DISCUSSION PAPER SERIES

IZA

IZA DP No. 9279

School Starting Age and the Crime-Age Profile

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August 2015

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zur Zukunft der Arbeit
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ABSTRACT

School Starting Age and the Crime-Age Profile*

This paper uses register-based data to investigate the effects of school starting age on crime. Through this, we provide insights into the determinants of crime-age profiles. We exploit that Danish children typically start first grade in the calendar year they turn seven, which gives rise to a discontinuity in school starting age for children born around New Year. Our analysis speaks against a simple invariant crime-age profile as is popular in criminology: we find that higher school starting age lowers the propensity to commit crime at young ages. We also find effects on the number of crimes committed for boys.

JEL Classification: I21, K42

Keywords: old-for-grade, school start, criminal charges, violence, property crime

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* Comments from Matthew Lindquist, Lance Lochner, Naci Mocan, Magne Mogstad, Bas van der Klaauw, Peter Sandholt Jensen, two anonymous referees as well as seminar participants at BeNA Humboldt University, Copenhagen University, ISER, University of Essex, University of Sussex, KORA, University of St. Gallen, Xiamn University, the joint University of Bergen & UCL workshop 2013, the workshop on "Economics of Successful Children" at Aarhus University 2013, the University of Chicago's Life Cycle and Inequality working group as well as participants at ESPE 2013, EALE 2013, and ASSA 2014 are appreciated. Financial support from the Danish Council for Strategic Research (CSER, 09-070295) and CIRRAU (Simonsen) is gratefully acknowledged. The usual disclaimer applies.

This paper investigates long-term effects of school starting age on crime while exploiting a discontinuity in school starting age for children born around New Year. Through this, we provide novel insights into the determinants of life-cycle criminal behaviour. The crime-age profile refers to an almost universally observed relationship between crime rates and age, where crime rates increase continuously until around age 18-20 and then decrease for the remainder of the life. We use the mechanical relationship between delayed school entry and delayed life-course to address whether the crime-age relationship is entirely caused by age through biological maturation (Gottfredson and Hirschi, 1990) or can be mediated by the timing of key life experiences (Sampson and Laub, 1995) and thus policy.

A large literature is concerned with effects of school starting age and subsequent educational outcomes and has convincingly shown that starting school later leads to improved test scores (e.g. Bedard and Dhuey, 2006). Black *et al.* (2011) and Crawford *et al.* (2010) refine this type of analysis and show that this result is completely driven by an age-at-test effect: children who start school later are simply older when they perform tests and this leads to better performance. Just as these authors show in the case of test scores, we show that it is important for our analysis whether crime is aligned in terms of age or life-course.

In contrast to existing studies, our paper is concerned with crime outcomes. School starting age may affect crime through several channels. Firstly, to some extent, enrolment leads to locking-in or incapacitation: when youth are in school, they simply have less time to commit crime (Lochner, 2011). Previous studies confirm this mechanism: Jacob and Lefgren (2003) and Luallen (2006) exploit plausibly exogenous changes in number of school days,[1] while Anderson (2014) uses

[1] They study urban schools and find that increasing the number of school days reduces arrests for property crimes but increases arrests for violent crimes. While the effect on property crime is thought to be due to incapacitation, the effect on violent crime is thought to be a network effect (spending more time with criminal peers).

changes in compulsory schooling laws.[2,3] Secondly, postponing school start delays graduation, which may in turn affect opportunity costs of crime at a given age (Grogger, 1998; Uggen, 2000). Thirdly, skill formation and behaviour may play a role. Cunha and Heckman (2008) show that cognitive and (especially) non-cognitive skills at pre-school ages are key determinants of later skill acquisition, behaviour, and adult outcomes. If different school starting ages are associated with different levels of skills ("school readiness"), then these differences may be amplified and affect other outcomes such as the tendency to engage in criminal activities. Lubotsky and Kaestner (2014) find some support for such complementarity as cognitive skills of pupils who are old-for-grade grow faster in kindergarten and first grade, but the gap fades away after first grade. Black *et al.* (2011) show that higher school starting age leads to improved mental health (for boys) and a lower risk of teenage pregnancies (for girls), while there is conflicting evidence regarding the risk of receiving ADHD diagnoses (Elder, 2010; Evans *et al.*, 2010; Dalsgaard *et al.*, 2012). A final channel is the individual's placement in the age hierarchy. Increasing school starting age by one year will most likely move the individual from being one of the youngest to being one of the oldest children in the classroom (e.g. Gaviria and Raphael (2001) and Sacerdote (2001)). However, the potential effects of such a change are ambiguous. On the one hand, having older peers who are more likely to engage in risky behaviour may spark risky behaviour at an earlier stage. On the other hand, having older peers might also increase skill acquisition and maturity, thus lowering delinquent behaviour and improving educational outcomes. Fredriksson and Öckert (2005) and

[2] Anderson estimates that a minimum dropout age of 18 decreases arrest rates for 16-18 year-olds by 17 %. The effect is present for both violent and property crime.

[3] One note of caution is that crime reports to the police may differ according to whether youth are in school or not. If criminal events taking place in school are treated differently than criminal events taking place outside school, this would lead to similar findings. This issue is likely more relevant for violent crime than for property crime or traffic incidents.

Black *et al.* (2013) find no substantial impact of the age composition of peers on educational and labour market outcomes or on teenage pregnancies among girls, and thus rule out that the relative age composition in class explains the impact of school starting age.

Cook and Kang (forthcoming) is the study most similar to ours.[4] They use administrative data for five cohorts of public school children in North Carolina to show that higher school starting age decreases juvenile delinquency but increases serious adult crime. As opposed to our study, their analysis is, however, complicated by grade retention and the existence of compulsory school leaving age legislation that creates a mechanical relationship between school starting age and length of compulsory education.

Our empirical analyses rely on exogenous variation in school starting age generated by administrative rules. In particular, we exploit that Danish children typically start first grade in the calendar year they turn seven, which gives rise to a fuzzy regression discontinuity design. By comparing children born in December with children born in January, we investigate the effects of starting first grade at the age of 6.6 compared to 7.6. Our analysis uses Danish register-based data for children born in the period from 1981-1993 with crucial information on exact birth dates, a range of crime outcomes, and a rich set of background characteristics.

We find that higher age at school start lowers the propensity to commit crime at young ages, but the effects begin to fade out in the 20ies. Hence, the crime-age profile can be modified by life-course and is not only determined by age *per se*. In addition to a delay in the onset of a criminal trajectory, for boys a higher school starting age also causes a persistent reduction in the *number* of crimes

[4] McAdams (forthcoming) also considers the link between school starting age and crime. The paper uses a difference-in-difference model (1960 U.S Census versus 1970 U.S Census) and exploits state level variation in cut-offs to show that a higher school starting age cut-off lowers incarceration.

committed, indicating that the persistence of criminal behaviour is affected by age and criminal opportunity in unison. For boys, a higher school starting age reduces criminal charges significantly until age 19, and the effect is mainly driven by property crime. For girls, a higher school starting age postpones the initiation of crime, and the effect is driven by violent crime. Finally, we find that the relative age of classroom peers does not seem to be behind the reduction in crime.

The paper is structured as follows: Section 1 briefly reviews the Danish institutional set-up and discusses mechanisms through which child behaviour may be affected by school starting age, and Section 2 describes the methodology. Section 3 presents our data, Section 4 the results, and finally Section 5 concludes.

1. Institutional Setting

1.1 Educational Institutions and School Starting Age

During the period relevant for this study, Danish law stipulated that education was compulsory from the calendar year of the child's 7th birthday and until completion of 9th grade.[5] This school system is fortunate for a study like ours because there is no automatic relationship between school starting age and minimum required schooling as there would be in the US and the UK systems, for instance.

[5] The school starting regulations are not strictly enforced and exemptions are granted based on applications from the parents. Exemptions are granted by the local municipality if it is considered beneficial for the child’s development. School start can only be delayed by one year, and school is no longer compulsory from July 31 in the calendar year of the child’s 17th birthday even if 9th grade has not been completed. School children do not pass or fail grades, but in collaboration with the parents, the school principal can decide that a child repeats a grade or jumps a grade again if this is considered beneficial for the child’s development. For more details consult the Danish Law of public schools.

After 9th grade, education was voluntary and could follow an academic path (starting with high school) or a vocational path (starting with vocational school).[6]

The year before entering first grade, children could enrol in a voluntary preschool class. The preschool class, compulsory schooling from 1st to 9th grade and post-compulsory schooling were free of charge. Furthermore, most children below the age of six were inscribed into some form of public child care, which was heavily subsidized.[7]

Parents and administrators have considerable leeway in deciding when children should start school.[8] Therefore, school starting age is not random and is most likely affected by a range of factors that may also correlate with the child's outcomes, behavioural as well as academic. For example, children's overall school readiness and behaviour in preschool are likely to affect the timing of school start.[9] But other factors may impact on the decision as well: as shown by the previous literature, starting later is likely to increase test scores. While this has not been found to impact significantly on long-term outcomes such as earnings,[10] higher grades may improve the consumption value of attending school and allow for a more extensive educational choice set. Finally, there is considerable variation in school starting age culture across municipalities even conditional on a rich set of observable characteristics. For completeness, we will investigate some of these hypotheses towards the end of the paper.

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[6] It was also possible to complete a voluntary 10th grade before continuing on to a vocational or academic path.

[7] A minimum of 67 % of the expenses is covered by the local authorities, c.f. the Danish Law of day care.

[8] UNI-C (2009) documents this and describes background characteristics of children across school starting age.

[9] This pattern is clear from Figure A1, which shows the distributions of social and emotional difficulties at age 4 among punctual and late school starters, drawn from an auxiliary data source (the Danish Longitudinal Survey of Children).

[10] Fredriksson and Öckert (2013) do find earnings gains for individuals with low-educated parents.

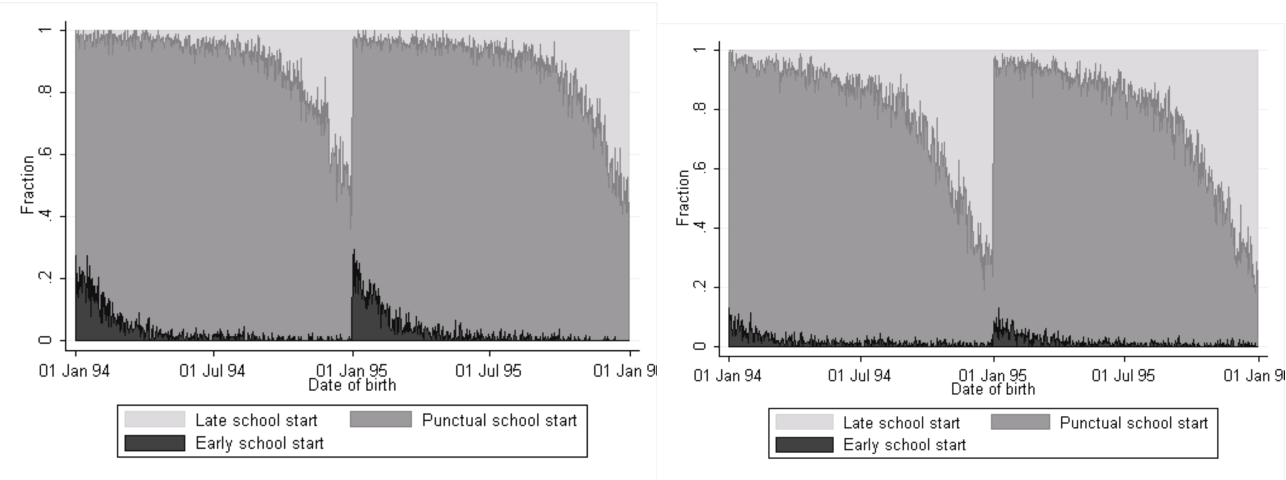
To meaningfully address consequences of school starting age, our empirical analysis will make use of the following observation: because the formal age at school start is defined by the year of birth, each January 1st provides a cut-off point at which children born on each side are subject to a one year difference in timing of administratively determined school start, even though they are born very close in time. Section 2 will formalize this idea. Some parents of children born close to this cut-off date do choose to manipulate their children's actual school starting age: children born at the end of the year are more likely to postpone school start one year, whereas children born early in the year are more likely to start school one year earlier than the law stipulates.[11] In consequence, some children born in December will start school one year later than they are supposed to - approximately at age 7.6 years - whereas the remainder of the children born in December will start when their age is around 6.6 years. Likewise, some children born in January will start school at age 6.6, which is one year earlier than the law stipulates, while the remainder will start school at age 7.6. As shown in Figure 1, school starting age for children born around the cut-off date is effectively reduced to a binary outcome: either children start at age 6.6 or they start at age 7.6. If children born around the cut-off date are 7.6 years old at school start, we label them "old-for-grade". Figure 2 shows the fraction of children who are old-for-grade by date of birth for each gender.

[11] A recent white-paper on school start concluded that "many parents worry whether their children are ready to start school, and these concerns are supported by the preschool staff", cf. Skolestartsudvalget (2006).

Figure 1

Fraction Punctual, Early, and Late School Start

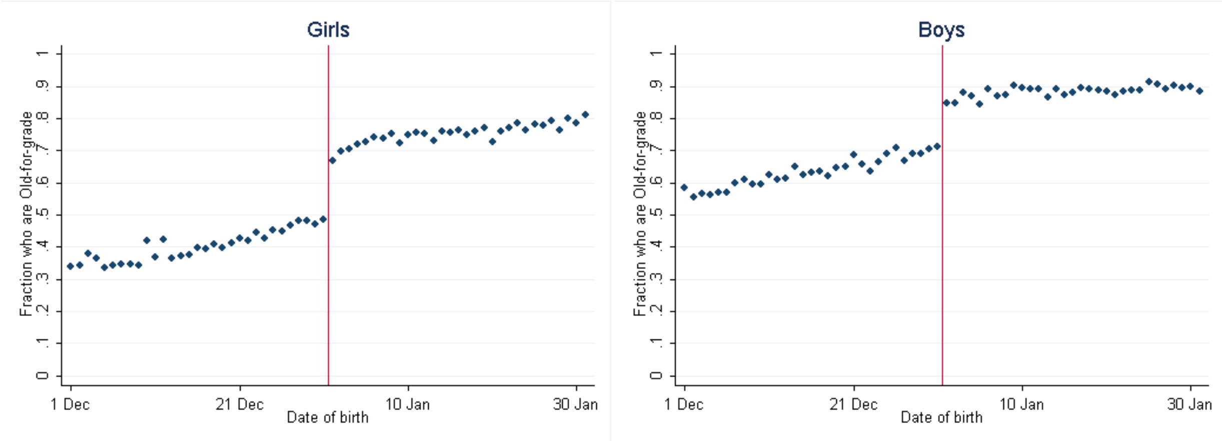
Girls Boys



Note: Figure shows the school starting pattern of the full population of children born January 1 1994 – January 1 1995. Early school start refers to school start the calendar year the child turns 6, punctual school start refers to school start the calendar year the child turns 7, and late school start refers to school start the calendar year the child turns 8.

Figure 2

Fraction Who Are Old-for-grade by Date of Birth



Note: Figure shows the fraction of children who are 'old-for-grade' by date of birth around New Year (marked by the vertical line). Being old-for-grade implies that the child starts school at age 7.6 instead of at age 6.6. Averages for population of children born in December or January from December 1981 to January 1993.

We see that there is a smooth upward trend in the fraction of girls and boys who are old-for-grade in December followed by a large discontinuity around New Year. The figure also shows that boys are much more likely than girls to be old-for-grade.

1.2 Institutions Relevant for Juvenile Crime

Below we describe the institutions that may be relevant for understanding the potential impact of schooling and school starting age, in particular, on criminal activity of teenagers.

In Denmark, the age of criminal responsibility is 15, which is high in an international comparison; England has an age of criminal responsibility of 10, while only few US states have a formal limit and in those cases the limit is 6-12 years.[12] Before their 15th birthday, Danish children cannot be

[12] http://www.unicef.org/pon97/p56a.htm.

arrested, brought to court or imprisoned, although they may be withheld up to 6 hours by the police in which case a social worker must be present during interrogation. This is true regardless of the severity of the crime, and there is no such thing as a youth court.[13] At ages 15-17, youths are considered fully responsible for their criminal acts, and may be imprisoned, though this should be separate from adult prisoners.[14] Thus, the focus is on prevention and rehabilitation rather than prosecution and punishment.

All local authorities have an interdisciplinary framework for prevention of juvenile crime involving the schools, the social services and the police (denoted SSP). This is a network of relevant players who collaborate to understand and prevent juvenile crime in the local area. They are concerned with general, specific as well as individual-oriented policies and interventions.

Reported victimisation rates in Denmark are falling like in the rest of the OECD. However, victimisation rates in Denmark are somewhat higher than in Norway and Sweden and also higher than the OECD average (19 vs. 16 %) while they are almost on par with the US (18 %) and the UK (21 %) rates.[15] Therefore, we have no particular reason to expect that the effects of school starting age on crime should be substantively different in Denmark compared to other countries.

[13] The question of guilt is, in fact, never determined for children below the age of criminal responsibility. The severity of the case is solely considered by the Attorney General.

[14] See the Danish Service Act.

[15] The reported victimisation rates reveal what proportion of a sample of 2000 individuals report that they themselves or persons in their households had experienced one of ten types of conventional crimes (such as vehicle-related crime, theft of personal property and contact crime), see OECD (2009).

2. Methodology

Our goal is to estimate the effect of school starting age (*SSA*) on associated crime outcomes. Our equation of interest is the following:

(1) $$Y_i = \alpha + \beta \cdot SSA_i + X_i'\gamma + \varepsilon_i$$

where Y denotes the outcome, X observable characteristics[16], and ε unobservable characteristics. ε is likely related to the choice of school starting age and would bias results if ignored. To circumvent the problem that *SSA* is not randomly allocated, we formally employ a strategy similar to Elder (2010), Evans *et al.* (2010), Black *et al.* (2011), and Fredriksson and Öckert (2013). In particular, we exploit that school starting rules imply that children born just prior to January 1st are on average younger when they enrol in school than children born immediately after January 1st.

In some sense, we can think about administrative school starting age rules as imposing time and effort costs on parents who choose to enrol their child later (or earlier) than prescribed. We can therefore instrument *SSA* with a dummy for being born immediately after January 1st. As argued by the previous literature, such cut-off dates constitute valid instruments in the sense of being uncorrelated with unobserved characteristics of child outcomes.

Yet, in order to estimate the local average treatment effect – the average effect of being old-for-grade for the group of children who would be inclined to increase their school starting age solely because they were born in January and not December – we also require that the monotonicity assumption is satisfied. Barua and Lang (2012), Aliprantis (2012), and Fiorini *et al.* (2013) argue,

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[16] *X* includes a constant and child and parental characteristics predictive of *SSA* and *Crime*: APGAR score, birth weight, gestation length for children, mother's age at the birth of first child, both parents' education and labour market participation, a flexible function of distance in days to the cut-off, and a constant.

however, that monotonicity is likely violated if the school starting age distribution of children born just after the cut-off date does not stochastically dominate the corresponding distribution for children born just before the cut-off date. In the US example given by Barua and Lang (2012) for children born in the 1950s, children born in the last quarter of the year were on average younger at school start than children born in the first quarter of the year, but the underlying choices were not monotonically related to the cut-off date: while children born in the last quarter of the year were less likely to start school at age 5 compared to children born in the first quarter of the year, they were at the same time less likely to be very young (4 years or younger) and more likely to be very old (6 years or older). Hence, it seems that being born after the cut-off date increases school starting age for some, but reduces it for others.

We do not find this to be an issue in our setting because no children start more than one year before/after the date at which they are supposed to start. This is illustrated by Figure 1 where we show that school starting age in our case is effectively reduced to a binary variable that indicates whether the child enrolls at age 6.6 or 7.6. Always-takers will start at age 7.6 regardless of when they were born, never-takers will start at age 6.6, and compliers will start at age 6.6 if born in December and 7.6 if born in January. For these groups, the instrument monotonically increases school starting age. We assume away defiers: a child whose parents choose to incur the costs of enroling him earlier (at age 6.6) than at the age specified by administrative rules if born in January but at the same time are willing to bear the additional costs of enroling him later (at age 7.6) than at the age specified by administrative rules if born in December. This type of behaviour would both be inconsistent with parental preferences for the child being among the youngest in his classroom as well as preferences for him being the oldest.

In practice, we consider a short bandwidth with children born ± 30 days around January 1st. In our main specification, we model *SSA* as a binary variable indicating a school starting age of 7.6 as opposed to 6.6. Along with various standard robustness tests, we show that results are robust to modelling *SSA* as a continuous variable. This will be discussed further in Section 4 below.

3. Data

We use Danish register-based data for children born in the period from mid-1981 until mid-1993 with crucial information on exact birth dates, charges[17] for property crime, violence and other types of crime (in particular traffic incidents), together with the specific dates of crime, and a rich set of background characteristics.

3.1 Crime Outcomes

As is true for most of the existing literature on school starting age, choosing the right outcome is a challenge: on the one hand, one wants to align children in terms of age. This is particularly relevant because crime is positively correlated with age in the age range considered in this paper. On the other hand, one wants to align children in terms of length of education because the agents that decide a child's school starting age may focus on these outcomes or because education may have a direct effect on the tendency to commit crime. The main point is that children who start school later are more likely to still be enroled in school when outcomes are aligned by age, but not when outcomes are aligned by grade level. To address these issues, our main outcomes consist of age-specific measures, but we also separately consider criminal charges at a given point in the educational cycle.

We consider two types of age-specific crime measures: one outcome measures whether an individual has been charged with a 'crime *at* a given age' from age 15 and onwards. This is a

[17] Using charges instead of convictions enables us to use three additional years of data. This is because of the time involved in processing cases through courts and because of the right to appeal that may delay the establishment of a conviction. Conclusions are robust to using convictions instead of charges.

memoryless measure which simply informs about the tendency to commit crime at any given age (i.e. from one birthday to the next). It is particularly useful for detecting sudden changes in the crime-age profile caused by school starting age. Our other type of outcome measures whether an individual has been charged with a 'crime *at or before* a given age' and in this way keeps track of earlier incidences. This is convenient if one wants to address more permanent effects on crime. Because of considerable recidivism,[18] both measures are required to give a full picture of the consequences of school starting age on the crime-age curve. We might see negative effects of school starting age on 'crime at a given age' but not on 'crime at or before a given age' if those committing the crime are simply the same individuals. Conversely, we could see effects on 'crime at or before a given age' and not on 'crime at a given age' if school starting age has a longer-lasting effect on criminal behaviour. It is clearly important to be able to distinguish between these scenarios.

Due to space considerations some of our descriptive analyses and all robustness tests will focus on the accumulated outcome, but the full set of descriptives and formal results are available on request. In addition to our main analyses, sub-analyses show results for types of crime (*property crime*, *violent crime*, and the residual *other crime*) and number of crimes to address differential effects on the intensive and extensive margin. Figure A2 illustrates means of our main outcome variables. The figure replicates the well-known age pattern where criminal activity peaks at ages 19-20 (Gottfredson and Hirschi, 1990). For girls, 2 % are charged with a crime at ages 19-20, while for boys 11 % are charged with a crime at ages 19-20, after which age the fraction declines. All over the age range, the proportion charged with a crime is higher for individuals who are old-for-grade compared to individuals who are young-for-grade. Our empirical analysis will reveal to what extent this reflects selection.

[18] Recidivism accounts for between 20 % and 67 % of crimes at a given age for boys and between 20 % and 90 % for girls, depending on the exact age.

Table 1

Means of Selected Outcome Variables by Types of Crime

Criminal charge (0/1) at or before age:	Girls Any	Property	Violence	Other	Observations	Boys Any	Property	Violence	Other	Observations
15	0.019	0.018	0.002	0.001	48,546	0.039	0.033	0.006	0.007	50,383
16	0.032	0.029	0.003	0.003	48,546	0.081	0.059	0.014	0.029	50,383
17	0.044	0.037	0.005	0.006	48,546	0.140	0.081	0.025	0.077	50,383
18	0.054	0.043	0.007	0.010	48,546	0.189	0.101	0.036	0.119	50,383
19	0.069	0.049	0.009	0.021	43,668	0.243	0.118	0.047	0.174	45,368
20	0.083	0.053	0.010	0.034	39,037	0.290	0.131	0.057	0.222	40,606
21	0.096	0.055	0.010	0.045	34,559	0.327	0.141	0.065	0.262	36,012
22	0.106	0.056	0.011	0.057	30,209	0.358	0.147	0.070	0.295	31,405
23	0.116	0.056	0.011	0.068	26,093	0.383	0.151	0.074	0.323	26,937
24	0.124	0.057	0.012	0.075	22,125	0.402	0.154	0.076	0.345	22,781
25	0.133	0.057	0.012	0.086	18,240	0.417	0.156	0.079	0.362	18,723
26	0.138	0.057	0.012	0.092	14,630	0.429	0.156	0.081	0.375	14,949
27	0.143	0.057	0.011	0.100	11,045	0.439	0.157	0.082	0.388	11,273

Note: Table shows average crime charges for population of children born in December or January from December 1981 to January 1993, by gender and month of birth.

To give a better sense of the nature of the crime committed, Table 1 summarizes the distribution of crime at or before a given age across three types of crime: property crime, violent crime, and other crime.[19] Throughout the age distribution, boys are three times more likely to have been charged with a crime than girls. At the youngest ages, property crimes tend to be most prevalent, but after age 18 when the individuals in the sample gradually acquire a driver's license, other crimes including traffic incidents accumulate. For girls, other crime dominates from age 22 onwards, while for boys it dominates already from age 18.

Violent crimes comprise the most severe types. The most common examples are ordinary assaults, aggravated assaults, threats, and violence towards public servants. 80 % of convictions for violence result in imprisonment or a suspended sentence for boys and 67 % for girls. Property crimes and

[19] Due to space considerations, we have chosen only three broad categories of crimes. Different classifications would be possible.

other crime are typically less severe crime. The most frequent examples of property crime are shoplifting, burglary, and vandalism. A quarter of all convictions for property crime result in imprisonment or a suspended sentence for boys, while this number falls below 10 % for girls. The category of other crimes is dominated by traffic related crime (50 % for girls and 90 % for boys) such as driving a car without a license, while the second largest category is drug or weapon related crime (e.g. selling drugs or possession of illegal weapons). Convictions for other crimes rarely lead to imprisonment.

Table A1 shows mean crime outcomes by birth-month and gender. Those born in December tend to be more likely to have been charged with a crime compared to those born in January. When we consider whether an individual has been charged with a crime at a given age, we see that boys born in December are more likely to have been charged with a crime at each age from 15 to 21, while the pattern is more scattered for girls (top panel). This outcome will be important for our analysis of incapacitation. When we consider the accumulated measure, namely whether or not the individual has been charged with a crime at or before a given age, we see that the difference is significant up until age 24 (bottom panel). As argued above, the accumulated outcome is more informative about potential catching up effects and other long-run effects.

Figure A3 shows the standard regression-discontinuity plots for the accumulated crime measures at ages 19 and 27. The figure reveals a discontinuity in the raw outcome variable which is only statistically significant for boys at age 19.

3.2 Measuring School Starting Age

Unfortunately, we do not have information on the specific timing of school starting age for the cohorts in question. Instead we use age in 8th grade as an approximation. We do observe children's exact ages at all grade levels from 2007 and onwards, and we use this data to check that the approximation of school starting age by age in 8th grade works very well (see Table A2). The vast majority of children who are old-for-grade at the end of elementary school are old-for-grade already in preschool class, while very few children are delayed from the first grade and onwards.[20] In addition, there is no relationship between the cut-off and being held back or skipping grades during primary school.

3.3 Background Characteristics

Using the registers we combine information on the children's birth weight, gestational length, APGAR score[21], demographic variables, educational variables, and crime by the unique individual identification number. In addition, we link these data to information about parents' education and labour market outcomes as measured one year prior to the child's birth. Importantly, we centre all covariates and outcome variables on the cut-off dates instead of by calendar year. Hence, we compare background information on children born in January year t to the information on children born in December year t-1 instead of comparing information on children born in January year t to the information on children born in December year t.[22] Similarly, outcome variables aligned by age for individuals born on each side of the cut-off date are measured at the same point in time: exactly one's birthday.

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[20] As an additional check we report results from the first stage regression at various grade levels by use of these recent data. Measurement errors in school starting age will impact on our results to the extent that they are correlated with the instrument. If children born in December are more likely to repeat a grade as suggested by Elder and Lubotsky (2009), our results will be biased towards zero.

[21] The APGAR score ranges from zero to 10 and summarizes the health of a new-born child based on five simple criteria: Appearance, Pulse, Grimace, Activity and Respiration.

[22] For children born in December 1981 or January 1982 we use parental characteristics measured in 1980, while we for children born in December 1982 or January 1983 use parental characteristics measured in 1981 etc.

Table 2 shows joint F-tests from a regression of the instrument on the rich set of background variables for children born ± 30 days around January 1st for girls and boys separately. These tests clearly suggest that the sample is balanced across the cut-off.[23]. We include all variables as covariates and bound potential biases by the approach by Nevo and Rosen (2012); see discussion in Section 4.

Table 2

Balancing Tests

	Girls	Boys
F-statistic	0.53	0.7
p-value	0.94	0.81
Observations	48,546	50,383
Distance to cut-off	X	X
Covariates	X	X

Note: Table shows F-statistics and associated p-values from regressions of birth-month (January=1) on the full set of covariates (background characteristics as presented in Table A3 and cohort fixed effects) as well as distance to cut-off in days.

[23] Figure A4 shows the variation of selected control variables on either side of the cut-off. We find no significant differences across the cut-off for any of the control variables, which further strengthens the credibility of the empirical approach.

4. Results

4.1 Timing of Birth within the Calendar Year and School Starting Age

Table 3 presents our first stage results, using a dummy for birth in January as instrument for school starting age. The table shows the first stage results estimated both with and without controls. All specifications include cohort fixed effects (indicator variables for being born Dec 1981-Jan 1982, Dec 1982-Jan 1983 etc.) and the distance in days to the cut-off linearly.[24] Remaining estimates may be found in Table A4 in the Appendix.

Table 3

First Stage Estimation Results

	Girls		Boys	
	(1)	(2)	(3)	(4)
January=1	0.245***	0.244***	0.172***	0.171***
	(0.008)	(0.008)	(0.007)	(0.007)
Days to cut-off, December	-0.005***	-0.005***	-0.005***	-0.005***
	(0.000)	(0.000)	(0.000)	(0.000)
Days to cut-off, January	0.003***	0.003***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.714***	2.282***	0.872***	1.540***
	(0.006)	(0.368)	(0.005)	(0.321)
Cohort fixed effects	X	X	X	X
Background characteristics		X		X
Observations	48,546	48,546	50,383	50,383
F-value for January dummy	877.29	891.59	574.01	576.22

Note: Table shows results from linear regressions of indicators for starting school at age 7.6 instead of 6.6 for children born in December or January while conditioning on the cut-off dummy (January=1), distance to cut-off, cohort fixed effects and background characteristics (see Table A3). * p<0.05, ** p<0.01, *** p<0.001.

[24] Results are robust to including more flexible polynomials of the running variable.

In line with Figure 2, we see that the instrument strongly predicts whether children start school at age 7.6 or 6.6: children born in January are significantly more likely to be relatively old when they start school compared to children born in December, and the effect is large. This is despite the tendency for some children born in December to delay enrolment and start at age 7.6 instead. The instrument is highly significant and the associated F-statistic easily passes the Staiger-Stock rule-of-thumb.[25, 26]

4.2 Crime Results: 2SLS

4.2.1 The propensity to commit crime

Figure 3 shows our main estimation results; see also the corresponding Table A6. The left hand side figures show the estimation results for crime at a given age, and the right hand side figures show the estimation results for crime at or before a given age (conditional on the full set of covariates). We find that being old-for-grade leads to a significant reduction in the propensity to commit crime at each age until age 19 for boys but only at age 15 for girls. Point estimates at older ages are primarily negative for girls but become very close to zero for boys.[27] We cannot formally detect statistically significant differences in coefficient estimates across ages because confidence bands are

---

[25] With one endogenous variable and one instrument, F should be greater than 10.

[26] As discussed above, our measure of school starting age is based on age in grade 8 rather than actual age at school start. In Table A5, we use the richer data from 2007 onwards to demonstrate that the first stage is literally unaffected by this approximation. For individuals born Dec 2000-Jan 2001, we can compare first stage as measured by being old-for-grade in preschool versus 2nd grade, while for individuals born Dec 1996-Jan 1997, we can compare first stage as measured by being old-for-grade in 4th versus 8th grade. While the first stage differs slightly across cohorts, it does not differ by grade level and thus supports the approximation.

[27] This is not driven by the smaller sample for which we observe outcomes at all ages. The estimate profile is in fact similar if we only include individuals born in 1985 or before.

too wide. Note that individuals who are young-for-grade turn 15 during their final year in comprehensive school, while individuals who are old-for-grade turn 16. Individuals who are young-for-grade turn 18 or 19 during their final year in high school, while individuals who are old-for-grade turn 19 or 20 (depending on whether they took the optional 10th grade or not). Formal analyses of the impact of being old-for-grade on enrolment and completed years of schooling at a given age show that a main effect of being old-for-grade is postponement of the educational cycle (see Figures A5 and A6). While short-run effects are large and fluctuate widely at different steps of the education-ladder, long-term effects of school starting age on educational outcomes are small or zero (in line with *Black et al.* (2011)). We therefore interpret the age pattern in our crime results as supportive of the incapacitation hypothesis. It suggests that compulsory school is protective against crime for girls, while also high school is protective against crime for boys.

When we instead look at the propensity to commit crime *at or before* a given age in Figure 3, we find a statistically significant effect for girls until age 19. After age 19, estimates are again primarily negative but imprecise. Hence, it seems that for girls, a higher school starting age initially reduces crime, and we see no catching up at older ages. Estimates for boys are significant until age 22, after which they become very close to zero. For boys therefore, we see a longer-lasting initial effect that eventually fades. The fading effect suggests that crime at the extensive margin is aligned to key life events rather than age. If criminal behaviour instead was fixed to age, any effects of school starting age on crime should shift the crime-age profile in vertical direction. However, Figure 3 shows that the effects fade in the long run when old-for-graders' and young-for-graders' educational attainment and life-course converge, which is consistent with a horizontal shift in the crime-age profile. Moreover, the fact that the effects only approach zero from below suggests that the crime-age profile is shifted in both vertical and horizontal direction.

The ‘delay’ in crime for late starters is large relative to the mean. The share of girls with any criminal charges at or before age 18, for example, is 0.054 among children born ± 30 days around January 1st. The effect of starting school at age 7.6, in comparison, is a 1.5 percentage points reduction, or just below 30 % of the mean. For boys, the effect of starting school at age 7.6 on criminal charges at age 18 is a 4 percentage points reduction, which should be seen relative to a share of boys with criminal charges of 0.19. Appendix A, Table A6 shows detailed estimation results where we gradually add control variables.

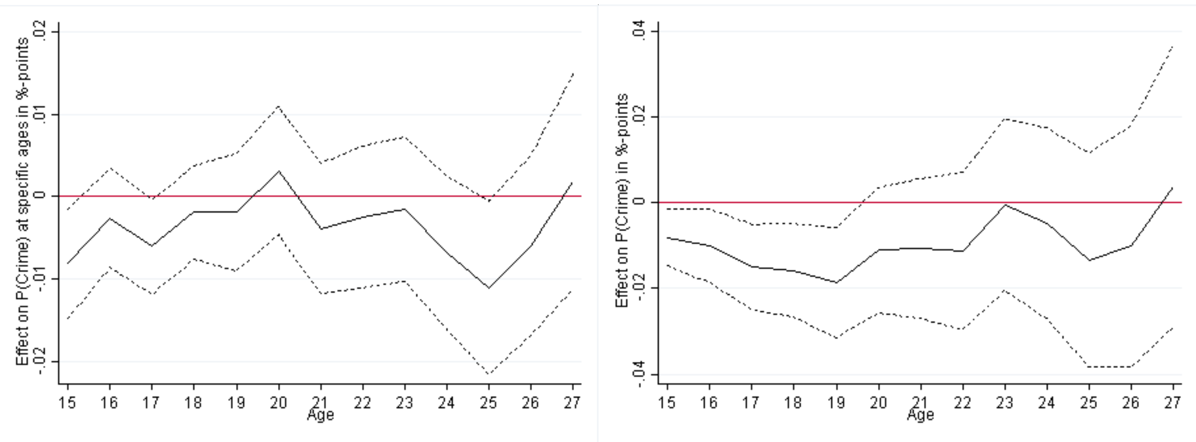
Our results are robust to standard robustness checks: extended bandwidth, “donut hole” strategies or including polynomials of the assignment variable. In addition, we perform a robustness check computing bounds according to Nevo and Rosen (2012), which allows for imperfect instruments. Our results are robust although the confidence bands are much wider. Finally, results are unchanged by modelling *SSA* as a continuous variable instead of a binary variable indicating late (7.6 years) as opposed to early (6.6 year) school start. Appendix B reports a selection of these robustness checks for the outcome crime *at or before* a given age.

Figure 3

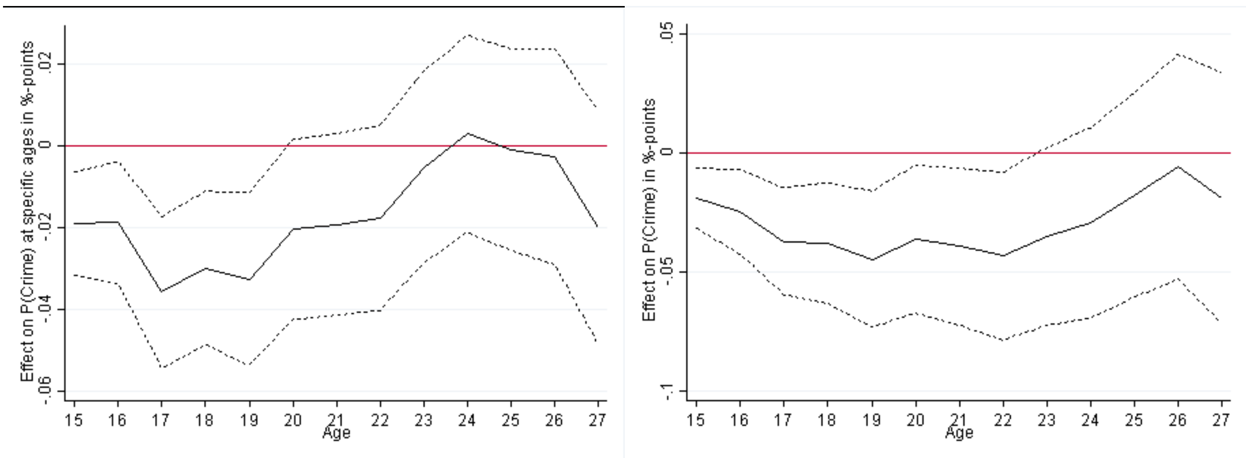
Estimation Results: Crime across Age

At age | At or before age

Girls



Boys



Note: Figures show the estimated effects of being old-for-grade based on 2SLS regressions of the probability of crime at (left) and at or before (right) a given age. Cut-off dummy (January=1) used as instrument. Conditioning set includes distance to cut-off, cohort fixed effects, and background characteristics (see Table A3). Dashed lines indicate 95 % confidence intervals.

4.2.2 Types of crime

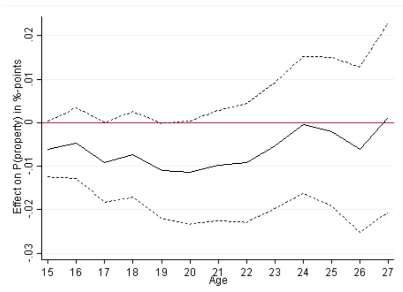
In Figure 4 we distinguish between types of crime. For girls the significant effects of school starting age on crime at or before a given age are mainly driven by the impact on violent crimes, while for boys the effects are primarily driven by the impact on property crimes, although the effects on the two other categories of crime are significant for some ages.

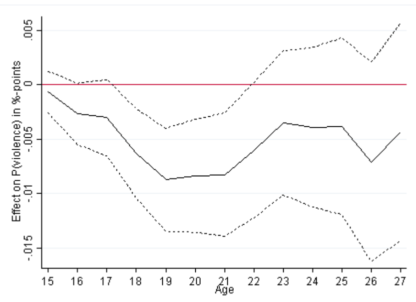
Figure 4

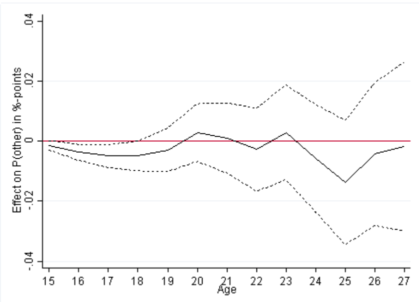
Estimation Results: Types of Crime

Property Violence Other

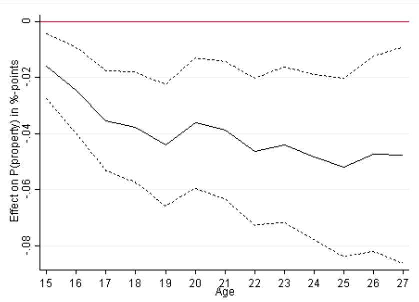
Girls

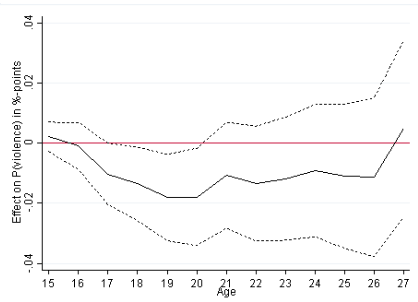


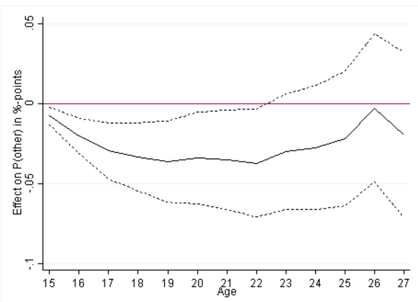




Boys







Note: Figures show the estimated effects of being old-for-grade based on 2SLS regressions of the probability of property, violent, and other crime at (left) and at or before (right) a given age. Cut-off dummy (January=1) used as instrument. Conditioning set includes distance to cut-off, cohort fixed effects, and background characteristics (see Table A3). Dashed lines indicate 95 % confidence intervals.

4.2.3 Grade alignment

Just as Black *et al.* (2011) show in the case of test scores, the way we align crime is extremely important for our conclusions. In particular, when age is held constant, the differences between individuals born before and after the cut-off reflect differences in age at school start, educational attainment and the probability to be enroled in school. On the other hand, when outcomes are aligned by grade levels, the differences reflect the effects of age at school start, age at measurement, and time of measurement. Here we use the grade level that individuals are expected to attend given their timing of school start.

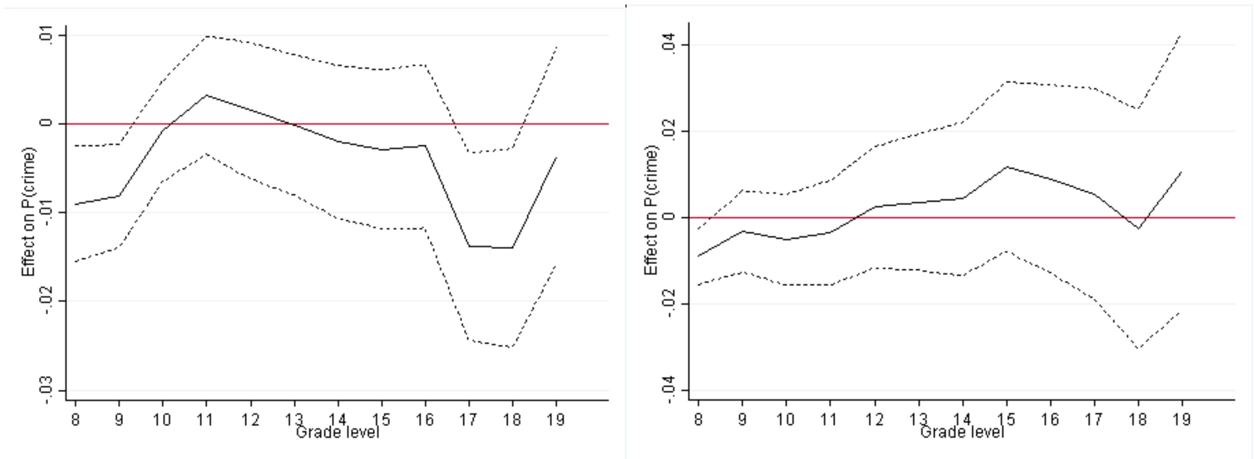
Figure 5 shows results that align children in terms of grades instead of age. The left hand side figures show the estimation results for crime at a given grade, and the right hand side figures show the estimation results for crime at or before a given grade (conditional on the full set of covariates). If the effects of school starting age only originated from delayed life-course, aligning grade should nullify the effects. The figure shows that the age-gradient is indeed smaller than for the age-aligned crime (though not significantly so); it nullifies the effect at some but not all grade levels (left part of figure). For girls, the effect of school starting age on crime is significantly negative for the two final years in comprehensive school (grade levels 8 and 9). This corresponds to the significantly negative results at age 15 in our main analysis above. For boys the effect is only significant at the transitions from one level to the next in the educational cycle (grade levels 10 and 13). For none of the genders the effects on the accumulated crime measure are significant (right part of figure). These results speak in favour of our interpretation of the results presented in Figure 3; being old-for-grade primarily lowers crime due to changes in the timing of life events, though we also see some signs of lower crime *per se* around the transitions.

Figure 5

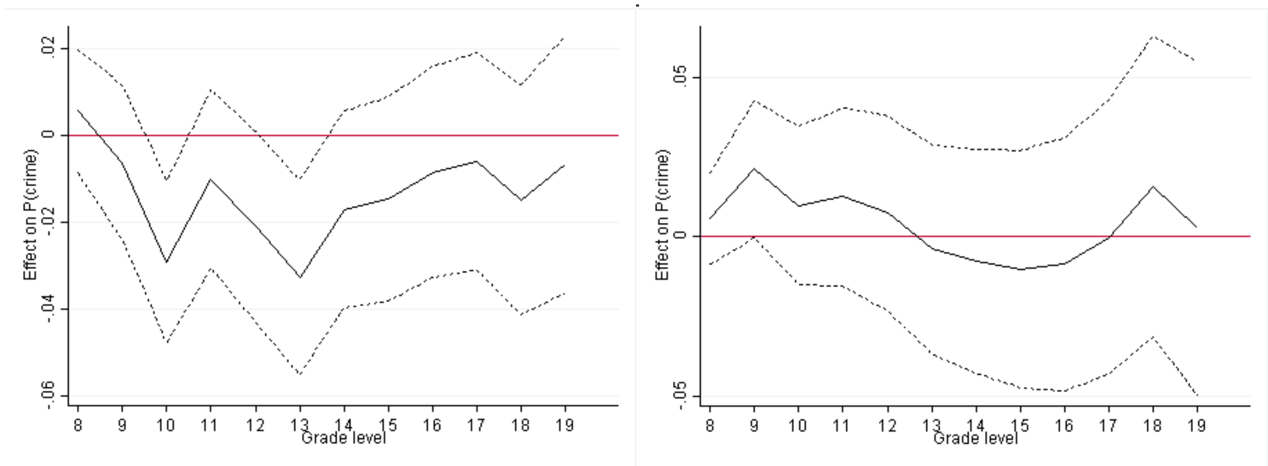
Estimation Results: Crime across School Grades

At grade At or before grade

Girls



Boys



Note: Figures show the estimated effects of being old-for-grade based on 2SLS regressions of the probability of crime at (left) and at or before (right) a given grade level (years since actual school start). Cut-off dummy (January=1) used as instrument. Conditioning set includes distance to cut-off, cohort fixed effects, and background characteristics (see Table A3). Dashed lines indicate 95 % confidence intervals.

4.2.4 Number of criminal charges

In Figure 6, we consider the effects on crime at the intensive margin. The figure presents the effects of increasing school starting age on the number of charges at or before a given age. The estimates for girls are significant in the same age range as the results for the indicator variable above. This is likely because the majority of girls only commit very few crimes, and it suggests that the school starting age mainly influences the extensive margin. For boys, however, this exercise reveals interesting additional insights that were not clear from the simple indicator analysis: estimates are much larger and effects last long into the twenties. In their mid-twenties young men who started school later as a consequence of being born in January have been charged with half a crime less than those who did not. This is a substantial effect which has large consequences for both the offenders and potential victims. Moreover, these large and persistent effects also show that on the intensive margin, the crime-age profile for boys is more strongly related to age than the extensive margin crime-age profile which we investigated earlier.[28] Criminal behaviour is thus not only determined by either age or key events but by both in interaction; it matters at what age one is exposed to different key events.

4.3 Heterogeneity

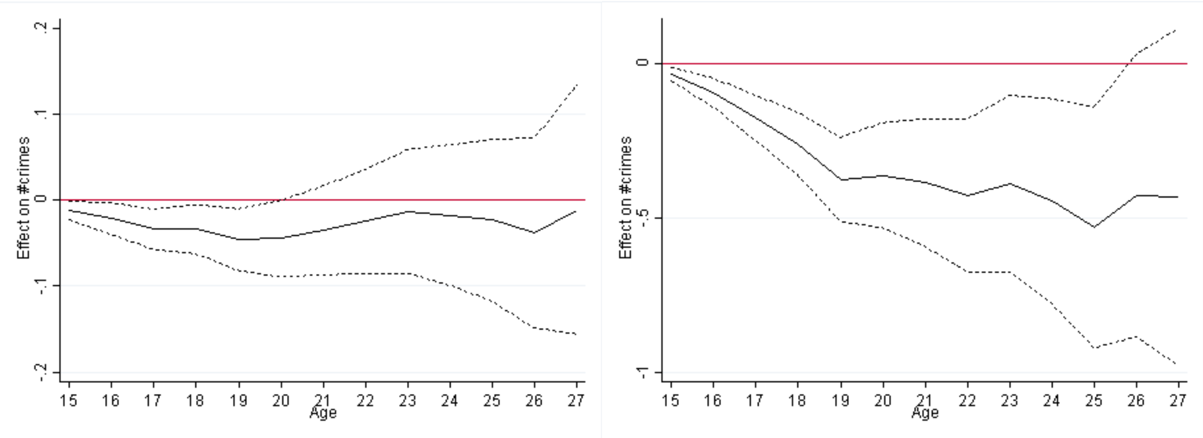
In this section we first investigate characteristics of those individuals who comply with the instrument and then study whether effects vary by subgroups defined by parental background.

[28] Even when we align outcomes by grade levels, we see a negative significant effect of being old-for-grade on the accumulated number of crimes at grade levels 11 to 14 for boys (available on request).

Figure 6

Estimation Results: Number of Crimes at or before age

Girls Boys



Note: Figures show the estimated effects of being old-for-grade based on 2SLS regressions of the number of crimes before or at a given age. Cut-off dummy (January=1) used as instrument. Conditioning set includes distance to cut-off, cohort fixed effects, and background characteristics (see Table A3). Dashed lines indicate 95 % confidence intervals.

4.3.1 Complier characteristics

In Table 4 below we summarize the average characteristics of the compliers (those who shift to being old-for-grade because of a change in the value of the instrument; see Abadie (2003)) along with the average characteristics of the full sample as comparison. Families who change the school start decision as a consequence of being born in January rather than December tend to have more favourable characteristics: parents are more often living together, birth weight is higher, and parents have higher education and stronger attachment to the labour market.

Table 4

Complier Characteristics

Variable	Girls			Boys		
	Sample mean	Complier mean	t-stat	Sample mean	Complier mean	t-stat
Immigrant	0,04	0,02	4,99 ***	0,04	0,02	3,91 ***
Parents married/cohabiting	0,79	0,81	-2,72 **	0,79	0,83	-3,45 ***
Apgar score=9	0,18	0,17	1,39	0,19	0,17	1,25
Apgar score=8	0,07	0,06	2,74 **	0,07	0,08	-2,39 *
Apgar score lower	0,08	0,09	-0,12	0,10	0,08	1,78
Birth weight, grams	3349,43	3414,11	-5,90 ***	3473,43	3589,40	-7,21 ***
Gestational length, weeks	39,55	39,61	-1,59	39,47	39,62	-2,67 **
Mother:						
- Months of schooling	137,41	139,08	-2,49 *	137,75	142,92	-5,87 ***
- Completed HS or equivalent	0,29	0,29	0,08	0,30	0,34	-3,51 ***
- Unemployed	0,13	0,11	3,00 **	0,13	0,10	2,31 *
- Out of the labour force	0,11	0,10	1,29	0,11	0,09	1,59
- Age at birth of first child	24,85	24,91	-0,76	24,92	25,18	-2,18 *
Father:						
- Months of schooling	140,15	143,26	-3,85 ***	140,38	146,86	-5,71 ***
- Completed HS or equvalent	0,19	0,18	2,58 **	0,20	0,23	-3,29 ***
- Unemployed	0,08	0,07	1,67	0,07	0,06	2,28 *
- Out of the labour force	0,06	0,04	4,51 ***	0,06	0,04	3,26 **

Note: Standard errors are bootstrapped, * indicates p < 0.05, ** p < 0.01, and *** p < 0.001

Note: Table shows selected mean characteristics of the full sample and compliers (i.e. those who are induced to be 'old-for-grade' because they are born on January 1st and not December 31st) following Abadie (2003). Standard errors calculated from 100 bootstraps. * p<0.05, ** p<0.01, *** p<0.001.

Figure 7 presents heterogeneous effects. When we divide the sample according to mother's education, we find that the effect of school starting age on crime is numerically smaller and insignificant when mothers have completed at least 12 years of education, which is what we would expect. However, when we divide the sample according to the employment status of the father, the picture is different: for girls, the effect of school starting age on crime is numerically smaller and insignificant when fathers are *not* employed, while for boys, the effects are of the same magnitude in either case.

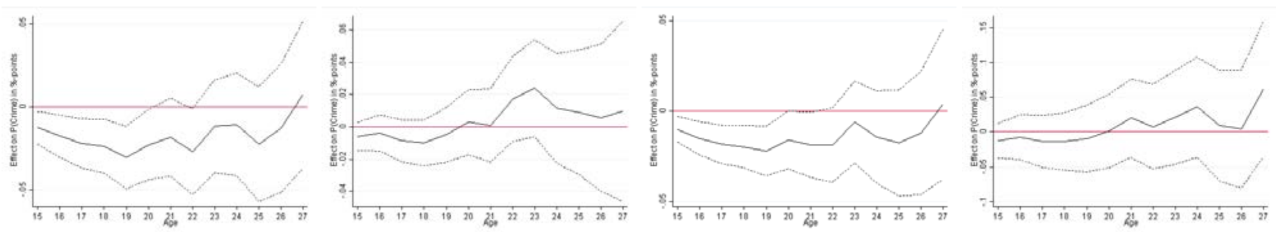
Altogether, the heterogeneity analysis suggests that individuals with favourable parental background characteristics respond *more* to the instrument, but the gains in terms of crime of increasing school starting age are also smaller compared to individuals with less favourable background characteristics. Conversely, individuals with less favourable parental background characteristics respond *less* to the instrument – they are more likely always 7.6 years old at school start regardless of their birthday – but they also gain more from starting at a higher age.

Figure 7

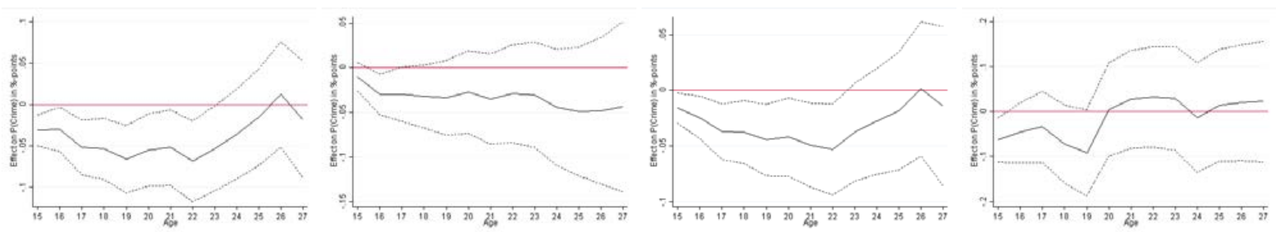
Estimation Results: Crime at or before Age X by Sub-group

Mother <12 vs. ≥12 years of education   Father employed vs. not employed

Girls



Boys



Note: Figures show the estimated effects of being old-for-grade based on 2SLS regressions of the probability of crime at or before a given age by mother's education (≥12 years of education (40 %) vs. <12 years of education (60 %)) and father's employment status (employed (86 %) vs. not employed (14 %)). Cut-off dummy (January=1) used as instrument. Conditioning set includes distance to cut-off, cohort fixed effects, and background characteristics (see Table A3). Dashed lines indicate 95 % confidence intervals.

4.4 Potential Mechanisms and Effects on Alternative Outcomes

This section first attempts to shed light on some of the different channels through which school starting age may affect crime outcomes. Specifically, we further investigate the importance of incapacitation and also consider the role played by the relative age of peers as in Black *et al.* (2013). We next address effects on an alternative range of outcomes. We discussed above that parents may choose to enrol their child later in school even if there are no long-run effects on income, for example. Because school starting age is linked to grades, it may also be linked to the quality of and consumption value associated with the type of degree. Finally, municipality based variation in culture may impact on parents' choices.

We first address incapacitation. In Figure A7, we investigate how school starting age affects crime across the week. For boys, the effect tends to be driven by crime committed during weekdays (Mon-Fri) and to a smaller extent by crime committed during weekends (Sat-Sun). For girls, the effect during the weekdays is not statistically different from zero, while it is for weekends until age 19. We interpret these findings as supportive of an incapacitation effect for boys throughout high school, which the age pattern of the main results already pointed at above. For girls the effects are smaller and the mechanism is more subtle: the effect is driven by violent crimes and crimes taking place during weekends, and the effect dies out at a younger age. Thus, it appears that girls who are old-for-grade are less likely to be involved in this type of crime while they are still in school. Notice, however, that these conclusions should be viewed with caution because the effects are not statistically significantly different.

Table A7 analyses the effect of the age of peers in line with Fredriksson and Öckert (2005) and Black *et al.* (2013). Formally, we include the average age of peers in one's school in 8th grade as an additional control variable in our models of crime outcomes. To handle endogeneity of the average age of peers, we instrument with the predicted average age of peers *had everybody started on time*.[29] We see that the mean age of peers has no statistically significant effect on crime outcomes and that the effect of own school starting age is completely unaffected by the inclusion of this extra control variable.[30] This is in line with the findings in the mentioned previous studies for Norway and Sweden.

We finally investigate other mechanisms that may explain why parents choose to postpone school start even when effects on children's primary long-term outcomes are moderate in size.

Table 5 shows the estimated effects of being old-for-grade on alternative outcomes such as grades and type of degree that may enhance the consumption value of school for children and parents. We find that the impact of school starting age on standardized math grades is statistically significant and large in magnitude.[31] This is in line with previous studies supporting large age-at-test effects (Crawford *et al.*, 2010; Black *et al.*, 2011). If such grades make a difference for educational preferences or feasible choices, they may influence long-term outcomes. Indeed, we do see that girls are more likely to enrol in one of the selective and competitive Medical Schools, while boys tend to obtain a slightly longer education if they are old-for-grade as a consequence of being born on the other side of the cut-off. Organisation (or effort) grades are not affected for boys or girls.

[29] We impute average age of peers for observations with fewer than 10 other children enrolled at the school in grade 8.

[30] Compare the results presented in Table A6B to those in Table A7.

[31] Danish grades are not affected (not shown).

To investigate the variation in the enforcement of the stipulated school starting age across municipalities, we look at the distribution of predicted school starting age using a rich set of observable characteristics against the actual school starting age across municipalities. Results are available upon request. We find little relationship between the predicted and actual school starting pattern on municipal level. Moreover, in ten percent of all municipalities, less than 68 % (49 %) of all boys (girls) born +/-30 days from the cut-off are old-for-grade, while in another ten percent of all municipalities more than 84 % (67 %) are old-for-grade conditional on observables. These numbers would be 50 % if all families followed the stipulated school starting rule. This suggests that local school start culture and legal enforcement of the regulations may play a role for the parents' decision.

Table 5

Effects of School Starting Age on Other Outcomes

Variable	Girls			Boys		
	OLS	2SLS	2SLS	OLS	2SLS	2SLS
Grades						
- Math, standardized	-0.028*	0.502***	0.510***	-0.052***	0.356***	0.295***
	(0.011)	(0.101)	(0.093)	(0.014)	(0.145)	(0.135)
- Effort, standardized	-0.116***	0.036	0.065	-0.093***	0.201	0.195
	(0.011)	(0.090)	(0.088)	(0.015)	(0.150)	(0.146)
Observations	27.909	27.909	27.909	27.974	27.974	27.974
Years of schooling, age 27	-0.448	-0.048	-0.045	-0.335***	0.234**	0.165
	(0.050)	(0.136)	(0.124)	(0.052)	(0.147)	(0.135)
Observations	11,045	11,045	11,045	11,273	11,273	11,273
College enrolment						
- P(Med School)	-0.007***	0.026*	0.026*	-0.006***	0.019	0.019
	(0.001)	(0.011)	(0.011)	(0.001)	(0.011)	(0.011)
- P(Law School)	-0.007***	-0.013	-0.011	-0.003	-0.006	-0.006
	(0.002)	(0.013)	(0.013)	(0.001)	(0.012)	(0.012)
Observations	30.209	30.209	30.209	31.405	31.405	31.405
Controls						
Distance to cut-off		X	X		X	X
Covariates	X		X	X		X

Note: Table shows the estimated effects of being old-for-grade based on 2SLS regressions of alternative education outcomes. Cut-off dummy (January=1) used as instrument. Conditioning set includes distance to cut-off, cohort fixed effects, and background characteristics (see Table A3).
* p<0.05, ** p<0.01, *** p<0.001.

5. Conclusion

This paper uses Danish register-based data to investigate the effect of school starting age on crime-age profiles while using exogenous variation in school starting age generated by administrative rules. We find that a higher school starting age lowers the propensity to commit crime in youth. In addition, boys experience a persistent reduction in the *number* of crimes committed. We show that crime at the extensive margin is largely driven by life events, whereas crime at the intensive margin is a complex function of both age and life-course.

Detailed studies of the age-profile of the effects indicate that the reductions to crime are likely to be caused by an incapacitating effect of schooling, as those who start school later graduate later. Although not directly testable, the pattern of results supports this hypothesis: Boys who are old-for-grade are less likely to be charged during the period until age 19, and this effect mainly stems from property crime. Girls who are old-for-grade are less likely to be charged until age 16, and this effect stems from violent crimes. For boys we find significant effects of school starting age on the accumulated number of crimes at or before a given age throughout the twenties. For girls the effects on accumulated crime measures die out, which suggests that school starting age only influences the criminal debut. For boys mainly property crime is reduced, while for girls violent crime is reduced. We also find that the effects are not caused by relative age of peers but by one's own school starting age.

Our results suggest that increasing school starting age could lower crime – more so for boys than for girls. Yet, our findings do not necessarily suggest that school starting age should be increased for everybody: our heterogeneity analyses show, for example, that children born to parents with favourable characteristics gain relatively little from an increase in school starting age. More fundamentally, we only analyse school start choice at the individual level and not a policy change influencing all children.

Appendix A. Supplementary material

Table A1

Means of Outcome Variables across Cut-off

Any criminal charge (0/1)			Girls				Boys	
at age:	December	January	Difference	Observations	December	January	Difference	Observations
15	0.021	0.017	0.003**	48546	0.042	0.036	0.007***	50383
16	0.016	0.015	0.001	48546	0.058	0.051	0.007***	50383
17	0.015	0.013	0.002*	48546	0.093	0.081	0.012***	50383
18	0.014	0.013	0.001	48546	0.095	0.084	0.010***	50383
19	0.021	0.020	0.001	43668	0.114	0.102	0.012***	45368
20	0.021	0.022	-0.001	39037	0.117	0.108	0.009**	40606
21	0.021	0.019	0.002	34559	0.112	0.103	0.008**	36012
22	0.022	0.020	0.001	30209	0.105	0.097	0.008	31405
23	0.020	0.019	0.001	26093	0.101	0.097	0.005	26937
24	0.020	0.018	0.003	22125	0.093	0.092	0.002	22781
25	0.023	0.018	0.005*	18240	0.085	0.082	0.003	18723
26	0.019	0.016	0.002	14630	0.081	0.077	0.004	14949
27	0.019	0.020	-0.001	11045	0.080	0.071	0.009	11273

Any criminal charge (0/1)			Girls				Boys	
at or before age:	December	January	Difference	Observations	December	January	Difference	Observations
15	0.021	0.017	0.003***	48546	0.042	0.036	0.007***	50383
16	0.035	0.03	0.004**	48546	0.086	0.076	0.010***	50383
17	0.047	0.041	0.006***	48546	0.147	0.133	0.014***	50383
18	0.057	0.051	0.006***	48546	0.196	0.181	0.015***	50383
19	0.073	0.065	0.008***	43668	0.252	0.234	0.018***	45368
20	0.086	0.081	0.005	39037	0.298	0.282	0.017***	40606
21	0.098	0.093	0.005	34559	0.336	0.318	0.018***	36012
22	0.109	0.104	0.005	30209	0.368	0.348	0.020***	31405
23	0.116	0.115	0.001	26093	0.391	0.374	0.018***	26937
24	0.125	0.123	0.003	22125	0.410	0.395	0.016**	22781
25	0.136	0.130	0.006	18240	0.423	0.41	0.013	18723
26	0.140	0.136	0.004	14630	0.433	0.424	0.009	14949
27	0.142	0.144	-0.001	11045	0.446	0.433	0.013	11273

Note: Table shows fraction of individuals who have been charged with a crime at (upper panel) and at or before (lower panel) a given age by gender. Population of children born December 1981 to January 1993. T-test for difference in means across month of birth: * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table A2

Fraction of Students Being Retained at Each Grade Level

Grade level	Fraction delayed/retained
Preschool	0.136
1st grade	0.014
2nd grade	0.003
3rd grade	0.004
4th grade	0.003
5th grade	0.003
6th grade	0.003
7th grade	0.002
8th grade	0.003
9th grade	0.005

Note: Calculations based on grade levels from 2007 and onwards.

Table A3

Sample Means

Birth weight, grams	3349	3474
Gestational length, weeks	39,55	39,47
Mother:		
- Months of schooling	137,4	137,7
- Completed HS or equvalent	0,290	0,296
- Unemployed	0,128	0,125
- Out of the labour force	0,108	0,105
- Age at birth of first child	24,9	24,9
Father:		
- Months of schooling	140,1	140,4
- Completed HS or equvalent	0,190	0,199
- Unemployed	0,077	0,075
- Out of the labour force	0,064	0,062
Observations	48.546	50.383

Note: Averages for population of children born in December or January from December 1981 to January 1993.

Table A4

First Stage Results (Supplement to Table 3 in Main Text)

	Girls		Boys	
	(1)	(2)	(3)	(4)
January=1	0.245***	0.244***	0.172***	0.171***
	(0.008)	(0.008)	(0.007)	(0.007)
Apgar score=9		0.005		-0.001
		(0.005)		(0.005)
Apgar score=8		0.012		0.001
		(0.008)		(0.007)
Apgar score lower		0.011		0.004
		(0.008)		(0.006)
Immigrant or descendant		-0.041***		-0.070***
		(0.012)		(0.011)
Birth weight		-0.000***		-0.000***
		(0.000)		(0.000)
Gestational length		-0.070***		-0.034*
		(0.019)		(0.017)
Gestational length, squared		0.001**		0.000+
		(0.000)		(0.000)
Mother years of schooling		0.001***		0.001***
		(0.000)		(0.000)
Mother years of schooling, squared		-0.000***		-0.000***
		(0.000)		(0.000)
Father years of schooling		0.001**		0.001***
		(0.000)		(0.000)
Father years of schooling, squared		-0.000***		-0.000**
		(0.000)		(0.000)
Mother completed high school or equivalent		-0.026***		-0.005
		(0.006)		(0.005)
Father completed high school or equivalent		-0.065***		-0.051***
		(0.007)		(0.006)

Table A4 continued

First Stage Results (Supplement to Table 3 in Main Text)

	Girls		Boys	
	(1)	(2)	(3)	(4)
Mother unemployed		0.020**		0.007
		(0.006)		(0.006)
Mother not in labour force		0.002		-0.025***
		(0.007)		(0.006)
Father unemployed		0.017*		0.001
		(0.008)		(0.007)
Father not in labour force		0.009		0.009
		(0.009)		(0.008)
Mother's age at first child		0.002***		0.004***
		(0.001)		(0.001)
Parents are married		0.001		0.002
		(0.005)		(0.005)
Days to cutoff, December	-0.005***	-0.005***	-0.005***	-0.005***
	(0.000)	(0.000)	(0.000)	(0.000)
Days to cutoff, January	0.003***	0.003***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.714***	2.282***	0.872***	1.540***
	(0.006)	(0.368)	(0.005)	(0.321)
Cohort fixed effects	X	X	X	X
Background characteristics		X		X
Parental covariates		X		X
Observations	48,546	48,546	50,383	50,383
F-value for January dummy	877,29	891,59	574,01	576,22

Note: Table shows results from linear regressions of indicators for starting school at age 7.6 instead of 6.6 while conditioning on cut-off dummies (January=1), distance to cut-off, cohort fixed effects and background characteristics (see Table A3). * p<0.05, ** p<0.01, *** p<0.001.

Table A5

First Stage Results at Different Grade Levels

	Girls				Boys			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Preschool	2nd grade	4th grade	8th grade	Preschool	2nd grade	4th grade	8th grade
	Born December 2000 January 2001		Born December 1996 January 1997		Born December 2000 January 2001		Born December 1996 January 1997	
January=1	0.249***	0.251***	0.231***	0.215***	0.129***	0.134***	0.112***	0.109***
	(0.024)	(0.024)	(0.023)	(0.023)	(0.017)	(0.018)	(0.018)	(0.018)
Days to cut-off, December	-0.004***	-0.005***	-0.005***	-0.005***	-0.004***	-0.004***	-0.004***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Days to cut-off, January	0.002*	0.003**	0.003**	0.003**	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	0.809***	0.792***	0.795***	0.797***	0.941***	0.941***	0.923***	0.928***
	(0.016)	(0.017)	(0.017)	(0.017)	(0.013)	(0.013)	(0.013)	(0.013)
Observations	4977	4977	5264	5264	5328	5328	5433	5433
F-value for January dummy	280.692	287.416	275.321	262.962	162.943	173.456	146.018	132.187

Note: Table shows results from linear regressions of indicators for starting school at age 7.6 instead of 6.6 while conditioning on the cut-off dummy (January=1), distance to cut-off, cohort fixed effects and background characteristics (see Table A3). School starting age is measured at different grade levels in the period 2007-2013. * p<0.05, ** p<0.01, *** p<0.001.

Table A6A

Detailed Estimation Results, Crime at or before Age

Age	Girls				Boys			
	OLS	2SLS	2SLS	Observations	OLS	2SLS	2SLS	Observations
15	-0.005***	-0.009**	-0.008*	48546	-0.005*	-0.026***	-0.019**	50383
	(0.001)	(0.003)	(0.003)		(0.002)	(0.007)	(0.006)	
16	-0.003	-0.011*	-0.010*	48546	-0.009***	-0.036***	-0.025**	50383
	(0.002)	(0.004)	(0.004)		(0.003)	(0.009)	(0.009)	
17	-0.003	-0.017**	-0.015**	48546	-0.006	-0.053***	-0.037**	50383
	(0.002)	(0.005)	(0.005)		(0.004)	(0.012)	(0.012)	
18	-0.003	-0.018**	-0.016**	48546	-0.002	-0.057***	-0.038**	50383
	(0.002)	(0.006)	(0.006)		(0.004)	(0.013)	(0.013)	
19	-0.003	-0.021**	-0.019**	43668	0.001	-0.068***	-0.045**	45368
	(0.002)	(0.007)	(0.007)		(0.005)	(0.015)	(0.015)	
20	-0.001	-0.013	-0.011	39037	0.000	-0.060***	-0.036*	40606
	(0.003)	(0.008)	(0.007)		(0.005)	(0.016)	(0.016)	
21	-0.004	-0.013	-0.011	34559	0.004	-0.063***	-0.039*	36012
	(0.003)	(0.009)	(0.008)		(0.005)	(0.018)	(0.017)	
22	-0.005	-0.014	-0.011	30209	0.004	-0.070***	-0.043*	31405
	(0.004)	(0.010)	(0.009)		(0.006)	(0.019)	(0.018)	
23	-0.002	-0.002	-0.000	26093	0.008	-0.059**	-0.035	26937
	(0.004)	(0.010)	(0.010)		(0.006)	(0.020)	(0.019)	
24	-0.001	-0.007	-0.005	22125	0.005	-0.050*	-0.030	22781
	(0.004)	(0.012)	(0.011)		(0.007)	(0.021)	(0.020)	
25	-0.002	-0.016	-0.013	18240	0.003	-0.04	-0.018	18723
	(0.005)	(0.013)	(0.013)		(0.008)	(0.023)	(0.022)	
26	-0.004	-0.011	-0.010	14630	0.005	-0.027	-0.006	14949
	(0.006)	(0.015)	(0.014)		(0.009)	(0.025)	(0.024)	
27	0.001	0.004	0.004	11045	0.004	-0.037	-0.019	11273
	(0.007)	(0.017)	(0.017)		(0.010)	(0.028)	(0.027)	
Cohort fixed effects	X	X	X		X	X	X	
Distance to cut-off		X	X			X	X	
Background characteristics	X		X		X		X	

Note: Table shows the estimated effects of being old-for-grade based on 2SLS regressions of crime at or before a given age. Cut-off dummy (January=1) used as instrument. Conditioning set includes distance to cut-off, cohort fixed effects, and background characteristics (see Table A3).

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table A6B

Detailed Estimation Results, Crime at Age

Age	Girls				Boys			
	OLS	2SLS	2SLS	Observations	OLS	2SLS	2SLS	Observations
15	-0.005***	-0.009**	-0.008*	48546	-0.005*	-0.026***	-0.019**	50383
	(0.001)	(0.003)	(0.003)		(0.002)	(0.007)	(0.006)	
16	0.002	-0.003	-0.003	48546	-0.007**	-0.027***	-0.019*	50383
	(0.001)	(0.003)	(0.003)		(0.002)	(0.008)	(0.008)	
17	0.001	-0.006*	-0.006*	48546	0.001	-0.045***	-0.036***	50383
	(0.001)	(0.003)	(0.003)		(0.003)	(0.010)	(0.009)	
18	0.001	-0.002	-0.002	48546	0.004	-0.040***	-0.030**	50383
	(0.001)	(0.003)	(0.003)		(0.003)	(0.010)	(0.010)	
19	0.001	-0.002	-0.002	43668	0.002	-0.045***	-0.033**	45368
	(0.001)	(0.004)	(0.004)		(0.003)	(0.011)	(0.011)	
20	0.002	0.003	0.003	39037	0.002	-0.032**	-0.021	40606
	(0.002)	(0.004)	(0.004)		(0.004)	(0.011)	(0.011)	
21	-0.001	-0.005	-0.004	34559	0.011**	-0.030**	-0.019	36012
	(0.002)	(0.004)	(0.004)		(0.004)	(0.012)	(0.011)	
22	0.001	-0.002	-0.002	30209	0.003	-0.029*	-0.018	31405
	(0.002)	(0.004)	(0.004)		(0.004)	(0.012)	(0.012)	
23	0.002	-0.002	-0.002	26093	0.006	-0.015	-0.005	26937
	(0.002)	(0.005)	(0.004)		(0.004)	(0.012)	(0.012)	
24	-0.001	-0.007	-0.007	22125	0.009*	-0.005	0.003	22781
	(0.002)	(0.005)	(0.005)		(0.004)	(0.012)	(0.012)	
25	-0.000	-0.012*	-0.011*	18240	0.005	-0.010	-0.001	18723
	(0.002)	(0.005)	(0.005)		(0.004)	(0.013)	(0.013)	
26	-0.000	-0.006	-0.006	14630	-0.002	-0.011	-0.003	14949
	(0.002)	(0.006)	(0.006)		(0.005)	(0.014)	(0.013)	
27	-0.000	0.002	0.002	11045	0.002	-0.027	-0.020	11273
	(0.003)	(0.007)	(0.007)		(0.005)	(0.015)	(0.015)	
Cohort fixed effects	X	X	X		X	X	X	
Distance to cut-off		X	X			X	X	
Background characteristics	X		X		X		X	

Note: Table shows the estimated effects of being old-for-grade based on 2SLS regressions of crime at a given age. Cut-off dummy (January=1) used as instrument. Conditioning set includes distance to cut-off, cohort fixed effects, and background characteristics (see Table A3).

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table A7

Absolute and Relative Effects of School Starting Age on Crime, at or before Age

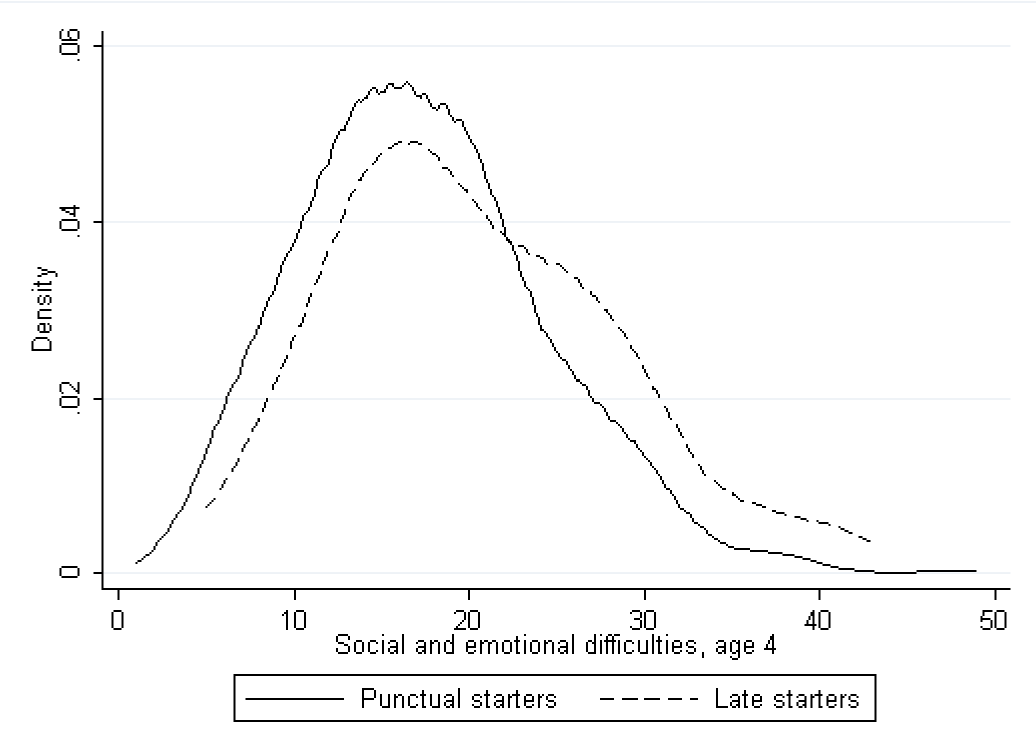
Age	Girls			Boys		
	Old-for-grade	Peer age	Observations	Old-for-grade	Peer age	Observations
15	-0.008*	0.014	48.546	-0.019**	0.002	50.383
	(0.003)	(0.029)		(0.006)	(0.044)	
16	-0.010*	-0.033	48.546	-0.025**	-0.017	50.383
	(0.004)	(0.037)		(0.009)	(0.062)	
17	-0.015**	-0.011	48.546	-0.037**	-0.058	50.383
	(0.005)	(0.043)		(0.012)	(0.078)	
18	-0.016**	-0.006	48.546	-0.038**	0.049	50.383
	(0.006)	(0.048)		(0.013)	(0.088)	
19	-0.018**	-0.060	43.668	-0.046**	0.116	45.368
	(0.007)	(0.057)		(0.015)	(0.101)	
20	-0.010	-0.063	39.037	-0.037*	0.065	40.606
	(0.008)	(0.064)		(0.016)	(0.112)	
21	-0.009	-0.113	34.559	-0.041*	0.093	36.012
	(0.008)	(0.071)		(0.017)	(0.119)	
22	-0.010	-0.116	30.209	-0.044*	0.027	31.405
	(0.009)	(0.080)		(0.018)	(0.129)	
23	0.001	-0.075	26.093	-0.036	0.054	26.937
	(0.010)	(0.088)		(0.019)	(0.136)	
24	-0.002	-0.124	22.125	-0.030	0.022	22.781
	(0.012)	(0.100)		(0.021)	(0.146)	
25	-0.008	-0.230*	18.240	-0.017	-0.058	18.723
	(0.013)	(0.109)		(0.022)	(0.159)	
26	-0.006	-0.152	14.630	0.003	-0.321	14.949
	(0.015)	(0.122)		(0.025)	(0.177)	
27	0.005	-0.030	11.045	-0.009	-0.321	11.273
	(0.018)	(0.142)		(0.028)	(0.201)	
Cohort fixed effects	X			X		
Distance to cut-off	X			X		
Background characteristi	X			X		

Note: Table shows the estimated effects of being old-for-grade based on 2SLS regressions of crime at or before a given age. Cut-off dummy (January=1) used as instrument for own school starting decision, predicted school starting age of peers if compliant with rules used as instrument for average peer age. In addition to average peer age, conditioning set includes distance to cut-off, cohort fixed effects, and background characteristics (see Table A3).

* p<0.05, ** p<0.01, *** p<0.001

Figure A1

Age 4 Social and Emotional Difficulties among Punctual and Late School Starters



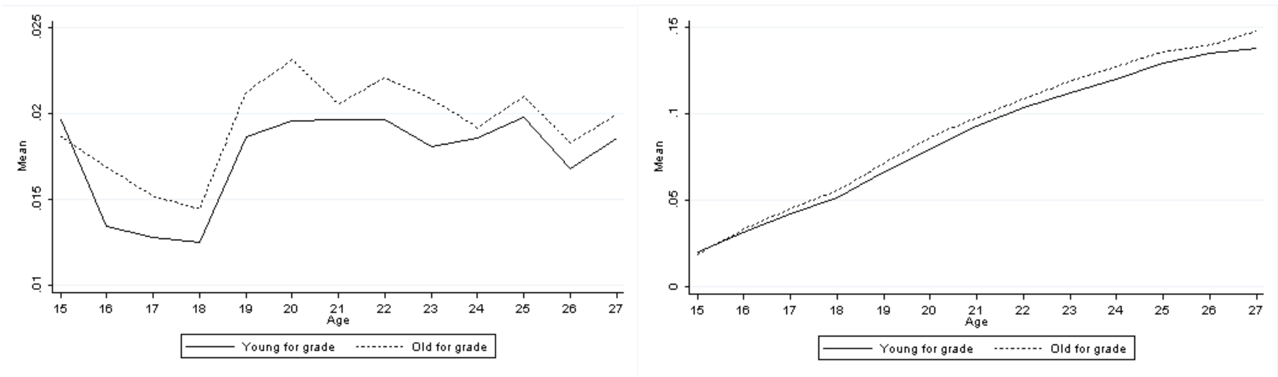
Note: Figure shows kernel density estimates of social and emotional difficulties scores at age 4 by school starting age. Data stem from the Danish Longitudinal Survey of Children that surveys children born in September and October of 1995. 'Punctual school starters' obey the rules and start school when they are supposed to start according to the rules, while 'late school starters' have been granted an exemption.

Figure A2

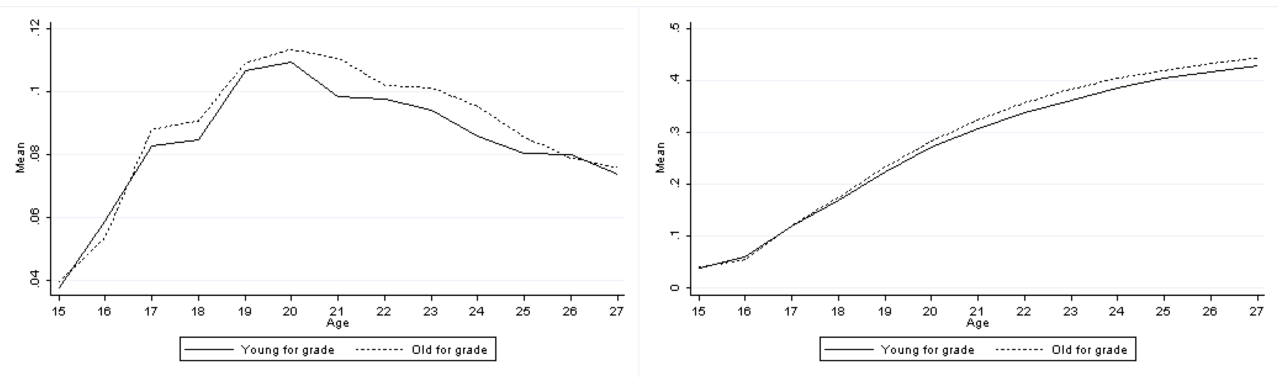
Main Outcome Variables: Crime across Ages

Any crime at given age | Any crime before or at age

Girls



Boys



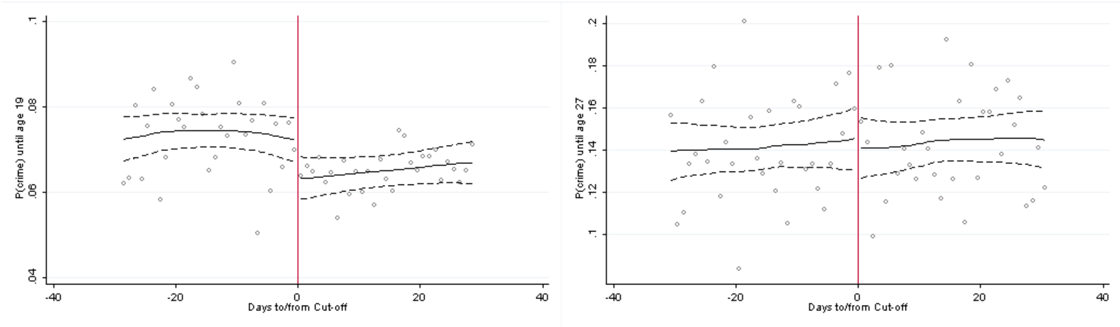
Note: Figures show 'Crime-Age Profiles': The fraction of individuals who have been charged with a crime at (left) and at or before (right) a given age. Population of children born 1981-1993

Figure A3

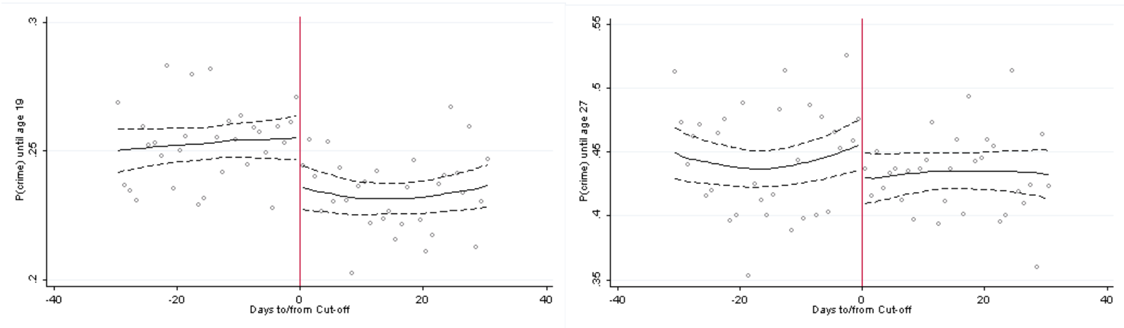
Crime at or before Age

Age 19 Age 27

Girls



Boys



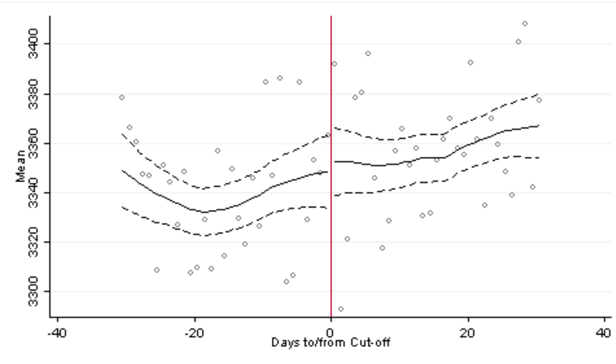
Note: Figures show scatterplots of crime at or before a given age **(**19 and 27**)** by date of birth. The solid line is a local polynomial smoothed line, and the corresponding dashed lines indicate 95 % confidence intervals.

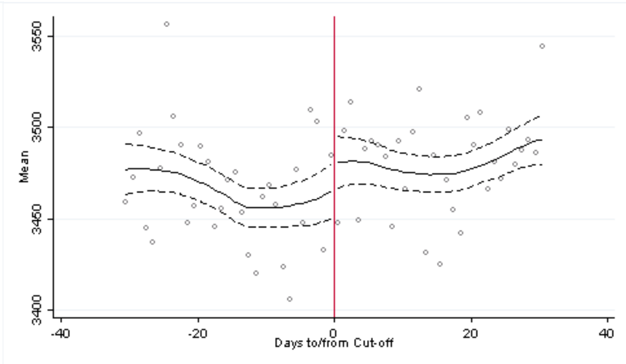
Figure A4

Control Variables

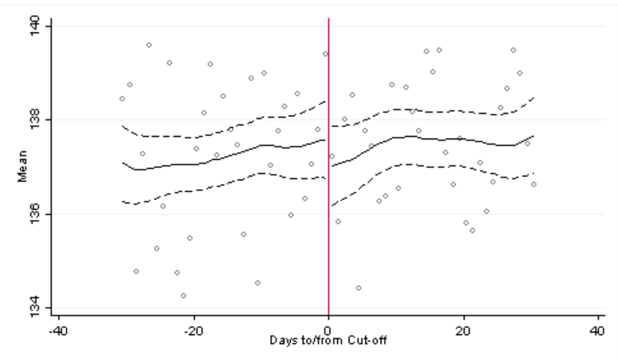
Girls Boys

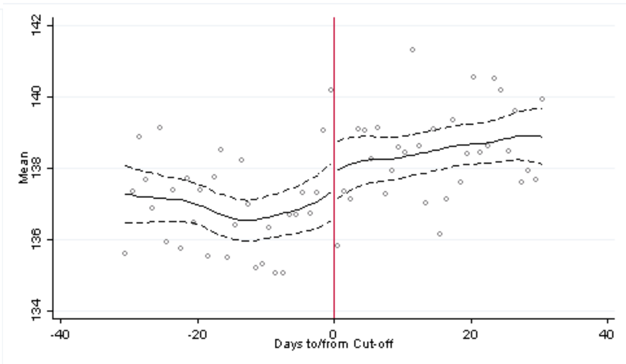
Birth weight



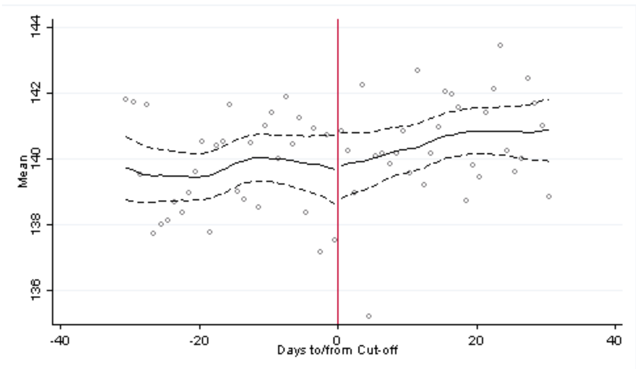


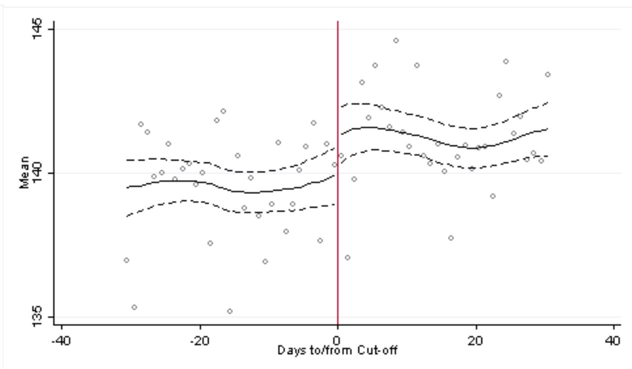
Mother's months of schooling





Father's months of schooling



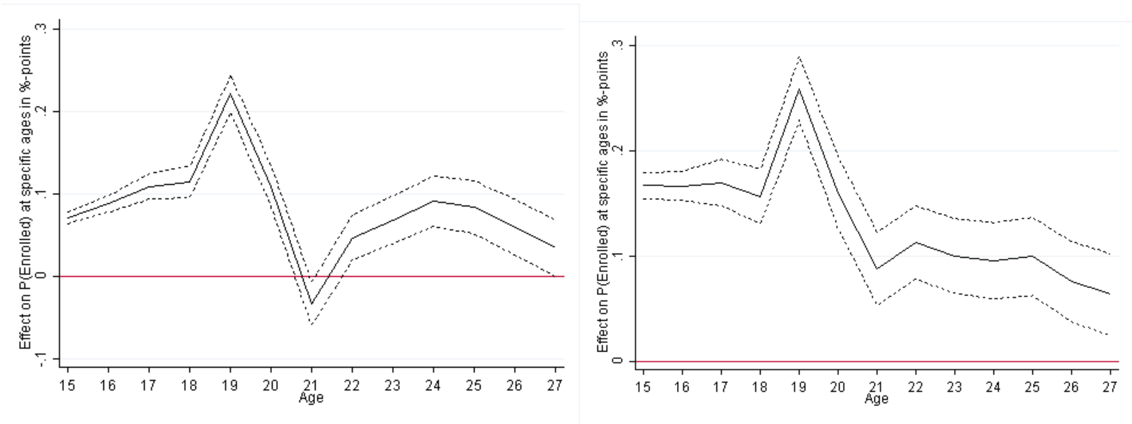


Note: Figures show scatterplots of selected variables by date of birth. The solid line is a local polynomial smoothed line and the corresponding dashed lines indicate 95 % confidence intervals.

Figure A5

Estimation Results: Enrolment in Education

Girls | Boys

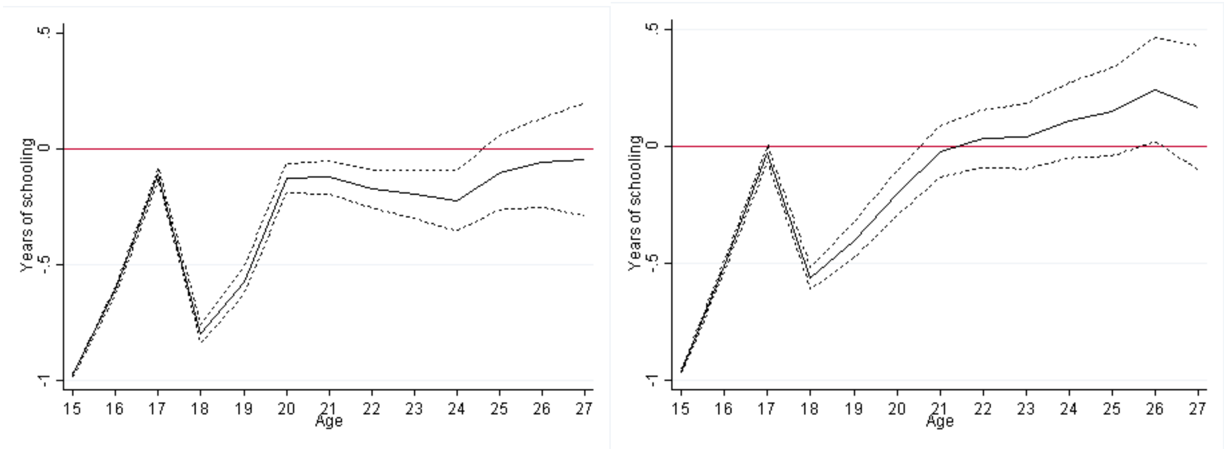


Note: Figures show the estimated effects of being old-for-grade based on 2SLS regressions of enrolment in education. Cut-off dummy (January=1) used as instrument. Conditioning set includes distance to cut-off, cohort fixed effects, and background characteristics (see Table A3). Dashed lines indicate 95 % confidence intervals.

Figure A6

Estimation Results: Years of Completed Schooling

Girls Boys



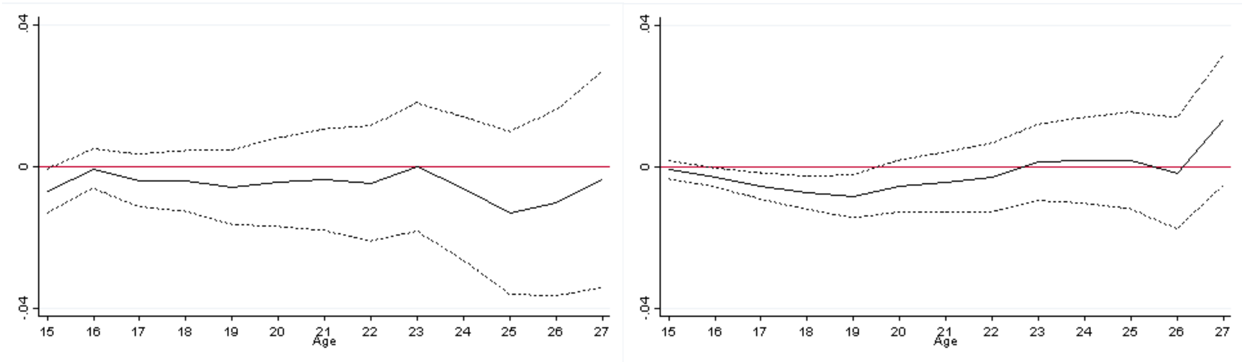
Note: Figures show the estimated effects of being old-for-grade based on 2SLS regressions of years of completed schooling. Cut-off dummy (January=1) used as instrument. Conditioning set includes distance to cut-off, cohort fixed effects, and background characteristics (see Table A3). Dashed lines indicate 95 % confidence intervals.

Figure A7

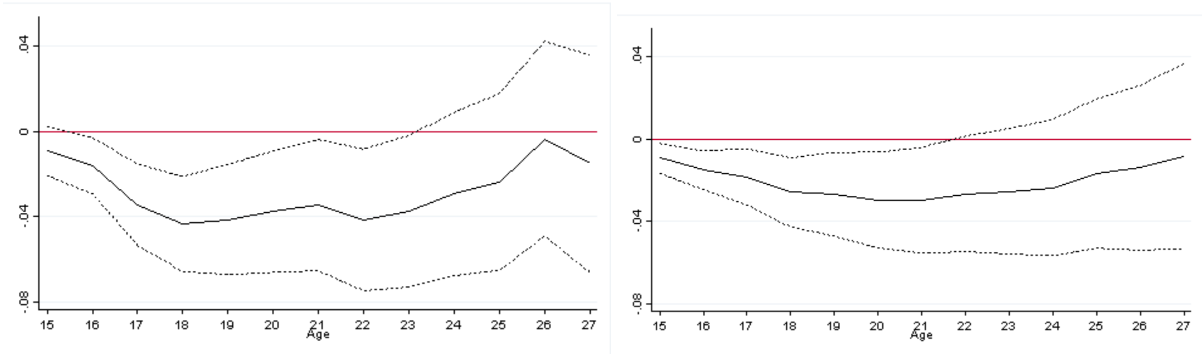
Estimation Results: Crime across the Week

Weekdays	Weekends

Girls



Boys



Note: Figures show the estimated effects of being old-for-grade based on 2SLS regressions of the probability of crime by weekdays (Mon-Fri) and weekends (Sat-Sun). Cut-off dummy (January=1) used as instrument. Conditioning set includes distance to cut-off, cohort fixed effects, and background characteristics (see Table A3). Dashed lines indicate 95 % confidence intervals.

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Additional Supporting Information may be found in the online version of this article:

Appendix B. Alternative approaches

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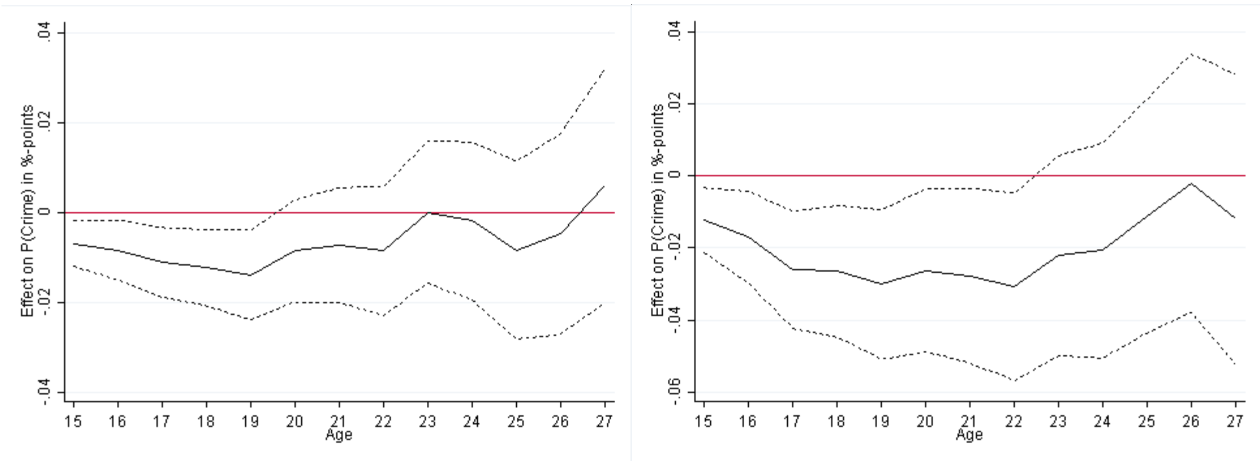
Appendix B. Alternative approaches (online appendix only)

Figure B1

Estimation Results: Continuous School Starting Age

Girls Boys



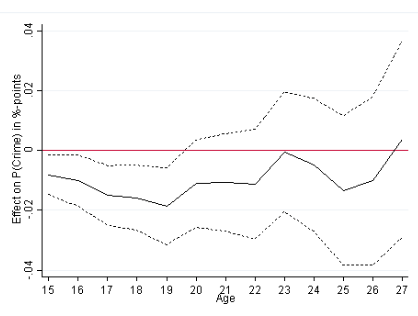
Note: Figures show the estimated effects of continuous school starting age based on 2SLS regressions of the probability of crime at or before a given age. Cut-off dummy (January=1) used as instrument. Conditioning set includes distance to cut-off, cohort fixed effects, and background characteristics (see Table A3). Dashed lines indicate 95 % confidence intervals.

Figure B2

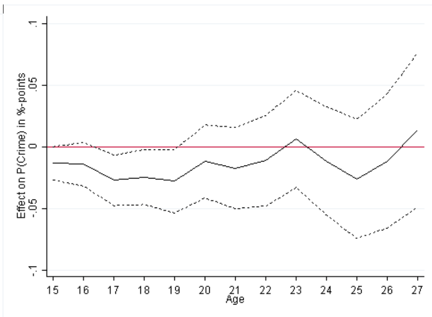
Estimation Results: Extended Bandwidth

Girls

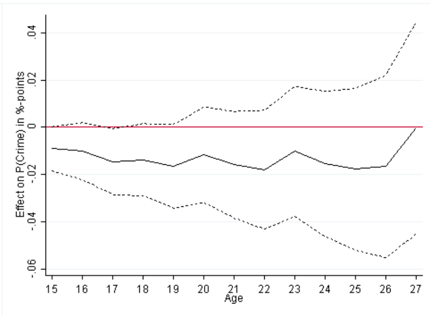
±30 days



±60 days

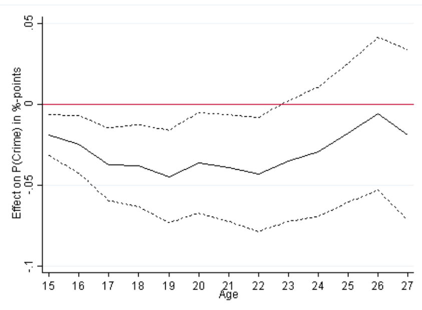


±90 days

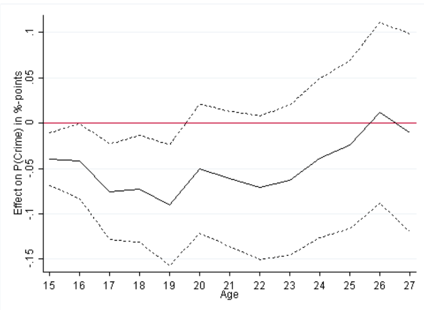


Boys

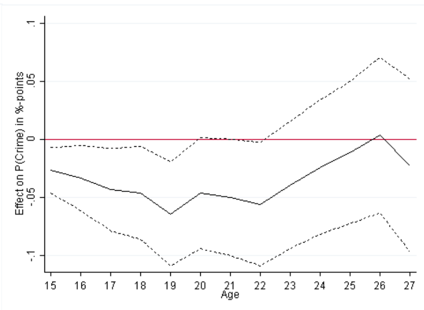
±30 days



±60 days



±90 days

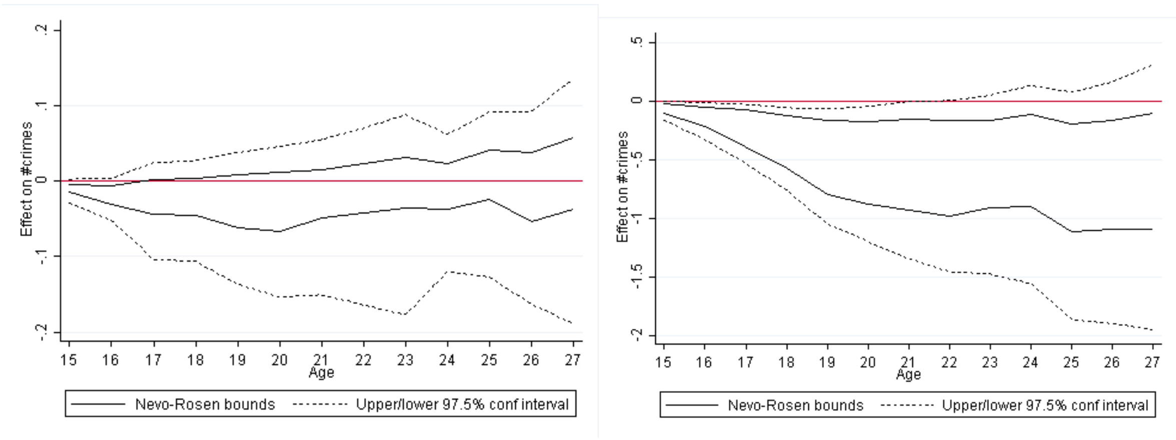


Note: Figures show the estimated effects of being old-for-grade based on 2SLS regressions of the probability of crime at or before a given age using varying sample bandwidths. Cut-off dummy (January=1) used as instrument. Conditioning set includes distance to cut-off, cohort fixed effects, and background characteristics (see Table A3). Dashed lines indicate 95 % confidence intervals.

Figure B3

Estimation Results: Nevo-Rosen Bounds

Girls Boys



Note: Figures show estimated effects of being old-for-grade on crime at or before a given age applying Nevo-Rosen bounds (Nevo and Rosen (2012)) and using a uniform cut-off as instrument and no covariates. Because the covariance between the instrument (here defined as January=1) and the treatment (7.6 instead of 6.6 years old at school start) is positive $\sigma_{SSA,z} > 0$, as being born in January predicts a higher probability of being old-for-grade, the bounds are given as:

$$Bounds\ B^* = \begin{array}{l} \left[\beta_z^{IV}, \dfrac{\sigma_{SSA}\sigma_{z,y} - \sigma_{SSA}\sigma_{OfG,y}}{\sigma_{SSA}(\sigma_{SSA,z} - \sigma_z\sigma_{SSA})}\right]\ if\ \sigma_{SSA,Z} > 0 \\ \left[\dfrac{\sigma_{SSA}\sigma_{z,y} - \sigma_{SSA}\sigma_{SSA,y}}{\sigma_{SSA}(\sigma_{SSA,z} - \sigma_z\sigma_{SSA})}, \beta_z^{IV}\right]\ if\ \sigma_{SSA,Z} < 0 \end{array}$$

Where *Z* denotes the instrument, *Y* crime at or before a given age, *SSA* is a binary indicator of being 7.6 instead of 6.6 years old at school start, and *V* unobserved characteristics affecting both treatment status and crime. Standard errors computed from 50 bootstraps.