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How Going to School Affects the Family

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Abstract:

This paper investigates intra-family spillovers from the timing of school start on outcomes for the entire family. We document how the timing of a child's school start affects the timing of all subsequent transitions between tiers in the educational system. Exploiting quasi-random variation in school starting age induced by date of birth, we find that the timing of transitions affect parental outcomes - including marriage and maternal employment - and older siblings' academic performance. Our results indicate that families redistribute resources across the entire family in response to a single family member's experiences as for example school start and graduation.

JEL: I21, J12.

Keywords: marital capital, marital dissolution, educational transition, regression discontinuity, spillover effects.

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1. Introduction

Education is one of the most important investments in children made by parents and society as a whole. School start is therefore a pivotal point in the life of the individual child, and the consequences of school starting age (henceforth SSA) have been investigated by many researchers. Children perform better *in school*, both in terms of grades (e.g. Bedard and Dhuey 2006) and behavior (e.g. Dee and Sievertsen 2015), when SSA is higher. Yet these short-term effects do not arise from SSA per se but rather from the persistent age difference between children who start school at different ages. The long-term impact of higher SSA is generally found to be miniscule (e.g. Black, Devereux and Salvanes 2011; Fredriksson and Öckert 2013; Landersø, Nielsen and Simonsen forthcoming; Larsen and Solli forthcoming; Dustmann, Puhani and Schönberg forthcoming).

While the short-term effects of SSA are considerable, there is a puzzling disconnect between the lack of long-term effects of higher SSA on child outcomes and the empirical observation that many parents choose to manipulate the timing of their child's school start.[1] Children's school start is, however, not only important in terms of their own human capital accumulation; it is also classified as a major stressful life event—a *life change unit*—both for the children in question as well as for their respective families (Holmes and Rahe 1967). This paper asks whether a policy manipulable variable, such as SSA and consequently the timing of a child's educational trajectory, has spillover effects to family members other than the targeted child, thereby affecting, for example, family stability and sibling school performance. In other words, is the parental choice to manipulate one child's SSA not limited to the human capital accumulation of the child in question but rather a result of maximizing the utility of the entire family? To the best of our knowledge, we are the first

[1] See for example Deming and Dynarski (2008).

to investigate whether the timing of a child's educational trajectory causally impacts both their parents' and siblings' outcomes.

Our empirical analysis exploits exogenous variation in SSA generated by administrative rules to circumvent the issue that SSA may correlate with important but unobservable individual and family characteristics. In particular, we exploit the circumstance that Danish children typically start first grade in the calendar year in which they turn seven, which gives rise to a fuzzy regression discontinuity design. By comparing the families of children born in December with families of children born in January, we investigate the effects of focal children starting grade 1 at age 6.6 compared to 7.6. Our analysis uses full population Danish register-based data for focal children born in the period 1986–2000 with information on exact birth dates, family and sibling outcomes, and a rich set of background characteristics.

We show how a child's SSA has direct consequences for their other family members. In the context of this paper, a higher SSA implies that the child spends an extra year in public childcare instead of going to school.[2] Delaying school start by one year improves parental relationship stability and increases maternal labor market participation while the child is of school age. We observe that, for the most part, the effects for parents' marital stability arise due to intertemporal changes; a 1-year postponement of a child's life-course due to a later school start results in a similar shift in parents' marital profiles. For maternal employment, however, the effects in the years following school start are different in nature. Here, we do not find an entire shift in profiles but instead a transitory effect around school start, likely because child maturity at school start eases the child's transition from pre-school and into school.

[2] This is to a lesser extent true in a US context, although US children have been enrolling in preschool at increasing rates as pointed out by Deming and Dynarski (2008),.

While none of the effects for parental outcomes persists after age 18–20 of the focal child, changes in parental labor market behavior but especially in family stability could be important because they reflect redistribution of resources within the family to counteract to impact of school start. We also find that postponing the school start of one child improves specific dimensions of the academic achievements of older siblings who are close to their final exam around the school start of the focal child. Grades associated with repetitive and rote learning, such as basic arithmetic and grammar (where parents can teach to the test), improve substantially, whereas grades associated with tasks that are more complex and general knowledge (e.g., essays and text analysis) are unaffected. Hence, delaying the school start of a younger sibling allows parents to redirect resources towards the dimensions in older siblings' upcoming exams that are most easily improved.

Our results have three major implications. First, they emphasize that educational policies may have substantial effects beyond their primary objectives and those directly affected by the policy; see also Garces et al. (2002), Joensen and Nielsen (2015), Nicoletti and Rabe (2014), and Qureshi (2015). Second, our results have bearing for the interpretation of the existing estimates of the consequences of SSA for child outcomes, as the behavioral adjustments of parents and siblings may be some of the mechanisms behind these results. We estimate a reallocation of resources to siblings, for example, which are likely to dampen the long-run effects of SSA on the focal child. But we also detect a delay in marital disruption, which may or may not improve outcomes of the focal child, depending on the quality of the prolonged relationship. Finally, our findings illustrate that within-family spillover is a general phenomenon and not only associated with adverse outcomes in disadvantaged families (Andersen and Wildeman 2014; Garces et al. 2002) or severe child disadvantages, such as ADHD and disability in general (e.g. Breining 2014; Black et al. 2017; Kvist et al. 2013).

As such, our paper speaks to the broader family economics literature, including the seminal papers by Becker and Tomes (1976, 1979) that are concerned with within-family variation in child outcomes and studies on how parents optimally and under constraints choose to allocate investments across different periods (Cunha and Heckman 2007). Our paper is also naturally related to the general literature on behavioral spillover effects; see Manski (1993, 2000) and Glaeser et al. (2003).

The paper is structured as follows: Section 2 introduces a simple conceptual framework and discusses channels through which focal children may impact the entire family, Section 3 reviews relevant institutional details, and Section 4 presents our empirical methodology. Section 5 presents our data and Section 6 presents the results. Finally, Section 7 concludes.

2. Background

This section first argues and shows empirically how delays in school start also delay subsequent transitions in the education cycle. We then present a simple conceptual framework and argue that the critical transitions in education potentially stress the entire family and lead to the reallocation of resources across family members.

2.1 Timing of the education cycle

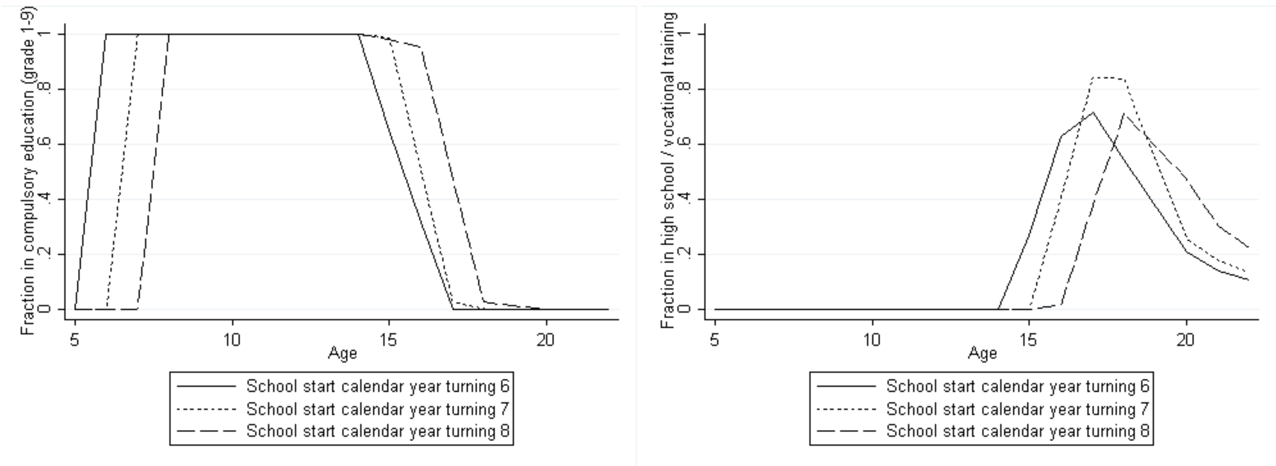
Our research design exploits quasi-random shifts in the timing of transitions into school. In any education system with low retention rates, however, there is a strong link between the timing of the transition into school and subsequent transitions in the education system. Figure 1 illustrates the relationship in the context of this paper. The figure shows how, among Danish schoolchildren who enroll in 1st grade during the calendar year when they turn 7 (which is most children), virtually all have left compulsory school and started upper secondary school or vocational school during the calendar year when they turn 17. This transition occurs one year later for individuals who started 1st grade during the calendar year when they turned eight and one year earlier for individuals who started 1st grade the calendar year they turned six. Figure 1 illustrates that postponing school start by one year implies postponing graduation by one year and so forth. Hence, a shock to SSA constitutes a shock to the timing of all of the critical transitions potentially stressing the entire family.

Figure 1

Enrollment into education by SSA and age

A) Fraction in compulsory schooling (grades 1–10) by age

B) Fraction in high school or vocational school by age



Notes: Figures show the fraction of children born 1986–2000 enrolled in education at each age by whether the child started school the calendar year they turned 6, 7 or 8. Figure A) shows the fraction of children enrolled in compulsory school (primary/lower secondary), and Figure B) shows the equivalent for enrollment into upper secondary or vocational school.

2.2 Conceptual framework

Economists have been concerned with the allocation of parental resources across children since the early work of Becker and Tomes (1976). This topic remains high on the scientific agenda, as seen in the study by Yi, Heckman, Zhang and Conti (2015), who study the reallocation of parental resources in a set-up with multiple skills in families with multiple children.

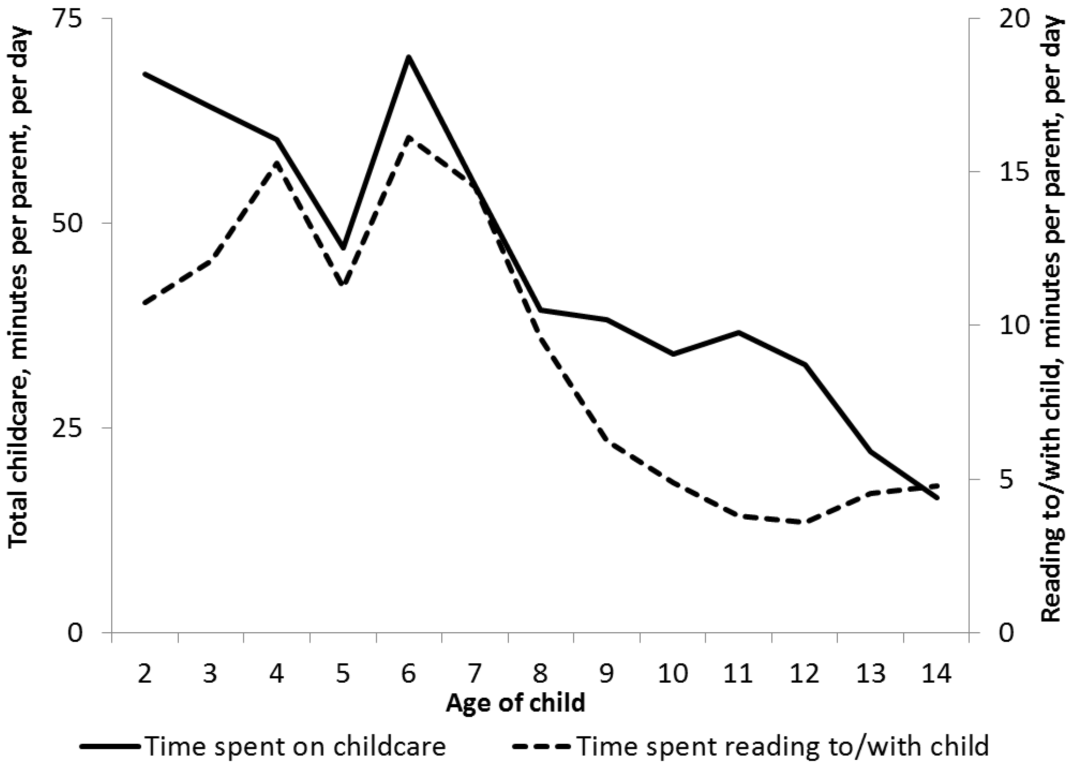
Given that parents are constrained in terms of resources such as time, money and mental capacity, we expect a shock to one child's trajectory to affect the type, timing and amount of investments parents are able to make in other children in the family. Parents have some flexibility in the allocation of money across various consumption and investment goods, which they can smooth out over time by borrowing and lending. They also have some leeway in the allocation of time between work, leisure and child investments, but the total available time is obviously fixed while mental capacity is fully constrained in that there is limited opportunity to smooth out resources over time.

Our starting point is the observation that the critical transition from daycare to primary school puts additional pressure on parents' resource constraints. Figure 2 shows how parents' time use varies across a child's age and that school start is associated with increased time use of parents. The figure shows the average daily time spent on childcare per parent during weekdays across a child's age. It is evident that both the average time parents spend with their children (the solid line) and time spent reading to the child (the solid line) decrease as children grow up, although with a local maximum around school start at ages 6–7.

In conclusion, the figure illustrates that school start is associated with a shock to parents' time-allocation, in line with, for example, findings from Cascio and Schanzenbach (2013), Pop-Eleches and Urquiola (2013), Fredriksson, Öckert and Oosterbeek (2016) and Kinsler and Pavan (2016), which also illustrates that the nature of a child's education affects the investment choices made by parents. Moreover, according to Holmes and Rahe (1967), educational transitions are classified as stressful life events that suggest how the mental capacity constraint may also be binding.

Figure 2

Parents' time-use with child, by age of child



Note: Figure shows average minutes spent per parent, per day, with the youngest child in the family on care and spare time activities (the solid line), and on reading to/with the child (the dashed line), across the age of the youngest child (DTUC 2008; see Bonke and Fallesen 2010).

Studies that model multi-period resource allocation, such as Cunha and Heckman (2007), illustrate that in a dynamic process, constraints in one period lead to sub-optimal investments in that period relative to other periods in life, possibly producing long-lasting consequences. A very similar intuition applies to our setting: Because families are time-constrained, possibly even facing unanticipated stress due to, for instance, complications in children's critical transitions in the education system, they have limited actions available. Such experiences might therefore have consequences reaching beyond the child in question.

As described above, a shock to the distribution of time use and the demands on mental resources may affect the investments that parents are able to make in their other children. More generally, because siblings also interact, such a shock may also introduce direct peer effects, just as the parents might invest differently in their own relationship because of such a shock. The directions of such effects must be determined through empirical analysis; for example, parents may either reinforce positive shocks (draw resources away from other siblings) or counteract positive shocks (redistribute resources to other siblings). Because the existing literature has shown how a higher SSA has immediate consequences for a child's *in-school* performance and well-being (e.g. Bedard and Dhuey 2006; Dee and Sievertsen 2015), we think of SSA as precisely such a shock and analyze its consequences for the outcomes of all family members.[3] Moreover, as already mentioned, the existing literature identifies particularly sensitive or stressful periods in the lives of siblings and parents. We therefore investigate if the effects of SSA are particularly pronounced in connection with such key stages (e.g. school start itself, graduation, child leaves home).

3. Institutional settings relevant for school starting age (SSA)

Our empirical analysis will exploit plausibly exogenous, institutionally induced variation in SSA. This section therefore describes the institutional details influencing school start decisions.

During the period of relevance 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.[4] This school

[3] In principle, parents with perfect foresight may respond to a higher SSA already before the actual school start.

[4] SSA regulations are not strictly enforced and exemptions are granted based on applications from the parents. Exemptions are granted by the local municipality if 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. Schoolchildren do not pass or fail grades; in collaboration with the

system is fortunate for a study like ours because there is no automatic relationship between SSA and minimum required schooling, as is the case in the US and UK systems. Pupils receive teacher-assessed grades in grades 8 and 9 and take compulsory exit exams in a number of subjects at the end of grade 9. Further education is voluntary and may follow a more academic path (starting with upper secondary school) or a vocational path (vocational school).[5]

The year before entering first grade, children can enroll in kindergarten,[6] which typically is located in the same building as the school for the early grades (and was voluntary during the period relevant for this study). Kindergarten, compulsory schooling from grades 1–9, secondary school and even most higher education programs are free of charge. Furthermore, already at age four, the vast majority (98% in 2004; see statistikbanken.dk) of children are enrolled in some form of public daycare, which is heavily subsidized.[7]

As mentioned above, parents and administrators have considerable leeway when deciding when children should start school.[8] Therefore, SSA is not random and is most likely affected by a range of factors that may also correlate with the child's outcomes and those of their family. For example, a child's overall maturity, school readiness and behavior in childcare may affect the timing of school start.

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parents, the school principal can decide that a child repeats or jumps a grade if considered beneficial for the child's development. For more details, consult the Education Act.

[5] It is also possible to complete an elective 10th grade before continuing on an academic or vocational path; opportunity that approximately 50% of a cohort make use of (e.g., 52% of the cohort born in 1994 opted for 10th grade).

[6] This is also denoted 0th grade.

[7] A minimum of 67% of the expenses is covered by the local authorities (c.f. the Children's Act).

[8] Skolestartsudvalget (2006) documents this and UNI-C (2009) describes background characteristics of children across SSA.

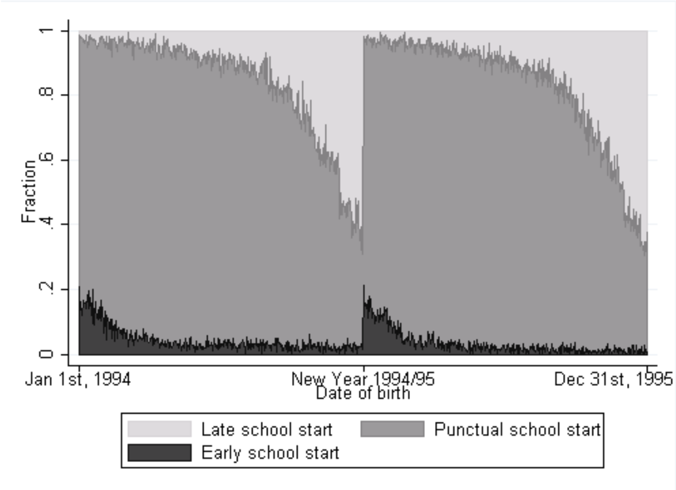
To address the consequences of SSA, our empirical analysis exploits that the formal age at school start is defined by birth year. January 1 is the relevant cutoff point: according to administrative rules, children born just before this date are supposed to start in school in one year and children born just after in the subsequent year. Some parents of children born close to this cutoff date do choose to manipulate their children's actual SSA: late-year children are more likely to postpone school start by a year, whereas early-year children are more likely to start school one year earlier than stipulated.[9] Consequently, 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 around age 6.6. 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 described in the beginning of this section, the alternative to starting school is to spend an additional year in public daycare. Only a negligible proportion of children are not in daycare immediately before school start.

As shown in Figure 3, SSA for children born around the cutoff date is effectively reduced to a binary outcome: children start at age either 6.6 or 7.6. If children born around the cutoff are 7.6 years old at school start, we label them "old-for-grade." Figure 4 shows the fraction of children who are old-for-grade by date of birth. We see that there is a smooth upward trend in the fraction of old-for-grade children in December followed by a large discontinuity around January 1.

[9] A 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 3

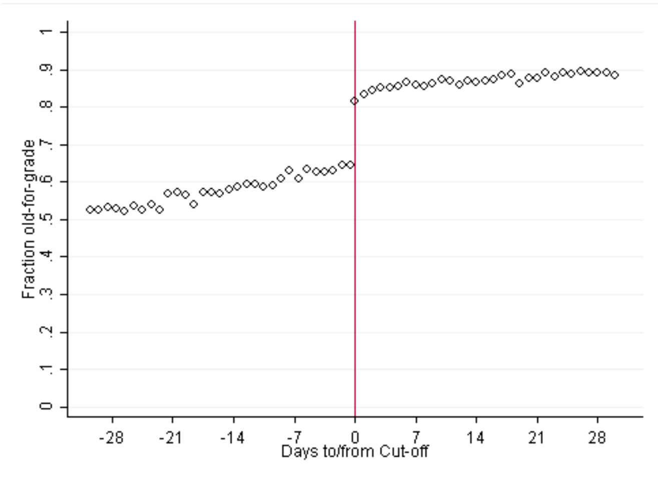
Fraction punctual, early and late school start



Note: Figure shows the school starting pattern of the full population of children born between January 1 1994 and 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 4

Fraction – old-for-grade by date of birth



Note: Figure shows the fraction of "old-for-grade" children by date of birth around January 1 (marked by the vertical line). Being old-for-grade implies that the child starts school at age 7.6 instead of 6.6. Averages for population of children born in December or January from December 1986 to January 2000.

4. Methodology

Our goal is to estimate the effect of SSA of child *i* in family *f* on outcomes of siblings and parents *j* in the same family. Our equation of interest is the following:

$$(1) \qquad Y_{jf} = \alpha + \beta \cdot SSA_{if} + X'_{if}\gamma + X'_{jf}\delta + \varepsilon_{jf}$$

where Y denotes the outcome, X observable characteristics[10] and ε unobservable characteristics. Unobservable characteristics of family members are possibly related to the choice of SSA, which may cause bias if ignored. To address 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 immediately prior to January 1 are on average younger when they enroll in school than those born immediately after January 1.

In some sense, we can think about administrative SSA rules as affecting the incentives to enroll children later (or earlier) than prescribed by imposing time and effort costs on parents who do not comply with the regime. We can therefore instrument SSA with a dummy for being born immediately after January 1. As argued in the existing literature, such cutoff dates constitute valid instruments in the sense of being uncorrelated with unobserved characteristics of child outcomes.[11] 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 SSA solely because they were born in

[10] *X* includes child and parental characteristics predictive of *SSA* and *outcomes*: child gender, an indicator of low birth weight, mother's age at the birth of child, father's age at the birth of child, dummies of number of siblings, whether parents are married measured at age 3 of the child, and whether parents are employed measured at age 3 of the child, and a flexible function of distance in days to the cutoff. In regressions for sibling outcomes, *X* also includes sibling gender, an indicator of low birth weight of sibling and age distance to focal child.

[11] Our results are unaffected by the finding that children's season of birth is not random (Buckles and Hungerman, 2013), because our identification is based on local exogeneity in the limit around January 1.

January as opposed to December—we also require that the monotonicity assumption is satisfied. Aliprantis (2012), Barua and Lang (2016), and Fiorini, Stevens, Taylor and Edwards (2013) argue, however, that monotonicity is likely to be 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. As explained in detail in Landersø, Nielsen and Simonsen (forthcoming), however, monotonicity is likely satisfied in our particular context, where no children start more than one year before/after the date at which they are supposed to start, and SSA in our case is effectively reduced to a binary variable indicating whether the child enrolls at age 6.6 or 7.6.

In practice, we consider a short bandwidth with focal children born ± 30 days around January 1. In our main specification, we model SSA as a binary variable indicating a SSA of 7.6 as opposed to 6.6. All results are robust to alternative specifications, such as using an extended bandwidth around the cutoff, a donut RD regression and with in- or exclusion of covariates and fixed effects (on cohort or family level); results available upon request.

5. Data

5.1 Data sources and samples

We exploit rich administrative Danish data containing information on all individuals residing in Denmark. Our data material provides the following crucial sets of information: 1) family outcomes and sibling school performance, 2) SSA and 3) rich background information. Registers are linked at the personal level via a unique personal identifier. Using parental identifiers, we are able to link children to their parents and siblings. More details about each of these types of information are provided below.

Our starting point is the set of children born from 1986 to 2000. Within this group, for the purposes of our formal analysis, we then select the *focal child sample* of individuals born around the January 1 cutoff. This sample consists of December-born children in the years 1986–1999 and January-born children from 1987–2000. The *parent sample* consists of the biological parents of focal children while the *sibling sample* is made up of siblings born to the same mothers as the focal children. Where two (or more) children from the same family are born around the January 1 cutoff in the observation period, there will be two (or more) focal children observed in the same family (7% of all families in our parent sample are represented more than once because multiple children are born in December or January within our study period).[12] Our sample initially includes both siblings who are younger and older than the focal children but our empirical analysis will consider older siblings, where the causal link to sibling outcomes is cleaner as older siblings have already started school and cannot be affected via changes to their own SSA. The Danish register data spans from 1980–2015, but grades are only available from 2002 and onwards. Therefore, we do not have a complete overlap between the samples used to study all outcomes for focal children, parents and siblings.

Figure 5 shows the distribution of siblings by age spacing. The histogram illustrates the fraction of siblings for whom we have support for each of the two schooling outcomes under investigation: grades for older siblings and SSA for younger siblings. The closer siblings are spaced; the relatively more complete is the observation of outcomes in the sibling sample.

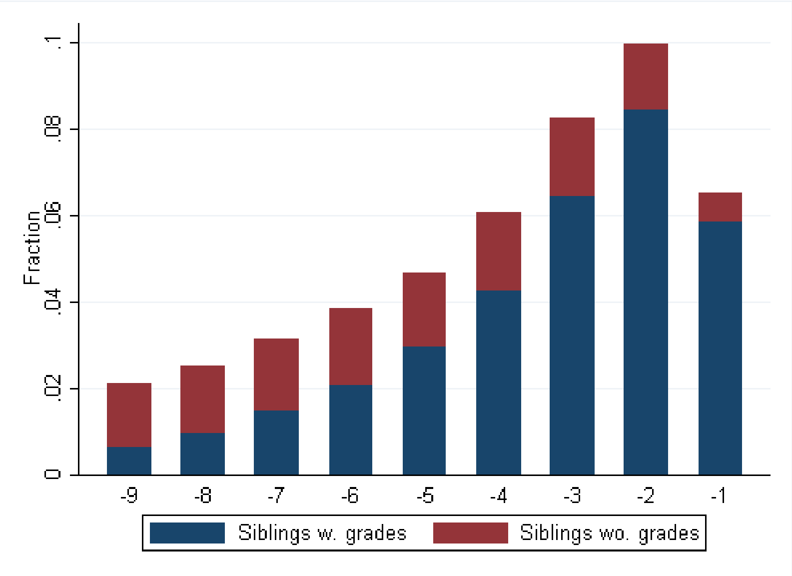
In order to ascertain that our results are not driven by skewness in spacing or calendar time in the estimation sample, we perform sensitivity analyses restricting the sample to cohorts where we have complete information about outcomes of all siblings within a maximum age distance of 3, 6 and 9 years, respectively. When we restrict the sample to a maximum of 3, 6 and 9 years age distance, we

[12] We discard all twins from the sample of children born around the cutoff in December and January but not from the sample of siblings.

have complete information about the outcomes for younger siblings for focal children born from 1986/87 until 1989/90, 1992/93 and 1995/96, respectively, and about the outcomes of older siblings for focal children born from 1990/91, 1993/94 and 1996/97, respectively, until 1999/2000.

Figure 5

Histogram showing distribution of distance in time between age of focal child and sibling



Note: Figure shows histogram of focal child's age minus siblings' age ("positive" means that sibling is younger) in the sample of children born in December 1986–1999 and January 1987–2000. The figure indicates the fraction of siblings for whom we have support for grades for older siblings (grades are available from 2002–2014).

5.2 Key variables and descriptive statistics

Measuring school starting age (SSA)

We do not observe the exact timing of SSA for the cohorts under analysis. Instead, we use age in 8th grade (minus eight) as an approximation. This works because the vast majority of old-for-grade children at the end of elementary school are already old-for-grade in kindergarten, whereas very few children are delayed from grade 1 onwards.[13]

Outcome variables

We consider the effects of the focal child's SSA on a range of family outcomes: parents' relationship stability (measured by an indicator variable for whether parents are married/cohabiting measured on January 1 every year following school start), parental employment (measured by an indicator variable for whether parents are employed or not in November each year following school start), and the likelihood of the focal child living with the parents in each year (measured by exact address information on January 1 each year). We also consider the impact on older siblings' academic performance in terms of exit exam results after grade 9 for older siblings.

To get a sense of the development in parents' relationship stability over time, Figure 6 illustrates the proportion of parents cohabiting or married at a given focal child age. When the focal child is 3 years old, around 85% of parents live together, while the number is down to 60% when the child turns 20. Figure 7 shows the development in parental employment: Around 87% of fathers and 72% of mothers are employed when the child is 3 years old. As the focal child grows older, the maternal employment rate approaches the paternal rate. As a result of the average age difference between mothers and fathers, parents' labor market trajectories intersect when the focal child reaches their teens. Paternal employment rates decline during the children's teens, while maternal employment rates start increasing before they also start to decline. Figure 8 shows the share of focal children living at home by age; at age 18, 90% still live with their parents, but this percentage decreases quickly: 50% live at home at age 20 and only 10% at age 24.

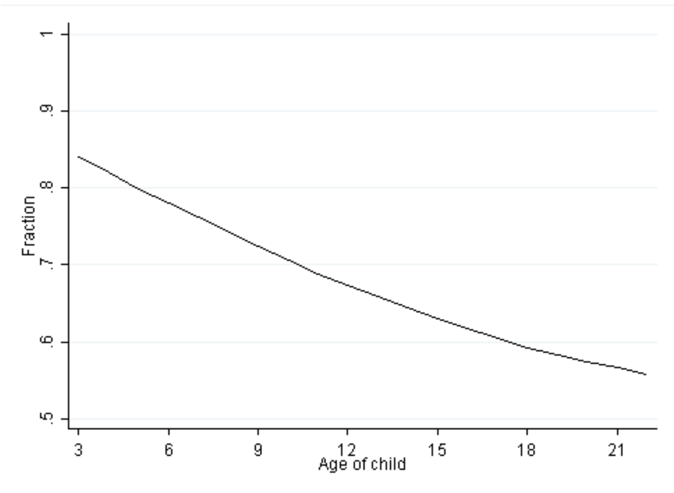
[13] See also Landersø, Nielsen and Simonsen (forthcoming), who exploit more recent data with information about exact SSA to validate the approach and implement a range of sensitivity analyses.

Tables 1–2 display descriptive statistics for siblings' outcomes. Table 1 shows the distribution of older siblings' grades. The Danish grading scale is numerical and corresponds directly to the reported ECTS scale. The modal grade is C, and 33% have grades lower than C in math while around 40% have grades lower than C in Danish. In the empirical analysis, we standardize grades to have mean 0 and standard deviation 1 based on the numerical scale.

One concern that might change the interpretation of our results throughout the paper is if the focal child's SSA is related to the subsequent birth of siblings (i.e. fertility). We have studied this relationship, which could introduce compositional changes in the younger sibling sample and additional shocks to parents' investments and time-use. We find that subsequent fertility is not significantly related to the focal child's SSA, which supports our interpretation of the results presented; see Section 6.

Figure 6

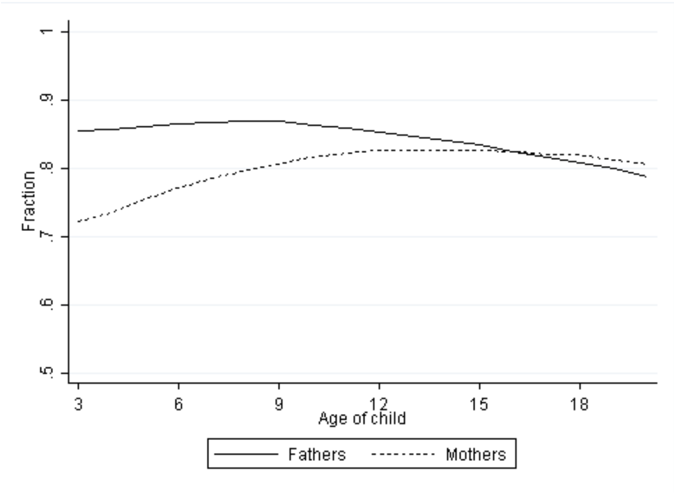
Fraction of parents cohabiting or married by focal child's age



Note: Figure shows the fraction of parents cohabiting at a given age of the focal child. The figure is based on the population of children born from 1986 to 2000.

Figure 7

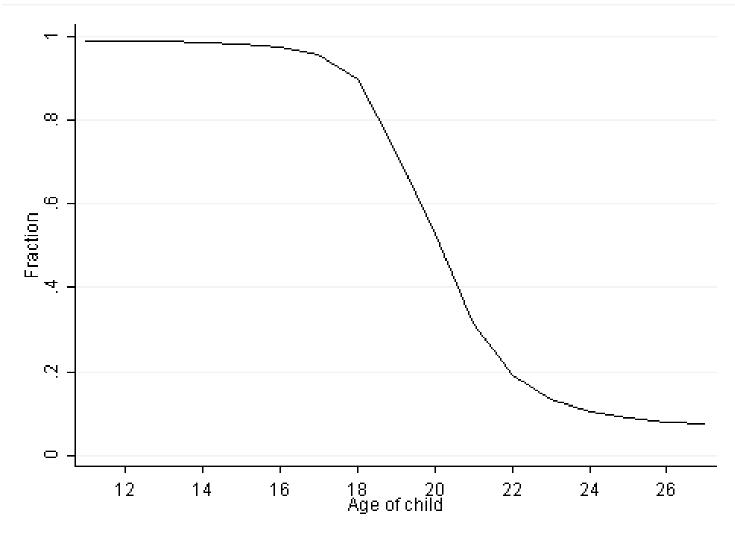
Parental employment by focal child's age



Note: Figure shows the fraction of parents working at a given age of the focal child. The figure is based on the population of children born from 1986 to 2000.

Figure 8

Fraction of focal children living at home by focal child's age



Note: Figure shows the fraction of children living with their parents at a given age of the focal child. The figure is based on the population of children born from 1986 to 2000.

Table 1

Distribution of older siblings' exit exam grades (grade 9)

Grades		Math	Danish (Written)	Danish (Grammar)	Danish (Oral)
12	A	0.082	0.047	0.051	0.155
10	B	0.203	0.153	0.132	0.193
7	C	0.387	0.405	0.404	0.337
04	D	0.177	0.254	0.225	0.184
02	E	0.100	0.119	0.139	0.103
00	Fx	0.051	0.022	0.049	0.029
-3	F	0.000	0.001	0.000	0.000
Observations		50,086	50,086	50,086	50,086

Note: Table shows the distribution of older siblings' exit exam grades at the end of grade 9. The table is based on the population of children born from 1986 to 2000.

Background characteristics

Using the registers, we combine information on the children's birth weight, demographic variables and educational variables by the unique individual identification number. We also link these data to information about parent's characteristics as measured one year prior to the birth of the child. Descriptive statistics for the background characteristics are reported in Table A1 in Appendix A.

Importantly, we center all covariates and outcome variables on the cutoff 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.[14] Table 2 shows joint F-tests from a regression of the instrument on the rich set of background variables for children born

[14] For children born in December 1986 or January 1987, we use parental characteristics measured in 1985, whereas for children born in December 1987 or January 1988 we use parental characteristics measured in 1986 etc.

± 30 days around January 1. These tests clearly suggest that the sample is balanced across the cutoff.[15] We include all variables as covariates.

Table 2

Balancing test

	(1)	(2)
F-statistic	1.00	0.84
p-value	0.43	0.55
Observations	132,039	132,039
Distance to cut-off	X	X
Focal child covariates	X	X
Cohort fixed effects		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 A1 and cohort fixed effects) as well as distance to cutoff in days.

Table A1 also shows the average characteristics of the compliers (those who are old-for-grade as a result of the administrative January 1 cutoff), estimated as described in e.g., Almond and Doyle (2011). The table illustrates how the average family size of compliers differs from the average family size of the remaining sample. Compliers have more siblings, which indicates that the complier families in question are particularly sensitive to shocks to time use and mental resources and more likely to be constrained in these aspects. There is also a weak tendency for compliers to be positively selected in terms of other characteristics.[16]

[15] We have plotted the variation of selected control variables on either side of the cutoff and find no significant differences across the cutoff for any of the control variables, which further strengthens the credibility of the empirical approach.

[16] This is different from the US context (see Deming and Dynarski, 2008).

6. Results

6.1 Timing of birth within the calendar year and school starting age

Table 3 presents the results from the first stage regression using an indicator variable for birth in January as instrument for SSA. The table shows the first stage results estimated both with and background variables. Note that the coefficient estimate associated with the instrument does not change with the inclusion these <of other control variables. All specifications include cohort fixed effects (indicator variables for being born Dec 1986–Jan 1987, Dec 1987–Jan 1988 etc.) and the distance in days to the cutoff linearly. Being born in January rather than December strongly predicts whether the child starts school at age 7.6 or 6.6 years.

Table 3

First stage results

	(1)	(2)
January (0/1)	0.201***	0.201***
	(0.005)	(0.004)
Distance to cut-off, January	0.002***	0.002***
	(0.000)	(0.000)
Distance to cut-off, December	-0.004***	-0.004***
	(0.000)	(0.000)
Birth weight<2000g		0.157***
		(0.015)
Boy		0.178***
		(0.002)
Boy*Birthweight<2000g		-0.079***
		(0.021)
Older siblings		-0.012***
		(0.003)
Parents married/cohab., at age 3		-0.005
		(0.003)
Mother's age at birth		-0.000
		(0.000)
Father's age at birth		-0.002***
		(0.000)
Constant	0.643***	0.627***
	(0.003)	(0.008)
Observations	132,039	132,039
R squared	0.113	0.154
F-statistics	5595	2398

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 cutoff dummy (January = 1), distance to cutoff, cohort fixed effects and background characteristics. Standard errors in parentheses +p < 0.10, *p < 0.05, **p < 0.01, ***p < 0.001.

6.2 Effects on parents

In this subsection, we present the results of the estimated effects of child's SSA on parents' outcomes. In order to first illustrate how a family's life is directly affected by the SSA of a child because school start and later life-course is strongly connected, Figure 9 shows the effect of SSA on the probability that the focal child lives with the parents at a given age. If the child starts school at age 7.6 rather than 6.6, they are more likely to live at home at ages 16–22. Most Danish youth graduate from secondary school and transition into college around ages 18–20. The increase in the probability of living at home hence reflects the fact that later school start postpones the entire life course, as discussed above. Also for the ages 11–16, we see a positive impact of having started school at age 7.6 rather than 6.6. The effect is small in magnitude but substantial compared to the mean probability of not living with the parents.

In the following two figures, we investigate parents' own responses to the timing of children's school start. Figure 10 illustrates the effects of being old-for-grade on the probability that parents live together (married or cohabiting) at a given age. Until age 6, the point estimates are small and not significantly different from zero. In some sense, we can think of this as a placebo test as children have yet to start school. From the January 1 cutoff when the child turns 7 and onwards, the family is more likely intact if the focal child is old-for-grade than young-for-grade, although the estimates are only borderline significant at a 10% level. The old-for-grade children are in kindergarten at this exact January 1 cutoff, whereas young-for-grade children are in grade 1. The coefficient estimates jump again around the focal child's 15th birthday. At this point in time, children who are old-for-grade are in the middle of 8th grade while the young-for-grade children are in the middle of grade 9 and approach the lower secondary exit exam taking place 6 months later in June. A further investigation shows that children who are the youngest in their sibship drive this effect (results available upon request). The significant effects of being old-for-grade on parents' relationship status are, however, not persistent. They peak when the child is aged 15–17 and approach zero afterwards.[17]

Figure 11 shows the estimated effects of a child's SSA on maternal employment. Mothers work more often when the focal child is age seven. At this age, young-for-grade children have started first grade while old-for-grade children are still in kindergarten.[18] Note that there are no significant effects on employment before age seven. Estimates are still positive once both treated and untreated children have started school (age eight and onwards) but become statistically insignificant.[19] Of course, as shown in Figure 7, around 75% of mothers are already working when the child is aged 7. We therefore expect effects on employment to be driven by mothers with relatively low socio-economic status. We explore this in our heterogeneity analyses below. We find no effects of a child's SSA on paternal employment (not shown), as paternal employment rates are high and stable across focal child age (see Figure 7).

In summary, the results from Figures 9 and 10 suggest that parents' outcomes are linked to their children's educational trajectory. However, the findings may arise due to several different mechanisms. On the one hand, child maturity upon the transition into primary and upper-secondary school, along with better academic achievement relative to younger peers, may influence family

[17] In order to reveal whether the results may reflect stress, we study the effect of increasing SSA on mother's traffic violations. We find a reduction of 2 percentage points when the focal child is 16 years old, but no other significant effects at other ages. This finding supports the interpretation that the family is exposed to stress around the focal child's transition to upper secondary school; results available upon request.

[18] In order to understand the extent of labor market responses beyond the extensive margin, we investigate the impact of increasing SSA on the mother's placement in the earnings distribution of her cohort. We find a significantly positive impact of SSA on being above the 25th percentile at ages 8–9, but no significant effects higher up in the earnings distribution; results available upon request.

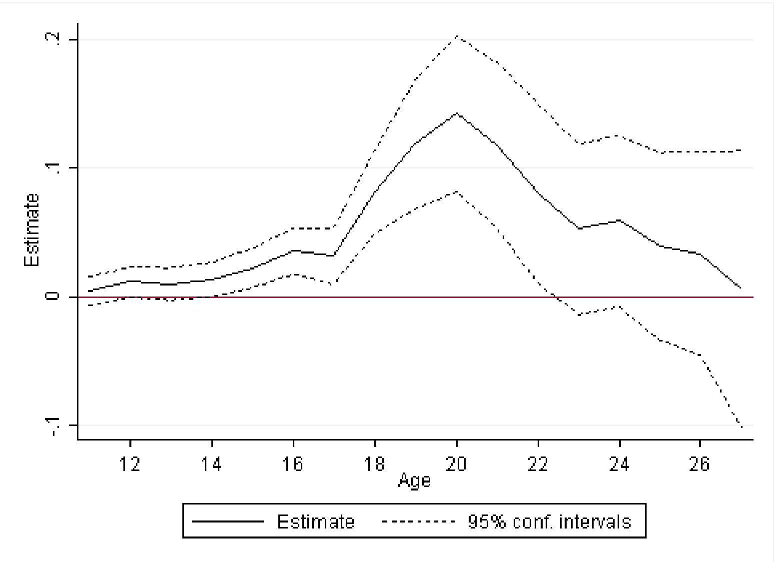
[19] Results are robust to extending the bandwidth to ± 60 days instead of ± 30 days around January 1; see Figures A2 and A3.

wellbeing and time constraints and therefore result in fewer split-ups. On the other hand, parents' outcomes may simply be linked to their child's life-course, which is postponed along with school start as evidenced from Figure 9. We will explore these mechanisms further in Section 6.4.

Furthermore, outcomes may be mutually dependent due to spillovers, thus blurring the exact (post-school start) causal paths. Marital stability, for example, may be affected by children leaving the nest, because a child leaving the home is known to be stressful from the parents' perspective (Holmes and Rahe 1967). Postponing movement from home might therefore explain why parents are less likely to split up during this period of their children's life. Yet the causality might also run in the reverse direction: If the parents split up, the offspring are more likely to leave the nest when parents establish new homes and new lives apart from each other.

Figure 9

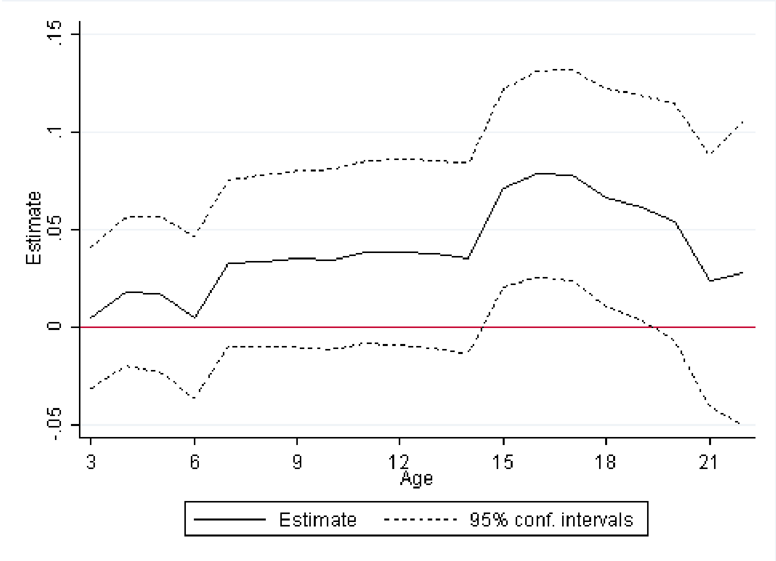
Estimation results: Fraction of focal children living at home by focal child age



Note: Figure shows the estimated effects of being old-for-grade based on 2SLS regressions of the fraction of children living at home at a given age. Cutoff dummy (January = 1) used as instrument. Conditioning set includes distance to cutoff, cohort fixed effects and background characteristics (see Table A1). Dashed lines indicate 95% confidence intervals.

Figure 10

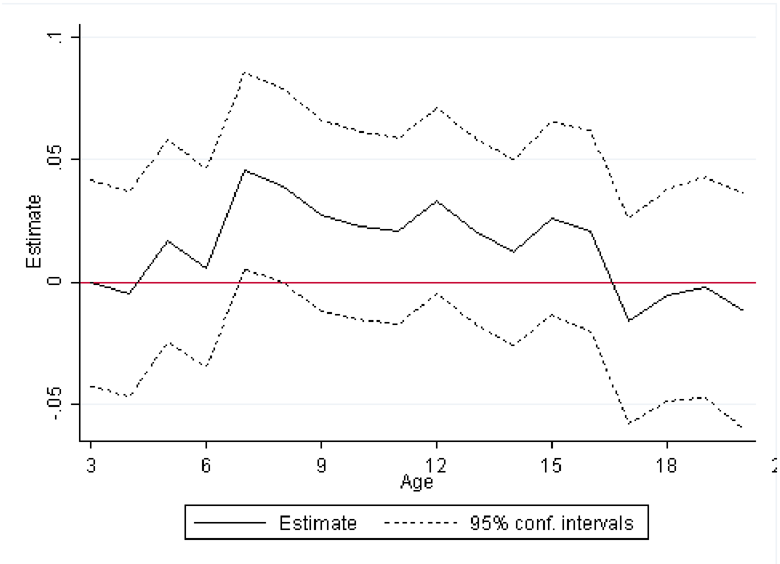
Estimation results: Fraction married or cohabiting by focal child age



Note: Figure shows the estimated effects of being old-for-grade based on 2SLS regressions of fraction of parents who are married or cohabiting at a given child age. Cutoff dummy (January = 1) used as instrument. Conditioning set includes distance to cutoff, cohort fixed effects and background characteristics (see Table A1). Dashed lines indicate 95% confidence intervals.

Figure 11

Estimation results: Employment of mother by focal child age



Note: Figure shows the estimated effects of being old-for-grade based on 2SLS regressions of mother's employment at a given child age. Cutoff dummy (January = 1) used as instrument. Conditioning set includes distance to cutoff, cohort fixed effects and background characteristics (see Table A1). Dashed lines indicate 95% confidence interval.

6.3 Effects on siblings

In Table 4 we present the estimation results for the effects of the focal child's SSA on sibling outcomes.[20] The results confirm the pattern seen above: that a higher SSA seems to ease school experience and release resources in the family. Table 4 shows that a higher SSA of the focal child does not significantly affect the grades of older siblings who are in lower or middle primary school at the time of the focal child's supposed school start. For siblings who are 7–9 years older than the focal child, however, a higher SSA improves grades substantially. Note, though, that estimated effects are imprecise and we suggest caution in interpreting the size. These older siblings receive their first teacher assessments and approach graduation at the time when the focal child transitions into elementary school.[21] An easier school start of the focal child likely improves the performance of the older siblings because the study environment at home is better or because parental resources are freed to assist with homework. In support of this interpretation, only grades in written math and Danish grammar are significantly improved as opposed to grades in essay writing or the oral examination of text analysis. The former disciplines require lower levels of cognition (knowledge, comprehension and application), whereas the latter disciplines require higher levels of cognition (analysis, synthesis, evaluation).[22] Thus, the former disciplines are more closely related to rote learning and thereby easier to practice, while the latter require verbal creativity and ideational fluency, which are not readily improved in the short run. Furthermore, the effects of SSA on sibling outcomes may also partly run through marital stability, as parental conflicts and/or divorce have been shown to have adverse effects on children's outcomes (Gruber 2004; Piketty 2003). Interestingly, we see that it is particularly the lower and mid-range grades that are affected by

[20] Table A2 documents that there is no effect on fertility, neither before nor after birth of the focal child.

[21] Table A3 in the Appendix revisits these results using smaller samples that are balanced in terms of the distribution of age distance between siblings across calendar time.

[22] See Bloom (1956).

sibling SSA. The probability of receiving a B, C or D in written math and Danish grammar increases with sibling SSA, while the effect of SSA on receiving an A is close to zero and the probability of receiving an E or F becomes less likely, thereby illustrating that the effects on siblings' grades are indeed concentrated in margins where the road to improved test scores is relatively straightforward; results available upon request.

6.4 Heterogeneity

We have investigated the degree to which our findings differ with parental education and income; results are available upon request. We see that SSA primarily affects labor supply and marriage patterns in the group of low education (and low income) mothers. Effects on siblings are qualitatively similar regardless of maternal education and family income prior to childbirth. Together, these results may suggest that mainly low resource families face constraints in terms of their own outcomes but that these are eased with increased SSA. All families, in contrast, are constrained in terms of investments in their children. Of course, the group of compliers may vary with maternal education, which complicates direct comparisons. Furthermore, because we see effects on sibling academic outcomes across groups but only effects on parental outcomes in the low socio-economic status group, we conclude that school start of one sibling in itself affects outcomes for other siblings and that these effects do not solely arise because of changes in parental outcomes.

Table 4

Estimation results: Older siblings' grades by distance in focal child's and sibling's age

Age difference	OLS	2SLS	2SLS	Observations
Math, standard deviations				
1-3 years	-0.107***	0.122	0.056	31,505
	(0.012)	(0.104)	(0.100)	
4-6 years	-0.128***	-0.032	-0.115	14,024
	(0.019)	(0.183)	(0.174)	
7-9 years	-0.132***	1.065**	0.985**	4,557
	(0.034)	(0.392)	(0.355)	
Danish essay, standard deviations				
1-3 years	-0.066***	0.134	0.116	31,505
	(0.012)	(0.104)	(0.098)	
4-6 years	-0.079***	0.096	0.003	14,024
	(0.018)	(0.182)	(0.171)	
7-9 years	-0.119***	0.037	0.101	4,557
	(0.033)	(0.345)	(0.311)	
Danish grammar, standard deviations				
1-3 years	-0.102***	0.106	0.079	31,505
	(0.012)	(0.103)	(0.099)	
4-6 years	-0.121***	0.279	0.189	14,024
	(0.018)	(0.184)	(0.175)	
7-9 years	-0.139***	0.768*	0.763*	4,557
	(0.033)	(0.368)	(0.336)	
Danish oral, standard deviations				
1-3 years	-0.081***	0.097	0.076	31,505
	(0.012)	(0.103)	(0.099)	
4-6 years	-0.101***	-0.043	-0.123	14,024
	(0.019)	(0.182)	(0.174)	
7-9 years	-0.105**	-0.124	-0.070	4,557
	(0.034)	(0.342)	(0.316)	
Distance to cut-off	X	X	X	
Covariates	X		X	

Note: Table shows the estimated effects of being old-for-grade based on OLS and 2SLS regressions of older siblings' grades at the end of grade 9. Cutoff dummy (January = 1) used as instrument. Conditioning set includes distance to cutoff, cohort fixed effects and background characteristics (see Table A1). Standard errors in parentheses +p < 0.10, *p < 0.05, **p < 0.01, ***p < 0.001.

6.5 A simple intertemporal response?

Our estimates of spillover effects on siblings suggest that the resources available for other family members increase with a higher focal child SSA. The effects on parental outcomes may arise because a later school start actually eases the child's transition into primary and upper-secondary school, thereby reducing the impact that school start (and graduation) has on the family. Or, they may occur as a simple intertemporal response driven by a delay in the focal child's life-course, including all critical transitions, by one year.

In an attempt at distinguishing between these two scenarios, we estimate compliers' potential outcomes if they were young-for-grade or old-for-grade, respectively, in line with Abadie (2002, 2003). Conversely, if the potential outcomes diverge exactly around the critical stages but are aligned otherwise, the hypothesis that effects arise due to an easier transition between different educational stages is supported. If, on the other hand, the potential outcomes are parallel across ages, this supports the hypothesis that effects stem from an intertemporal response to the postponement of the child's life-course. We combine this analysis with a re-estimation of the effects of being old-for-grade on parents' relationship status and maternal employment rates where we align the outcomes by grade level instead of age. If results arise from parallel changes to parents' trajectories as a consequence of the delayed life-course, the estimated effects for outcomes centered by grade level should be miniscule, whereas if our findings arise because children's SSA actually eases the transition, effects should persist around the timing of the transitions, even when we center outcomes by grade level.

Figure 12A shows a smooth reduction in parental marital/cohabitation rates if the child is young-for-grade (Y_0). If the child is old-for-grade (Y_1), parents would follow the same trajectory until school start, where parental marital/cohabitation rates stagnate for one year. From age 7 until 14, parents' marital/cohabitation rates follow similar trends disregarding SSA. At 14, marital/cohabitation rates of old-for-grade parents stagnate, while they decline for young-for-grade parents. This is followed by a gradual convergence between the two counterfactual outcomes until the child is 21 years old. Consequently, in Figure 12B, the estimated effects from daycare until grade 8, when we align parents' relationship status by grade level, are insignificant and close to zero, suggesting that any initial response on parents' relationship stability is a result of the focal child's postponed life-course. From grades 10–12, however, significant effects of being old-for-grade on the parents' marital/cohabitation rates emerge.[23] These are exactly the years when the child finishes compulsory schooling and enrolls in secondary school, suggesting that the transitions into primary school and later into upper secondary school do not impact family resources similarly. In fact, considerable existing research emphasizes the importance of non-cognitive skills for a successful transition into upper secondary school; see e.g. Almlund et al. (2011) and Heckman and Rubenstein (2001).

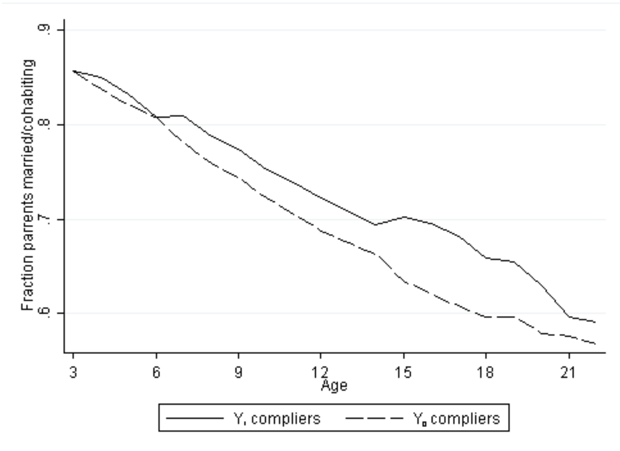
For maternal employment rates, we see a different picture. Figure 12C shows how the counterfactual employment rates diverge at age 7. Employment rates for mothers who have old-for-grade children are consistently above those for mothers who have young-for-grade children until the child turns 17, the largest differences being in the early schooling years. Hence, a later school start allows parents to allocate more resources to themselves, and parents (mothers) respond to the decision of delayed school start immediately after the decision has been put into effect. This is confirmed by Figure 12D, which shows that being old-for-grade results in significant positive effects. During grades 1–3, maternal employment rates increase by 4–6 percentage points (5–7% relative to the Y_0) if their child is old-for-grade.

[23] We also find that increasing SSA reduces mother's traffic violations in grade 9; results available upon request.

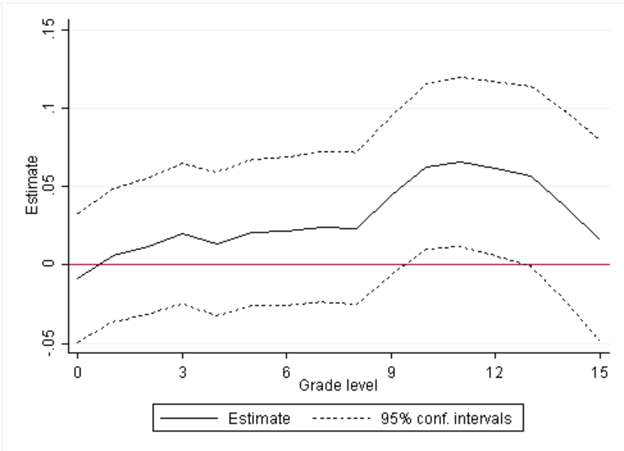
Figure 12

Parental marriage/cohabitation and maternal employment rates: Compliers' potential counterfactual outcomes and 2SLS estimates with outcomes by grade level

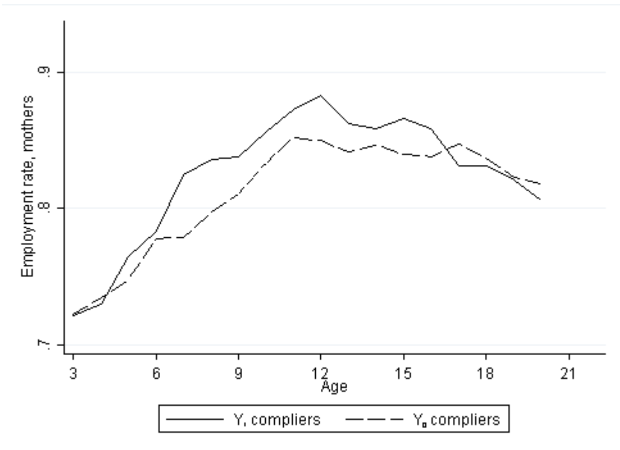
A) Counterfactual marriage/cohabitation rates



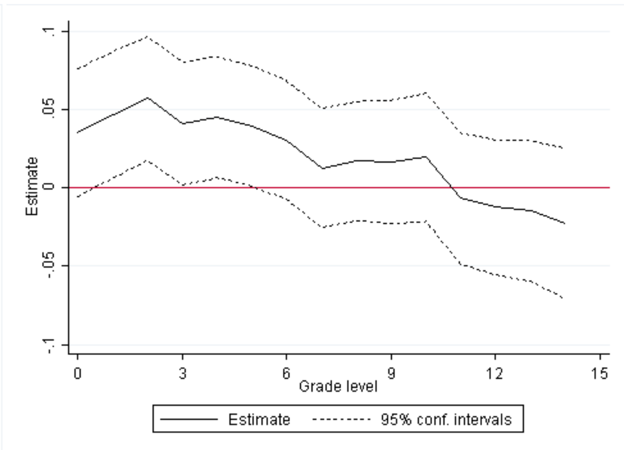
B) Marriage/cohabitation rates by grade level



C) Counterfactual employment rates, mothers



D) Maternal employment rates by grade level



Note: Panels A and C show the potential marriage and employment rates, respectively, for compliers at a given child age estimated according to Abadie (2002). Panels B and D show the estimated effects of being old-for-grade based on 2SLS regressions of marriage and employment, respectively, at a given grade level. Cutoff dummy (January = 1) used as instrument. Conditioning set includes distance to cutoff, cohort fixed effects and background characteristics (see Table A1). Dashed lines indicate 95% confidence interval.

In conclusion, Figure 12 shows that the consequences of transitions into primary schools and upper secondary school on families differ in nature and in the types of strain they impose. School start affects both parents' marital/cohabitation rates and maternal employment rates, while in the former it is affected by the transition to upper secondary school.

6.6 Selection on unobserved variables and external validity

We next investigate selection on unobserved variables and, within that context, discuss the external validity of our results.

We employ the two tests of the conditional independence assumption suggested by Black et al. (2015). We test expected outcomes for never-takers ($E(Y_0|D_1 = 0,D_0 = 0)$) and always-takers ($E(Y_1|D_1 = 1,D_0 = 1)$) against the outcomes for compliers and compute "biases," B_0 and B_1. These measure the difference in expected outcome of never-takers compared to compliers conditional on being young-for-grade (B_0), and the difference in the expected outcome of compliers compared to always-takers conditional on being old-for-grade (B_1).

Figure A4 in Appendix A shows B_0 and B_1 using parents' marriage/cohabitation rates as outcome. The figure reveals that the relationship stability of compliers' parents is higher because they are favorably selected compared to always-takers, even measured before school start, whereas there are no differences between non-treated compliers and never-takers. At age 15, however, marriage/cohabitation rates of parents to old-for-grade compliers are substantially higher relative to the parents of always-takers, suggesting that the parental response during the years around graduation from compulsory education is a consequence only certain types of families experience.

Likewise, Figure A5 in Appendix A shows B_0 and B_1 for maternal employment rates. The figure shows that there are not any significant pre-school differences in maternal employment rates, while substantial differences emerge later in the children's life.

In conclusion, Figures A4 and A5 emphasize that we more often than not reject that the expected outcomes conditional on treatment for compliers are equal to that observed for never-takers and always-takers. The figures thus illustrate that the family responses to later school start are present for particular types of families, and not readily extrapolated to the population as a whole.

7. Conclusion

This paper explores intra-family spillovers from the timing of an important life event, namely the age at which one child in the family starts school. Because the age at school start affects the entire life course, it also naturally impacts the timing of other important educational transitions. Our research design exploits quasi-random shifts in the timing of transitions into (and effectively also away from) school induced by date of birth around an administrative cutoff date.

Our findings reveal that SSA is important for family outcomes for parents and siblings. Parents are more likely to remain together during their child's childhood and adolescence and mothers work more at the time of school start if the child starts grade 1 at age 7.6 rather than 6.6. Older siblings improve their academic achievement if the focal child is older when they enroll in school. Hence, the key to understanding the increasing average age of school start seen around the Western world may lie in families – not only the individual children. This is because educational institutions, policies and decisions affect not only the child in question but also generate substantial spillover effects and affect important decisions made within the family.

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Appendix A. Supplementary tables and figures

Table A1

Summary characteristics

	All	Old-for-grade =0	Old-for-grade =1	December	January	Compliers
Old-for-grade	0.725	0.000	1.000	0.577	0.871	-
	(0.446)	-	-	(0.494)	(0.335)	
January=1	0.504	0.236		0.000	1.000	-
	(0.500)	(0.425)	(0.489)	-	-	
Boy	0.510	0.349	0.571	0.510	0.510	0.379[A,C,D,E]
	(0.500)	(0.477)	(0.495)	(0.500)	(0.500)	
Birthweight<2000g	0.042	0.031	0.046	0.043	0.040	0.056[B]
	(0.200)	(0.173)	(0.209)	(0.203)	(0.197)	
Mother's age at birth	28.940	29.070	28.890	28.916	28.963	28.880
	(4.857)	(4.899)	(4.841)	(4.889)	(4.825)	
Father's age at birth	31.598	31.835	31.510	31.570	31.626	31.765
	(5.715)	(5.870)	(5.653)	(5.755)	(5.675)	
No siblings	0.183	0.178	0.185	0.186	0.180	0.179
	(0.387)	(0.383)	(0.388)	(0.389)	(0.384)	
1 sibling	0.581	0.569	0.585	0.579	0.583	0.538[A,C,D,E]
	(0.493)	(0.495)	(0.493)	(0.494)	(0.493)	
2 siblings	0.191	0.203	0.187	0.190	0.193	0.225[A,C,D,E]
	(0.393)	(0.402)	(0.390)	(0.392)	(0.395)	
3 siblings	0.037	0.040	0.035	0.037	0.036	0.045
	(0.188)	(0.196)	(0.184)	(0.188)	(0.187)	
4 or more siblings	0.008	0.009	0.007	0.008	0.008	0.013
	(0.089)	(0.097)	(0.086)	(0.091)	(0.087)	
Parents married/cohab., age 3	0.841	0.842	0.840	0.835	0.846	0.869[D]
	(0.366)	(0.364)	(0.367)	(0.371)	(0.361)	
Father employed, age 3	0.854	0.848	0.857	0.851	0.857	0.862
	(0.353)	(0.359)	(0.351)	(0.356)	(0.350)	
Mother employed, age 3	0.721	0.721	0.722	0.715	0.727	0.742
	(0.448)	(0.449)	(0.448)	(0.451)	(0.445)	
Observations	132,039	36,252	95,787	65,554	66,485	

Note: Table shows results from averages and (standard deviation) for population of children born in December or January from December 1986 to January 2000, by old-for-grade and month of birth. The final column shows average characteristics of the compliers (those whose treatment status is determined by the cutoff) estimated using observations close to the cutoff as $\frac{\pi^A+\pi^C}{\pi^A}\left[E(X|D=1,Z=1)-\frac{\pi^A}{\pi^A+\pi^C}E(X|D=1,Z=0)\right]$ where π^A is the fraction of always-takers, π^N the fraction of never-takers, $\pi^C=1-\pi^A-\pi^N$ is the fraction of compliers (assuming monotonicity), D is old-for-grade (0/1), and Z is the cutoff (0/1). Standard errors are estimated from 100 bootstraps. Significant differences at a 10% level: [A]: average characteristics of full sample and compliers. [B]: average characteristics of children with old-for-grade = 0 and compliers. [C]: average characteristics of children with old-for-grade = 1 and compliers. [D]: average characteristics of children born in December and compliers. [E]: average characteristics of children born in January and compliers.

Table A2

Estimation results: Fertility by focal child's and sibling's age

Age difference	OLS	2SLS	2SLS
9 years older	-0.000	0.004	0.003
	(0.001)	(0.005)	(0.005)
8 years older	-0.001	-0.012*	-0.013*
	(0.001)	(0.006)	(0.006)
7 years older	0.000	0.001	-0.001
	(0.001)	(0.006)	(0.006)
6 years older	-0.002*	0.002	0.000
	(0.001)	(0.007)	(0.007)
5 years older	-0.000	0.003	0.001
	(0.001)	(0.008)	(0.008)
4 years older	-0.001	-0.001	-0.003
	(0.001)	(0.009)	(0.009)
3 years older	-0.002	-0.010	-0.011
	(0.001)	(0.011)	(0.011)
2 years older	-0.005**	0.018	0.018
	(0.001)	(0.012)	(0.012)
1 year older	-0.003**	0.008	0.008
	(0.001)	(0.009)	(0.009)
1 year younger	0.000	-0.015	-0.014
	(0.001)	(0.010)	(0.010)
2 years younger	0.001	-0.022+	-0.019
	(0.002)	(0.013)	(0.013)
3 years younger	0.001	0.013	0.016
	(0.001)	(0.012)	(0.012)
4 years younger	-0.002+	0.003	0.005
	(0.001)	(0.010)	(0.010)
5 years younger	-0.003**	0.005	0.006
	(0.001)	(0.008)	(0.008)
6 years younger	-0.000	0.010	0.011
	(0.001)	(0.007)	(0.007)
7 years younger	-0.001+	0.002	0.003
	(0.001)	(0.006)	(0.006)
8 years younger	-0.000	-0.009	-0.008
	(0.001)	(0.006)	(0.006)
9 years younger	-0.000	-0.001	-0.000
	(0.001)	(0.005)	(0.005)
Distance to cut-off	X	X	X
Covariates	X		X
Observations	132,039	132,040	132,041

Note: Table shows the estimated effects of being old-for-grade based on OLS and 2SLS regressions on fertility before and after birth of the focal child. Cutoff dummy (January = 1) used as instrument. Conditioning set includes distance to cutoff, cohort fixed effects and background characteristics (see Table A1). Standard errors in parentheses +p < 0.10, *p < 0.05, **p < 0.01, ***p < 0.001.

Table A3

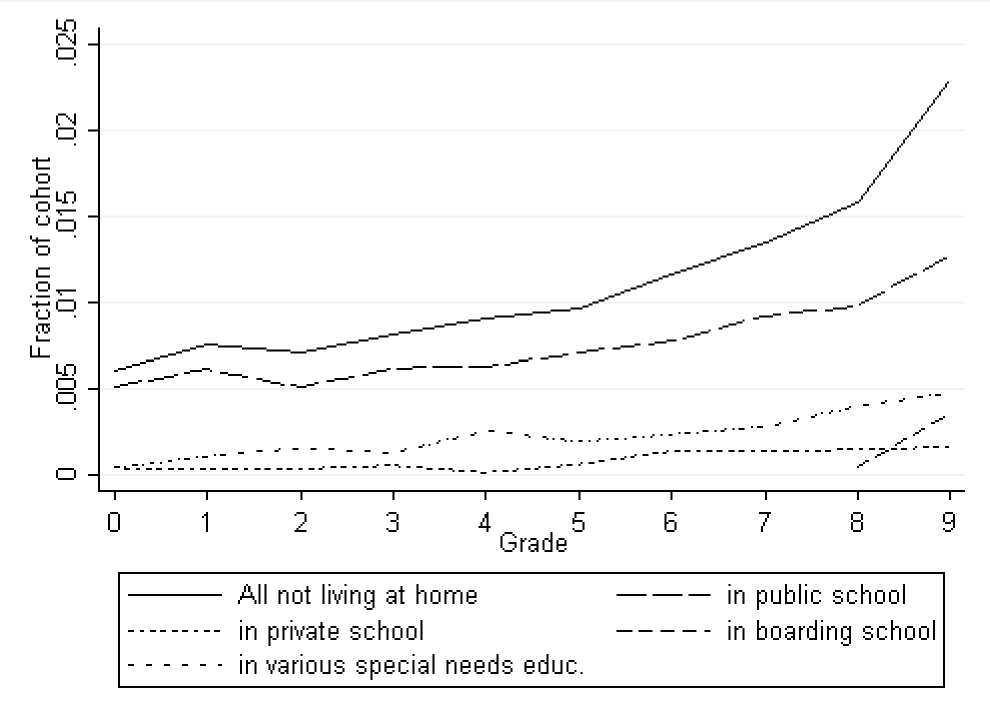
Estimation results: Older siblings' grades by distance in focal child's and sibling's age

	Age difference	OLS	2SLS	2SLS	Observations	OLS	2SLS	2SLS	Observations	OLS	2SLS	2SLS	Observations
	Sample:	Max 3 years age distance				Max 6 years age distance				Max 9 years age distance			
Math, standard deviations	1-3 years	-0.106***	0.130	0.068	29342	-0.115***	0.093	0.022	21235	-0.127***	0.200	0.132	12203
		(0.013)	(0.108)	(0.104)		(0.015)	(0.136)	(0.130)		(0.020)	(0.199)	(0.186)	
	4-6 years					-0.129***	-0.024	-0.126	12244	-0.140***	0.188	0.019	7038
						(0.020)	(0.216)	(0.204)		(0.027)	(0.299)	(0.287)	
	7-9 years									-0.151***	1.079*	1.096**	3556
										(0.039)	(0.439)	(0.416)	
Danish essay, standard deviations	1-3 years	-0.067***	0.112	0.090	29342	-0.064***	0.066	0.073	21235	-0.066***	0.148	0.196	12203
		(0.013)	(0.109)	(0.102)		(0.015)	(0.137)	(0.128)		(0.020)	(0.200)	(0.185)	
	4-6 years					-0.077***	0.139	0.028	12244	-0.084**	0.053	-0.060	7038
						(0.020)	(0.217)	(0.201)		(0.026)	(0.294)	(0.280)	
	7-9 years									-0.127***	0.089	0.179	3556
										(0.037)	(0.384)	(0.358)	
Danish grammar, standard deviations	1-3 years	-0.098***	0.054	0.025	29342	-0.101***	0.015	0.006	21235	-0.109***	0.144	0.165	12203
		(0.013)	(0.108)	(0.103)		(0.015)	(0.136)	(0.129)		(0.020)	(0.198)	(0.185)	
	4-6 years					-0.115***	0.293	0.183	12244	-0.126***	0.356	0.233	7038
						(0.020)	(0.218)	(0.205)		(0.026)	(0.300)	(0.287)	
	7-9 years									-0.138***	0.588	0.641+	3556
										(0.038)	(0.405)	(0.383)	
Danish oral, standard deviations	1-3 years	-0.081***	0.097	0.076	29342	-0.082***	0.094	0.094	21235	-0.094***	0.123	0.159	12203
		(0.012)	(0.103)	(0.099)		(0.015)	(0.137)	(0.130)		(0.020)	(0.198)	(0.185)	
	4-6 years					-0.098***	-0.018	-0.115	12244	-0.085**	-0.160	-0.265	7038
						(0.020)	(0.215)	(0.204)		(0.027)	(0.295)	(0.286)	
	7-9 years									-0.108**	0.056	0.120	3556
										(0.038)	(0.382)	(0.365)	
Focal child cohorts		1990/91-1999/00				1993/94-1999/00				1996/97-1999/00			
Distance to cut-off		X	X	X		X	X	X		X	X	X	
Covariates		X		X		X		X		X		X	

Note: Table shows the estimated effects of being old-for-grade based on OLS and 2SLS regressions on older siblings' grades at the end of 9th grade. Cutoff dummy (January = 1) used as instrument. Conditioning set includes distance to cutoff, cohort fixed effects and background characteristics (see Table A1). Standard errors in parentheses +p < 0.10, *p < 0.05, **p < 0.01, ***p < 0.001.

Figure A1

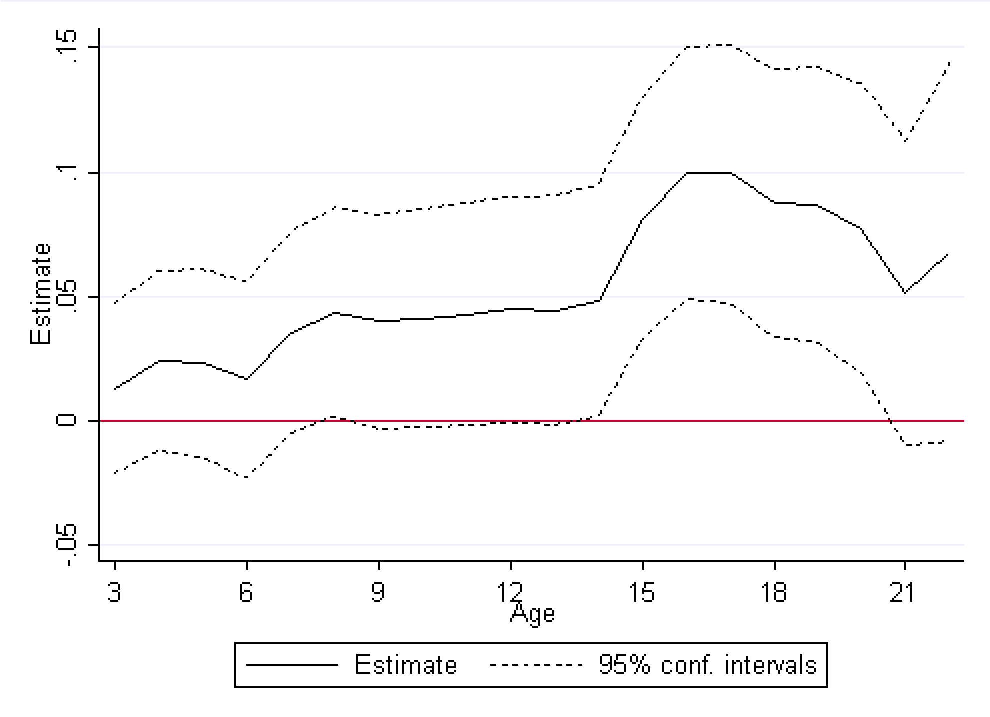
Proportion of children not living with their parents by school type and grade



Note: Figure shows the proportion of children not living with their parents by school type and grade level. Based on information from 2012.

Figure A2

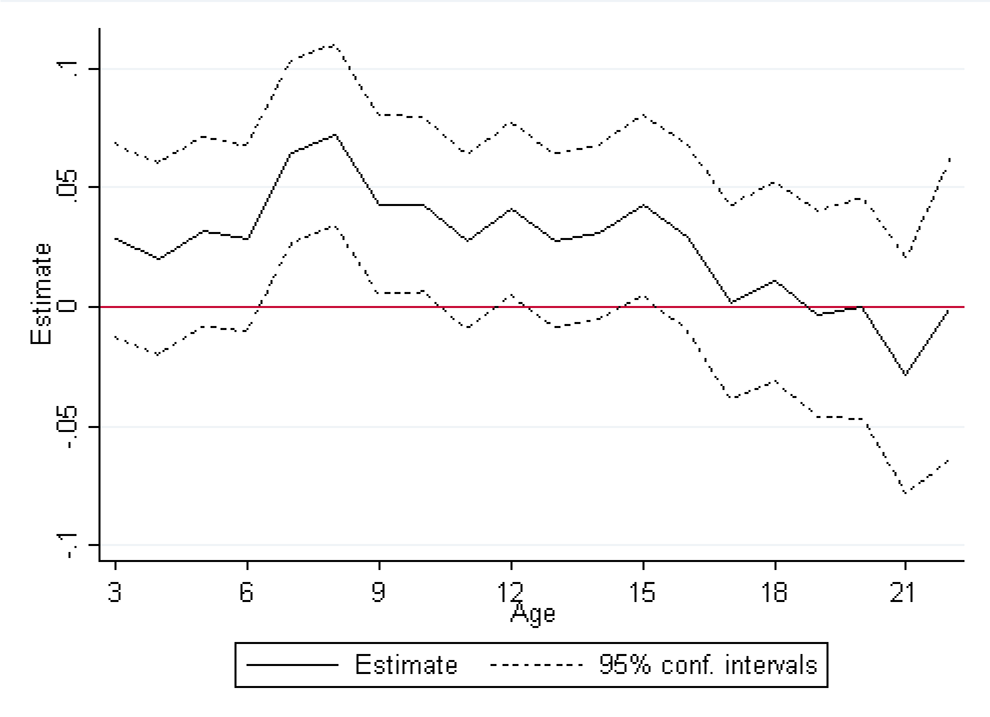
Estimation results with extended bandwidth: Fraction married or cohabiting by focal child age



Note: Figure shows the estimated effects of being old-for-grade based on 2SLS regressions of fraction of parents who are married or cohabiting at a given child age. Cutoff dummy (January = 1) used as instrument. Conditioning set includes distance to cutoff, cohort fixed effects and background characteristics (see Table A1). Dashed lines indicate 95% confidence interval.

Figure A3

Estimation results with extended bandwidth: Employment of mother by focal child age

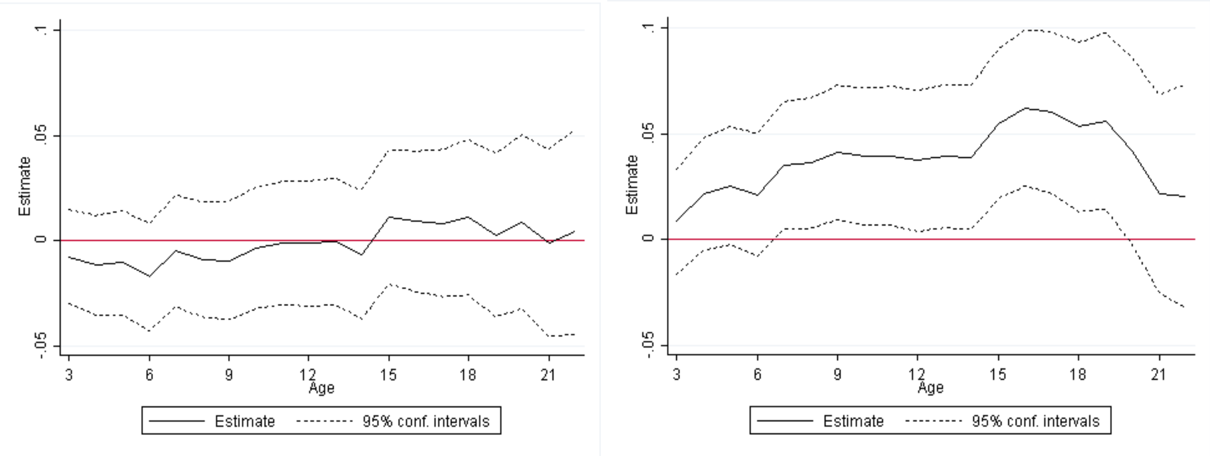


Note: Figure shows the estimated effects of being old-for-grade based on 2SLS regressions of mother's employment at a given child age. Cutoff dummy (January = 1) used as instrument. Conditioning set includes distance to cutoff, cohort fixed effects and background characteristics (see Table A1). Dashed lines indicate 95% confidence interval.

Figure A4

Selection bias as measured for the outcome parents married or cohabiting

B_0 (never-takers compared to compliers) *B_1 (compliers compared to always-takers)*

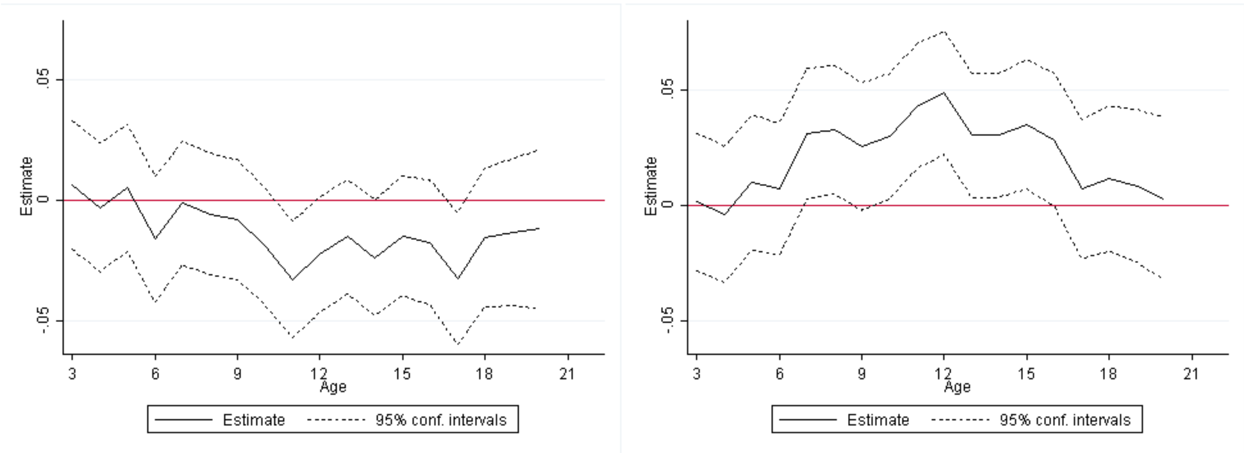


Note: Figure shows test for the conditional independence assumption ATET (left) and ATEN (right).

Figure A5

Selection bias as measured for the outcome maternal employment rates

B_0 (never-takers compared to compliers) *B_1 (compliers compared to always-takers)*



Note: Figure shows test for the conditional independence assumption ATET (left) and ATEN (right).

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