Can Academic Redshirting Shrink the Education Gender Gap? Causal Evidence on Student Achievement and Mental Health

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Abstract

I study voluntary delays in the school entry of age-eligible children ("academic redshirting"), using Hungarian administrative test score and medical prescription data, and mental health surveys. I identify a new Local Average Treatment Effect of starting school a year older due to redshirting, by exploiting a school-readiness evaluation required only for potentially redshirted children born before January 1. Instrumenting with post-January 1 births, I estimate effects for non-school-ready children who could overcome developmental deficits with redshirting but are deterred by the evaluation. I find improved test scores, high school track choices, educational aspirations, and graduation rates—but only for boys, who are also less anxious, more confident, and less bullied. Thus, redshirting can shrink the education gender gap.

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1 Introduction

A non-trivial and potentially increasing share of parents are postponing the entry of their age-eligible children into primary education, a practice known as "academic redshirting." For instance, 96 percent of 6-year-olds were enrolled in kindergarten in the United States in 1968, a figure which fell to only 84 percent by 2005 (Deming and Dynarski, 2008). The share of redshirted children is 5-10 percent among US kindergartners, with boys and high-status children being over-represented among them (Bassok and Reardon, 2013; Dobkin and Ferreira, 2010; Dhuey et al., 2019; Graue and DiPerna, 2000; Schanzenbach and Larson, 2017; Cook and Kang, 2020)—just as non-school-ready children, who are typically the lowest achieving, are disproportionately redshirted (Ricks, 2022; Cook and Kang, 2018). Even in many European countries, where the prescribed school-starting age (SSA) is 6 years of age for all,[1] the share of redshirted children is also 5-10 percent. In Hungary, where the prescribed SSA is 6 or 7 based on birth month, the share of redshirted children increases from 7 to 60 percent among students born between September and May. Also, with free childcare, non-school-readiness is the main driver of redshirting in Hungary, and conditional on early childhood development markers, low-status children are not any more or less likely to be redshirted. Thus, the Hungarian setting is particularly suited to study redshirting for non-school-ready children, who are typically the target of redshirting.

Despite millions of families making decisions about redshirting each year and its importance for policy, no previous research has identified the effect of starting school a year older due to the voluntarily delayed school entry of age-eligible children, i.e., due to redshirting itself. Schanzenbach and Larson (2017) even notes that "*[n]o one has conducted a true randomized trial related to redshirting.*" And while there is extensive previous research on the effects of starting school a year older due to complying with school entry rules and enrollment cutoff dates, redshirting is a fundamentally different decision made under different considerations for potentially different children on the margin. Although Cook and Kang (2018) suggests that "*[t]here is every reason to believe that redshirting would convey the same 'old for grade' benefits,*" whether it actually does so remains an empirical question.

To identify the effect of starting school a year older due to academic redshirting, I exploit the unique setting of the pre-2018 Hungarian educational system, which has two distinct cutoff dates. First, the primary school enrollment cutoff date is June 1, prescribing children born before June 1 to start school at age 6, and requiring children born on/after June 1 to start school at age 7. In this setting, redshirting is the voluntary practice of parents choosing to delay their children's school entry—particularly of children born before the school enrollment cutoff date—and consciously opting not to comply with the school entry rule. Second, the January 1 cutoff affects the practice of redshirting: While September-to-May-born children are allowed to be redshirted, only those children whose parents intend to delay their school entry and who were born before January 1 must take a school-readiness evaluation; children who were born on/after January 1 are exempt from such a test and can be redshirted on the parents' decision alone. Such regulation exogenously varies the likelihood that the parents' intention of redshirting is realized at the January 1 cutoff, independent of their perceptions and preferences. I am not aware of any other country with such a unique administrative barrier that can be used to identify the effect of starting school a year older due to academic redshirting.

My key contribution is the novel identification of the Local Average Treatment Effect (LATE) of

[1] OECD Education Statistics, Education At a Glance: Enrollment Rate by Age (2020); available here, Table B1.1.

starting school at age 7 instead of 6 specifically due to academic redshirting, and showing its first causal estimates. I instrument with an indicator for post-January 1 births, among children born in a 3-month window around January 1. I identify the LATE for compliers from the discontinuous jumps at the cutoff—in the share of redshirted children (the first-stage relationship) and in student outcomes (the reduced-form relationship). While previous research on starting school older due to complying with enrollment cutoff dates is rich, direct evidence on redshirting is more relevant for parents who may consider redshirting given concerns about their child's early childhood developmental trajectory, for policy-makers who may need to deliberate on regulating parents' opportunities for and institutions' roles in redshirting, and for childcare teachers who may need to give recommendations on redshirting.

The unique Hungarian setting allows me to identify the LATE of starting school a year older due to academic redshirting for the policy-relevant group of children—those deemed less school-ready. At the January 1 cutoff, compliers are those who are not redshirted if born before January 1 (for whom the compulsory school-readiness evaluation makes redshirting costly), but who would be redshirted had they been born on or after January 1 (for whom redshirting is relatively costless, as no school-readiness evaluation is required). Many of these complier children are likely also *not school-ready*, at least as perceived by their parents, for whom an extra year before school could help overcome developmental deficits; yet, they are deterred from redshirting by the official school-readiness evaluation. In contrast, parents of children born just before the Hungarian school enrollment cutoff date, June 1, have the option to redshirt their younger and thus typically less school-ready children without the evaluation, and compliers at the June 1 cutoff are *school-ready*. That is, compliers at the June 1 cutoff are those that start school at age 7 solely because they were born on or after June 1 and, had they been born before, would have started school at age 6 *without being redshirted*. I provide new direct evidence that compliers at the January 1 cutoff are indeed less school-ready. Thus, the non-school-ready compliers at the novel January 1 cutoff in this paper offer new insights about redshirting, distinct from what we learn from the existing literature on starting school a year older due school entry rules and enrollment cutoffs – which is indirectly informative about the effects for less mature children, as school entry rules are variably enforced across contexts, and thus affect non-school-ready children to a different extent.

Additionally, I assess if redshirting shrinks the education gender gap, by estimating the LATE of starting school at age 7 due to redshirting by gender. In Hungary, the education gender gap is huge: In 2008–2017, Grade 10 boys were 42 percent more likely to attend the vocational secondary track and 27 percent less likely to attend the academic track; and boys were 70 percent more likely to aspire for less than a high school degree and, ultimately, were 10 percent less likely to obtain a high school degree. Given that boys' and girls' developmental trajectories start to diverge early (e.g., DiPrete and Jennings, 2012; Fidjeland et al., 2023; Brandlistuen et al., 2021; Stangeland et al., 2018, among others), and that redshirting is primarily intended to help non-school-ready children, among whom boys may be overrepresented, redshirting may have meaningful potential to shrink the gap.

I show that starting school at age 7 due to academic redshirting is not only more prevalent for boys, but that it sets boys—but not girls—up to be college-bound in high school tracks, closes the gender gap in high school diploma aspirations, and narrows the high school completion gap by up to 60 percent. Given the "boys' crisis" in education (e.g., Fortin et al., 2015; Bertrand and Pan, 2013; Cornwell et al., 2013; Golsteyn and Schils, 2014; Autor et al., 2019), my results suggest that academic redshirting is among the few educational options with a bigger effect for boys—thus rendering it a particularly viable option for shrinking education gender gaps—and support the policy recommendation made by Reeves (2022) of academic redshirting of boys. Based on the extensive review on the origins of the education gender gap and of the empirical evidence by Haeck et al. (2023), none of the early childhood and educational policies have a clearly bigger positive impact for boys. Several studies even suggest that girls might benefit more from early childhood education and care (ECEC) enrollment in terms of skill development (e.g., Goodman and Sianesi, 2005; Anderson, 2008; Cornelissen et al., 2017; Havnes and Mogstad, 2015). Thus, my results on the gender differences are important and policy-relevant.

The remainder of the paper is organized as follows. Section 2 reviews the previous deep literature on starting school a year older due to complying with school entry rules and enrollment cutoff dates. Section 3 describes the Hungarian ECEC setting, with a focus on the school-readiness evaluation that is compulsory only for potentially redshirted children who were born before January 1. Section 4 describes the data sources and the outcomes: Using Hungarian administrative test score data for 2008–2017, I look at mathematics and reading test scores for Grades 6, 8, and 10, grade repetition, secondary school track choice, and educational aspirations in Grade 10. Linking administrative labor records to the test scores, I observe actual graduation outcomes from high school, and linking administrative health records to test scores, I look at prescriptions for psycholeptics (a medication for anxiety). Using Hungarian survey data for 2006, I look at self-reported mental health of Grade 8 students, measured by anxiety and self-confidence, and their experiences of being bullied in class.

Section 5 discusses my identification strategy for the LATE of starting school at age 7 due to redshirting: I examine the relationship between the share of children starting school at age 7 and average student achievement and mental health outcomes of children born on either side of the January 1 cutoff, controlling for a linear trend in birth month around the cutoff and a rich set of background characteristics. Section 5.3 provides two pieces of reassuring descriptive evidence: (i) neither the share of boys nor the share of children of various backgrounds change discontinuously at the January 1 cutoff and there is no bunching on either side; and (ii) even though the propensity of being redshirted is related to adverse events in early childhood (e.g., a shock at birth or developmental challenges), the share of children experiencing such adverse events does not change discontinuously at the cutoff.

Section 6.1.1 presents the LATE estimates of starting school at age 7 due to academic redshirting: 0.12 standard deviations (SDs) on Grade-10 mathematics testscores, a lower (higher) likelihood to attend the vocational (academic) high school track by 36 (15) percent, a higher propensity to aspire for a tertiary degree by 12 percent, and a higher chance to graduate from high school by 11 percent. In comparison (from Section 6.2), the LATE estimates of starting school at age 7 due to complying with the school enrollment (June 1) cutoff date for mathematics and reading test scores are 0.14 and 0.18 SDs, respectively, which are in line with existing literature (e.g., with Puhani and Weber, 2007; McEwan and Shapiro, 2008; Elder and Lubotsky, 2009). I find both boys *and* girls to have significantly positive estimates of starting school at age 7 due to complying with the enrollment cutoff, which is in stark contrast with my estimates due to redshirting that are only significantly positive for *boys*.

Section 6.1.2 shows that starting school at age 7 instead of 6 due to redshirting significantly affects *boys* only: for boys, the effect is 0.20-0.25 SDs on Grade 10 test scores, it is -51 and 31 percent on the likelihood of attending the vocational and the academic tracks, respectively, it is 26 percent on the propensity to aspire for a tertiary degree, and it is 15 percent on the chance to graduate from high school. There is no detectable effect for girls for any of the outcomes. These estimates are robust to varying windows and functional forms of the trend in month of birth, and to accounting for years spent in childcare and to relative age effects in the classroom.

Section 7 looks at the mechanisms. I show new evidence for the negative selection among the redshirted compliers that can partially explain the differential LATE estimates by gender. First, I show that the propensity of being redshirted is related to adverse events in early childhood—shocks at birth, chronic and developmental difficulties, family tragedies—using exceptionally rich early childhood data. Second, I show direct evidence that the compliers are, on average, less likely to be school-ready at the redshirting cutoff of January 1: they are significantly more likely to have been born with low birth weight, been separated from their mother before their 1^{st} birthday, started speaking at an older age, and had difficulties with speaking, cognition, coordination, and attention at ages 4–5. Boys are not only overrepresented among the compliers around the January 1 cutoff, but they also started to speak at an older age than girls and were more likely to have been diagnosed with ADHD at ages 4–5. Low-status children, while more likely to be redshirted in the Hungarian setting, are not any more or less likely to be redshirted conditional on developmental markers and shocks in early childhood.

Lastly, I show new evidence that mental health is another key mechanism: Boys who start school at age 7 due to redshirting are less anxious and less likely to take anxiety medications, feel more confident and are less prone to being bullied in class. Thus, even though boys fall behind girls at ages 1–4 in speaking, attention, and impulse control, my results suggest that redshirting may help boys overcome non-school-readiness by improving both their student achievement and their mental health.

2 Literature Review

The existing in-depth research on school-starting age (SSA) overwhelmingly uses school (or kindergarten in the US) enrollment cutoff dates or entrance ages for identification, and thus identifies the impact of starting school a year older due to being born after and complying with the age cutoff.

In terms of research design, existing literature typically uses variation in children's SSA that stems from different birthdate and age cutoffs for prescribed school or kindergarten entry, potentially by geography. For example, Elder and Lubotsky (2009) exploits variation in prescribed kindergarten entry age (KEA) stemming from the distribution of birthdates among those who comply with their state's enrollment cutoff date, and differences across state cutoff dates among children born on the same day (but in different states). Similarly, Barua and Lang (2016) instruments actual KEA (age 6 *versus* 5) with an indicator of whether the child was required by state law to start school a year older (i.e., if the child's birthdate falls after their state's kindergarten entry age cutoff date). For Germany, Puhani and Weber (2007) contrasts children's actual and prescribed SSAs, the latter of which depends on birth month and state. Attar and Cohen-Zada (2018) exploits variation in cutoffs that correspond to the same (Jewish lunar calendar) cutoff date in Israel, by comparing the outcomes of children born on different days of the same calendar year over time. Jenkins and Fortner (2019) and Cook and Kang (2020) exploit a policy change in the birthdate enrollment cutoff in North Carolina that forced children born in a six-week window to delay kindergarten entry. These studies—even if using the term 'redshirting'—focus on the school entry decisions of those who comply with school enrollment policies.

Previous research focuses primarily on the effect of starting school a year older on student achievement, measured by test scores up to Grade 8, grade repetition, choice of secondary school track, IQ at age 18, and initial earnings at labor market entry. Non-cognitive and prosocial outcomes are also examined (e.g., persistence, stability, internalizing behavior, teenage pregnancy, and crime).

The literature on test scores is consistent across countries in that children who enter school at an older age—due to the school enrollment cutoff date—tend to perform better on achievement tests. For example, Altwicker-Hamori and Kollo (2012) provides evidence for Hungary, Attar and Cohen-Zada (2018) for Israel, Crawford et al. (2010) for England, Dong (2010) and Elder and Lubotsky (2009) for the United States (and Dhuey et al. (2019) for Florida), Fredriksson and Ockert (2014) for Sweden, McEwan and Shapiro (2008) for Chile, Puhani and Weber (2007) for Germany, Ponzo and Scoppa (2014) for Italy, and Smith (2009) for British Columbia. The range of the impact on test scores is 0.25–0.4 SDs, generally decreasing across Grades 4–8 with slightly larger effects for disadvantaged children; e.g., Datar (2006a) finds that higher KEA is more effective for at-risk children.

Similarly positive effects on grade repetition and secondary school track choice are documented in McEwan and Shapiro (2008), Muhlenweg and Puhani (2010), and Puhani and Weber (2007). The authors find that higher SSA decreases (increases) the chance of repeating Grade 1 (being in the most advanced school track) by 2 (12) percentage points, while early school entrants are significantly less likely to enter the academic track (Gymnasium). Closely related to educational success, Gorlitz et al. (2022) finds that higher SSA increases competencies in receptive vocabulary, and Balestra et al. (2020) finds that children with higher SSA are less likely to develop behavioral and speech problems, but their ADHD and dyslexia/dyscalculia seem unaffected. At the same time, Barua and Lang (2016) and Fertig and Kluve (2005) do not find any significant effects of higher KEA and SSA on educational attainment, respectively, and Hemelt and Rosen (2016) finds mixed evidence that children eligible to start school at a younger age are more likely to complete high school, but underperform while enrolled.

Non-cognitive and behavioral measures have also been found to be related to SSA (due to the school enrollment cutoff date). For example, Cook and Kang (2016) finds that those born just after the kindergarten cutoff date are more likely to drop out of high school and to commit a felony offense by age 19 in the US, while Landerso et al. (2017) finds that a higher SSA lowers crimes committed by boys on both margins in Denmark. Evans et al. (2010) and Elder (2010) show that older children have a significantly lower incidence of ADHD diagnosis and treatment, and Muhlenweg et al. (2012) presents German evidence that a higher SSA has a stable positive effect on persistence, a short-run negative impact on hyperactivity, and a long-run effect on adaptability to change. Fortin et al. (2013) finds that being younger in class aggravates an underlying propensity towards ADHD, which is more prevalent for boys in Canada. Exploiting US state-level variation in school enrollment cutoff dates, McAdams (2016) finds that higher SSA reduces the incidence of incarceration. Dee and Sievertsen (2015) finds that a higher SSA reduces inattention and hyperactivity for ages 7–11 in Denmark, Datar and Gottfried (2015) finds positive effects on self-control, interpersonal skills, and approaches to learning in the US, and Horn et al. (2024) finds positive effects on internal Locus of Control in Hungary. Yet, Lubotsky and Kaestner (2016) does not find a relationship between internalizing behavior or self-control and KEA, and Pena and Duckworth (2018) finds inconsistent effects of higher SSA on grit.

Regarding adult outcomes and SSA, the evidence is mixed. Using data on the entire population of Norway, Black et al. (2011) finds a small positive effect of lower SSA on IQ scores at age 18, and a short-run positive effect on earnings that disappears by age 30. However, Dobkin and Ferreira (2010) finds no evidence in California and Texas that SSA affects job market outcomes (wages or the probability of employment). For Sweden, Fredriksson and Ockert (2014) finds that SSA affects educational attainment and the timing of labor supply but not earnings (except for those with low-educated parents). Du et al. (2012) finds suggestive evidence that relative age has a long-lasting effect on career, given the disproportionately small share of summer-born CEOs. Arnold and Depew (2018) finds that a higher SSA for boys is linked to better health in adulthood, but with no long-term labor impacts. Oosterbeek et al. (2021) finds that children born just before the school enrollment cutoff enter the labor market younger and with more experience, leading to higher earnings until age 40.

Within the literature on SSA, relative age (RA) effects have been found to be positively related to educational success. For instance, Dhuey and Lipscomb (2010) shows that an additional month in RA decreases the probability of receiving special education by 2–5 percent, and Bedard and Dhuey (2006), Pena (2017), and Sprietsma (2010) show that it increases test scores. Dhuey and Lipscomb (2008) finds that the oldest students are more likely to be leaders in high school (and receive an associated wage premium), and Schneeweis and Zweimuller (2014) shows that they choose more ambitious secondary tracks. Patalay et al. (2015) finds that lower RA is associated with increased internalizing symptoms, poorer quality peer relationships, and greater mental health difficulties on functioning at school, and Aliprantis (2014) finds that returns to RA may be negative for the oldest students. Johansen (2021) finds that the youngest girls in class are more likely to have an abortion, alcohol poisoning, and earlier motherhood (Black et al. (2011) substantiates these for Norway), Ballatore et al. (2020) finds that they are more likely to be victimized, and Fumarco et al. (2024) finds that RA affects eating disorders.

Existing literature on relative age effects does not definitely answer the question of whether these effects remain important beyond school-starting age, which may be an important consideration for parents (who need to decide whether to redshirt their children or be concerned about other parents' redshirting behavior potentially harming their children) and policy-makers (for how to regulate school-starting age). Cascio and Schanzenbach (2016) supplies evidence from the Project STAR experiment, in which kindergarten-aged children were randomly assigned to classrooms, effectively randomizing their relative age. They find that holding own age constant, younger children performed no worse on achievement tests, and were no more (less) likely to repeat a grade (take college-entry exams).

Evidence on selection into academic redshirting is more scarce, beyond the result that, at least in the US, it is more common among males, whites, and high-status non-minority children (e.g., Bassok and Reardon, 2013; Schanzenbach and Larson, 2017; Dobkin and Ferreira, 2010). The results of Cook and Kang (2018) suggest an interaction between abilities and family background: Among first-graders in North Carolina public schools, low-status children are more likely to be redshirted without controlling for academic indicators capturing school-readiness, but are less likely to be redshirted once those are taken into account. Jenkins and Fortner (2017) finds that redshirters are more prone to have special educational needs, but also finds a positive selection into redshirting. Using data on kindergartners at Michigan public schools and a Marginal Treatment Effects framework, Ricks (2022) finds that redshirters would have been the lowest-achievers otherwise, but they benefit the most from it. Finally, some studies also analyze the implications on gaps and equity-efficiency trade-offs (Ricks, 2022; Lenard and Pena, 2018), with some focusing on gender gaps (e.g., Cook and Kang (2018, 2020)).

In sum, existing literature that uses school enrollment cutoff dates for identification, shows positive impacts of starting school a year older due to being born after and complying with the age cutoff on student achievement and non-cognitive skills is positive, but the evidence on adult outcomes is mixed.

3 Institutional Background

Hungary has a universal and free childcare system, in which more than 90 percent of children spend at least 3 years in childcare before primary school.[2] The last childcare year is the school-preparation year, roughly equivalent to the North-American kindergarten year. The minimum compulsory childcare is 4 hours per day, starting from September 1 of the calendar year in which the child turns 5 (Public Education Act, 24(3)). Academic redshirting is then the voluntary delay of school entry of an age-eligible child which, in the Hungarian context, means postponing the start of primary school to the next academic year and repeating the school-preparation year (which, in the North-American context, would be equivalent to repeating the 'developmental kindergarten' or 'young kindergarten'). Given the free childcare, there is no direct cost of redshirting in Hungary (unlike in North America).

In Hungary, the compulsory childcare ends with the start of compulsory schooling. All children who turn 6 before June 1 in a given calendar year are prescribed to start primary school on September 1 of that particular calendar year (Public Education Act 6(1)). Thus, the Hungarian school enrollment cutoff date is June 1. If complying with the prescribed SSA, (i) children born between January and May start primary school at an average age of 6 years 3.5 months to 6 years 7.5 months, respectively; (ii) children born between June and August start primary school at an average age of 7 years 0.5 months to 7 years 2.5 months; and (iii) children born between September and December start primary school at an average age of 6 years 8.5 months to 6 years 11.5 months. This rule generally results in the youngest children in a class being born in May, and the oldest ones being born in June. While, under exceptional circumstances, it is possible for a child to start school a year earlier than prescribed, this is rare: less than 1 percent of children born between September and May start school at age 5, and more than 95 percent of those born in the summer start school at age 7 (as prescribed).

With some restrictions, redshirting is possible in Hungary, and before 2018 it was decided by the parents together with the childcare institution's board of teachers, and (depending on birth month) the local so-called 'Developmental Advisory Board' (DAB). While both the childcare institution and the local DAB are part of the public education system, the latter is an independent pedagogic institution maintained by local governments, which evaluates the school-readiness of potentially redshirted children (Hungarian Public Education Act, 35(4)). The local DAB also evaluates children's learning and socialization skills until adulthood, and proposes skill-enhancing activities involving the parents.

Table 1 summarizes primary school entry in Hungary, by month of birth, under the regulations in place before 2018. *Regime 1* pertains to children born in September–December. These children are prescribed to enter primary school at age 6, and the administrative barrier to academic redshirting is present for them—they can be redshirted only if the parents request an additional year in childcare and have their children take the compulsory school-readiness evaluation, and both the childcare institution's board of teachers and the local Developmental Advisory Board support the parents' re-

[2] The description here pertains to before 2018. For more institutional details, please see Appendix A.

quest (Hungarian Public Education Act, 24(5)). *Regime 2* pertains to children born in January–May. These children are prescribed to enter primary school at age 6, and there is no administrative barrier to redshirting for them: they can be redshirted simply if the parents request an additional year in childcare, and the childcare institution's board of teachers supports the parents' request;[3] i.e., the Developmental Advisory Board's evaluation or support is not needed. *Regime 3* pertains to children born in June–August, who are required to enter primary school at age 7, and thus redshirting is not allowed for them (and a summer-born child would need to be tested to start school at age 6).

Table 1: **Predicted Path into Primary School, by Month of Birth**

Month of Birth:	**Prescribed School-Starting Age (SSA)**	**Opportunity to Redshirt?**	**Regime**
September–December	6 years, 8.5–11.5 months	yes, with administrative barrier	1
January–May	6 years, 3.5–7.5 months	yes; no administrative barrier	2
June–August	7 years, 0.5–2.5 months	no	3

Besides SSA, the January 1 cutoff also affects childcare-starting age, although to a much lesser extent. Specifically, childcare institutions can admit 2-year-old children, provided that all the age 3 and older applicants living in the municipality are admitted. Nonetheless, children who are turning 3 within 6 months of acceptance are preferred. As acceptances typically take place at the beginning of June and childcare starts only at the beginning of September, children born between January–May are most likely to enter childcare at age 3, while the summer-born children most likely at age 4. Thus, fall-born children are more likely to enter childcare early at age 2, as these children are preferred by the childcare institutions if there is a wait list (Hungarian Public Education Act 24(1)); they also have already spent 4 years in childcare when the possibility of redshirting becomes relevant for them.

The school-readiness examination conducted by the local Developmental Advisory Board is standardized and free of charge. In these exams, developmental experts first examine how children behave in social settings with others; typically, during drawing exercises, they investigate to what extent children can work without being distracted, cope with and adapt to new situations, and avoid being overly influenced by others. Then, in a one-on-one situation, the experts assess the extent of children's awareness about their environment and themselves, and children's cognitive and recall abilities.

For insight into the costs of a school-readiness examination from the parents' perspective, I conducted interviews with experts of local Developmental Advisory Boards in Budapest, Hungary, in the summer of 2013. First, in most cases, the accompanying parent needs to miss work for the duration of the examination. Additionally, travel costs are incurred by families that live in municipalities that do not have a local Developmental Advisory Board nearby. Second, parents might be afraid of their child potentially receiving a poor or problematic evaluation, the documentation of which parents may presume to harm their child's ensuing educational career. Third, parents might be reluctant to go in front of an unknown formal committee, especially if the default option is to discuss their child's development and school entry with childcare teachers, with whom the parents are already familiar.

[3]According to the enactment of the Ministry of Education, 11/1994. (VI. 8.), 15(5)b, the childcare institution's board is obliged to suggest an additional year in childcare for children assigned to start primary school on September 1 in a given academic year, but who are not school-ready based on the opinion of the board of childcare teachers.

There are no other direct costs of redshirting, since childcare is free in Hungary.

In sum, the main institutional reason for expecting a discontinuous jump in the share of redshirted children at the January 1 cutoff is the higher cost, due to the compulsory school-readiness evaluation for potentially redshirted children born before January 1. I use this source of exogenous institutional variation at the January 1 cutoff to identify the LATE of starting school a year older (at age 7, not 6) due to academic redshirting. In Appendix C.4, I show that my main LATE estimates are robust to accounting for the potentially endogenous nature of childcare-starting age and years spent in childcare.

4 Data, Sample, and Measurement

4.1 Sources: Administrative Data on Test Scores, Prescriptions and High School Graduation, and Survey Data on Mental Health and Early Childhood Events

I use data from three sources: (1) administrative data from the *Hungarian National Assessment of Basic Competences* (HNABC), spanning 2008–2017, on test scores in Grades 6, 8, and 10, grade repetition, high school track, educational aspirations, month of birth, childcare attendance, and socio-economic characteristics; (2) survey data from the *Hungarian Life Course Survey* (HLCS) on self-reported anxiety, mental exhaustion, confidence, and experiences of being bullied, and retrospective information on early childhood events, for a 10 percent sample of 8^{th}-graders in 2006; and (3) administrative data from *Admin3* (for a 50 percent random sample of (1)), spanning 2009–2017, on medical prescriptions for psycholeptics (for treating anxiety), and a subsample on high school graduation.

The HNABC covers all—public, private, and religious—schools. The national tests do not assess students' factual knowledge of the compulsory curriculum; they measure the extent to which students are able to apply their acquired skills in realistic settings, and if they possess the necessary competences for further development. The tests are low-stakes, their completion is mandatory for all, almost all students' tests are centrally processed, and the results count toward each school's average. Exceptions are made for students with special educational needs, such as autists, and students with a temporary injury that render them physically unable to complete the test. While students with behavioral problems or dyslexia/dysgraphia/dyscalculia are required to complete the test, their results are not processed centrally, and do not count toward their schools' average score.

While almost all students are in the HNABC and do have valid test scores, I restrict my sample to those students for whom basic background information is available. I obtain socio-economics variables (such as parents' education levels), year and month of birth, and time spent in childcare from the HNABC student background survey. This survey provides detailed information about the students' background, but its completion was not compulsory. I restrict the analysis to children for whom information about gender, time in childcare, and birth year and month is available and who have a valid test score. The final sample contains 76.5-83 percent of the original sample, and previous analyses by the data provider suggest that the excluded observations are random by gender and birth date (see Table B2). Unfortunately, the HNABC does not make available the exact day of birth.

The HLCS is an individual panel survey conducted annually. The original 2006 sample (with 10,022 respondents) is a selected 10 percent random sample from the population of Grade 8 students with valid HNABC test scores in that year. Students were followed throughout their school career and were asked questions on their ethnicity, school, and family background (including poverty and home environment). Each wave had a particular block of questions on, for instance, early childhood environment, secondary school application, alcohol and drug usage, social networks, and prejudices.

Administrative data on medication expenditures for a 50 percent random sample of the HNABC, spanning 2009–2017, is merged from the *National Health Insurance Fund Administration*. The data contain all social insurance expenditures on medications prescribed either by a public or a private provider, by codes of "Anatomical Therapeutic Chemical" (ATC). For a random subset of this sample, I also observe the highest completed level of secondary education (prior to tertiary education) stemming from the *Educational Authority* and the *Ministry of Finance*. For the graduation outcomes, I use information on all students in Grades 6, 8, and 10 in this subsample, who were born in cohorts that had a chance to graduate by 2017 (the last year in the data containing the information on graduation).

4.2 Measurement of the Endogenous Variable (Starting School at Age 7 or 6), the Outcome Variables, and Control Variables

Starting School at Age 7 or 6. I construct the binary indicator of starting school at age 7 from information on year and month of birth, year-of-the-test and grade repetition as follows. First, the child's age at the time of the test is estimated by assuming that children were born on the 15^{th} of their respective birth month, using the information that the national test is in mid-May in all years:

$$\text{age-at-the-test (in months)} = (\text{year-of-the-test} - \text{year-of-birth} - 1) \times 12 + (12.5 - \text{month-of-birth}) + 4.5.$$

School-starting age is then backed out from age-at-the-test, subtracting time elapsed between primary school entry and the test: e.g., for those in Grade 6, assuming that school starts on September 1, students who did not repeat a grade spent 68.5 months between primary school entry and the Grade 6 test; with repeating 1, 2 and 3 grades, the time between primary school entry and the Grade 6 test becomes 80.5, 92.5, and 104.5 months, respectively. Subtracting time elapsed from age-at-the-test gives the information if the child started school at the age of 7 or 6. I verify that my constructed measure is consistent with information on childcare-starting age and years in childcare in the HNABC background survey (which does not contain whether the child was evaluated by the school-readiness committee or whether concerns were expressed about their school-readiness around age 6; the exact day of birth is also not known, which would be needed for implementing an RDD by day of birth).

Outcome Variables. With respect to students' performance in school, I measure:
— *student achievement* with test scores, which have mean of 0 and standard deviation of 1, by the interactions of Grade (6, 8, and 10), year (2008–2017), and subject (mathematics and reading);
— *grade repetition* with an indicator variable of repeating at least one grade by Grades 6, 8, or 10;
— *secondary school track choice* with binary variables indicating whether the student attends the j^{th} track in Grade 10, where track #1 is the lowest secondary school track, granting a vocational degree upon completion but no high school diploma, and continuation onto tertiary education is not allowed; track #2 is the middle secondary school track, granting both vocational and high school diplomas

upon completion; and track #3 is the highest (academic) secondary school track, which grants a high school diploma upon completion; #2 and #3 allow continuation onto tertiary education;
— *educational aspirations* with indicators of whether the student aspires to earn a high school diploma or more advanced post-secondary degree(s) as their highest level of education;
— *graduation* with indicators of whether the student drops out of secondary school without a degree, or obtains a vocational degree or a secondary school (i.e., high school) degree.

With respect to the students' mental health and well-being outcomes, I measure:
— *standardized anxiety* with the (standardized) mean score from students' responses to how often they experience the following indicators of Generalized Anxiety Disorder (GAD) (5: 'daily'; 4: 'several times per week'; 3: 'weekly'; 2: 'monthly'; 1: 'never or rarely'): (i) stomachache, (ii) bad mood and fatigue, (iii) irritability, (iv) fearfulness, (v) nervousness, (vi) problems with falling asleep, (vii) waking up during the night, (viii) dizziness, (ix) exhaustion, (x) nausea and vomiting; then, *often anxious* is a binary variable indicating experiencing any of the (i)–(x) conditions daily or several times per week;
— *lack of confidence* with a binary variable indicating whether the student agrees with any of the followings: (i) "*I am inclined to consider myself unsuccessful without any talent.*" (ii) "*I feel I cannot be proud of myself.*" (iii) "*I wish I had more respect for myself.*" (iv) "*I feel I am not good at anything.*"
— *bullied* with an indicator variable for being bullied in class for appearance or student achievement;
— *N05d expenditures* which capture prescriptions for psycholeptics (ATC code N05; antipsychotics and sedatives form the major groups in this category, primarily for treating anxiety).

Background Control Variables: include parental education (primary education or less, vocational degree, secondary school degree, and tertiary degree); children are labelled as 'disadvantaged' or 'low-status' if their parents obtained at most a vocational degree. Further control variables capture the family's financial situation (e.g., whether the child is considered disadvantaged or excessively disadvantaged by law (by eligibility for child protection support and, in the latter case, also having parents who completed at most Grade 8 (Act LXXIX of 1993 on Public Education (121(1)), whether the child considers their family to be poor, is eligible for free meals and/or free books at school, and has their own desk at home), the composition of the household (e.g., members of the household, whether the child lives with a biological father and mother *versus* a stepfather and stepmother), goods at home (internet, books), and the characteristics of the municipality (region and type of settlement).

The Hungarian Life Course Survey includes very rich data on early childhood events, starting from the birth of the child, including information on family shocks at various ages (such as income shocks and separation from parents), chronic conditions (such as ear infections or asthma), and developmental markers (such as the age of starting to speak or being diagnosed with ADHD at ages 4–5).

5 Identification

In this section, I present the identification of the Local Average Treatment Effect (LATE) of starting school a year older (1) due to academic redshirting using the January 1 cutoff, and (2) due to complying with the school enrollment cutoff of June 1. The endogenous variable is an indicator of starting school at age 7, as opposed to age 6. In my identification strategy for (1), I use being born after January 1 as an Instrumental Variable (IV) for starting school at age 7, and compare outcomes of children born on either side of January 1 in a 3-month window. I control for a linear trend in birth month around the cutoff, allowing for it to have different slopes on either side. My IV strategy is essentially a fuzzy Regression Discontinuity (RD) design, with month of birth as the running variable.

Below, after the estimation equations and the IV validity requirements, I discuss the compliers around the cutoffs and the formula for estimating their average characteristics (Almond and Doyle, 2011), and close with the interpretation of my LATE estimates in light of the existing SSA literature.

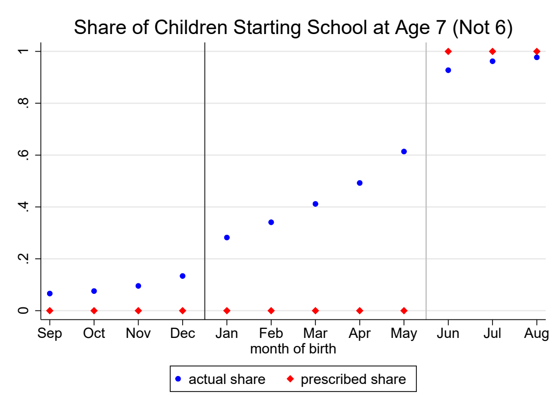
5.1 The Identification Challenge and the Intuition Behind Identification

Identification of the effect of starting school at age 7 (instead of age 6) is challenging due to the fact that, in the Hungarian educational system, as in many others, the actual age at which children start school is determined not only by an exogenous rule based on date of birth, but also by children's abilities at their entry-relevant age and/or their parents' preferences—both of which are unobserved in my data. One option to account for parental preferences is to use within-family variation across siblings, as in Dhuey et al. (2019); unfortunately, family identifiers are also unavailable in my data.

The core idea of my identification strategy is shown in Figures 1–2 for 10^{th}-graders in 2008–2017.

Figure 1 shows the raw first-stage relationship between birth month and the share of children starting school at age 7 (not 6), where cutoffs of January 1 and June 1 are marked with solid lines. The share of Grade 10 children who start school at age 7 steadily increases over birth months September–August, with clear jumps at the cutoffs. At the January 1 cutoff, the share of redshirted children sharply increases by 14.8 percentage points (from 13.4 to 28.2), and at the June 1 cutoff, the share of children starting school at age 7 sharply increases by 31.3 percentage points (from 61.4 to 92.7). These jumps at the January 1 and June 1 cutoffs are used to identify the impact of starting school at age 7 due to academic redshirting and due to the school enrollment cutoff date, respectively.

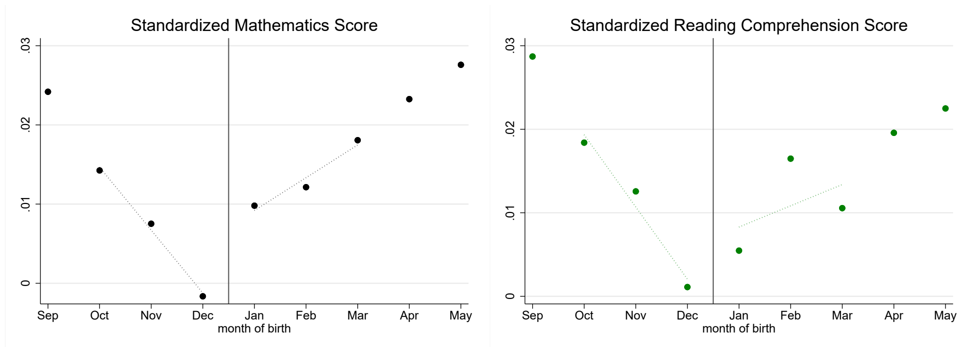
Figure 1: **Share of Children Starting School at Age 7, by Month of Birth, in Hungary**



Notes: These figures show the actual and prescribed shares of children starting school at age 7, by month of birth.
Source of data: Hungarian National Assessment of Basic Competences (Grade 10), 2008-2017.

Two panels in Figure 2 show the raw reduced-form relationship between month of birth and two selected student outcomes in Grade 10 (mathematics and reading comprehension test scores); they show jumps in test scores at the January 1 cutoff, and foreshadow a significant LATE estimate of starting school a year older (at age 7) due to redshirting—which, intuitively, is the reduced-form estimate rescaled by the first-stage estimate (controlling for a trend on the two sides of the cutoff).

Figure 2: **Average Test Scores, by Month of Birth Around January 1, Administrative Data – Grade 10**



Notes: These figures show the average test scores, by month of birth, for all, around the cutoff date of January 1.
Source of data: Hungarian National Assessment of Basic Competences (Grade 10), 2008-2017.

5.2 Fuzzy RD Design and the Two-Stage Equations

To identify the LATE of starting school at age 7 due to redshirting and due to the school enrollment cutoff, I instrument with births after January 1 and June 1, respectively. This IV/Fuzzy RD design, under specific identification requirements, captures the LATE on the *compliers*, whose propensity to start school at age 7 changes when hypothetically moving from the left of the cutoff to the right.

As day of birth is not available in my data, the bins on either side of the cutoffs contain children of the same birth month. I use a 3-month window, leading to 3-3 bins on either side of the cutoffs, by using data on children born in October–March, and in March–August. I control for month of birth linearly, and I allow the slope to vary on either side of the cutoffs. Note that this linear estimator will under-(over-)estimate effects based on a true convex (concave) first-stage relationship from the left and over-(under-)estimate effects based on a true convex (concave) reduced-form from the left.

The Two-stage Least Squares (2SLS) consists of the following first- and second-stage equations:

$$\begin{aligned} \mathbb{1}\{\text{start school at 7}\}_i = \; & \beta_0^d + \beta_1^d \mathbb{1}\{\text{birth-month}_i \geq x_d\} + \beta_2^d \text{birth-month}_i + \\ & \beta_3^d \mathbb{1}\{\text{birth-month}_i \geq x_d\} \times \text{birth-month}_i + \beta_4^d C_i + \textstyle\sum_{t=1}^{T-1} \mu_{1t}^d F_{ti} + \eta_i, \end{aligned} \tag{1}$$

$$\begin{aligned} Y_i = \; & \gamma_0^d + \gamma_1^d \mathbb{1}\{\text{start school at 7}\}_i + \gamma_2^d \text{birth-month}_i + \\ & \gamma_3^d \mathbb{1}\{\text{birth-month}_i \geq x_d\} \times \text{birth-month}_i + \gamma_4^d C_i + \textstyle\sum_{t=1}^{T-1} \omega_{1t}^d F_{ti} + \upsilon_i, \end{aligned} \tag{2}$$

where Y_i is the outcome variable from the set of {test scores, grade repetition, secondary school track choice, aspirations, obtaining a high school degree, mental well-being}; $\mathbb{1}\{\text{start school at 7}\}_i$ is an indicator variable denoting whether child i starts primary school at age 7 (instead of age 6); birth-month$_i$ is month of birth (linear trend, re-centered at x_d), where the discontinuity cutoff date, x_d, is either January 1 or June 1, and $\mathbb{1}\{\text{birth-month}_i \geq x_d\}$ is the discontinuity dummy; C_i denotes the vector of control variables, and F_t a full set of birth cohort dummies (defined to start in September and end in August, and t denotes years). Only the discontinuity dummy $\mathbb{1}\{\text{birth-month}_i \geq x_d\}$ is used as an Instrumental Variable (IV), and is excluded from the second-stage equation (2). The coefficients of interest are β_1^d and γ_1^d. I estimate (1)–(2) separately around the January 1 and June 1 cutoff dates, hence the d superscript on all the coefficients. Finally, η_i and υ_i are the unobserved error terms.

5.3 Discussion of IV Validity

It is implausible that the potential outcomes, and thus the effect of starting school a year older, are homogeneous in the population. Therefore, following Angrist and Pischke (2008), the requirements for the instrument $\mathbb{1}\{\text{birth-month}_i \geq x_d\}$ to be valid in a heterogeneous framework are that (i) the instrument is related to the endogenous variable (*relevance, or the existence of a strong first-stage relationship*); (ii) the instrument is as good as randomly assigned: it is independent of the vector of potential outcomes and potential treatment assignments (*independence*); (iii) the instrument has no direct relationship to student achievement, only an indirect relationship through the decision made about primary school-starting age (*exclusion restriction*). In turn, I discuss each of these requirements.

The *first-stage* requires birth month to be systematically and strongly related to school-starting age: Children who were born on or just after January 1 and June 1 should have a higher probability of starting primary school at age 7 (instead of 6), than children born just before these cutoff dates, *ceteris paribus*. This requirement is expected to hold, because—as discussed in Section 3—the January 1 and June 1 cutoffs greatly influence children's educational paths in Hungary. First, the local Developmental Advisory Board's school-readiness evaluation is not needed for redshirting a child who was born on January 1, but a school-readiness evaluation is required for a potentially redshirted child who was born on December 31. Second, a child born on June 1 *versus* May 31 is prescribed to enter primary school at age 7 instead of age 6. Figure 1 foreshadows—and Appendix B.1 formally tests—the very strong first-stage relationship: in the descriptive Figure 1, at the January 1 cutoff, the share of redshirted children increases by 14 percentage points, and at June 1, it increases by 31 percentage points.

A sufficient condition for the *independence* assumption to hold is that birth month around the two cutoff points is effectively random. This requirement could fail, among others, in the following two notable cases. First, if children born on one side of the cutoff dates have above-average innate abilities than children born on the other side (i.e., if innate ability is not uniformly distributed across the birth months near the cutoffs). Second, it fails if parents with particular preferences tend to have June babies rather than May babies (i.e., tend to conceive their baby in September rather than August), and thus their child is likely to be among the older students in class. On the distribution of abilities and parental preferences, existing evidence is inconclusive;[4] but, given that I compare children born

[4] Dickert-Conlin and Elder (2010) tests whether parents systematically plan childbirth to capture the option value of sending their children to school at a younger age, thus avoiding an additional year of childcare costs, and they find no evidence of the option value influencing timing of birth. Also, using exact birthdates, McEwan and Shapiro (2008) and, despite a somewhat different context, McCrary and Royer (2011), also find no evidence of birth frequencies or students' observed socio-economic characteristics varying sharply around enrollment cutoff dates, suggesting that unobserved characteristics also vary smoothly around these dates. There also exists evidence suggesting that *quarter* of birth may be related to maternal and child characteristics; e.g., (i) Bound and Jaeger (2001) reviews potential factors associated with month of birth, such as health or personality traits, and find schizophrenia, intellectual disability, autism, and

in two consecutive quarters, with one of the cutoffs being in the winter and the other in the summer, differences between summer- and winter-born children, suggested by Bound and Jaeger (2001) and Buckles and Hungerman (2013), are not a concern. In Appendix B.2, I still test the relationship between being born after the cutoffs and observable characteristics, and check for bunching.

Reassuring evidence supporting a smooth distribution of births around the January 1 cutoff can be seen in Appendix B.2. Table B3 shows no bunching on either side of the cutoff, and Table B4 shows the estimated coefficient on the discontinuity dummy $\mathbb{1}\{\text{birth-month} \geq x_d\}$ from model (1) on gender, parental education, family background variables listed in Section 4.2, and early childhood shocks listed in Table 5. None of the coefficient estimates ($\hat{\beta}_1$) differ from 0, supporting the *independence* identification assumption and IV validity. That is, neither the fraction of boys, nor the share of children with low parental education or any other background characteristics, or with various developmental obstacles in early childhood, are discontinuously changing at the January 1 cutoff.

Regarding the *exclusion restriction*, it fails if children born after the appropriate cutoffs have systematically different outcomes for reasons other than an increased likelihood of starting school at age 7. A potential threat is that older students, all else equal (including age at school entry), tend to have higher test scores. As an example, consider the cutoff of January 1 and two otherwise identical children: Dora born in December and Julie born in January (of the next year). Suppose that both children are redshirted. Then, even though Julie had a higher *ex-ante* likelihood of being redshirted, Dora is 1 month older than Julie at the time of the test. Thus, children born after January 1, such as Julie, may have different test scores for at least two reasons: a higher *ex-ante* likelihood of spending an extra year in childcare, but also being younger at the time of the test (*ceteris paribus*).

The above concern can be eliminated by comparing two children born just before or just after the cutoff, by shrinking the window as much as possible, and selecting two otherwise identical children who were born just one day apart. Such an approach is possible when the exact day of birth is known, information that is unavailable in my data. At the same time, it is necessary to control for month of birth, to account for the steadily increasing share of children starting school at age 7, across the birth months (as in Figure 1), and to identify the impact *exclusively from the jump* in the share of redshirted children at the cutoffs. Thus, within a birth cohort, I include children born in a 3-month window around the cutoffs, and control for trends in birth month linearly, allowing for different slopes on each side of the cutoffs, but in Appendix C.2, I check robustness to a quadratic trend.

Exclusion would also be violated if there are other policies that use the same birthday cutoff, generating differential human capital investments on either side. As mentioned in Section 3, the cutoffs in my setting affect not only SSA but also childcare-starting age and thus years spent in childcare, although to a much smaller extent. Thus, Appendix C.4 checks to what extent my main redshirting estimates are robust to account for the potentially endogenous years spent in childcare.

It may also happen that, because redshirted children were considered to be not mature enough to start school on time, this judgment affects the behaviors of teachers, parents, and children; e.g., school administrators, teachers, and parents may try to compensate redshirted children by providing

manic depression to vary across quarter of birth; (ii) Buckles and Hungerman (2013) documents that women giving birth in the winter months tend to be younger, less educated, and less likely to be married. Huang et al. (2020) finds that more educated Chinese mothers tend to move their delivery date forward to have their children start school younger.

additional resources like remedial tutoring or help with homework. Appendix Table B5 checks for such remediation and finds no evidence that teachers and parents compensate redshirted children by providing additional resources, like remedial tutoring or help with homework. Thus, the LATE estimates presented below represent effects of starting school at age 7 (either due to redshirting or due to the school enrollment cutoff), as opposed to effects of different treatments.

I make four final remarks on identification and inference: (i) Given that it is important to control for trends in birth month as discussed below, a 1-month window is insufficient for identification. (ii) Appendix C.2 checks the extent to which my main results for the LATE of starting school a year older due to redshirting are robust to (a) a 1-month window around the January 1 cutoff, without a trend; (b) a 2-month window with a linear trend; and (c) a 3-month window with a quadratic trend around the cutoff. (iii) I do not use September 1 for identification, because on the left of the cutoff prescribed age at school start is 7, and redshirting is not possible; also, a child born on September 1 would need to be tested to start school at age 7, but a child born on August 31 would need to be tested to start school at age 6, thus incentives for testing are not monotone around this cutoff. (iv) Clustering of standard errors (SEs) at the birthdate level would lead to an insufficient number of clusters (6 birth-months). To avoid a downward bias in the SE estimates due to a small number of clusters, for good coverage, and because I expect the regressors and errors to be correlated across students within schools, I cluster SEs at the school level, based on Cameron and Miller (2015). Appendix C.3 demonstrates that clustering at the school level is the most conservative relative to clustering at birth month and cohort level, and accounting for the discrete running variable (Kolesar and Rothe, 2018).

5.4 Monotonicity, LATE, and Compliers with Their Average Characteristics

For the IV estimator to produce the LATE, the *monotonicity* requirement also has to hold. Monotonicity requires that, while the instrument may not influence starting school a year older for some children, all of those who are influenced must be influenced in the same direction. Around the January 1 cutoff, monotonicity requires the following to hold for all children: If a child born before January 1 is redshirted (in the presence of the administrative barrier to redshirting), then they would definitely be redshirted if they were born even a day later, on or after January 1 (in absence of the administrative barrier). In other words, it cannot happen that a child is not redshirted if born on January 1 (in the absence of the barrier, when redshirting is costless), but the same child would be redshirted if born on January 1 (in the absence of the barrier, when redshirting is costless). Since redshirting under the administrative barrier is more costly, this assumption is a non-controversial one.[5]

Given *relevance, independence, exclusion,* and *monotonicity,* the 2SLS estimand has the interpretation of the (local average) effect of starting school at age 7 (instead of 6), for the *compliers*, whose treatment status is influenced by the change in the instrument's value around the discontinuity cutoffs.

[5]Around the June 1 cutoff, monotonicity requires, for all children, that if a child born before June 1 starts school at age 7 (equivalent to being redshirted), they would definitely start school at age 7 if born on or after June 1 (as required). Or, it cannot happen that a child born before June 1 delays school entry from age 6 to 7, but is brought forward from 7 to 6 if born after June 1. Aliprantis (2012) and Barua and Lang (2016) argue that, due to redshirting parents, monotonicity around the enrollment cutoff date could be violated. Given that children born after June 1 in the Hungarian setting are required to start school at age 7 and cannot be delayed further but only a tiny fraction of them start school at age 6 (around 3 percent, according to Figure 1), these concerns are empirically less relevant here.

Around the January 1 cutoff, the compliers are *non-school-ready* children who are deterred from redshirting by the administrative barrier. These children would be redshirted if the parents were only required to discuss the issue of redshirting with the childcare teachers, but would not be redshirted if the parents had to ask for the Developmental Advisory Board's evaluation. For such parents, it is too costly to search for, travel to, and let the child take the school-readiness examination, either since time costs are too high for them, or such parents are less informed and/or distrust the educational system. (There may be also a group of complier children, whose parents *think* they are not school-ready and who would be redshirted if only the childcare teachers' approval were needed, but who would not be advised to be redshirted by developmental psychologists in the school-readiness evaluation.)

Around the school enrollment cutoff of June 1, the compliers are *school-ready* children who enter primary school as prescribed by the regulation: at age 6 if born before June 1 (i.e., not redshirted), and at age 7 if born on or after June 1. That is, complier children around June 1 who start school a year older at age 7 do so solely because they were born after the enrollment cutoff and, had they been born before, would not have been redshirted and started school at a relatively younger age of 6. (There might also be parents who never want to redshirt, even if their child were born before June 1 (say, in May) and was not school-ready. But, given that in Hungary an overwhelming fraction (60 percent) of May-borns are redshirted (Figure 1), the share of such parents must be negligible.)

While it is impossible to identify the compliers in the data set—as they, and every other child, are observed only in the actual state of the world, and not in the counterfactual state—it is possible to assess their average characteristics. Following Almond and Doyle (2011), let us first define two independent binary variables, the instrument Z_j and the endogenous variable D^j, as follows:

$$Z_d = \left\{ \begin{array}{ll} 1 & \text{if born on/after the cutoff day} \\ 0 & \text{if born before the cutoff day} \end{array} \right\}; \; D^d = \left\{ \begin{array}{ll} 1 & \text{if start school at the age of 7} \\ 0 & \text{if start school at the age of 6} \end{array} \right\};$$

with d=1,2 corresponding to the January 1 and June 1 cutoffs, respectively. In addition, let us denote $D^j_{Z_d}$ as the value that D^d would take if Z_d were either 1 or 0. Then, compliers are those with $D^d_1 = 1$ and $D^d_0 = 0$. Let us denote their share as π_{C_d}. Then, their observable characteristics can be written as

$$E\left(X|D^d_1 = 1, D^d_0 = 0\right) = \frac{\pi_{C_d} + \pi_{A_d}}{\pi_{C_d}}\left[E\left(X|D^d = 1, Z_d = 1\right) - \frac{\pi_{A_d}}{\pi_{C_d} + \pi_{A_d}} E\left(X|D^d = 1, Z_d = 0\right)\right], \tag{3}$$

where π_{A_d} is the share of always-takers (for whom $D^d_1 = 1$ and $D^d_0 = 1$) and $\pi_{C_d} = 1 - \pi_{A_d} - \pi_{N_d}$, where π_{N_d} is the share of never-takers (for whom $D^d_1 = 0$ and $D^d_0 = 0$). Defiers (for whom $D^d_1 = 0$ and $D^d_0 = 1$) are assumed away by the monotonicity assumption. Using independence between Z_d and D^d, the shares π_{A_d} (π_{N_d}) can be estimated by the empirical counterpart of $Prob(D^d_0 = 1)$ ($Prob(D^d_1 = 0)$).

5.5 Relationship to Existing Literature and Interpretation of LATE Estimates

This paper's identification strategy is most similar to the one used in Puhani and Weber (2007) and Altwicker-Hamori and Kollo (2012), which use predicted school entry age as an IV for the endogenous primary SSA, which is a function of the child's birth month, the primary school start month, and the enrollment cutoff month. Note that Puhani and Weber (2007)'s IV is of the same form as mine around the cutoff date of June 1 (but they do not control for a linear trend in month of birth). I control for the trend in birth month, allowing for different slopes on either side of the discontinuity cutoffs.

After establishing that this paper measures the causal impact of academic redshirting, for *non-school-ready* children, it is important to be transparent about what this paper does *not* measure. For this, I note that as I look at tests taken at a given stage of students' school career (at a given grade), the following collinearity holds (in months): $SSA + \textit{months since primary school entry} = \textit{age-at-the-test}$.

First, I do not attempt to disentangle the effect of SSA from the effect of age-at-the-test. The Hungarian institutional setup is not suitable for this, as children are tested on the same date in a given grade. Black et al. (2011) disentangles these by using scores from Norwegian IQ tests taken outside of school, exploiting the variation in the mapping between birthdate and year-of-the-test. I, however, look at the effect of SSA on grade repetition (retention) in further grades, as an outcome.

Second, I do not attempt to disentangle age-at-the-test effects from the impacts of time spent in school. There are institutional setups in other countries that provide a suitable natural experiment to compare children in the same grade with the same age at the time of the test, but who have spent different amounts of time in pre-school. For example, Dustmann and Cornelissen (2019) exploits the variation in age-at-school-entry and the effective length of the first year in school in the UK, and Carlsson et al. (2015) exploits conditionally random variation in the assigned test date for IQ tests of 18-year-old Swedish men in preparation for military service, so that both age-at-the-test and number of days spent in school vary randomly after holding date of birth, parish, and graduation date fixed.[6]

Third, I do not compare a redshirted child's test scores to those of a non-redshirted child of the same age-at-the-test, because among non-grade-repeaters, there is no variation in SSA by age-at-the-test. The small variation in SSA by age-at-the-test that exists comes solely from grade-repeaters, and is thus endogenous. I do not control for grade repetition, but I look at it as an outcome variable itself. Hence, my main redshirting estimate picks up the *combined* effect of starting school later—with one more year to mature before school entry—and being older at the time of the test.

In sum, I measure the effect of starting school at age 7 (instead of 6), which may encompass both absolute (maturity) and relative age effects in class, and more years in childcare. But, I will show that my redshirting estimates are robust to accounting for time in childcare (Appendix C.4), are driven by absolute and not relative age effects (Appendix C.5), and there is no indication that teachers and parents compensate redshirted children with additional resources (e.g., tutoring; Appendix Table B5).

6 Results

6.1 LATE Estimates of Starting School at Age 7 due to Academic Redshirting

6.1.1 Estimates for All Students

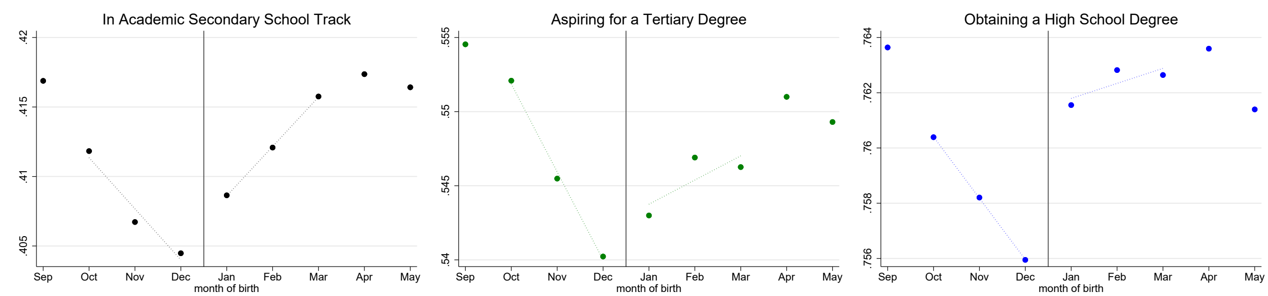
First-Stage Results. The left panel of Table B1 shows the first-stage results on starting school a year older due to redshirting, using the administrative test score data. Grade 10 children born on or after January 1 are 11.2 percentage points more likely to start school at age 7, as opposed to starting school

[6] The effect of longer kindergarten, without controlling for age-at-the-test, is measured by DeCicca and Smith (2013).

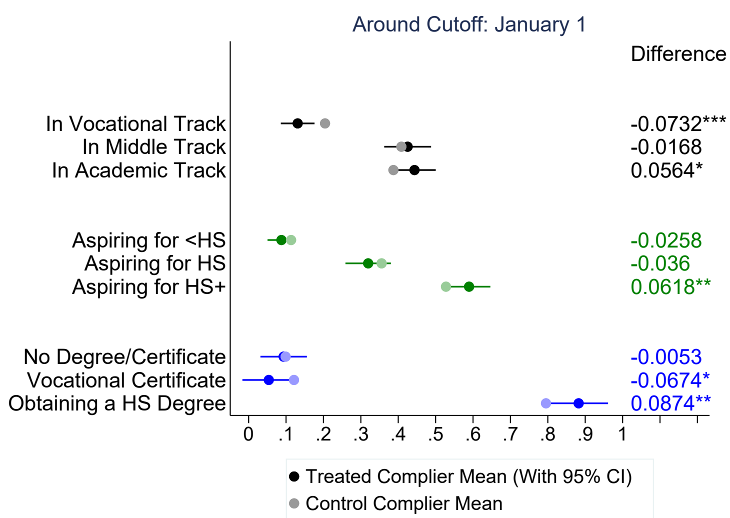
at age 6, than children born before January 1 (given the baseline pof 9.3 percent, this corresponds to a large, 120 percent, effect). The first-stage relationship is reassuringly very strong, with no concerns of passing tests of weak instruments (Stock et al., 2002; Olea and Pflueger, 2013; Lee et al., 2022).

Figure 3: **Share of Students for Various Binary Student Outcomes by Month of Birth, and Control and Treated Complier Means Around January 1 Cutoff, Administrative Data – Grade 10**

(a) (b) (c)



(d) Treated&Control Complier Means: Tracks, Aspirations, Graduation



Notes: [1] The upper figures show the raw reduced-form relationship between month of birth and various student outcomes, for all, by month of birth, around the cutoff date of January 1 that is relevant for academic redshirting in Hungary.
[2] The bottom figure shows the control complier means (estimated using eq. (A2) in Abdulkadiroglu et al. (2018)), and the treated complier means with 95 percent CI. The treated-control difference corresponds to the LATE estimate of starting school at age 7 due to redshirting. The treated complier mean is the sum of the control complier mean and the coefficient estimate on $\mathbb{1}\{\text{start school at 7}\}$, for a given outcome&subgroup. The underlying coefficients, SE estimates, and means are in Tables E4-E5.
Source of data: Hungarian National Assessment of Basic Competences (Grade 10), 2008-2017.

Second-Stage Results on Test Scores and Grade Repetition. In Table 2, the first column in the left portion of *Panel A* reveals that children who start school at age 7 due to academic redshirting have, on average, a significantly higher mathematics test score by 0.12 SDs on Grade 10 testings, compared to on-time children. The point estimate for reading test score is 0.09 SDs and on the propensity of repeating a grade is −3.24 percentage points, and they are not significantly different from zero.

Second-Stage Results on Secondary School Track. Panel (a) in Figure 3 shows the raw reduced-form relationship between month of birth and the share of children in the academic track, around the January 1 cutoff. The crucial outcome of secondary school track is measured with a binary variable indicating whether the student attends a given track at Grade 10—where the lowest track grants only a vocational degree, without the option of tertiary-level education; the middle track grants both a vocational and high school degree; the academic track grants a high school degree.

The bottom panel (d) in Figure 3 shows the difference between the treated and control complier means around the January 1 cutoff—corresponding to the LATE estimates for starting school a year older due to academic redshirting—on secondary school track. For all children, starting school at age 7 due to redshirting decreases the propensity for the vocational track by 7.3 percentage points (-36 percent), and increases the propensity for the academic track by 5.6 percentage points (15 percent).

Second-Stage Results on Highest Educational Aspirations. Panel (b) in Figure 3 shows the raw reduced-form relationship between month of birth and the share of children aspiring for a tertiary degree. The bottom panel (d) shows the difference between the treated and control complier means around the January 1 cutoff—corresponding to the LATE estimates for starting school a year older due to academic redshirting—on aspirations for highest level of educational attainment. For all children, the LATE estimates of starting school at age 7 due to redshirting are significantly positive for aspiring for a tertiary degree (6.2 percentage points, or 12 percent of the baseline).

Second-Stage Results on Graduation from Secondary School. Panel (c) in Figure 3 shows the raw reduced-form relationship between month of birth and graduation from high school with a high school degree. Panel (d) shows the difference between the treated and control complier means around the January 1 cutoff—corresponding to the LATE estimates of starting school a year older due to academic redshirting—on graduation from high school. For all children, the LATE estimate due to redshirting is -6.74 percentage points (-56 percent) on obtaining a vocational degree and 8.74 percentage points (11 percent) on obtaining a high school degree, significant at the 10 and 5 percent levels, respectively.

6.1.2 Estimates by Gender

First-Stage Results. The left portion of *Panel B* in Table B1 shows the first-stage results on starting school a year older due to redshirting, using the administrative test score data. The first-stage relationship is significantly stronger for boys in absolute terms (with p-value $p = 0.000$),[7] but not in relative terms: A boy (girl) in Grade 10 is 13.1 (9.4) percentage points more likely to be redshirted if born on or after January 1 than if born just before. However, given boys' higher baseline propensity to be redshirted relative to girls of around 4 percentage points (among those born in the fall), the effect for boys is around 115 percent, while for girls, it is around 127 percent.

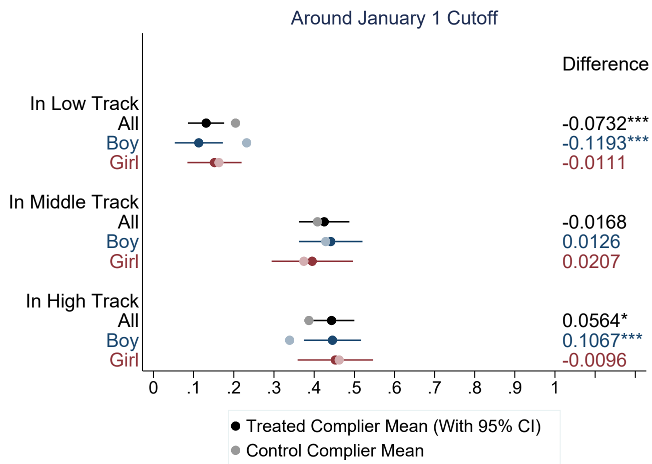
Second-Stage Results on Test Scores and Grade Repetition. According to *Panel B* of Table 2, the positive LATE estimates due to redshirting on student achievement in Grade 10 are only present for boys. Redshirted boys, on average, achieve a significantly higher score in mathematics and reading by 0.2 and 0.25 SDs, respectively, on Grade 10 standardized tests. Furthermore, they are 9.4 percentage points less likely than on-time children to repeat a grade by Grade 10 (which is a large effect size, compared to the control complier share of 11 percent). In contrast, the LATE estimates for girls are

[7]Note that given the number of potential ways to cut the data and the numerous outcome variables, it is important to test the differences between boys' and girls' estimates, which I do and report the p-values.

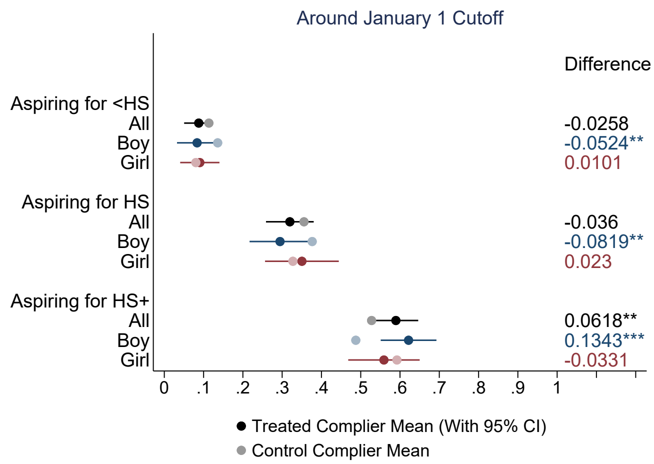
insignificant across all outcomes. Formally testing the equality of boys' and girls' coefficient estimates gives $p = 0.019$, $p = 0.001$, and $p = 0.000$ for mathematics score, reading score, and grade repetition.

Figure 4: **Control and Treated Complier Means Around January 1 Cutoff, By Gender**

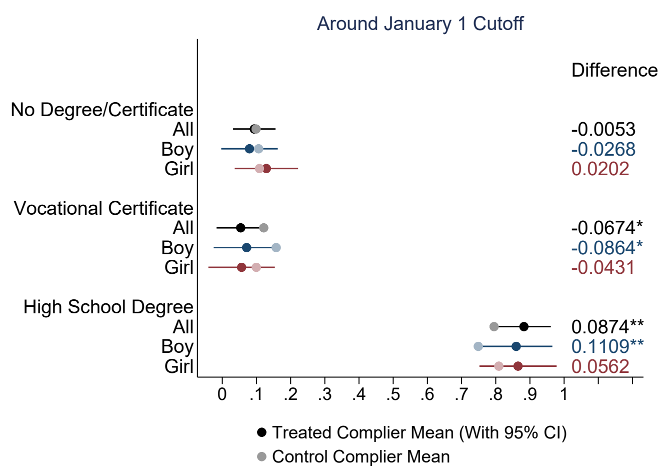
(a) Secondary School Track Choice



(b) Aspirations for Highest Educational Attainment



(c) High School Graduation / Vocational Certificate



Notes: These figures show the control complier means (estimated using eq. (A2) in Abdulkadiroglu et al. (2018)), and the treated complier means with 95 percent CI. The treated-control difference corresponds to the LATE estimate of starting school at age 7 due to redshirting. The treated complier mean is the sum of the control complier mean and the coefficient estimate on $\mathbb{1}\{\text{start school at 7}\}$, for a given outcome&subgroup. The underlying coefficients, SE estimates, and means are in Tables E4-E5.
Source of data: Hungarian National Assessment of Basic Competences (Grade 10), 2008-2017.

Second-Stage Results on High School Track Choice. Panel (a) of Figure 4 shows that the positive LATE estimates of starting school a year older due to redshirting on high school track choice are only present for boys: it is 11 percentage points (31 percent) on the likelihood of attending the academic track, and it is -12 percentage points (-51 percent) on the likelihood of attending the vocational track; there are no detectable effects for girls. Formally testing the equality of boys' and girls' coefficient estimates gives $p = 0.001$ and $p = 0.005$ for the outcomes of attending the vocational and academic tracks, respectively. Given the 12.4 percentage point difference between boys' and girls' control complier share in the academic track, the estimate for boys implies that starting school a year older due to redshirting shrinks the baseline gender gap in attending the academic track by 80 percent.

Second-Stage Results on Educational Aspirations. Panel (b) of Figure 4 shows that, again, the positive LATE estimates of starting school a year older due to redshirting on aspirations are only present for boys; for boys, it is 5.2 percentage points (43 percent) on aspiring for less than a high school degree, -8.2 percentage points (-22 percent) on aspiring for only a high school degree, and 13.4 percentage points (26 percent) on aspiring for a tertiary degree. Meanwhile, there is no detectable effect for girls for any of the outcomes. Formally testing the equality of boys' and girls' coefficient estimates gives $p = 0.01$, $p = 0.002$, and $p = 0.009$ for the outcomes of aspiring for less than a high school degree, a high school degree, and a tertiary degree, respectively. Given that among the control compliers, boys' baseline shares of aspirations for $< HS$, HS, $HS+$ exceed those of girls by 6, 7, and 13 percentage points, respectively, the estimates imply that starting school a year older due to redshirting could fully eliminate the gender gap in aspirations for the highest educational attainment.

Second-Stage Results on High School Graduation. Panel (c) of Figure 4 shows that for boys, starting school a year older due to redshirting leads to a 8.64 percentage points (55 percent) lower likelihood of obtaining a vocational degree, and a 11.1 percentage points (15 percent) higher chance of obtaining a high school degree, while there is no significant effect for girls. Starting school a year older due to redshirting closes the baseline gender gap by up to 60 percent in obtaining a high school degree, given the 8.5 percentage point difference between boys' and girls' control complier shares, and the 5.47 difference in the estimated effect. Also, it closes 75 percent of the baseline gender gap in obtaining a vocational degree, given the 5.8 percentage point difference between boys' and girls' control complier shares, and the 4.34 difference in the estimated effect. There is a negative effect on obtaining a vocational degree for boys, given redshirted boys are less likely to end up in the vocational track to begin with (and there is no significant effect on dropping out, i.e., finishing without a degree).

In sum, starting school a year later at age 7 due to redshirting leads to higher test scores, more academic high school track enrollment, and higher aspirations, which translate to higher secondary school graduation rates. However, these positive effects are significantly present only for boys.

6.1.3 Placebo and Robustness Checks

Appendix Table C1 shows the first-stage and reduced-form estimates on the discontinuity dummy $\mathbb{1}\{\text{birth-month} \geq x_d\}$, using a 1-month or 2-month window, whenever possible, around placebo cutoffs (x_d : October 1,...,April 1). There are two main reassuring conclusions: (i) the biggest jump in the share of redshirted children is at the January 1 cutoff (an increase of 16.43 percentage points for boys); and (ii) the reduced-form estimates are either insignificant or have a different sign at the placebo cutoffs than the reduced-form estimate at the January 1 cutoff.

Then, Appendix Tables C2 and C3 show that the point estimates and their precision do not change meaningfully when switching to a 2-month window (from a 3-month window) around the January 1 cutoff date, and when controlling for a quadratic trend in birth month with the 3-month window (instead of a linear trend). When restricting to a 1-month window, the point estimates are somewhat smaller, but the main conclusions still hold. Appendix Table C4 shows that my chosen level of clustering (at the school level) leads to larger, more conservative, standard errors and lower values of t-statistics than by clustering at the birth-month level or birth-month$\times$year level, or accounting for the fact that there are only 3 months of births on each side of the cutoffs (Kolesar and Rothe, 2018).

Appendix C.4 shows that my estimates for starting school a year older due to redshirting are robust to accounting for *years in childcare*, a decision variable of parents which, besides age at school start, might also change at the January 1 cutoff. Indeed, Table C5 shows that the share of children who start childcare at the age of 3 or later jumps by 19 percentage points at the January 1 cutoff, and the average time spent in childcare is 0.092 years *lower* among January-born children than among December-born ones; e.g., 28.7 percent of December-born children start childcare at the age of 2, but only 9.3 percent of January-born do so. The share of children who start childcare at age 4 or older jumps by 4.5 percentage points at the January 1 cutoff. Thus, the cutoff affects *years in childcare* to some extent. Since $SSA = \textit{years in childcare} + CSA$, in the robustness check in Appendix C.4 I jointly estimate the effects of the first two, using as an additional source of variation the fraction of 3-year-olds in childcare in the municipality when the child was 2 years old, stemming from the institutional background.[8] Standard tests support IV strength and exogeneity (Tables C6–C7). Then, holding years in childcare fixed, starting school at age 7 due to redshirting results in higher test scores in mathematics and reading, by 0.26 and 0.28 SDs on Grade 10 testings, respectively. It also results in 7.4 percentage points lower likelihood to repeat a grade by Grade 10, and a 11 (13) percentage points higher chance to attend the academic track (aspire for a tertiary degree). For girls, there are no such positive impacts (Table C7, except one estimate is significant at the 10 percent level).

Finally, Tables C8 and C9 in Appendix C.5 show that my main estimates are more conservative than when also accounting for relative age (RA) effects in class, through which higher age at school entry may manifest. My unique data, containing all students' birthdates and class identifiers in a cohort, allows me to construct a precise measure of each students' within-class RA rank, to separate absolute and within-class RA effects. By exploiting natural variation in birth month and the share of summer-born children in the class, I horse-race absolute and relative age effects behind starting school a year older due to redshirting. I find the positive redshirting effects on student achievement to be driven solely by absolute age effects (as RA effects are insignificant across the board).

6.2 LATE Estimates of Starting School at Age 7 due to the Enrollment Cutoff

For mathematics and reading test scores, the estimates are 0.14 and 0.18 SDs in Grade 10, respectively (see the right portion of Table 2). These estimates are consistent with existing literature, e.g., Puhani and Weber (2007), Altwicker-Hamori and Kollo (2012), McEwan and Shapiro (2008), and Elder and

[8] The order in which parents make decisions about their children's educational path is ambiguous. They may decide sequentially, first for CSA, and only then consider redshirting at the relevant SSA, or they may very well be forward-looking, and decide on CSA with the possibility of redshirting in mind. With the former, one would want to control for CSA, as in Table C7. With the latter, CSA should not be a control, as the shorter time spent in childcare is part of the mechanism through which redshirting impacts child outcomes. Given the ambiguity regarding the timing of parental decisions, I present the regression that accounts for *years spent in childcare* as a robustness check only.

Lubotsky (2009).[9] Also, children who start school at age 7 just because they were born after (and thus comply with) the enrollment cutoff of June 1 are less likely to fail a grade by 5.8 percentage points by Grade 10 (or by 51 percent), aspire for more education (for instance, they are 4 percentage points less likely to aspire for at most a high school degree, and are 8 percentage points more likely to aspire for a tertiary degree, as shown in the right portion of Table E4). They are also more likely to enroll in academic high school tracks (by 6 percentage points), but are less likely to enroll in vocational tracks (by 8 percentage points). Finally, they are 3.4 percentage points more likely to obtain a high school degree (Table E5). The significantly positive effects of higher SSA due to complying with the school enrollment cutoff date typically disappears when accounting for years spent in childcare (Table C7).

In sum, the LATE estimates of starting school a year older at age 7 due to redshirting (of Section 6.1) are, on average, very similar to that of the school enrollment cutoff (of Section 6.2). However, the key policy-relevant distinction lies in who drives the positive impacts. If starting school at age 7 is due to redshirting, the positive effects are only present for *boys* (and they are significantly larger than for girls), but, both boys and girls experience positive effects if it is due to the enrollment cutoff.

7 Mechanisms

7.1 Characteristics of Redshirted Children and the Compliers

In this subsection, I show new evidence on factors associated with being redshirted, and who the compliers are around the January 1 and June 1 cutoffs for whom the LATEs are estimated.

Figure 5 shows the shares of boys and girls born between October and March with different early childhood events and developmental obstacles, by whether they were redshirted. Panel (a) reveals important gender differences in non-redshirted boys' and girls' developmental trajectories. The gender gap in speaking ability already opens up at the age of 1 (boys are more likely to say their first words at age 1 or older, and tend to say their first full sentence even later), and persists to age 4–5 (boys are more likely to have problems with speaking and cognition and are more likely to be diagnosed with ADHD). Boys are more likely to suffer from chronic ear illnesses before turning age 3. Appendix Figure D1 shows that these gender differences are not significantly different for redshirted children.

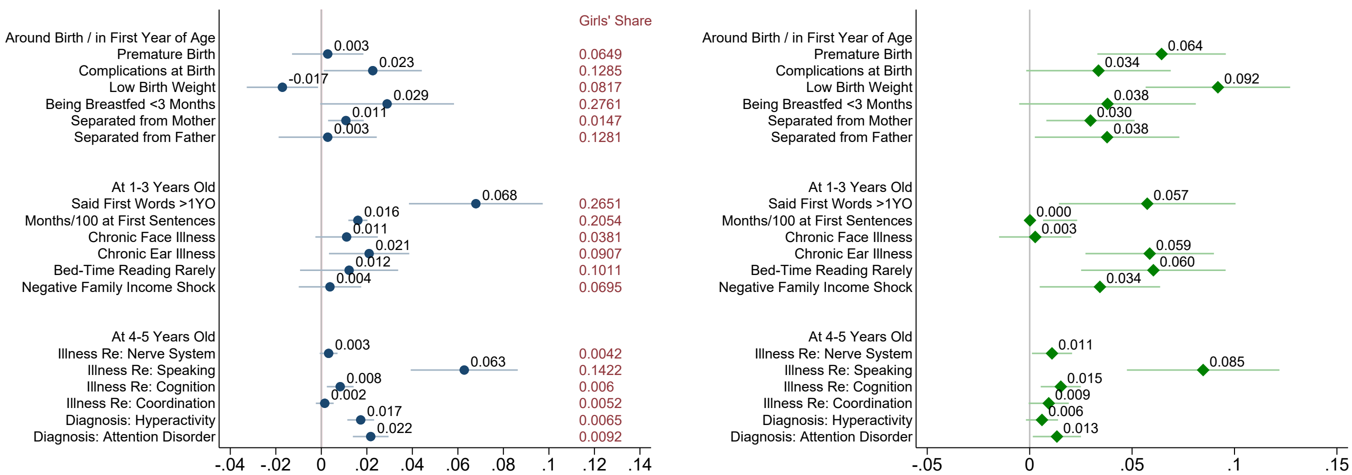
Panel (b) of Figure 5 reveals four key relationships between adverse events in early childhood and D: (i) Boys and girls are significantly more likely to be redshirted if they experienced a family shock (e.g., they were separated from their father before age 5, or the family had a negative income shock

[9] Puhani and Weber (2007) presents evidence that entering the German primary school system at age 7 instead of 6 increases Grade 6 reading test scores by 0.4 SDs. Altwicker-Hamori and Kollo (2012) shows that the effects are larger for disadvantaged children: among children whose mother finished at most primary school, the effect in Grade 4 is 0.8 SDs and in Grade 8 0.25–0.4 SDs (the corresponding effects for children whose mother received a tertiary degree are 0.3 and 0.2 SDs). McEwan and Shapiro (2008) finds that starting school one year later leads to more than a 0.3 SD increase test scores for Grades 4 and 8. Elder and Lubotsky (2009) finds evidence that entering kindergarten one year older leads to a 0.53 SD increase in reading test scores and a 0.83 SD increase in mathematics test scores during the fall semester of kindergarten. However, these effects fade away faster for disadvantaged children. Dhuey et al. (2019) finds that the impact of being one-month older in class (born in September *vs.* August) is around 0.2 SDs, irrespective of family background, birth weight, or school quality; moreover, there is a positive impact on college attainment and a negative impact on juvenile crime. Attar and Cohen-Zada (2018) finds that entering school a year older boosts Grade 5 test scores in Hebrew and mathematics by 0.34 and 0.19 SDs; by Grade 8, the effect on math scores almost doubles.

before then, or their parents rarely read to them). In contrast, (ii) girls are significantly more likely to be redshirted if they were separated from their mother before age 5. Furthermore, (iii) boys and girls are significantly more likely to be redshirted if they experienced a shock at birth (e.g., low birth weight, early-term delivery before week 36, or birth with complications), chronic illnesses in early childhood (e.g., sinusitis at ages 1-3), or said their first words only when they were 1 year old, or older. Finally, (iv) developmental problems at ages 4-5 (e.g., attention/cognition/verbal disorders, coordination problems) or diagnosis of ADHD are also significantly associated with the propensity of being redshirted at age 6 (significantly so for boys and attention disorders). The length of having been breastfed is negatively related to being redshirted, yet only with 10 percent significance level.

Figure 5: **Factors in Early Childhood Associated with Being 'Redshirted' and Gender – Survey Data**

(a) Coefficient Estimates for Variable 'Boy' (b) Coefficient Estimates for Variable 'Redshirted'



Notes: [1] This figure shows the estimated coefficients and their standard errors for a given outcome Y, regressed on *Boy*, $\mathbb{1}\{\text{start school at 7}\}$ (equivalent to 'Redshirted'), and their interaction 'Boy × Redshirted', without any control variables. In Panel (a), the coefficient estimate can be interpreted as the difference in a given outcome between non-redshirted boys and non-redshirted girls (where the non-redshirted girls' share is on the right). In Panel (b), the coefficient estimate can be interpreted as the difference in a given outcome between redshirted girls and non-redshirted girls. In Panel (c), the coefficient estimate can be interpreted as the boy-girl difference in a given outcome between redshirted and non-redshirted students.
[2] Standard errors are clustered at the school-level, *** p<0.01, ** p<0.05, * p<0.1.
[3] The sample includes children born between October and March (around the January 1 'redshirting' cutoff).
Source of data: Survey data – Hungarian Life Course Survey.

Table B3 shows that on both sides of the January 1 cutoff, the share of children whose parents have at most primary or vocational education is higher for redshirted than on-time children (24 *versus* 9 percent and 35 *versus* 29 percent, respectively, for fall-born children). Thus, redshirting is more prevalent among low-status students—but, this relationship between the propensity of being redshirted and socio-economic status vanishes if early childhood indicators are controlled for in Table 5.

Table 3 shows the average characteristics of the compliers. Around January 1, boys are over-represented among the compliers, but their other average characteristics are indistinguishable from the sample (except for number of siblings and whether they live in the capital). Around June 1, boys are under-, and those from better family backgrounds are over-represented, among the compliers.

Column (7) in *Panel B* of Table 3 shows the results of the direct test of whether compliers around the January 1 redshirting cutoff are indeed less school-ready than compliers around the June 1 school enrollment cutoff. Compliers around the January 1 cutoff are overrepresented precisely in the dimensions that have been found to be systematically related to redshirting and indicative of non-school-readiness. They are significantly more likely to have (i) been born with low birth weight, (ii) been separated from their mothers before their 1^{st} birthday, (iii) been older when they started to speak, (iv) suffered chronic ear illnesses in their first 3 years of life, (v) had nervous system/verbal/coordination problems and been diagnosed with an attention disorder at ages 4-5. The estimated share of compliers around January 1 is 32.5 percent, and the share of always-takers is 5.9 percent; around June 1, the share of compliers is 53.5 percent, and the share of always-takers is 38.5 percent.

Table E1 shows the average characteristics of the compliers by gender. Within the compliers around January 1, boys started to speak at an older age than girls, and they are more likely to have been diagnosed with ADHD at ages 4–5. Thus, among the compliers around January 1, boys are even more negatively selected than girls, and show more severe signs of non-school-readiness.

In sum, redshirted children are on average more likely to have suffered adverse developmental events in early childhood, and less school-ready at age 6, indicated by their lag in developmental markers at age 6. Also, compliers around the January 1 cutoff—relevant for academic redshirting—seem to have been indeed significantly less school-ready than compliers around the school enrollment cutoff of June 1, with complier boys seeming even less school-ready than complier girls.

7.2 Student Achievement in Primary School

The significantly positive impact of starting school at age 7 due to redshirting already appears for test scores in primary school. *Panel A* of Table E2 reveals that the estimated effects are 0.21 and 0.145 SDs, on Grade 6 and 8 mathematics testings, respectively. *Panel A* of Table E2 reveals that the estimated impacts on the Grade 6 and 8 reading scores are 0.2 and 0.13 SDs (*Panel C*).

Again, the impact is only significant for boys (*Panel B*). For example, the LATE estimates for boys are 0.3 and 0.14 SDs, on the Grade 6 and 8 math scores, respectively. In contrast, for girls only the estimate on the Grade 8 math score is significant at the 10 percent level. For reading, the estimates are 0.29 and 0.22 SDs, respectively, for boys, and all are small and insignificant for girls (*Panel D*).

Table E3 reveals that starting school at age 7 due to redshirting decreases the likelihood of repeating a grade by Grade 6 by 3 percentage points. The effect size is large compared to the baseline grade repeaters' share of 4.4 percent, even though there are no detectable effects by Grade 8. Yet, again, the impact on grade repetition is only significant for boys (-5.1 percentage points by Grade 6). For girls, all point estimates are much smaller (in absolute terms) and none of them are statistically significant. Testing the equality of boys' and girls' coefficient estimates for mathematics (reading) scores in Grades 6 and 8 gives $p = 0.01$ and $p = 0.52$ ($p = 0.052$ and $p = 0.002$), and for retention, $p = 0.02$ and $p = 0.2$.

In sum, my results suggest that starting school at age 7 due to redshirting helps boys perform better in secondary school, aspire for more education, and graduate from high school, *because* it helps them perform better already in primary school. Thus, it appears to improve both the student achievement trajectories and presumably how much students like to go to school, already earlier than high school.

7.3 Mental Health (Anxiety), Confidence, and Student Well-Being

Having shown that starting school a year older at age 7 due to redshirting benefits student achievement for boys only, I now turn to mental health outcomes to shed further light on the mechanisms.

Starting school at age 7 due to redshirting reduces boys' symptoms of anxiety. The left portion of *Panel A* in Table 4 reveals that redshirted boys have lower anxiety scores than on-time children by over 1 SD in Grade 8, and are 46 percentage points less likely to experience frequent symptoms of Generalized Anxiety Disorder (GAD). Again, none of the corresponding estimates for girls are significantly different from zero, and testing the equality of boys' and girls' coefficient estimates gives $p = 0.054$ and $p = 0.11$ for the anxiety score and the likelihood of experiencing frequent symptoms of GAD, respectively. Column (3) conforms these results, using administrative health data. Starting school a year older due to academic redshirting significantly decreases expenditures on prescriptions for psycholeptics—primarily used for treating anxiety—by 4 percent for boys, and this point estimate is significantly larger ($p = 0.042$) than the estimate for girls (which is insignificant).

Starting school at age 7 due to redshirting also improves boys' feeling of confidence and reduces boys' experiences of being bullied in class. The left portion of *Panel B* in Table 4 reveals that redshirted boys' propensity to feel unvalued and untalented decreases by 50 percentage points, and they are significantly less likely to be bullied in class for their appearance or poor academic performance by 40 and 33 percentage points, respectively. Girls' estimates are all insignificant, and the respective p-values for testing the equality of estimates between genders are $p = 0.07$, $p = 0.2$, and $p = 0.04$. These results are also in line with findings that boys generally benefit more from environments that improve children's socio-emotional skills (Walton and Cohen, 2011; Sisk et al., 2018).

The estimates on confidence, anxiety, and bullying are meaningful and large in the context of student achievement: Given that boys who lack confidence score, on average, around 0.4 SD lower on the standardized math and reading tests, the -0.5 point estimate on $\mathbb{1}\{\text{start school at 7}\}$ due to academic redshirting on the '*lack of confidence*' outcome corresponds to approximately 0.2 SD higher test scores through the confidence channel. Similarly, given that boys who are bullied score, on average, around 0.33 SD lower on the standardized math and reading tests, the -0.33 point estimate on $\mathbb{1}\{\text{start school at 7}\}$ due to redshirting corresponds to 0.1 SD higher test scores through the (no) bullying channel. Or, given that boys who feel often anxious score, on average, around 0.05 SD lower on the standardized math and reading tests, the -0.46 point estimate on $\mathbb{1}\{\text{start school at 7}\}$ due to redshirting corresponds to 0.023 SD higher test scores through the (no) anxiety channel.

These results suggest that mental health is an important mechanism behind why starting school at age 7 due to redshirting improves boys' educational achievements through secondary school, and increases their educational aspirations and actual attainment. For boys, starting school a year older due to redshirting leads to better mental health (lower anxiety), improves self-confidence (feeling valued and talented), and boosts overall well-being. Taking into account that boys already fall behind girls at ages 1–4 in their speaking, attention and impulse control abilities, and are less likely to be school-ready, redshirting also helps boys overcome non-school-readiness, and thus leads to a boost in boys' human capital and maturity. The aforementioned factors—mental well-being, self-confidence, maturity and conquered developmental obstacles—are all associated with better student achievement.

8 Conclusion

This paper provides the first causal estimates for starting school a year older due to academic redshirting, using Hungarian administrative test score and medical data, and survey data on mental health. Specifically, I estimate the effect of starting school a year older due to redshirting for non-school-ready children, exploiting an administrative barrier—a school-readiness evaluation—which is compulsory only for potentially redshirted fall-born children. Crucially, an extra year before school could help these students overcome developmental deficits, but they are deterred from redshirting by the barrier.

Understanding redshirting brings a new angle on school-readiness, and it is relevant for parents, teachers, and policy-makers alike. The Hungarian setting, besides providing a separate birth cutoff for identification (January 1), is unique in that childcare is free, redshirted children come from neither rich nor poor families, and a large share of children—e.g., 68 percent of May-born boys—are redshirted.

Starting school a year older due to academic redshirting increases students' mathematics testscores in high school, decreases (increases) their share in the vocational (academic) high school track, increases their propensity to aspire for a tertiary degree, and increases their likelihood to actually graduate from high school. But, starting school at age 7 due to redshirting is significantly beneficial only for boys: it sets boys—but not girls—up to be college-bound in high school tracks, closes the gender gap in aspirations, and narrows the high school completion gap by up to 60 percent; it also makes boys more confident, less anxious (even spending less on anxiety medications), and less prone to being bullied in class. I show new evidence for the negative selection among the redshirted compliers that can partially explain the differential effects by gender: the propensity of being redshirted is related to adverse events in early childhood, including experiencing developmental obstacles, and the compliers are, on average, less likely to be school-ready at the redshirting cutoff of January 1.

Overall, boys gain from a later school start, irrespective of whether it is due to redshirting barriers or school entry rules, but girls only gain if it is due to the latter (although, girls also do not lose from being redshirted). Given that children around the redshirting cutoff (January 1) are older and less school-ready than children around the school enrollment cutoff (June 1), it seems that young school-ready girls benefit from an extra year in their student achievement, but older and moderately non-school-ready girls do not. Yet, both young school-ready boys and older non-school-ready boys benefit from an extra year in terms of student achievement *and* mental health.

Future research may want to formally model parents' redshirting decision (as in, e.g., Ricks (2022) or Datar (2006b)), with an emphasis on their beliefs on their children's school-readiness and their returns to redshirting. One intriguing finding of this paper is that girls, on average, do not seem to benefit from starting school a year older due to redshirting, yet a significant proportion of them are still redshirted. Young school-ready girls' LATE estimates for starting school a year older due to complying with the school enrollment cutoff is significantly positive, suggesting that these girls do experience the"gift-of-age" gains. The two LATE estimates for girls, at first glance, suggest that either some *actually school-ready* girls are redshirted despite no need for it, or that parents may be unaware of or underestimate the potentially negative effects of holding back an older and, to some extent non-school-ready girl, opening up interesting avenues for further research.

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Tables

Table 2: ***Effect of Starting School at Age 7 Due to Academic Redshirting and Due to the School Enrollment Cutoff Date* – LATE Estimates on 10^{th}-Grade Standardized Mathematics and Reading Test Scores, and Grade Repetition by Grade 10, Administrative Data – Grade 10, For All and By Gender**

	School Start at 7 Due to Academic Redshirting *cutoff x_d : January 1*			***School Start at 7 Due to Enrollment Cutoff Date*** *cutoff x_d : June 1*		
outcome Y in grade 10:	**math**	**reading**	$1\{repeat\}$	**math**	**reading**	$1\{repeat\}$
Panel A: all students						
$\mathbb{1}$ {**start school at 7**}	0.1220**	0.0892	-0.0324	0.1393***	0.1809***	-0.0577***
	[0.056]	[0.056]	[0.020]	[0.030]	[0.029]	[0.011]
R^2	0.2433	0.2828	0.0644	0.2489	0.2807	0.0645
N	334,552	334,633	334,767	343,060	343,159	343,272
control complier mean of Y			*0.0942*			*0.1014*
Panel B: by gender						
	***male** students*					
$\mathbb{1}$ {**start school at 7**}	0.1960***	0.2469***	-0.0941***	0.1596***	0.2045***	-0.0743***
	[0.0733]	[0.0727]	[0.0265]	[0.0570]	[0.0552]	[0.0200]
R^2	0.2205	0.2321	0.0489	0.2359	0.2515	0.0690
N	161,464	161,479	161,565	166,681	166,709	166,778
control complier mean of Y			*0.1119*			*0.1199*
	***female** students*					
$\mathbb{1}$ {**start school at 7**}	0.0301	-0.1175	0.0468	0.1260***	0.1668***	-0.0494***
	[0.0883]	[0.0866]	[0.0289]	[0.0332]	[0.0319]	[0.0112]
R^2	0.2526	0.3010	0.0668	0.2465	0.2840	0.0672
N	173,088	173,154	173,202	176,379	176,450	176,494
control complier mean of Y			*0.0872*			*0.0581*

Notes: [1] This table shows the estimated coefficients and standard errors of the 2^{nd}-stage equation of Y on $\mathbb{1}$ {start school at 7} and control variable, where $\mathbb{1}$ {start school at 7} is a binary variable denoting age at primary school entry (1: child entered primary school at age 7, 0: child entered primary school at age 6). Control variables include: linear trend in month of birth and its interaction with a binary variable denoting quarter of birth, cohort fixed effects, and family background variables listed in Section 4, and missing response characteristics controls. The corresponding regression equation is equation (2).
[2] The sample includes children born in the three-months window around the cutoff dates of January 1 and June 1, respectively, for starting school at age 7 due to academic redshirting and due to the school enrollment cutoff date.
[3] In columns (1) and (4), the outcome is standardized mathematics test score in Grade 10; in columns (2) and (5), it is standardized reading test score in Grade 10; in columns (3) and (6), it is an indicator for having repeated a grade, by Grade 10.
[4] Standard errors are in brackets, and are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.
[5] "Control complier mean of Y" pertains to the estimated mean of Y_0 (the potential outcome of Y without the treatment), for children in the various *complier* subgroups, who were born in the three-months window around the cutoff dates of January 1 and June 1, respectively, estimated using the equation (A2) in Abdulkadiroglu et al. (2018).
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 10) 2008-2017.

Table 3: ***Start School at 7 Due to Academic Redshirting (January 1) and Due to the Enrollment Cutoff Date (June 1),*** **Average Characteristics of Compliers, Administrative Data – Grade 10 and Survey Data**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	cutoff x_d : *January 1*			*cutoff* x_d : *June 1*			
	for Academic Redshirting			**School Enrollment Cutoff**			
Panel A: Admin. Test Score Data	**share among**		**p-value △**	**share among**		**p-value △**	
	compliers	**sample**	**(1)-(2)**	**compliers**	**sample**	**(4)-(5)**	**(1)-(4)**
gender: boy	0.5611	0.4923	0.000***	0.4101	0.4941	0.000***	0.000***
parental education: primary	0.0982	0.1040	0.130	0.0857	0.1042	0.000***	0.002***
parental education: vocational	0.2648	0.2707	0.205	0.2832	0.2714	0.006***	0.000***
parental education: secondary	0.3173	0.3124	0.192	0.3308	0.3115	0.000***	0.009***
parental education: tertiary	0.2989	0.2945	0.477	0.2836	0.2943	0.052*	0.005***
family disadvantaged	0.2873	0.2952	0.178	0.2824	0.2943	0.030**	0.391
subsidized meals at school	0.2683	0.2715	0.343	0.2546	0.2733	0.000***	0.003***
free textbooks at school	0.4835	0.4921	0.115	0.4698	0.4919	0.000***	0.018**
only 0-50 books at home	0.1655	0.1668	0.753	0.1456	0.1657	0.000***	0.000***
own books at home	0.9461	0.9468	0.747	0.9587	0.9471	0.000***	0.000***
family considered poor	0.1616	0.1573	0.156	0.1432	0.1571	0.000***	0.000***
neighbourhood considered poor	0.1173	0.1212	0.254	0.1158	0.1204	0.178	0.684
number of siblings in household	1.3393	1.3675	0.017**	1.3045	1.3711	0.000***	0.010***
lives with stepmother	0.0194	0.0174	0.083*	0.0125	0.0173	0.000***	0.000***
lives in Budapest (capital)	0.1404	0.1219	0.033**	0.0989	0.1233	0.000**	0.000***
Panel B: Survey Data	**share among**		**p-value △**	**share among**		**p-value △**	
	compliers	**sample**	**(1)-(2)**	**compliers**	**sample**	**(4)-(5)**	**(1)-(4)**
gender: boy	0.5547	0.5046	0.0043***	0.4035	0.5101	0.002***	0.002***
age in months	181.32	175.67	0.000***	162.77	173.70	0.000***	0.000***
birth happened prematurely	0.0882	0.0803	0.4987	0.0668	0.0836	0.1905	0.3205
birth happened w/ complications	0.1430	0.1437	0.9562	0.1371	0.1424	0.744	0.8178
born with low birth weight	0.0979	0.0915	0.6051	0.0390	0.0852	0.0003***	0.0084*
was breastfed for <3 months	0.2932	0.2920	0.9409	0.2597	0.2820	0.2748	0.3021
separated from mother (<1y old)	0.0317	0.0231	0.1731	0.0024	0.0222	0.0026***	0.0135**
separated from father (<1y old)	0.1536	0.1399	0.3024	0.1292	0.1417	0.4273	0.3435
said first words >1y old	0.3612	0.3128	0.0033***	0.2531	0.3096	0.0064	0.0011***
age (months): said first sentences	23.07	21.77	0.000***	20.45	21.74	0.0003***	0.000***
chronic face illness (<3y old)	0.0648	0.0476	0.029**	0.0523	0.0497	0.8107	0.4603
chronic ear illness (<3y old)	0.1436	0.1102	0.0046***	0.1053	0.1147	0.5155	0.092*
chronic asthma (<3y old)	0.0463	0.0513	0.4937	0.0485	0.0476	0.9242	0.8828
⊖ *income shock to family (<3y old)*	0.0961	0.0798	0.1026	0.0747	0.0822	0.5551	0.289
problems w/ nerv.sys. (4-5y old)	0.0123	0.0068	0.0637*	0.0011	0.0063	0.1355	0.0607*
problems w/ speaking (4-5y old)	0.2178	0.1889	0.0562*	0.1349	0.1786	0.0125**	0.0042***
problems w/ cognition (4-5y old)	0.0219	0.0151	0.1604	0.0088	0.0156	0.1827	0.127
problems w/ coordination (4-5y old)	0.0126	0.0083	0.2506	0.0012	0.0079	0.1049	0.0968*
hyperactivity (4-5y old)	0.0222	0.0272	0.274	0.0132	0.0201	0.2388	0.2736
attention disorder (4-5y old)	0.0482	0.0182	0.0014***	0.0036	0.0280	0.0001***	0.0001***

Notes: [1] The relevant sample includes children who were born in the months October to March or March to August.
[2] The corresponding equation is (3). Standard errors are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.
Source of data: Administrative test score data – HNABC (grade 6) 2008-2017, and Hungarian Life Course Survey.

Table 4: ***Effect of Starting School at Age 7 Due to Academic Redshirting* – LATE Estimates on Anxiety, Confidence and Being Bullied Measures at Grade 8, By Gender, Survey Data and Admin. Health Data**

Panel A: Outcomes of Anxiety and Psycholeptics Drug Expenditures; *cutoff* x_d : *January 1*						
	boys			**girls**		
outcome Y**:**	***std. anxiety score***	***often feeling anxious***	***log(exp.) N05***	***std. anxiety score***	***often feeling anxious***	***log(exp.) N05***
$\mathbb{1}$ {**start school at 7**}	-1.1614**	-0.4555**	-0.0398**	-0.1132	-0.2905	0.0138
	[0.5101]	[0.2204]	[0.0181]	[0.6338]	[0.3212]	[0.0193]
N	38,516	38,516	62,027	37,844	37,844	64,264
control complier mean of Y		0.5149			0.6629	
1^{st}*-stage F-statistic:*	*39.44*	*39.44*	*435.54*	*27.5*	*27.5*	*406.93*

Panel B: Outcomes of Confidence and Being Bullied in Class; *cutoff* x_d : *January 1*						
	boys			**girls**		
outcome Y**:**	***lack of***	***bullied for***		***lack of***	***bullied for***	
	confidence	***appearance***	***studies***	***confidence***	***appearance***	***studies***
$\mathbb{1}$ {**start school at 7**}	-0.5021**	-0.3993*	-0.3272*	-0.0573	-0.166	0.0991
	[0.2476]	[0.2278]	[0.1860]	[0.3439]	[0.3348]	[0.2204]
N	38,480	38,489	38,496	37,850	37,857	37,827
control complier mean of Y	0.7604	0.3445	0.3732	0.6931	0.4465	0.0844
1^{st}*-stage F-statistic:*	*39.2*	*39.6*	*39.5*	*27.4*	*27.47*	*27.37*

Notes: [1] This table shows the estimated coefficients and standard errors of the 2^{nd}-stage equation of Y on $\mathbb{1}$ {start school at 7} and control variable, where $\mathbb{1}$ {start school at 7} is a binary variable denoting age at primary school entry (1: child entered primary school at age 7, 0: child entered primary school at age 6). Control variables include: linear trend in month of birth and its interaction with a binary variable denoting quarter of birth, cohort fixed effects, and family background variables listed in Section 4, and missing response characteristics controls. The corresponding regression equation is equation (2).
[2] The sample includes children born September-to-May around the cutoff date of January (the cutoff relevant for redshirting).
[3] Standard errors are in brackets, and are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.
[4] "Control complier mean of Y" pertains to the estimated mean of Y_0 (the potential outcome of Y without the treatment), for children in the various *complier* subgroups, who were born in the three-months window around the cutoff dates of January 1, estimated using the equation (A2) in Abdulkadiroglu et al. (2018).
[5] Outcome Y for each column (regression) can be seen in bold in the column titles and they are:
— *std. anxiety score* is the (standardized) mean score from students' responses to how often they experience indicators of Generalized Anxiety Disorder (GAD) in Grade 8 (score 5: 'daily'; 4: 'several times per week'; 3: 'weekly'; 2: 'monthly'; 1: 'never or rarely'): (i) having stomachache, (ii) being in bad mood and fatigued, (iii) being irritable, (iv) being fearful, (v) being nervous, (vi) having problems with falling asleep, (vii) waking up, (viii) being dizzy or exhausted, (ix) feeling nausea and vomiting;
— *often feeling anxious* is an indicator of student experiencing any of the GAD symptoms daily/several times per week;
— *lack of confidence* by a binary variable indicating whether the student agrees with any of the following statements, in Grade 8: (i) "*I am inclined to consider myself as an unsuccessful person without any talent.*"; (ii) "*I feel I cannot be proud of myself.*"; (iii) "*I wish I had more respect for myself.*"; (iv) "*I feel I am not good at anything.*"
— *bullied* by a binary variable for being bullied in class for appearance or poor student achievement, in Grade 8;
— *N05d expenditures* which capture prescriptions for psycholeptics, in Grade 8 (ATC code N05; antipsychotics and sedatives form the major groups in this category, primarily for treating anxiety).
Source of data: Hungarian Life Course Survey and 50 percent of the administrative test score data – Hungarian National Assessment of Basic Competences (grade 8) 2009-2017, linked with drug expenditures data.

Online Appendix

A Additional Institutional Details

Hungary has a universal childcare system, with a minimal (less than 4 percent) fraction of private institutions. Public childcare institutions provide an 8-hour long service per day free of charge, except for meal fees. According to the Act No. LXXIX of 1993 on Public Education (24(1)), childcare institutions educate children from age 3 until the start of compulsory school. According to the enactment of the Ministry of Education, 137/1996. (VIII. 28.), childcare teachers have to satisfy the physical and mental needs of children: to assure a healthy lifestyle, to provide emotional security, socialization, and to help mental skill enhancement.

According to Vago (2005, pp.744), a flexible system of primary school start in Hungary came into effect in 1986,[10] with the intention to provide opportunity for the less developed and immature children to spend one additional year in childcare, while for the more developed and mature children to start school earlier than required. There were strong social and institutional reasons, other than child abilities around school entry, for redshirting. In the middle of the 1980s, Hungary started open up and for many people studying and working abroad became possible; the increasing entrepreneurial opportunities and high unemployment rates among the unskilled and young around the transition from socialism incentivized primarily the middle-class parents to provide better quality, rather than fast education for their children. Moreover, elite primary schools required students to pass increasingly difficult entrance exams that were (more) easily taken by mature children.

The physical, psychological and social requirements for school-readiness are described in the enactment of the Ministry of Education, 137/1996. (VIII. 28.), VI./2. Children meeting the physical requirements can be characterized by well-proportioned body, more and more harmonious movements, proper audition, developing coordinational skills and the ability of controlling motion, behavior and physical needs. Children meeting the psychological requirements are open to enter primary school with gradually enhancing perceptional and remembering skills, being able to pay attention to others and knowing the primary behavioral requirements, knowing the own body and primary information about herself and her environment. Children meeting the social requirements are ready to accept the school life and primary school teacher, gradually become able to cooperate, to initiate contact and to conform to the rules. The aforementioned ones are generally needed for heightened ability to learn in school. For instance, proper audition and vision is essential to learn how to read and write properly. Knowing the own body is essential, since individuals generally determine different object of the world relative to themselves. There is also some "knowledge stock" children need to have at age primary school start, or it considerably helps them later. The developmental experts highlighted the following ones in general: own name, age, parents' name and occupation, siblings' name and age, different parts of the body and their role, assessing distance ("near" and "close"), assessing the order of events, different colors, different parts of the day, different seasons, the relationship between weather and clothing, organs and senses, basic counting abilities ("more" and "less", "shorter" and longer", "whole" and "part"), *etc*.

[10] Vago, I. (2005). Felfele Terjeszkedo Ovodaztatas - Stagnalo Hozzaferes. *Educatio*, 4:742-760.

B Results Supporting IV Validity

B.1 First-Stage Results on Starting School at the Age of 7

Table B1: ***Effect of Quarter of Birth*** **- First-Stage Results on D (1: Starting School at the Age of 7 (vs. 6)), Due to Academic Redshirting and Due to the School Enrollment Cutoff Date, Administrative Data – Grades 6/8/10, For All and By Gender**

outcome: $\mathbb{1}\{$**start school at 7**$\}$	***School Start at 7 Due to Academic Redshirting***			**School Start at 7 Due to** ***Enrollment Cutoff Date***		
	cutoff x_d : January 1			*cutoff x_d : June 1*		
grades:	**6**	**8**	**10**	**6**	**8**	**10**
Panel A: all students						
$Z = 1\{$**birth date** $X \geq x_d\}$	0.1241***	0.1243***	0.1119***	0.1703***	0.1923***	0.2141***
	[0.003]	[0.003]	[0.003]	[0.004]	[0.004]	[0.004]
R^2	0.1761	0.1679	0.1412	0.3117	0.3243	0.3389
N	357,280	341,964	334,767	375,276	356,691	343,272
baseline mean of Y	*0.1157*	*0.0986*	*0.0929*	*0.5703*	*0.5417*	*0.5010*
Panel B: by gender						
	male *students*					
$Z = 1\{$**birth date** $X \geq x_d\}$	0.1388***	0.1413***	0.1307***	0.1233***	0.1451***	0.1665***
	[0.0044]	[0.0045]	[0.0043]	[0.0045]	[0.0047]	[0.0052]
R^2	0.1809	0.1750	0.1558	0.2720	0.2811	0.2949
N	175,557	165,514	161,565	185,021	173,806	166,778
baseline mean of Y	*0.1464*	*0.1256*	*0.1133*	*0.6410*	*0.6138*	*0.5715*
	female *students*					
$Z = 1\{$**birth date** $X \geq x_d\}$	0.1096***	0.1079***	0.0941***	0.2164***	0.2385***	0.2603***
	[0.0038]	[0.0036]	[0.0036]	[0.0051]	[0.0052]	[0.0056]
R^2	0.1533	0.1449	0.1135	0.3416	0.3562	0.3720
N	181,723	176,450	173,202	190,255	182,885	176,494
baseline mean of Y	*0.0862*	*0.0733*	*0.0739*	*0.5009*	*0.4730*	*0.4346*

Notes: [1] The table shows the estimated coefficients and standard errors of a Linear Probability Model (LPM) of D on Z and control variables. D is a binary variable denoting age at primary school entry (1: child entered primary school at age 7, 0: child entered primary school at age 6). Z is a binary variable denoting quarter of birth (1: child was born on/after January 1 or June 1, 0: child was born before January 1 or June 1). Control variables include: linear trend in month of birth and its interaction with a binary variable denoting quarter of birth, cohort fixed effects, and family background variables listed in Section 4, and missing response characteristics controls. The corresponding regression equation is (1).
[2] The sample includes children born in the three-months window around the cutoff dates of January 1 and June 1, respectively.
[3] Standard errors are in brackets, and are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.
[4] "Baseline mean of Y" pertains to mean of D_i, for children in the various subgroups, who were born in the three-months window around the cutoff dates of January 1 and June 1, respectively, for whom also $Z_i = 0$.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grades 6,8,10) 2008-2017.

B.2 Sample Selection, Descriptives, and Evidence on Remediation

Table B2: **Details of Sample Selection, Administrative Data – Grade 10**

	2008	**2009**	**2010**	**2011**	**2012**	**2013**	**2014**	**2015**	**2016**	**2017**
All [N]	107,654	100,620	96,898	94,047	93,167	93,907	92,544	94,127	93,214	92,198
info on: yrs in childcare	80.83%	80.48%	83.12%	82.94%	84.54%	84.38%	84.15%	82.24%	84.72%	85.94%
info on: birthdate	79.82%	80.43%	82.96%	82.80%	82.72%	84.04%	84.06%	82.43%	84.54%	82.74%
has valid test score	79.69%	80.38%	82.73%	82.62%	82.11%	83.77%	83.71%	82.19%	83.12%	80.28%
info on: parental educ.	76.54%	77.84%	80.03%	80.97%	81.94%	81.11%	81.29%	81.23%	82.12%	79.94%

Notes: [1] Each cell in the $2^{nd} - 5^{th}$ row shows the fraction (%) of sample remaining after keeping children who have information on how many years they spent in childcare, who have valid information on their year and month of birth, and who have at least one valid test score, respectively; the 4^{th} row corresponds to the *analysis sample*.
[2] The last row shows the shows sample after keeping children who have valid information about their parent's education.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 10) 2008-2017.

I restrict the analysis to children for whom information about gender, time in childcare, and birth year and month is available and who have a valid test score. As shown in Table B2, the final sample contains 76.5-82.1 percent of the original sample. Reassuringly, the excluded observations seem to be random by gender, as there is no systematic relationship found between gender and non-response in the background survey, in the missing response analysis of the 2006–2007 HNABC of the Economics of Education and Labor Economics Research Unit at the *Hungarian Institute of Economics, Centre for Economic and Regional Studies* (available here, only in Hungarian).

Table B3: **Share of Boys and Children with Different Parental Education, by Month of Birth and School-Starting Age, Administrative Data – Grade 10**

Month of Birth:	**October-December**		**January-March**	
cutoff x_d : January 1	**Starting at 6**	**Starting at 7**	**Starting at 6**	**Starting at 7**
fraction of boys	47.5%	61.7%	44.1%	58.2%
(mean)	*49.4%*		*49.2%*	
fraction of parental educ: at most 8 years in school	9.3%	24.2%	8.8%	15.3%
(mean)	*11.584%*		*11.310%*	
fraction of parental educ: at most vocational training	28.7%	35.1%	28.1%	31.4%
(mean)	*29.437%*		*29.4%*	
fraction of parental educ: at most high school degree	32.4%	23.467%	32.8%	28.8%
(mean)	*31.043%*		*31.2%*	
Month of Birth:	**March-May**		**June-August**	
cutoff x_d : June 1	**Starting at 6**	**Starting at 7**	**Starting at 6**	**Starting at 7**
fraction of boys	41.2%	55.7%	35.4%	49.7%
(mean)	*49.6%*		*49.5%*	
fraction of parental educ: at most 8 years in school	9.1%	12.3%	9.8%	10.9%
(mean)	*11.4%*		*11.6%*	
fraction of parental educ: at most vocational training	28.9%	28.9%	23.8%	29.7%
(mean)	*28.9%*		*29.6%*	
fraction of parental educ: at most high school degree	32.4%	30.3%	27.7%	31.5%
(mean)	*30.9%*		*30.9%*	

Notes for Interpretation: [1] The first row shows, e.g., that 47.5 (58.2) percent of children who were born October–December (January–March), and started school at the age of 6, are boys.
[2] The second row shows that the share of boys born in the three-month window around the January 1 cutoff, is around 49 percent on both sides of the cutoff: it is 49.4 percent for children born in October–December and 49.2 percent for those in January–March.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 10) 2008-2017.

Table B4: ***Exogeneity of Quarter of Birth Around January 1* - "First-Stage" Results on Various Background and Early Childhood Characteristics, Administrative Data – Grade 6 and Survey Data**

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Admin. Test Score Data			***parental education***			
	boy	***primary***	***vocational***	***secondary***	***tertiary***	***disadv.***
$\mathbb{1}\{\text{birth-month} \geq x_d\}$	0.0025	0.0011	-0.0037	-0.001	0.0032	-0.0022
	[0.004]	[0.002]	[0.003]	[0.003]	[0.003]	[0.002]
R^2	0.0101	0.4030	0.1360	0.0823	0.3077	0.6516
N	369,016	369,016	369,016	369,016	369,016	369,016
	meals at school		***books at school***	***books at home***	***own desk***	***family***
	free	***supported***	***free***	***0-50***	***at home***	***poor***
$\mathbb{1}\{\text{birth-month} \geq x_d\}$	-0.0007	0.0013	-0.0035	0.0025	-0.0012	0.0021
	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
R^2	0.3499	0.6762	0.5657	0.2198	0.1177	0.1890608
N	369,016	369,016	369,016	369,016	369,016	369016
	household	***number of***	***lives with***	***neighbourhood***	***(capital)***	***North-Eastern***
	size	***siblings***	***stepmother***	***poor***	***Budapest***	***Hungary***
$\mathbb{1}\{\text{birth-month} \geq x_d\}$	0.0047	0.0095	-0.0003	-0.0009	0.0016	-0.002
	[0.006]	[0.007]	[0.001]	[0.002]	[0.002]	[0.002]
R^2	0.6349	0.2830	0.0174	0.1748	0.0479	0.0238
N	368,065	369,016	369,016	369,016	368,065	369,016
Survey Data	***premature***	***birth***	***born w/ low***	***breastfed***	***separated from***	
0-1y old	***birth***	***complications***	***birth weight***	***<3 months***	***mother***	***father***
$\mathbb{1}\{\text{birth-month} \geq x_d\}$	-0.0076	-0.001	-0.0123	-0.0423	0.0073	-0.0098
	[0.016]	[0.021]	[0.017]	[0.029]	[0.007]	[0.019]
R^2	0.0088	0.0116	0.0496	0.0339	0.3542	0.0955
N	79,441	79,441	79,422	77,148	78,369	78,383
1-3y old	1^{st} ***words***	1^{st} ***sentences***		***chronic illness in <3y***		***income***
	> 1y old	***age in months***	***face***	***ear***	***asthma***	***shock***
$\mathbb{1}\{\text{birth-month} \geq x_d\}$	-0.0053	-0.2461	-0.0137	0.0028	0.0111	0.0037
	[0.027]	[0.410]	[0.013]	[0.018]	[0.012]	[0.014]
R^2	0.0217	0.0382	0.0137	0.0072	0.0137	0.0516
N	76,921	68,468	78,810	78,904	79,381	78,983
4-5y old		***problems with***			***hyper-***	***attention***
	nervsys	***speaking***	***cognition***	***coord***	***activity***	***disorder***
$\mathbb{1}\{\text{birth-month} \geq x_d\}$	-0.005	-0.0247	-0.0038	-0.0016	0.0016	0.0086
	[0.005]	[0.023]	[0.006]	[0.004]	[0.006]	[0.007]
R^2	0.0040	0.0229	0.0133	0.0048	0.0163	0.0232
N	79,427	79,182	79,375	79,387	79,343	79,365

Notes: [1] The table shows the estimated coefficients and standard errors of various outcome variables (indicated in bold in the column titles) on $\mathbb{1}\{\text{birth-month} \geq x_d\}$ and control variables. $\mathbb{1}\{\text{birth-month} \geq x_d\}$ is a binary variable denoting quarter of birth (1: child was born on/after January 1, 0: child was born before January 1). Control variables include: linear trend in month of birth and its interaction with a binary variable denoting quarter of birth, cohort fixed effects, and family background variables listed in Section 4, and missing response characteristics controls. The corresponding regression equation is (1).
[2] The sample includes children born in the three-months window around the cutoff dates of January 1.
[3] Respondents in the survey data are weighted up to the population level.
[4] Standard errors are in brackets, and are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 10) 2008-2017, and Hungarian Life Course Survey.

Table B5: ***Effect of Starting School at Age 7 Due to Redshirting and Due to the Enrollment Cutoff Date* – LATE Estimates on Parental Investments and Tutoring, Administrative (Grade 10) and Survey Data**

outcome: $Y = 1\{$**parents invest/help**$\}$	***School Start at 7 Due to Academic Redshirting*** *cutoff x_d : January 1*			***School Start at 7 Due to Enrollment Cutoff Date*** *cutoff x_d : June 1*		
parental investments	**1**	**2**	**3**	**1**	**2**	**3**
Panel A: all students						
$\mathbb{1}\{$**start school at 7**$\}$	-0.008	-0.0124	-0.0064	-0.0066	-0.0480***	-0.0006
	[0.017]	[0.031]	[0.018]	[0.009]	[0.017]	[0.009]
N	329,949	334,767	334,767	338,098	343,272	343,272
control complier mean of Y	*0.0723*	*0.5872*	*0.0867*	*0.0607*	*0.5970*	*0.0728*
Panel B: by gender						
	male *students*					
$\mathbb{1}\{$**start school at 7**$\}$	0.0063	0.0455	-0.0115	0.0001	-0.0807**	0.0169
	[0.0222]	[0.0402]	[0.0212]	[0.0169]	[0.0318]	[0.0173]
N	158,720	161,565	161,565	163,701	166,778	166,778
control complier mean of Y	*0.0770*	*0.5455*	*0.0905*	*0.0680*	*0.5586*	*0.0733*
	female *students*					
$\mathbb{1}\{$**start school at 7**$\}$	-0.0275	-0.0866*	-0.0006	-0.0106	-0.0288	-0.0114
	[0.0265]	[0.0492]	[0.0298]	[0.0102]	[0.0194]	[0.0106]
N	171,229	173,202	173,202	174,397	176,494	176,494
control complier mean of Y	*0.0657*	*0.6506*	*0.0805*	*0.0550*	*0.6270*	*0.0725*
outcome: $Y = 1\{$**tutoring**$\}$	***School Start at 7 Due to Academic Redshirting***			***School Start at 7 Due to Enrollment Cutoff Date***		
tutoring	**any**	**math**	**teacher**	**any**	**math**	**teacher**
Panel C: all students						
$\mathbb{1}\{$**start school at 7**$\}$	-0.0025	0.018	0.0289	-0.015	-0.0018	-0.0284
	[0.029]	[0.027]	[0.2034]	[0.014]	[0.013]	[0.0886]
N	319,616	323,986	66,488	327,456	332,096	59,495
control complier mean of Y	*0.2346*	*0.2082*	*0.5451*	*0.1917*	*0.1781*	*0.5202*
Panel D: by gender						
	male *students*					
$\mathbb{1}\{$**start school at 7**$\}$	0.0178	0.0189	-0.1166	0.0027	-0.0043	-0.1251
	[0.0352]	[0.0303]	[0.2665]	[0.0264]	[0.0240]	[0.1434]
N	152,944	155,533	33,457	157,712	160,448	30,303
control complier mean of Y	*0.2158*	*0.1960*	*0.5782*	*0.1797*	*0.1568*	*0.5472*
	female *students*					
$\mathbb{1}\{$**start school at 7**$\}$	-0.0281	0.0174	0.1977	-0.0263	-0.0003	0.0784
	[0.0472]	[0.0472]	[0.3337]	[0.0165]	[0.0163]	[0.1165]
N	166,672	168,453	32,897	169,744	171,648	29,094
control complier mean of Y	*0.2596*	*0.2278*	*0.5109*	*0.1984*	*0.1909*	*0.5051*

Notes: '**1**' is 1 if the parents help the child with their homework, '**2**' is 1 if the parents discuss with the child what happened in school, '**3**' is 1 if the parents discuss with the child what the child has read; **tutoring** is 1 if the child is attending any/math tutoring classes, and **tutoring: teacher** is 1 if the child says that the teacher devotes extra attention to them in/outside class.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 10) 2008-2017, and Hungarian Life Course Survey (for the **tutoring: teacher** outcome).

C Placebo and Robustness Checks

C.1 Placebo Tests for Alternative Cutoffs Around January 1

Table C1: ***Placebo Tests for Alternative Cutoffs Around January 1*** **- First-Stage and Reduced-Form Estimates, For All and For Boys, Administrative Data – Grade 10**

	placebo cutoffs:	***Oct1***	***Nov1***	***Dec1***	***Jan1***	***Feb1***	***Mar1***
		1^{st}-***stage*** // $\mathbb{1}$ {**start school at 7**}					
all	$\mathbb{1}$ {birth-month $\geq x_d$}	0.0077***	0.0123***	0.0344***	**0.1387*** **	0.0563***	0.0111**
		[0.0015]	[0.0027]	[0.0019]	[0.0026]	[0.0028]	[0.0049]
boys	$\mathbb{1}$ {birth-month $\geq x_d$}	0.0100***	0.0163***	0.0435***	**0.1645*** **	0.0616***	0.0127*
		[0.0023]	[0.0041]	[0.0030]	[0.0038]	[0.0039]	[0.0071]
		reduced-form // Y_i: ***std. mathematics score in grade 10***					
all	$\mathbb{1}$ {birth-month $\geq x_d$}	-0.0007	-0.0039	-0.0032	**0.0097***	0.0007	0.0006
		[0.0050]	[0.0086]	[0.0052]	[0.0051]	[0.0051]	[0.0088]
boys	$\mathbb{1}$ {birth-month $\geq x_d$}	-0.003	-0.0025	-0.0065	**0.0200*** **	-0.0015	-0.0004
		[0.0074]	[0.0129]	[0.0075]	[0.0076]	[0.0077]	[0.0134]
		reduced-form // Y_i: ***std. reading score in grade 10***					
all	$\mathbb{1}$ {birth-month $\geq x_d$}	-0.0022	-0.0036	-0.0036	**0.0026**	0.007	-0.0162*
		[0.0051]	[0.0089]	[0.0048]	[0.0049]	[0.0051]	[0.0086]
boys	$\mathbb{1}$ {birth-month $\geq x_d$}	0.0036	-0.0175	-0.0111	**0.0168** **	0.0081	-0.0212
		[0.0075]	[0.0133]	[0.0071]	[0.0074]	[0.0076]	[0.0130]
		reduced-form // $Y_i = 1$ {***having repeated a grade by grade 10***}					
all	$\mathbb{1}$ {birth-month $\geq x_d$}	-0.0001	0.0026	-0.0019	**-0.0031***	0.0015	-0.0022
		[0.0017]	[0.0029]	[0.0018]	[0.0018]	[0.0017]	[0.0030]
boys	$\mathbb{1}$ {birth-month $\geq x_d$}	-0.0004	0.0036	-0.0003	**-0.0104*** **	0.0015	-0.0003
		[0.0026]	[0.0044]	[0.0027]	[0.0028]	[0.0027]	[0.0048]
		reduced-form // $Y_i = 1$ {**being in track** ”*vocational*”}					
all	$\mathbb{1}$ {birth-month $\geq x_d$}	0.0021	0.0029	-0.0037*	**-0.0057*** **	0.0009	0.0032
		[0.0020]	[0.0036]	[0.0020]	[0.0019]	[0.0019]	[0.0033]
boys	$\mathbb{1}$ {birth-month $\geq x_d$}	0.0013	0.0036	-0.0008	**-0.0119*** **	0.0012	0.005
		[0.0031]	[0.0057]	[0.0031]	[0.0030]	[0.0031]	[0.0052]
		reduced-form // $Y_i = 1$ {**being in track** ”*academic*”}					
all	$\mathbb{1}$ {birth-month $\geq x_d$}	-0.0026	-0.0009	-0.0003	**0.004**	0.0012	0.0005
		[0.0026]	[0.0044]	[0.0026]	[0.0026]	[0.0027]	[0.0048]
boys	$\mathbb{1}$ {birth-month $\geq x_d$}	-0.0002	-0.0028	-0.0048	**0.0106*** **	0.0001	0.0018
		[0.0038]	[0.0065]	[0.0037]	[0.0037]	[0.0036]	[0.0064]
		reduced-form // $Y_i = 1$ {**aspiring for** $HS+$ **(in grade 10)**}					
all	$\mathbb{1}$ {birth-month $\geq x_d$}	0.0007	-0.0053	-0.0036	**0.0018**	0.0011	-0.0046
		[0.0025]	[0.0044]	[0.0026]	[0.0026]	[0.0027]	[0.0048]
boys	$\mathbb{1}$ {birth-month $\geq x_d$}	0.0061	-0.0154**	-0.0053	**0.0084** **	0.0021	-0.0059
		[0.0037]	[0.0065]	[0.0038]	[0.0038]	[0.0039]	[0.0067]

Notes: [1] The table shows the estimated coefficients and standard errors of the 1^{st}-stage and reduced form equations, when using a 1-month window around a particular cutoff x_d; for instance, column (1) uses only children who were born in either September or in October. Z is a binary variable denoting month of birth (1: child was born on/after a given cutoff x_d, 0: child was born before the given cutoff x_d). Control variables include: cohort fixed effects, and family background variables listed in Section 4, and missing response characteristics controls. The corresponding regression equation is (1).
[2] Standard errors are in brackets, and are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 10) 2008-2017.

C.2 Robustness Check: LATE of Starting School at Age 7 Due to Redshirting; Window Length and Functional Form around January 1

Table C2: ***Robustness Check with a 1–2–3-Months Window and a Linear/Quadratic Trend Around January 1*** **– LATE Estimates of Starting School at Age 7 Due to Academic Redshirting on Standardized Test Score, Grade Repetition, Secondary School Track Choice and Educational Aspirations, All Children, Administrative Data – Grades 6/8/10**

	1-M Window, No Trend			***2-M Window, Lin. Trend***			***3-M Window, Quadr. Trend***		
Y: mathematics test score									
grade	**6**	**8**	**10**	**6**	**8**	**10**	**6**	**8**	**10**
D	0.0868***	0.0711**	0.0702*	0.2088***	0.1782**	0.1225	0.2809**	0.2572**	0.1303
	[0.0315]	[0.0340]	[0.0367]	[0.0776]	[0.0838]	[0.0857]	[0.1097]	[0.1172]	[0.1198]
R^2	0.218	0.227	0.245	0.204	0.215	0.243	0.196	0.207	0.242
N	121,641	116,361	114,077	235,839	225,493	221,062	357,158	341,803	334,552
Y: reading test score									
grade	**6**	**8**	**10**	**6**	**8**	**10**	**6**	**8**	**10**
D	0.0792**	0.0540*	0.0187	0.1974***	0.1500*	0.057	0.2636**	0.2491**	0.2134*
	[0.0308]	[0.0326]	[0.0356]	[0.0756]	[0.0796]	[0.0796]	[0.1062]	[0.1164]	[0.1133]
R^2	0.279	0.291	0.284	0.269	0.281	0.285	0.263	0.271	0.268
N	121,662	116,376	114,106	235,889	225,535	221,114	357,230	341,881	334,633
$Y = 1$ {***having repeated a grade***}									
grade	**6**	**8**	**10**	**6**	**8**	**10**	**6**	**8**	**10**
D	-0.0194***	-0.0094	-0.0220*	-0.0263	-0.0189	-0.0109	-0.0096	-0.0241	0.0139
	[0.0072]	[0.0081]	[0.0129]	[0.0180]	[0.0197]	[0.0302]	[0.0253]	[0.0281]	[0.0412]
R^2	0.082	0.077	0.064	0.083	0.074	0.067	0.087	0.074	0.068
N	121,682	116,402	114,155	235,926	225,591	221,208	357,280	341,964	334,767
$Y = 1$ {**being in track** j}									
track	*middle*	*high*	*low*	*middle*	*high*	*low*	*middle*	*high*	*low*
D	-0.0412***	0.0123	0.0289	-0.0184	-0.0236	0.042	-0.0394	0.0026	0.0369
	[0.0140]	[0.0209]	[0.0188]	[0.0328]	[0.0466]	[0.0427]	[0.0480]	[0.0664]	[0.0637]
R^2	0.231	0.061	0.235	0.238	0.061	0.235	0.236	0.060	0.236
N	114,155	114,155	114,155	221,208	221,208	221,208	334,767	334,767	334,767
$Y = 1$ {**aspiration** j}									
level	$< HS$	HS	$HS+$	$< HS$	HS	$HS+$	$< HS$	HS	$HS+$
D	-0.014	0.0008	0.0132	0.0043	-0.0577	0.0534	-0.0281	-0.0757	0.1038
	[0.0124]	[0.0206]	[0.0190]	[0.0287]	[0.0466]	[0.0432]	[0.0408]	[0.0703]	[0.0648]
R^2	0.182	0.115	0.284	0.187	0.111	0.281	0.183	0.109	0.274
N	112,368	112,368	112,368	217,784	217,784	217,784	329,424	329,424	329,424

Notes: [1] The table shows the estimated coefficients and standard errors of the 2^{nd}-stage equation of Y on D and control variables. D is $\mathbb{1}$ {start school at 7}, a binary variable denoting age at primary school entry (1: child entered primary school at age 7, 0: child entered primary school at age 6). Control variables include: linear trend in month of birth and its interaction with a binary variable denoting quarter of birth, cohort fixed effects, and family background variables listed in Section 4, and missing response characteristics controls. The corresponding regression equation is (2).
[2] The sample includes children born in the two- vs. three-months window around the cutoff dates of January 1.
[3] Standard errors are in brackets, and are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grades 6,8,10) 2008-2017.

Table C3: ***Robustness Check with a 1–2–3-Months Window and a Linear/Quadratic Trend Around January 1* – LATE Estimates of Starting School at Age 7 Due to Academic Redshirting on Standardized Test Score, Grade Repetition, Secondary School Track Choice and Educational Aspirations, Boys, Administrative Data – Grades 6/8/10**

	1-M Window, No Trend			***2-M Window, Lin. Trend***			***3-M Window, Quadr. Trend***		
Y: mathematics test score									
grade	**6**	**8**	**10**	**6**	**8**	**10**	**6**	**8**	**10**
D	0.1601***	0.0810*	0.1216***	0.3345***	0.1647	0.2170**	0.3716**	0.2677*	0.2467
	[0.0422]	[0.0431]	[0.0464]	[0.1098]	[0.1124]	[0.1071]	[0.1447]	[0.1616]	[0.1559]
R^2	0.217	0.233	0.229	0.190	0.222	0.218	0.186	0.210	0.212
N	59,790	56,272	55,230	115,888	109,295	106,767	175,507	165,447	161,464
Y: reading test score									
grade	**6**	**8**	**10**	**6**	**8**	**10**	**6**	**8**	**10**
D	0.1483***	0.1322***	0.1021**	0.2894***	0.3095***	0.2242**	0.3299**	0.4369***	0.4747***
	[0.0401]	[0.0421]	[0.0452]	[0.1056]	[0.1085]	[0.1024]	[0.1391]	[0.1619]	[0.1576]
R^2	0.245	0.250	0.244	0.229	0.225	0.234	0.188	0.168	0.155
N	59,791	56,276	55,242	115,898	109,306	106,775	181,633	170,849	166,273
$Y = 1$ {***having repeated a grade***}									
grade	**6**	**8**	**10**	**6**	**8**	**10**	**6**	**8**	**10**
D	-0.0326***	-0.0171	-0.0634***	-0.0412	0.0003	-0.0845**	-0.0215	0.025	-0.0882
	[0.0098]	[0.0106]	[0.0171]	[0.0259]	[0.0275]	[0.0397]	[0.0343]	[0.0398]	[0.0550]
R^2	0.087	0.081	0.055	0.086	0.082	0.052	0.092	0.080	0.051
N	59,806	56,290	55,272	115,923	109,334	106,836	175,557	165,514	161,565
$Y = 1$ {**being in track** j}									
track	*middle*	*high*	*low*	*middle*	*high*	*low*	*middle*	*high*	*low*
D	-0.0723***	0.0077	0.0646***	-0.0905**	-0.0359	0.1263**	-0.1359**	0.0075	0.1284
	[0.0184]	[0.0249]	[0.0226]	[0.0445]	[0.0586]	[0.0529]	[0.0660]	[0.0864]	[0.0791]
R^2	0.225	0.058	0.212	0.223	0.056	0.202	0.211	0.056	0.203
N	55,272	55,272	55,272	106,836	106,836	106,836	161,565	161,565	161,565
$Y = 1$ {**aspiration** j}									
level	$< HS$	HS	$HS+$	$< HS$	HS	$HS+$	$< HS$	HS	$HS+$
D	-0.028	-0.0232	0.0512**	-0.0062	-0.1075*	0.1137**	-0.0649	-0.1081	0.1729**
	[0.0171]	[0.0258]	[0.0230]	[0.0402]	[0.0600]	[0.0540]	[0.0573]	[0.0895]	[0.0816]
R^2	0.192	0.088	0.265	0.197	0.081	0.258	0.188	0.080	0.244
N	54,322	54,322	54,322	105,045	105,045	105,045	158,710	158,710	158,710

Notes: [1] The table shows the estimated coefficients and standard errors of the 2^{nd}-stage equation of Y on D and control variables. D is $\mathbb{1}$ {start school at 7}, a binary variable denoting age at primary school entry (1: child entered primary school at age 7, 0: child entered primary school at age 6). Control variables include: linear trend in month of birth and its interaction with a binary variable denoting quarter of birth, cohort fixed effects, and family background variables listed in Section 4, and missing response characteristics controls. The corresponding regression equation is (2).
[2] The sample includes children born in the two- vs. three-months window around the cutoff dates of January 1.
[3] Standard errors are in brackets, and are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grades 6,8,10) 2008-2017.

C.3 Robustness Check: Standard Error Estimation

Based on Lee and Card (2008), the ideal level of clustering of standard errors (SEs) would be at the birthdate level in my setup, but that would lead to an insufficient number of clusters (6). To avoid a downward bias in the SE estimates due to a small number of clusters, and as I expect the regressors and errors to be correlated across students within schools, I cluster SEs at the school level, based on Cameron and Miller (2015). In this Appendix, I demonstrate that clustering at the school level is the most conservative relative to alternatives (clustering at birth month and cohort level) and accounting for the discrete running variable (Kolesar and Rothe, 2018).

Table C4: ***Robustness Check* – Standard Error Estimates and t-Statistics of Coefficient Estimates on Variable $\mathbb{1}\{$start school at 7$\}$, for Alternative Ways of Estimating Standard Errors, Administrative Data – Grade 10, For All and for Boys**

		clustering on:						**estimates with ‘rdhonest’**		
		school ID		***birth month***		***(birthm)×yr***				
outcome//(sub)group	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Y: std. math score	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$
all	0.1220	0.0564	2.16	0.0029	42.65	0.0561	2.18	0.1806	0.0660	2.82
boys	0.1960	0.0733	2.67	0.0108	18.11	0.0720	2.72	0.2837	0.0846	3.35
Y: std. reading score	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$
all	0.0892	0.0558	1.60	0.0262	3.41	0.0537	1.66	0.1440	0.0658	2.19
boys	0.2469	0.0727	3.40	0.0320	7.73	0.0706	3.50	0.3138	0.0836	3.76
$Y = 1\{repeat\}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$
all	-0.0324	0.0199	-1.63	0.0096	-3.39	0.0185	-1.75	-0.0318	0.0200	-1.59
boys	-0.0941	0.0265	-3.55	0.0034	-28.05	0.0243	-3.87	-0.0930	0.0265	-3.51
$Y = 1\{track : low\}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$
all	-0.0732	0.0233	-3.14	0.0194	-3.78	0.0217	-3.37	-0.0936	0.0257	-3.64
boys	-0.1193	0.0306	-3.9	0.0113	-10.54	0.0267	-4.47	-0.1479	0.0349	-4.24
$Y = 1\{track : high\}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$
all	0.0564	0.0290	1.94	0.0059	9.52	0.0264	2.14	0.0797	0.0328	2.42
boys	0.1067	0.0365	2.93	0.0070	15.2	0.0283	3.77	0.1373	0.0391	3.51
$Y = 1\{asp :< HS\}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$
all	-0.0258	0.0190	-1.36	0.0110	-2.36	0.0152	-1.70	-0.0381	0.0201	-1.89
boys	-0.0524	0.0261	-2.01	0.0169	-3.1	0.0162	-3.22	-0.0686	0.0285	-2.41
$Y = 1\{asp : HS+\}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$
all	0.0618	0.0293	2.11	0.0085	7.3	0.0230	2.69	0.0887	0.0336	2.64
boys	0.1343	0.0362	3.71	0.0113	11.87	0.0358	3.75	0.1657	0.0420	3.95
$\mathbb{1}\{$**birth-month** $\geq x_d\}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$			
all	0.1119	0.0029	38.58	0.0045	24.86	0.0119	9.42			
boys	0.1307	0.0043	30.29	0.0057	23.03	0.0134	9.77			

Notes: [1] This table shows the standard error estimates and corresponding value of t-statistics, of coefficient estimates on variable $\mathbb{1}\{$start school at 7$\}$, for alternative ways of estimating standard errors (different level of clustering, as well as with the ‘rdhonest’ STATA command implementing Kolesar and Rothe (2018), for the case when the running variable only takes a moderate number of distinct values – in this case, the month of birth takes on just 6 values).
[2] Columns (1)–(3) show the baseline results, already presented in Tables 2 and E4.
[3] Columns (4)–(5) show the standard error estimates and t-statistics if clustering was at the level of birth-month ($N_{clusters} = 6$) and Columns (6)–(7) show them if clustering was at the level of (birth-month)×year ($N_{clusters} = 60$).
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 10) 2008-2017.

C.4 Robustness Check: LATE of Starting School at Age 7, by Accounting for Childcare-Starting Age and Years Spent in Childcare

The estimated effects presented in Sections 6.1 and 6.2 encompass the fact that children who start school a year older typically also start childcare a year later. Moreover, children around the January 1 and June 1 cutoffs may differ in their propensities to start childcare at various ages, affecting the length of childcare. Then, one might wonder to what extent results presented in Sections 6.1 and 6.2 are robust to accounting for the potentially endogenous childcare-starting age (CSA) and years spent in childcare ($yearsCC$). Given the identity of $SSA = CSA + yearsCC$ holds, in this Appendix, I jointly estimate the effect of SSA and $yearsCC$, where SSA is still captured by an indicator $\mathbb{1}\{\text{start school at 7}\}$ that is 1 if the child started school at the age of 7 (*versus* at age of 6).

First, in Table C5 below, I show that the share of children who start childcare at the age of 3 or later jumps by 19 percentage points at the January 1 cutoff, which comes from the fact that 28.7 percent of December-born children start childcare at the age of 2, but only 9.3 percent of January-born children do so. (Note that there is no jump in this share at the June 1 cutoff.) At the same time, the share of children who start childcare at the age of 4 or later jumps by only 4.5 percentage points at the January 1 cutoff, and jumps by 12 percentage points at the June 1 cutoff. The average years spent in childcare is 0.092 ($3.146 - 3.238$) *lower* among January-born children than among December-born ones, and it is 0.158 ($3.498 - 3.340$) *higher* among June-born children than among May-born ones. Thus, the cutoffs do influence to some extent years spent in childcare ($yearsCC$).

To jointly estimate the effect of $\mathbb{1}\{\text{start school at 7}\}$ and $yearsCC$, I present a variant of estimation equation (2), in which I include and instrument for $yearsCC$. Note that it is ambiguous in what order parents make decisions about their children's educational path. They might decide sequentially, first deciding about childcare-starting age, and only at the relevant school-starting age do they decide about redshirting, determining $yearsCC$. However, they might very well be forward-looking, and decide about CSA with the possibility of redshirting already in mind. If the latter is the case, then the less time spent in childcare is part of the mechanism through which redshirting impacts child outcomes, and is ideally not controlled for. Given the ambiguity regarding the timing of parental decisions, I present the regression that accounts for CSA as a variant, and not as the main specification (but, the two specifications yield very similar estimates for the impact of redshirting).

To account for the endogenous nature of $yearsCC$, I use the fraction of 3-year-old children in childcare in the child's municipality at her age of 2 as an instrument, for the following institutional reason: as described in Section 3, September-to-December-born children are more likely to enter childcare at age 2, as they are preferred by the childcare institutions if there is a queue for childcare by 2-year-olds.[11] Thus, the fraction of 3-year-old children in childcare is strongly related to whether a 2-year-old can enter.[12]

Table C6 shows the result of the first-stage regressions on the variable D_i, that is 1 if the child entered school at the age of 7, as opposed to 6, and years spent in childcare, $yearsCC_i$, for children born in a 3-months window around the January 1 and June 1 cutoffs, respectively. Regarding $yearsCC_i$, for both boys and girls, the estimated coefficients on the fraction of 3-year-old children in childcare (in the child's municipality) at her age of 2 are in line with as predicted by the institutional background. On the one hand, a higher fraction of 3-year-olds is associated with more years in childcare (due to a lower likelihood of entering at age 3+) for children born before January 1; these children turn 3 years old within 6 months of acceptance, and thus are preferred to enter at age 2 if local supply constraints permit. On the other hand, a higher fraction of 3-year-olds is associated with a significantly lower increase in years of childcare for children born after January 1; these children are not preferred to enter at age 2, if there is queuing for childcare at that age. The *joint F-statistic* is above 80 for both D_i and CC_i, and the values of both of the *Cragg-Donald Wald F-statistics* and the *Kleibergen-Papp F-statistics* are above 80 as well. Therefore, weak identification is not a concern.

Table C7 reveals that the effect of starting school a year older, due to academic redshirting using the January 1 cutoff, is remarkably robust for both boys and girls, when accounting for $yearsCC$: fixing it, redshirted boys achieve a significantly higher score in math and reading, on average, by 0.6 and 0.275 SDs, on Grade 10 testings, respectively, and they are 7.4 percentage points less likely than on-time children to repeat a grade by Grade 10;

[11] In particular, the childcare institution can admit 2-year-old children, provided that all the 3-year-old and older applicants living in the particular municipality are admitted (Public Education Act 24(1)).

[12] I construct the instrument the following way. First, for each municipality, I collect the number of 3-years-old children in childcare from KIR-Stat, and aggregate institution-level data to municipality-level. /KIR-Stat provides the most comprehensive data about the Hungarian educational system; all institutions all years are required to fill out a data form according to the enactment of the Ministry of Education 229/2006. (XI. 20.)./ Second, for each municipality, I collect the total number of 3-years-old children from the municipality-level demographic data set by cohorts of the Hungarian Statistical Authority. Third, I form the fraction of 3-years-old children in childcare and attach this measure to each child given the municipality the child lived in at her age of 2, by assuming that her family has not moved since.

they are 15 percentage points less likely to attend the lowest, vocational, high school track, and 13.5 percentage points more likely to attend the highest, academic high school track; they are 6 (13) percentage points less (more) likely to aspire to educational levels that are lower (higher) than obtaining a high school degree. For girls, we continue to see no significantly positive effects of redshirting. Interestingly, the significantly positive effects of starting school a year older due to being born after and complying with the school enrollment cutoff date of June 1 typically disappears when accounting for $yearsCC$. The *Hansen J-statistics* are typically below 1, and never higher than 2.6, thus the null hypothesis of valid (excludable) instruments cannot be rejected at the usual sig. levels. My results are in line with those of Szabo-Morvai et al. (2023).

Table C5: **Distribution, Fraction of Children Starting School at Age 7 and Average Years Spent in Childcare, by Month of Birth and Childcare-Starting Age (CSA), Administrative Data – Grade 10**

month of birth	childcare-starting age (CSA)	2	3	4	5	average
September	**frequency**	**48.29%**	**39.56%**	**8.69%**	**3.46%**	
	fraction of children entering school at age 7	*0.89%*	*7.71%*	*26.80%*	*29.52%*	6.62%
	average years spent in childcare	3.987	3.078	2.316	1.736	3.404
October	**frequency**	**43.16%**	**43.93%**	**9.21%**	**3.69%**	
	fraction of children entering school at age 7	*1.26%*	*7.84%*	*28.80%*	*31.38%*	7.57%
	average years spent in childcare	3.992	3.079	2.337	1.729	3.355
November	**frequency**	**38.03%**	**47.95%**	**9.88%**	**4.14%**	
	fraction of children entering school at age 7	*1.65%*	*9.30%*	*33.35%*	*35.48%*	9.55%
	average years spent in childcare	3.996	3.094	2.385	1.766	3.312
December	**frequency**	**28.66%**	**55.62%**	**11.14%**	**4.57%**	
	fraction of children entering school at age 7	*2.78%*	*12.23%*	*38.41%*	*39.89%*	13.38%
	average years spent in childcare	4.001	3.124	2.437	1.794	3.238
January	**frequency**	**9.27%**	**70.50%**	**14.18%**	**6.05%**	
	fraction of children entering school at age 7	*10.74%*	*23.25%*	*56.36%*	*56.43%*	28.22%
	average years spent in childcare	4.040	3.233	2.635	1.961	3.146
February	**frequency**	**7.19%**	**71.28%**	**15.32%**	**6.21%**	
	fraction of children entering school at age 7	*13.98%*	*28.79%*	*61.06%*	*60.24%*	34.10%
	average years spent in childcare	4.046	3.289	2.685	1.965	3.169
March	**frequency**	**5.50%**	**71.21%**	**16.52%**	**6.76%**	
	fraction of children entering school at age 7	*21.16%*	*35.55%*	*66.17%*	*63.43%*	41.18%
	average years spent in childcare	4.089	3.357	2.735	2.005	3.203
April	**frequency**	**4.40%**	**70.41%**	**17.92%**	**7.26%**	
	fraction of children entering school at age 7	*28.39%*	*43.28%*	*72.70%*	*69.53%*	49.26%
	average years spent in childcare	4.122	3.434	2.805	2.052	3.251
May	**frequency**	**3.44%**	**69%**	**20.07%**	**7.48%**	
	fraction of children entering school at age 7	*39.76%*	*56.27%*	*79.90%*	*74.28%*	61.38%
	average years spent in childcare	4.188	3.565	2.881	2.098	3.340
June	**frequency**	**2.19%**	**58.05%**	**29.87%**	**9.89%**	
	fraction of children entering school at age 7	*74.24%*	*90.90%*	*97.02%*	*95.80%*	92.75%
	average years spent in childcare	4.455	3.911	3.040	2.247	3.498
July	**frequency**	**2.02%**	**55.07%**	**32.56%**	**10.34%**	
	fraction of children entering school at age 7	*80.51%*	*95.30%*	*98.35%*	*97.42%*	96.20%
	average years spent in childcare	4.418	3.955	3.044	2.279	3.494
August	**frequency**	**1.84%**	**52.29%**	**35.18%**	**10.69%**	
	fraction of children entering school at age 7	*83.42%*	*97.22%*	*98.91%*	*98.41%*	97.71%
	average years spent in childcare	4.405	3.976	3.049	2.283	3.477

Note: Bold percentages in each row add up to 100%. The averages are shown for given month of birth.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 10) 2008-2017.

Table C6: ***Effect of Quarter of Birth and the Fraction of 3-Years-Olds in Childcare* - First-stage Results on Starting School at Age 7 (D) and on Years in Childcare ($yearsCC$), Administrative Data – Grade 10**

	cutoff x_d : *January 1*		*cutoff* x_d : *June 1*	
	$\mathbb{1}$ {start school at 7}	$yearsCC$	$\mathbb{1}$ {start school at 7}	$yearsCC$
Panel A: all students				
$1\{birth\text{-}month \geq x_d\}$	0.0859***	0.0317**	0.2663***	0.0729***
	[0.008]	[0.015]	[0.009]	[0.016]
$frac\_3yo\_in\_CC$	0.0120**	0.3344***	0.0704***	0.2500***
	[0.006]	[0.017]	[0.010]	[0.016]
$1\{birth\text{-}month \geq x_d\} \times$	0.0281***	-0.1021***	-0.0844***	0.0200
$frac\_3yo\_in\_CC$	[0.010]	[0.019]	[0.012]	[0.020]
R^2	0.151	0.043	0.341	0.080
N	211,628	211,628	193,885	193,885
joint F-statistic:	*298.81*	*175.02*	*536.82*	*170.72*
Cragg-Donald Wald F-statistic:	*280.03*		*437.61*	
Kleibergen-Paap rk Wald F-statistic:	*217.88*		*130.62*	
Panel B: by gender				
	***male** students*			
$1\{birth\text{-}month \geq x_d\}$	0.0847***	0.0447**	0.2225***	0.0259
	[0.012]	[0.020]	[0.012]	[0.021]
$frac\_3yo\_in\_CC$	0.008	0.3169***	0.0799***	0.2520***
	[0.009]	[0.021]	[0.013]	[0.022]
$1\{birth\text{-}month \geq x_d\} \times$	0.0583***	-0.0845***	-0.0961***	0.0076
$frac\_3yo\_in\_CC$	[0.014]	[0.025]	[0.015]	[0.026]
R^2	0.164	0.038	0.299	0.065
N	102,691	102,691	94,466	94,466
joint F-statistic:	*194.92*	*96.51*	*212.06*	*79.56*
Cragg-Donald Wald F-statistic:	*181.04*		*191.13*	
Kleibergen-Paap rk Wald F-statistic:	*124.00*		*78.40*	
	***female** students*			
$1\{birth\text{-}month \geq x_d\}$	0.0871***	0.0191	0.3114***	0.1214***
	[0.010]	[0.019]	[0.012]	[0.021]
$frac\_3yo\_in\_CC$	0.0156**	0.3505***	0.0624***	0.2496***
	[0.006]	[0.021]	[0.013]	[0.020]
$1\{birth\text{-}month \geq x_d\} \times$	-0.0005	-0.1179***	-0.0758***	0.0286
$frac\_3yo\_in\_CC$	[0.012]	[0.025]	[0.014]	[0.027]
R^2	0.123	0.052	0.370	0.101
N	108,937	108,937	99,419	99,419
joint F-statistic:	*122.92*	*147.32*	*447.02*	*167.87*
Cragg-Donald Wald F-statistic:	*104.01*		*251.50*	
Kleibergen-Paap rk Wald F-statistic:	*107.38*		*102.87*	

Notes: [1] The table shows the first-stage results of $\mathbb{1}$ {start school at 7} and CC_i, where $\mathbb{1}$ {start school at 7} is a binary variable denoting age at primary school entry of child i (1: child entered primary school at age 7 *versus* at age 6), and CC_i is a binary variable denoting age at childcare entry of child i (1: child entered childcare at age 3+, 0: child entered childcare at age 2). $1\{\text{birth-month} \geq x_d\}$ is a binary variable denoting quarter of birth of child i (1: child was born on/after January 1, 0: child was born before January 1). $frac\_3yo\_in\_CC$ denotes the fraction of 3-year-old children in childcare in child i's municipality at her age of 2. Control variables include: linear trend in month of birth and its interaction with a binary variable denoting quarter of birth, cohort fixed effects, and family background variables listed in Section 4, and missing response characteristics controls.
[2] The sample includes children born in the three-months window around the cutoff dates of January 1.
[3] Standard errors are in brackets, and are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 6) 2008-2017, KIR-Stat/DEM 1998-2006; DEM: the municipality-level demographic data set by cohorts of the Hungarian Statistical Authority.

Table C7: **Second-stage//IV Results of Starting School at Age 7 and Years in Childcare**

Panel A: outcome Y in grade 10:	*cutoff x_d : January 1*			*cutoff x_d : June 1*		
	math	**reading**	$1\{repeat\}$	**math**	**reading**	$1\{repeat\}$
	***male** students*					
$\mathbb{1}$ {**start school at 7**}	0.2621**	0.2746***	-0.0740**	0.0177	0.0748	-0.0372
	[0.131]	[0.094]	[0.030]	[0.084]	[0.078]	[0.026]
$yearsCC$	0.2410***	0.1455*	-0.0407*	0.2684***	0.2283**	-0.0461*
	[0.069]	[0.083]	[0.022]	[0.097]	[0.092]	[0.024]
N	102,637	102,647	102,691	94,409	94,431	94,466
Hansen J-statistics:	*0.899*	*2.104*	*0.562*	*0.101*	*1.31*	*0.512*
	***female** students*					
$\mathbb{1}$ {**start school at 7**}	0.1884*	0.0229	0.0487	-0.1110*	-0.1017*	-0.0524***
	[0.096]	[0.130]	[0.038]	[0.064]	[0.061]	[0.017]
$yearsCC$	0.2127**	0.1626**	0.0141	0.3224***	0.2984***	0.0455**
	[0.086]	[0.064]	[0.016]	[0.087]	[0.083]	[0.019]
N	108,859	108,921	108,937	99,352	99,404	99,419
Hansen J-statistics:	*0.379*	*1.815*	*0.588*	*0.026*	*0.149*	*0.136*
Panel B: HS tracks in grade 10:	*cutoff x_d : January 1*			*cutoff x_d : June 1*		
	low	*middle*	*high*	*low*	*middle*	*high*
	***male** students*					
$\mathbb{1}$ {**start school at 7**}	-0.1499***	0.0147	0.1352***	-0.0750**	-0.0078	0.0828**
	[0.038]	[0.051]	[0.045]	[0.032]	[0.043]	[0.041]
$yearsCC$	0.0076	-0.1076*	0.1	-0.0253	-0.0832	0.1085
	[0.042]	[0.062]	[0.072]	[0.046]	[0.071]	[0.082]
N	102,691	102,691	102,691	94,466	94,466	94,466
Hansen J-statistics:	*0.447*	*0.581*	*0.056*	*0.071*	*0.075*	*0.298*
	***female** students*					
$\mathbb{1}$ {**start school at 7**}	-0.05	-0.0505	0.1005	-0.0217	0.0079	0.0139
	[0.048]	[0.081]	[0.083]	[0.022]	[0.049]	[0.052]
$yearsCC$	-0.0092	-0.0832	0.0924	-0.0133	-0.0991	0.1124
	[0.027]	[0.064]	[0.071]	[0.029]	[0.083]	[0.090]
N	108,937	108,937	108,937	99,419	99,419	99,419
Hansen J-statistics:	*1.579*	*0.716*	*0.021*	*0.451*	*0.243*	*0.963*
Panel C: educ. aspirations:	*cutoff x_d : January 1*			*cutoff x_d : June 1*		
	$< HS$	HS	$HS+$	$< HS$	HS	$HS+$
	***male** students*					
$\mathbb{1}$ {**start school at 7**}	-0.0587*	-0.073	0.1318***	-0.0580**	0.0257	0.0323
	[0.032]	[0.051]	[0.045]	[0.028]	[0.044]	[0.042]
$yearsCC$	0.019	-0.0688*	0.0498	0.0007	-0.0724*	0.0717
	[0.024]	[0.036]	[0.039]	[0.028]	[0.042]	[0.045]
N	100,918	100,918	100,918	92,888	92,888	92,888
Hansen J-statistics:	*0.106*	*0.016*	*0.012*	*0.41*	*0.668*	*0.145*
	***female** students*					
$\mathbb{1}$ {**start school at 7**}	-0.0127	0.0357	-0.023	-0.0275*	-0.0228	0.0503
	[0.035]	[0.067]	[0.064]	[0.016]	[0.033]	[0.032]
$yearsCC$	-0.0069	-0.0235	0.0304	-0.008	-0.0543	0.0623
	[0.017]	[0.033]	[0.033]	[0.019]	[0.041]	[0.041]
N	107,475	107,475	107,475	98,066	98,066	98,066
Hansen J-statistics:	*2.61*	*0.097*	*0.443*	*0.007*	*2.055*	*2.338*

C.5 Robustness Check: Accounting for Relative Age Effects

I now investigate whether the positive impacts of redshirting on boys' test scores are robust to accounting for within-class relative age effects. In what follows, I horse-race (absolute) school-starting age D_i and (relative) age rank in class RR_i by instrumenting with birth month and the share of summer-born children in class. Dominating absolute age effects would suggest that redshirting matters, because it boosts boys' human capital, rather than simply making them older relative to their classmates.

First, I construct each child's relative rank (RR) within the class, using date of birth and unique school and class identifiers for all students. In contrast to the data in the reviewed literature, the Hungarian administrative data used in this paper has information about all students' birth month and school-starting age in a given class.[13] Thus, the child's relative age rank within their class is known. Thus, crucially for this data creation step, there is no sample selection based on test score availability or childcare attendance. To create RR, I have data on more than 95 percent of all children. RR is measured in percentiles, where the oldest child in the class receives a value of 100, and children born in the same month have the same value. To instrument for RR, I construct the share of summer-born children in each class ($peers_summer$) similarly.

Then, I augment (2) and also include the child's relative age rank in her class as a regressor:

$$Y_i = \pi_0 + \pi_1 \mathbb{1}\{\text{start school at } 7\}_i + \pi_2 RR_i + \pi_3 X_i + \pi_4 C_i + \sum_{t=1}^{T-1} \tau_{1t} F_t + \varsigma_i, \tag{4}$$

where RR_i is the child's relative age rank in her class (in percent), and all other variables are defined as before; notably, $\mathbb{1}\{\text{start school at } 7\}$ takes the value of 1 if the child started school at the age of 7, as opposed to 6. Out of the coefficients of interests, π_1 represents the absolute age effect, i.e. the impact of entering school a year older, by comparing two similar children who also have the same position in the age distribution in their class; π_2 represents the relative age effect, i.e., it shows the impact of moving up in the class age distribution by 1 percentile, by comparing two similar children from the same cohort who started school at the same age.

First, one might wonder where variation in within-class RR comes from, conditional on $\mathbb{1}\{\text{start school at } 7\}$ (or, in general, absolute age in months), and how large this variation may be. The variation stems from the fact that classes are relatively small, and children in different classes face different age distributions of their classmates (i.e., there is natural variation in classmates' age). Holding the variance of the class age distribution fixed, a student with a given absolute age will have a lower RR in a class with a higher mean absolute age. However, holding the mean, minimum, and maximum absolute age of classes fixed, a student with a given absolute age (and thereby also a constant relative-to-the-class-mean age) will have different ranks (RR) in classes with different variance in absolute age. Note that the same idea is applied by Murphy and Weinhardt (2020), except for test score ranks. Table C10 shows for two select years—2010 and 2014—that there is still substantial variation in within-class RR, even for children of the same age and school-starting age in Grade 6.

In (4), both $\mathbb{1}\{\text{start school at } 7\}$ and RR may be endogenous. Besides $1\{\text{birth-month} \geq x_d\}$, the two additional instrumental variables are the share of summer-born children in the class and its interaction with $1\{\text{birth-month} \geq x_d\}$. The intuition is as follows: Summer-born children start school at age 7 by law and, in the absence of many redshirted peers, are typically the oldest in the class. That is, having a larger share of summer-born children decreases the relative age rank of any summer-born child within their class, *ceteris paribus*.

The share of summer-born children is a valid instrument if it is only related to student achievement (say, test score) through the endogenous variables, $\mathbb{1}\{\text{start school at } 7\}$ and RR. As opposed to the child's relative age rank, the propensity to start school at age 7 is less likely to be related to the share of summer-born children. Nevertheless, this share violates the exclusion restriction if schools sort children non-randomly into classes based on student achievement. Since summer-born children generally have higher scores, classes with more high-achieving students would likely have an over-representation of summer-born children. To identify absolute and relative age effects, and to respect the exclusion restriction, it is essential to restrict the sample to children who study in schools that do not sort children systematically into classes based on student achievement.

To separate non-sorting schools from sorting schools, I follow a slightly modified method of Horvath (2015). For each school, I test whether classroom assignment is related to student achievement. I run an *F-test* on the joint significance of classroom effects within each school on mathematics test score (with year fixed effects). The H_0 of this test corresponds to "no sorting," while the H_a corresponds to "sorting."[14] I classify "sorting"

[13] The class identifiers do not have a meaning across years, and thus cannot be used for estimating class fixed effects.

[14] The intuition behind these tests is that they relate the within-class sum-of-squares of test scores to the between-class sum-of-squares. A sufficiently large within-class variation is indicative of heterogeneous student composition within

schools as those with *p-values* of less than 0.15, a more conservative requirement than the usual 0.05 level.

Table C8 shows the results from first-stage regressions on the endogenous $\mathbb{1}\{\text{start school at 7}\}$ and RR variables. Being born between January–March, as opposed to October–December, significantly increases children's likelihood of starting school at an older absolute age of 7, but not their relative age rank in the class. The share of summer-born peers in the class is not strongly related to SSA, as expected, but is significantly related to (relative) class age rank. For fall-born boys (girls), a 1 percentage point increase in the share of summer-born classmates leads to a 0.27 (0.33) percentage point decrease in RR, *ceteris paribus*. For children born between January–March, a 1 percentage point increase in the share of summer-born classmates leads to a 0.06 percentage point increase in RR. Regarding the strength of the IVs, the *joint F-statistic* is 85-100 for D and 140-210 for RR, and the values of both of the *Cragg-Donald Wald F-statistics* and the *Kleibergen-Papp F-statistics* are higher than 80. Thus, weak identification is not a concern (Stock et al., 2002; Olea and Pflueger, 2013; Lee et al., 2022).

Table C9 shows the second-stage / IV results for the effect of $\mathbb{1}\{\text{start school at 7}\}$ and RR on Grade 6 test scores,[15] and reveals that RR has no additional causal effect on test scores above and beyond $\mathbb{1}\{\text{start school at 7}\}$. Boys who enter primary school at age 7 are predicted to have higher test scores than children who enter at age 6 by 0.32-0.4 SDs and have the same ranking in the class' age distribution. The values of the *Hansen J-statistic* are below 5, thus the null hypothesis of valid (excludable) instruments cannot be rejected, even at the 1% significance level. The result implies that absolute age effects dominate within-class relative age effects for boys. This result is consistent with Cascio and Schanzenbach (2016), which finds that holding absolute age constant, children who were relatively young in their class performed no worse in school, and with Cook and Kang (2020), which finds that children's absolute age, rather than age relative to classmates, plays the dominant role in parents' decision to redshirt.[16]

Table C8: ***Effect of Quarter of Birth and the Share of Summer-Born Classmates* – First-Stage Results on Starting School at Age 7 and Relative Rank, Administrative Data – Grade 6, By Gender**

cutoff x_d : January 1	**boys**		**girls**	
	$\mathbb{1}\{\text{start school at 7}\}$	$RR(\%)$	$\mathbb{1}\{\text{start school at 7}\}$	$RR(\%)$
$1\{\textit{birth-month} \geq x_d\}$	0.1285***	2.2311***	0.0948***	-0.5874
	[0.0128]	[0.8596]	[0.0105]	[0.7707]
$peers\_summer(\%)$	-0.0001	-0.2679***	-0.0003	-0.3263***
	[0.0003]	[0.0188]	[0.0002]	[0.0162]
$1\{X \geq x_d\} \times$	0.00002	0.2090***	0.0002	0.2514***
$peers\_summer(\%)$	[0.0004]	[0.0290]	[0.0004]	[0.0260]
R^2	0.176	0.225	0.145	0.264
N	58,233	58,233	61,283	61,283
joint F-statistic:	*103.54*	*142.74*	*84.64*	*210.56*
Cragg-Donald Wald F-statistic:	*98.68*		*82.932*	
Kleibergen-Paap rk Wald F-statistic:	*98.44*		*82.031*	

Notes: [1] $1\{\text{birth-month} \geq x_d\}$ is a binary variable denoting quarter of birth of child i (1: child was born on/after January 1, 0: child was born before January 1). $peers\_summer_i$ denotes the fraction of summer-born children in child i's class (measured in percentages). Control variables include: linear trend in month of birth and its interaction with a binary variable denoting quarter of birth, cohort fixed effects, and family background variables listed in Section 4, and missing response characteristics controls.
[2] The sample includes children born in the three-months window around the cutoff dates of January 1. The sample excludes students who study in a "sorting school" based on prior student achievement, as defined in Section 4.
[3] Standard errors are in brackets, and are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 6) 2008-2017.

classes, which implies classes are roughly similar to each other, so that "no sorting" cannot be rejected.

[15] Results for higher grades and other student achievement outcomes are similar, and are available upon request.

[16] My results are less comparable with that of Bedard and Dhuey (2006), Dhuey and Lipscomb (2008), and Dhuey and Lipscomb (2010), as they do not horse-race school-starting age and relative age rank in the same way that I do.

Table C9: ***Effect of Starting School at Age 7 Due to Academic Redshirting and Relative Age Rank in Class* – LATE Estimates on 6^{th}-Grade Standardized Test Scores, Admin. Data – Grade 6, By Gender**

cutoff x_d : January 1	**boys**				**girls**			
Y **– std. test score:**	**math**		**reading**		**math**		**reading**	
$\mathbb{1}$ {start school at 7}		0.4071**		0.3190*		0.081		0.2018
		[0.2068]		[0.1902]		[0.1976]		[0.1854]
$RR(\%)$	0.0044**	-0.0026	0.0046**	-0.001	-0.0002	-0.0016	0.0008	-0.0028
	[0.0022]	[0.0028]	[0.0021]	[0.0026]	[0.0026]	[0.0023]	[0.0025]	[0.0020]
R^2	0.197	0.202	0.227	0.235	0.207	0.207	0.276	0.276
N	58,224	58,224	58,226	58,226	61,258	61,258	61,279	61,279
Hansen J-statistic:		*3.873*		*0.6440*		*2.9510*		*0.3380*

Notes: [1] Control variables include: linear trend in month of birth and its interaction with a binary variable denoting quarter of birth, cohort fixed effects, and family background variables listed in Section 4, and missing response characteristics controls.
[2] The sample includes children born in the three-months window around the cutoff dates of January 1. The sample excludes students who study in a "sorting school" based on prior student achievement, as defined in Section 7.
[3] Standard errors are in brackets, and are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 6) 2008-2017.

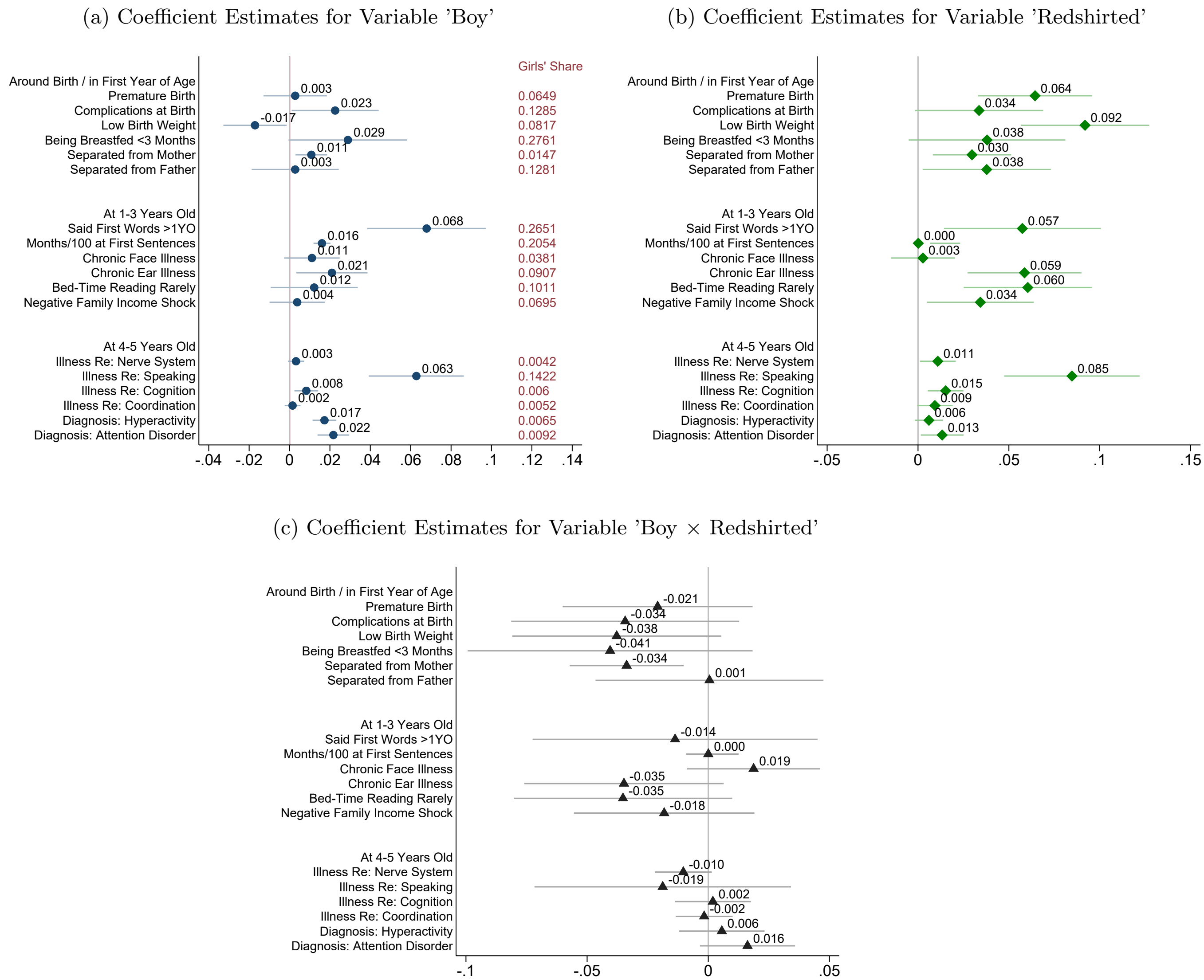
Table C10: **Distribution of Relative Rank Variable (RR), by Age in Months and Starting School at Age 7 *versus* Starting School at Age 6, in 2010 and 2014, Administrative Data – Grade 6**

	2010						**2014**					
	$\mathbb{1}$ {start school at 7} = 1			$\mathbb{1}$ {start school at 7} = 0			$\mathbb{1}$ {start school at 7} = 1			$\mathbb{1}$ {start school at 7} = 0		
age	***min***	***max***	***sd***	***min***	***max***	***sd***	***min***	***max***	***sd***	***min***	***max***	***sd***
12y0m				0.51	36.36	3.99				0.63	38.46	3.88
12y1m				1.11	52.94	5.50				0.98	40.00	5.68
12y2m				1.39	47.06	6.74				1.56	80.00	6.99
12y3m				1.09	62.50	8.01				1.89	100	7.93
12y4m				2.38	75.00	9.15				3.03	100	9.07
12y5m				3.33	73.08	10.24				3.23	69.23	10.04
12y6m				3.85	100	11.03				4.00	80.00	10.87
12y7m				5.88	81.82	11.38				4.55	82.35	11.49
12y8m				5.88	92.86	11.93				4.35	90.00	12.09
12y9m	6.25	100	12.41	19.35	77.78	17.34	5.56	100	12.67	30.95	66.67	10.01
12y10m	7.14	100	12.55	15.00	88.00	16.07	5.56	100	12.74	30.77	82.14	12.44
12y11m	6.67	100	12.42	37.50	93.33	14.45	9.09	100	12.76	42.86	90.91	12.09
13y0m	12.50	100	12.43	22.73	94.44	14.32	7.14	100	12.26	18.75	100	14.12
13y1m	16.67	100	12.02				5.26	100	11.76			
13y2m	16.67	100	11.16				10.53	100	11.74			
13y3m	14.29	100	11.66				15.38	100	11.16			
13y4m	8.33	100	11.37				15.79	100	10.79			
13y5m	10.00	100	11.82				33.33	100	10.45			
13y6m	27.78	100	11.50				20.00	100	10.98			
13y7m	37.50	100	10.27				21.05	100	10.43			
13y8m	27.27	100	11.22				28.57	100	9.98			
13y9m	7.69	100	13.07				20.00	100	11.79			
13y10m	14.29	100	12.08				38.89	100	9.14			
13y11m	50.00	100	9.99				13.33	100	11.02			

Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 6) 2008-2017.

D Further Figures

Figure D1: **Factors in Early Childhood Associated with Being ‘Redshirted’ and Gender – Survey Data**



Notes: [1] This figure shows the estimated coefficients and their standard errors for a given outcome Y, regressed on *Boy*, $\mathbb{1}\{\text{start school at 7}\}$ (equivalent to 'Redshirted'), and their interaction 'Boy × Redshirted', without any control variables. In Panel (a), the coefficient estimate can be interpreted as the difference in a given outcome between non-redshirted boys and non-redshirted girls (where the non-redshirted girls' share can be on the right). In Panel (b), the coefficient estimate can be interpreted as the difference in a given outcome between redshirted girls and non-redshirted girls. In Panel (c), the coefficient estimate can be interpreted as the boy-girl difference in the difference in a given outcome between redshirted and non-redshirted students.
[2] Standard errors are clustered at the school-level, *** p<0.01, ** p<0.05, * p<0.1.
[3] The sample includes children born between October and March (around the January 1 ‘redshirting’ cutoff).
Source of data: Survey data – Hungarian Life Course Survey.

E Further Tables

E.1 Average Characteristics of the Compliers by Gender

Table E1: ***Start School at 7 Due to Academic Redshirting (cutoff: January 1) and School Start at 7 Due to Enrollment Cutoff Date (June 1),*** **Average Characteristics of Compliers, By Gender, Survey Data**

	(1)	(2)	(3)	(4)	(5)
	boys		***girls***		
Panel A: cutoff x_d : January 1	**share among**		**share among**		**p-value △**
for Academic Redshirting	**compliers**	**sample**	**compliers**	**sample**	**(1)-(3)**
age in months	181.56	176.35	181.10	174.98	0.2443
birth happened prematurely	0.0777	0.0794	0.1029	0.0809	0.3412
birth happened w/ complications	0.1420	0.1511	0.1456	0.1362	0.8986
born with low birth weight	0.0858	0.0802	0.1127	0.1029	0.3162
was breastfed for <3 months	0.2840	0.3001	0.3066	0.2840	0.5219
separated from mother (<1y old)	0.0224	0.0242	0.0434	0.0221	0.1818
separated from father (<1y old)	0.1494	0.1409	0.1606	0.1384	0.7014
said first words >1y old	0.3882	0.3460	0.3280	0.2788	0.0822*
age (months): said first sentences	23.87	22.65	22.14	20.87	0.0117**
chronic face illness (<3y old)	0.0768	0.0567	0.0500	0.0385	0.1055
chronic asthma (<3y old)	0.0427	0.0652	0.0509	0.0373	0.6101
⊖ income shock to family (<3y old)	0.0969	0.0808	0.0936	0.0784	0.8861
problems w/ nerv.sys. (4-5y old)	0.0085	0.0076	0.0172	0.0061	0.2281
problems w/ speaking (4-5y old)	0.2248	0.2209	0.2112	0.1563	0.6848
problems w/ cognition (4-5y old)	0.0209	0.0205	0.0233	0.0097	0.805
problems w/ coordination (4-5y old)	0.0078	0.0091	0.0188	0.0076	0.1735
hyperactivity (4-5y old)	0.0348	0.0286	0.0106	0.0076	0.0252**
attention disorder (4-5y old)	0.0656	0.0413	0.0241	0.0128	0.0008***
	boys		***girls***		
Panel B: cutoff x_d : June 1	**share among**		**share among**		**p-value △**
School Enrollment Cutoff	**compliers**	**sample**	**compliers**	**sample**	**(1)-(3)**
age in months	160.72	174.32	165.07	173.08	0.000***
birth happened prematurely	0.0607	0.0792	0.0747	0.0884	0.273
birth happened w/ complications	0.1394	0.1463	0.1352	0.1381	0.973
born with low birth weight	0.0382	0.0769	0.0430	0.0938	0.1523
was breastfed for <3 months	0.2547	0.2831	0.2667	0.2812	0.5452
separated from mother (<1y old)	0.0065	0.0240	0.0004	0.0203	0.5834
separated from father (<1y old)	0.1495	0.1496	0.1115	0.1335	0.3952
said first words >1y old	0.2580	0.3320	0.2492	0.2863	0.7939
age (months): said first sentences	20.96	22.57	19.97	20.88	0.4747
chronic asthma (<3y old)	0.0717	0.0594	0.0275	0.0354	0.0327**
⊖ income shock to family (<3y old)	0.0833	0.0823	0.0662	0.0812	0.7035
problems w/ nerv.sys. (4-5y old)	0.0020	0.0062	0.0009	0.0064	0.6185
problems w/ speaking (4-5y old)	0.1727	0.2116	0.1016	0.1449	0.0645*
problems w/ cognition (4-5y old)	0.0148	0.0202	0.0035	0.0108	0.4525
problems w/ coordination (4-5y old)	0.0055	0.0087	-0.0022	0.0070	0.6901
hyperactivity (4-5y old)	0.0203	0.0311	0.0058	0.0087	0.2785
attention disorder (4-5y old)	0.0032	0.0430	0.0032	0.0125	0.6849

E.2 LATE Estimates on Student Achievement in Primary School and on Intermediate Outcomes in Secondary School

Table E2: ***Effect of Starting School at Age 7 Due to Academic Redshirting and Due to the Enrollment Cutoff Date*** – **LATE Estimates on 6^{th}-Grade and 8^{th}-Grade Standardized Mathematics and Reading Test Scores, Administrative Data – Grades 6 and 8, For All and By Gender**

outcome:	***School Start at 7 Due to***		***School Start at 7 Due to***	
Y: std. mathematics score	***Academic Redshirting***		***Enrollment Cutoff Date***	
	cutoff x_d : January 1		*cutoff x_d : June 1*	
in grades:	**6**	**8**	**6**	**8**
Panel A: all students				
$\mathbb{1}$ {**start school at 7**}	0.2124***	0.1448***	0.2520***	0.1482***
	[0.051]	[0.054]	[0.039]	[0.033]
R^2	0.2057	0.2226	0.2203	0.2330
N	357,158	341,803	375,163	356,517
Panel B: by gender				
	male *students*			
$\mathbb{1}$ {**start school at 7**}	0.3015***	0.1433**	0.1934**	0.1225*
	[0.0691]	[0.0686]	[0.0757]	[0.0659]
R^2	0.1984	0.2277	0.2303	0.2385
N	175,507	165,447	184,955	173,727
	female *students*			
$\mathbb{1}$ {**start school at 7**}	0.103	0.1408*	0.2836***	0.1612***
	[0.0784]	[0.0823]	[0.0418]	[0.0366]
R^2	0.2102	0.2170	0.2098	0.2258
N	181,651	176,356	190,208	182,790
outcome:	***School Start at 7 Due to***		***School Start at 7 Due to***	
Y: std. reading score	***Academic Redshirting***		***Enrollment Cutoff Date***	
	cutoff x_d : January 1		*cutoff x_d : June 1*	
in grades:	**6**	**8**	**6**	**8**
Panel C: all students				
$\mathbb{1}$ {**start school at 7**}	0.2043***	0.1302**	0.2750***	0.2397***
	[0.050]	[0.051]	[0.036]	[0.034]
R^2	0.2700	0.2853	0.2830	0.2872
N	357,230	341,881	375,223	356,619
Panel D: by gender				
	male *students*			
$\mathbb{1}$ {**start school at 7**}	0.2906***	0.2167***	0.2731***	0.2691***
	[0.0659]	[0.0658]	[0.0732]	[0.0661]
R^2	0.2317	0.2426	0.2575	0.2531
N	175,527	165,474	184,991	173,769
	female *students*			
$\mathbb{1}$ {**start school at 7**}	0.0937	0.0203	0.2729***	0.2207***
	[0.0770]	[0.0767]	[0.0386]	[0.0355]
R^2	0.2852	0.3025	0.2881	0.2976
N	181,703	176,407	190,232	182,850

Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grades 6/8) 2008-2017.

Table E3: ***Effect of Starting School at Age 7 Due to Academic Redshirting and Due to the Enrollment Cutoff Date*** **- LATE Estimates on Grade Repetition by a Given Grade, Administrative Data, Grades 6 and 8, For All and By Gender**

outcome: $Y = 1\{$***having repeated a grade***$\}$	***School Start at 7 Due to Academic Redshirting***		***School Start at 7 Due to Enrollment Cutoff Date***	
	cutoff x_d *: January 1*		*cutoff* x_d *: June 1*	
by grades	**6**	**8**	**6**	**8**
Panel A: all students				
$\mathbb{1}\{$**start school at 7**$\}$	-0.0299***	-0.0144	-0.0306***	-0.0360***
	[0.011]	[0.012]	[0.009]	[0.008]
R^2	0.0826	0.0753	0.0907	0.0754
N	357,280	341,964	375,276	356,691
control complier mean of Y	*0.04383*	*0.0482*	*0.0394*	*0.0456*
Panel B: by gender				
	male *students*			
$\mathbb{1}\{$**start school at 7**$\}$	-0.0512***	-0.0209	-0.0310*	-0.0540***
	[0.0160]	[0.0166]	[0.0182]	[0.0165]
R^2	0.0834	0.0795	0.1041	0.0838
N	175,557	165,514	185,021	173,806
control complier mean of Y	*0.0501*	*0.0584*	*0.0485*	*0.0581*
	female *students*			
$\mathbb{1}\{$**start school at 7**$\}$	-0.0044	-0.0056	-0.0309***	-0.0267***
	[0.0163]	[0.0175]	[0.0088]	[0.0082]
R^2	0.0784	0.0692	0.0860	0.0744
N	181,723	176,450	190,255	182,885
control complier mean of Y	*0.0334*	*0.0335*	*0.0318*	*0.0364*

Notes: [1] This table shows the estimated coefficients and standard errors of the 2^{nd}-stage equation of Y on $\mathbb{1}\{\text{start school at 7}\}$ and control variable, where $\mathbb{1}\{\text{start school at 7}\}$ is a binary variable denoting age at primary school entry (1: child entered primary school at age 7, 0: child entered primary school at age 6). Control variables include: linear trend in month of birth and its interaction with a binary variable denoting quarter of birth, cohort fixed effects, and family background variables listed in Section 4, and missing response characteristics controls. The corresponding regression equation is equation (2).
[2] The sample includes children born in the three-months window around the cutoff dates of January 1 and June 1, respectively, for starting school at age 7 due to academic redshirting and due to the school enrollment cutoff date.
[3] Standard errors are in brackets, and are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.
[4] "Control complier mean of Y" pertains to the estimated mean of Y_0 (the potential outcome of Y without the treatment), for children in the various *complier* subgroups, who were born in the three-months window around the cutoff dates of January 1 and June 1, respectively, estimated using the equation (A2) in Abdulkadiroglu et al. (2018).
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grades 6/8) 2008-2017.

Table E4: ***Effect of Starting School at Age 7 Due to Academic Redshirting and Due to the Enrollment Cutoff Date*** – **LATE Estimates on Secondary School Track and Aspirations, Administrative Data**

outcome: $Y = 1\{$**being in track** $j\}$ $(j = low, middle, high)$	***School Start at 7 Due to Academic Redshirting*** *cutoff* x_d *: January 1*			***School Start at 7 Due to Enrollment Cutoff Date*** *cutoff* x_d *: June 1*		
tracks	*low*	*middle*	*high*	*low*	*middle*	*high*
Panel A: all students						
$\mathbb{1}\{$**start school at 7**$\}$	-0.0732***	0.0168	0.0564*	-0.0576***	-0.0202	0.0778***
	[0.023]	[0.032]	[0.029]	[0.012]	[0.018]	[0.017]
R^2	0.2285	0.0601	0.2337	0.2089	0.0506	0.1880
N	334,767	334,767	334,767	343,272	343,272	343,272
control complier mean of Y	*0.2043*	*0.4084*	*0.3871*	*0.1628*	*0.4116*	*0.4255*
Panel B: by gender						
	male *students*					
$\mathbb{1}\{$**start school at 7**$\}$	-0.1193***	0.0126	0.1067***	-0.1021***	0.0223	0.0797***
	[0.0306]	[0.0403]	[0.0365]	[0.0214]	[0.0303]	[0.0273]
R^2	0.2168	0.0558	0.2077	0.2336	0.0584	0.2202
N	161,565	161,565	161,565	166,778	166,778	166,778
control complier mean of Y	*0.2320*	*0.4289*	*0.3390*	*0.2170*	*0.4471*	*0.3358*
	female *students*					
$\mathbb{1}\{$**start school at 7**$\}$	-0.0111	0.0207	-0.0096	-0.0363***	-0.0508***	0.0871***
	[0.0344]	[0.0516]	[0.0480]	[0.0125]	[0.0196]	[0.0185]
R^2	0.2256	0.0711	0.2283	0.2271	0.0715	0.2247865
N	173,202	173,202	173,202	176,494	176,494	176494
control complier mean of Y	*0.1629*	*0.3745*	*0.4625*	*0.1229*	*0.3865*	*0.4904*
outcome: $Y = 1\{$**aspiration** $j\}$ $(j =< HS, HS, HS+)$	***School Start at 7 Due to Academic Redshirting*** *cutoff* x_d *: January 1*			***School Start at 7 Due to Enrollment Cutoff Date*** *cutoff* x_d *: June 1*		
aspiration	$< HS$	HS	$HS+$	$< HS$	HS	$HS+$
Panel C: all students						
$\mathbb{1}\{$**start school at 7**$\}$	-0.0258	-0.036	0.0618**	-0.0380***	-0.0442***	0.0822***
	[0.019]	[0.031]	[0.029]	[0.009]	[0.016]	[0.015]
R^2	0.1835	0.1134	0.2812	0.1830	0.1159	0.2815
N	329,424	329,424	329,424	337,665	337,665	337,665
control complier mean of Y	*0.1135*	*0.3556*	*0.5277*	*0.0842*	*0.3477*	*0.5699*
Panel D: by gender						
	male *students*					
$\mathbb{1}\{$**start school at 7**$\}$	-0.0524**	-0.0819**	0.1343***	-0.0623***	-0.038	0.1003***
	[0.0261]	[0.0396]	[0.0362]	[0.0197]	[0.0305]	[0.0277]
R^2	0.1914	0.0840	0.2544	0.1944	0.0891	0.2635
N	158,710	158,710	158,710	163,816	163,816	163,816
control complier mean of Y	*0.1359*	*0.3765*	*0.4875*	*0.2148*	*0.4484*	*0.3366*
	female *students*					
$\mathbb{1}\{$**start school at 7**$\}$	0.0101	0.023	-0.0331	-0.0237***	-0.0462***	0.0699***
	[0.0255]	[0.0479]	[0.0464]	[0.0085]	[0.0176]	[0.0173]
R^2	0.1593	0.1507	0.2858	0.1537	0.1536	0.2825
N	170,714	170,714	170,714	173,849	173,849	173,849
control complier mean of Y	*0.0803*	*0.3273*	*0.5922*	*0.1216*	*0.3870*	*0.4913*

Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 10) 2008-2017.

Table E5: ***Effect of Starting School at Age 7 Due to Academic Redshirting and Due to the Enrollment Cutoff Date*** – **LATE Estimates on Obtaining a Vocational Degree or a High School Degree**

School Start at 7 Due to:	***Academic Redshirting***			***Enrollment Cutoff Date***		
	cutoff x_d *: January 1*			*cutoff* x_d *: June 1*		
outcome Y***:***	$1\{dropout\}$	$1\{VOCdegr\}$	$1\{HSdegr\}$	$1\{dropout\}$	$1\{VOCdegr\}$	$1\{HSdegr\}$
Panel A: all students						
$\mathbb{1}$ **{start school at 7}**	-0.0053	-0.0674*	0.0874**	0.0094	-0.0204	0.0343**
	[0.0317]	[0.0360]	[0.0401]	[0.0144]	[0.0157]	[0.0174]
N	142,508	142,508	142,508	163,387	163,387	163,387
control complier mean of Y	*0.0989*	*0.1213*	*0.7951*	*0.0910*	*0.1180*	*0.8041*
Panel B: by gender						
	male *students*					
$\mathbb{1}$ **{start school at 7}**	-0.0268	-0.0864*	0.1109**	0.0209	-0.0176	0.0219
	[0.0422]	[0.0492]	[0.0540]	[0.0291]	[0.0334]	[0.0361]
N	68,876	68,876	68,876	79,233	79,233	79,233
control complier mean of Y	*0.1064*	*0.1577*	*0.7489*	*0.1020*	*0.1686*	*0.7418*
	female *students*					
$\mathbb{1}$ **{start school at 7}**	0.0202	-0.0431	0.0562	0.0053	-0.0211	0.0383**
	[0.0473]	[0.0496]	[0.0576]	[0.0154]	[0.0155]	[0.0178]
N	73,632	73,632	73,632	84,154	84,154	84,154
control complier mean of Y	*0.0925*	*0.0903*	*0.8343*	*0.0834*	*0.0831*	*0.8085*

Notes: [1] This table shows the estimated coefficients and standard errors of the 2^{nd}-stage equation of Y on $\mathbb{1}$ {start school at 7} and control variable, where $\mathbb{1}$ {start school at 7} is a binary variable denoting age at primary school entry (1: child entered primary school at age 7, 0: child entered primary school at age 6). Control variables include: linear trend in month of birth and its interaction with a binary variable denoting quarter of birth, cohort fixed effects, and family background variables listed in Section 4, and missing response characteristics controls. The corresponding regression equation is equation (2).
[2] The sample includes children born in the three-months window around the cutoff dates of January 1 and June 1, respectively, for starting school at age 7 due to academic redshirting and due to the school enrollment cutoff date.
[3] In columns (1) and (4), the outcome is a binary variable, indicating dropping out from high school without a vocational or high school degree; in columns (2) and (5), it is a binary variable, indicating graduating from high school with a vocational degree; in columns (3) and (6), it is a binary variable, indicating graduating from high school with a high school degree.
[4] Standard errors are in brackets, and are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.
[5] "Control complier mean of Y" pertains to the estimated mean of Y_0 (the potential outcome of Y without the treatment), for children in the various *complier* subgroups, who were born in the three-months window around the cutoff dates of January 1 and June 1, respectively, estimated using the equation (A2) in Abdulkadiroglu et al. (2018).
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grades 6,8,10) 2008-2014, Linked to Administrative Labor Data.