063***	0.000	-0.000
				(0.001)	(0.001)	(0.001)
Mean Dependent Variable	0.038	0.038	0.038	0.038	0.038	0.038
No. Observations (Full Sample)	1574442	1574442	1574442	1574442	1574442	1574442
No. Youths (Full Sample)	524814	524814	524814	524814	524814	524814
Any SDA from Age 15 to 17	0.085***	0.002	0.001			
	(0.003)	(0.003)	(0.003)			
Count of SDAs from Age 15 to 17				0.064***	0.002	0.002
				(0.002)	(0.002)	(0.002)
Mean Dependent Variable	0.038	0.038	0.038	0.038	0.038	0.038
No. Observations (60 days Bandwidth)	504015	504015	504015	504015	504015	504015
No. Youths (60 days Bandwidth)	168005	168005	168005	168005	168005	168005
Individual Characteristics	Yes	No	No	Yes	No	No
Year of Birth Fixed Effects	Yes	No	No	Yes	No	No
Crime Category Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	No	Yes	Yes	No	Yes	Yes
Count of Crimes from Age 15 to 17	No	No	Yes	No	No	Yes

Notes: Table 9 shows estimates of the link between the receipt of SDAs at age 15 to 17 and crime committed at age 18 to 24 (extensive margin). All estimates are obtained from OLS regression specifications. Robust standard errors (clustered at the individual level) are reported in parentheses. Individual Characteristics variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Crime offences include property damage and theft, drugs and violence by offender in a year. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. SDAs include property misconduct SDAs, illicit substance SDAs and physical misconduct SDAs received per youth in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

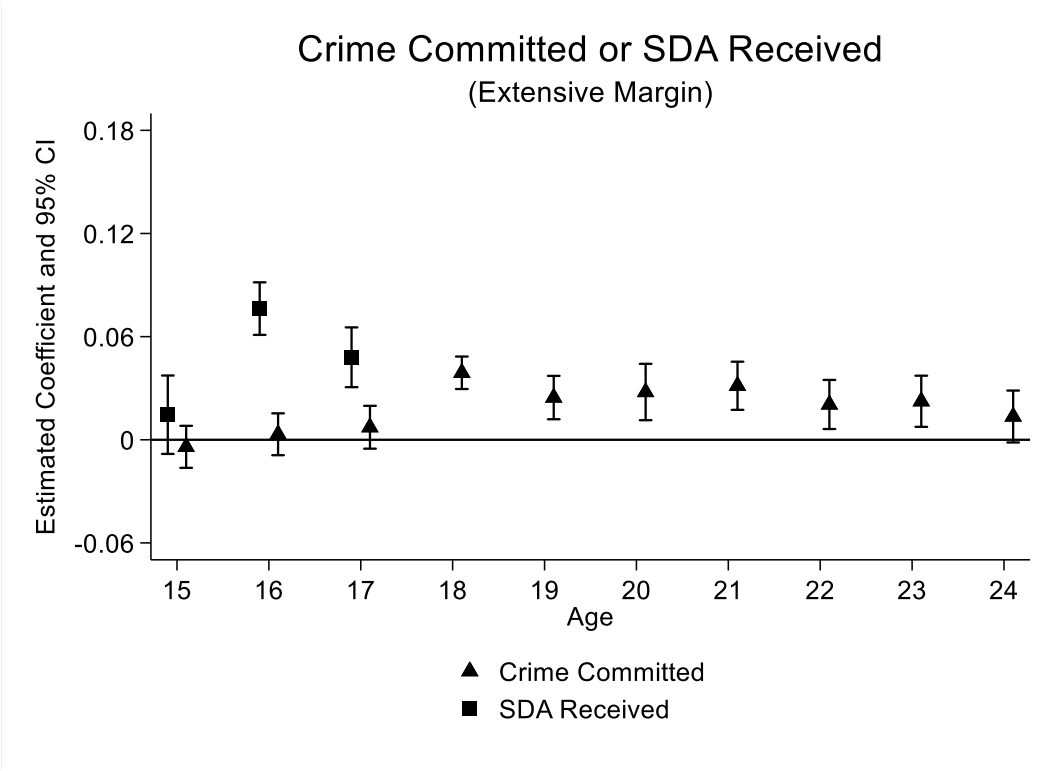
APPENDIX

Figure A.1. Fraction of Young in Cohort (YINC) Juveniles by Date of Birth.



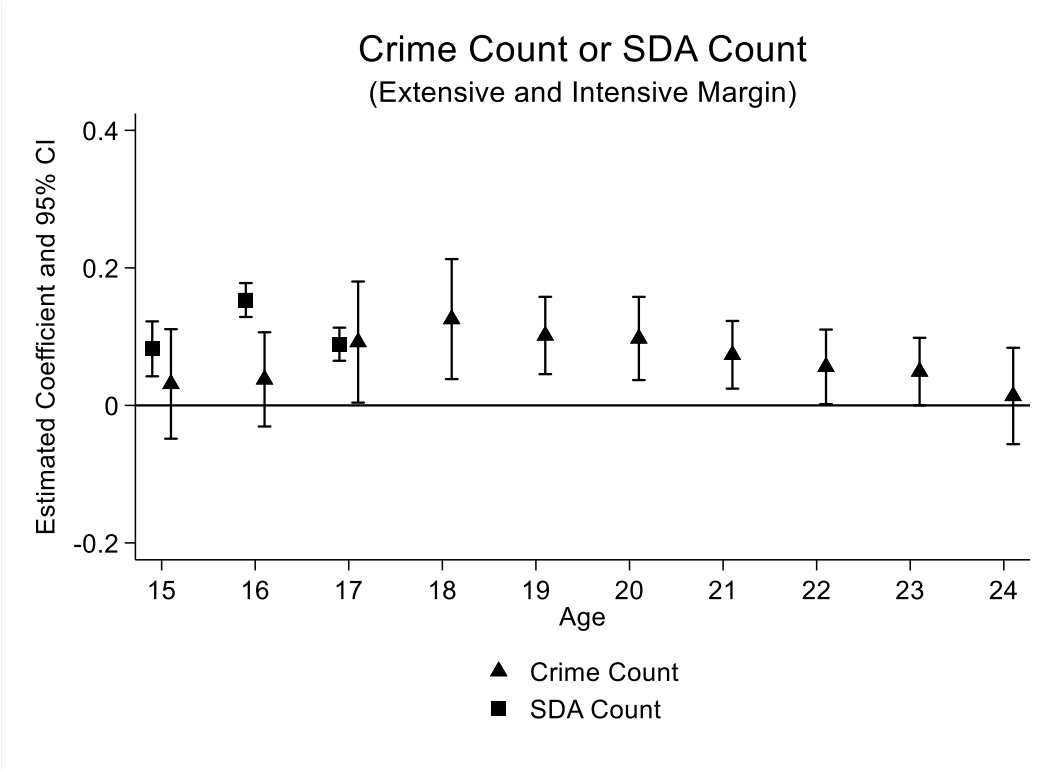
Notes: Figure A.1 shows the fraction of juveniles who are 'young-in-cohort' at the beginning of secondary school by date of birth for juveniles born 60 days either side of the 1st January cut-off.

Figure A.2. IV Estimates of YINC Impact on Crime and SDAs by Age at the Extensive Margin.



Notes: Figure A.2 shows point estimates and 95 percent confidence intervals of the causal effect of being young in cohort in secondary school (YINC) on the likelihood to commit crime (extensive margin) and on the likelihood to receive school disciplinary absences (SDAs, extensive margin) by age. All estimates are obtained from IV regression specifications, which include distance to the 1st January cut-off, pre-cut-off indicator (Nov-Dec = 1), their interaction term and discontinuity fixed effects. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Crime offences include property damage and theft, drugs, violence and other crimes by offender in a year. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. Other crimes includes burglary, dangerous or negligent acts endangering persons, blackmail, fraud, deception and related offences, robbery, weapon offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations offences, and miscellaneous offences by offender in a year. SDAs include property misconduct SDAs, illicit substance SDAs, physical misconduct SDAs and other SDAs received per youth in a year. Other SDAs include absence SDAs, licit substance SDAs, persistently disruptive behaviour SDAs, refusal to participate SDAs, verbal SDAs and other misconduct SDAs.

Figure A.3. IV Estimates of YINC Impact on Crime and SDAs by Age at the Extensive and Intensive Margin.



Notes: Figure A.3 shows point estimates and 95 percent confidence intervals of the causal effect of being young in cohort in secondary school (YINC) on the count of crime offences (extensive and intensive margin) and on the count of school disciplinary absences (SDAs, extensive and intensive margin) by age. All estimates are obtained from IV regression specifications, which include distance to the 1st January cut-off, pre-cut-off indicator (Nov-Dec = 1), their interaction term and discontinuity fixed effects. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Crime offences include property damage and theft, drugs, violence and other crimes by offender in a year. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. Other crimes include burglary, dangerous or negligent acts endangering persons, blackmail, fraud, deception and related offences, robbery, weapon offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations offences, and miscellaneous offences by offender in a year. SDAs include property misconduct SDAs, illicit substance SDAs, physical misconduct SDAs and other SDAs received per youth in a year. Other SDAs include absence SDAs, licit substance SDAs, persistently disruptive behaviour SDAs, refusal to participate SDAs, verbal SDAs and other misconduct SDAs.

Table A.1. Means of Selected Crime Outcomes by Types of Crime.

	Crime Committed (Extensive Margin)					Crime Count (Extensive and Intensive Margin)					
Age:	All Crimes	Property Crime	Drugs Crime	Violence Crime	Other Crimes	All Crimes	Property Crime	Drugs Crime	Violence Crime	Other Crimes	No. Youths
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
15	0.032	0.023	0.006	0.007	0.007	0.096	0.059	0.009	0.010	0.019	203355
16	0.034	0.023	0.008	0.008	0.007	0.101	0.059	0.013	0.011	0.019	202979
17	0.036	0.021	0.012	0.008	0.007	0.098	0.052	0.020	0.011	0.015	185578
18	0.036	0.019	0.015	0.008	0.006	0.094	0.046	0.026	0.010	0.012	168005
19	0.033	0.015	0.015	0.007	0.004	0.079	0.034	0.027	0.009	0.009	149832
20	0.029	0.013	0.015	0.006	0.003	0.065	0.026	0.025	0.008	0.006	131342
21	0.026	0.011	0.014	0.006	0.003	0.056	0.021	0.024	0.007	0.005	112424
22	0.023	0.010	0.012	0.005	0.002	0.052	0.019	0.022	0.006	0.004	93816
23	0.021	0.009	0.011	0.005	0.002	0.047	0.016	0.020	0.006	0.004	75148
24	0.020	0.008	0.011	0.005	0.002	0.045	0.015	0.021	0.006	0.003	57330
Overall	0.031	0.017	0.011	0.007	0.005	0.081	0.041	0.020	0.009	0.012	203355

Notes: Table A.1 shows means of selected crime outcomes by types of crime. All crimes includes property damage and theft, drugs, violence and other crimes by offender in a year. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. Other crimes include burglary, dangerous or negligent acts endangering persons, blackmail, fraud, deception and related offences, robbery, weapon offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations offences, and miscellaneous offences by offender in a year.

Table A.2. Means of Selected SDA Outcomes by Types of SDA.

	SDA Received (Extensive Margin)					SDA Count (Extensive and Intensive Margin)					
Age:	All SDAs	Property Misconduct	Illicit Substance	Physical Misconduct	Other SDAs	All SDAs	Property Misconduct	Illicit Substance	Physical Misconduct	Other SDAs	No. Youths
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
15	0.091	0.011	0.003	0.032	0.066	0.153	0.011	0.003	0.036	0.103	203355
16	0.069	0.007	0.002	0.020	0.052	0.103	0.007	0.002	0.022	0.071	202979
17	0.043	0.004	0.001	0.009	0.033	0.057	0.004	0.001	0.010	0.042	185578
Overall	0.069	0.007	0.002	0.021	0.051	0.105	0.008	0.002	0.023	0.073	203355

Notes: Table A.2 shows means of selected school disciplinary absence (SDA) outcomes by types of SDA. All SDAs includes property misconduct SDAs, illicit substance SDAs, physical misconduct SDAs and other SDAs received per youth in a year. Other SDAs include absence SDAs, licit substance SDAs, persistently disruptive behaviour SDAs, refusal to participate SDAs, verbal SDAs and other misconduct SDAs.

Table A.3. First Stage Estimates of ESE Impact on Young in Cohort (YINC) Status.

	YINC	YINC	YINC	YINC
	(1)	(2)	(3)	(4)
	OLS		Local Polynomial	
ESE (Nov-Dec = 1)	0.260***	0.259***	0.262***	0.261***
	(0.005)	(0.005)	(0.006)	(0.006)
No. Youths	203355	203355	203355	203355
Primary School Characteristics	No	Yes	No	Yes
Individual Characteristics	Yes	Yes	Yes	Yes
Discontinuity Fixed Effects	Yes	Yes	Yes	Yes

Notes: Estimates in columns (1) and (2) are obtained from OLS regression specifications. Estimates in columns (3) and (4) are obtained from local linear regression specifications where a triangular kernel with bandwidth of 60 days is used. All specifications include distance to the 1st January cut-off, pre-cut-off indicator (Nov-Dec = 1) and their interaction term. Regressions in columns (1) and (3) also include controls for whether the juvenile is male, whether s/he is a native speaker and whether the native language is unknown. Regressions in columns (2) and (4) also include Primary School Characteristics variables calculated in the final year of primary school. Primary School Characteristics variables included are the School Deprivation Index, the School ICSEA Index, the share of male classmates, the share of English native speaker classmates, the number of classmates and binary indicators for whether the Primary School Deprivation Index is unknown, for whether the Primary School ICSEA Index is unknown, for whether the share of male classmates in the final year of primary school is unknown, for whether the share of English native speaker classmates in the final year of primary school is unknown, and for whether the number of classmates in the final year of primary school is unknown. Robust standard errors are clustered at the date of birth level. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.4. IV Estimates of YINC Impact on Crime.

	Crime Committed (Extensive Margin)			Crime Count (Extensive and Intensive Margin)		
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	IV	IV	IV	IV	IV
YINC	0.013***	0.021***	0.020***	0.052***	0.076***	0.070***
	(0.003)	(0.003)	(0.003)	(0.015)	(0.015)	(0.014)
No. Youths (90 days Bandwidth)	308628	308628	308628	308628	308628	308628
YINC	0.013***	0.021***	0.018***	0.063***	0.084***	0.073***
	(0.004)	(0.003)	(0.003)	(0.020)	(0.019)	(0.018)
No. Youths (60 days Bandwidth)	203355	203355	203355	203355	203355	203355
YINC	0.014***	0.019***	0.017***	0.062***	0.077***	0.063***
	(0.005)	(0.004)	(0.003)	(0.024)	(0.024)	(0.018)
No. Youths (30 days Bandwidth)	101285	101285	101285	101285	101285	101285
Primary School Characteristics	No	Yes	Yes	No	Yes	Yes
Primary School x Discontinuity Fixed Effects	No	No	Yes	No	No	Yes
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Discontinuity Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.4 shows estimates of the causal effect of being young in cohort in secondary school (YINC) on crime. All estimates are obtained from IV regression specifications. All specifications include distance to the 1st January cut-off, pre-cut-off indicator (Nov-Dec = 1) and their interaction term. Robust standard errors (clustered at the date of birth level) are reported in parentheses. Individual Characteristics variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Primary School Characteristics variables included are calculated in the final year of primary school and they are the School Deprivation Index, the School ICSEA Index, the share of male classmates, the share of English native speaker classmates, the number of classmates and a series of binary indicators for whether the Primary School Deprivation Index is unknown, for whether the Primary School ICSEA Index is unknown, for whether the share of male classmates in the final year of primary school is unknown, for whether the share of English native speaker classmates in the final year of primary school is unknown, and for whether the number of classmates in the final year of primary school is unknown. Crime offences include property damage and theft, drugs, violence and other crimes by offender in a year. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. Other crimes include burglary, dangerous or negligent acts endangering persons, blackmail, fraud, deception and related offences, robbery, weapon offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations offences, and miscellaneous offences by offender in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.5. IV Estimates of YINC Impact on SDAs.

	SDA Received (Extensive Margin)			SDA Count (Extensive and Intensive Margin)		
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	IV	IV	IV	IV	IV
YINC	0.043***	0.044***	0.043***	0.104***	0.105***	0.104***
	(0.005)	(0.005)	(0.005)	(0.009)	(0.009)	(0.009)
No. Youths (90 days Bandwidth)	308628	308628	308628	308628	308628	308628
YINC	0.042***	0.043***	0.043***	0.100***	0.101***	0.101***
	(0.007)	(0.007)	(0.006)	(0.011)	(0.011)	(0.011)
No. Youths (60 days Bandwidth)	203355	203355	203355	203355	203355	203355
YINC	0.031***	0.031***	0.028***	0.075***	0.076***	0.072***
	(0.010)	(0.010)	(0.009)	(0.016)	(0.017)	(0.018)
No. Youths (30 days Bandwidth)	101285	101285	101285	101285	101285	101285
Primary School Characteristics	No	Yes	Yes	No	Yes	Yes
Primary School x Discontinuity Fixed Effects	No	No	Yes	No	No	Yes
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Discontinuity Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.5 shows estimates of the causal effect of being young in cohort in secondary school (YINC) on school disciplinary absences (SDAs). All estimates are obtained from IV regression specifications. All specifications include distance to the 1st January cut-off, pre-cut-off indicator (Nov-Dec = 1) and their interaction term. Robust standard errors (clustered at the date of birth level) are reported in parentheses. Individual Characteristics variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Primary School Characteristics variables included are calculated in the final year of primary school and they are the School Deprivation Index, the School ICSEA Index, the share of male classmates, the share of English native speaker classmates, the number of classmates and a series of binary indicators for whether the Primary School Deprivation Index is unknown, for whether the Primary School ICSEA Index is unknown, for whether the share of male classmates in the final year of primary school is unknown, for whether the share of English native speaker classmates in the final year of primary school is unknown, and for whether the number of classmates in the final year of primary school is unknown. SDAs include property misconduct SDAs, illicit substance SDAs, physical misconduct SDAs and other SDAs received per youth in a year. Other SDAs include absence SDAs, licit substance SDAs, persistently disruptive behaviour SDAs, refusal to participate SDAs, verbal SDAs and other misconduct SDAs. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.6. Local Polynomial Estimates of ESE Impact and YINC Impact on Crime and SDAs.

		Crime Committed (Extensive Margin)	Crime Count (Extensive and Intensive Margin)	SDA Received (Extensive Margin)	SDA Count (Extensive and Intensive Margin)
		(1)	(2)	(3)	(4)
(1)	ESE	0.002**	0.013**	0.011***	0.026***
		(0.001)	(0.005)	(0.002)	(0.003)
(2)	YINC	0.010**	0.051**	0.045***	0.104***
		(0.004)	(0.020)	(0.009)	(0.014)
	Mean Dependent Variable	0.031	0.081	0.069	0.105
	No. Youths (60 days Bandwidth)	203355	203355	203355	203355
	Primary School Characteristics	No	No	No	No
	Primary School x Discontinuity Fixed Effects	No	No	No	No
	Individual Characteristics	Yes	Yes	Yes	Yes
	Age Fixed Effects	Yes	Yes	Yes	Yes
	Discontinuity Fixed Effects	Yes	Yes	Yes	Yes

Notes: Row (1) of Table A.6 shows estimates of the causal effect of early school eligibility (ESE) on crime and school disciplinary absences (SDAs) in secondary school. Estimates in Row (1) are obtained from sharp local linear regression specifications, which include distance to the 1st January cut-off, pre-cut-off indicator (Nov-Dec = 1) and their interaction term. Row (2) of Table A.6 shows estimates of the causal effect of being young in cohort in secondary school (YINC) on crime and SDAs in secondary school. Estimates in Row (2) are obtained from fuzzy local linear regression specifications, which include distance to the 1st January cut-off, pre-cut-off indicator (Nov-Dec = 1) and their interaction term. A triangular kernel is used in all cases. Robust standard errors (clustered at the date of birth level) are reported in parentheses. Control variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Crime offences include property damage and theft, drugs, violence and other crimes by offender in a year. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. Other crimes include burglary, dangerous or negligent acts endangering persons, blackmail, fraud, deception and related offences, robbery, weapon offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations offences, and miscellaneous offences by offender in a year. SDAs include property misconduct SDAs, illicit substance SDAs, physical misconduct SDAs and other SDAs received per youth in a year. Other SDAs include absence SDAs, licit substance SDAs, persistently disruptive behaviour SDAs, refusal to participate SDAs, verbal SDAs and other misconduct SDAs. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.7. Robustness Estimates of ESE Impact and YINC Impact on Crime and SDAs.

		Crime Committed (Extensive Margin)	Crime Count (Extensive and Intensive Margin)	SDA Received (Extensive Margin)	SDA Count (Extensive and Intensive Margin)
		(1)	(2)	(3)	(4)
(1)	ESE	0.003**	0.015**	0.005**	0.012***
		(0.002)	(0.007)	(0.002)	(0.004)
(2)	YINC	0.011**	0.052**	0.016**	0.042***
		(0.005)	(0.026)	(0.008)	(0.015)
	Mean Dependent Variable	0.033	0.081	0.060	0.086
	No. Youths (60 days Bandwidth)	57330	57330	57330	57330
	Primary School Characteristics	No	No	No	No
	Primary School x Discontinuity Fixed Effects	No	No	No	No
	Individual Characteristics	Yes	Yes	Yes	Yes
	Age Fixed Effects	Yes	Yes	Yes	Yes
	Discontinuity Fixed Effects	Yes	Yes	Yes	Yes

Notes: Row (1) of Table A.7 shows estimates of the causal effect of early school eligibility (ESE) on crime and school disciplinary absences (SDAs) in secondary school. Estimates in Row (1) are obtained from OLS regression specifications, which include distance to the 1st January cut-off, pre-cut-off indicator (Nov-Dec = 1) and their interaction term. Row (2) of Table A.7 shows estimates of the causal effect of being young in cohort in secondary school (YINC) on crime and SDAs in secondary school. Estimates in Row (2) are obtained from IV regression specifications, which include distance to the 1st January cut-off, pre-cut-off indicator (Nov-Dec = 1) and their interaction term. Robust standard errors (clustered at the date of birth level) are reported in parentheses. Control variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Crime offences include property damage and theft, drugs, violence and other crimes by offender in a year. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. Other crimes include burglary, dangerous or negligent acts endangering persons, blackmail, fraud, deception and related offences, robbery, weapon offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations offences, and miscellaneous offences by offender in a year. SDAs include property misconduct SDAs, illicit substance SDAs, physical misconduct SDAs and other SDAs received per youth in a year. Other SDAs include absence SDAs, licit substance SDAs, persistently disruptive behaviour SDAs, refusal to participate SDAs, verbal SDAs and other misconduct SDAs. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.8. IV Estimates of School Leaving Grade Impact on Crime and SDAs.

	School Leaving Grade	Crime Committed at age 15 to 17	SDA Received at age 15 to 17	Crime Count at age 15 to 17	SDA Count at age 15 to 17	Crime Count at age 15 to 17 + SDA Count at age 15 to 17	
		(Extensive Margin)		(Extensive and Intensive Margin)		(Extensive Margin)	(Extensive and Intensive Margin)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	IV	IV	IV	IV	IV	IV
EL Reform	0.509*** (p = 0.003)						
School Leaving Grade		-0.017*** (p = 0.006)	0.035 (p = 0.128)	-0.058*** (p = 0.006)	0.061 (p = 0.107)	0.014 (p = 0.525)	0.002 (p = 0.929)
Pre EL Reform Mean Dep. Var.	11.414	0.042	0.058	0.114	0.081	0.091	0.195
No. Youths (Full Sample)	293562	293562	293562	293562	293562	293562	293562
EL Reform	0.503*** (p = 0.004)						
School Leaving Grade		-0.012* (p = 0.054)	0.036 (p = 0.146)	-0.042* (p = 0.084)	0.062 (p = 0.125)	0.018 (p = 0.415)	0.019 (p = 0.632)
Pre EL Reform Mean Dep. Var.	11.390	0.040	0.059	0.108	0.084	0.091	0.192
No. Youths (60 days Bandwidth)	93924	93924	93924	93924	93924	93924	93924
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.8 shows estimates of the causal effect of the school leaving grade on crime at age 15 to 17 and school disciplinary absences (SDAs) at age 15 to 17. All estimates are obtained from IV regression specifications. The school leaving grade is instrumented with the Earning or Learning (EL) Reform, and column (1) shows the first stage estimates of the impact of the EL reform on the school leaving grade. Standard errors were clustered at the year of birth level and the p-values from Wild Cluster Bootstrap estimation are reported in parentheses. Control variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Crime offences include property damage and theft, drugs, violence and other crimes by offender in a year. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. Other crimes include burglary, dangerous or negligent acts endangering persons, blackmail, fraud, deception and related offences, robbery, weapon offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations offences, and miscellaneous offences by offender in a year. SDAs include property misconduct SDAs, illicit substance SDAs, physical misconduct SDAs and other SDAs received per youth in a year. Other SDAs include absence SDAs, licit substance SDAs, persistently disruptive behaviour SDAs, refusal to participate SDAs, verbal SDAs and other misconduct SDAs. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.9. IV Estimates of School Leaving Grade Impact on Property Crime and Property SDAs.

	School Leaving Grade	Property Crime Committed	Property SDA Received	Property Crime Count	Property SDA Count	Property Crime Count + Property SDA Count	
		(Extensive Margin)		(Extensive and Intensive Margin)		(Extensive Margin)	(Extensive and Intensive Margin)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	IV	IV	IV	IV	IV	IV
EL Reform	0.509*** (p = 0.003)						
School Leaving Grade		-0.010*** (p = 0.005)	0.005 (p = 0.102)	-0.035*** (p = 0.006)	0.006* (p = 0.097)	-0.006 (p = 0.140)	-0.029*** (p = 0.007)
Pre EL Reform Mean Dep. Var.	11.414	0.028	0.005	0.067	0.005	0.032	0.072
No. Youths (Full Sample)	293562	293562	293562	293562	293562	293562	293562
EL Reform	0.503*** (p = 0.004)						
School Leaving Grade		-0.007 (p = 0.136)	0.005 (p = 0.117)	-0.023 (p = 0.150)	0.006* (p = 0.085)	-0.003 (p = 0.520)	-0.018 (p = 0.250)
Pre EL Reform Mean Dep. Var.	11.390	0.027	0.005	0.063	0.005	0.031	0.068
No. Youths (60 days Bandwidth)	93924	93924	93924	93924	93924	93924	93924
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.9 shows estimates of the causal effect of the school leaving grade on property crime at age 15 to 17 and property misconduct school disciplinary absences (SDAs) at age 15 to 17. All estimates are obtained from IV regression specifications. The school leaving grade is instrumented with the Earning or Learning (EL) Reform, and column (1) shows the first stage estimates of the impact of the EL reform on the school leaving grade. Standard errors were clustered at the year of birth level and the p-values from Wild Cluster Bootstrap estimation are reported in parentheses. Control variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Crime offences include property damage and theft by offender in a year. SDAs include property misconduct SDAs received per youth in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.10. IV Estimates of School Leaving Grade Impact on Drugs Crime and Illicit Substance SDAs.

	School Leaving Grade	Drugs Crime Committed	Illicit Substance SDA Received	Drugs Crime Count	Illicit Substance SDA Count	Drugs Crime Count + Illicit Substance SDA Count	
		(Extensive Margin)		(Extensive and Intensive Margin)		(Extensive Margin)	(Extensive and Intensive Margin)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	IV	IV	IV	IV	IV	IV
EL Reform	0.509***						
	(p = 0.003)						
School Leaving Grade		-0.009***	-0.000	-0.016***	-0.000	-0.010***	-0.017***
		(p = 0.005)	(p = 0.425)	(p = 0.005)	(p = 0.479)	(p = 0.004)	(p = 0.005)
Pre EL Reform Mean Dep. Var.	11.414	0.011	0.002	0.018	0.002	0.013	0.020
No. Youths (Full Sample)	293562	293562	293562	293562	293562	293562	293562
EL Reform	0.503***						
	(p = 0.004)						
School Leaving Grade		-0.008***	-0.001	-0.015***	-0.001	-0.009***	-0.016***
		(p = 0.007)	(p = 0.204)	(p = 0.010)	(p = 0.239)	(p = 0.005)	(p = 0.006)
Pre EL Reform Mean Dep. Var.	11.390	0.010	0.002	0.018	0.002	0.012	0.020
No. Youths (60 days Bandwidth)	93924	93924	93924	93924	93924	93924	93924
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.10 shows estimates of the causal effect of the school leaving grade on drugs crime at age 15 to 17 and illicit substances school disciplinary absences (SDAs) at age 15 to 17. All estimates are obtained from IV regression specifications. The school leaving grade is instrumented with the Earning or Learning (EL) Reform, and column (1) shows the first stage estimates of the impact of the EL reform on the school leaving grade. Standard errors were clustered at the year of birth level and the p-values from Wild Cluster Bootstrap estimation are reported in parentheses. Control variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Crime offences include drugs crimes by offender in a year. SDAs include illicit substances SDAs received per youth in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.11. IV Estimates of School Leaving Grade Impact on Violence Crime and Violence SDAs.

	School Leaving Grade	Violence Crime Committed	Violence SDA Received	Violence Crime Count	Violence SDA Count	Violence Crime Count + Violence SDA Count	
		(Extensive Margin)		(Extensive and Intensive Margin)		(Extensive Margin)	(Extensive and Intensive Margin)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	IV	IV	IV	IV	IV	IV
EL Reform	0.509*** (p = 0.003)						
School Leaving Grade		-0.001 (p = 0.158)	0.013* (p = 0.088)	-0.001 (p = 0.467)	0.015* (p = 0.071)	0.011 (p = 0.169)	0.015 (p = 0.118)
Pre EL Reform Mean Dep. Var.	11.414	0.008	0.017	0.010	0.018	0.024	0.029
No. Youths (Full Sample)	293562	293562	293562	293562	293562	293562	293562
EL Reform	0.503*** (p = 0.004)						
School Leaving Grade		0.000 (p = 0.924)	0.013 (p = 0.109)	0.001 (p = 0.172)	0.016* (p = 0.085)	0.012 (p = 0.141)	0.017* (p = 0.068)
Pre EL Reform Mean Dep. Var.	11.390	0.008	0.017	0.010	0.019	0.024	0.029
No. Youths (60 days Bandwidth)	93924	93924	93924	93924	93924	93924	93924
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.11 shows estimates of the causal effect of the school leaving grade on violence crime at age 15 to 17 and violence school disciplinary absences (SDAs) at age 15 to 17. All estimates are obtained from IV regression specifications. The school leaving grade is instrumented with the Earning or Learning (EL) Reform, and column (1) shows the first stage estimates of the impact of the EL reform on the school leaving grade. Standard errors were clustered at the year of birth level and the p-values from Wild Cluster Bootstrap estimation are reported in parentheses. Control variables included are dummies for whether the youths are male, whether they are native English speakers and whether the native language is unknown. Violence crime includes violence against the person, sexual offences, and public order offences by offender in a year. SDAs include physical misconduct SDAs received per youth in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.