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Age in Cohort, School Indiscipline and Crime: Regression-Discontinuity Estimates for Queensland

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NON-TECHNICAL SUMMARY

Globally, millions of juveniles commit criminal offences every year, imposing considerable costs on society and drawing billions of dollars away from other productive uses. In Australia the police proceeded against almost 55,000 youth offenders from July 2015 to June 2016 (Australian Bureau of Statistics, 2017). As in many other countries, crime rates in Australia are observed to increase steadily until around age 18–20 and then decrease later in life. This paper examines the effect of school starting age on in-school disciplinary sanctions and youth crime. A growing economics literature has started to document that starting school at a younger age can place juveniles at a greater risk of committing crime. This paper contributes to this literature by focusing on indiscipline in school to develop an understanding of how behaviour in school may relate with the propensity to commit crime outside school.

This paper uses administrative records from the Department for Education of Queensland linked at the individual level with administrative records from the Queensland Police. The empirical findings suggest that younger pupils in cohort are more likely to commit criminal offences at all ages from the age of 18 to 24. Prior to reaching age 18, younger pupils in cohort do not appear more likely to commit criminal offences but they appear more likely to receive SDAs in school. Our findings also show that, compared to their older peers, younger pupils in cohort appear more likely to enrol in the final year of secondary school, and they appear equally likely to obtain a good certificate at the end of secondary school. Thus, the greater likelihood to commit crime after secondary school by younger pupils in cohort does not appear to derive from their poorer labour market prospects at the end of secondary school.

To test whether the observed pattern reflects the crime-reducing effect of school, this study exploits the introduction of the Earning or Learning education reform in 2006. This reform increased the minimum school leaving age from 16 to 17 in Queensland. This analysis shows that younger pupils in cohort who were incapacitated in school at age 16 due to the Earning or Learning (2006) reform committed fewer crime offences but, crucially, they received more SDAs in school at age 16. This finding is consistent with a view of crime and SDAs as similar acts and substitutes, and it supports the hypothesis that school incapacitates juveniles and, thus, it decreases their possibility to engage in crime. The increase in the propensity to receive SDAs reflects the fact that, to some extent, incapacitating these juveniles in school may move crime from outside to inside the school premises.

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ABSTRACT

Youth crime involves millions of people each year, imposing extensive costs on society. This paper examines the effect of school starting age on in-school disciplinary sanctions and youth crime. Using administrative data matching education and criminal records for Queensland State secondary school students, the paper exploits school-entry administrative rules to define a regression discontinuity design. Younger pupils in cohort appear to receive more disciplinary sanctions during secondary school and to commit more crime after secondary school. A recent school-leaving age reform is also exploited to show that this crime-age profile is consistent with an incapacitation effect of school on crime.

Keywords: youth crime; age in cohort; school discipline; Australia

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1. Introduction

Millions of juveniles commit criminal offences every year, imposing extensive costs on society and drawing billions of dollars away from other productive uses (Anderson, 1999). Crime rates are observed almost universally to increase steadily until around age 18–20 and then decrease later in life (Landersø, Nielsen and Simonsen, 2016). In the US, arrests of juveniles accounted for one in five of all arrests in recent years (Jacob and Lefgren, 2003). Juveniles are twice as likely as adults to be victims of serious violent crime and three times as likely to be victims of assault (H. Snyder and M. Sickmund, 1999). In Australia, the institutional setting of focus in this paper, the police proceeded against almost 55,000 youth offenders from July 2015 to June 2016. In that year, juveniles accounted for roughly 13 percent of the total offender population, and 35 percent of them were proceeded against for theft offences (Australian Bureau of Statistics, 2017).

This paper examines the effect of school starting age on in-school disciplinary sanctions and youth crime. Courtesy of the administrative rules that govern school entry eligibility in Queensland, children born around the 1st January are subject to a discontinuity in school entry eligibility that can be exploited to define a regression discontinuity design. A growing literature has started to document that starting school at a younger age can place juveniles at a greater risk of committing crime (Cook and Kang, 2016; Depew and Eren, 2016; and Landersø et al., 2016). This paper contributes to the existing literature by focusing on indiscipline in school to develop an understanding of how behaviour in school may relate with the propensity of juveniles to commit crime outside school. Behaviour in school may relate with criminal activity outside school in a number of ways. Firstly, the same personality traits that lead juveniles to engage in youth crime may also induce them to misbehave in school. If this is the case, then one should observe a decrease in crime committed outside school when juveniles are forced into school and an increase in bad behaviour in school in the same years. This pattern would also imply that bad behaviour in school could be used to predict juvenile crime. Secondly, behaviour in school may have a direct effect on the educational output of pupils (Lazear, 2001); better education is likely to increase the stock of

human capital and hence the rewards from work, thus, inducing a substitution away from crime (Becker 1968, Freeman 1999). Although these hypotheses are not mutually exclusive, this paper constitutes an effort to explore their relative roles in explaining of how poor behaviour in school may relate with criminal behaviour at young ages.

A large empirical literature has examined the impact of school starting age on academic performance (among others, Bedard & 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). The consensus is that starting school at an older age leads to a significant academic advantage. There are a number of possible explanations for the academic advantage, which include the role of absolute age of starting school, age at test date and relative age within the cohort. Although, in general, it is not possible to identify the separate effects of the different components, Crawford et. al. (2010) shed some light on the issue as they observe two distinct test scores for the same set of students, one measured for a given grade and the other at a given age. They suggest that age at test is the key determinant of the better performance of older students. Cunha & Heckman (2008) suggest that starting school at a later age may have significant lifetime effects, as they show that early childhood learning has the potential to alter the entire path of skill acquisition.

Unlike these studies, this paper is concerned with behavioural and criminal outcomes. Recent studies by Cook and Kang (2016), Depew and Eren (2016) and Landersø, Nielsen and Simonsen (2016) use administrative data for North Carolina, Louisiana and Denmark respectively to provide convincing evidence in support of a negative link between school starting age and juvenile crime[1]. This paper follows in the same tradition using matched administrative data on education and crime for the State of Queensland, Australia. It adopts a similar empirical strategy, as it uses the administratively determined school enrolment cut-off date, i.e. starting secondary school

[1] In a related study, McAdams (2016) uses microdata from the U.S. Census to show that a higher school starting age threshold reduces incarceration rates among both those directly affected by the laws and those only indirectly affected.

in the calendar year the child turns 13, as an instrument for secondary school starting age. It focuses on individuals born around the 1st January and adopts a fuzzy regression discontinuity approach (Imbens & Lemieux, 2008; Lee & Lemieux, 2010; Calonico, Cattaneo & Titiunik, 2014) which considers parental over-riding of administrative rules.

The paper has a number of novel features. In the first part of the analysis, a similar exercise to Cook and Kang (2016) and Landersø et al (2016) is conducted, as the impact of being younger in cohort on the likelihood to commit crime until the age of 24 is analysed. Unlike previous studies, this paper examines an important additional dimension, namely the pattern of receipt of school disciplinary sanctions by age in cohort. This allows us to examine both the extent to which being younger in cohort affects the level of juvenile crime outside school, and the extent to which it influences student misbehaviour in school. This paper uses administrative records of school disciplinary absences (SDAs) as a measure of behaviour in school. The empirical findings suggest that younger pupils in cohort are more likely to commit criminal offences at all ages from the age of 18 to 24. Prior to reaching age 18, younger pupils in cohort do not appear more likely to commit criminal offences but they appear more likely to receive SDAs in school.

Although this pattern appears consistent with an incapacitation effect of school on crime, as younger pupils in cohort do not commit more crime until the age of 18, i.e., beyond the age of compulsory schooling, the observed crime-age profile may simply reflect an age effect. Regardless of whether they attend school or not, younger pupils in cohort may become more likely to engage in crime as they grow older. In this scenario, SDAs in secondary school may be viewed as a form of "soft juvenile crime", which precedes later engagement in youth crime. Another, non-competing, possibility is that poorer labour market prospects towards the end of secondary school may induce younger pupils in cohort to commit crime after the end of secondary school. In the second part of our study the relative importance of these potential explanations for the observed crime-age profile is explored.

Our findings show that, compared to their older peers, younger pupils in cohort appear more likely to enrol in the final year of secondary school, and they appear equally likely to obtain a good certificate at the end of secondary school. Thus, the greater likelihood to commit crime after secondary school by younger pupils in cohort does not appear to derive from their poorer labour market prospects at the end of secondary school. These findings are consistent with Cook and Kang (2016) that find older pupils in cohort in North Carolina to be more likely to drop out of secondary school as soon as they reach the minimum school leaving age. Since the laws in North Carolina governing school leaving behaviour specify a minimum age requirement for dropping out, rather than a minimum grade attainment, older pupils in cohort have almost one more year to contemplate dropout prior to graduation (Cook and Kang, 2016). The consistency between their results and the findings of this paper are best explained by the fact that the school leaving rules in North Carolina and in Queensland are specified in the same way.

To test whether the observed pattern reflects an age effect or a crime-reducing effect of school, this study exploits the introduction of the Earning or Learning education reform in 2006. This reform increased the minimum school leaving age from 16 to 17 in Queensland. Thus, this study tests the effects of the Earning or Learning (2006) reform on crime and SDAs at the age of 16. Evidence of a reduction in crime at this age among the cohorts exposed to this reform would be consistent with an incapacitation effect of school on crime. A large body of empirical evidence has documented the role that policies leading to increased years of compulsory education can play to reduce crime, particularly for males (Lochner and Moretti 2004; Machin, Marie, and Vujic 2011; Anderson 2014; Gilpin and Pennig, 2015; Hjalmarsson, Holmlund, and Lindquist 2015; and Beatton et. al., 2017). A number of studies has also examined the incapacitation effect of school on the day-to-day propensity and desire to commit crime in the short run (Jacob and Lefgren 2003; Luallen 2006).

Our findings show that younger pupils in cohort who were incapacitated in school at age 16 due to the Earning or Learning (2006) reform committed fewer crime offences but, crucially, they

received more SDAs in school at age 16. This finding is consistent with a view of crime and SDAs as similar acts and substitutes, and it supports the hypothesis that school incapacitates juveniles and, thus, it decreases their possibility to engage in crime. The increase in the propensity to receive SDAs reflects the fact that, to some extent, incapacitating these juveniles in school may move crime from outside to inside the school premises. This is in fact a possibility that has been raised in the existing literature (Gilpin and Pennig, 2015). However, a set of simulation exercises in which similar criminal offences and similar SDAs are added and modelled together suggests that incapacitating pupils in cohort has a net crime-reducing effect, i.e., even after accounting for the increase in SDAs in school. Starting from the observed level of SDAs at age 16, the count of SDAs at age 16 should increase by more than 70 percent for the incapacitation effect of school on crime to be completely wiped out.

The remainder of the paper is structured as follows. Section 2 provides some institutional details of the State of Queensland education system. Section 3 outlines the empirical strategy underlying the analysis and Section 4 describes the data. Section 5 presents the main econometric results and Section 6 provides further discussion. Section 7 concludes.

2. The Queensland State School System

In Queensland, the educational setting of this study, approximately three quarters of the universe of students attend the state-run school sector that is funded by the Queensland State and Federal Australian Government (QGOV, 2018a). The remainder of students attend private schools or migrate out of Queensland. In the Queensland public school system, pupils attend 12 years of education (grades 1 to 12), with primary school consisting of grades 1 to 7 and high school consisting of grades 8 to 12. At the end of secondary school, pupils are expected to sit high-stakes exams in order to obtain an Overall Position (OP) certificate. The OP score indicates a student's position in a state-wide rank order based on their overall achievement in high school subjects at the end of grade 12. The score in the OP certificate indicates how well a student has done in

comparison to all other OP-eligible students in Queensland. OP certificates are not mandatory to complete secondary school, but they are required in order to access university. OPs range from 1 (top 2% of students) to 25 and acceptable cut-offs for university entry are OP scores ranging from 1 to 13[2].

The school year in Queensland runs from the third week of January to mid-December. Administrative rules imply that pupils are expected to start school from the third week of January of the calendar year in which they turn 6 years old (QGOV, 2018c). This is crucial in the ensuing empirical analysis as it implies that children born one day apart, i.e. December 31st and January 1st, are expected to start school one year apart. This institutional feature of the education system in Queensland is exploited here to define our treatment and control groups and compare their respective criminal and behavioural outcomes.

To measure behaviour, this paper uses school disciplinary absences (SDAs), a set of actions that are available to schools in Queensland to restore discipline in school. Queensland State School Principals can use SDAs in response to breaches of school rules and unacceptable school behaviour. Reasons for SDAs include truancy and, more generally, unjustified absences from school; poor conduct arising from persistent disruptive behaviour (typically disrupting others in their class); physical misconduct, such as fighting at school; property misconduct, such as destruction of school property or the property of others on the school premises; verbal and non-verbal misconduct (swearing and inappropriate gestures); refusal to participate in the school programme; substance misconduct involving legal (cigarettes/drinking alcohol) or illegal (drugs) substances; and other residual types of misconduct (QGOV, 2018b). In any one year, principals can discipline students with multiple short exclusions of 1 to 10 days or with multiple long exclusions of 10 to 20 days. In rare cases involving extreme and repeated bad behaviour a principal may expel a student from school.

[2] Refer to QCAA, 2018a, 2018b for a detailed explanation of OP values and QUT, 2017 for example OP requirements for a typical Queensland university.

Regarding the minimum school-leaving age, the law in Queensland currently mandates that a young person must attend school up to the age of 17. However, during our study period, a change in the minimum school leaving age occurred. Prior to 2006, students in Queensland were required to attend school until either completing grade 10 or turning 16, whichever occurred first; this is referred to as the *compulsory school phase*. The 2006 Earning or Learning reform (ACARA, 2009) introduced a *compulsory participation obligation* which mandated that young people 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. Thus, the new compulsory participation phase required juveniles to either stay on at school until obtaining a high school Senior Certificate[3]; complete a vocational education Certificate III[4]; or participate in paid employment for at least 25 hours per week until turning age 17. This change in the legislation governing school leaving behaviour is exploited here in the empirical analysis.

3. Empirical Strategy

In Queensland, education is compulsory from the calendar year in which a child reaches the age of six until the day in which a pupil reaches the minimum school leaving age. Until 2006, the minimum school leaving age in Queensland was 16. Starting from 2006, as explained above, the Earning or Learning reform modified this, as pupils became legally bound either to be in school or to have a full-time job until the day they reach the age of 17. This education system is comparable to the ones in the US and UK.

Parents and administrators have a substantial degree of autonomy in the enrolment decision i.e. the age at which a child starts school. Pupils can be held back one year if parents and administrators deem it appropriate and, thus, the school starting age is not random and is most plausibly correlated with cognitive and non-cognitive factors likely to affect later behavioural and

[3] A Senior Certificate is awarded after an individual has completed grade 12.
[4] A Certificate III is a level three vocational qualification gained either at a high school or at a vocational training college.

criminal outcomes. For example, the school readiness of pupils as well as their behaviour in the pre-school years can (and often do) influence the decision of parents and administrators to hold back a pupil for one school year. The reviewed literature found that starting school later has a positive causal effect on school test scores, thus increasing the consumption value of education and allowing for a greater array of educational choices.

Due to the extensive leeway allowed in Queensland in relation to the school starting age decision, our empirical analysis exploits the fact that the formal age at school start is defined by the year of birth. Therefore, children born either side of the 1st January cutoff are subject to a one-year difference in timing of administratively determined school start, even though they were born only a few days apart in time. Although parents and administrators can decide to alter the school starting age of a child, the 1st January cutoff can be thought of as placing an administrative incentive to parents and administrators to enroll in school two children born either side of the 1st January cutoff in two different school years. As a result, at a given 1st January cutoff, children born in the final months of the calendar year (i.e., November-December) will likely start school one year earlier than their counterpart born in January-February. In secondary school, children born before the January cutoff will be more likely to start high school aged 12, i.e., in the calendar year in which they turn 13 years old but almost one year before their 13th birthday. In contrast, children born after the January cutoff will be more likely to start secondary school aged 13, i.e., with the starting date of secondary school roughly coinciding with their 13th birthday.

Figure 1 illustrates that the secondary school starting age in our sample is in fact reduced to a binary variable, as pupils either start secondary school at age 12 or they start secondary school at age 13. We label 'younger-in-cohort' children born around the cutoff date that start secondary school roughly one year prior to their 13th birthday. Figure 2 shows the fraction of pupils who are 'younger-in-cohort' by date of birth for all the pupils in our sample born within 45 days either side of the 1st January cutoff. Figure 2 shows pupils born before the 1st January to be much more likely to be classified as 'younger-in-cohort'. In contrast, hardly any of the pupils born from the 1st

January to the 14th February start secondary school one year prior to their 13th birthday. Figure 2 shows a smooth downward trend in the likelihood of being classified as younger-in-cohort, followed by a large discontinuity around the 1st January.

We exploit this institutional feature of the school system in Queensland to estimate the effect of being younger in cohort (YIC) in secondary school on crime outcomes by the age of 24 and SDAs in secondary school. Formally, our equation of interest can be expressed as follows:

$$Y_{it} = \alpha + \beta * YIC_i + \gamma_t + X_i\delta + u_i, \quad (1)$$

where Y is the outcome of interest for pupil i in year t, X is a vector of individual observable characteristics, γ is a year fixed effect and u is the error term. Since, as explained above, considerable autonomy is granted to parents and administrators in the decision to hold pupils back one year, the relative age in cohort in secondary school of pupil i is not random. Rather, it is likely to be correlated with cognitive and non-cognitive factors and to have a direct and independent effect on Y. In (1), this implies that being younger in cohort (*YIC*) in secondary school is likely to be correlated with u, and, thus, that the OLS would lead to a biased estimate of the causal impact of *YIC* on Y. To circumvent this problem, we exploit the administrative burden imposed by the school-starting age rule in Queensland on pupils born before the 1st January cutoff if they wish to delay school entry. Although, as Figure 1 shows, a considerable fraction of pupils born before the 1st January cutoff are held back one year, Figure 2 shows that pupils born prior to the 1st January cutoff are much more likely to be younger-in-cohort. Focusing on pupils born 45 days either side of the cutoff, we can therefore instrument *YIC* with a binary Intention-To-Treat (*ITT*) variable that takes up value 1 for individuals born immediately prior to the 1st January, and value 0 for individuals born just after new year. Insofar as this cutoff date is uncorrelated with unobserved characteristics of pupils, this cutoff constitutes a valid instrument that is as good as randomly allocated among pupils born only a few days either side of the 1st January.

Since no pupils in our data start secondary school more than one year before or after the date at which they are supposed to start, our instrument monotonically increases the likelihood of being younger in cohort in secondary school. Thus, the monotonicity assumption is satisfied in the current context. This is necessary in order to be able to estimate the local average treatment effect of interest, i.e., the average effect of being younger-in-cohort for the group of pupils who would be inclined to decrease their relative age in cohort in secondary school just because they were born prior to the 1st January and not afterwards.

In practice, we include in our analysis a short bandwidth with pupils born +/- 45 days either side of the 1st January. In our main specification, we model *YIC* as a binary variable indicating whether pupil *i* started secondary school almost one year prior to her 13th birthday (i.e., $\mathrm{YIC} = 1$) or started in proximity to her 13th birthday (i.e., $\mathrm{YIC} = 0$). We instrument *YIC* with a binary *ITT* variable that takes a value of 1 if pupil *i* was born within 45 days prior to the 1st January, and value 0 if pupil *i* was born within 45 days after the 1st January. We exclude from our analysis pupils born in the remainder of the calendar year.

4. Data

We use Queensland State Government administrative data matched at the individual level from two agencies, the Department of Education and Training (DET) and the Queensland Police Service (QPS). Thus, we have 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 2002 to 2014[5].

The crime data refers to alleged criminal offences in a given year, and the focus is on whether a 14 to 24 year old individual in a given year is an alleged offender. An alleged offender is a person who has allegedly committed a crime and has been processed for that offence by arrest,

[5] Table A.1 in the appendix provides descriptive statistics of the population of juveniles in the public school system in Queensland.

caution or warrant of apprehension. We matched these data at individual level to the Queensland schooling data for every year from 2002 to 2014. In the latter we observe individuals until they complete their compulsory education. Thus a major advantage of our data relative to that employed by previous literature is that we can match education and criminal offence data at the individual level. We follow the same individual through the state education system, simultaneously tracking criminal offences.

The richness of the school administration data allows us to examine whether an individual pupil in a given class and school received an SDA in a given school year, the number of SDAs a pupil received in a given year, and the reason for each SDA. Availability of data on exact birth dates and on the class and school attended in every secondary school year allow us to identify our set of treatment and control pupils who were born within 45 days either side of the 1st January cut-off. Thus, we are able to look at whether pupils who were 'younger-in-cohort' when they started secondary school committed more criminal offences by the age of 24 and received more SDAs during secondary school from the age of 14 to 17.

In our analysis, we focus on pupils born +/- 45 days either side of the 1st January cut-off; we also show robustness of our results using different bandwidths of 60 days and 30 days either side of the 1st January window. Since student mobility in and out of the state school system is unlikely to be random, the characteristics of students entering or leaving the Queensland school system may be systematically different from those of the individuals initially enrolled and staying in the system. Therefore, like Cook and Kang (2016), we restrict our analysis to individuals observed continuously from the start of secondary school until the year before they reach the minimum school leaving age.

We align individuals in terms of age and we consider an array of age-specific crime measures and SDAs. In our analysis of crime outcomes, our measure of interest captures whether pupil *i* has been charged with a criminal offence at a given age from the age of 14 to 24. We also study separately the effect of being 'younger-in-cohort' on the count of criminal offences committed by pupil *i* at a given age. As our data contains information on the reason why an

individual was charged with a criminal offence, we can distinguish between different types of crime by offender at a given age. Our main outcome of interest is a general measure of criminal offences which includes property offences, drug offences and violent offences by offender in a year. We also study separately the effect of being younger-in-cohort on property offences, which include burglary, theft and handling of stolen goods, and criminal damage; on theft offences; on drug offences; on violent offences, which include violence against the person, sexual offences and robbery; and other offences, which is a residual category that broadly includes dangerous or negligent acts endangering persons, blackmailing, fraud, deception and related offences, weapon offences, public order offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations, and miscellaneous offences.

In our analysis of SDAs, our outcome measures whether pupil *i* has been charged with an SDA in secondary school at a given age from the age of 14 to 17. We also look separately at the count of SDAs received by pupil *i* in secondary school at a given age. The richness of our data allows us to study the effect of being younger in cohort on all SDAs, as well as separately on different categories of SDA. We look separately at 'Absence SDAs', which include SDAs due to truancy; 'Conduct SDAs', which include SDAs due to persistent disruptive behaviour, physical misconduct, property misconduct and other misconduct; 'Verbal SDAs', which include verbal and non-verbal misconduct; 'Non-Participate SDAs', which include SDAs due to refusal to participate in the school programme; and 'Substance SDAs', which include SDAs due to substance misconduct involving legal or illegal substances.

Tables 1 and 2 show the means of our selected outcome variables by type of crime and SDA for our sample of 88,078 pupils born within 45 days either side of the 1st January cut-off and that we could follow continuously from the start of secondary school until the year before they reach the minimum school leaving age. Both our measures of crime and SDAs are memoryless outcomes that simply inform about the tendency to engage in crime or behave poorly in school at any given age. Thus, these outcome measures are especially suitable for investigating changes in the crime-age and

SDA-age profiles caused by the secondary school starting age. For every crime category, Table 1 shows both the average of pupils that were charged with a criminal offence by crime category at a given age (in columns (1), (3), (5), (7), (9) and (11)), and the average count of criminal offences by pupil at a given age for that crime category (in columns (2), (4), (6), (8), (10) and (12)). Table 2 follows precisely the same structure for SDAs.

Table 1 shows that, for pupils in state-maintained schools in Queensland, the likelihood of being charged with a criminal offence increases with age until the age of 18, whereas it starts falling from the age of 19 and it almost halves by the age of 24. The average count of crime offences per individual also peaks in the late teens and falls monotonically in the early twenties. These patterns appear homogeneous across different crime categories. Table 2 suggests that SDAs are most common in secondary school at the age of 14, and they decrease steadily with age. This holds true whether we look at the incidence of SDAs by age or whether we look at the average count of SDAs by pupil at a given age. The breakdown by type of SDA reveals this result to be driven by conduct SDAs, which include SDAs due to persistent disruptive behaviour, physical misconduct, property misconduct and other misconduct. Other categories of SDAs appear less common in our sample of pupils, both at the extensive and at the intensive margin.

5. Results

Timing of Birth and Age in Cohort

A starting point in our empirical analysis is to test the power of our instrumental variable to predict the likelihood of being younger in cohort in secondary school. Table 3 shows our first-stage results. Our measure of being younger in cohort was regressed on a binary indicator that takes up value 1 for pupils born within 45 days prior to the 1st January cut-off, and value 0 for pupils born within 45 days after the 1st January cut-off. In columns (1) and (2) a linear probability model was estimated, whereas columns (3) and (4) report results from a local linear regression specification where a triangular kernel was used. All estimated specifications include a control of distance in birthdays to

the 1st January cut-off, its interaction with the pre-cut-off indicator and cohort-specific fixed effects (i.e., fixed effects for pupils born in December 1988-January 1989, December 1989-January 1990 etc.). Specifications in columns (2) and (4) also included a parsimonious set of controls for whether individual *i* is a male, whether s/he is a native English speaker, and an index of financial resources of the primary school attended by individual *i* (ICSEA index). The ICSEA index is treated as a proxy for the family background of individual *i*.

Consistent with Figure 2, Table 3 suggests that our instrument strongly predicts the likelihood of being younger in cohort in secondary school. Regardless of whether a parametric or a non-parametric specification is chosen, and whether or not background characteristics are included in the specification, children born just prior to the 1st January are much more likely to be younger in cohort compared to their counterparts born just after the 1st January. This holds true despite the considerable leeway granted to parents and administrators in Queensland to defer school enrolment when pupils become eligible to start school. Figure 1 reveals a discontinuous jump on the 1st January in the likelihood of starting secondary school punctually. Visual inspection suggests that the magnitude of the discontinuity is very similar to the estimated coefficients in Table 3. The reported robust standard errors, clustered at the birthday level in all specifications, confirm that our instrument is highly significant and easily passes the Staiger-Stock rule-of-thumb.

Table 4 shows a set of balancing tests of the distribution of our parsimonious set of covariates across the 1st January cut-off. In each case, each individual covariate was regressed on the pre-cut-off indicator, a control of distance in birthday to the 1st January cut-off, its interaction with the pre-cut-off indicator, cohort-specific fixed effects and other controls. Results from an OLS specification are shown in columns (1) to (3), and results from a local linear regression specification are shown in columns (4) to (6). Both estimation methods reach very similar conclusions, as they suggest males, native speakers and ICSEA indices to be uniformly distributed across the 1st January discontinuity.

Figure 3 shows a density test of the distribution of our observations across the 1st January cut-off. If individuals were disproportionately distributed on one side or the other of the discontinuity, this would raise concerns regarding the validity of our experiment. If, for example, parents planned births to have children just after the 1st January, we would expect a disproportionate number of individuals born just after the 1st January. This, in turn, would potentially invalidate our experiment. However, this is not what is found. Birthdays in our sample appear to be uniformly distributed either side of the 1st January, as very similar densities of births are found either side of the window. The null hypothesis of equality in the densities either side of the 1st January could not be rejected at the 10 percent level, based on the main local linear regression specification with a 45-day bandwidth and triangular kernel, and whether conventional or bias-robust confidence intervals were used.

Crime and SDA Results: Fuzzy Regression Discontinuity Estimates

Table 5 shows our estimates of the causal effect of being younger in cohort in secondary school on crime and SDAs. All estimates are obtained from OLS regression specifications, which include distance in birthdays to the 1st January cut-off, a pre-cut-off indicator (Nov-Dec = 1), their interaction term, age fixed effects and cohort fixed effects. Robust standard errors were clustered at the birthday level in all cases. Control variables included are dummies for whether individuals are male, whether they are native English speakers and the ICSEA index of the primary school attended by individual *i*[6]. In Table 5, results are reported for our general measure of crime offences, which includes property offences, drug offences and violent offences by offender at time *t*. Results are also reported in Table 5 for our general measure of SDAs, which includes all SDAs by pupil at time *t*.

[6] Results from OLS regression are discussed here and in the rest of the paper. However, all results were also estimated using a local linear regression with a triangular kernel, and these results are reported in the appendix. The choice between parametric and non parametric estimation methods turned out to be irrelevant for the conclusions of our empirical analysis.

For all outcomes, results are shown using our preferred 45-day bandwidth, as well as 60-day and 30-day bandwidths either side of the discontinuity.

The results in Table 5 clearly suggest that younger pupils in cohort in secondary school are much more likely to engage in youth crime by the age of 24. This holds true whether the incidence of crime at time *t* or the count of criminal offences per individual at time *t* is modelled. This suggests that the estimated effect is not driven by a few serial youth criminals, but also induces criminality at the extensive margin. Younger pupils in cohort in secondary school are 1.5 percentage points more likely to engage in crime by the age of 24. The strong positive effect of being younger in cohort on crime is not affected by the choice of the bandwidth in our analysis. Table 5 also shows the effect of being younger in cohort on the count of SDAs received at time *t*, as well as on the likelihood of receiving any SDAs at all at time *t*. Looking at our preferred specification, a positive and significant effect appears when the count of SDAs per pupil is modelled as our dependent variable. The effect of being younger in cohort on the likelihood to receive any SDAs at all does not appear statistically significant, although the effect does appear strongly significant when a 60-day bandwidth is used either sides of the cut-off. Given that, also in this case, using different bandwidths does not change numerically the size of our estimated coefficients, our interpretation of the results in Table 5 is that younger pupils in cohort appear more likely to receive SDAs while they are in secondary school.

A central interest of the paper is to derive a crime-age profile for young individuals similar to the profile in Landersø et al (2016). The richness of our data also allows us to draw an SDA-age profile. To our knowledge, this has not been done previously in the existing literature. Table 5 suggests that examining the pattern of SDAs may aid in understanding why younger pupils in cohort engage in more crime by the age of 24. To this end, Figures 4 and 5 break down crime offences and SDAs respectively by age and type. Equation (1) was estimated separately at each age and for each outcome of interest, and estimates were obtained from 2SLS regression specifications, which again included distance to the 1st January cut-off, a pre-cut-off indicator (Nov-Dec = 1), their

interaction term, cohort fixed effects and controls for background characteristics. Robust standard errors were clustered again at the date of birth level in all cases.

Figure 4 shows the effect of being younger in cohort on crime outcomes by crime type and age. Interestingly, younger pupils in cohort at the age of 14 appear significantly less likely to engage in crime. Starting from the age of 15, no significant discrepancy appears in the propensity to commit crime between younger and older pupils in cohort in the years of compulsory schooling. However, a steep increase in the likelihood of committing crime for younger pupils in cohort appears at the age of 18, i.e., post-compulsory schooling age. Younger pupils in cohort remain significantly more likely to commit crime at all ages until the age of 24. The breakdown of crime outcomes by crime category in the remainder of Figure 4 reveals the effect at ages 18 to 21 to be mostly driven by property crime, theft and drug offences. Starting from age 22, the greater likelihood of younger pupils in cohort to commit crime seems to be mostly driven by their greater propensity to engage in other residual types of crime, e.g., driving offences.

Figure 5 shows the results of a similar exercise for SDAs in secondary school. Younger pupils in cohort appear less likely to receive SDAs at the age of 14, a result that echoes the result for crime at age 14 in Figure 4. However, by the age of 15 and for the rest of secondary schooling, they appear significantly more likely to receive SDAs at school. The breakdown of SDAs by type reveals this effect to be driven by what we label 'misbehaviour SDAs' and 'verbal SDAs' respectively, where the former include SDAs due to persistent disruptive behaviour, physical misconduct, property misconduct and other misconduct by the pupil, while the latter include verbal and non-verbal misconduct.[7]

[7] The results examining heterogeneity in our estimates by gender, socio-economic status and ethnic status are included in the appendix. Results where estimated both using OLS and using a local linear regression with a triangular kernel.

6. Discussion

The results in Figures 4 and 5 reveal an interesting pattern. While younger kids in cohort at the age of 14 are less likely to engage in youth crime and to receive SDAs at school, by the age of 15 their propensity to commit crime is aligned with that of their older peers in class, and their propensity to receive SDAs in school is significantly greater than their older peers. Younger pupils in cohort continue to be more likely than older pupils in cohort to receive SDAs in school and are equally likely to commit crime outside school until the end of compulsory schooling. As they turn 18, when they are no longer legally bound to attend school, they become significantly more likely to engage in criminal activity, an effect that persists at all ages until the age of 24.

A primary interest of this paper is to develop an understanding of the features of the educational experience of these juveniles that may help explain why this is the case. As stated in the introduction, bad behaviour in school may relate with future criminal behaviour in multiple ways. Bad behaviour in school may impact on the quality of education (Lazear, 2001), thus worsening the labour market prospects of these pupils. A Becker type explanation of the results in Figure 4 would in fact attribute the decision to engage in crime after the end of compulsory schooling to the poor labour market prospects of younger pupils in cohort. A second hypothesis is that the SDA-crime-age profile documented in Figures 4 and 5 may reveal an incapacitation effect of school on crime that leads younger pupils in cohort to behave poorly in school without committing more crime outside school. A third hypothesis is that the SDA-crime-age profile documented in Figures 4 and 5 may reveal instead a simple age effect, showing that, as juveniles become older, they switch from bad behaviour in school, which can be thought of as a form of 'soft crime', to more serious crime outside school. In the remainder of the paper, we test these hypotheses and discuss our findings.

Poor labour market prospects

A large literature has documented that the school starting age has an important effect on test scores at school and, later in life, on labour market outcomes. As discussed above, this literature generally

finds older pupils in cohort to outperform younger pupils in cohort. This paper examines whether younger pupils in cohort face less favourable labour market prospects at the end of secondary school by estimating the impact of being younger in cohort on two outcomes of interest. Namely, the likelihood of dropping out of school prior to Year 12, which is the final year of secondary school, and the likelihood of leaving secondary school with a good certificate. At the end of Year 12, pupils in Queensland sit OP exams, i.e. standardised tests which determine university entry. In our analysis, a good certificate is defined as obtaining an OP award with a test score that is at least equal to the score required to enter the two major universities of Queensland, namely the University of Queensland and the Queensland University of Technology. If the greater likelihood of younger pupils in cohort to engage in criminal activities is motivated by their inability to thrive in the labour market, one would expect an increased tendency to drop out of school prior to completion of secondary school, and to be less likely to obtain a valid certificate to enter university.

Table 6 examines evidence of younger pupils in cohort experiencing poorer labour market prospects at the end of secondary school. Results shown conflict with this explanation, as younger pupils in cohort appear significantly less likely to drop out of school before Year 12, and equally likely to obtain a good OP certificate *vis a vis* their older peers. The choice of the bandwidth does not affect our conclusions. The lower likelihood of younger pupils in cohort to drop out of school is consistent with the results in Cook and Kang (2016) for North Carolina. This result is best explained by the fact that older pupils in cohort reach the age of 17 at the beginning of year 12, whereas younger pupils in cohort, born almost one year later, reach the age of 17 towards the end of year 12. Although, as shown in Table 6, younger pupils in cohort are more likely to experience grade retention at some point in secondary school, nevertheless they appear more likely to reach the end of compulsory schooling. The results in Table 6 provide no empirical support for the notion that

poor labour market prospects lie behind the greater likelihood of younger pupils in cohort to engage in crime[8].

Incapacitation effect of school on crime

Figures 4 and 5 document a greater likelihood of younger pupils in cohort to receive SDAs during secondary school, and to commit crime outside school starting from age 18, i.e., after completion of year 12. One hypothesis is that these patterns might simply reveal an age effect: as juveniles become older, regardless of whether they attend school or not, they may switch from bad behaviour in school to more serious crime outside school. An alternative explanation is that younger pupils in cohort may be incapacitated in school during secondary school. In other words, they may be unable to commit more crime outside school because they have to attend school, while being more likely to behave poorly within the school premises and, thus, receive an SDA. The logical corollary of this is that SDAs and crime are very similar acts, that occur inside and outside the school premises respectively, but that share similar features and originate from similar characteristics of individuals. Indeed, a behavioural model in which, at any time *t*, crime outside school and SDAs within school are substitutes would be consistent with the observed patterns in Figures 4 and 5.

We test the substitutability between crime outside school and SDAs exploiting the Earning or Learning Reform in 2006. As explained above, this reform raised the minimum school leaving age from 16 to 17. This additional year of compulsory education either had to be spent in school, VET or in a full-time job. Since the first cohort affected by the reform was born in 1990, all the available data for the cohorts of juveniles that were born from 1986 to 1993 within 45 days either side of the 1st January cut-off was used for this exercise. Since our data starts in 2002, we are unable to restrict this analysis to pupils which we can follow from the beginning of grade 8 onwards, as this would result in the loss of all the cohorts born prior to 1989. However, we do not regard this as

[8] Also in this case, sensitivity of the results by gender, indigenous status and socio-economic class are provided in the appendix. Results where estimated both using OLS and using a local linear regression with a triangular kernel.

an issue because, in this section, our focus is simply to evaluate whether the school leaving age reform in 2006, which incapacitated kids at the age of 16, resulted in lower crime and greater SDAs at the age of 16. In formal terms, for this analysis we estimated the following equation:

$$Y_{i16} = \alpha + \beta_1 * ITT_i + \beta_2 * EL + \beta_3 ITT * EL + \gamma_t + X_i\delta + e_i, \quad (2)$$

where *Y* is crime or SDA at the age of 16. *ITT* is the instrumental variable used to instrument *YIC* in (1), and is a binary variable that takes up value 1 for pupils born just before the 1st January, and value 0 for pupils born just after the 1st January. *EL* takes up value 1 for pupils born in 1990-93 who were subject to the Earning or Learning Reform in 2006, and value 0 for pupils born in 1986-89. Unlike in our main analysis, here we estimate the reduced form equation and interact the exposure to the Earning or Learning reform with *ITT*, i.e., the pre-cutoff dummy variable. This is because we only observe pupils at the age of 16 here and, as explained above, due to data restrictions we are unable to trace all the pupils back since grade 8. In (2), β_3 is the coefficient of interest, as it measures whether Intention-To-Treat (*ITT*) younger pupils in cohort, as a result of the Earning or Learning Reform, were incapacitated to commit crime outside school at the age of 16, but were also more likely to receive SDAs in school. Finally, γ captures time fixed effects, *X* represents a vector of background characteristics, and *e* is the error term.

Tables 7 and 8 show the results of our analysis. Table 7 shows that the Earning or Learning Reform had a negative and significant effect on the propensity of *ITT* younger kids in cohort to commit crime at the age of 16. This effect appears driven by the decrease in property crime and theft committed by these kids at the age of 16. This appears plausible, as both property and theft offences would normally take place outside the school premises. Thus, the results in Table 7 are consistent with an incapacitation effect of school on crime. However, Table 8 shows that the Earning or Learning Reform also resulted in a steep increase in the likelihood of receiving SDAs at the age of 16 for *ITT* younger kids in cohort. The result appears driven by an increase in the

likelihood of persistent disruptive behaviour, physical misconduct, property misconduct and other misconduct, as well as verbal and non-verbal misconduct and refusal to participate in the school programme. Also in this case, for reasons of space sensitivity of results to gender and social class are shown in the appendix.

While the results in Table 6 fail to provide support for the hypothesis that poor labour market prospects may explain the greater likelihood of younger pupils in cohort to engage in crime from the age of 18, the results in Tables 7 and 8 provide support for the hypothesis that younger pupils in cohort were incapacitated in school until the end of secondary school. The evidence suggests that incapacitating pupils in school for one more year is likely to have opposite effects on crime outside school and SDAs inside school at the age of 16. While reflecting a crime-reducing role of school, these results also suggest that, at least to some extent, incapacitating juveniles in school moved crime from outside to inside the school premises. But what is, therefore, the net effect of school on the short run desire and possibility to commit crime? The final set of results presented in this paper aims to tackle this question.

In light of the earlier results, a natural question to ask is in fact whether the Earning or Learning reform simply moved crime from outside to inside school. More generally, a frequent critique made in the literature on the incapacitation effect of school on crime is that crime in school is simply not recorded by the police, and thus the crime-reducing effect of school is simply an artefact of the non-random lack of data on crime between inside and outside school. This paper attempts to address this critique by aggregating comparable types of crime and SDAs. We focused on three categories of crime offences and SDAs, namely those relating to property, drugs and violence.

Table 9 shows our results. Column (1) shows that the Earning or Learning reform reduced the likelihood of *ITT* younger pupils in cohort to commit property crime offences at the age of 16, although column (2) shows that it increased their likelihood to receive SDAs due to property misconduct. Column (3) shows the results when property crime offences and property misconduct

SDAs are added and modelled together as the dependent variable. A significant crime-reducing effect of school for *ITT* younger pupils in cohort still appears despite the increase in SDAs in school. This further confirms that the Earning or Learning reform reduced the likelihood of *ITT* younger pupils in cohort to commit property crime offences at the age of 16, whether inside or outside school.

Table 10 also shows by how much SDAs in school would need to increase in order to offset the crime-reducing effects of school in Table 9. In order to test this, we simulated increasingly higher rates of SDAs at the age of 16 and re-estimate equation (2) separately for each dependent variable. Column (1) in Table 10 shows our original *ITT* estimate from column (3) in Table 9. Columns (2) to (12) assume increasingly higher rates of SDAs at the age of 16. What appears clear from the simulation in Table 16 is that the crime reducing effect of school for property crime at the age of 16 is very robust. It is only when the actual count of SDAs at the age of 16 is increased by more than 70 percent that our crime-reducing effect of school for younger pupils in cohort becomes insignificant, although still negative and numerically very similar to our original estimate in column (1). In contrast, the Earning or Learning reform did not affect the likelihood of *ITT* younger pupils in cohort to commit drug offences at the age of 16, and it increased the likelihood of *ITT* younger pupils in cohort to receive SDAs due to violent behaviour in school at the age of 16. The net effect of school on crime in this case is positive, reflecting the concentration effect that school can have on this type of crime (Jacob and Lefgren, 2003).

7. Conclusion

It is frequently argued that one way to prevent engagement in criminal activity is to increase the duration of the school day or school year and/or to keep juveniles busy with an array of activities when school is not in session. Underlying such policy prescriptions to address juvenile crime is the idea that incapacitation would keep pupils out of trouble. Advocates of after-school and other youth programs often suggest that teen violence peaks when school is not in session. Although the

principle behind such policy prescriptions is logical, the school environment can have a variety of different effects on juvenile crime that are far from clear *a priori*.

This study documents an important and unexplored link between bad behaviour in school and criminal behaviour of juveniles outside school. It does so by documenting the fact that youths who start school at a younger age are not only more likely to engage in youth crime, but they are also more likely to receive disciplinary sanctions while they attend secondary school. A very clear profile emerges from our analysis: younger pupils in cohort are more likely to receive disciplinary sanctions in school and, by the time they leave school, they show a greater propensity to commit crime which persists until the age of 24.

Among the hypotheses which are contemplated in this study to explain this pattern, the incapacitation effect of school on crime during secondary school by younger pupils in cohort finds empirical support in our results. The availability of a recent change in the laws governing school leaving behaviour in Queensland allows us to show the substitutability at a given point in time between crime outside school and bad behaviour within the school premises. Our findings suggest that, even when SDAs are treated as crime inside school, the net effect of school attendance on crime is negative. This conclusion is consistent with a net incapacitation effect of school attendance on crime. However, our findings also document the important predictive role of SDAs for future engagement in juvenile crime. A policy advice that logically follows is that prompt interventions should be put in place to address bad behaviour in school before juveniles leave school, as this may potentially help break the SDA- to crime-age profile documented in this study.

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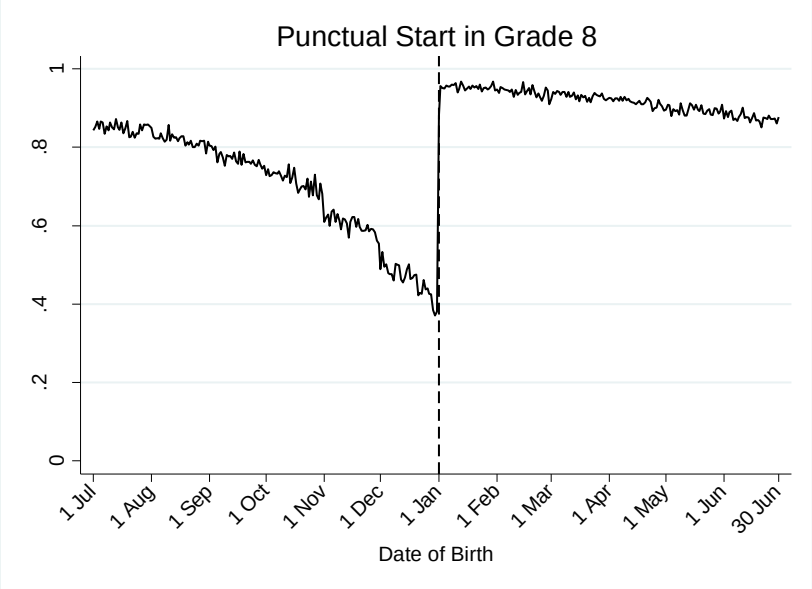
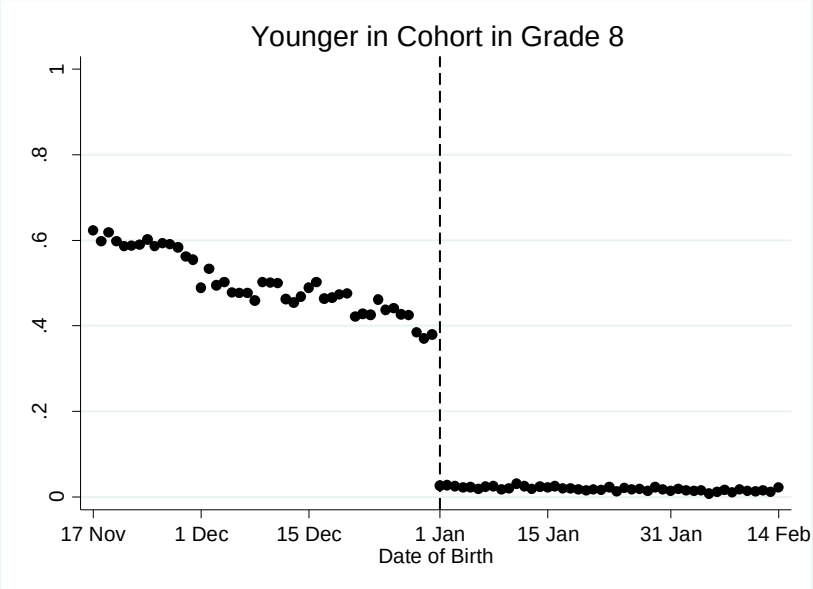
Figure 1. Fraction of Punctual Secondary School Start in Grade 8 by Date of Birth



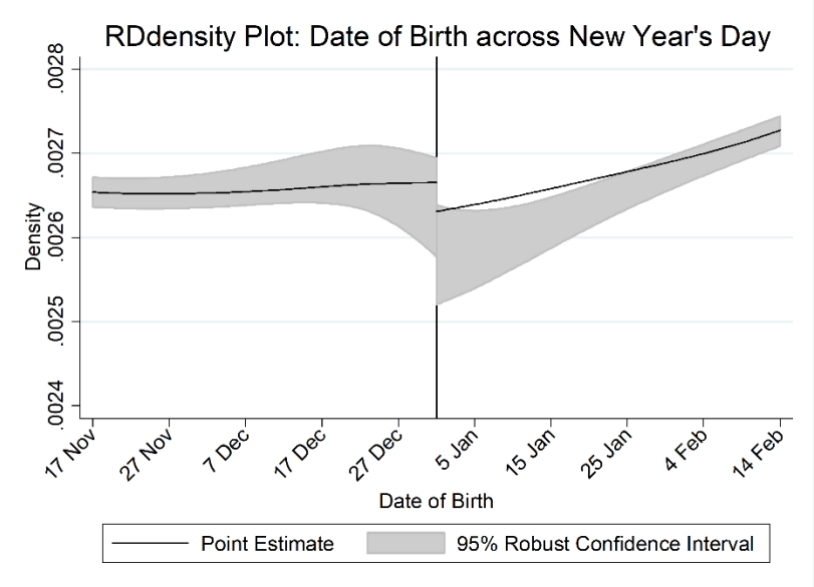
Notes: Figure 1 shows the secondary school starting pattern of the full population of pupils in our sample by date of birth. Punctual start refers to secondary school start in the calendar year the pupil turns 13 years of age.

Figure 2. Fraction Who Are Younger in Cohort in Grade 8 by Date of Birth



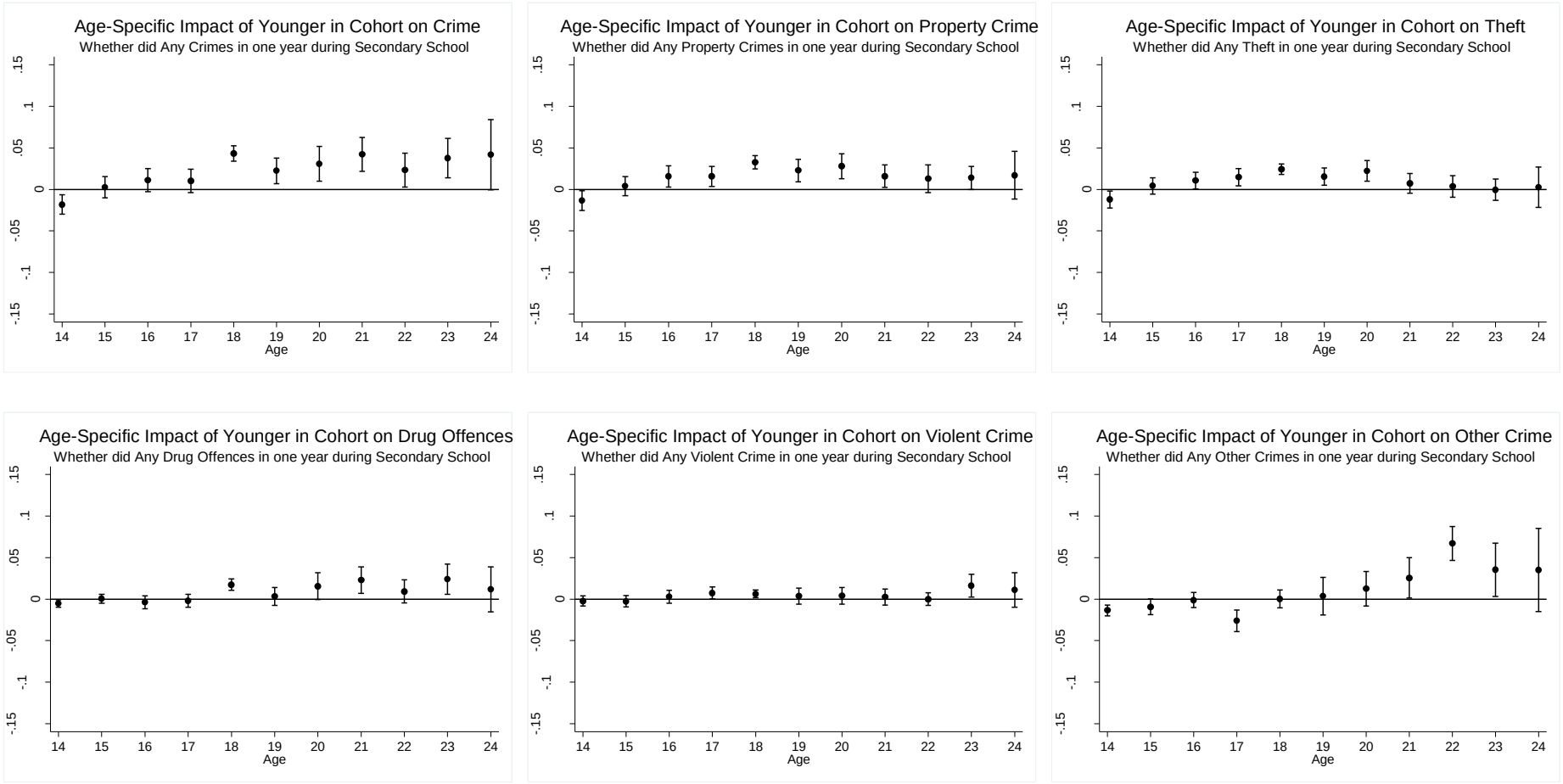
Notes: Figure 2 shows the fraction of pupils who are 'younger-in-cohort' in Grade 8 by date of birth for all pupils in our sample born 45 days either sides of the 1st January cutoff.

Figure 3. Density Test Across Discontinuity



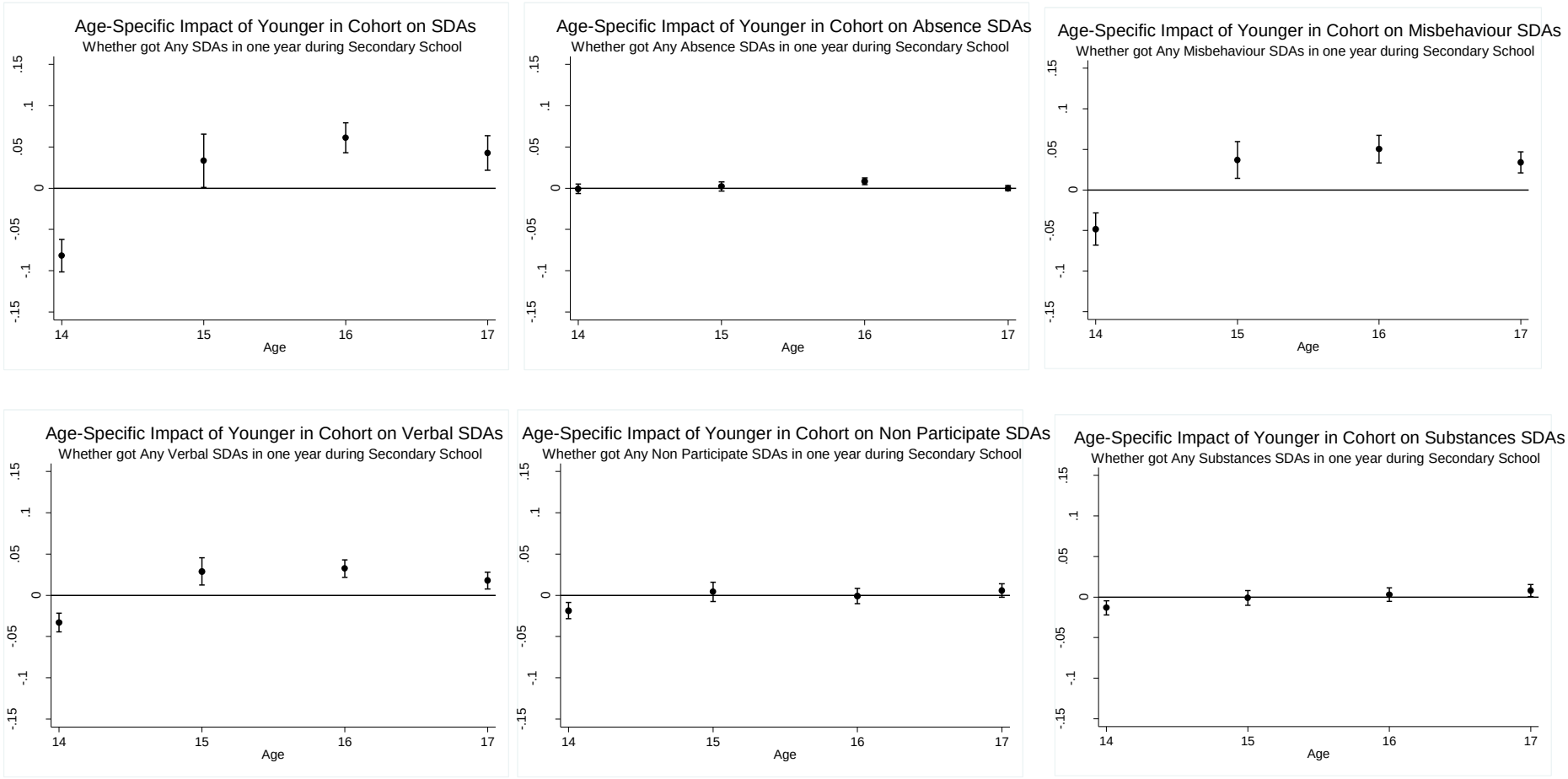
Notes: Horizontal axis represents the date of birth for our entire sample. Solid curves represent local linear smoother using a triangle kernel and a 45-day bandwidth. The shaded area represents 95 percent bias-robust confidence intervals. The null hypothesis of equality in the densities either sides of the 1st January cutoff could not be rejected at the 10 percent level, based on the main local linear regression specification with a 45-day bandwidth and triangle kernel, and whether we used conventional or bias-robust confidence intervals.

Figure 4. Estimates of Young Relative Age Impact on Different Types of Crime by Age.



Notes: Figure 4 shows point estimates and 95 percent confidence intervals of the causal effect of young relative age in secondary school on occurrence of (0/1) different types of crime by age. All estimates are obtained from 2SLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. Crime offences include property offences, drug offences and violent offences by offender in a year. Property offences include burglary, theft and handling of stolen goods, and criminal damage by offender in a year. Violent offences include violence against the person, sexual offences and robbery by offender in a year. Other offences include dangerous or negligent acts endangering persons, blackmailing, fraud, deception and related offences, weapon offences, public order offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations, and miscellaneous offences by offender in a year.

Figure 5. Estimates of Young Relative Age Impact on Different Types of School Disciplinary Absences (SDAs) by Age.



Notes: Figure 5 shows point estimates and 95 percent confidence intervals of the causal effect of young relative age in secondary school on receipt of (0/1) different types of school disciplinary absences (SDAs) by age. All estimates are obtained from 2SLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. SDAs include all categories of SDAs by pupil in a year. Absence SDAs include SDAs due to truancy by pupil in a year. Conduct SDAs include SDAs due to persistent disruptive behaviour, physical misconduct, property misconduct and other misconduct by pupil in a year. Verbal SDAs include verbal and non-verbal misconduct by pupil in a year. Non-Participate SDAs include SDAs due to refusal to participate in the school programme by pupil in a year. Substance SDAs include SDAs due to substance misconduct involving legal or illegal substances.

Table 1. Means of Selected Outcome Variables by Types of Crime

Number of Criminal Charges at Age:	Crime Offences (Any) (1)	Crime Offences (No.) (2)	Property Offences (Any) (3)	Property Offences (No.) (4)	Theft Offences (Any) (5)	Theft Offences (No.) (6)	Drug Offences (Any) (7)	Drug Offences (No.) (8)	Violent Offences (Any) (9)	Violent Offences (No.) (10)	Other Offences (Any) (11)	Other Offences (No.) (12)	No. Pupils (13)
14	0.032	0.087	0.026	0.072	0.019	0.042	0.004	0.006	0.007	0.009	0.011	0.013	88078
15	0.040	0.122	0.032	0.099	0.023	0.055	0.007	0.011	0.009	0.013	0.017	0.022	88078
16	0.041	0.120	0.030	0.091	0.021	0.049	0.009	0.016	0.010	0.013	0.023	0.029	87934
17	0.043	0.121	0.028	0.083	0.020	0.046	0.014	0.024	0.010	0.014	0.034	0.043	79158
18	0.043	0.115	0.025	0.073	0.017	0.036	0.017	0.030	0.010	0.013	0.049	0.061	70742
19	0.038	0.093	0.020	0.052	0.013	0.029	0.017	0.030	0.009	0.011	0.056	0.068	61928
20	0.034	0.074	0.017	0.037	0.012	0.022	0.017	0.028	0.007	0.009	0.056	0.067	53276
21	0.030	0.065	0.014	0.031	0.009	0.016	0.015	0.026	0.007	0.008	0.050	0.060	44455
22	0.027	0.056	0.012	0.024	0.007	0.014	0.014	0.026	0.005	0.007	0.046	0.054	35715
23	0.023	0.051	0.010	0.022	0.006	0.013	0.013	0.023	0.005	0.007	0.039	0.046	27187
24	0.022	0.049	0.010	0.020	0.007	0.013	0.012	0.023	0.005	0.006	0.033	0.039	18610
Overall	0.037	0.097	0.023	0.065	0.016	0.036	0.012	0.020	0.008	0.011	0.032	0.039	88078

Notes: Table 1 shows means of selected outcome variables by types of crime. Crime offences include property offences, drug offences and violent offences by offender in a year. Property offences include burglary, theft and handling of stolen goods, and criminal damage by offender in a year. Violent offences include violence against the person, sexual offences and robbery by offender in a year. Other offences include dangerous or negligent acts endangering persons, blackmailing, fraud, deception and related offences, weapon offences, public order offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations, and miscellaneous offences by offender in a year.

Table 2. Means of Selected Outcome Variables by Types of School Disciplinary Absences (SDAs)

Number of Criminal Charges at Age:	SDAs (Any)	SDAs (No.)	Absence SDAs (Any)	Absence SDAs (No.)	Conduct SDAs (Any)	Conduct SDAs (No.)	Verbal SDAs (Any)	Verbal SDAs (No.)	Non-Participate SDAs (Any)	Non-Participate SDAs (No.)	Substance SDAs (Any)	Substance SDAs (No.)	No. Pupils
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
14	0.134	0.244	0.006	0.006	0.100	0.147	0.043	0.053	0.019	0.023	0.014	0.015	88078
15	0.133	0.226	0.006	0.006	0.091	0.126	0.044	0.053	0.021	0.024	0.016	0.017	88078
16	0.091	0.136	0.003	0.003	0.054	0.068	0.028	0.032	0.018	0.020	0.011	0.012	87934
17	0.056	0.074	0.002	0.002	0.029	0.034	0.016	0.018	0.013	0.014	0.007	0.007	79158
Overall	0.105	0.173	0.004	0.004	0.070	0.095	0.033	0.040	0.018	0.020	0.012	0.013	88078

Notes: Table 2 shows means of selected outcome variables by types of school disciplinary absences (SDAs). SDAs include all categories of SDAs by pupil in a year. Absence SDAs include SDAs due to truancy by pupil in a year. Conduct SDAs include SDAs due to persistent disruptive behaviour, physical misconduct, property misconduct and other misconduct by pupil in a year. Verbal SDAs include verbal and non-verbal misconduct by pupil in a year. Non-Participate SDAs include SDAs due to refusal to participate in the school programme by pupil in a year. Substance SDAs include SDAs due to substance misconduct involving legal or illegal substances.

Table 3. First Stage Estimates

	OLS		Local Polynomial	
	(1)	(2)	(3)	(4)
Nov-Dec = 1	0.384***	0.394***	0.377***	0.383***
	(0.007)	(0.007)	(0.008)	(0.008)
Cohort Fixed Effects	Yes	Yes	Yes	Yes
Background Characteristics	No	Yes	No	Yes
Pupils	88078	88078	88078	88078

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 45 days is used. All specifications include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Regressions in columns (2) and (4) also include binary indicators for whether the pupil is male, the primary school ICSEA Index of the pupil and whether the pupil is a native English speaker. 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 4. Balancing of Covariates Across Discontinuity

	OLS			Local Polynomial		
	Male	Native English Speaker	ICSEA Primary School	Male	Native English Speaker	ICSEA Primary School
	(1)	(2)	(3)	(4)	(5)	(6)
Nov-Dec = 1	0.009	0.012	0.002	0.010	0.015	0.004
	(0.008)	(0.007)	(0.005)	(0.010)	(0.010)	(0.009)
Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Background Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Pupils	88078	88078	88078	88078	88078	88078

Notes: Presented in each column is the discontinuity estimate of the given variable at the cutoff date. Robust standard errors are clustered at the date of birth level. Estimates in columns (1), (2) and (3) are obtained from OLS regression specifications. Estimates in columns (4), (5) and (6) are obtained from local linear regression specifications where a triangular kernel with bandwidth of 45 days is used. All specifications include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, cohort fixed effects and other individual covariates. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 5. Estimates of Young Relative Age Impact on Crime and School Disciplinary Absences (SDAs).

	Crime Offences (No.)	Crime Offences (Any)	SDAs (No.)	SDAs (Any)
	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
Younger in Cohort	0.071***	0.016***	0.061***	0.019***
	(0.022)	(0.003)	(0.013)	(0.007)
No. Pupils (60 days Bandwidth)	117746	117746	117746	117746
Younger in Cohort	0.060**	0.015***	0.049***	0.012
	(0.026)	(0.004)	(0.015)	(0.008)
No. Pupils (45 days Bandwidth)	88078	88078	88078	88078
Younger in Cohort	0.060**	0.014***	0.021	0.006
	(0.029)	(0.004)	(0.018)	(0.010)
No. Pupils (30 days Bandwidth)	58426	58426	58426	58426
Control Variables	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes

Notes: Table 5 shows estimates of the causal effect of young relative age in secondary school on crime and SDAs. All estimates are obtained from OLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors (clustered at the date of birth level) are reported in parentheses. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. Crime offences include property offences, drug offences and violent offences by offender in a year. SDAs include all SDAs by pupil in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 6. Estimates of Young Relative Age Impact on Grade Retention and Dropout Choice before End of Secondary School.

	Grade Retention by End of Grade 12	Enrolled in Grade 12	Good OP Award
	(1)	(2)	(3)
	IV	IV	IV
Younger in Cohort	0.020***	0.134***	0.032*
	(0.006)	(0.015)	(0.019)
No. Pupils (60 days Bandwidth)	117746	117746	92776
Younger in Cohort	0.025***	0.137***	0.035
	(0.008)	(0.017)	(0.023)
No. Pupils (45 days Bandwidth)	88078	88078	69442
Younger in Cohort	0.011	0.142***	0.040
	(0.009)	(0.021)	(0.028)
No. Pupils (30 days Bandwidth)	58426	58426	46050
Subsample	Full Sample	Full Sample	Full Sample
Control Variables	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes

Notes: Table 6 shows estimates of the causal effect of young relative age in secondary school on grade retention, likelihood to be enrolled in Grade 12 and likelihood to obtain a sufficient OP score to be eligible for university in Queensland (i.e., OP < 14). All estimates are obtained from 2SLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors (clustered at the date of birth level) are reported in parentheses. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 7. Estimates of Young Relative Age * Earning or Learning Reform Impact on Crime.

Dep. Var. measured at age 16.	Crime Offences (No.)	Crime Offences (Any)	Property Crime Offences (No.)	Property Crime Offences (Any)	Theft Offences (No.)	Theft Offences (Any)	Drug Offences (No.)	Drug Offences (Any)	Violent Offences (No.)	Violent Offences (Any)	Other Offences (No.)	Other Offences (Any)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT
Younger in Cohort * Earning or Learning Reform	-0.026**	-0.002	-0.027**	-0.004	-0.025***	-0.006***	0.002	0.002	-0.001	0.000	-0.005*	-0.003
	(0.013)	(0.003)	(0.012)	(0.003)	(0.008)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)
No. Pupils (45 days Bandwidth)	92472	92472	92472	92472	92472	92472	92472	92472	92472	92472	92472	92472
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 7 shows estimates of the causal effect of young relative age in secondary school and exposure to the Earning or Learning (2006) reform on crime at age 16 by crime type. All estimates are obtained from OLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors (clustered at the date of birth level) are reported in parentheses. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. Crime offences include property offences, drug offences and violent offences by offender in a year. Property offences include burglary, theft and handling of stolen goods, and criminal damage by offender in a year. Violent offences include violence against the person, sexual offences and robbery by offender in a year. Other offences include dangerous or negligent acts endangering persons, blackmailing, fraud, deception and related offences, weapon offences, public order offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations, and miscellaneous offences by offender in a year.* indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 8. Estimates of Young Relative Age * Earning or Learning Reform Impact on School Disciplinary Absences (SDAs).

Dep. Var. measured at age 16.	SDAs (No.)	SDAs (Any)	Absence SDAs (No.)	Absence SDAs (Any)	Misbehav. SDAs (No.)	Misbehav. SDAs (Any)	Verbal SDAs (No.)	Verbal SDAs (Any)	No Partic SDAs (No.)	No Partic SDAs (Any)	Subst. SDAs (No.)	Subst. SDAs (Any)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT
Younger in Cohort * Earning or Learning Reform	0.036***	0.021***	0.001	0.001	0.016***	0.012***	0.010***	0.008***	0.007***	0.006***	0.002	0.002
	(0.006)	(0.003)	(0.001)	(0.001)	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
No. Pupils (45 days Bandwidth)	92472	92472	92472	92472	92472	92472	92472	92472	92472	92472	92472	92472
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 8 shows estimates of the causal effect of young relative age in secondary school and exposure to the Earning or Learning (2006) reform on SDAs at age 16 by type of SDAs. All estimates are obtained from OLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors (clustered at the date of birth level) are reported in parentheses. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. SDAs include all SDAs by pupil in a year. Absence SDAs include SDAs due to truancy by pupil in a year. Conduct SDAs include SDAs due to persistent disruptive behaviour, physical misconduct, property misconduct and other misconduct by pupil in a year. Verbal SDAs include verbal and non-verbal misconduct by pupil in a year. Non-Participate SDAs include SDAs due to refusal to participate in the school programme by pupil in a year. Substance SDAs include SDAs due to substance misconduct involving legal or illegal substances. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 9. Estimates of Young Relative Age * Earning or Learning Reform Impact on Crime and SDAs.

Dep. Var. measured at age 16.	Property Crime Offences (No.)	Property Misconduct SDAs (No.)	Property Crime & SDAs (No.)	Drug Offences (No.)	Drug SDAs (No.)	Drug Crime & SDAs (No.)	Violent Offences (No.)	Violent SDAs (No.)	Violent Crime & SDAs (No.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT
Younger in Cohort * Earning or Learning Reform	-0.027**	0.004***	-0.023*	0.002	0.000	0.003	-0.001	0.009***	0.008**
	(0.012)	(0.001)	(0.012)	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)	(0.003)
No. Pupils (45 days Bandwidth)	92472	92472	92472	92472	92472	92472	92472	92472	92472
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 9 shows estimates of the causal effect of young relative age in secondary school and exposure to the Earning or Learning (2006) reform on crime and SDAs at age 16 by crime type and SDA type. All estimates are obtained from OLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors (clustered at the date of birth level) are reported in parentheses. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. Property offences include burglary, theft and handling of stolen goods, and criminal damage by offender in a year. Violent offences include violence against the person, sexual offences and robbery by offender in a year.* indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

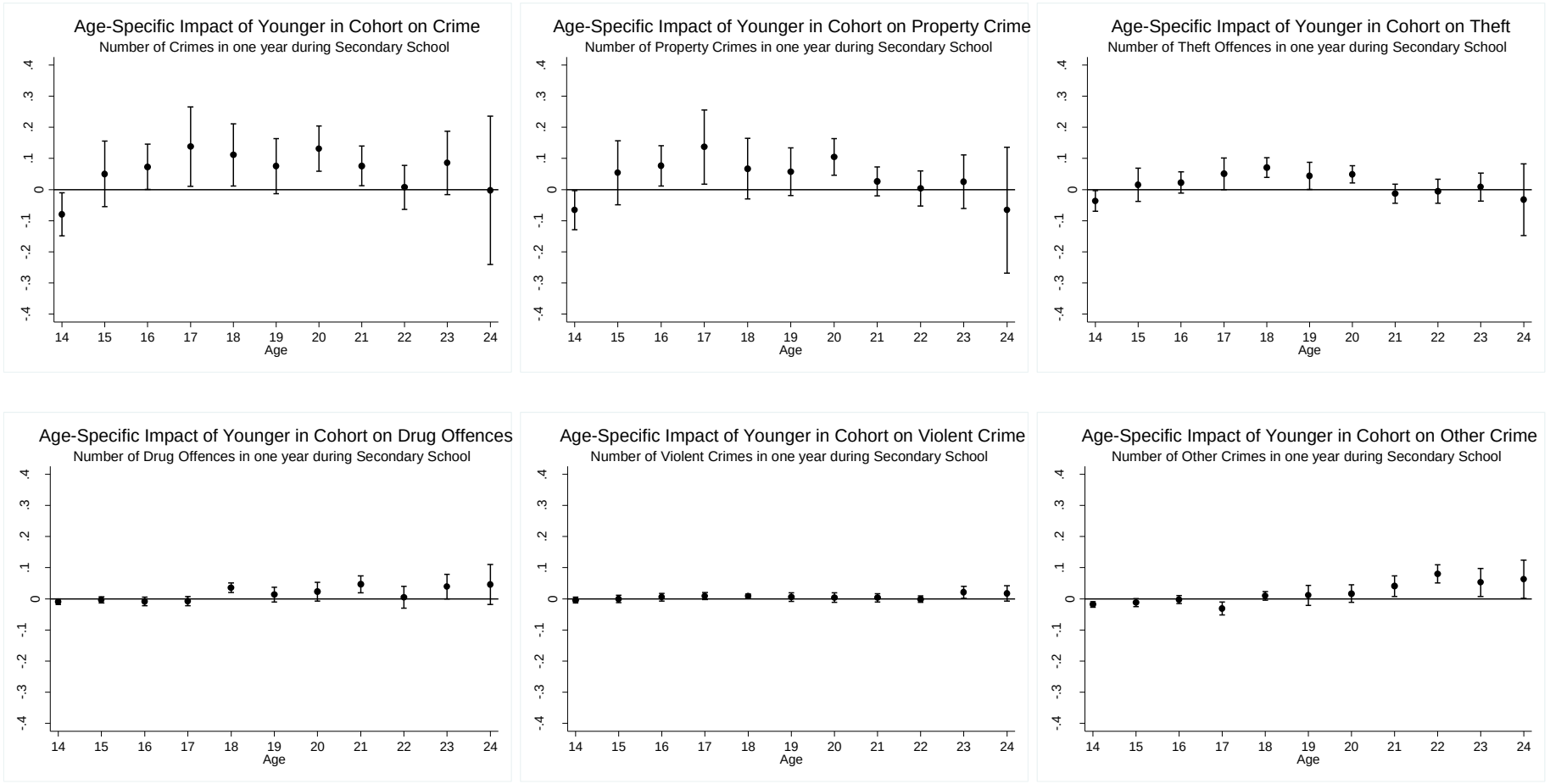
Table 10. Simulation Estimates of Young Relative Age * Earning or Learning Reform Impact on Property Crime and SDAs.

	Young Relative Age * Earning or Learning Reform Impact on Property Crime & SDAs (No.)											
Dep. Var. measured at age 16.	Original ITT	(1) + 5% SDAs	(1) + 10% SDAs	(1) + 15% SDAs	(1) + 20% SDAs	(1) + 30% SDAs	(1) + 40% SDAs	(1) + 50% SDAs	(1) + 60% SDAs	(1) + 70% SDAs	(1) + 80% SDAs	(1) + 90% SDAs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT
Younger in Cohort * Earning or Learning Reform	-0.023* (0.012)	-0.023* (0.012)	-0.023* (0.012)	-0.023* (0.012)	-0.022* (0.012)	-0.022* (0.012)	-0.021* (0.012)	-0.021* (0.012)	-0.021* (0.012)	-0.020* (0.012)	-0.020 (0.012)	-0.019 (0.012)
No. Pupils (45 days Bandwidth)	92472	92472	92472	92472	92472	92472	92472	92472	92472	92472	92472	92472
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 10 shows simulation estimates of the causal effect of young relative age in secondary school and exposure to the Earning or Learning (2006) reform on property crime and SDAs at age 16. All estimates are obtained from OLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors (clustered at the date of birth level) are reported in parentheses. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. Property offences include burglary, theft and handling of stolen goods, and criminal damage by offender in a year. In columns (2) to (12), the number of SDAs at age 16 is increased by the reported percentage to simulate the size of the increase in SDAs that would be necessary to completely offset the incapacitation effect of school on crime. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

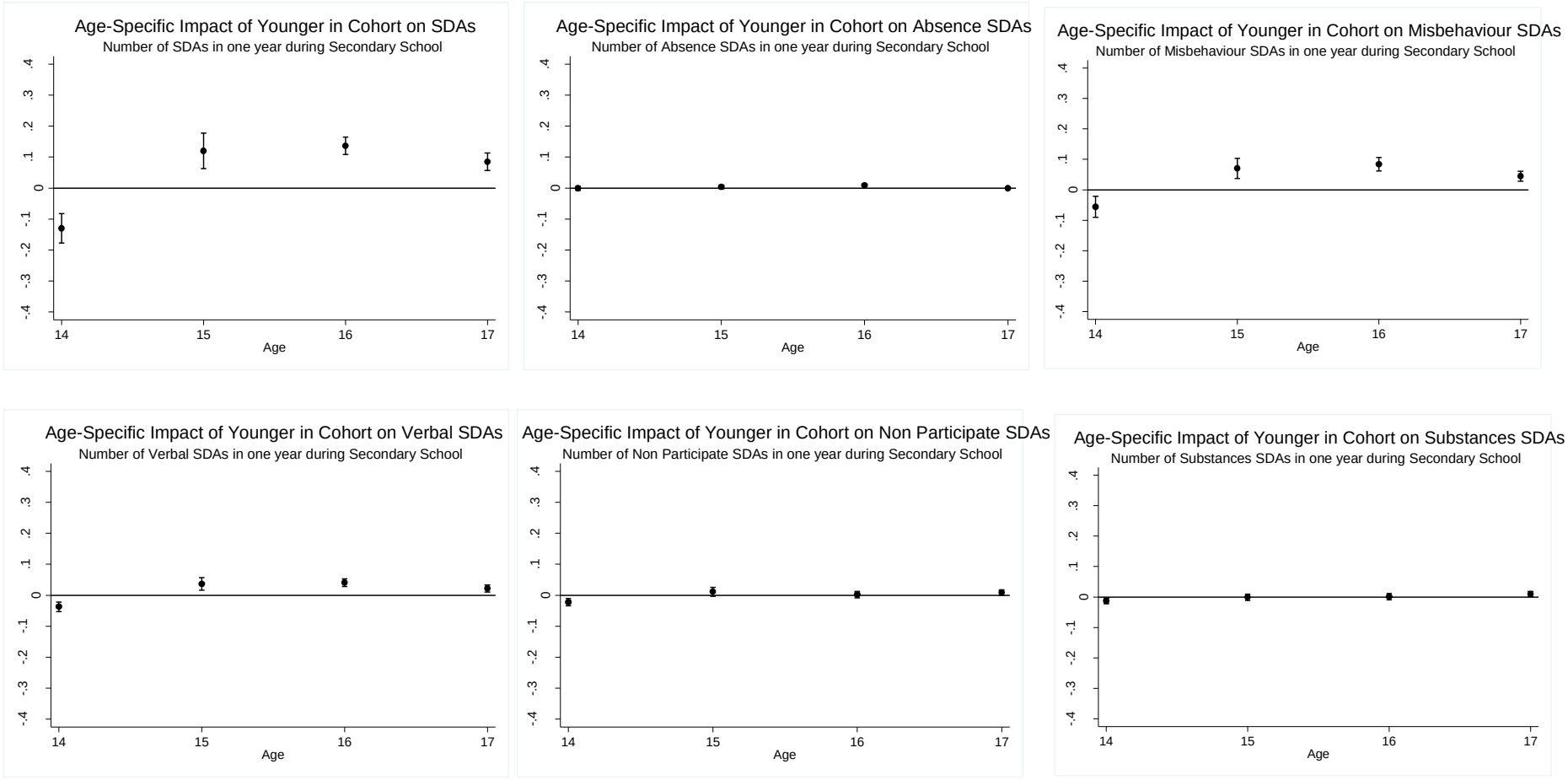
APPENDIX FIGURES & TABLES

Figure A.1. Estimates of Young Relative Age Impact on Different Types of Crime by Age.



Notes: Figure A.1 shows point estimates and 95 percent confidence intervals of the causal effect of young relative age in secondary school on number of different types of crime by age. All estimates are obtained from 2SLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. Crime offences include property offences, drug offences and violent offences by offender in a year. Property offences include burglary, theft and handling of stolen goods, and criminal damage by offender in a year. Violent offences include violence against the person, sexual offences and robbery by offender in a year. Other offences include dangerous or negligent acts endangering persons, blackmailing, fraud, deception and related offences, weapon offences, public order offences, traffic and vehicle regulatory offences, offences against justice procedures, government security and government operations, and miscellaneous offences by offender in a year.

Figure A.2. Estimates of Young Relative Age Impact on Different Types of School Disciplinary Absences (SDAs) by Age.



Notes: Figure A.2 shows point estimates and 95 percent confidence intervals of the causal effect of young relative age in secondary school on number of different types of SDAs by age. All estimates are obtained from 2SLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. SDAs include all categories of SDAs by pupil in a year. Absence SDAs include SDAs due to truancy by pupil in a year. Conduct SDAs include SDAs due to persistent disruptive behaviour, physical misconduct, property misconduct and other misconduct by pupil in a year. Verbal SDAs include verbal and non-verbal misconduct by pupil in a year. Non-Participate SDAs include SDAs due to refusal to participate in the school programme by pupil in a year. Substance SDAs include SDAs due to substance misconduct involving legal or illegal substances.

Table A.1. Descriptive Statistics of Juveniles in the Public School System in Queensland.

	Mean	Std. Dev.	Min	Max
Year	2009.797	3.120	2002	2014
Female	0.483	0.500	0	1
Year of birth	1992.447	2.746	1988	1998
Month of birth	6.486	3.415	1	12
Indigenous or Torres Strait Islander	0.038	0.191	0	1
Age	17.351	3.122	11	25
Proportion of individuals aged:				
14	0.120	0.325	0	1
15	0.120	0.325	0	1
16	0.120	0.325	0	1
17	0.108	0.310	0	1
18	0.096	0.295	0	1
19	0.084	0.278	0	1
20	0.072	0.259	0	1
21	0.060	0.238	0	1
22	0.048	0.214	0	1
23	0.036	0.187	0	1
24	0.025	0.156	0	1
School grade	9.767	1.363	8	12
Good Overall Position (OP) of 1 to 14	0.121	0.326	0	1
Index of Community Socio-Educational Advantage index (ICSEA)	979.153	57.111	590	1157
Below median ICSEA	0.253	0.435	0	1

Notes: Table A.1 shows descriptive statistics for pupils born throughout the entire calendar year and not only for our treatment and control pupils that were used in the empirical analysis. For pupils included in our empirical analysis, statistics looked very similar.

Table A.2. Estimates of Young Relative Age Impact on Crime for Males, Pupils in Schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and Indigenous Pupils.

	Crime Offences (No.)	Crime Offences (Any)	Crime Offences (No.)	Crime Offences (Any)	Crime Offences (No.)	Crime Offences (Any)
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	IV	IV	IV	IV	IV
Younger in Cohort	0.118*	0.030***	0.074***	0.019***	0.021	0.015
	(0.062)	(0.009)	(0.028)	(0.004)	(0.181)	(0.023)
No. Pupils (45 days Bandwidth)	45810	45810	81009	81009	7742	7742
Subsample	Males	Males	High ICSEA	High ICSEA	Indigenous	Indigenous
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.2 shows estimates of the causal effect of young relative age in secondary school on crime for males, for pupils in schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and for indigenous pupils. All estimates are obtained from 2SLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. Crime offences include property offences, drug offences and violent offences by offender in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.3. Estimates of Young Relative Age Impact on SDAs for Males, Pupils in Schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and Indigenous Pupils.

	SDAs (No.)	SDAs (Any)	SDAs (No.)	SDAs (Any)	SDAs (No.)	SDAs (Any)
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	IV	IV	IV	IV	IV
Younger in Cohort	0.094***	0.022	0.037***	0.012	0.106	0.013
	(0.033)	(0.017)	(0.014)	(0.009)	(0.071)	(0.035)
No. Pupils (45 days Bandwidth)	45810	45810	81009	81009	7742	7742
Subsample	Males	Males	High ICSEA	High ICSEA	Indigenous	Indigenous
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.3 shows estimates of the causal effect of young relative age in secondary school on school disciplinary absences (SDAs) for males, for pupils in schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and for indigenous pupils. All estimates are obtained from 2SLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. SDAs include all categories of SDAs by pupil in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.4. Estimates of Young Relative Age Impact on SDAs for Males, Pupils in Schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and Indigenous Pupils.

	Grade Retention by End of Grade 12	Enrolled in Grade 12	Good OP Award	Grade Retention by End of Grade 12	Enrolled in Grade 12	Good OP Award	Grade Retention by End of Grade 12	Enrolled in Grade 12	Good OP Award
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	IV	IV	IV	IV	IV	IV	IV	IV	IV
Younger in Cohort	0.032**	0.120***	0.046	0.028***	0.103***	0.054	0.082***	0.142***	0.018
	(0.014)	(0.028)	(0.032)	(0.009)	(0.027)	(0.037)	(0.028)	(0.042)	(0.029)
No. Pupils (45 days Bandwidth)	45810	45810	45810	81009	81009	81009	7742	7742	7742
Subsample	Males	Males	Males	High ICSEA	High ICSEA	High ICSEA	Indigenous	Indigenous	Indigenous
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.4 shows estimates of the causal effect of young relative age in secondary school on grade retention, likelihood to be enrolled in Grade 12 and likelihood to obtain a sufficient OP score to be eligible for university in Queensland (i.e., OP < 14) for males, for pupils in schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and for indigenous pupils. All estimates are obtained from 2SLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.5. Estimates of Young Relative Age* Earning or Learning Reform Impact on Crime for Males, Pupils in Schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and Indigenous Pupils.

Dep. Var. measured at age 16.	Crime Offences (No.)	Crime Offences (Any)	Crime Offences (No.)	Crime Offences (Any)	Crime Offences (No.)	Crime Offences (Any)
	(1)	(2)	(3)	(4)	(5)	(6)
	ITT	ITT	ITT	ITT	ITT	ITT
Younger in Cohort * Earning or Learning Reform	-0.049**	-0.002	-0.018	0.003	-0.176	-0.018
	(0.025)	(0.005)	(0.019)	(0.004)	(0.126)	(0.023)
No. Pupils (45 days Bandwidth)	46752	46752	60857	60857	3987	3987
Subsample	Males	Males	High ICSEA	High ICSEA	Indigenous	Indigenous
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.5 shows estimates of the causal effect of young relative age in secondary school and exposure to the Earning or Learning (2006) reform on crime for males, for pupils in schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and for indigenous pupils. All estimates are obtained from OLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. Crime offences include property offences, drug offences and violent offences by offender in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.6. Estimates of Young Relative Age* Earning or Learning Reform Impact on SDAs for Males, Pupils in Schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and Indigenous Pupils.

Dep. Var. measured at age 16.	SDAs (No.)	SDAs (Any)	SDAs (No.)	SDAs (Any)	SDAs (No.)	SDAs (Any)
	(1)	(2)	(3)	(4)	(5)	(6)
	ITT	ITT	ITT	ITT	ITT	ITT
Younger in Cohort * Earning or Learning Reform	0.051***	0.028***	0.026***	0.015***	0.062	0.030
	(0.011)	(0.006)	(0.006)	(0.004)	(0.045)	(0.024)
No. Pupils (45 days Bandwidth)	46752	46752	60857	60857	3987	3987
Subsample	Males	Males	High ICSEA	High ICSEA	Indigenous	Indigenous
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.6 shows estimates of the causal effect of young relative age in secondary school and exposure to the Earning or Learning (2006) reform on SDAs for males, for pupils in schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and for indigenous pupils. All estimates are obtained from OLS specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. SDAs include all categories of SDAs by pupil in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.7. Estimates of Young Relative Age * Earning or Learning Reform Impact on Crime and SDAs for Males, Pupils in Schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and Indigenous Pupils.

Dep. Var. measured at age 16.	Property Crime & SDAs (No.)	Drug Crime & SDAs (No.)	Violent Crime & SDAs (No.)	Property Crime & SDAs (No.)	Drug Crime & SDAs (No.)	Violent Crime & SDAs (No.)	Property Crime & SDAs (No.)	Drug Crime & SDAs (No.)	Violent Crime & SDAs (No.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT	ITT
Younger in Cohort * Earning or Learning Reform	-0.043*	0.003	0.011**	-0.018	0.003	0.004	-0.140	-0.015	-0.016
	(0.022)	(0.005)	(0.005)	(0.017)	(0.003)	(0.003)	(0.114)	(0.021)	(0.028)
No. Pupils (45 days Bandwidth)	46752	46752	46752	60857	60857	60857	3987	3987	3987
Subsample	Males	Males	Males	High ICSEA	High ICSEA	High ICSEA	Indigenous	Indigenous	Indigenous
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.7 shows estimates of the causal effect of young relative age in secondary school and exposure to the Earning or Learning (2006) reform on crime and SDAs at age 16 by crime type and SDA type for males, for pupils in schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and for indigenous pupils. All estimates are obtained from OLS regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. Robust standard errors (clustered at the date of birth level) are reported in parentheses. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. Property offences include burglary, theft and handling of stolen goods, and criminal damage by offender in a year. Violent offences include violence against the person, sexual offences and robbery by offender in a year.* indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.8. Estimates of Young Relative Age Impact on Crime and School Disciplinary Absences (SDAs).

	Crime Offences (No.)	Crime Offences (Any)	SDAs (No.)	SDAs (Any)
	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
Younger in Cohort	0.063***	0.015***	0.047***	0.013*
	(0.024)	(0.003)	(0.014)	(0.008)
No. Pupils (60 days Bandwidth)	117746	117746	117746	117746
Younger in Cohort	0.059**	0.015***	0.034**	0.008
	(0.028)	(0.004)	(0.016)	(0.009)
No. Pupils (45 days Bandwidth)	88078	88078	88078	88078
Younger in Cohort	0.063*	0.013***	0.018	0.000
	(0.034)	(0.004)	(0.020)	(0.012)
No. Pupils (30 days Bandwidth)	58426	58426	58426	58426
Control Variables	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes

Notes: Table A.8 shows estimates of the causal effect of young relative age in secondary school on crime and SDAs. All estimates are obtained from local linear regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. 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 pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. Crime offences include property offences, drug offences and violent offences by offender in a year. SDAs include all SDAs by pupil in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.9. Estimates of Young Relative Age Impact on Crime for Males, Pupils in Schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and Indigenous Pupils.

	Crime Offences (No.)	Crime Offences (Any)	Crime Offences (No.)	Crime Offences (Any)	Crime Offences (No.)	Crime Offences (Any)
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	IV	IV	IV	IV	IV
Younger in Cohort	0.113*	0.025***	0.062**	0.018***	0.114	0.016
	(0.067)	(0.009)	(0.029)	(0.004)	(0.180)	(0.024)
No. Pupils (45 days Bandwidth)	45810	45810	81009	81009	7742	7742
Subsample	Males	Males	High ICSEA	High ICSEA	Indigenous	Indigenous
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.9 shows estimates of the causal effect of young relative age in secondary school on crime for males, for pupils in schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and for indigenous pupils. All estimates are obtained from local linear regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. A triangular kernel with bandwidth of 45 days is used in all cases. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. Crime offences include property offences, drug offences and violent offences by offender in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.10. Estimates of Young Relative Age Impact on SDAs for Males, Pupils in Schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and Indigenous Pupils.

	SDAs (No.)	SDAs (Any)	SDAs (No.)	SDAs (Any)	SDAs (No.)	SDAs (Any)
	(3)	(4)	(5)	(6)	(7)	(8)
	IV	IV	IV	IV	IV	IV
Younger in Cohort	0.067*	0.018	0.028*	0.011	0.092	0.009
	(0.037)	(0.019)	(0.016)	(0.010)	(0.081)	(0.040)
No. Pupils (45 days Bandwidth)	45810	45810	81009	81009	7742	7742
Subsample	Males	Males	High ICSEA	High ICSEA	Indigenous	Indigenous
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.10 shows estimates of the causal effect of young relative age in secondary school on school disciplinary absences (SDAs) for the full sample, for males, for pupils in schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and for indigenous pupils. All estimates are obtained from local linear regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. A triangular kernel with bandwidth of 45 days is used in all cases. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. SDAs includes all SDAs by pupil in a year. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table A.11. Estimates of Young Relative Age Impact on SDAs for Males, Pupils in Schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and Indigenous Pupils.

	Grade Retention by End of Grade 12	Enrolled in Grade 12	Good OP Award	Grade Retention by End of Grade 12	Enrolled in Grade 12	Good OP Award	Grade Retention by End of Grade 12	Enrolled in Grade 12	Good OP Award	Grade Retention by End of Grade 12	Enrolled in Grade 12	Good OP Award
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV
Younger in Cohort	0.018**	0.147***	0.042	0.023*	0.142***	0.054	0.023**	0.124***	0.048	0.078***	0.144***	0.025
	(0.008)	(0.019)	(0.025)	(0.014)	(0.025)	(0.033)	(0.010)	(0.028)	(0.042)	(0.029)	(0.040)	(0.031)
No. Pupils (45 days Bandwidth)	88078	88078	88078	45810	45810	45810	81009	81009	81009	7742	7742	7742
Subsample	All	All	All	Males	Males	Males	High ICSEA	High ICSEA	High ICSEA	Indigenous	Indigenous	Indigenous
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.11 shows estimates of the causal effect of young relative age in secondary school on grade retention, likelihood to be enrolled in Grade 12 and likelihood to obtain a sufficient OP score to be eligible for university in Queensland (i.e., OP < 14) for the full sample, for males, for pupils in schools with High (>=p50) Index of Community Socio-Educational Advantage (ICSEA) and for indigenous pupils. All estimates are obtained from local linear regression specifications, which include distance to the 1st January cutoff, pre-cutoff indicator (Nov-Dec = 1), their interaction term, and cohort fixed effects. A triangular kernel with bandwidth of 45 days is used in all cases. Robust standard errors were clustered at the date of birth level. Control variables included are dummies for whether the pupils are male, whether they are native English speakers and the pupils' primary school ICSEA Index. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.