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

Tímea Laura Molnár
Central European University*

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Abstract

I study voluntary delays in the school entry of age-eligible children ("academic redshirting"), using Hungarian administrative data on test scores and medical prescriptions, and survey data on mental health, for 2008–2017. I identify a new Local Average Treatment Effect (LATE) of higher school-starting age (SSA), specifically due to academic redshirting. I exploit an administrative barrier—a school-readiness evaluation—compulsory for potentially redshirted fall-born children. By instrumenting for births after January 1, my new LATE estimate of redshirting is primarily for *non-school-ready* children—for whom waiting an extra year before school could help overcome developmental deficits, but who were deterred from redshirting by the administrative barrier. In contrast, the existing LATE estimates in the deep SSA literature are mainly for *school-ready* children—who start school a year older just because they were born after the school enrollment cutoff date (in Hungary June 1). I find that (i) redshirted children tend to achieve higher test scores in Grades 6–10 by 0.1–0.2 s.d., are less likely to fail a grade, aspire for higher levels of education, and are more likely to attend academic high school tracks and actually graduate from high school, but (ii) these positive redshirting effects are only present for *boys*. I present new evidence that boys fall behind already at ages 1–4 in their speaking, hyperactivity and attention abilities, are less likely to be school-ready, and that redshirted boys are less anxious, more confident and less prone to being bullied. Thus, academic redshirting can shrink the education gender gap, by helping boys overcome non-school-readiness and improve student achievement and mental health.

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1 Introduction

A non-trivial and potentially increasing share of parents are postponing the entrance of their age-eligible children into primary education, a practice known as "academic redshirting."[1] 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.[2] The share of redshirted children is around 5-10 percent among US kindergartners (Aud et al., 2013; Bassok and Reardon, 2013; Stipek, 2002),[3] with boys and high-status children typically 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). The share of redshirted children remains at around 5-10 percent even in European countries, where the prescribed school-starting age (SSA) is 6 for all.[4,5] In Hungary, where the prescribed SSA is 6 or 7 depending on birth month, the share of redshirted children increases gradually from 6 percent to 70 percent for student birth months September to May, respectively—thereby rendering Hungary an ideal environment to study redshirting, of which the net causal effect is *ex-ante* ambiguous.

Academic redshirting specifically refers to parents' voluntary practice of choosing to delay a child's entry into school, particularly for children born before the school enrollment cutoff date; these parents consciously opt *not to comply with the school entry rule*. Despite its importance for parents and policy-makers, no previous research has identified the causal effect of redshirting itself; i.e., none has identified the impact of higher SSA due to the voluntarily delayed school entry of age-eligible children. Previous deep research on SSA almost exclusively uses the school enrollment cutoff date for identification, and thus identifies the Local Average Treatment Effect (LATE) of higher SSA due to being born after and complying with the school enrollment cutoff date. Not only is this compliance inherently different from the practice of redshirting, but also pertains to different *compliers*: who start school a year older just because they were born after the school enrollment cutoff date, but, who would *not be redshirted*, had they been born before (if redshirting is an option for young children). Schanzenbach and Larson

[1] Redshirting is "*named for the red jersey worn in intra-team scrimmages by college athletes kept out of competition for a year*." ("Youngest Kid, Smartest Kid?" by Maria Konnikova (The New Yorker, September 19, 2013)). Also *see* "When Should a Kid Start Kindergarten?" by Elizabeth Weil (The New York Times, June 3, 2007).

[2] According to Deming and Dynarski (2008), the school attendance rate of 6-year-olds has not decreased, but they are more likely to be enrolled in kindergarten rather than first grade. However, only 25 percent of the aforementioned change can be explained by changes in school entry laws that increased kindergarten entrance ages. Since 1965, there has been a trend of moving school enrollment cutoff dates from September-February to June-September in the US (Cannon and Lipscomb, 2008). Note that several states do not have a uniform cutoff date for all school districts (but delegate it to the discretion of Local Education Agencies) and some states do not even regulate kindergarten entry age.

[3] In 2010 in the US, 6 percent of kindergartners entered kindergarten before they turned 5, 85 percent started when they were 5–6 years old, and 9 percent after they turned 6—out of which two-thirds were delayed-entry kindergartners based on school system age requirements ("Kindergarten Entry Status: On-Time, Delayed-Entry, and Repeating Kindergartners," U.S. Department of Education, National Center for Education Statistics, Early Childhood Longitudinal Study, Kindergarten Class of 2010-11 (ECLS-K:2011), Preliminary Restricted-Use Data File; available here).

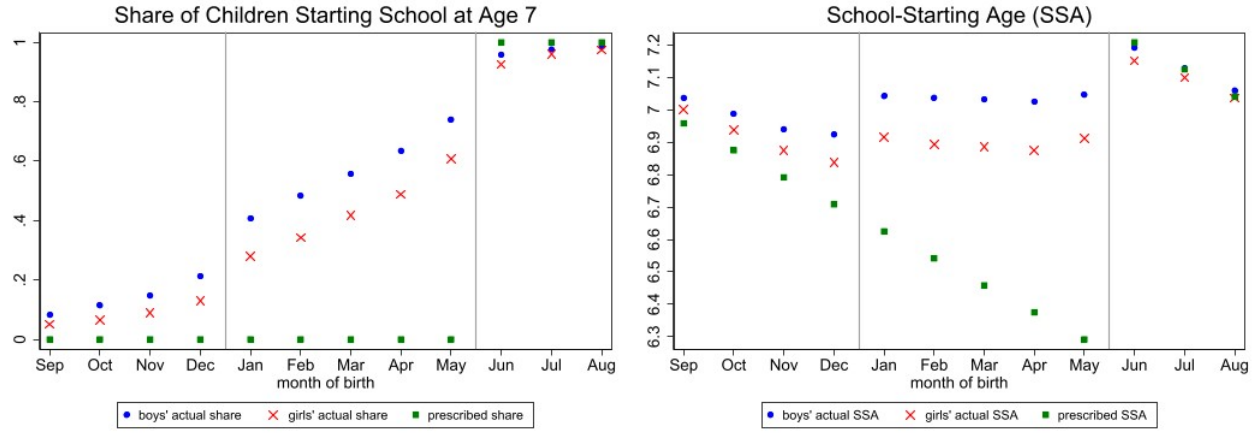
[4] The share of 6-year-old girls and boys who had not yet started (ISCED2011 level 1) primary education in 2010–2018 was, respectively, 6 and 12 percentages in Denmark and Belgium, 7 and 9 in Luxembourg, 6 and 10 in Slovenia, 4 and 5 in Greece, 3 and 4 in Spain, 2 and 2 in Italy, 0.5 and 1.5 in France, and zero in Ireland and the Netherlands (OECD Education Statistics, Education at a glance: Enrollment rate by age (2020 Edition); available here. There may be changing trends in redshirting, as well, given that Denmark's 2007 figures were 11 and 21 percent (Gortz et al., 2018).

[5] European countries also differ in parents' decision-making role in the redshirting process. In countries where, in addition to age, school-readiness is an admission criterion for starting school, parents are not the key decision-makers; the main determinant of SSA is the assessment of either the childcare institution (as in Estonia, Hungary, Liechtenstein, and Luxembourg) or the primary school (as in Austria, Bosnia-Herzegovina, Croatia, the Netherlands, and Serbia) (Key Data on Early Childhood Education and Care (ECEC), 2019, Eurydice Report, EC, pp. 105–107; available here).

(2017) even points out that "*[n]o one has conducted a true randomized trial related to redshirting.*"[6]

In this paper I come close to the ideal experiment for obtaining random variation in higher SSA due to academic redshirting. I exploit a feature of the Hungarian education system, in which, for children born on one side of an arbitrary cutoff date, redshirting is easy and costless, while for children born on the other side, redshirting is possible but more costly. In Hungary before 2014, children born between September and May were allowed to be redshirted; however, those potentially redshirted children born before January 1 had to take a school-readiness evaluation, while those born on or after January 1 were exempt from such a test. Notably, this school-readiness cutoff date of January 1 differs from the Hungarian school enrollment cutoff date of June 1, and induces exogenous variation in children's propensity of being redshirted that is arguably independent of parental perceptions and preferences.

These figures summarize how month of birth and actual and prescribed SSA are related in Hungary for 6^{th}-graders in 2008–2017. September-to-May-born children, prescribed to start to school at age 6, are allowed to be redshirted, and 7-to-70 percent of them indeed start school at age 7 (redshirted).



As boys are more likely to be redshirted, I add to the extensive literature on 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), on the extent to which the policy of redshirting may mitigate gender inequalities, and help boys to not fall behind already in the early stages of their educational trajectory.[7]

My empirical contribution is the novel identification of the LATE of higher SSA that is specifically due to academic redshirting, and providing its first causal estimates. I use an indicator for child birthdays that fall on or after January 1 as an Instrumental Variable (IV) in a Fuzzy Regression Discontinuity Design (RDD). I identify the causal effect of redshirting from the discontinuous jumps at the January 1 cutoff, in the share of redshirted children (in the first-stage relationship), and in student outcomes, such as academic achievement and mental well-being (in the reduced-form relationship).

To illustrate the idea behind the identification strategy, consider the following comparison for one selected outcome: at the January 1 cutoff, the share of redshirted boys jumps discretely by 19

[6]They continue that "*[i]nstead, researchers have sought out opportunities to isolate the effects on student outcomes of the two variables that by definition change when a student is redshirted: age itself and the student's age relative to those of classroom peers," and* then cite Elder and Lubotsky (2009) and Cascio and Schanzenbach (2016); the former use cross-state variation in cutoff dates for kindergarten entry age, and the latter use the experiment of Tennessee's Project STAR (Student–Teacher Achievement Ratio), in which kindergarten-aged children were randomly assigned to classrooms, resulting in different within-class relative age for two children with the exact same birthdate.

[7]Academic redshirting of boys is also the policy recommendation made in the book Reeves (2022).

percentage points, and their average reading test score (measured in standard deviations) jumps by 3.1 percentage points. Intuitively, the estimate for the LATE of higher SSA due to redshirting is the ratio of the reduced-form and first-stage jumps (3.1 and 19, respectively) for the compliers—i.e., for boys who are *not redshirted* if born *before* January 1 (in which case the compulsory school-readiness evaluation makes redshirting costly), but who *would be redshirted* had they been born *on or after* the cutoff (in which case redshirting is relatively *costless*, as no school-readiness evaluation is required).

My conceptual contribution is to provide insights into how differences in the *complier* population around the January 1 and June 1 cutoffs determine what one can learn from the SSA effect identified from each cutoff, for parents' and teachers' potential redshirting decisions. In principle, both (1) higher SSA due to academic redshirting (as in this paper, using the January 1 cutoff), and (2) higher SSA due to being born after and complying with the enrollment cutoff date (as in the existing literature) are conceptually similar: both lead to a higher SSA and being older in every grade thereafter. At the same time, the complier groups at these cutoffs differ in their underlying school-readiness. Thus, the compliers at the unique cutoff of January 1 used in this paper delivers insights about redshirting that is different from what we learn from the typical SSA literature that uses the school enrollment cutoff.

There are two groups of compliers around (1) January 1: (i) *non-school-ready* children, for whom an extra year before school could help boost their human capital, and overcome any behavioral or developmental deficits, but who are deterred from redshirting by the administrative barrier, and (ii) *school-ready* children, who would not be advised to be redshirted by developmental psychologists conducting the school-readiness evaluation. In contrast, in a setting like the Hungarian one, in which parents of children born before the school enrollment cutoff date have the option to redshirt, compliers around (2) June 1 are all *school-ready* children.[8] That is, those who start school a year older do so solely because they were born after the school enrollment cutoff date and, had they been born before the cutoff, would *not have been redshirted*. Furthermore, given that children born just before the enrollment cutoff are typically the youngest in their cohort, they are more likely to be non-school-ready, in which case parents typically would want to redshirt them. Ultimately, the compliers around June 1 are indeed the school-ready ones, and I provide direct empirical evidence for this assertion.

My main result is that higher SSA due to academic redshirting has a positive effect on student achievement only for *boys*, while higher SSA due to the school enrollment cutoff date has a positive effect for both boys and girls. Thus, if parents, teachers, or policy-makers were to base academic redshirting decisions on the evidence stemming from the existing SSA enrollment cutoff literature alone, girls may be subjected to unnecessary and sub-optimal educational delays.

Regarding mechanisms, I present new evidence that boys fall behind already at ages 1–4 in their speaking, hyperactivity and attention abilities, and are less likely to be school-ready. Thus, redshirting seems to help boys to overcome non-school-readiness. It also gives them time to work on their anxiety and confidence, which makes them less prone to being bullied and have better mental health overall.

Section 2 reviews previous literature on SSA. While previous literature on SSA is deep, direct evidence on the LATE of academic redshirting may be more relevant for parents (who are considering redshirting their children, given their birthdate and developmental state around the age of 6) and

[8] In principle, there might also be parents who never want to redshirt, even if their child was non-school-ready and born before the school enrollment cutoff date (say, in May). However, given in Hungary an overwhelming fraction (74 and 60 percent) of May-born boys and girls are redshirted, I assert that the share of such parents is very low.

policy-makers (who may need to deliberate on regulating parents' opportunities for, and institutions' roles in, academic redshirting), rather than the LATE of higher SSA from being born after and complying with the school enrollment cutoff date. The reason is that the child's exact birthdate is difficult to manipulate, and once the child is born, academic redshirting is the parents' only remaining option to have them start school a year later. Thus, educational policy may want to regulate parents' options to influence their children's SSA, using the direct evidence on the causal impact of redshirting.

Section 3 describes the Hungarian ECEC institutional setting, with a focus on the school-readiness evaluation, compulsory for potentially redshirted children born before the January 1 evaluation cutoff.

Section 4 describes the data sources and 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. I link administrative labor records to the test scores, to see actual graduation outcomes in from high school, and I link administrative health records to test scores, to look at prescriptions for psycholeptics, a medication for anxiety disorders. Using Hungarian survey data for 2006, I look at self-reported mental health in Grade 8, measured by anxiety and confidence about themselves, as well as experiences of being bullied in class.

Section 5 discusses the identification of the LATE of higher SSA due to academic redshirting. In my identification strategy, I examine the relationship between SSA and various student achievement and mental health outcomes of children born on either side of the January 1 cutoff, controlling for a linear (spline) trend in birth month around the cutoff, as well as a rich set of background characteristics. I also provide two pieces of reassuring evidence for identification: (i) neither the fraction of boys, nor the share of children of various backgrounds change discontinuously around the January 1 birth cutoff in the administrative data, and (ii) even though the propensity of being redshirted is related to adverse events in early childhood (such as a shock at birth, a family shock during early childhood, and chronic illnesses or developmental difficulties in the first years), the fraction of children experiencing such adverse events is not changing discontinuously at the January 1 birth cutoff in the survey data.

Section 5 presents direct evidence that the compliers are, on average, less likely to be school-ready around the redshirting cutoff of January 1, than around the school enrollment cutoff of June 1. For example, compliers around January 1 are significantly more likely to have (i) been born with low birth weight, (ii) been separated from their mother before their 1^{st} birthday, (iii) started speaking at an older age, and (iv) had problems with speaking, cognition, coordination, and attention at ages 4–5.

Section 6 presents the LATE estimates of higher SSA due to academic redshirting: (i) redshirted children generally attain higher test scores by 0.1–0.2 standard deviations, are less likely to fail a grade, aspire for more ambitious educational paths, and are more (less) likely to enrol in academic (vocational) high school tracks; but (ii) these positive redshirting effects are only present for *boys*. In comparison, the LATE estimates I obtain using the school enrollment (June 1) cutoff date for mathematics (reading) test scores from Grades 6, 8, and 10 are 0.24, 0.13, and 0.13 (0.28, 0.23, and 0.18) standard deviations, respectively, which are in line with existing literature (e.g., with Puhani and Weber (2007), McEwan and Shapiro (2008), or Elder and Lubotsky (2009)). In this exercise where higher SSA is due to the enrollment cutoff date, both boys and girls have significantly positive LATE estimates, which is in stark contrast with the LATE estimates of higher SSA from redshirting.

Section 7 investigates the underlying mechanisms. First, my unique dataset containing the universe of students in a cohort, with class identifiers, and birthdate information allows me to construct a precise measure of each student's within-class relative age rank. I am able to compare the absolute and relative age effects of higher SSA due to redshirting, by exploiting natural variation in the share of summer-born children in each class. I find that the positive effects of redshirting on boys' Grade 6 test scores are driven solely by absolute age effects. Thus, academic redshirting seems to help boys by boosting not only their human capital to overcome non-school-readiness but also their maturity in class, instead of simply making them older relative to their classmates. Second, I find that mental health is a key mechanism, but again only for boys: redshirted boys are less anxious, more self-confident, and less prone to being bullied in class for their appearance or their poor student achievement.

As boys are more likely to be redshirted and the redshirting effect is positive only for boys, academic redshirting, as a policy, has the potential to mitigate the education gender gap—which is even more important, given my finding of the gender gap in speaking and hyperactivity-inattention to open up already at ages 1–4. My results on mental health are in line with findings of boys benefiting more from environments that improve socio-emotional skills (Walton and Cohen, 2011; Sisk et al., 2018).

2 Literature Review

Previous research on school-starting age (SSA) overwhelmingly uses school (usually kindergarten) enrollment cutoff dates or entrance ages for identification, and thus identifies the impact of starting school a year later simply due to being born after and complying with the age cutoff.

In terms of the research design, existing literature typically uses variation in children's SSA that stems from different birthdates and age cutoffs for prescribed school entry, potentially by geography. For example, Elder and Lubotsky (2009) exploits variation in prescribed kindergarten entry age (KEA) stemming from two sources: the distribution of birthdates among those who comply with their state's enrollment cutoff date, and differences across states in cutoff dates among children born on the same day (but in different states). Similarly, Barua and Lang (2016) instruments actual KEA (6 *versus* 5) with a binary variable indicating whether the child was required by state law to delay kindergarten entry (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 SSA and prescribed SSA, the latter of which depends on birth month and state. Attar and Cohen-Zada (2018) exploits variation in cutoff dates 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.

Existing research focuses primarily on the effect of SSA on student achievement, measured by test scores up to Grade 8, with additional evidence on 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—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, and Smith (2009) for Canada (British Columbia). The range of the impact on test scores is between 0.25 and 0.4 SDs, with generally decreasing impacts across Grades 4–8 and slightly larger effects for disadvantaged children. For example, Datar (2006) finds that entering kindergarten at an older age is more effective for at-risk children.[9] 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).[10] Gorlitz et al. (2022) finds that higher SSA increases competencies in receptive vocabulary.[11]

Non-cognitive and behavioral measures have also been found to be related to SSA (due to the school enrollment cutoff date). For example, Black et al. (2011) presents evidence from Norway that children who start school at a younger age are more likely to experience teenage pregnancy but have a short-run advantage in earnings (although this disappears by age 30). Using US administrative data, 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, while Landerso et al. (2017) finds that a higher SSA lowers the propensity to commit crime at young ages, and the number of crimes committed by boys. Evans et al. (2010) shows that older children have a significantly lower incidence of Attention Deficit Hyperactivity Disorder (ADHD) diagnosis and treatment, and Muhlenweg et al. (2012) presents evidence from Germany that 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 inattention/hyperactivity which is more prevalent for boys.[12] Using Danish mental health data, Dee and Sievertsen (2015) finds that starting school a year older reduces inattention/hyperactivity at age 7, and the effect persists to age 11. Lubotsky and Kaestner (2016), in contrast, does not find that growth in non-cognitive measures, such as internalizing behavior or self-control, were related to kindergarten entry age.

Regarding adult outcomes and SSA, the evidence is mixed. Using data on the entire population of

[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, they find very large effects for student achievement in Grade 4 (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. They show that entering primary school one year older increases Grade 4 test scores by 0.29 SDs in mathematics and 0.38 SDs in language. 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 *versus* August) is around 0.2 SDs, irrespective of family background, birth weight, or school quality; moreover, the authors find 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 by 0.34 SDs and in mathematics by 0.19 SDs; by Grade 8, the effect on mathematics test scores almost doubles.

[10] The authors find that higher SSA decreases (increases) the chance of repeating Grade 1 (in the most advanced school track) by 2 (12) percentage points, while early school entrants are significantly less likely to enter Gymnasium.

[11] 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.

[12] Exploiting US state-level variation in school enrollment cutoff dates, McAdams (2016) uses a Difference-in-Difference identification strategy, and finds that starting school older reduces the incidence of incarceration.

Norway, Black et al. (2011) finds a small positive effect of starting school earlier on IQ scores at age 18, and a short-run positive effect on earnings that disappears by age 30. Using data from California and Texas, Dobkin and Ferreira (2010) finds no evidence that SSA would affect job market outcomes, such as wages or the probability of employment. For Sweden, Fredriksson and Ockert (2014) finds that school-starting age affects educational attainment and the timing of labor supply but not prime-age earnings (except in the case of disadvantaged students). Du et al. (2012) finds suggestive evidence that relative age has a long-lasting influence on career, given the disproportionately small share of summer-born CEOs. Arnold and Depew (2018) finds that higher school-starting ages for boys is linked to better health in adulthood, but with no long-term labor market impacts.

Within the literature on SSA, relative age (RA) effects have been found to be positively related to educational success. For example, 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 enjoy the associated wage premium), and Schneeweis and Zweimuller (2014) shows that they choose more ambitious secondary tracks. However, Aliprantis (2014) finds that returns to RA may be negative for the oldest students.[13]

Existing literature on relative age effects does not definitely answer the question of whether these effects remain important beyond school-starting age. This is an important consideration for parents (who need to decide whether to redshirt their children or be concerned about whether other parents' redshirting behavior would harm 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).

3 Institutional Background

Hungary has a universal, free, childcare system, in which more than 90 percent of children spend at least 3 years in childcare before primary school.[14] 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)). Then, academic redshirting is 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.

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 required to start primary school on September 1 of that particular calendar year (Public Education Act 6(1)). Thus, the Hungarian school enrollment cutoff

[13] Indirectly related to educational success, Johansen (2021) finds that the youngest girls in class are more likely to experience abortion, alcohol poisoning in adolescence, earlier motherhood, and contraceptive use, and Ballatore et al. (2020) finds that being among the youngest increases the probability of being victimized (bullied).

[14] For more institutional details, see Appendix A.

date is June 1. Then, if complying with the prescribed SSA, (i) children born between January and May start primary school at an average age of 6 years 7.5 months to 6 years 3.5 months, respectively; (ii) children born between June and August start primary school at an average age of 7 years 2.5 months to 7 years 0.5 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 May-born children being the youngest, and June-born children being the oldest in a classroom.

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 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.[15]

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

Month of Birth:	**Predicted 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

Table 1 summarizes primary school entry in Hungary, by month of birth, as they pertained to before 2018. *Regime1* pertains to children born in the fall term (September–December): these children are required to enter primary school at age 6, and the administrative barrier to academic redshirting is present for them. Specifically, they can be redshirted only if the parents request an additional year in childcare, have their children take the compulsory school-readiness evaluation, and both the childcare institution's board of teachers and the local DAB support the parents' request.[16] *Regime2* pertains to children born in the spring term (January–May): these children are required to enter primary school at age 6, and there is no administrative barrier to redshirting for them. Specifically, they can be redshirted if the parents request an additional year in childcare, and the childcare institution's board of teachers supports the parents' request[17]—the local DAB's evaluation or support is not needed in this case. *Regime3* pertains to children born in the summer term (June–August): these children are already required to enter primary school at age 7, and thus redshirting is not allowed for them.

The school-readiness examination conducted by local DABs is standardized and free of charge. Developmental experts first examine how the child behaves in social settings with other children (typically, during drawing exercises, they investigate how children are able to work while being distracted,

[15] While, under exceptional circumstances, it is possible for a child to start school a year earlier than prescribed, this is rare: roughly 0.3 (0.7) percent of children born January–May (September–December) start school at age 5. 94, 97, and 98 percent of those born in June, July, and August, respectively, start school at age 7 (as prescribed).

[16] Hungarian Public Education Act, 24(5).

[17] 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 AY, but who are not school-ready based on the opinion of the board of childcare teachers.

how much they are influenced by others, able to adapt, and able to cope with new situations). Then, in a one-on-one situation, the developmental experts assess the extent of children's awareness about their environment and themselves, as well as their cognitive and recall abilities.

For insight into the costs of a school-readiness examination from the perspective of the parents, I conducted interviews with experts of local DABs 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 DAB (for context, there are 4-5 local DABs in each of Hungary's 20 counties). Second, parents might be afraid of their children potentially receiving a poor or problematic evaluation, the documentation of which parents may presume to harm their children's educational career later. Third, parents might be reluctant to go in front of an unknown formal committee, especially if the default option is to discuss their children's development and school entry with the childcare teachers, with whom the parents are already familiar. But, since childcare is free in Hungary, there are no other direct costs of redshirting.

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 of redshirting a child born before January 1, due to the compulsory school-readiness evaluation. I use this source of exogenous institutional variation at the January 1 cutoff to identify the LATE of higher SSA due to academic redshirting specifically.[18]

4 Data, Sample, and Measurement

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

I use (1) administrative data from the *Hungarian National Assessment of Basic Competences* (HNABC), spanning the years 2008–2017, for Grades 6, 8, and 10, which includes student test scores in mathematics and reading, grade repetition, secondary school track choice, educational aspirations, birthdate, childcare attendance, and various background characteristics; (2) survey data from the *Hungarian Life Course Survey* (HLCS) on self-reported mental health indicators, such as anxiety and mental exhaustion, confidence and experiences of being bullied in class, and early childhood events for 8^{th}-graders in 2006; and (3) administrative data (for a 50 percent random sample of (1)), spanning 2009–2017, on medical prescriptions for psycholeptics, which primarily treat anxiety disorders.[19]

[18] Besides date of birth, childcare-starting age (CSA) also affects SSA, although to a smaller extent. Children born September–December are more likely to enter childcare early at age 2, as these children are preferred by the childcare institutions if there is a waitlist (Hungarian Public Education Act 24(1)): childcare institutions can admit 2-year-old children, provided that all the 3-year-old and older applicants living in the municipality are admitted. This being said, 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 (with very few exceptions), children born January–May are most likely to enter childcare at age 3, while those born July–August are most likely to enter at age 4. As a result, those born September–December who entered childcare at age 2 will have already spent 4 years in childcare when the possibility of redshirting becomes relevant. Tables G1 and G2 in Appendix G show that my main estimates are robust to—and more conservative—accounting for the potentially endogenous nature of childcare-starting age (CSA).

[19] The administrative data used in this paper are the property of the *National Health Insurance Fund Administration*, the *Central Administration of National Pension Insurance*, the *National Tax and Customs Administration*, the *National Employment Service*, and the *Educational Authority of Hungary*. The data were processed and provided by the *Databank of the Centre for Economic and Regional Studies in Hungary*.

The HNABC does not assess students' factual knowledge of the compulsory curriculum. Instead, the test seeks to measure the extent to which students are able to apply their acquired skills in realistic settings, and the extent to which they possess the necessary competences for further development. The tests are low-stake, the completion of the tests are mandatory for all children, almost all students' tests are centrally processed, and the results count toward each school's average.[20]

I obtain student background variables, year of birth, month of birth, time spent in childcare, from the HNABC student background survey. This survey provides detailed information about the students' demographic and family background, but completion of the survey was non-compulsory. I restrict the analysis to children for whom information about gender, their time spent in childcare, year of birth, and month of birth is available and who have at least one valid test score.[21]

The HLCS is an individual panel survey conducted annually. The original 2006 sample (10,022 respondents) is a selected 10 percent random sample from the population of 8^{th}-graders 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 the years 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 private provider, by codes of "Anatomical Therapeutic Chemical" (ATC).

4.2 Measurement of Outcomes, Endogenous Variables, and Controls

With respect to student achievement outcomes, I measure:
— *student achievement* by standardized test scores which have mean 0 and standard deviation of 1, by the interactions of grade (6/8/10), year (2008-2017), and subject (mathematics/reading);
— *grade repetition* by a binary variable indicating repeating at least one grade by Grades 6, 8, or 10;
— *secondary school track choice* by binary variables indicating whether the child 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;

[20] Exceptions are made for students with special educational needs or autistic students. While students who suffer from behavioral problems or dyslexia/dysgraphia/dyscalculia are required to complete the test sheet, their results are not processed centrally. Children who suffered a temporary injury that rendered them physically unable to complete the test were exempted, as were those who missed class on the day of the test.

[21] The final sample contains 80-83 percent of the original sample (see Table B1). 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, in Hungarian). While there is no information available about non-response and birth date, this analysis suggests that students with a privileged family background and better student achievement may be over-represented in my sample (it finds that among students with no valid test scores, those with lower parental education are overrepresented, and that students with valid test scores obtained a systematically higher grade from mathematics the year before). Given that only the effect of higher SSA due to the June 1 school enrollment cutoff date (but not that of higher SSA due to redshirting) is found to be slightly higher for disadvantaged children (as will be presented in Sections 6.1 and 6.2), the estimated effects shown in this paper are presumably a lower bound, and the true impacts are even higher.

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* by indicators of whether the child aspires to earn a high school diploma or more advanced post-secondary degree(s).

With respect to the mental health and student well-being outcomes, I measure:
— *standardized anxiety* by the (standardized) mean score from students' responses to how often they experience the following indicators of anxiety (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 during the night, (viii) being dizzy, (ix) being exhausted, (x) feeling nausea and vomiting;
— *often anxious* by a binary variable indicating whether the student experienced any of the above (i)–(x) conditions daily or several times per week;
— *lack of confidence* by a binary variable indicating whether the student agrees with any of the following statements: (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) "*Sometimes I feel I am not good at anything.*";
— *bullied* by a binary variable for being bullied in class for appearance or poor 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).

With respect to background variables, the background survey of the HNABC does not contain information on whether the child was evaluated by the school-readiness committee or whether concerns were expressed about their school-readiness around the age of 6. The exact day of birth is also not available, which would be needed for the implementation of an RDD. I construct SSA from birth date, time spent in childcare, and grade repetition as follows. First, the child's age at the time of the test, in absence of exact birth dates, can be estimated by assuming that all children were born on the 15^{th} of their respective birth month and that the achievement test was taken on May 15^{th}, as follows:

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

Second, SSA can be backed out with age-at-the-test and subtracting time spent between primary school entry and the Grade 6 test. Assuming that primary school starts on September 1 in all years, students who did not repeat any grade spent 68.5 months between primary school entry and the Grade 6 test (and the same logic follows for Grades 8 and 10). Similarly, with repetition of Grades 1, 2, and 3, the time between primary school entry and Grade 6 becomes 80.5, 92.5, and 104.5 months. I verify that SSA is consistent with information on childcare-starting age and years in childcare.

I measure parental education with four categories: at most primary education, obtained a vocational degree, a secondary school degree, or a tertiary degree. I label children as 'disadvantaged' or 'low-status' if their parents obtained at most a vocational degree, but not a high school degree. The child background control variables are indicators about the family's financial situation (e.g., whether

the child is considered to be disadvantaged or excessively disadvantaged by law (meaning being eligible for child protection support, or, having, in addition, parents who completed at most Grade 8 (Act No. LXXIX of 1993 on Public Education (121(1)), whether the child considers their family to be poor, whether the child is eligible for free meals and/or free books at school, whether the child has their own desk at home), the composition of the household (e.g., members of the household, whether the child lives together with a biological father and mother *versus* a stepfather and stepmother), cultural goods at home (internet, books), and the characteristics of the municipality in which the family lives (region and type of settlement, such as living in a village or in a town).

For disentangling absolute and relative age effects, 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 Grade 6. Thus, crucially for this data creation step, there is no sample selection based on test score availability or childcare attendance. To create the child's relative rank within the class, I have data on more than 95 percent of all children. Relative rank 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 similarly.

5 Identification

Now I discuss identification, and compare my research design with those in the existing literature.

5.1 Identification of the LATE of Higher SSA due to (1) Academic Redshirting (January 1) and (2) the School Enrollment Cutoff Date (June 1)

In this subsection, I first discuss the identification of the Local Average Treatment Effect (LATE) of higher SSA due to (1) academic redshirting, using the January 1 cutoff, and (2) being born after and complying with the enrollment cutoff date of June 1. In my main identification strategy (1), I use being born on or after January 1 as an Instrumental Variable (IV), to compare SSA and various academic achievements and mental health outcomes of children born on either side of the January 1 cutoff in a 3-month window, controlling for a linear (spline) trend in birth month around the cutoff, as well as a rich set of background characteristics. Next, I present the estimation equations, and I discuss the IV validity requirements.[22] Finally, I present the raw first-stage relationship in the data, discuss who the likely compliers are in my research design, and present their average characteristics.

5.1.1 The Identification Challenge

Identification of the effect of starting school at age 7 (instead of age 6) is difficult due to the fact that, in the Hungarian educational system, as in many others, primary SSA 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. As a consequence, Ordinary Least Squares (OLS) estimates of the effect of SSA on student achievement may be biased.

[22] Section 7 discusses how I separately identify (3) absolute and within-class relative age effects behind redshirting.

Unobserved abilities bias the OLS estimates downward. This is because unobserved child abilities around age 6 are negatively correlated with their propensity to start school a year later. Furthermore, all else equal, a child with worse abilities has, on average, worse student achievement.

Unobserved parental preferences, on the other hand, lead to a bias in the OLS estimates of ambiguous sign, as it depends on the characteristics of the parents with a higher valuation of childcare services, who are more likely to delay their children's primary school entry. On the one hand, these parents may be those who try to compensate for insufficient skill-enhancing resources at home, and whose children presumably score lower on achievement tests, *ceteris paribus*—leading to a downward bias. On the other hand, these parents may be those who, besides other early childhood resources, view childcare as equally important and want to provide it in the greatest possible extent, and whose children presumably score higher on achievement tests, *ceteris paribus*—leading to an upward bias.

One option to eliminate the bias stemming from parental preferences is to exploit within-family variation across siblings, as in Dhuey et al. (2019). Unfortunately, family identifiers are unavailable in my data, and thus I use the Wald estimand for fuzzy Regression Discontinuity Design (RDD)/IV.

5.1.2 The Fuzzy RDD/IV Design and the First-Stage & Second-Stage Equations

To overcome the bias in the OLS estimates, I use the two discontinuity cutoffs in birth months, (1) January 1 and (2) June 1, to identify the LATE of higher SSA due to (1) academic redshirting, and (2) the school enrollment cutoff date, respectively. I use the Wald estimand for a fuzzy RDD/IV design that, under the conditions discussed below, captures the LATE on *compliers*, who, by definition, are children whose SSA changes, if 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 discontinuity cutoffs contain children of the same birth month. I use a 3-month window, leading to 3-3 bins, by using data on children born in (1) October–March, and (2) March–August. I control for month of birth linearly, and I allow for this linear trend to have different slopes on the two sides of the cutoffs.

Formally, the Two-stage Least Squares (2SLS) first-stage and second-stage equations are:

$$D_i = \beta_0 + \beta_1 1\{X_i \geq x_d\} + \beta_2 X_i + \beta_3 1\{X_i \geq x_d\} \times X_i + \beta_4 C_i + \sum_{t=1}^{T-1} \mu_{1t} F_t + \eta_i, \tag{1}$$

$$Y_i = \gamma_0 + \gamma_1 D_i + \gamma_2 X_i + \gamma_3 1\{X_i \geq x_d\} \times X_i + \gamma_4 C_i + \sum_{t=1}^{T-1} \omega_{1t} F_t + \upsilon_i, \tag{2}$$

where Y_i is the outcome variable from the set of {test scores, grade repetition, secondary school track choice, aspirations, mental health}; D_i is a binary variable denoting age at primary school entry of child i (1: child entered primary school at age 7, 0: child entered primary school at age 6); X_i is month of birth (linear trend, re-centered at x_d), where discontinuity point x_d is either January 1 or June 1 for academic redshirting or starting school at age 7 due to the school enrollment cutoff date, respectively; $1\{X \geq x_d\}$ is the discontinuity dummy; C_i denotes the vector of control variables; and F_t denotes academic year dummies (defined to start in September and end in August). Only the discontinuity dummy $1\{X_i \geq x_d\}$ is used as an instrument, and is excluded from the second-stage equation (2).

The coefficients of interest are β_1 and γ_1.

I make the following four 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 not sufficient for identification; (ii) I show in Table E1 in Appendix E that my main results for the LATE of 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-side of that cutoff is SSA of 7 by default, and redshirting is not possible; (iv) based on Lee and Card (2008), the ideal level of clustering of standard errors would be at the level of birthdate in my setup, but that would lead to an insufficient number of clusters (only 6). To avoid a downward bias in the standard error estimates due to a small number of clusters, and because I expect the regressors and errors to be correlated across students within the same school, I cluster standard errors at the school level based on Cameron and Miller (2015). Table F1 in Appendix F demonstrates that my chosen level of clustering (at the school level) is most conservative relative to alternative levels of clustering (such as by birth month and cohort).

5.1.3 Discussion of IV Validity

It is implausible that the potential outcomes, and thus the effect of school-starting age, are homogeneous in the population. Therefore, following Angrist and Pischke (2008), the requirements for the instrument $1\{X_i \geq x_d\}$ 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 in detail.

The *first-stage* requires birth month to be systematically and strongly related to school-starting age. Specifically, it is required that children who were born on or just after January 1 and June 1 have significantly higher probability of starting primary school at age 7 (*versus* 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 and opportunities in Hungary, for the following reasons. First, the local DAB's evaluation or support is not needed for redshirting children born on January 1, but the administrative requirement of the local DAB's evaluation is present for a child born on December 31. Second, a child born on June 1 *versus* May 31 is required to enter primary school at age 7 *versus* 6.

Figure 1 shows the raw first-stage relationship. Between children born in December *vs.* January and May *vs.* June, there is a large jump in the share of children with SSA of 7, of 17 and 28 percentage points, respectively; this share is steadily increasing, across birth months September–August.

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; or, to phrase it differently, if innate ability is not uniformly

distributed across the birth months near the cutoffs. Second, it fails if parents with tend to have June babies rather than May babies (i.e., tend to conceive their baby in September rather than August), their child is likely to be among the older students in class. On the distribution of abilities and parental preferences, existing evidence is inconclusive;[23] but, given that I compare children born in two consecutive quarters, with one of the cutoffs being in the middle of the winter season, and the other at the beginning of the summer season, differences between summer- and winter-born children, as suggested by Bound and Jaeger (2001) and Buckles and Hungerman (2013), are less of a concern.

Reassuring evidence supporting a smooth distribution of births around the January 1 cutoff can be seen in Tables B2, B3, and C1. There is no discontinuity for the fraction of boys, or the share of children with low parental education or any other background characteristics, or various developmental obstacles in early childhood, nor is there any bunching on either side of the 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 SSA), 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.[24] 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*, including SSA).

The above concern can be eliminated by comparing two children born just before or just after the cutoff, by shrinking the window around the cutoff 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 (Figure 1), and to identify the impact exclusively from the jump

[23] On the one hand, (i) 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; (ii) 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. On the other hand, there is 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; schizophrenia, intellectual disability, autism, multiple sclerosis, and manic depression are found to vary across quarter of birth, while an association has been found between shyness and month of birth (the evidence on the association between IQ and month of birth is inconclusive); (ii) Buckles and Hungerman (2013) shows differences in maternal characteristics for births throughout the year, documenting that women giving birth in the winter months are younger, less educated, and less likely to be married; they present evidence in favor of the relationship between month of birth and later outcomes being driven by differences in fertility patterns across socio-economic groups, rather than by natural phenomena or schooling laws. Huang et al. (2020) finds that more educated mothers in China are more likely to move their delivery date forward to send their children to school at a younger age in 2014–2016 (from seven days after the school enrollment cutoff date to seven days before).

[24] Similarly, consider the June 1 cutoff and two otherwise identical children, the first born in March, the second in July. Suppose that the first child is redshirted and the second complies to the required school-starting age of 7. Even though the second child had a higher likelihood of starting school at age 7, spending an additional, 4th year in childcare, the first one is still 4.5 months older than the second at the time the test is taken.

in the share of redshirted children at the cutoffs. Thus, I include children born in a 3-month window and control for trends in birth month, allowing for different slopes on each side of the cutoffs. I note that a 1-month window is insufficient to control for linear trends in month of birth.

5.1.4 First-Stage and Reduced-Form Relationships and the Identification's Intuition

The idea behind my identification strategy in a nutshell is shown in Figures 1 and 2. In Figure 1, the raw first-stage relationship between birth month and the share of children starting school at age 7 is shown in the left panel, and the relationship between birth month and (actual and prescribed) SSA is shown in the right panel. The cutoffs of January 1 and June 1 are marked with solid lines. Figure 2 shows the raw reduced-form relationship between month of birth and student outcomes.

In Figure 1, the share of Grade 6 children who start school at age 7 steadily increases over birth months September–August, but there are clear jumps at the cutoffs. At the January 1 cutoff, the share of redshirted boys sharply increases by 19 percentage points (from 21 to 40), and at the June 1 cutoff, the share of boys starting school at age 7 sharply increases by 21 percentage points (from 73 to 94). These jumps at the January 1 and June 1 cutoffs are used as instruments for academic redshirting and starting school at age 7 due to the school enrollment cutoff date, respectively.

Intuitively, the estimate for the LATE of academic redshirting is the reduced-form estimate rescaled by the first-stage estimate (without any control variables, using a 1-month window around January 1). For instance, the share of redshirted Grade 6 boys jumps by 19 percentage points at the January 1 cutoff, and their average reading (mathematics) test score jumps by 3.1 (3.0) percentage points of a standard deviation. Then, 3.1 (3.0) rescaled by 19 yields a raw LATE estimate of 0.16 (0.16) SDs.[25]

5.1.5 Descriptive Statistics and Supporting Descriptive Evidence for IV Validity

In this subsection, I first add new evidence to the literature on academic redshirting, on how the propensity of being redshirted is related to adverse events in early childhood—made possible thanks to the exceptionally rich data on early childhood events in the HLSC dataset. I find that redshirted children are more likely to have suffered such adverse events in early childhood, and are less likely to be school-ready, on average, indicated by their lag in various developmental markers at age 6.

Table 2 shows the shares of boys and girls born between September and May with different early childhood events and developmental obstacles, by redshirting status (SSA). This table reveals four key relationships between adverse events in early childhood and being redshirted: (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 before then, or their parents read to them only very occasionally), while (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

[25] Similarly, the share of (i) grade repeaters by Grade 10, (ii) those who aspire for a $HS+$ (tertiary) degree, and (iii) those who end up in the vocational and academic high school track, increases by -0.6, 0.7, -0.9, and 0.6 percentage points, respectively; then, one arrives at the raw LATE estimate by dividing by 19.

childhood (such as sinusitis at ages 1-3), and/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 or coordination problems) or diagnosis of attention deficit hyperactivity disorder (ADHD) are also significantly associated with the propensity of being redshirted at age 6. The length of having been breastfed is negatively related to being redshirted, yet not significantly at the usual levels.

Table 2 also reveals important gender differences in 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 illnesses before turning age 3.

Table C1 shows the estimated coefficient on the discontinuity dummy $1\{X_i \geq x_d\}$ from model (1) on gender, parental education, family background variables listed in Section 4.2, and early childhood shocks presented in Table 2. None of the $\hat{\beta}_1$'s differ from 0, providing reassuring support for 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.

5.1.6 Monotonicity, LATE, and Compliers with Their Average Characteristics

For the IV estimator to produce the LATE, the *monotonicity* requirement also has to be satisfied. Monotonicity requires that, while the instrument may not influence school-starting age 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), they would definitely be redshirted if they were born on or after January 1 (in absence of this barrier). Given that redshirting under the administrative barrier is more costly, this assumption is a non-controversial one.[26]

Given *relevance, independence, exclusion,* and *monotonicity,* the Wald estimand for fuzzy RDD/IV has the interpretation of the effect of starting school at age 7 (*versus* 6), for the *compliers*, whose treatment status is influenced by the change in the instrument's value around the discontinuity cutoffs.

Around the January 1 cutoff, which is relevant for redshirting, the first group of complier children are those who are *not school-ready* and who 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 local DAB's evaluation, as well. For such parents, it is too costly to search for, travel to, and let the child take the school-readiness examination, either because the time costs are too high for them, or such parents are less informed and/or distrust the educational system. The second

[26] 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, equivalently, it cannot be possible for a child born before June 1 to delay school entry from age 6 to 7, but is brought forward from 7 to 6 when being born after. A stream of literature, e.g., Aliprantis (2012) and Barua and Lang (2016), argues that, due to redshirting parents, monotonicity around the enrollment cutoff date could be violated. Given that this paper's focus is on academic redshirting that uses January 1 for identification, for which the monotonicity assumption is not controversial, I acknowledge the aforementioned concerns, and note that they only pertain to identifying the effect of higher SSA due to the enrollment cutoff date.

group of complier children are *school-ready* and would be redshirted if only the childcare teachers' approval was needed; at the same time, these children would not be advised to be redshirted by DAB developmental psychologists conducting the school-readiness evaluation.

Around the school enrollment cutoff of June 1, the compliers are *school-ready* children who enter primary school as required 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. (There might also be parents who never want to redshirt, even if their child was born before the school enrollment cutoff date (say, in May) and was non-school-ready. However, given in Hungary an overwhelming fraction (74 and 60 percent) of May-born boys and girls 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 in only 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:

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

with j=1,2 corresponding to the January 1 and June 1 cutoffs, respectively. In addition, let us denote $D^j_{Z_j}$ as the value that D^j would take if Z_j were either 1 or 0. Then, compliers are those with $D^j_1 = 1$ and $D^j_0 = 0$. Let us denote their share π_{C_j}. Then, their observable characteristics can be written as

$$E\left(X|D^j_1 = 1, D^j_0 = 0\right) = \frac{\pi_{C_j} + \pi_{A_j}}{\pi_{C_j}} \left[E\left(X|D^j = 1, Z_j = 1\right) - \frac{\pi_{A_j}}{\pi_{C_j} + \pi_{A_j}} E\left(X|D^j = 1, Z_j = 0\right) \right], \tag{3}$$

where π_{A_j} is the share of always-takers (for whom $D^j_1 = 1$ and $D^j_0 = 1$) and $\pi_{C_j} = 1 - \pi_{A_j} - \pi_{N_j}$, where π_{N_j} is the share of never-takers (for whom $D^j_1 = 0$ and $D^j_0 = 0$). Defiers (for whom $D^j_1 = 0$ and $D^j_0 = 1$) are assumed away by the monotonicity assumption. Using the independence between Z_j and D^j, $\pi_{A_j} = Prob(D^j_0 = 1)$ and $\pi_{N_j} = Prob(D^j_1 = 0)$.

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 cutoff (for redshirting) are indeed less school-ready than compliers around the June 1 cutoff (the school enrollment cutoff date). Compliers' share around the January 1 cutoff are significantly higher, precisely in the dimensions that have been found to be systematically related to redshirting and indicative of non-school-readiness. Specifically, compliers around January 1 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 developmental problems at ages 4-5 (e.g., attention/cognition/verbal disorders or coordination problems) or been diagnosed with an attention disorder.

5.2 Relationship to Existing Literature and Interpretation of LATE Estimates

This paper's identification strategy is most similar to the identification strategy used in Puhani and Weber (2007) and Altwicker-Hamori and Kollo (2012). To overcome the endogeneity of primary school-starting age, they use theoretical or predicted school entry age as an IV, that 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 instrument is of the same form as mine around the cutoff date of June 1.[27] Compared to their approach, I additionally control for the trend in birth month, allowing for different slopes on the two side of the discontinuity cutoffs, to account for different trends.

Having said this, it is important to be transparent of 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, the Hungarian institutional setup is not suitable for disentangling the effect of SSA from the effect of age-at-the-test, since all children are tested on the same date in a given grade. Using Norwegian data, Black et al. (2011) disentangles these by using scores from IQ tests taken outside of school and exploiting the variation in the mapping between birthdate and the year-of-the-test.

Second, I do not compare a redshirted child's test scores with that 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, is thus endogenous, and I do not control for grade repetition, but look at it as an outcome itself. Hence, the effect of SSA I 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.

Third, I do not attempt to disentangle age-at-the-test effects from the impacts of time spent in childcare or 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 United Kingdom. The setup in Carlsson et al. (2015) is closest to the ideal natural experiment to disentangle age-at-the-test effects from impacts of time spent in school. The authors exploit conditionally random variation in the assigned test date for IQ tests of 18-year-old Swedish men in preparation for military service. In their setup, both age-at-the-test and number of days spent in school vary randomly after holding date of birth, parish, and expected graduation date fixed.[28]

This paper, however, makes a contribution in measuring the causal impact of academic redshirting specifically, mostly for *non-school-ready* children, for whom an extra year before school can help boost their human capital and overcome any developmental or behavioral deficits.

[27] Note that Puhani and Weber (2007) does not control for a linear trend in month of birth—they only note that if b_i and s_i are exogenous, the theoretical school entry age is exogenous and can be used as an instrument for the actual age of school entry (Puhani and Weber, 2007, at 367). They discuss factors that would invalidate their instrument, such as parental planning of conception, and acknowledge that month of birth "might exert some direct effect on physical and psychological health" (Puhani and Weber, 2007, at 371).

[28] The effect of longer exposure to kindergarten, without controlling for age-at-the-test, is measured by DeCicca and Smith (2013), which exploits the introduction of the "dual entry" scheme into kindergarten in 1990/1991 in BC, Canada.

6 LATE Estimates

6.1 LATE Estimates of Higher SSA due to Academic Redshirting

6.1.1 First-Stage Results

The left panel of Table 4 shows the first-stage results on starting school a year older due to redshirting, using the administrative test score data. Children born after January 1 are 12 percentage points more likely to start school at age 7, as opposed to 6, compared to children born before the cutoff (the baseline is 11.6 percent in Grade 6). The first-stage relationship is very strong, with no concerns of passing tests of weak instruments (Stock et al., 2002; Olea and Pflueger, 2013; Lee et al., 2022).

The first-stage relationship is stronger for boys in absolute—but not in relative—terms. For example, a boy (girl) in Grade 6 is around 14 (11) 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 5-6 percentage points (among those born October–December), the effect for boys is around 100 percent, while for girls, it is around 130 percent.

There are no significant differences in the estimated first-stage coefficients for disadvantaged and privileged students. However, given disadvantaged students' higher baseline propensity to be redshirted relative to privileged students by 7-9 percentage points (among the fall-born), the relative effect of being born on either side of the cutoff is larger for privileged students.

6.1.2 LATE Estimates for Mathematics and Reading Test Scores

Academic redshirting has a significantly positive impact on test scores, up to Grade 10. The left portion of *Panel A* of Table 5 reveals that redshirted children achieve a significantly higher score in mathematics, on average, by 0.21 and 0.145 SDs, on Grade 6 and 8 testings, respectively, compared to on-time children. The effects, though dropping to 0.12, persist into secondary school. The impacts on the Grade 6, 8, and 10 reading scores are 0.2, 0.13, and 0.09, respectively (Table 6).

The positive redshirting effect on test scores is only present for boys; e.g., boys' effect of redshirting is 0.3, 0.14, and 0.2 SDs, on the Grade 6, 8, and 10 mathematics testings, respectively, compared to on-time children. In contrast, the effect of redshirting for girls is only significant at the 10 percent level on their Grade 8 mathematics test score. For reading, the corresponding effects are 0.29, 0.22, and 0.25 SDs, respectively, for boys, and all point estimates are small and insignificant for girls.

The point estimates for the effect of academic redshirting on test score are larger in primary school, and remain significant in secondary school for mathematics, but only for disadvantaged children.

6.1.3 LATE Estimates for Grade Repetition

Table 7 shows the impact on grade repetition, which captures unsatisfactory school performance in a dichotomous form, and reveals that redshirted children are 3 percentage points less likely than on-time children to repeat a grade by Grade 6. The effect size is large compared to the baseline grade repeaters' share of 3.6 percent, even though there are no detectable effects by Grades 8 and 10.

Similar to test scores, the impact of redshirting on grade repetition is only present for boys; e.g., the impact of redshirting on grade repetition for boys is -5.1 percentage points by Grade 6 and -9.4 percentage points by Grade 10. Again, for girls, all point estimates are much smaller (in absolute terms) and none of them are statistically different from zero.

The effect of academic redshirting does not seem to vary by parental background; e.g., the impact by Grade 6 is significantly negative (-6.75 percentage points) for disadvantaged students, and it is also significantly negative (-5.44 percentage points) by Grade 10 for privileged students.

6.1.4 LATE Estimates for Secondary School Track Choice

Table 8 shows the impact of academic redshirting on secondary school track choice. This crucial intermediate school outcome is measured by a binary variable indicating whether the child attends the j-th track at Grade 10. Recall that track #1 is the lowest track, granting a vocational degree but no high school diploma upon completion, without the option of continuing onto tertiary-level education; track #2 is the middle track, granting both a vocational and high school degree; track #3 is the highest (academic) track, granting a high school degree. Redshirting significantly decreases the propensity of going to the vocational track by 7.3 percentage points (49 percent), and significantly increases the propensity of going to the academic track by 5.6 percentage points (14 percent).

Similar to test scores and grade repetition, the impact of redshirting on secondary school track choice is only present for boys: redshirted boys are significantly more likely to attend the academic track (by 11 percentage points, or 30 percent), and significantly less likely to attend the vocational track (by 11.9 percentage points, or 57 percent), while there is no detectable significant effect for girls.

6.1.5 LATE Estimates for Aspirations for Highest Educational Attainment in Grade 10, and Actual Graduation Propensity From High School

Table 9 displays the impact of redshirting on aspirations for highest level of educational attainment (i.e., less than high school, high school diploma, and more than high school: tertiary degree).

According to *Panel A*, academic redshirting significantly increases the propensity to aspire for a tertiary degree, by 6.2 percentage points (or 11 percent relative to the baseline mean). Similar to other outcomes, the impact of academic redshirting on aspirations is only present for boys. For instance, redshirted boys are significantly less likely to aspire for less than a high school degree (by 5.2 percentage points, or 43 percent), significantly less likely to aspire for a high school degree (by 8.2 percentage points, or 22 percent), and significantly more likely to aspire for a tertiary degree (by 13.4 percentage points, or 26 percent), while there is no detectable effect for girls for any of the outcomes.

Table 10 displays the impact of redshirting on actual graduation from high school. According to *Panel A*, academic redshirting significantly increases the propensity to graduate from high school by 17 percentage points (or 25 percent). The impact of redshirting on actual high school graduation is only present for boys: redshirted boys are significantly more likely to graduate from high school (by 20 percentage points, or 31 percent), while there is no detectable effect for girls for any of the outcomes, leading to academic redshirting more than closing the graduation gender gap.

6.1.6 Placebo and Robustness Checks

Appendix Table D1 shows the 1^{st}-stage and reduced-form estimates on the discontinuity dummy $Z_i = 1\{X_i \geq x_d\}$ (with D_i and various outcomes, respectively), using a 1- or a 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 a different sign at the placebo cutoffs than the reduced-form estimate at the January 1 cutoff.

Then, Appendix Table E1 shows 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 (*versus* a linear trend). When restricting to a 1-month window, the point estimates are somewhat smaller, but the main conclusions still hold. Appendix Table F1 shows that my chosen level of clustering (at the school level) leads to larger, more conservative, standard errors than by clustering at the birth-month level or birth-month×year level, the latter of which leads to 60 clusters.

Finally, Tables G1 and G2 in Appendix G show that my main estimates are robust to—and more conservative than when—accounting for the potential endogenous nature of childcare-starting age (CSA) and including it into the regression (and instrumenting for it).[29]

6.2 LATE Estimates of Higher SSA due to the School Enrollment Cutoff Date

In this subsection, I present the estimated effects of starting school at the age of 7, as opposed to 6, due to being born after and complying with the enrollment cutoff date.

For test scores, the estimates are 0.25, 0.15, and 0.14 SDs in Grades 6, 8, and 10, respectively, for mathematics, and 0.28, 0.24, and 0.18 SDs, respectively, for reading (see the right portion of *Panel A* in Tables 5 and 6)—these estimates are consistent with existing literature, e.g., with Puhani and Weber (2007), Altwicker-Hamori and Kollo (2012), McEwan and Shapiro (2008), and Elder and Lubotsky (2009). Also, children who start school at the age of 7 just because they were born after the enrollment cutoff date are less likely to fail a grade (by 3–6 percentage points across Grades 6–10; see *Panel A* in Table 7), aspire for more ambitious educational paths (4 percentage points less likely to aspire for at most a high school degree, and 8 percentage points more likely to aspire for a tertiary degree; see *Panel A* in Table 9), and are more (less) likely to enrol in academic (vocational) high school tracks (by 6 and 8 percentage points, respectively; see *Panel A* in Table 8).

The LATE estimates of higher SSA due to redshirting (of Section 6.1) are, on average, very similar to the LATE estimates of higher SSA due to the school enrollment cutoff (of Section 6.2). Crucially,

[29] It is important to note that 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 G2 in Appendix G. 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 CSA as a variant and not as the main specification. It turns out that the main specification yields more conservative yet still significantly positive estimates for the impact of redshirting, anyway. My results are in line with those of Szabo-Morvai et al. (2023).

however, there is a key policy-relevant difference regarding which groups of children are driving the positive impacts of higher SSA. The causal effect of academic redshirting is significantly positive only for *boys,* on all outcomes. But, both boys and girls experience significantly positive effects of starting school a year older due to the enrollment cutoff date. Thus, the existing evidence from the enrollment cutoff can be misleading and may lead to suboptimal redshirting decisions, especially for girls.

7 Mechanisms

7.1 Absolute *versus* Relative Within-Class Age Effects Behind Redshirting

7.1.1 Identification of Absolute Age Effects (D_i) vs. Relative Age Effects (RR_i)

To get at the mechanisms, I now investigate whether the positive impacts of redshirting on boys' test scores are driven by absolute or within-class relative age effects. I horse-race (absolute) school-starting age and (relative) age rank in class 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.

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.[30] Thus, the child's relative age rank within their class is known. To separate absolute age effects from relative age effects, I augment (2) and also include the child's relative age rank in her class as a regressor:

$$Y_i = \pi_0 + \pi_1 D_i + \pi_2 RR_i + \pi_3 X_i + \pi_4 C_i + \sum_{t=1}^{T-1} \tau_{1t} F_t + \varsigma_i, \tag{4}$$

where RR_i is the child's relative age rank in her class (in percent), and all other variables are defined as before. The coefficients of interest are π_1 and π_2, where 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 very similar children from the same cohort who started school at the same age (both start at either age 6 or 7).

First, one might wonder where variation in within-class RR comes from, conditional on D (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 B4 in Appendix B shows

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

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.

Both D_i and RR_i may be endogenous. Besides $1\{X_i \geq x_d\}$, the two additional instrumental variables are the share of summer-born children in the class and its interaction with $1\{X_i \geq x_d\}$. 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, D_i and RR_i. 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 test scores, classes with more high-achieving students would very likely have an over-representation of summer-born children. In order 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 null hypothesis of this test corresponds to "no sorting," while the alternative hypothesis corresponds to "sorting."[31] As a last step, I classify "sorting" schools as those with *p-values* of less than 0.15, a much more conservative requirement than the usual 0.05 level.

7.1.2 Results

Table 11 shows the results from first-stage regressions on the endogenous D_i and RR_i 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_i and 140-210 for RR_i, 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 12 shows the IV results for the effect of D_i (corresponding to absolute age) and RR_i (corresponding to relative age) on Grade 6 test scores, and reveals that RR_i has no additional causal

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

effect on test scores, once primary school starts at age 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.[32]

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

Having shown that the positive impacts of redshirting on student outcomes are present only for boys, I now turn to mental health outcomes, to shed light on the underlying mechanisms.

Academic redshirting has a significantly positive impact on reducing boys' anxiety. The left of *Panel A* of Table 13 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. Column (3) conforms these results, using administrative health data: higher school-starting age due to academic redshirting significantly decreases expenditures on prescription psycholeptics—primarily used for treating anxiety—by 4 percent for boys.

Academic redshirting has a significantly positive impact on boys' confidence and a negative impact on being bullied in class. The left of *Panel B* of Table 13 reveals that redshirted boys' propensity to feel unvalued and untalented decreases, and they are significantly less likely to be bullied in class for their appearance or potentially poor academic performance.

These results suggest that mental health is an important mechanism for boys. For boys, extra time before school seems to lead to a boost to human capital, increased maturity, better mental health, increased confidence and student well-being, which are associated with better student achievement.

8 Conclusion

This paper is the first to measure the causal impact of academic redshirting, using administrative test score and medical data, and survey data on mental health. Exploiting an administrative barrier—a school-readiness evaluation—which is compulsory for potentially redshirted fall-born children, I identify a new LATE of higher SSA specifically from redshirting. This LATE captures primarily *non-school-ready* children, whom an extra year before school could help overcome developmental deficits—but who are deterred from redshirting by the barrier. In contrast, the LATE in existing literature of higher SSA due to the school enrollment cutoff date is mainly for *school-ready* children, who start school a year older just because they were born after that cutoff.

Understanding redshirting brings a new and important angle on school-readiness, as it is relevant for parental decision-making, for policy, and for looking at heterogeneous impacts. First, direct evidence on the impacts of academic redshirting may be more relevant for parents (who are considering

[32] 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.

redshirting their children, given their birthdate and skill levels around age 6) and policy-makers (who need to regulate parents' opportunities for academic redshirting), than the existing evidence on the impact of starting school a year older due to being born after the school enrollment cutoff and complying. Second, whether these two types of LATEs for higher SSA (due to redshirting *versus* the school enrollment cutoff) are being driven by the same group of children is an empirical question.

Redshirting helps boys only. I find that (i) redshirted children achieve higher test scores in Grades 6-10 by 0.1-0.2 SDs, are less likely to fail a grade, aspire for more ambitious educational paths, and are more likely to attend the academic high school track and graduate from high school, but (ii) these positive redshirting effects are present only for boys. I also present new evidence that boys fall behind already at ages 1–4 in their speaking, hyperactivity and attention abilities, and are more likely to be non-school-ready than girls. Regarding the underlying mechanisms, I find that redshirted boys are less anxious, more confident and less prone to being bullied in class (for either their appearance or their student achievement). My results suggest that academic redshirting as a policy has the potential to shrink the education gender gap and mitigate the "boys' crisis" in education, by helping boys overcome non-school-readiness and improve their student achievement and mental health at the same time.

Future research may want to formally model parents' redshirting decision, with an emphasis on their beliefs regarding the returns to academic redshirting for their children. One intriguing finding of this paper is that girls, on average, do not benefit from redshirting, yet a significant proportion of them are still redshirted. Girls' LATE estimates for higher SSA due to the school enrollment cutoff date is significantly positive, suggesting that girls do experience the "gift-of-age" gains, net of any potential negative effects of having to start school a year older despite being *school-ready*. The two LATE estimates for girls, at first glance, suggest that many *school-ready* girls are redshirted despite the potential negative effects of being held back, which parents may be unaware of or underestimate,[33] opening up potentially interesting avenues for further research.

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[33] Intuitively, in a comparison of $LATE_{Jun1}$ and $LATE_{Jan1}$ in a simple framework, where the "gift-of-age" gains are uniform across ages and cutoffs, the latter will always be larger if the compliers around the January 1 cutoff is comprised only of *non-school-ready* children. If *school-ready* children are also among the compliers, then $LATE_{Jun1}$ will be closer to $LATE_{Jan1}$. The smaller the share of *non-school-ready* redshirted compliers, the larger the potential negative effects of being held back, despite being school-ready. $LATE_{Jun1}$ estimates close to (or even larger than) $LATE_{Jan1}$ estimates are suggestive of many *school-ready* children being redshirted despite the negative effects of being held back.

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Tables

Table 2: **Fraction of Children with Different Early Childhood and Developmental Obstacles Born Between September and May, by Gender and School-Starting Age, Survey Data**

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Panel A: around birth	***premature birth***	***birth complications***	***born w/ low birth weight***	***breastfed <3 months***	***separated from mother***	***separated from father***
boy	0.0039	0.0206**	-0.0157**	0.0277**	0.0055	-0.0007
	[0.0068]	[0.0098]	[0.0080]	[0.0130]	[0.0042]	[0.0093]
D_i	**0.0593*** **	**0.0318** **	**0.0816*** **	**0.0295**	**0.0259*** **	**0.0333** **
	[0.0134]	[0.0149]	[0.0154]	[0.0195]	[0.0087]	[0.0154]
$boy_i \times D_i$	-0.0153	-0.0229	-0.0354*	-0.0269	-0.0208*	-0.0024
	[0.0173]	[0.0198]	[0.0191]	[0.0262]	[0.0107]	[0.0207]
const	0.0675***	0.1291***	0.0917***	0.2822***	0.0200***	0.1343***
	[0.0048]	[0.0071]	[0.0061]	[0.0095]	[0.0029]	[0.0066]
Panel B: 1-3y old	1^{st} ***words > 1y old***	1^{st} ***sentences age in months***	***chronic illness in <3y face***	***ear***	***few reading few books***	***income shock***
boy	0.0743***	1.6693***	0.0230***	0.0137**	0.0119	0.0038
	[0.0140]	[0.1988]	[0.0083]	[0.0058]	[0.0095]	[0.0072]
D_i	**0.0707*** **	**1.3351*** **	**0.0590*** **	**0.0099**	**0.0752*** **	**0.0356*** **
	[0.0197]	[0.3600]	[0.0142]	[0.0089]	[0.0162]	[0.0131]
$boy_i \times D_i$	-0.0233	0.3538	-0.0368**	0.0034	-0.0444**	-0.0209
	[0.0268]	[0.4810]	[0.0183]	[0.0122]	[0.0210]	[0.0169]
const	0.2692***	20.8039***	0.0914***	0.0347***	0.1117***	0.0775***
	[0.0100]	[0.1486]	[0.0059]	[0.0041]	[0.0069]	[0.0056]
Panel C: 4-5y old	***problems with nervsys***	***problems with speaking***	***problems with cognition***	***problems with coord***	***hyper-activity***	***attention disorder***
boy	0.0037	0.0671***	0.0081**	0.0028	0.0219***	0.0267***
	[0.0024]	[0.0106]	[0.0034]	[0.0026]	[0.0039]	[0.0045]
D_i	**0.0126** **	**0.0756*** **	**0.0180*** **	**0.0097***	**0.0067***	**0.0203*** **
	[0.0051]	[0.0162]	[0.0066]	[0.0050]	[0.0039]	[0.0071]
$boy_i \times D_i$	-0.0111*	-0.0083	0.0066	-0.0004	0.0008	0.0137
	[0.0063]	[0.0233]	[0.0097]	[0.0070]	[0.0084]	[0.0114]
const	0.0051***	0.1414***	0.0107***	0.0067***	0.0083***	0.0138***
	[0.0014]	[0.0071]	[0.0020]	[0.0016]	[0.0019]	[0.0023]

Notes: [1] This table shows the estimated coefficients and standard errors of a given outcome Y_i (seen in bold in the columns) on boy_i, $D_i = 1\{$***start school at age 7***$\}$, and their interactions (without control variables).
[2] In *Panel A*, outcomes pertain to events/shocks around birth: (1) the child was born before 37 weeks of pregnancy have been completed; (2) birth complications occurred at birth; (3) child was born with very low birth weight /<2kg/; (4) the child was breastfed for less than 3 months; (5)-(6) the child was separated from her mother-father for at least a month in the first 1 year.
Panel B pertains to events/shocks/obstacles in the child's first 3 years of life: (1) the child said the first words being more than 1-year-old; (2) age in months when the child said the first complete sentences; (3)-(4) the child had a chronic face-ear illness; (5) only a few times per month the child was read to from books; (6) the family suffered a major income shock.
Panel C pertains to events/shocks/obstacles at ages 4-5: (1) child was diagnosed with problems with nervous system; (2) child was diagnosed with problems with speaking; (3) child was diagnosed with problems with cognition; (4) child was diagnosed with problems with movement and coordination; (5) child was diagnosed as hyperactive; (6) child was diagnosed with attention disorder.
[3] Standard errors are clustered at the school-level, *** p<0.01, ** p<0.05, * p<0.1.
[4] The sample includes children born between September and May.
[5] The estimate for the *const* shows, for a given outcome, the fraction of non-redshirted girls, and the coefficient estimate for *boy* shows to what extent, on average, the share of non-redshirted boys differs from that of non-redshirted girls', while the coefficient estimate on D shows to what extent, on average, the share of redshirted girls differs from that of non-redshirted girls'.
Source of data: Survey data – Hungarian Life Course Survey; $N = 6,901$.

Table 3: ***Academic Redshirting (cutoff: January 1) and School Start at 7 Due to Enrollment Cutoff Date (June 1),* Average Characteristics of Compliers, Administrative Data – Grade 6 and Survey Data**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	cutoff x_d : January 1			*cutoff x_d : June 1*			
	for Academic Redshirting			**for School Enrollment**			
Panel A: Admin. Test Score Data	**share among**		**p-value △**	**share among**		**p-value △**	
	compliers	**sample**	**(1)-(2)**	**compliers**	**sample**	**(4)-(5)**	
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 meals at school	0.2643	0.2561	0.140	0.2330	0.2514	0.000***	0.000***
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 bookss at home	0.9461	0.9468	0.747	0.9587	0.9471	0.000***	0.000***
own desk at home	0.9186	0.9162	0.326	0.9260	0.9168	0.000***	0.022**
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***
lives in North-Eastern of Hungary	0.1225	0.1297	0.464	0.1332	0.1302	0.750	0.105
Panel B: Survey Data	**share among**		**p-value △**	**share among**		**p-value △**	
	compliers	**sample**	**(1)-(2)**	**compliers**	**sample**	**(4)-(5)**	**(1)-(4)**
birth happened prematurely	0.0875	0.0790	0.493	0.0915	0.0852	0.582	0.073*
birth happened w/ complications	0.1433	0.1433	0.994	0.1405	0.1421	0.424	0.228
born with low birth weight	0.0985	0.0899	0.514	0.0847	0.0902	0.001***	0.000***
was breastfed for <3 months	0.2933	0.2940	0.969	0.2884	0.2937	0.265	0.140
separated from mother (<1y old)	0.0318	0.0226	0.193	0.0198	0.0262	0.003***	0.003***
separated from father (<1y old)	0.1531	0.1388	0.318	0.1548	0.1449	0.764	0.545
said first words >1y old	0.3611	0.3112	0.005***	0.3127	0.3176	0.021**	0.000***
age (months): said first sentences	23.0822	21.7264	0.000***	21.9734	22.0030	0.004***	0.000***
chronic face illness (<3y old)	0.0648	0.0469	0.040**	0.0507	0.0463	0.924	0.339
chronic ear illness (<3y old)	0.1438	0.1104	0.009***	0.1238	0.1173	0.590	0.086*
chronic asthma (<3y old)	0.0465	0.0505	0.610	0.0499	0.0473	0.600	0.689
⊖ *income shock to family (<3y old)*	0.0962	0.0772	0.082*	0.0864	0.0852	0.305	0.057*
problems w/ nervous system (4-5y old)	0.0122	0.0068	0.105	0.0065	0.0075	0.110	0.044
problems w/ speaking (4-5y old)	0.2183	0.1892	0.073*	0.1835	0.1864	0.004***	0.000***
problems w/ cognition (4-5y old)	0.0215	0.0140	0.154	0.0201	0.0203	0.083*	0.008***
problems w/ coordination (4-5y old)	0.0124	0.0079	0.251	0.0075	0.0099	0.014**	0.007***
hyperactivity (4-5y old)	0.0238	0.0173	0.276	0.0206	0.0223	0.307	0.222
attention disorder (4-5y old)	0.0460	0.0255	0.003***	0.0299	0.0354	0.001***	0.000***

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 – Hungarian National Assessment of Basic Competences (grade 6) 2008-2017, and Hungarian Life Course Survey.

Table 4: ***Effect of Quarter of Birth*** **- First-Stage Results on Higher SSA (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 Subgroups**

outcome: $D_i = 1\{$***start school at age 7***$\}$	***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*		
grades:	**6**	**8**	**10**	**6**	**8**	**10**
Panel A: all students						
$Z_i = 1\{$**birth date** $X_i \geq x_d\}$	0.1241***	0.1243***	0.1119***	0.1703***	0.1923***	0.2141***
	[0.003]	[0.003]	[0.003]	[0.004]	[0.004]	[0.004]
N	0.1761	0.1679	0.1412	0.3117	0.3243	0.3389
R^2	357,280	341,964	334,767	375,276	356,691	343,272
mean of dependent variable:	*0.1157*	*0.0986*	*0.0929*	*0.5703*	*0.5417*	*0.5010*
Panel B: by sub-groups						
	male *students*					
$Z_i = 1\{$**birth date** $X_i \geq x_d\}$	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
mean of dependent variable:	*0.1464*	*0.1256*	*0.1133*	*0.6410*	*0.6138*	*0.5715*
	female *students*					
$Z_i = 1\{$**birth date** $X_i \geq x_d\}$	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
mean of dependent variable:	*0.0862*	*0.0733*	*0.0739*	*0.5009*	*0.4730*	*0.4346*
	disadvantaged *students*					
$Z_i = 1\{$**birth date** $X_i \geq x_d\}$	0.1284***	0.1241***	0.1165***	0.1988***	0.2304***	0.2634***
	[0.0050]	[0.0052]	[0.0051]	[0.0053]	[0.0055]	[0.0064]
R^2	0.1593	0.1538	0.1303	0.2847	0.3066	0.3323
N	137,024	130,481	117,336	144,312	135,135	119,047
mean of dependent variable:	*0.1720*	*0.1439*	*0.1235*	*0.5947*	*0.5566*	*0.5014*
	privileged *students*					
$Z_i = 1\{$**birth date** $X_i \geq x_d\}$	0.1213***	0.1244***	0.1095***	0.1527***	0.1688***	0.1884***
	[0.0036]	[0.0036]	[0.0034]	[0.0045]	[0.0047]	[0.0047]
R^2	0.1784	0.1715	0.1459	0.3281	0.3361	0.3437
N	220,256	211,483	217,431	230,964	221,556	224,225
mean of dependent variable:	*0.0804*	*0.0706*	*0.0763*	*0.5552*	*0.5327*	*0.5008*

Notes: [1] The table shows the estimated coefficients and standard errors of a Linear Probability Model (LPM) of D_i on Z_i and control variables. D_i is a binary variable denoting age at primary school entry of child i (1: child entered primary school at age 7, 0: child entered primary school at age 6). Z_i is a binary variable denoting quarter of birth of child i (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] "Disadvantaged" students are defined as students whose parents have not completed high school and obtained at most a vocational degree. "Privileged" students are defined as students whose parents have obtained at least a high school diploma.
[5] "Mean of dependent variable" 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.

LATE Estimates on Standardized Mathematics and Reading Test Scores, Grade Repetition, Secondary School Track Choice and Aspirations for Highest Educational Degree

Table 5: ***Effect of Higher SSA Due to Academic Redshirting and Due to the Enrollment Cutoff Date*** **- LATE Estimates on $6^{th}/8^{th}/10^{th}$-Grade Standardized Mathematics Test Score, Administrative Data –Grades 6/8/10, For All and By Subgroups**

outcome: Y_i***: std. mathematics score***	***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*		
in grades:	**6**	**8**	**10**	**6**	**8**	**10**
Panel A: all students						
$D_i = 1\{$**start school at 7**$\}$	0.2124***	0.1448***	0.1220**	0.2520***	0.1482***	0.1393***
	[0.051]	[0.054]	[0.056]	[0.039]	[0.033]	[0.030]
R^2	0.2057	0.2226	0.2433	0.2203	0.2330	0.2489
N	357,158	341,803	334,552	375,163	356,517	343,060
Panel B: by sub-groups						
	male *students*					
$D_i = 1\{$**start school at 7**$\}$	0.3015***	0.1433**	0.1960***	0.1934**	0.1225*	0.1596***
	[0.0691]	[0.0686]	[0.0733]	[0.0757]	[0.0659]	[0.0570]
R^2	0.1984	0.2277	0.2205	0.2303	0.2385	0.2359
N	175,507	165,447	161,464	184,955	173,727	166,681
	female *students*					
$D_i = 1\{$**start school at 7**$\}$	0.103	0.1408*	0.0301	0.2836***	0.1612***	0.1260***
	[0.0784]	[0.0823]	[0.0883]	[0.0418]	[0.0366]	[0.0332]
R^2	0.2102	0.2170	0.2526	0.2098	0.2258	0.2465
N	181,651	176,356	173,088	190,208	182,790	176,379
	disadvantaged *students*					
$D_i = 1\{$**start school at 7**$\}$	0.2319***	0.1940**	0.1512*	0.3011***	0.1225***	0.1110***
	[0.0786]	[0.0887]	[0.0910]	[0.0539]	[0.0450]	[0.0406]
R^2	0.0773	0.0934	0.1306	0.0947	0.1182	0.1490
N	136,967	130,408	117,249	144,257	135,054	118,961
	privileged *students*					
$D_i = 1\{$**start school at 7**$\}$	0.2119***	0.1178*	0.1072	0.2138***	0.1754***	0.1605***
	[0.0666]	[0.0682]	[0.0735]	[0.0545]	[0.0487]	[0.0424]
R^2	0.0806	0.1036	0.1369	0.0934	0.1066	0.1386
N	220,191	211,395	217,303	230,906	221,463	224,099

Notes: [1] The table shows the estimated coefficients and standard errors of the 2^{nd}-stage equation of Y_i on D_i and control variables. The outcome Y_i is standardized mathematics score. D_i is a binary variable denoting age at primary school entry of child i (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 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] "Disadvantaged" students are defined as students whose parents have not completed high school and obtained at most a vocational degree. "Privileged" students are defined as students whose parents have obtained at least a high school diploma.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grades 6,8,10) 2008-2017.

Table 6: ***Effect of Higher SSA Due to Academic Redshirting and Due to the Enrollment Cutoff Date* - LATE Estimates on $6^{th}/8^{th}/10^{th}$-Grade Standardized Reading Test Score, Administrative Data – Grades 6/8/10, For All and By Subgroups**

outcome: Y_i*: std. reading score*	***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*		
in grades:	**6**	**8**	**10**	**6**	**8**	**10**
Panel A: all students						
$D_i = 1\{$**start school at 7**$\}$	0.2043***	0.1302**	0.0892	0.2750***	0.2397***	0.1809***
	[0.050]	[0.051]	[0.056]	[0.036]	[0.034]	[0.029]
R^2	0.2700	0.2853	0.2828	0.2830	0.2872	0.2807
N	357,230	341,881	334,633	375,223	356,619	343,159
Panel B: by sub-groups						
	male *students*					
$D_i = 1\{$**start school at 7**$\}$	0.2906***	0.2167***	0.2469***	0.2731***	0.2691***	0.2045***
	[0.0659]	[0.0658]	[0.0727]	[0.0732]	[0.0661]	[0.0552]
R^2	0.2317	0.2426	0.2321	0.2575	0.2531	0.2515
N	175,527	165,474	161,479	184,991	173,769	166,709
	female *students*					
$D_i = 1\{$**start school at 7**$\}$	0.0937	0.0203	-0.1175	0.2729***	0.2207***	0.1668***
	[0.0770]	[0.0767]	[0.0866]	[0.0386]	[0.0355]	[0.0319]
R^2	0.2852	0.3025	0.3010	0.2881	0.2976	0.2840
N	181,703	176,407	173,154	190,232	182,850	176,450
	disadvantaged *students*					
$D_i = 1\{$**start school at 7**$\}$	0.2657***	0.2980***	0.1184	0.3302***	0.2173***	0.1798***
	[0.0749]	[0.0864]	[0.0901]	[0.0494]	[0.0453]	[0.0397]
R^2	0.1313	0.1360	0.1808	0.1521	0.1743	0.1812
N	136,996	130,440	117,283	144,287	135,101	118,990
	privileged *students*					
$D_i = 1\{$**start school at age 7**$\}$	0.1776***	0.029	0.0766	0.2306***	0.2667***	0.1834***
	[0.0644]	[0.0636]	[0.0707]	[0.0511]	[0.0475]	[0.0413]
R^2	0.1282	0.1581	0.1724	0.1383	0.1428	0.1678
N	220,234	211,441	217,350	230,936	221,518	224,169

Notes: [1] The table shows the estimated coefficients and standard errors of the 2^{nd}-stage equation of Y_i on D_i and control variables. The outcome Y_i is standardized reading score. D_i is a binary variable denoting age at primary school entry of child i (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 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] "Disadvantaged" students are defined as students whose parents have not completed high school and obtained at most a vocational degree. "Privileged" students are defined as students whose parents have obtained at least a high school diploma.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grades 6,8,10) 2008-2017.

Table 7: ***Effect of Higher SSA Due to Academic Redshirting and Due to the Enrollment Cutoff Date* - LATE Estimates on Grade Repetition by a Given Grade, Administrative Data, Grades 6/8/10, For All and By Subgroups**

outcome: $Y_i = 1\{$***having repeated a grade***$\}$	***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*		
by grades	**6**	**8**	**10**	**6**	**8**	**10**
Panel A: all students						
$D_i = 1\{$**start school at age 7**$\}$	-0.0299***	-0.0144	-0.0324	-0.0306***	-0.0360***	-0.0577***
	[0.011]	[0.012]	[0.020]	[0.009]	[0.008]	[0.011]
N	0.0826	0.0753	0.0644	0.0907	0.0754	0.0645
R^2	357,280	341,964	334,767	375,276	356,691	343,272
mean of dependent variable:	*0.0362*	*0.0410*	*0.0926*	*0.0494*	*0.0523*	*0.1113*
Panel B: by sub-groups						
	male *students*					
$D_i = 1\{$**start school at age 7**$\}$	-0.0512***	-0.0209	-0.0941***	-0.0310*	-0.0540***	-0.0743***
	[0.0160]	[0.0166]	[0.0265]	[0.0182]	[0.0165]	[0.0200]
R^2	0.0834	0.0795	0.0489	0.1041	0.0838	0.0690
N	175,557	165,514	161,565	185,021	173,806	166,778
mean of dependent variable:	*0.0449*	*0.0512*	*0.1089*	*0.0646*	*0.0691*	*0.1378*
	female *students*					
$D_i = 1\{$**start school at age 7**$\}$	-0.0044	-0.0056	0.0468	-0.0309***	-0.0267***	-0.0494***
	[0.0163]	[0.0175]	[0.0289]	[0.0088]	[0.0082]	[0.0112]
R^2	0.0784	0.0692	0.0668	0.0860	0.0744	0.0672
N	181,723	176,450	173,202	190,255	182,885	176,494
mean of dependent variable:	*0.0287*	*0.0324*	*0.0785*	*0.0389*	*0.0407*	*0.0928*
	disadvantaged *students*					
$D_i = 1\{$**start school at age 7**$\}$	-0.0675***	-0.0046	0.0035	-0.0484***	-0.0637***	-0.1025***
	[0.0249]	[0.0263]	[0.0388]	[0.0163]	[0.0152]	[0.0188]
R^2	0.0651	0.0707	0.0545	0.0856	0.0721	0.0548
N	137,024	130,481	117,336	144,312	135,135	119,047
mean of dependent variable:	*0.0759*	*0.0799*	*0.1508*	*0.0982*	*0.0982*	*0.1782*
	privileged *students*					
$D_i = 1\{$**start school at age 7**$\}$	-0.0051	-0.0190*	-0.0544**	-0.0197***	-0.0158**	-0.0282**
	[0.0094]	[0.0112]	[0.0215]	[0.0075]	[0.0077]	[0.0117]
R^2	0.0263	0.0247	0.0345	0.0314	0.0286	0.0444
N	220,256	211,483	217,431	230,964	221,556	224,225
mean of dependent variable:	*0.0142*	*0.0191*	*0.0628*	*0.0224*	*0.0264*	*0.0767*

Notes: [1] The table shows the estimated coefficients and standard errors of the 2^{nd}-stage equation of Y_i on D_i and control variables. Y_i is a binary variable denoting whether the child repeated at least one grade by grades 6, 8 and 10, respectively (1: child repeated grade, 0: child never repeated a grade). D_i is a binary variable denoting age at primary school entry of child i (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 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] "Disadvantaged" students are defined as students whose parents have not completed high school and obtained at most a vocational degree. "Privileged" students are defined as students whose parents have obtained at least a high school diploma.
[5] "Mean of dependent variable" pertains to mean of Y_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 $D_i = 0$.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grades 6,8,10) 2008-2017.

Table 8: ***Effect of Higher SSA Due to Academic Redshirting and Due to the Enrollment Cutoff Date* - LATE Estimates on Secondary School Track, Administrative Data – Grade 10, For All and By Subgroups**

outcome: $Y_i = 1\{$**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* /vocational/	*middle*	*high* /academic/	*low* /vocational/	*middle*	*high* /academic/
Panel A: all students						
$D_i = 1\{$**start school at age 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]
N	0.2285	0.0601	0.2337	0.2089	0.0506	0.1880
R^2	334,767	334,767	334,767	343,272	343,272	343,272
mean of dependent variable:	*0.1597*	*0.4055*	*0.4348*	*0.1592*	*0.4024*	*0.4384*
Panel B: by sub-groups						
	male *students*					
$D_i = 1\{$**start school at age 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
mean of dependent variable:	*0.2081*	*0.4375*	*0.3544*	*0.2194*	*0.4359*	*0.3447*
	female *students*					
$D_i = 1\{$**start school at age 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
mean of dependent variable:	*0.1201*	*0.3783*	*0.5016*	*0.1175*	*0.3784*	*0.5041*
	disadvantaged *students*					
$D_i = 1\{$**start school at age 7**$\}$	-0.0865*	0.0384	0.048	-0.1062***	0.0468**	0.0594***
	[0.0505]	[0.0529]	[0.0417]	[0.0213]	[0.0232]	[0.0193]
R^2	0.1567	0.0438	0.0780	0.164278	0.0433093	0.0801178
N	117,336	117,336	117,336	119047	119047	119047
mean of dependent variable:	*0.3331*	*0.4540*	*0.2129*	*0.3337*	*0.4513*	*0.2150*
	privileged *students*					
$D_i = 1\{$**start school at age 7**$\}$	-0.0643***	0.0038	0.0605	-0.0285**	-0.0755***	0.1041***
	[0.0236]	[0.0419]	[0.0399]	[0.0127]	[0.0236]	[0.0230]
R^2	0.0828	0.0845	0.1732	0.0956	0.0858	0.1748
N	217,431	217,431	217,431	224,225	224,225	224,225
mean of dependent variable:	*0.0712*	*0.3807*	*0.5482*	*0.0687*	*0.3771*	*0.5542*

Notes: [1] The table shows the estimated coefficients and standard errors of the 2^{nd}-stage equation of Y_i on D_i and control variables. Secondary school track choice Y_i is measured by binary variables indicating whether the child attends the j-th track at grade 10. Track #1 is the lowest ("vocational") secondary school track, granting a vocational, but no high school degree upon completion. Track #2 is the middle secondary school track, granting both a vocational and a high school degree upon completion. Track #3 is the highest ("academic") secondary school track, granting a high school degree upon completion. D_i is a binary variable denoting age at primary school entry of child i (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 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] "Disadvantaged" students are defined as students whose parents have not completed high school and obtained at most a vocational degree. "Privileged" students are defined as students whose parents have obtained at least a high school diploma.
[5] "Mean of dependent variable" pertains to mean of Y_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 $D_i = 0$.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 10) 2008-2017.

Table 9: ***Effect of Higher SSA Due to Academic Redshirting and Due to the Enrollment Cutoff Date* - LATE Estimates on Educational Aspirations, Administrative Data – Grade 10, For All and By Subgroups**

outcome: $Y_i = 1\{$**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 A: all students						
$D_i = 1\{$**start school at age 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]
N	0.1835	0.1134	0.2812	0.1830	0.1159	0.2815
R^2	329,424	329,424	329,424	337,665	337,665	337,665
mean of dependent variable:	*0.0852*	*0.3370*	*0.5778*	*0.0837*	*0.3334*	*0.5829*
Panel B: by sub-groups						
	male *students*					
$D_i = 1\{$**start school at age 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
mean of dependent variable:	*0.1229*	*0.3743*	*0.5028*	*0.1307*	*0.3039*	*0.4937*
	female *students*					
$D_i = 1\{$**start school at age 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
mean of dependent variable:	*0.0545*	*0.3064*	*0.6390*	*0.0515*	*0.3035*	*0.6446*
	disadvantaged *students*					
$D_i = 1\{$**start school at age 7**$\}$	-0.0387	0.002	0.0368	-0.0588***	-0.0061	0.0648***
	[0.0444]	[0.0584]	[0.0474]	[0.0186]	[0.0244]	[0.0215]
R^2	0.1524	0.0097	0.1194	0.1543	0.0110	0.1184
N	115,294	115,294	115,294	116,859	116,859	116,859
mean of dependent variable:	*0.1923*	*0.5027*	*0.3050*	*0.1896*	*0.5033*	*0.3071*
	privileged *students*					
$D_i = 1\{$**start school at age 7**$\}$	-0.018	-0.0563	0.0743**	-0.0221***	-0.0746***	0.0967***
	[0.0164]	[0.0345]	[0.0351]	[0.0085]	[0.0206]	[0.0206]
R^2	0.0546	0.1143	0.1636	0.0538	0.1163	0.1646
N	214,130	214,130	214,130	220,806	220,806	220,806
mean of dependent variable:	*0.0306*	*0.2526*	*0.7168*	*0.0290*	*0.2455*	*0.7255*

Notes: [1] The table shows the estimated coefficients and standard errors of the 2^{nd}-stage equation of Y_i on D_i and control variables. Aspiration Y_i is measured by binary variables indicating whether the child aspires to (i) have less than high-school education ($< HS$), (ii) obtain a high-school degree (HS), (iii) or obtain a degree higher than high-school ($HS+$). D_i is a binary variable denoting age at primary school entry of child i (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 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] "Disadvantaged" students are defined as students whose parents have not completed high school and obtained at most a vocational degree. "Privileged" students are defined as students whose parents have obtained at least a high school diploma.
[5] "Mean of dependent variable" pertains to mean of Y_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 $D_i = 0$.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 10) 2008-2017.

Table 10: ***Effect of Higher SSA Due to Academic Redshirting and Due to the Enrollment Cutoff Date* - LATE Estimates on Graduation from High School and Vocational Training, For All and By Subgroups**

outcome: $Y_i = 1\{$**graduation** $j\}$ $(j = HS, VOC)$	***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*	
graduation	HS	VOC	HS	VOC
Panel A: all students				
$D_i = 1\{$**start school at age 7**$\}$	0.1696**	-0.1705***	0.0471*	-0.0574**
	[0.074]	[0.063]	[0.028]	[0.024]
N	0.2595	0.0695	0.2926	0.0988
R^2	47,513	47,513	57,361	57,361
mean of dependent variable:	*0.6884*	*0.1092*	*0.7147*	*0.1062*
Panel B: by sub-groups				
	male *students*			
$D_i = 1\{$**start school at age 7**$\}$	0.1983**	-0.2115**	0.0184	-0.0677
	[0.0979]	[0.0857]	[0.0579]	[0.0516]
R^2	0.2412	0.0588	0.2780	0.0999
N	22,795	22,795	27,651	27,651
mean of dependent variable:	*0.6387*	*0.1445*	*0.6590*	*0.1569*
	female *students*			
$D_i = 1\{$**start school at age 7**$\}$	0.1402	-0.1272	0.0608**	-0.0516**
	[0.1061]	[0.0827]	[0.0293]	[0.0231]
R^2	0.2681	0.0619	0.3010	0.0746
N	24,718	24,718	29,710	29,710
mean of dependent variable:	*0.7123*	*0.0737*	*0.7533*	*0.0710*

Notes: [1] The table shows the estimated coefficients and standard errors of the 2^{nd}-stage equation of Y_i on D_i and control variables. D_i is a binary variable denoting age at primary school entry of child i (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 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] "Disadvantaged" students are defined as students whose parents have not completed high school and obtained at most a vocational degree. "Privileged" students are defined as students whose parents have obtained at least a high school diploma.
[5] "Mean of dependent variable" pertains to mean of Y_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 $D_i = 0$.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 10) 2008-2014, Linked to Administrative Labor Data.

Mechanisms: Results on Absolute *versus* Relative Age Effects and Mental Health

Table 11: ***Effect of Quarter of Birth and the Share of Summer-Born Classmates* - First-Stage Results on Higher SSA ($D_i = 1\{$start school at 7$\}$) and RR_i (Relative Age Rank in Class), Administrative Data – Grade 6, By Gender**

cutoff x_d : January 1	**boys**		**girls**	
	D_i	$RR_i(\%)$	D_i	$RR_i(\%)$
$1\{X_i \geq x_d\}$	0.1285***	2.2311***	0.0948***	-0.5874
	[0.0128]	[0.8596]	[0.0105]	[0.7707]
$peers_summer_i(\%)$	-0.0001	-0.2679***	-0.0003	-0.3263***
	[0.0003]	[0.0188]	[0.0002]	[0.0162]
$1\{X_i \geq x_d\} \times$	0.00002	0.2090***	0.0002	0.2514***
$peers_summer_i(\%)$	[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]The table shows the first-stage results of D_i and RR_i, where D_i is a binary variable denoting age at primary school entry of child i (1: child entered primary school at age 7, 0: child entered primary school at age 6). RR_i is the continuous measure of the child's relative age rank in the class, measured in percentiles; thus, 100 means the child is the oldest in class. $1\{X_i \geq x_d\}$ 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. The corresponding 2^{nd}-stage equation is (4).
[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.

Table 12: ***Effect of Higher SSA Due to Academic Redshirting and Relative Age Rank in Class* - LATE Estimates on 6^{th}-Grade Standardized Test Scores, Administrative Data – Grade 6, By Gender**

cutoff x_d : January 1	**boys**				**girls**			
Y_i **– std. test score:**	**math**		**reading**		**math**		**reading**	
D_i		0.4071**		0.3190*		0.081		0.2018
		[0.2068]		[0.1902]		[0.1976]		[0.1854]
$RR_i(\%)$	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] The table shows the 2^{nd}-stage results of Y_i on D_i and RR_i, where D_i is a binary variable denoting age at primary school entry of child i (1: child entered primary school at age 7, 0: child entered primary school at age 6). RR_i is the continuous measure of the child's relative age rank in the class, measured in percentiles; thus, 100 means the child is the oldest in class. Y_i is either mathematics or reading test score in grade 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 for coefficients and standard errors is (4).
[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 13: ***Effect of Higher SSA Due to Academic Redshirting*** **- LATE Estimates on Anxiety, Confidence and Being Bullied Measures at Grade 8, By Gender, Survey Data and Administrative Health Data**

Panel A: Outcomes of Anxiety and Psycholeptics Drug Expenditures; *cutoff x_d : January 1*						
	boys			**girls**		
Y_i :	***std. anxiety score***	***often feeling anxious***	***log(exp.) N05***	***std. anxiety score***	***often feeling anxious***	***log(exp.) N05***
D_i	-1.1614**	-0.4555**	-0.0398**	-0.1132	-0.2905	0.0138
	[0.5101]	[0.2204]	[0.0181]	[0.6338]	[0.3212]	[0.0193]
cons	0.4360*	0.5149***	-0.01	0.5491**	0.6629***	0.0151
	[0.2508]	[0.1032]	[0.0131]	[0.2419]	[0.1210]	[0.0119]
N	38,516	38,516	62,027	37,844	37,844	64,264
1^{st}*-stage F-statistic:*	*39.44*	*39.44*	*435.54*	*27.5*	*27.5*	*406.93*
data	*survey*	*survey*	*admin health*	*survey*	*survey*	*admin health*

Panel B: Outcomes of Confidence and Being Bullied*; cutoff x_d : January 1*						
	boys			**girls**		
Y_i :	***lack of confidence***	***bullied for appearance***	***bullied for studies***	***lack of confidence***	***bullied for appearance***	***bullied for studies***
D_i	-0.5021**	-0.3993*	-0.3272*	-0.0573	-0.166	0.0991
	[0.2476]	[0.2278]	[0.1860]	[0.3439]	[0.3348]	[0.2204]
cons	0.7604***	0.3445***	0.3732***	0.6931***	0.4465***	0.0844
	[0.1123]	[0.1041]	[0.0888]	[0.1244]	[0.1196]	[0.0880]
N	38,480	38,489	38,496	37,850	37,857	37,827
1^{st}*-stage F-statistic:*	*39.2*	*39.6*	*39.5*	*27.4*	*27.47*	*27.37*
data	*survey*	*survey*	*survey*	*survey*	*survey*	*survey*

Notes: [1] The table shows the estimated coefficients and standard errors of the 2^{nd}-stage equation of Y_i on D_i and control variables, on various mental health, confidence and experience of being bullied outcomes. D_i is a binary variable denoting age at primary school entry of child i (1: child entered primary school at age 7). Control variables include: linear trend in month of birth and its interaction with a binary variable denoting quarter of birth, cohort fixed effects, grade fixed effects, 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 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: Hungarian Life Course Survey and 50 percent of the administrative test score data – Hungarian National Assessment of Basic Competences (grades 6 and 8) 2009-2017, linked with drug expenditures data.

Figures

Figure 1: **Shares of Children who Start School at Age 7 and School-Starting Age (SSA), by Month of Birth and Gender, Administrative Data – Grade 6**

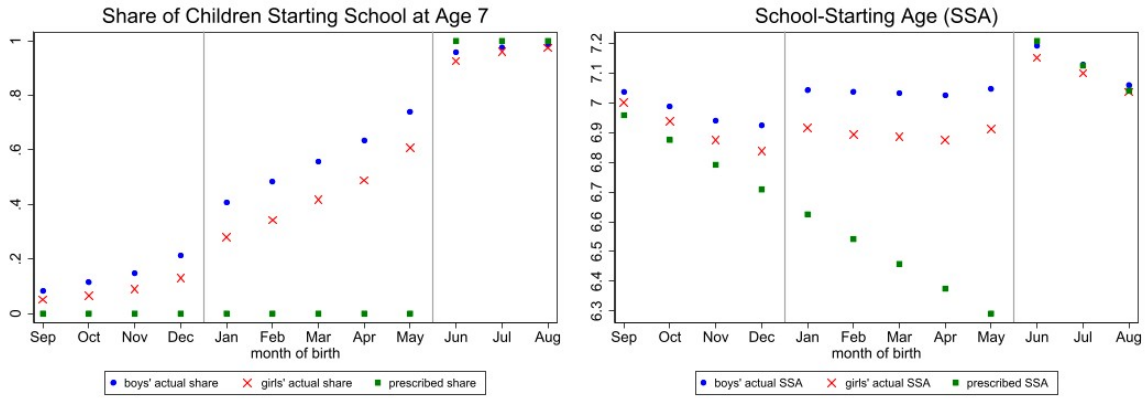
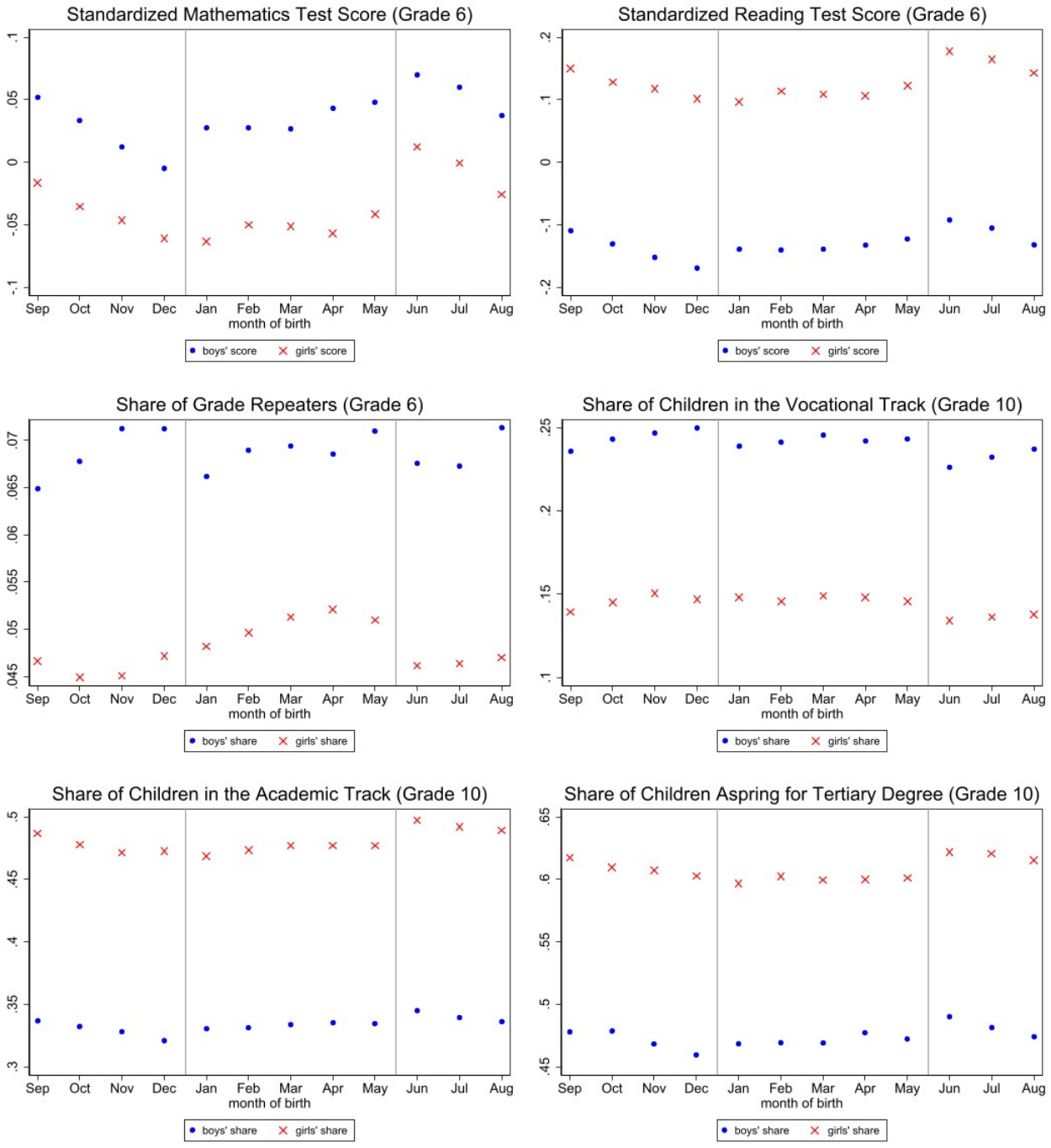


Figure 2: **Average Student Outcomes (Test Scores, Grade Repetition, Secondary School Track, Aspirations), by Month of Birth and Gender, Administrative Data – Grades 6 and 10**



Notes: Figure 1 shows the raw first-stage relationship between month of birth and actual school-starting age (SSA), by gender, and the prescribed SSA by the institutional setting. Figure 2 shows the raw reduced-form relationship between month of birth and various student outcomes, by gender. January 1 is the cutoff for redshirting, and June 1 is the school enrollment cutoff date.
Source of data: Hungarian National Assessment of Basic Competences (grades 6 and 10) 2009-2017.

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,[34] 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*.

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

B Sample Selection, Descriptives, and the Relative Rank Variable

Table B1: **Details of Sample Selection, Administrative Data – Grade 6**

	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 6) 2008-2017.

Table B2: **Share of Boys and Children with Different Parental Education, by Month of Birth and School-Starting Age, Administrative Data – Grade 6**

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 6) 2008-2017.

Table B3: **Distribution, Fraction of Children Entering School at Age 7 and Average Years Spent in Childcare, by Month of Birth and Childcare-Starting Age (CSA), , Administrative Data – Grade 6**

month of birth	childcare-starting age	2	3	4	5	average
September	**frequency**	**27.86%**	**5.08%**	**3.84%**	**4.28%**	**8.744%**
	fraction of children entering school at age 7	*1.60%*	*8.10%*	*22.07%*	*34.45%*	*0.062*
	average years spent in childcare	3.995	3.078	2.238	1.520	3.477
October	**frequency**	**23.21%**	**5.69%**	**3.92%**	**4.18%**	**8.325%**
	fraction of children entering school at age 7	*2.09%*	*9.84%*	*27.30%*	*39.76%*	*0.082*
	average years spent in childcare	4.003	3.096	2.292	1.585	3.426
November	**frequency**	**19.35%**	**6.16%**	**4.17%**	**4.62%**	**8.013%**
	fraction of children entering school at age 7	*2.78%*	*12.28%*	*31.43%*	*44.11%*	*0.109*
	average years spent in childcare	4.009	3.121	2.327	1.606	3.373
December	**frequency**	**14.02%**	**7.68%**	**4.80%**	**5.52%**	**8.180%**
	fraction of children entering school at age 7	*3.92%*	*17.06%*	*40.48%*	*48.05%*	*0.165*
	average years spent in childcare	4.011	3.169	2.426	1.663	3.288
January	**frequency**	**4.64%**	**10.07%**	**6.25%**	**7.77%**	**8.386%**
	fraction of children entering school at age 7	*10.50%*	*30.22%*	*61.80%*	*63.98%*	*0.337*
	average years spent in childcare	4.052	3.302	2.650	1.840	3.217
February	**frequency**	**2.99%**	**9.41%**	**6.16%**	**7.17%**	**7.652%**
	fraction of children entering school at age 7	*17.38%*	*36.26%*	*66.17%*	*67.17%*	*0.403*
	average years spent in childcare	4.099	3.363	2.697	1.880	3.246
March	**frequency**	**2.44%**	**10.30%**	**7.23%**	**8.65%**	**8.357%**
	fraction of children entering school at age 7	*22.56%*	*43.27%*	*72.28%*	*72.66%*	*0.478*
	average years spent in childcare	4.151	3.434	2.756	1.936	3.286
April	**frequency**	**1.68%**	**9.72%**	**7.54%**	**8.69%**	**7.924%**
	fraction of children entering school at age 7	*28.86%*	*50.82%*	*76.86%*	*75.61%*	*0.553*
	average years spent in childcare	4.159	3.509	2.800	1.986	3.328
May	**frequency**	**1.38%**	**9.84%**	**8.71%**	**9.26%**	**8.170%**
	fraction of children entering school at age 7	*42.49%*	*63.34%*	*83.83%*	*79.58%*	*0.671*
	average years spent in childcare	4.283	3.635	2.866	1.984	3.414
June	**frequency**	**0.83%**	**8.68%**	**13.71%**	**12.76%**	**8.378%**
	fraction of children entering school at age 7	*74.49%*	*92.83%*	*98.05%*	*96.77%*	*0.942*
	average years spent in childcare	4.541	3.930	3.013	2.168	3.544
July	**frequency**	**0.84%**	**8.97%**	**16.43%**	**13.30%**	**9.032%**
	fraction of children entering school at age 7	*81.46%*	*96.15%*	*98.82%*	*98.63%*	*0.969*
	average years spent in childcare	4.568	3.964	3.020	2.197	3.547
August	**frequency**	**0.77%**	**8.41%**	**17.23%**	**13.81%**	**8.837%**
	fraction of children entering school at age 7	*84.33%*	*97.62%*	*99.24%*	*98.51%*	*0.980*
	average years spent in childcare	4.605	3.980	3.027	2.226	3.532

Notes: This table shows the distribution of children by month of birth and age at childcare entry, along with the share of children entering school at age 7 and average years spent in childcare in each cell. Bold percentages in each column add up to 100%.
Source of data: Administrative test score data – Hungarian National Assessment of Basic Competences (grade 6) 2008-2017.

Table B4: **Distribution of Relative Rank Variable (RR), by Age in Months and School-Starting Age (SSA), in 2010 and 2014, Administrative Data – Grade 6**

	2010						**2014**					
	$SSA = 7$			$SSA = 6$			$SSA = 7$			$SSA = 6$		
age	***min***	***max***	***sd***	***min***	***max***	***sd***	***min***	***max***	***sd***	***min***	***max***	***sd***
11y9m				0.70	10.00	1.89				0.51	14.29	2.37
11y10m				0.40	25.00	2.79				1.03	23.08	3.00
11y11m				0.47	25.00	3.36				0.36	50.00	3.89
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			
14y0m	42.86	100	10.53				28.57	100	11.85			

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

C Exogeneity Tests of Quarter of Birth Around January 1

Table C1: ***Exogeneity of Quarter of Birth Around January 1*** **- "First-Stage" Results on Various Background and Early Childhood Characterics, 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.***
Z_i	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***
Z_i	-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***
Z_i	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***
Z_i	-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***
Z_i	-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***
Z_i	-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 Z_i and control variables. Z_i is a binary variable denoting quarter of birth of child i (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.
[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, and Hungarian Life Course Survey.

D Placebo Tests for Alternative Cutoffs Around January 1

Table D1: ***Placebo Tests for Alternative Cutoffs Around January 1*** **-First-Stage and Reduced-Form Estimates on Standardized Test Score, Grade Repetition, Secondary School Track Choice and Educational Aspirations, For All and For Boys, Administrative Data – Grade 10**

		placebo cutoffs x_d					
		Oct1	***Nov1***	***Dec1***	***Jan1***	***Feb1***	***Mar1***
		1^{st}***-stage*** // $D_i = 1\{$**start school at 7**$\}$					
all	$Z_i = 1\{X_i \geq x_d\}$	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	$Z_i = 1\{X_i \geq x_d\}$	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	$Z_i = 1\{X_i \geq x_d\}$	-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	$Z_i = 1\{X_i \geq x_d\}$	-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	$Z_i = 1\{X_i \geq x_d\}$	-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	$Z_i = 1\{X_i \geq x_d\}$	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	$Z_i = 1\{X_i \geq x_d\}$	-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	$Z_i = 1\{X_i \geq x_d\}$	-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	$Z_i = 1\{X_i \geq x_d\}$	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	$Z_i = 1\{X_i \geq x_d\}$	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	$Z_i = 1\{X_i \geq x_d\}$	-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	$Z_i = 1\{X_i \geq x_d\}$	-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	$Z_i = 1\{X_i \geq x_d\}$	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	$Z_i = 1\{X_i \geq x_d\}$	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_i is a binary variable denoting month of birth of child i (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.

E Robustness Check: LATE of Higher SSA Due to Redshirting; 2-Months Window and Quadratic Trend around January 1

Table E1: ***Robustness Check with a 2-Months Window and a Quadratic Trend Around January 1*** **- LATE Estimates of SSA 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_i***: mathematics test score***									
grade	**6**	**8**	**10**	**6**	**8**	**10**	**6**	**8**	**10**
D_i	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_i***: reading test score***									
grade	**6**	**8**	**10**	**6**	**8**	**10**	**6**	**8**	**10**
D_i	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
$Y_i = 1\{$***having repeated a grade***$\}$									
grade	**6**	**8**	**10**	**6**	**8**	**10**	**6**	**8**	**10**
D_i	-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
$Y_i = 1\{$**being in track** $j\}$									
track	*middle*	*high*	*low*	*middle*	*high*	*low*	*middle*	*high*	*low*
D_i	-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
$Y_i = 1\{$**aspiration** $j\}$									
level	$< HS$	HS	$HS+$	$< HS$	HS	$HS+$	$< HS$	HS	$HS+$
D_i	-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_i on D_i and control variables. The outcome Y_i is standardized mathematics score. D_i is a binary variable denoting age at primary school entry of child i (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.

F Robustness Check: Standard Error Clustering

Table F1: ***Robustness Check*** **- Standard Error Estimates and T-Statistics of Coefficient Estimates on Variable $D_i = 1\{$start school at 7$\}$, for Alternative Levels of Clustering of Standard Errors, Administrative Data – Grades 6/10, For All and for Boys**

		clustering on:					
		school ID		***birth month***		***(birthm)×year***	
outcome//(sub)group	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Y_i***: std. math score in grade 6***	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$
all	0.2124	0.0512	4.15	0.0119	17.89	0.0401	5.3
boys	0.3015	0.0691	4.36	0.1416	21.29	0.6045	4.99
Y_i***: std. reading score in grade 6***	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$
all	0.2043	0.0499	4.09	0.0104	19.69	0.0431	4.74
boys	0.2906	0.0659	4.41	0.0040	72.99	0.0560	5.19
$Y_i = 1\{$***having repeated a grade by grade 6***$\}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$
all	-0.0299	0.0114	-2.61	0.0032	-9.34	0.0110	-2.73
boys	-0.0512	0.0160	-3.21	0.0053	-9.62	0.0160	-3.19
$Y_i = 1\{$**being in track** "*vocational*"$\}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$
all	-0.0732	0.0233	-3.14	0.0194	-3.78	0.0217	-3.37
boys	-0.1193	0.0306	-3.9	0.0113	-10.54	0.0267	-4.47
$Y_i = 1\{$**being in track** "*academic*"$\}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$
all	0.0564	0.0290	1.94	0.0059	9.52	0.0264	2.14
boys	0.1067	0.0365	2.93	0.0070	15.2	0.0283	3.77
$Y_i = 1\{$**aspiring for** $< HS$ **(in grade 10)**$\}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$
all	-0.0258	0.0190	-1.36	0.0110	-2.36	0.0152	-1.7
boys	0.0524	0.0261	-2	0.0169	-3.1	0.0162	-3.22
$Y_i = 1\{$**aspiring for** $HS+$ **(in grade 10)**$\}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$
all	0.0618	0.0293	2.11	0.0085	7.3	0.0230	2.69
boys	0.1343	0.0362	3.71	0.0113	11.87	0.0358	3.75
$Z_i = 1\{X_i \geq x_d\}$	$\hat{coeff}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$	$\hat{SE}$	$\hat{t}$
all	0.1241	0.0031	40.62	0.0067	18.6	0.0122	10.19
boys	0.1388	0.0044	31.37	0.0090	15.35	0.0142	9.75

Notes: [1] This table shows the standard error estimates and corresponding value of t-statistics, of coefficient estimates on Variable $D_i = 1\{$start school at 7$\}$, for alternative levels of clustering.
[2] Columns (1)–(3) show the baseline results, already presented in Tables 5, 6, 7, 8, 9.
[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 (grades 6,10) 2008-2017.

G Robustness Check: LATE of Higher SSA Due to Redshirting on Test Scores, by Accounting for Childcare-Starting Age (CSA)

The estimated effect of redshirting in the main specification (2) encompasses the fact that redshirted children typically start childcare a year later, besides starting primary school a year later. Then, to the extent time spent in childcare (all else equal) positively affects student achievement and mental health, any positive effect of redshirting presented in Section 6.1 prevails *despite* the fact that redshirted children typically enter childcare a year older, and spend one less year in childcare. One might wonder to what extent results presented in Section 6.1 are robust to accounting for the endogenous nature of childcare-starting age (CSA).

To assess the aforementioned question, as well as to see by how much redshirting compensates for the relatively little time in childcare, I present a variant of estimation equation (2), in which I include and instrument for CSA. 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 (CSA), and only at the relevant school-starting age (SSA) do they decide about redshirting. 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 (which main specification turns out to yield more conservative estimates for the impact of redshirting).

To account for the endogenous nature of CSA, 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.[35] Thus, the fraction of 3-year-old children in childcare is strongly related to whether a 2-year-old can enter.[36]

Table G1 shows the result of the first-stage regressions on the variable reflecting SSA (D_i, that is 1 if the child entered school at the age of 7, as opposed to 6) and CSA (CC_i, that is 1 if the child entered childcare at the age of 3+, as opposed to 2) variables, for children born in a 3-months window around January 1, the cutoff relevant for academic redshirting. Regarding CC_i, 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 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 larger increase in the likelihood of entering at age 3+ 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 100 for both D_i and CC_i, and the value of both of the *Cragg-Donald Wald F-statistics* and the *Kleibergen-Papp F-statistics* is above 150. Therefore, weak identification is not a concern.

Table G2 shows the LATE estimates for academic redshirting, by accounting for CSA: the point estimate is 30 percent of a standard deviation for both subjects in grade 6, 25 percent in grade 8, and slightly smaller in grade 10. Regarding the validity of the instruments, the *Hansen J-statistics* are below 1, therefore the null hypothesis of valid (excludable) instruments cannot be rejected at the usual significance levels.

In comparison, the point estimates (not accounting for childcare-starting age) in Tables 5 and 6 are 0.21-0.14-0.12 percent of a standard deviation for mathematics across grades 6, 8, and 10, and they are 0.25-0.15-0.14 for reading, respectively; they show that redshirting has a significantly large effect on test scores, despite the fact that redshirted children typically spent less time in childcare, and are significantly less likely to enter childcare at age 2—Table G2, in addition, reveals that if one compared two children with the same CSA, the redshirted child would realize even larger gains from redshirting.

[35] 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)).

[36] 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.

But, it is important to keep in mind that—as discussed above—it is ambiguous in what order parents make decisions about their children's educational path; if they are forward-looking, and decide about CSA with the possibility of redshirting in mind, then the less time spent in childcare is part of the mechanism through which redshirting impacts children, and is ideally not controlled for.

Table G1: ***Effect of Academic Redshirting*** **- First-stage Results on Academic Redshirting (Starting School at Age 7, D_i) and of Entering Childcare at Age 3+ (CC_i), Administrative Data – Grades 6/8/10**

cutoff x_d : January 1	**grade 6**		**grade 8**		**grade 10**	
	D_i	CC_i	D_i	CC_i	D_i	CC_i
$1\{X_i \geq x_d\}$	0.0898***	0.0425***	0.0927***	0.0407***	0.0857***	0.0279***
	[0.007]	[0.006]	[0.007]	[0.005]	[0.008]	[0.007]
$frac_3yr_in_childcare_i$	0.0016	-0.1028***	0.0027	-0.1052***	0.0114*	-0.0965***
	[0.006]	[0.008]	[0.006]	[0.008]	[0.006]	[0.007]
$1\{X_i \geq x_d\} \times$	0.0502***	-0.0269***	0.0445***	-0.0264***	0.0283***	-0.0114
$frac_3yr_in_childcare_i$	[0.010]	[0.007]	[0.009]	[0.007]	[0.010]	[0.009]
R^2	0.178	0.098	0.173	0.082	0.152	0.056
N	346,138	346,138	293,259	293,259	211,628	211,628
joint F-statistic	*544.76*	*119.19*	*473.67*	*95.16*	*298.54*	*110.49*
Cragg-Donald Wald F-statistic:	*568.79*		*492.46*		*226.53*	
Kleibergen-Paap rk Wald F-statistic:	*263.13*		*214.9*		*150.27*	

Notes: [1] The table shows the first-stage results of D_i and CC_i, where D_i is a binary variable denoting age at primary school entry of child i (1: child entered primary school at age 7, 0: child entered primary school 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). $1\{X_i \geq x_d\}$ 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). $frac_3yr_in_childcare_i$ 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.

Table G2: ***Effect of Academic Redshirting*** **- IV Results of the Effect of Academic Redshirting (Starting School at Age 7, D_i) and of Entering Childcare at Age 3+ (CC_i), Administrative Data – Grades 6/8/10**

cutoff x_d : January 1	**grade 6**		**grade 8**		**grade 10**	
Y_i **– std. test score**	**math**	**reading**	**math**	**reading**	**math**	**reading**
$D_i = 1\{$school entry	0.2899***	0.2935***	0.2565***	0.2536***	0.2417***	0.1933**
at age 7$\}$	[0.055]	[0.053]	[0.060]	[0.057]	[0.080]	[0.081]
$CC_i = 1\{$childcare entry	-0.6294***	-0.5913***	-0.6174***	-0.6047***	-0.6701***	-0.5029***
at age 3+$\}$	[0.144]	[0.115]	[0.146]	[0.122]	[0.187]	[0.175]
R^2	0.184	0.244	0.199	0.254	0.218	0.267
N	346,019	346,089	293,123	293,200	211,496	211,568
Hansen J-statistic:	*0.6855*	*0.1297*	*0.489*	*0.4277*	*0.4916*	*0.4012*

Notes: [1] The table shows the 2^{nd}-stage results of Y_i on D_i and CC_i, where D_i is a binary variable denoting age at primary school entry of child i (1: child entered primary school at age 7, 0: child entered primary school at age 6), 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), and Y_i is either mathematics or reading test score in grades 6/8/10. 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.