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# Early Skill Effects on Parental Beliefs, Investments and Children Long-Run Outcomes<sup>1</sup>

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## Abstract

This paper examines the effects of early skill advantages on parental beliefs, investments, and children's educational outcomes measured up to age 27. We exploit exogenous variation in skills due to school entry rules, combining 20 years of Chilean administrative records with a regression discontinuity design. Our results show that these rules change parental beliefs and influence their material investments. Children benefited from the early skill advantage have higher in-school performance and college entrance scores, and sizable effects on college attendance and enrollment at selective institutions. These long-run effects are more pronounced for low-income families, and likely mediated by parental beliefs and investments.

*Keywords:* Early Life Shocks; Long-run Outcomes; Skills; Parental Investments; College Attendance, Test Scores, Low-income, Developing Country

*JEL codes:* I21, I26, I28, J24, J31.

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# 1 Introduction

Several studies show that early life disparities affect children’s development (see, e.g., [Bharadwaj et al., 2013](#)), with lasting effects on adult outcomes ([Almond et al., 2018](#)). Parental investments can mediate these long term consequences and therefore there is growing interest in better understanding how parents respond to their children’s (dis)advantages (e.g., [Carneiro et al., 2019](#); [Dizon-Ross, 2019](#); [Boneva and Rauh, 2018](#)).<sup>1</sup>

In this paper we examine how parental beliefs and investments respond to perceived signals of children’s ability and how those responses are related to long term effects on their children. We study this question exploiting exogenous variation in skills due to school entry rules, combining twenty years of Chilean administrative micro-data with a regression discontinuity design. We supplement the administrative records with survey data containing information on parental investments and beliefs reported by parents and students.

We start documenting effects on in-school measures of performance, replicating results from the existing literature that uses age-at-entry rules.<sup>2</sup> Second, we contribute with novel evidence to this literature by showing how parental beliefs and investments respond to the initial skill disparities produced by the entry rule and how these responses vary by type of investment. Third, we follow children over twenty years to estimate long run effects. Finally, we place our findings within a human capital accumulation framework that connects parental responses to the effects of early skill gaps on children’s future human capital.

Our research design mimics a local experiment where children are exogenously allocated to start school at different ages due to birth date cutoff rules. [Figure 1](#) shows that these age differences translate into large disparities in multiple skills, measured just before school entry.<sup>3</sup>

We find that children who start school with higher skills perform better on several in-school outcomes like GPA and test scores (0.20 standard deviations), measured from the 1st through the 4th grade. We also show that by the end of 4th grade parents are more likely to believe those children will complete post-secondary degrees, such as graduate school, college and technical careers (effect sizes of 13%, 4% and 2%). Parents do not invest time differentially, but reinforce initial skill gaps by investing an additional 0.11 standard deviation of financial resources in children with higher

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<sup>1</sup>Parental investments in children have been a topic of study for a long time (e.g., [Becker and Tomes, 1976](#) and [Behrman et al., 1982](#)), but interest has risen sharply in more recent years. See, e.g., [Francesconi and Heckman \(2016\)](#), [Doepke and Zilibotti \(2017\)](#) and [Attanasio et al. \(2020c\)](#). The recent literature on parental investments has studied beliefs about their returns (e.g., [Carneiro et al., 2019](#); [Boneva and Rauh, 2018](#); [Cunha et al., 2013](#)), information frictions ([Dizon-Ross, 2019](#)), time and budget constraints (e.g., [Bono et al., 2016](#), [Dahl and Lochner, 2012](#)), or preferences (e.g., [Beuermann and Jackson, 2020](#); [Bharadwaj et al., 2018](#)).

<sup>2</sup>Our paper complements the article by [McEwan and Shapiro \(2008\)](#) who studied in-school effects for first and fourth grades in Chile. While we replicate their results for those early outcomes, our paper contribution focuses on studying effects on different parental investments, long run outcomes up to age 27, and a human capital accumulation model relating both. More examples of the wide literature studying in-school outcomes with age-at-entry rules are [Attar and Cohen-Zada \(2018\)](#), [Bedard and Dhuey \(2006\)](#), [Calsamiglia and Loviglio \(2020\)](#), [Datar \(2006\)](#), [Elder and Lubotsky \(2009\)](#), [Fletcher and Kim \(2016\)](#), [Foureaux Koppensteiner \(2018\)](#), [Nam \(2014\)](#), [Peña \(2017\)](#), and [Smith \(2010\)](#).

<sup>3</sup>Among other factors, previous research has shown that older children have been exposed to more parenting time and are more mature than their younger peers and so can perform higher in cognitive test scores and can better develop different skills (e.g., [Black et al., 2011](#); [Deming, 2009](#); [Dhuey et al., 2019](#); [Lubotsky and Kaestner, 2016](#)).

skills at entry.

Following the same students over time, we find that early skill gaps lead to higher take-up (6 percent) and scores (0.08 standard deviations) in the national college entrance exam, and a higher probability of college enrollment, both overall (15 percent) and at selective programs (20 percent).

Leveraging large sample sizes, we are also able to estimate precisely how our results vary by socioeconomic background. All of our effects on financial investments and college enrollment are more pronounced for low-income children. The magnitude of the estimates on college enrollment are sizable and within those found by the early childhood interventions literature (Elango et al., 2015), suggesting that policy shocks on early skills can be as important as programs that are especially designed to bolster children’s capacities.

Our work adds to recent research connecting parental investments and beliefs (e.g., Biroli et al., 2020; Boneva and Rauh, 2018; Dizon-Ross, 2019; Attanasio et al., 2020a). Our findings on beliefs suggest that parents interpret the results on in-school performance as signals of their child ability, and adjust their investments according to their perceived return.

Our findings on the neutrality of parental time investments are consistent with the results by Bharadwaj et al. (2013) for Norway and Chile, in the context of an early health intervention. We add to these results by showing that financial investments respond differently to early skills gaps. These results are consistent with parents of high-performing children perceiving that the returns to monetary investments are higher than time investments.

Our paper also contributes to the literature that studies long term outcomes of early life disparities.<sup>4</sup> We track students for twenty years with repeated measures across time. Few studies are able to observe outcomes in the middle years of life (the ‘missing middle’ in Almond et al., 2018), which are important to fully understand effects. For instance, Heckman et al. (2006a), Elango et al. (2015) and Beuermann and Jackson (2020) highlight that the effects of early life disparities might fade out in the medium term but emerge in the long run. We measure relevant outcomes at ages 6 (GPA), 10 (test scores), 14 (primary school completion), 18 to 20 (high school completion and college entrance exams) and up to 27 (college completion).

Overall, our results suggest that parents respond to perceived signals of children’s ability in different ways depending on the type of investment. Those responses may reinforce early gaps with consequences on long-term educational outcomes. We argue that signals could disappear if adjusted by age, but parents and children themselves observe and react based on the unadjusted in-school differences. The natural policy implication of these findings is to provide parents both raw and age-adjusted measures of performance. Schools could also train teachers to communicate results to parents so that they take age into account when assessing children’s skills, particularly during the early school years when age-performance gaps are larger.

The remaining sections of the paper are organized as follows. In Section 2 we introduce a simple

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<sup>4</sup>We thus add evidence to a literature that is growing but still scarcer for developing countries. Most of the related evidence studying age-at-entry effects on long-run outcomes comes from developed countries, like Norway, Sweden or the United States. See, e.g., Black et al. (2011), Cascio and Schanzenbach (2016), Dobkin and Ferreira (2010), Fredriksson and Öckert (2013), Kawaguchi (2011), and Larsen and Solli (2017).

model of human capital accumulation and outline our empirical strategy. In Section 3 we describe the data. Section 4 presents the results and connects our main findings to the conceptual framework. Section 5 concludes.

## 2 Methods

In this section we outline a simple model of human capital accumulation that serves to understand how different types of parental investment might respond to early shocks. Then we describe the empirical strategy we use to estimate the causal effect of early disparities on children’s outcomes and parental investments.

### 2.1 Conceptual Framework

We present a conceptual framework that describes the mapping of early childhood shocks and parental investments into a child’s future human capital, building on multiple studies from the related literature (e.g., Almond et al., 2018, Boneva and Rauh, 2018, Cunha et al., 2010, and Francesconi and Heckman, 2016).

We consider a simple model with two periods, where the first period is childhood and the second period is child  $i$ ’s young adulthood. Child  $i$ ’s human capital in the second period is determined by the following production technology,<sup>5</sup>

$$h_{2i} = h(\theta_{0i}, I_{1i}^m, I_{1i}^t, \zeta_{1i}),$$

where  $\theta_{0i}$  represents endowed skills,  $I_{1i}^m$  are monetary investments made by parents of child  $i$  (such as school-related expenditures) in period 1,  $I_{1i}^t$  is child  $i$ ’s parent time investment (such as mentoring activities) in period 1, and  $\zeta_{1i}$  is a shock during childhood (e.g., a skill advantage in the first grade). We assume that  $h(\cdot)$  is differentiable, monotone, weakly increasing, and concave in  $I_{1i}^m, I_{1i}^t$ .

Parents have an expectation about the level of human capital that their child will achieve in adulthood, which depends on their beliefs about child  $i$ ’s skill endowment. We introduce these beliefs to point out that parents decide to invest considering their child’s expected human capital in adulthood, which may differ from the human capital that they finally acquire ( $h_{2i}$ ). Importantly, it may be the case that the shock  $\zeta_{1i}$  does not change the skill endowment of child  $i$ ,  $\theta_{0i}$ , but does change parents’ beliefs about it.

Parents choose optimal monetary and time investments ( $I_{1i}^m, I_{1i}^t$ ) in their child during the childhood period (e.g., spending additional time on educational activities or investing additional money on school-related items), to maximize their own utility subject to the production technology, and their own budget, and time constraints.<sup>6</sup>

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<sup>5</sup>Since the choice of the production function might govern the response to early childhood shocks (Almond et al., 2018), we do not presuppose a particular functional form for preferences or technology relating human capital to later outcomes. This allows different investments to vary in magnitude and sign as a response to early shocks.

<sup>6</sup>We explicit the complete model, with the constraints and objective functions in the Appendix.

Given these optimal investment decisions, the effect of an early shock on human capital in the next period can be decomposed as

$$\underbrace{\frac{\delta h_{2i}^*}{\delta \zeta_{1i}}}_A = \underbrace{\frac{\delta h(\cdot)}{\delta \zeta_{1i}}}_B + \underbrace{\frac{\delta h(\cdot)}{\delta I_{1i}^{*m}} \times \frac{\delta I_{1i}^{*m}}{\delta \zeta_{1i}}}_C + \underbrace{\frac{\delta h(\cdot)}{\delta I_{1i}^{*t}} \times \frac{\delta I_{1i}^{*t}}{\delta \zeta_{1i}}}_D$$

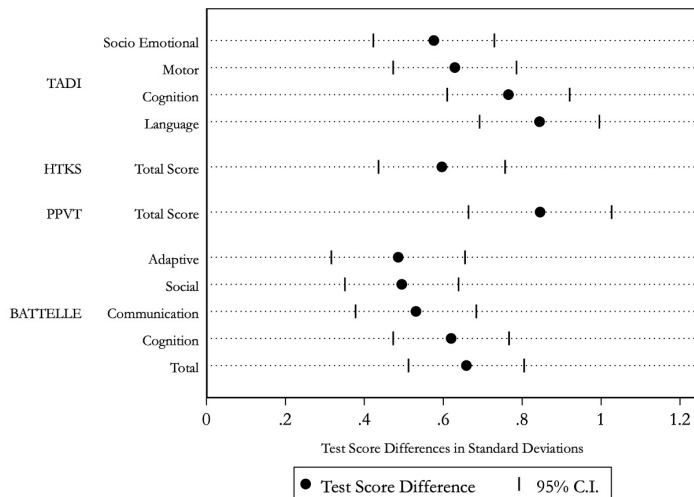
The total effect,  $A$ , equals to a direct effect of an early shock,  $B$ , which can be mitigated or reinforced through behavioral effects of different investment decisions,  $C$  and  $D$ . We assume that human capital is weakly increasing in investments, and so the sign of  $C$  and  $D$  is determined by how parental investments respond to the shock. A reinforcing investment decision is one that increases investment as a response to a positive shock, while a compensating strategy consists in parents increasing investment as a response to a negative shock.

Parents might respond to shocks differently by type of investment. We hypothesize that the response would differ by the productivity of each investment given the shock and socioeconomic background of the family. Our rich data and research strategy allows us to test these hypotheses in our empirical analysis. We outline our empirical strategy below.

## 2.2 Empirical Strategy

Our research design resembles a local experiment where children born days apart due to chance start primary school at different ages and thus with very different set of skills at school entry. In [Figure 1](#) we show that there are large differences between older and younger children in a host of tests, measured just before starting school. The age differences translate into skill gaps, measured in standard deviation units ( $\sigma$ ) that range from  $0.52\sigma$  to  $0.86\sigma$  on a battery of tests commonly used in the early childhood literature (e.g., [Rubio-Codina et al., 2016](#)).

**Figure 1:** Baseline Differences in Skills



Notes: [Figure 1](#) plots mean differences (with 95% confidence intervals) on a host of tests for July- and June-born children, measured just before they start their respective 1<sup>st</sup> grade. Due to the birth date cutoff rule, July-born children are about a year older than June-born children at school entry. The y-axis shows the measure of different tests and subjects measured. The TADI test is the *Test de Aprendizaje y Desarrollo Infantil*, a test developed by Chilean research centers that specialize in early life development measures. The HTKS test is the Head, Toes, Knees and Shoulders exam; the PPVT test is the Peabody Picture Vocabulary Test, and the BATTLE test corresponds to the Battelle Developmental Inventory for Young Children. The data comes from the *Encuesta Longitudinal de Primera Infancia* (ELPI), a nationally representative longitudinal survey that follows cohorts of children since birth until early youth.

Our empirical strategy takes advantage of the birth date cutoff rules in Chile, which states that prospective students who are not six years old by June 30 of the academic year should start in the next one.<sup>7</sup> We employ a regression discontinuity (RD) design using exact birth dates for children born in June and July to compare outcomes between children born days apart but with very different skill levels at school entrance.

Our identifying assumptions are standard for RD designs. Essentially, we assume that there are no other changes occurring at the threshold that could confound our analysis. In [Appendix A](#) we run a series of robustness tests showing that there are no differences in a host of different covariates at the cutoff and no evidence of manipulation of birth dates around the threshold, and our estimates are stable to using different bandwidths and specifications.

Our main estimating equation is

$$Y_i = \alpha_0 + \alpha_1 Z_i + f(B_i) + \alpha_2 X_i + \mu_i \quad (1)$$

The variable  $Z_i$  is equal to one if child  $i$  is born in July and is equal to zero if child  $i$  is born in June of the same year.  $f(B_i)$  is a function of birth date ( $B_i$ ) interacted with  $Z_i$  to allow for different slopes on each side of the cutoff. We cluster standard errors within birth date. We also include a set of

<sup>7</sup>The [National Law #1718](#) explicitly states that the rule is equally enforced in all schools (voucher and regular public schools), because both receive funding from the government. We expand on this topic in [Appendix C](#).

predetermined variables as controls in  $X_i$ , such as child gender, measures of household socioeconomic status, class size, school rurality, and type of school. All these control variables behave smoothly near the cutoff (see [Appendix A](#)) and serve mainly to improve precision of our RD estimates. We also add year of birth indicators to control for secular trends common to all children.

Our parameter of interest is  $\alpha_1$ , which is the *intention-to-treat* effect of starting school older—with a skill advantage—on the outcome  $Y_i$ . We restrict ourselves to these reduced-form effects and do not “scale up” our estimates instrumenting starting age with the threshold because in that case we would need the LATE additional assumptions to hold.<sup>8</sup>

We still consider carefully whether there could be a potential violation of the monotonicity assumption in our setup, as described in [Barua and Lang \(2016\)](#) and eloquently addressed in [Dhuey et al. \(2019\)](#). While monotonicity is a (directly) untestable assumption, we make three points aiming to moderate this concern.

First, we include a time trend at the daily level. Dates of birth are often only available aggregated at some level, say at a monthly, quarterly, or annual frequency level. In RD designs for these cases, outcomes in very narrow bins just to the right and left of the cutoff point cannot be compared ([Lee and Lemieux, 2010](#)), suffering a higher risk of monotonicity violations. In our setup, we feed our regression discontinuity design with *exact* birth dates, available at the day level. Aided by having many observations per day, we include a time trend at the daily level, which substantially reduces the potential of monotonicity violation because the estimated effect is computed at (an infinitely small approximation of) the threshold.

Second, our estimations include controls that resemble endowments. As [Dhuey et al. \(2019\)](#) highlight, potential monotonicity problems might also arise due to essential heterogeneity ([Heckman et al., 2006b](#)). Therefore, we control for proxies of endowments that might help address that essential heterogeneity, like proxies of socioeconomic status and measures of paternal and maternal schooling.

Third, we implemented a sibling fixed effects strategy. Even after controlling for socioeconomic status and parental schooling, there might be some other unobservables playing a role. To address this concern we gathered the necessary additional administrative records, and implemented a sibling fixed effects design to address this potential issue, as proposed by [Dhuey et al. \(2019\)](#).

We describe in detail the sibling fixed effects strategy, the data and report the results in [Appendix E](#). The estimates from the sibling fixed effects strategy are essentially the same as in our main specification. These results suggest that potential monotonicity violations are not playing an important role in our setup.

As a final methodological point, we estimate Equation (1) on many outcomes and therefore simultaneously test multiple hypothesis. To account for the probability of incorrectly rejecting one or more null hypotheses belonging to a family of hypotheses, we follow [Anderson \(2008\)](#) and adjust our standard errors controlling for the family-wise error rate. In the next section, we describe the rich administrative records that we use to implement our empirical strategy.

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<sup>8</sup>For instance, we would need to defend that the exclusion restriction holds in this setup. See [Jones \(2015\)](#) and [Dhuey et al. \(2019\)](#) for a discussion on this topic.



## 3 Data

We use administrative data provided by the Ministry of Education (MINEDUC) for the population of students in Chile supplemented with test scores, parental surveys, and student surveys. We link students across their entire school life using an encrypted national identification number and also follow them as they complete high school, take the college entrance exam, enroll in higher education, and graduate from college. We describe our data below.

### 3.1 Sources

Our primary data source comes from administrative datasets with yearly information on the population of students in primary school (1st to 8th grade) and high school (9th to 12th grade) since year 2002 and up to 2019. Each dataset provides individual data on exact birth date, gender and school characteristics, and in-school outcomes like GPA scores and passing rates.

We supplement these data with standardized test scores from the SIMCE (*Sistema de Medicion de Calidad Escolar*) exam, accompanied by parent and student surveys administered in the fourth grade. We use the surveys to measure parental investments, which we describe in detail in the next subsection. We further combine these data with three additional sources of information to measure long term outcomes. The first comes from the national college entrance exam (*Prueba de Selección Universitaria*, PSU) for years 2004 to 2018. The exam is taken at the end of the high school senior year and is required to get admitted to most universities in the country.<sup>9</sup> The second and third sources are further administrative datasets on higher education enrollment and graduation, respectively. The data is available for years 2007 to 2019. Each year, the MINEDUC collects information from all higher education institutions in the country and produces individual level lists of all students and graduates with information on major, area of study and institution.

### 3.2 Measures of Parental Investments and Beliefs

We use two surveys to measure parental investment. In one survey, parents provide information on investment in school-related items. For example, if they have a computer or Internet at home, the number of books they own, and the money they spend every month on their child’s education. We define binary indicators for these last two variables because they are reported in brackets. We label these variables “ten or more books” and “higher spending”, each of which equals one for half of respondents and zero otherwise. About half of parents report to spend more than 10K \$CH, which is the threshold we use as a proxy for “higher” spending.<sup>10</sup> Our results are robust to other ways of grouping items of investments, as we show in Appendix B.4.

In a separate survey, students are asked about the time spent with their parents on educational activities. In particular, children report in a 1-4 Likert scale whether their parents help them study

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<sup>9</sup>We signed an agreement with the agency in charge of developing and administering the exam (DEMRE), which provided us the data with the same encrypted identification number contained in the MINEDUC data.

<sup>10</sup>10K \$CH is the median of educational spending, and amounts for about one third of the per-student funding provided by the government.

or with their homework, help them understand difficult subjects, whether parents know their grades, and whether parents demand improving grades. Available answers for each item are on the scale of “Never”, “Sometimes”, “Most of the time”, and “Always”. We generate variables that equal one if the child answered that her parent does each activity “Always”, and zero otherwise. As with financial investments, our analyses are robust to how we group answers.

Figure 2 shows descriptive non-parametric plots of the raw data relating financial and time investment variables to test scores (in standard deviation units). Figure 2a shows that parental financial investments are positively correlated with test scores. Figure 2b also shows a positive correlation between test scores and measures of whether parents always know and congratulate children for their grades. The figure shows a flatter, slightly u-shaped correlation between test scores and measures of whether parents demand good grades, help with homework, and help them study.

We also generated two summary variables for each type of parental investment. The first is a simple average of all investments within type, which is plotted as ‘Average Index’ in Figure 2a and Figure 2b, respectively. The second is a ‘Factor Index’, computed using principal components, which reduces the dimensionality of the investment measures to one composite score. We provide details of the composite score computation in appendix B.3.

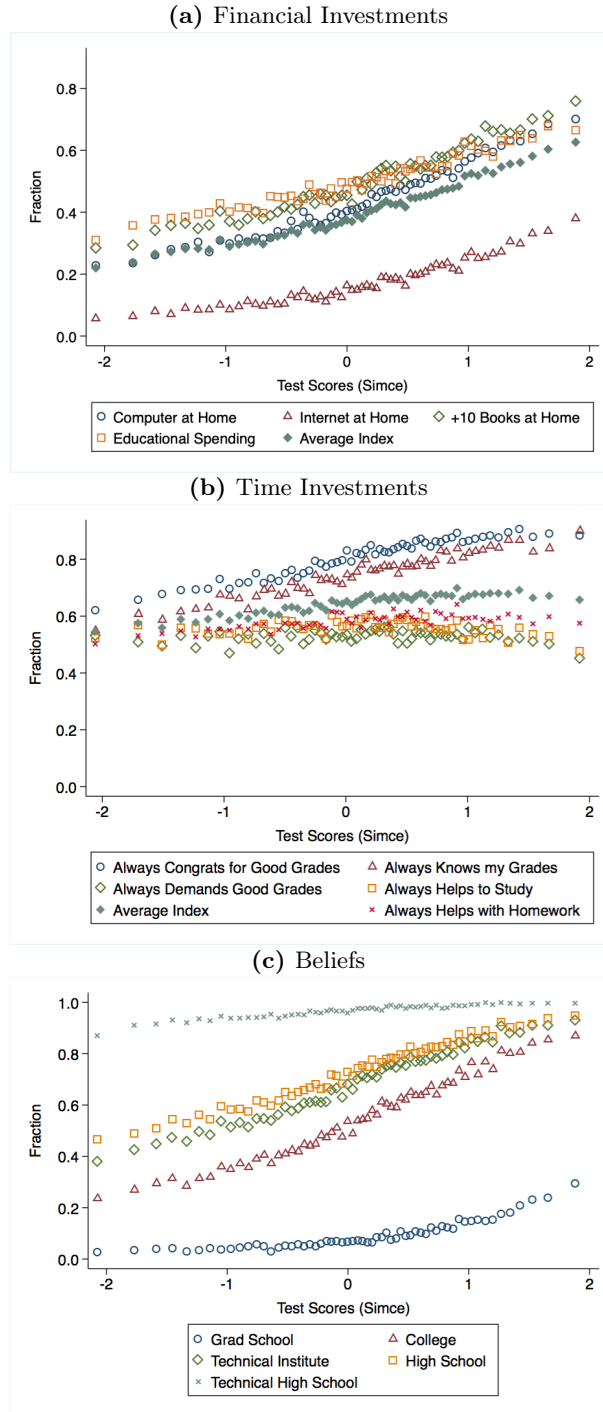
Parents also report their beliefs on their child’s educational attainment in the future. We plot their answers against test scores in Figure 2c. Each answer takes value one when the parent reports that the child will attain at least the respective level of education. The data shows a positive correlation between higher expected educational attainment and test scores. The lower correlation comes from parents who expect their child to complete at least technical high school diploma, because a high fraction (more than 90%) thinks children will reach that educational level.

### 3.3 Long Run Outcomes

Our main long run outcomes are take-up rates and scores of the national college entrance exam, college enrollment and college graduation. We construct take-up and scores of the entrance exam measuring them up to age 20 for first graders. The college entrance exam is taken by the end of high school senior year, when students are approximately 17 to 18 years old. While every year some test-takers are older (the test-taker median age is 19 years old), a very small fraction of all test-takers (less than 5 percent) take the test after turning 20 years old. Therefore measuring take-up at age 20 is a good proxy of ever taking the entrance exam.

We measure college enrollment similarly for first graders. We define college enrollment as the rate of students who enroll as freshmen in any college in the country. As for the entrance exam take-up, a very small fraction of freshmen are older than 20 years old. We also measure enrollment at selective institutions. The selective universities are non-profit institutions, grouped in the *Council of Rectors of the Universities of Chile* (CRUCH), which receive students with highest scores in the country. Finally we measure college graduation as the rate of students who graduated from any college in the country at age 27.

**Figure 2:** Parental Investments, Beliefs and Test Scores



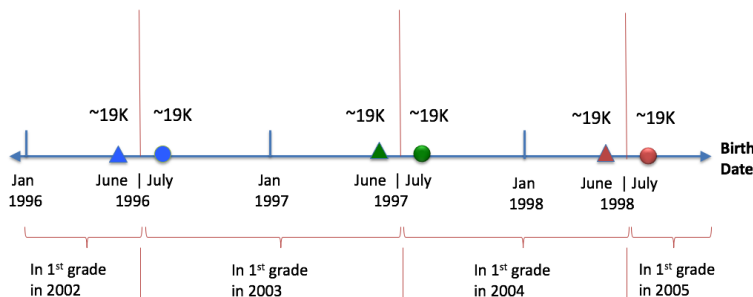
Note: The graphs in [Figure 2](#) plot measures of parental investments and beliefs within equal sized bins of 4th grade test scores (math-language average, in standard deviation units). The y-axis variables are all binary in [Figure 2a](#), [Figure 2b](#) and [Figure 2c](#). The variables in [Figure 2a](#) are our measures of parental financial investments (having a computer, Internet connection and more than ten books at home, and spending above the median on educational items). The variables in [Figure 2b](#) are our measures of parental time investments, and take value one if the child answered that her parent does each activity “Always” and zero otherwise. The variables in [Figure 2c](#) are our measures of parental expectations, where parents answer their beliefs about child’s expected educational attainment.

### 3.4 Working Sample

We study two sets of student cohorts. The first cohorts corresponds to first graders in years 2002 to 2005 (born between 1996 to 1998), and the second cohorts consist of eighth graders in the same years (born between 1989 and 1991). The data permits measuring outcomes up to age 20 for first graders because the youngest first graders were born in 1998 and we have data up to year 2019. Analogously, the younger eighth graders were born in 1991, and hence we can follow them until they are 27 years old in 2019.

We build our working samples for first graders and eighth graders as follows. After excluding the 7 percent of children enrolled in private schools who do not use the July 1 cutoff to enroll students, our administrative records contain approximately one million children in the first grade ( $N = 987,264$ ) and eighth grade ( $N = 1,048,983$ ). The first sample is composed of first graders born in June and July from years 1996 to 1998, and the second contains eighth graders born in June and July from 1989 to 1991. Figure 3 shows our research design for first graders. Those born in July just missed the cutoff date and therefore would start the first grade in the next academic year. For example, those born in June from 1996 would start the first grade in 2002, while those born in July of the same year start in 2003. We exploit the three discontinuities occurring between June and July from years 1996 to 1998 and pool our sample according to month of birth (June of July) in year  $T$ , and school starting date, in year  $T+6$  or year  $T+7$ . We control for cohort differences in our analyses.

**Figure 3:** Research Design for 1st Graders



Note: Figure 3 illustrates our research design for first graders. About 19K children were born in either June or July in years 1996 to 1998. According to the age at entry rule those born in June from year  $T$  should start the first grade in year  $T+6$ , while July-born children should start in year  $T+7$ .

Ideally, we would like to have birth records to avoid attrition between birth and first grade enrollment. In addition, if that attrition was differential by month of birth, it would also affect the internal validity of our analysis. We believe that neither is an important problem in the Chilean context because first grade enrollment is mandatory and compliance is very high nationwide. According to official vital statistics (MINSAL, 1996), the number of births was close to 21,000 each month for the years we study. If we exclude 7 percent of the children (those enrolled in private schools), the total

number of monthly births is very close to our sample of 19,000 per month. In addition, the same source indicates that the number of births was evenly distributed by month of birth, as we also find in our data with first-grade enrollment.

### 3.5 Summary Statistics

**Table 1** presents mean characteristics for students in our working samples. Column (1) presents values for our working sample of first graders, while column (2) does the same for the total population of first graders as a benchmark. Columns (3) and (4) describe eighth graders analogously. Overall, **Table 1** shows that our working samples and the student population are fairly similar in a host of individual and school baseline characteristics, suggesting that results using our working samples are not prone to external validity bias ([Andrews and Oster, 2019](#)). **Table 1** also suggests that births are uniformly distributed by month because half of the students in our working samples were born in June and half in July, and the fraction born each month is 8 percent of the respective benchmark population. This result is consistent with the fact that each month of a given year accounts for approximately 8.3 percent of the births.

**Table 1:** Summary Statistics

Variable	(1)	(2)	(3)	(4)
	1st Graders Sample	All	8th Graders Sample	All
Father's Schooling	10.95	10.89	N.A.	N.A.
Mother's Schooling	10.88	10.82	N.A.	N.A.
Girl	0.49	0.49	0.51	0.50
Class Size	30.27	30.12	32.80	32.35
School Vulnerability (0-100)	29.84	30.00	28.29	29.74
School in Capital Region	0.37	0.37	0.37	0.37
Rural School	0.14	0.14	0.11	0.13
Public School	0.52	0.53	0.57	0.59
Voucher School	0.48	0.47	0.43	0.41
Observations	117,709	987,264	111,664	1,048,983
F-test (p-value)	0.621		0.826	

Notes: **Table 1** shows the mean of each variable in rows. The first two columns describe students in first grade in 2002 to 2005. Column (1) presents values for our working sample of first graders, while column (2) does the same for the full population of 1st graders as a benchmark. Columns (3) and (4) describe 8th graders analogously. The measures of parental schooling refer to years of completed education and come from SIMCE surveys that have a response rate of 75% for both our working sample and all first graders. Parental schooling is not available for eighth graders since there was no SIMCE survey implemented in the '90s, when these students were in the fourth grade. The school vulnerability index measures percentage of students receiving free or reduced price lunch. The schools type is either public or voucher. The last row reports the p-values of the F-test for joint significance of all variables, between the samples and all students not born in June or July.

The descriptive statistics situate the sample in a context of a middle-income country. For instance, average parental schooling is close to 11 years, which is less than the 12 years needed to get a

high school diploma. Levels of schooling in Chile are higher today,<sup>11</sup> but our data describe students and their parents about 15 years ago, when the country exhibited lower levels of development. The average class size for first graders is about 30 students and 32 students for an average eighth grade class, which again are similar to rates in developing countries. For reference, at about the same time (in the mid-2000s), the class size in primary school was 21 in the US and 27 in Turkey ([OECD Stats 2019](#)).

The data also show that about half of the sample are girls, and students attend schools with a vulnerability index close to 30, on average. This index ranges between 0 and 100 and resembles the percentage of students receiving free or reduced-price lunch, similar to the index often used in the US as a proxy for poverty. In Chile, this index is computed by the government agency responsible for school meal programs (National School Assistance and Scholarship Board, JUNAEB).<sup>12</sup> Approximately 37 percent of the students attend schools within the Metropolitan Region, which includes the national capital, Santiago, and 14 percent of first graders and 11 percent of eighth graders attend schools in rural areas. Finally, 52 percent of first graders and 57 percent of eighth graders attend public schools, with the remaining fraction attending voucher schools.<sup>13</sup>

## 4 Results

We start by briefly presenting results on in-school outcomes to focus then on the effects on parental investments and beliefs. Next we describe our estimates on long run outcomes, and finish the results section discussing our findings by socioeconomic status.

### 4.1 In-school Effects

In [Figure 4](#) and [Table 2](#) we present effects on in-school outcomes for our sample of first graders. July-born children start the first grade 0.48 years older than their June-born counterparts (see [Figure 4a](#)) and so are more likely to enjoy a skill advantage over those who start younger, as discussed previously and depicted in [Figure 1](#). [Figures 4b](#), [4c](#), and [4d](#) show that a skill advantage in the first grade translates into higher GPAs ( $0.26\sigma$ ) and higher passing rates (2.4 percentage points, pp) in the first grade and higher test scores ( $0.21\sigma$ ) in the fourth grade.

These results on in-school outcomes are consistent in direction and magnitude with the related literature. In the next sections we complement the findings on educational outcomes exploring how parents react to these perceived signals of children ability. We then supplement the short run effects during school with estimates of long run outcomes.

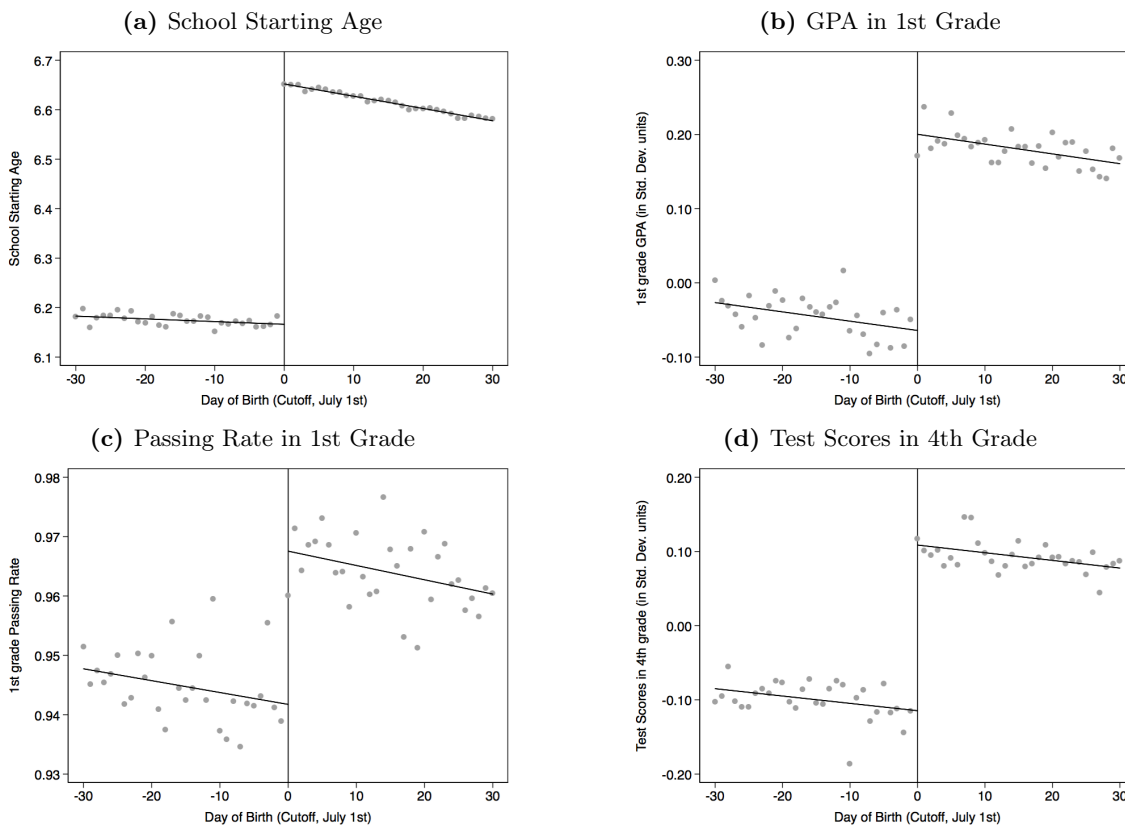
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<sup>11</sup>Chile has reached almost universal levels of educational coverage in primary (99 percent) and secondary school (92 percent), well above Latin American countries (Unesco-OECD 2010; IDB 2018).

<sup>12</sup>For details see [JUNAEB \(2019\)](#).

<sup>13</sup>Public schools are both publicly funded and administered. Voucher schools receive public funding but are privately managed, similar to charter schools in the US.

**Figure 4: In-school Effects**



Note: The graphs in Figure 4 plot the mean of the y-axis variable within day of birth, and fit estimated lines using all the underlying data, allowing for different slopes on each side of the cutoff. Each day of birth contains about 2K observations. Table 2 shows the results from the estimation of equation (1) for each of these outcomes.

**Table 2: In-school Effects**

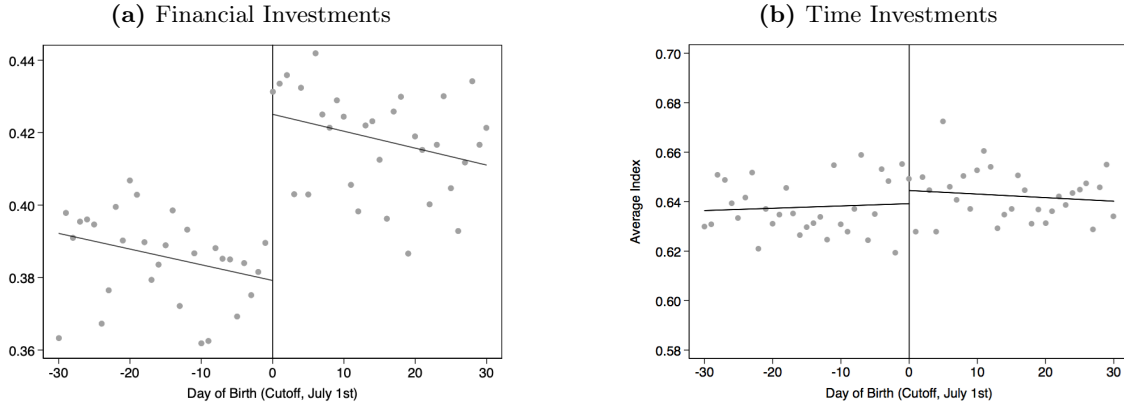
	(1)	(2)	(3)	(4)
	Age at Entry	GPA in 1st Grade	Pass Rate in 1st Grade	Test Scores in 4th Grade
$\hat{\alpha}_1$	0.482*** (0.003)	0.263*** (0.012)	0.024*** (0.003)	0.207*** (0.010)
June Mean	6.169	-0.059	0.944	-0.111
Observations	117,709	117,709	117,709	117,709

Notes: Table 2 shows the coefficient  $\hat{\alpha}_1$  estimated from the equation (1) for first graders on age at school entry, and in-school outcomes. The ‘June Mean’ is the mean of the dependent variable just below the threshold. The outcomes are first grade GPA (standardized within school and grade) and pass rate, and the Language-Math score in 4th grade. All estimations include cohort fixed effects and control for child gender, class size, school vulnerability, school rurality and type of school (public or voucher). Robust standard errors (in parentheses) are clustered by day of birth.

## 4.2 Effects on Parental Investments and Beliefs

*Investments.* Figure 5 summarizes our main findings on parental investments. Figure 5a shows that July-born children receive 3.4pp ( $\sim 0.11\sigma$ ) of additional financial investments, while Figure 5b shows no effects on time investments. Going back to our conceptual framework, these findings suggest that parents reinforce skill gaps using financial investments, but do not use time investments to respond to differences in school performance.

**Figure 5:** Effects on Parental Investments



Note: The graphs in Figure 5 plot the mean of the y-axis variable within day of birth, and fit estimated lines using all the underlying data, allowing for different slopes on each side of the cutoff.

We present the corresponding point estimates in Table 3. In Appendix B (sections B.1 and B.2) we provide details on sample sizes and robustness checks. Panel A in Table 3 shows the results for financial investments. Column (1) shows the effect on the average index, plotted in Figure 5a. Parents increase average financial investments by 3.4 pp, which represents an effect of  $0.11\sigma$ . In column (2) we show the effect on the factor index which shows the same effect of  $0.11\sigma$ . Next, columns (3)-(6) show the effects for each investment variable separately. July-born children are 10 percent (4.3 pp over 40%) more likely to have a computer at home, 20 percent (3.2 pp over 16%) more likely to have an Internet connection, and 8 percent (3.7 pp over 48%) more likely to have ten or more books at home. In addition to investing in more educational assets, parents are 5 percent (2.2 pp over 48%) more likely to spend above the median of monthly expenditures for school items in our sample.

We examine parental responses in terms of time investments in Panel B of Table 3. Column (1) shows that the estimate on the average index, plotted in Figure 5b, is a precise zero (0.4 pp over a mean of 64%). The factor index effect is of  $0.01\sigma$  and not different from zero, as shown in column (2). These results are consistent with the much smaller if not zero correlation between the index of time investments and test scores in Figure 2b.

When revising effects for each variable in particular, we also find precise zero effects in whether parents congratulate their child for good grades (mean of 81%) and whether they know their grades (75%). This is despite the strong positive correlation between these variables and test scores shown in Figure 2b. We also find precise zeros in whether parents demand good grades (mean of 53% for



both groups) and whether they help with study (56%). Finally, we find a small positive effect on whether parents help with homework (2pp over a mean of 57%).<sup>14</sup>

**Table 3:** Effects on Parental Investments

<b>Panel A: Financial Investments</b>						
	(1)	(2)	(3)	(4)	(5)	(6)
	Average Index	Factor Index	Computer at Home	Internet at Home	More 10 Books	High Spending
$\widehat{\alpha}_1$	0.034*** (0.005)	0.108*** (0.016)	0.043*** (0.008)	0.032*** (0.006)	0.037*** (0.008)	0.022*** (0.007)
June Mean	0.379	-0.070	0.404	0.158	0.478	0.478
Effect Size	0.107	0.110	0.107	0.204	0.078	0.047
Observations	51,818	51,818	51,818	51,818	51,818	51,818

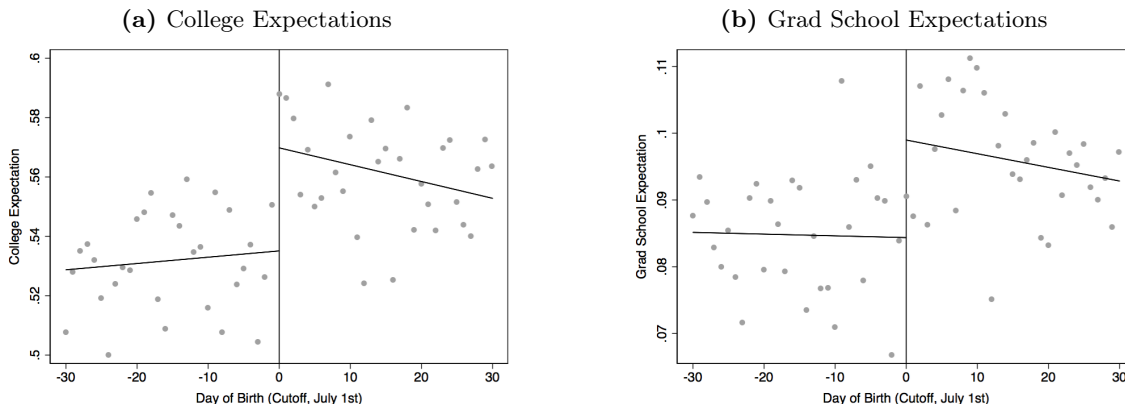
<b>Panel B: Time Investments: ‘My parent always...’</b>							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Average Index	Factor Index	Congrats for good grades	Knows my grades	Demands good grades	Helps to study	Helps with homework
$\widehat{\alpha}_1$	0.004 (0.006)	0.013 (0.022)	-0.010 (0.006)	0.010 (0.010)	0.001 (0.012)	0.001 (0.009)	0.018** (0.009)
June Mean	0.643	0.011	0.810	0.752	0.526	0.559	0.569
Effect Size	0.015	0.014	-0.012	0.013	0.002	0.001	0.032
Observations	47,646	47,646	47,646	47,646	47,646	47,646	47,646

Notes: [Table 3](#) shows the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) on measures of parental financial investment (Panel A), and parental time investments (Panel B), previously presented in [Figure 2a](#) and [Figure 2b](#). The ‘June Mean’ is the mean of the dependent variable just below the threshold, and the ‘Effect Size’ corresponds to  $\widehat{\alpha}_1/(\text{June Mean})$  for binary outcomes, and to  $\widehat{\alpha}_1/(\text{standard deviation below the threshold})$  for non-binary outcomes. The dependent variables are described in [subsection 3.2](#). All estimations include cohort fixed effects and controls for child gender, class size, school vulnerability, school rurality and type of school (public or voucher). Robust standard errors (in parentheses) are clustered by day of birth.

*Beliefs.* Parents of July-born children also have higher expectations about their child’s educational future, driven by beliefs on children completing postsecondary degrees as we show in [Table 4](#) and [Figure 6](#). Parents are more likely to believe that their child will complete college (4 percent, or 2.1 pp over 53%), grad school (13 percent, or 1.2pp over 8.7%), and a 4-year degree at a technical institute (2 percent, or 1.3 pp over 67%). We found no effects on beliefs on high school completion or technical high school completion (columns (5) and (6)). We hypothesize that these expectations about high school graduation are harder to move because they are already set at a high level.

<sup>14</sup>It is possible that more refined information (e.g., a more concise time use survey) would help us in confirming that parents do not change their time investments at the intensive margin.

**Figure 6:** Effects on Parental Beliefs



Note: The figures in [Figure 6](#) are graphical analog to estimates in columns (1) and (2) in [Table 4](#). Each graph plots the mean of the y-axis variable within day of birth, and fit estimated lines using all the underlying data, allowing for different slopes on each side of the cutoffs. The dependent variables are described in [subsection 3.2](#).

These empirical findings are consistent with beliefs responding positively to signals about the child’s ability, which parents have been receiving between 1st and 4th grade. As documented in [subsection 4.1](#) the July-born children started by chance, with a skill advantage over those who started younger, and we know that parents are aware. Survey data from SIMCE indicates that 75 percent of students report that their parents know their grades (Panel B, column (4) in [Table 3](#)). The fact that parents of July-born children are mostly aware that they perform relatively well (and the opposite for parents of June-born children) seems to translate into higher expectations about their educational long term outcomes.

**Table 4:** Effects on Parental Beliefs

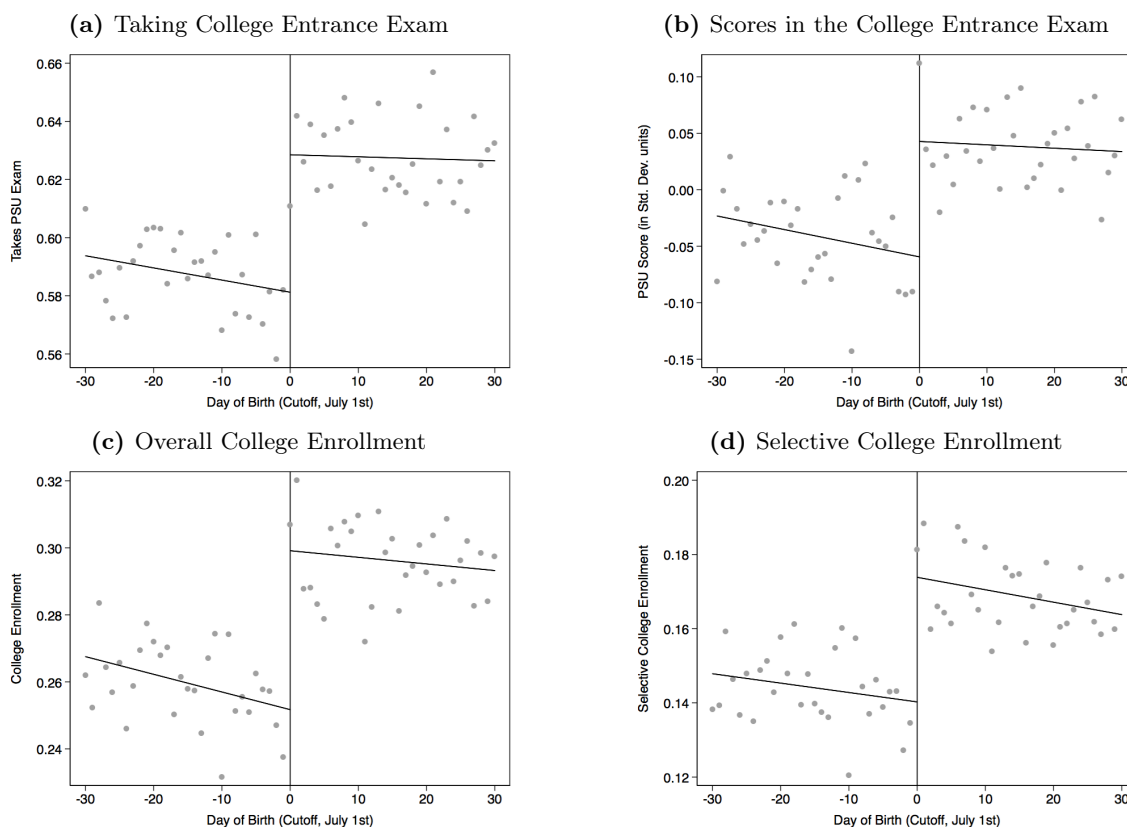
	(1)	(2)	(3)	(4)	(5)
	College	Grad School	Institute	High School	Tech HSchool
	Expectation	Expectation	Expectation	Expectation	Expectation
$\widehat{\alpha}_1$	0.021*** (0.007)	0.012** (0.005)	0.013* (0.008)	0.013 (0.008)	0.003 (0.003)
June Mean	0.530	0.087	0.666	0.709	0.963
Effect Size	0.039	0.133	0.020	0.019	0.003
Observations	51,818	51,818	51,818	51,818	51,818

Notes: [Table 4](#) shows the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) on measures of parental expectations described in [Figure 2c](#). The ‘June Mean’ is the mean of the dependent variable just below the threshold, and the ‘Effect Size’ corresponds to  $\widehat{\alpha}_1$ /(June Mean) for binary outcomes. The dependent variables are described in [subsection 3.2](#). All estimations include cohort fixed effects and controls for child gender, class size, school vulnerability, school rurality and type of school (public or voucher). Robust standard errors (in parentheses) are clustered by day of birth.

### 4.3 Long-Run Outcomes

Figure 7 summarizes our main effects on long run outcomes. Following up children until they are 20 years old, we find that the early skill gaps lead to higher college entrance exam take-up and scores, and higher college enrollment rates both overall and at selective programs.

Figure 7: Effects on Long Run Outcomes



Note: The graphs in Figure 7 plot the mean of the y-axis variable within day of birth, and fit estimated lines using all the underlying data, allowing for different slopes on each side of the cutoff. Each day of birth contains about 2K observations.

We show our point estimates in Table 5. Column (3) presents the estimate corresponding to Figure 7a showing that July-born students are 6 percent (3.6 pp over a mean of 58%) more likely to take the national college entrance exam.

Conditional on taking the test, students with an early skill advantage score  $0.08\sigma$  higher, as shown in Figure 7b and in column (4) of Table 5. We interpret this effect as a lower bound because, among non-test-takers, those who start school with a skill advantage would arguably have performed better had they taken the test. In any case, if there are positive effects on the college entrance exam (taking the exam or scoring higher), these should translate into effects on college enrollment.

The next results show indeed effects on multiple measures of college enrollment, reported in columns (5) to (8) in Table 5. We find a 15 percent increase (3.7 pp over a mean of 25%) in college enrollment (see Figure 7c) for July-born students, which is consistent with the positive effects on the

likelihood of being a test-taker. Meanwhile, the increases on enrollment at more selective universities (20 percent; 2.8 pp over a mean of 14%, see [Figure 7d](#)) and STEM programs (14 percent; 1 pp over a mean of 7%) are consistent with higher scores on the college entrance exam.

**Table 5:** Effects on Long Run Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Primary Grad	High-School Grad	PSU Exam	PSU Score	College Enroll	Selective Enroll	STEM Enroll	College Grad
$\widehat{\alpha}_1$	0.013*** (0.003)	0.004 (0.005)	0.036*** (0.006)	0.084*** (0.017)	0.037*** (0.005)	0.028*** (0.004)	0.010*** (0.003)	0.007* (0.004)
June Mean	0.912	0.684	0.580	-0.067	0.253	0.138	0.069	0.165
Effect Size	0.014	0.006	0.063	.	0.147	0.203	0.141	0.042
Observations	117,709	117,709	117,709	71,509	117,709	117,709	117,709	111,664

Notes: [Table 5](#) show the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) on a host of outcomes. The ‘June Mean’ is the mean of the dependent variable just below the threshold, and the ‘Effect Size’ corresponds to  $\widehat{\alpha}_1/(\text{June Mean})$  for binary outcomes. [Table 5](#) presents results for our sample of first graders in columns (1) to (7) and for eighth graders in column (8). All the dependent variables are outcomes measured at age 20, except the last outcome, measured at 27. ‘Primary Grad’ and ‘High-School Grad’ are primary and high-school graduation rates, respectively; ‘PSU Exam’ and ‘PSU Score’ measure whether children took the college entrance exam and their scores if they did. ‘College Enrollment’, ‘Selective Enroll’ and ‘STEM Enroll’ measure whether children enrolled at any college, at selective institutions and at STEM majors, respectively. ‘College grad’ indicates whether the children graduated from college. All estimations include cohort fixed effects and control for child gender, class size, school vulnerability, school rurality and type of school (public or voucher). Robust standard errors (in parentheses) are clustered by day of birth.

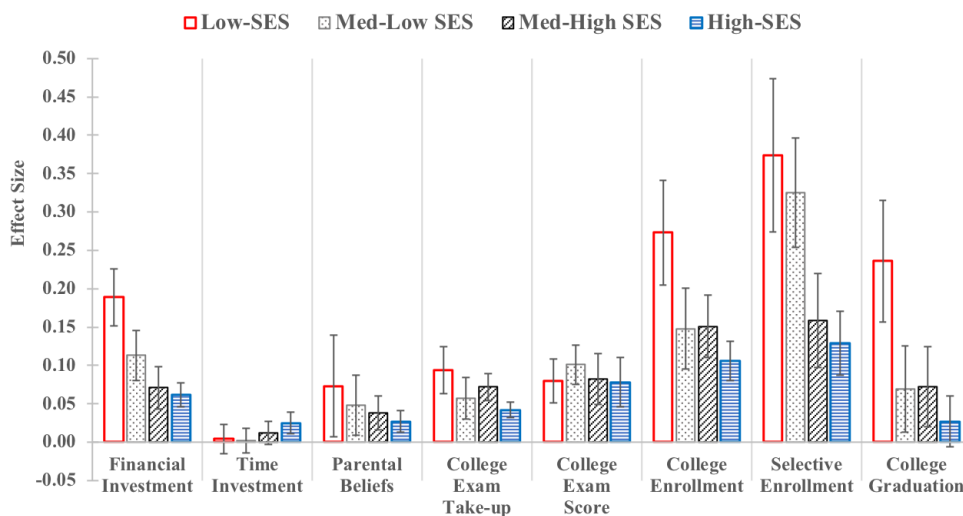
We finally use our sample of eighth graders to estimate effects on college graduation later on. The last column of [Table 5](#) shows a precisely estimated effect of 0.7 pp, which represents an effect size of 4 percent (0.7 pp over a mean of 16.5%). On average, a share of 16.5 percent of both June- and July-born students obtain a college degree by age 27. These rates are similar to back of the envelope computations from official reports by MINEDUC on higher education completion rates ([MINEDUC, 2019](#)).

## 4.4 Effects by Socioeconomic Background

Figure 8 and Table 6 summarize our main results on parental investments, beliefs, and children long run outcomes by socioeconomic status (SES). The main takeaway is that effect sizes on financial investments and college related outcomes are larger for lower SES children with a clear negative SES gradient in most outcomes.

We divide the sample of students into quartiles of the school vulnerability index and label each quartile as low SES, med-low SES, med-high SES, and high SES, accordingly. In this section we present our findings using effect sizes as they are informative of the relative importance of the effects for each SES group. We show results for financial and time investments using the respective average index described in subsection 3.2, and using college completion expectations for parental beliefs. In Appendix C we include results for each individual measure of investment and beliefs, that behave similarly to the summary measures presented here. Long run outcomes consist on the college entrance exam take-up and scores, college enrollment (overall and at selective institutions), and college graduation.

**Figure 8:** Effect Sizes by Socioeconomic Status



Note: Figure 8 plots effect sizes (with their standard errors) on parental investments, beliefs, and children’s long run outcomes by quartiles of the school vulnerability index. Financial and time investments are each measured by the average index described in subsection 3.2. Parental beliefs are measured as college completion expectations. The long run outcomes are the college entrance exam take-up and scores, college enrollment (overall and at selective institutions), and college graduation.

*Parental Investments and Beliefs.* The effects on parental financial investments display a steep, negative SES gradient. The effect sizes are three times as large for the low SES versus the high SES group (19 vs 6 percent). Our estimates on time investments are close to zero with no distinguishable differences across groups. This result suggests that the average null effect shown in Table 3 did not hide differential effects by socioeconomic status.

Effects on parental beliefs, measured as college expectations, are decreasing by socioeconomic status but measured noisily. There is a difference of 4.6 percentage points between the low and high

SES groups that we cannot distinguish from zero.

*Long-Run Outcomes.* Our estimates on the college exam take-up show an effect of 9 percent for low SES students, about 6 percent for students in the med-low and med-high group and 4 percent for high SES students. Test scores increase similarly by about  $0.08\sigma$  for each group, but from a lower baseline score for the more disadvantaged SES groups (i.e.,  $-0.42\sigma$  for low SES versus  $0.37\sigma$  for high SES). These results suggests that we should see a relatively larger effect on college enrollment for lower SES students.

Consistently, for college enrollment, we find effect sizes of 27 percent for the low SES group, 15 percent for the med-low and med-high SES and 11 percent for the high SES group. This pattern is also present in our measures of enrollment at selective programs, with effects of 37% for low SES and 13% for high SES students. Finally, the results also show that the null average effect on college graduation in [Table 5](#) was masking large SES differences. Low SES students with a skill advantage are 23% more likely to graduate from college, versus a noisily estimated 3% for the higher SES group

**Table 6:** Effect Sizes by Socioeconomic Status

Outcome	(1)	(2)	(3)	(4)	(5)
	Low SES	Med-Low SES	Med-High SES	High SES	Low vs High Difference
<b>Financial Investments</b>	0.189***	0.113***	0.071***	0.062***	0.127***
Average Index	(0.0372)	(0.0330)	(0.0274)	(0.0155)	(0.040)
<b>Time Investments</b>	0.004	0.002	0.012	0.025*	-0.021
Average Index	(0.0194)	(0.0157)	(0.0151)	(0.0144)	(0.024)
<b>Parental Beliefs</b>	0.073	0.048	0.038*	0.027*	0.046
College Expectation	(0.0663)	(0.0393)	(0.0217)	(0.0144)	(0.068)
<b>Long Run Outcomes</b>					
Takes PSU Exam	0.094***	0.057**	0.072***	0.042***	0.052
	(0.0307)	(0.0273)	(0.0174)	(0.0100)	(0.032)
PSU Exam Scores	0.080***	0.101***	0.082**	0.078**	0.002
	(0.0286)	(0.0256)	(0.0333)	(0.0322)	(0.043)
College Enrollment	0.273***	0.148***	0.151***	0.106***	0.167**
	(0.0680)	(0.0525)	(0.0403)	(0.0255)	(0.073)
Selective College Enrollment	0.374***	0.325***	0.159***	0.129***	0.245**
	(0.1001)	(0.0713)	(0.0612)	(0.0419)	(0.109)
College Graduation	0.236***	0.069	0.072	0.027	0.209**
	(0.0792)	(0.0563)	(0.0522)	(0.0335)	( 0.086)

Notes: [Table 6](#) shows the effect sizes on parental investments, beliefs, and children long run outcomes by quartiles of the school vulnerability index. We label each quartile as low SES, med-low SES, med-high SES, and high SES, in columns (1)-(4). Column (5) show the difference between the low and high SES effect size. Standard errors for the effect sizes are computed using the delta method.

## 4.5 Are Parental Investments and Beliefs Mediating Long Run Results?

**Conceptual Framework.** Our empirical results show that the long-run effects of an early skill advantage are positive and large. For example, the probability of college enrollment increases by 15 percent overall. In our conceptual framework, we decompose the global effect ( $A$ ) into a direct effect component ( $B$ ) and two components related to monetary and time investments,  $C$  and  $D$  respectively. Our findings show that monetary investments reinforce the shock (i.e.,  $C$  is positive) while time-intensive investments are neutral (i.e.,  $D$  is zero). This result suggests that the overall effect of a skill advantage at school entry,  $A$ , is explained by a direct effect and by reinforcing financial investments from parents,  $B + C$ .

In our model, optimal investments of parents are a function of beliefs about their child’s abilities. Our empirical finding shows that parents have higher beliefs about their child’s future human capital if she benefited –by chance– from an early skill advantage. This result is consistent with parents interpreting in-school performance as signals of their child ability, and adjusting their investments according to their perceived productivity.

Such mechanism is in line with [Attanasio et al. \(2020b\)](#) who show that investments vary according to parental beliefs on heterogeneity of returns to such investments. In our context, parents may perceive that investing in books, computer, or other school related materials can be productive to complement the skills of children with higher grades. Our null results on time-intensive investments suggests that parents perceive their productivity to be similar for children with different skills at school entry.

**Mediation Analysis.** We further explore to what extent parental beliefs and investments might be mediating long run effects, applying methods from the latest related literature ([Fagereng et al., 2021](#)), building on [Heckman et al. \(2013\)](#).

As described elsewhere (see, e.g., [Almond and Mazumder, 2013](#)) we readily acknowledge that identifying the interactions between shocks and investments would require an exogenous variation on investments. Examples of recent papers studying these type of interactions are [Duque et al. \(2019\)](#), [Johnson and Jackson \(2019\)](#), [Malamud et al. \(2016\)](#), and [Rossin-Slater and Wüst \(2019\)](#).<sup>15</sup>

We follow [Fagereng et al. \(2021\)](#) making explicit assumptions in a mediation model.<sup>16</sup> We adapt their model of mediation to our setup, and empirically compute the parameters associated to our mediators. While we see this exercise as mostly descriptive, it provides an approximation of how much different channels (investments, beliefs and the skill gap itself) might be contributing to differences in outcomes.<sup>17</sup> In [Appendix D](#) we write our mediation model and describe the estimation results in detail.

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<sup>15</sup>In the absence of an additional instrument to correct for endogenous parental behavior, other papers jointly model parental behavior and interventions or shocks in structural models used to isolate parameters of parental behavior from the human capital production function. Examples of recent papers in this area are [Attanasio et al. \(2020b\)](#) and [Jervis \(2017\)](#).

<sup>16</sup>We thank the authors for sharing their programs and code.

<sup>17</sup>We thank the editor and referees for this suggestion.

We find that our mediator variables (parental investment and beliefs) explain nearly 34% of the causal effect on college enrollment, while 66% is explained by the direct effects of the early skill gap (see panel (a) in [Figure 9](#)). As a benchmark, [Fagereng et al. \(2021\)](#) find that their mediators (mainly parental wealth transfers) explain about 37% of the causal effect of having wealthier parents on children’s accumulation of wealth.

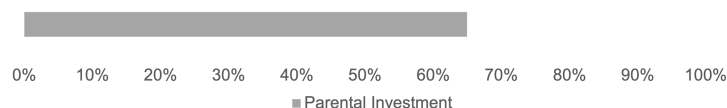
Parental investments account for almost two thirds of the mediated effect (see panel (b) in [Figure 9](#)), indicating that they account for a sizable fraction of the effect on children’s long run outcomes. The last section of [Appendix D](#) shows that these results are robust to imposing a range of different assumptions on the mediation model.

**Figure 9:** Decomposition of Causal Effects

(a) Direct and Indirect (Mediated) Effects as a Share of the Causal Effect



(b) Parental Investments as a Share of the Indirect (Mediated) Effect



Note: Panel (a) in [Figure 9](#) decomposes the causal effect of starting school later on college enrollment into shares of direct effect and an indirect effect (mediated by investments and beliefs). Panel (b) shows how much of the indirect effect can be attributed to financial investments made by parents.

**Children within Same Families.** We also study whether our main results are driven by family characteristics. The logic is that even after controlling for socioeconomic status and parental schooling, there might be some other unobservables playing a role in our estimated effects on parental investments, beliefs and long run outcomes.

We gathered the necessary additional administrative records, and implemented a sibling fixed effects design to address this potential issue, analog to [Dhuey et al. \(2019\)](#). In [Appendix E](#) we describe in detail the data and the sibling fixed effects strategy, and report the results. The estimates from the sibling fixed effects strategy are essentially the same as in our main specification, suggesting that our main results are not driven by family characteristics.<sup>18</sup>

<sup>18</sup>These results also suggest that potential monotonicity violations are not playing an important role in our setup. The findings are consistent with [Dhuey’s et al. \(2019\)](#), who write that “...it is clear that controlling for family characteristics and behavior does not substantially affect the estimated relationship between school starting age and test scores.” (page 13, second-to-last paragraph).



## 5 Conclusion

This paper studies the effects of having an early skill advantage at school entry on different types of parental investments, beliefs and long-term outcomes. We combine rich administrative records with a regression discontinuity design in a middle-income country, and interpret our results within a human capital accumulation model.

Parents have stronger beliefs about the future prospects of children who -by chance- started school older and are therefore perceived as of higher skills. While parents do not invest time differentially, they do reinforce the initial skill gaps investing additional material resources in children benefited from the early skill advantage. These reinforcing investments are more pronounced for children from lower socioeconomic background.

We further document that the early skill advantage translates into higher rates of college enrollment, with larger effects for low socioeconomic status students as well. The magnitude of these effects are within those found by the early childhood interventions literature (see, e.g., [Elango et al., 2015](#)) suggesting that policy rules affecting early skills can be as important as programs designed to bolster children’s capacities.<sup>19</sup> Overall, these results add to the literature by emphasizing that early skill gaps can have sizable and heterogeneous consequences in adulthood.

Our results have direct policy implications. Age-at-entry rules may generate noisy signals of skill because high-ability young starters might signal low ability. Due to age, they perform worse on tests early on in school, and hence receive less investments than their older, high-ability equivalents. These noisy signals could disappear if adjusted by age, but parents and children themselves observe and react based on the unadjusted in-school differences. The most direct policy recommendation would be to make parents aware of the age-differences, providing them with both raw and age-adjusted measures of children’s performance.

From a broader perspective, our results suggest that if parents simply perceive that their children are higher ability, then they will invest more. This behavior shows up in the school starting age differences, but could also generalize to, for example, having a teacher that is encouraging and positive about a child versus a different randomly assigned teacher who is more negative about the child’s skills. Schools could train teachers to communicate and help parents interpret the assessments of their child’s skills, particularly during the early school years.

Finally, our findings underscore the importance of advancing the research agenda connecting beliefs, and types of parental investments with long run outcomes.

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<sup>19</sup>[Elango et al. \(2015\)](#) provide an excellent review of these programs. For Head Start, [Currie et al. \(2002\)](#) show that there is a marginally significant increase in nine percent in the probability of attending college when they compare Head Starters to non-Head Starters. Likewise, [Ludwig and Miller \(2007\)](#) find that Head Start increases the likelihood of attending college by five percent. [Deming \(2009\)](#) shows that the same program increases the probability of attending college by ten percent approximately. [Anderson \(2008\)](#) shows that participants of the Perry Preschool Program are 21 percent more likely to attend any college.

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# Appendices

## A Appendix: Robustness

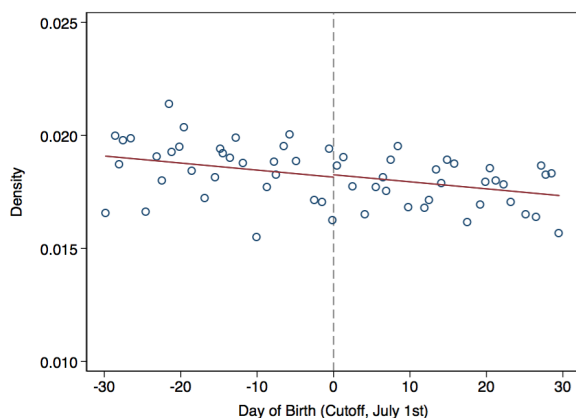
### A.1 Density of Running Variable

We test for differences in unobserved characteristics by examining whether there is manipulation of birth dates near the cutoffs in our data. For example, it could be the case that more motivated parents planned the timing of their children’s birth in order for them to be older when enrolling in primary school. If parents consider school starting age rules when timing conceptions or births dates by scheduling C-sections, for instance, our results would be subject to manipulation and sample selection bias.

In addition, we observe children once they are in first grade and ideally we would like to have birth records to perform our analysis. Data from official vital statistics (MINSAL 1996, 1997, 1998) show that the number of births is about 21K each month for the years we study. If we exclude the 7% of those children (who enroll in the private schools), then we get very close to our sample of 19K per month. In addition, the same source indicates that the number of births was evenly distributed by month of birth (taking into account the different number of days each month has), as we also find in our data with first grade enrollment.

We test for manipulation using a nonparametric test of discontinuity in the density of students born at each side of the eligibility rule, provided by Cattaneo et al. (2018). The manipulation test is  $-0.3668$ , with a p-value of  $0.7138$ , which indicates that there is no statistical evidence of systematic manipulation of the running variable.

**Figure A.1:** Birth-Density per Day



Note: Figure A.1 plot the density of observations by each day in our data, and fits estimated lines using all the underlying data, allowing for different slopes on each side of the cutoff. The sample size is  $N=117,709$ .

Figure A.1 provides a graphical representation of the continuity in density test approach, plotting the density of observations by each day in our data. As we describe in the main text, we have on average about 2K observations per day. Dividing those observations over the total in our working sample for first graders (117K), we get a density value of about 0.017 each day, which is exactly what Figure A.1 shows. The density varies by holidays or weekends, and the fitted lines on both sides of the cutoff in Figure A.1 take that into account. This plot is consistent with the results from the formal test from Cattaneo et al. (2018), as the density estimates above and below the cutoff (the two intercepts in the figure) are very near each other.

In addition to the nonparametric test by Cattaneo et al. (2018) we also test parametrically whether the density changes at the cutoff in Table A.1. Columns (1) to (7) show the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) for first graders using the density of observations per day as dependent variable. The different columns add controls for weekends, holidays and birth year, and also vary the days near the cutoff used to run our regressions. The results are again consistent with both the graphical representation of the data and the nonparametric test, indicating no statistical evidence of systematic manipulation.

**Table A.1:** Testing Manipulation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\widehat{\alpha}_1$	0.001 (0.001)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)	0.001 (0.001)	-0.000 (0.001)	0.002 (0.001)
Days near the Cutoff	30 days	30 days	30 days	30 days	20 days	10 days	3 days
Weekends	No	Yes	Yes	Yes	Yes	Yes	Yes
Holidays	No	No	Yes	Yes	Yes	Yes	Yes
Birth Year	No	No	No	Yes	Yes	Yes	Yes
Observations	117,709	117,709	117,709	117,709	79,007	40,303	13,167

Notes: Table A.1 show the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) for first graders using the density of observations per day as dependent variable. Robust standard errors (in parentheses) are clustered by day of birth.

## A.2 Covariates Smoothness

In this section we show that there are no other changes in our observable covariates occurring at the birth date threshold that could confound our analysis. Our research design mimics a local experiment where children are exogenously (to potential outcomes) allocated to either being born in June or July.

Accordingly, we find precise zeros when estimating equation (1) using each covariate in Table 1 as dependent variable. The effect sizes reported in Table A.2 are never higher than 0.02. and are precisely estimated (small standard errors). Some of these tiny estimates (e.g., class size and probability of going to a public school) are statistically significant at conventional levels, because we are highly powered to detect them (regressions in Table A.2. have almost 118K observations) and not because of their magnitude. We complement these results with a graphical illustration for every covariate in Figure A.2 and Figure A.3 which provide further evidence of a smooth behavior at the July 1 cutoff.

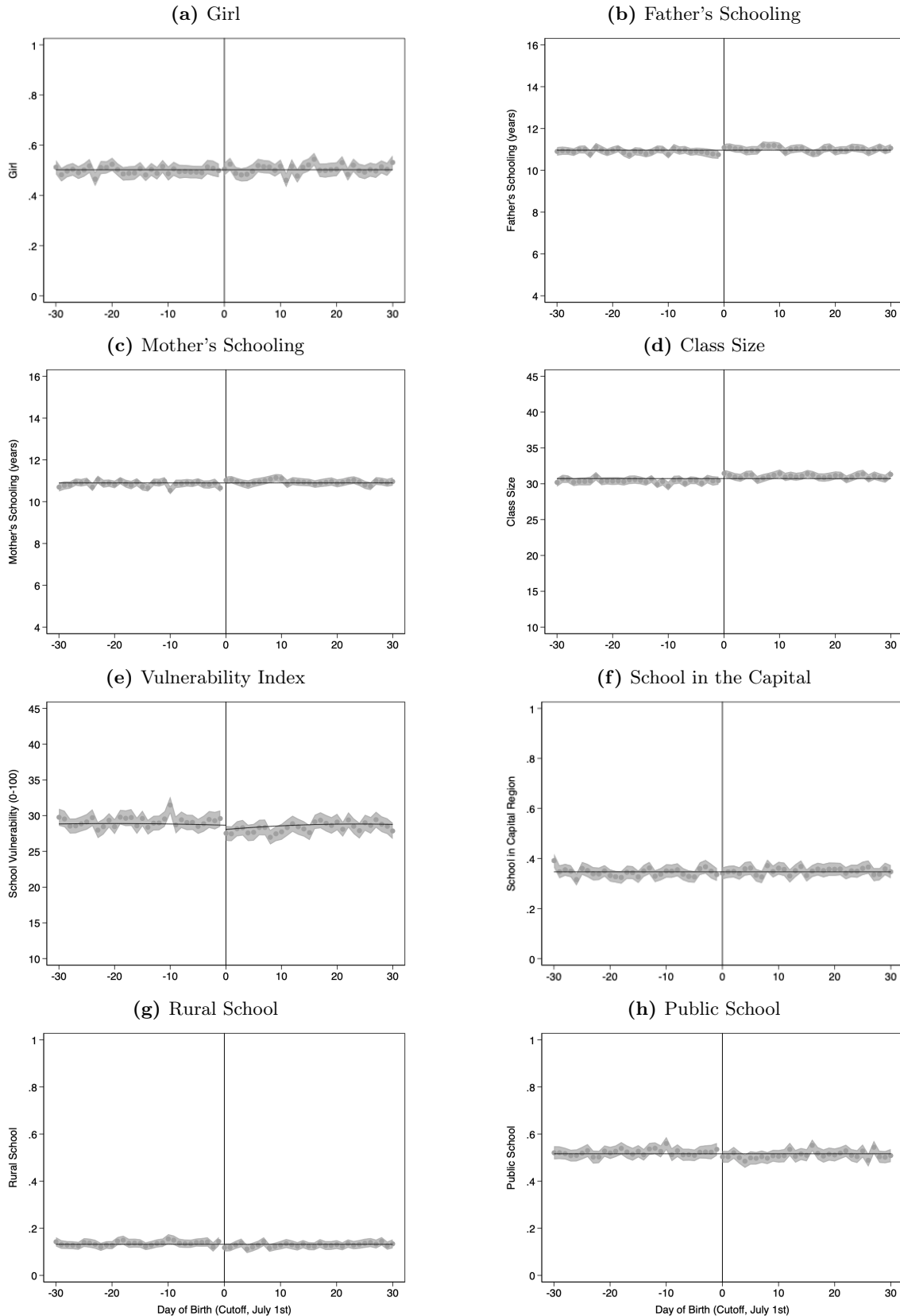
**Table A.2:** Covariates Smoothness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Girl	Father's Schooling	Mother's Schooling	Class Size	Vulnerability Index	Capital Region	Rural School	Public School
$\widehat{\alpha}_1$	0.002 (0.006)	0.052 (0.041)	0.009 (0.041)	0.531*** (0.102)	-0.003* (0.002)	-0.005 (0.005)	0.003 (0.003)	-0.011*** (0.004)
June Mean	0.487	10.897	10.823	29.920	0.303	0.366	0.146	0.531
$\widehat{\alpha}_1$ /(June Mean)	0.005	0.005	0.001	0.018	0.011	0.015	0.019	0.021
Observations	117,709	85,753	89,404	117,709	117,709	117,709	117,709	117,709

Notes: Table A.2 show the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) on covariates. Robust standard errors (in parentheses) are clustered by day of birth.

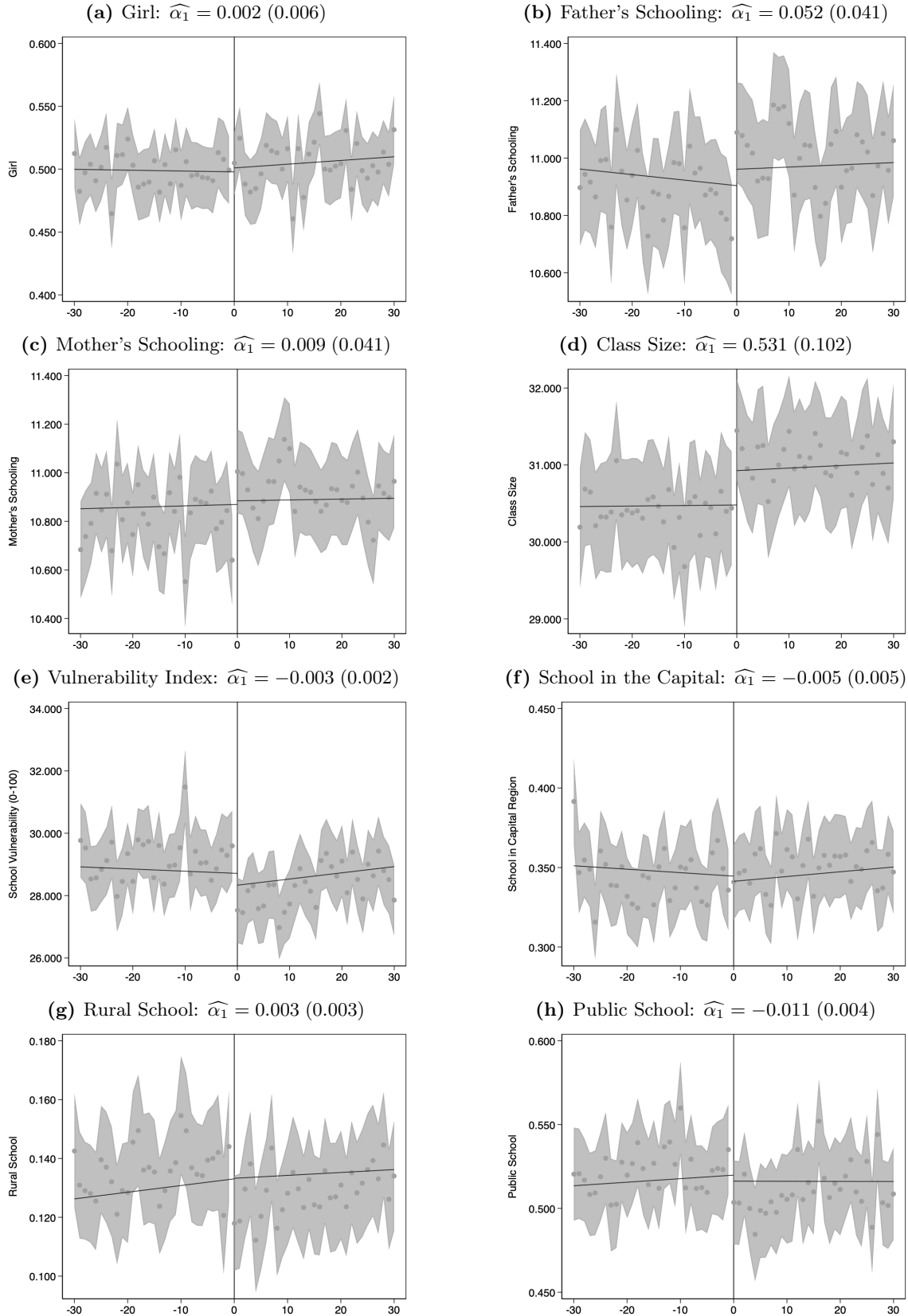


**Figure A.2:** Covariates Smoothness with Confidence Intervals



Note: The graphs in [Figure A.2](#) plot the mean of the y-axis variable within day of birth, and fit estimated lines using all the underlying data, allowing for different slopes on each side of the cutoff. Each day of birth contains about 2K observations.

**Figure A.3:** Covariates Smoothness with Confidence Intervals and Re-scaled Y-axes



Note: The graphs in [Figure A.3](#) replicate those in [Figure A.2](#) with re-scaled y-axes.

### A.3 Robustness to Including Different Sets of Covariates

As yet another robustness check, [Table A.3](#) reports the coefficients  $\widehat{\alpha}_1$  estimated from the equation (1) on long run outcomes using different set of controls.

In Panel (A) we include cohort fixed effects, weekends, and holidays; in Panel (B) we include the controls in (A) and add a set of demographics (female, class size, rural school, school in capital region); in Panel (C) we add the school vulnerability Index; in Panel (D) we add public school attendance

Our main estimates remain practically unchanged across panels when we add controls, and the same compared with [Table 5](#) in the main text.

**Table A.3:** Robustness

	(1) Primary Grad	(2) High-School Grad	(3) PSU Exam	(4) PSU Score	(5) College Enroll	(6) Selective Enroll	(7) STEM Enroll	(8) College Grad
<b>Panel (A)</b>								
$\widehat{\alpha}_1$	0.01*** (0.00)	0.01 (0.01)	0.04*** (0.01)	0.08*** (0.02)	0.05*** (0.01)	0.03*** (0.00)	0.01*** (0.00)	0.01* (0.00)
June Mean	0.912	0.684	0.580	-0.067	0.253	0.138	0.069	0.165
Observations	117,709	117,709	117,709	71,509	117,709	117,709	117,709	111,664
<b>Panel (B)</b>								
$\widehat{\alpha}_1$	0.01*** (0.00)	0.01 (0.01)	0.04*** (0.01)	0.08*** (0.02)	0.04*** (0.01)	0.03*** (0.00)	0.01*** (0.00)	0.01* (0.00)
June Mean	0.912	0.684	0.580	-0.067	0.253	0.138	0.069	0.165
Observations	117,709	117,709	117,709	71,509	117,709	117,709	117,709	111,664
<b>Panel (C)</b>								
$\widehat{\alpha}_1$	0.01*** (0.00)	0.00 (0.01)	0.04*** (0.01)	0.07*** (0.01)	0.04*** (0.00)	0.03*** (0.00)	0.01*** (0.00)	0.00* (0.00)
June Mean	0.912	0.684	0.580	-0.067	0.253	0.138	0.069	0.165
Observations	117,709	117,709	117,709	71,509	117,709	117,709	117,709	111,664
<b>Panel (D)</b>								
$\widehat{\alpha}_1$	0.01*** (0.00)	0.00 (0.01)	0.03*** (0.01)	0.07*** (0.01)	0.04*** (0.00)	0.03*** (0.00)	0.01*** (0.00)	0.00* (0.00)
June Mean	0.912	0.684	0.580	-0.067	0.253	0.138	0.069	0.165
Observations	117,709	117,709	117,709	71,509	117,709	117,709	117,709	111,664

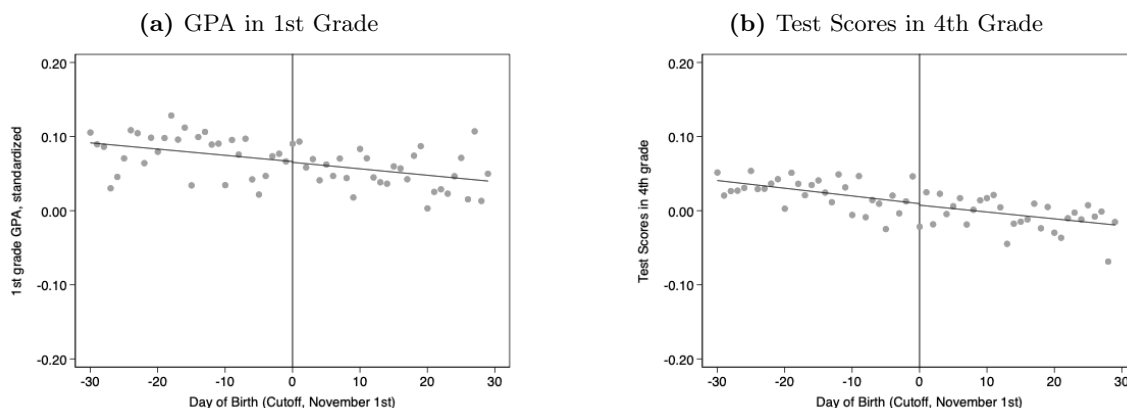
Notes: [Table A.3](#) reports the coefficients  $\widehat{\alpha}_1$  estimated from the equation (1) on long run outcomes using different set of controls. In Panel (A) we include cohort fixed effects, weekends, and holidays; in Panel (B) we add set of demographics (female, class size, rural school, school in capital region); in Panel (C) we add the school vulnerability Index; in Panel (D) we add public school attendance. Robust standard errors (in parentheses) are clustered by day of birth.

## A.4 Placebo Tests

We implemented a host of placebo tests following your suggestion and confirm that there is no gap on in-school outcomes, parental investments and long term outcomes. We use students born in October and November and estimate the coefficient at the arbitrary November 1st threshold.

We find precise zero effects on GPA in 1st grade and test scores in fourth grade, as shown in [Figure A.4](#) and [Table A.4](#).

**Figure A.4:** Placebo: Figures for In-school Effects



Note: The graphs in [Figure A.4](#) plot the mean of the y-axis variable within day of birth, and fit estimated lines using all the underlying data, allowing for different slopes on each side of the cutoff. Each day of birth contains about 2K observations.

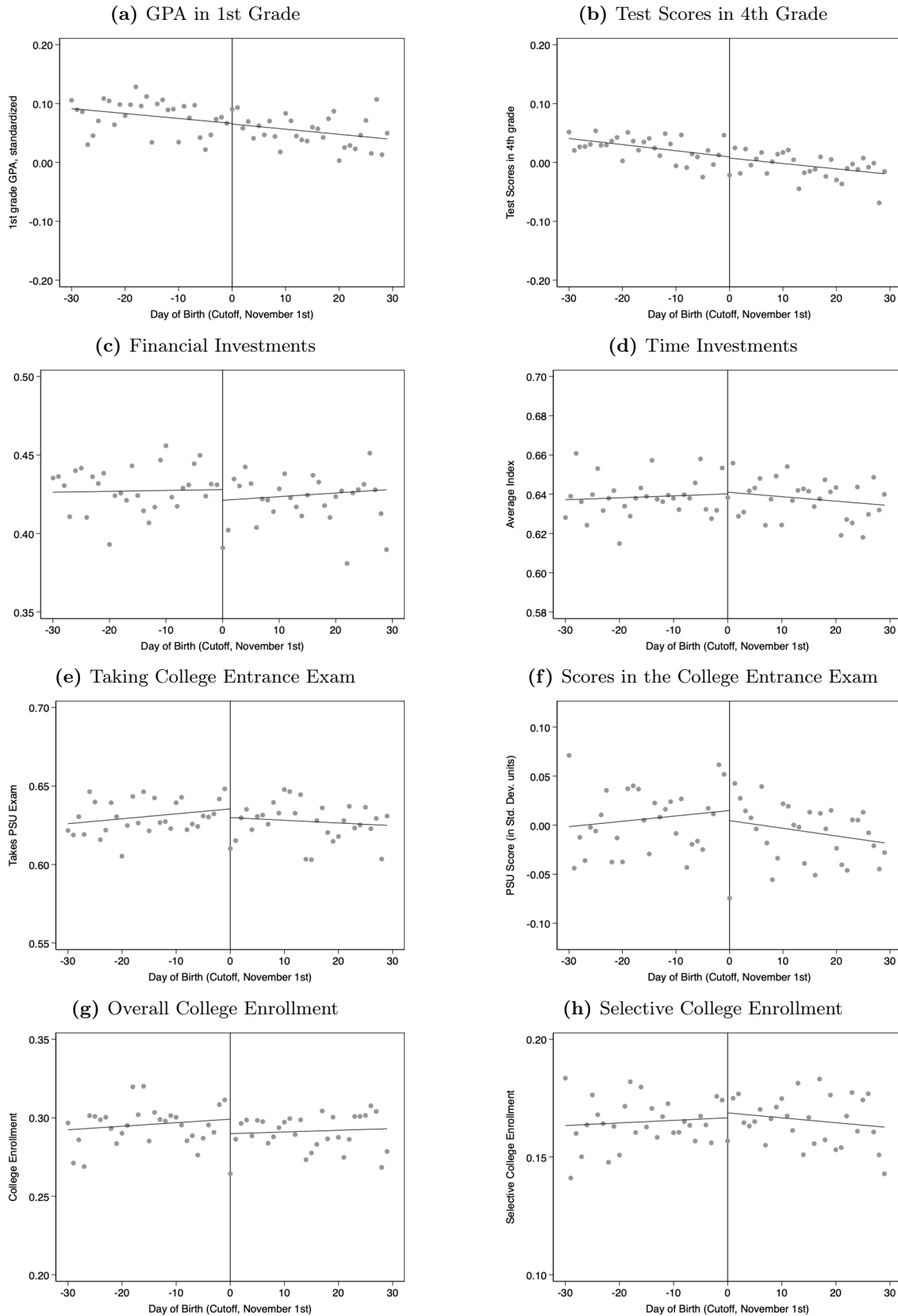
**Table A.4:** Placebo: Table with In-school Effects

	(1) GPA in 1st Grade	(2) Test Scores in 4th Grade
$\hat{\alpha}_1$	-0.003 (0.012)	-0.002 (0.009)
October Mean	0.057	0.009
Observations	121,331	121,331

Notes: [Table A.4](#) shows the coefficient  $\hat{\alpha}_1$  estimated from the equation (1) for first graders on age at school entry, and in-school outcomes. The ‘October Mean’ is the mean of the dependent variable just below the threshold. The outcomes are first grade GPA (standardized within school and grade) and pass rate, and the Language-Math score in 4th grade. All estimations include cohort fixed effects and control for child gender, class size, school vulnerability, school rurality and type of school (public or voucher). Robust standard errors (in parentheses) are clustered by day of birth.

We also find precise zero effects on parental investments and long term outcomes. [Figure A.5](#) shows results for all outcomes, while [Table A.5](#) and [Table A.6](#) show the coefficient  $\hat{\alpha}_1$  estimated from the equation (1) on parental investments and long run outcomes.

Figure A.5: Placebo Figures



Note: The graphs in Figure A.5 plot the mean of the y-axis variable within day of birth, and fit estimated lines using all the underlying data, allowing for different slopes on each side of the cutoff. Each day of birth contains about 2K observations.

**Table A.5:** Placebo: Effects on Parental Investments

**Panel A: Financial Investments**  
November 1 threshold

	(1)	(2)	(3)	(4)	(5)	(6)
	Average Index	Factor Index	Computer at Home	Internet at Home	More 10 Books	High Spending
$\widehat{\alpha}_1$	-0.007 (0.005)	-0.024 (0.017)	-0.017 (0.008)	-0.006 (0.006)	-0.006 (0.009)	-0.000 (0.008)
October Mean	0.431	0.033	0.476	0.200	0.523	0.527
Effect Size	-0.022	-0.024	-0.036	-0.028	-0.011	-0.000
Observations	52,775	52,775	52,775	52,775	52,775	52,775

**Panel B: Time Investments: ‘My parent always...’**  
November 1 threshold

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Average Index	Factor Index	Congrats for good grades	Knows my grades	Demands good grades	Helps to study	Helps with homework
$\widehat{\alpha}_1$	0.001 (0.005)	0.003 (0.019)	0.002 (0.008)	0.009 (0.009)	-0.002 (0.010)	0.007 (0.009)	-0.013 (0.008)
October Mean	0.640	0.007	0.795	0.730	0.519	0.561	0.596
Effect Size	0.003	0.003	0.002	0.013	-0.003	0.013	-0.021
Observations	49,611	49,611	49,611	49,611	49,611	49,611	49,611

Notes: [Table A.5](#) shows the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) on parental investments. The ‘October Mean’ is the mean of the dependent variable just below the threshold. The outcomes are defined in the main text. Robust standard errors (in parentheses) are clustered by day of birth.

**Table A.6:** Placebo Effects on Long Run Outcomes

November 1 threshold

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Primary Grad	High-School Grad	PSU Exam	PSU Score	College Enroll	Selective Enroll	STEM Enroll
$\widehat{\alpha}_1$	0.001 (0.003)	-0.001 (0.005)	-0.006 (0.005)	-0.009 (0.015)	-0.009 (0.005)	0.002 (0.004)	-0.001 (0.003)
October Mean	0.930	0.711	0.636	-0.104	0.298	0.167	0.081
Effect Size	0.005	0.001	0.011	0.010	0.020	0.006	0.003
Observations	119,006	119,006	119,006	74,849	119,006	119,006	119,006

Notes: [Table A.6](#) shows the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) on long run outcomes. The ‘October Mean’ is the mean of the dependent variable just below the threshold. The outcomes are defined in the main text. Robust standard errors (in parentheses) are clustered by day of birth.

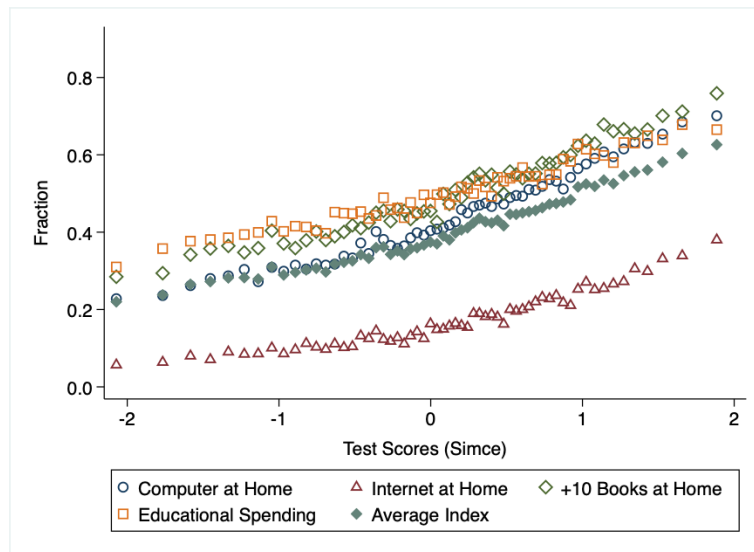
## B Appendix: Investments

### B.1 Financial Investments

We use data for children who were in first grade in years 2002, 2003 and 2004, whose parents were surveyed by SIMCE in years 2005, 2006 and 2007. We use these survey-years because they ask students about the time parents spend with them on educational activities and this survey was not implemented the previous years, and because questions change in the next years. The sample size is of about 500K observations.

We first we show in [Figure B.1](#) the raw data relating financial investments and college expectations to SIMCE test scores (in standard deviation units). [Figure B.1](#) is a non-parametric plot illustrative of the positive correlation between parental financial investments and children's test scores.

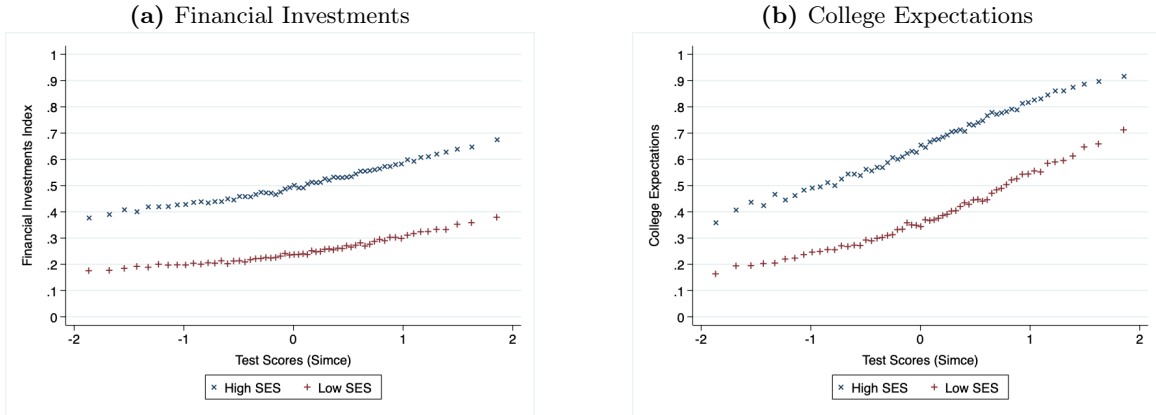
**Figure B.1:** Financial Investments and Test Scores



Note: The graphs in [Figure B.1](#) plot the mean of the y-axis variables within equal sized bins of SIMCE test scores (in standard deviation units) in 4th grade, with a sample of approximately 500K observations. The y-axis variables are our measures of parental financial investments (having computer, internet connection and more than 10 books at home, spending above the median in educational items and an the average of those four variables in an Index).

[Figure B.2](#) plots the average index of financial investments and parental beliefs (measured by college expectations), by socioeconomic status. The graphs shows that the positive correlation holds for both high and low socioeconomic status, with a gap that remains constant along the test score distribution. Overall, graphs in [Figure B.1](#) and [Figure B.2](#) show that in the raw data, parental financial investments and college expectations are correlated with test scores, and that the correlation also exists by socioeconomic group.

**Figure B.2:** Financial Investments, College Expectations and Test Scores by SES



Note: The graphs in [Figure B.2](#) plot the mean of the y-axis variables within equal sized bins of SIMCE test scores (in standard deviation units) in 4th grade, with approximately 250K observations each. The y-axis variables are the average of our measures of parental financial investments (having computer, internet connection and more than 10 books at home, spending above the median in educational items) and whether parents think their child will attend college in the future.

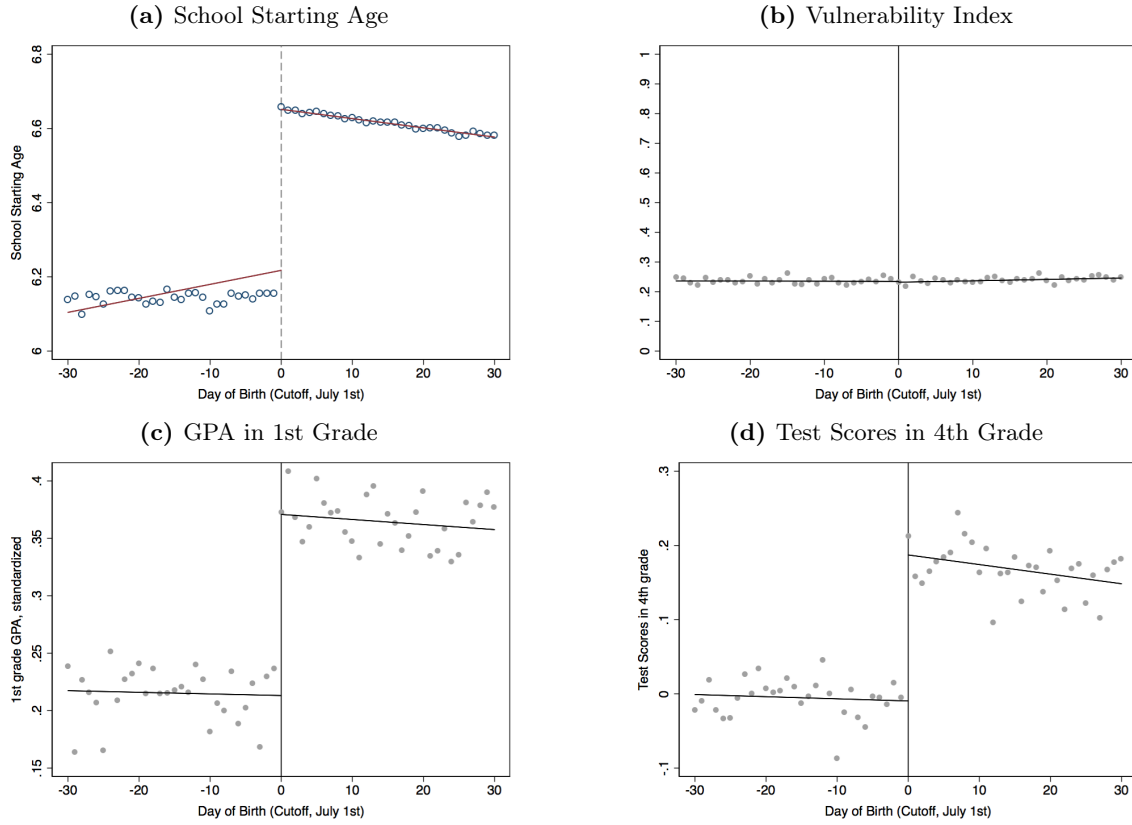
### Effects on the Survey Sample

Having data on first graders in years 2002, 2003 and 2004 allows us to exploit two discontinuities, using data for children born in June and July in 1996 and 1997 (as explained in our research design in [Figure 3](#)). Therefore we use two thirds of our original sample of 117K, and then given that survey response is about 75% we are left with approximately 50K observations to test effects on financial investments and college expectations.

[Figure B.3](#) and [Table B.1](#) show that results for this sample are similar to our working sample in the main text. There is a jump of about half a year in school starting age near the July 1 threshold, while mothers' schooling is smooth. In terms of outcomes, July-born students have higher GPAs ( $0.16\sigma$ ) in first grade and higher test scores ( $0.18\sigma$ ) in the fourth grade, very similar to what we found in our working samples. As an additional robustness check we also show results for the vulnerability index, which behaves smoothly near the cutoff as shown in [Figure B.3](#) and [Table B.1](#).



**Figure B.3:** School Starting Age, In-school Results and Vulnerability Index



Note: The graphs in [Figure B.3](#) plot the mean of the y-axis variable within day of birth, and fit estimated lines using all the underlying data, allowing for different slopes on each side of the cutoff. Each day of birth contains about 2K observations.

**Table B.1:** Results for the Sample with Financial Investments

	(1)	(2)	(3)	(4)
	Age at Entry	Vulnerability Index	GPA in 1st Grade	Test Scores in 4th Grade
$\widehat{\alpha}_1$	0.501*** (0.005)	0.000 (0.000)	0.162*** (0.012)	0.178*** (0.013)
June Mean	6.143	0.286	0.215	-0.005
Effect Size	0.082	0.000	0.753	35.055
Observations	51,818	51,818	51,818	51,818

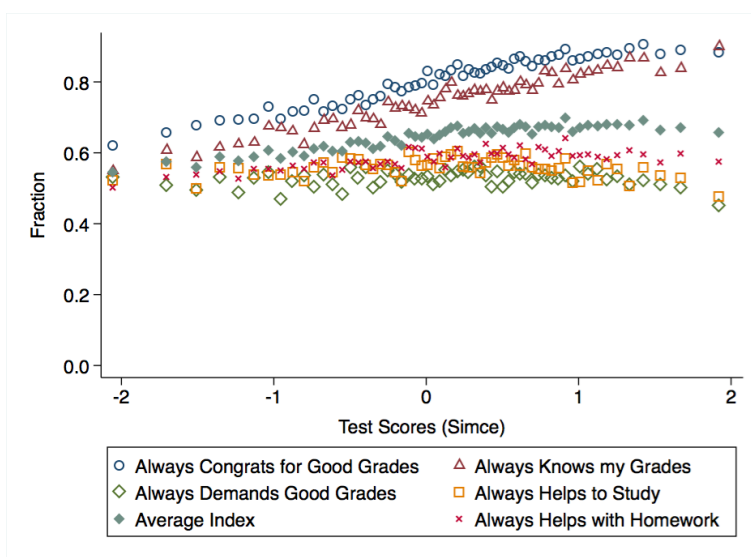
Notes: [Table B.1](#) show the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) for first graders on age at school entry, the vulnerability index and in-school outcomes. These are first grade GPA (GPA1, standardized within school and grade) and the Language-Math score in 4th grade. Robust standard errors (in parentheses) are clustered by day of birth.

## B.2 Parental Time Investments

We use data for children who were in first grade in years 2008 to 2010, and were surveyed by SIMCE in years 2011 to 2013. We use these surveys because they ask students about the time parents spend with them on educational activities and this survey was not implemented the previous years, and because questions change in the next years. For each cohort we have approximately 233K students and a survey response rate of two-thirds, leaving us with a the dataset of 460K observations.

We show in [Figure B.4](#) the raw data relating parental time investments to SIMCE test scores (in standard deviation units). The non-parametric plot in [Figure B.4](#) shows a positive correlation for two measures of parental involvement (whether parents always know and congratulate children for their grades), and a flat, slightly u-shaped correlation for three other measures (whether parents demand good grades, help with homework and help to study).

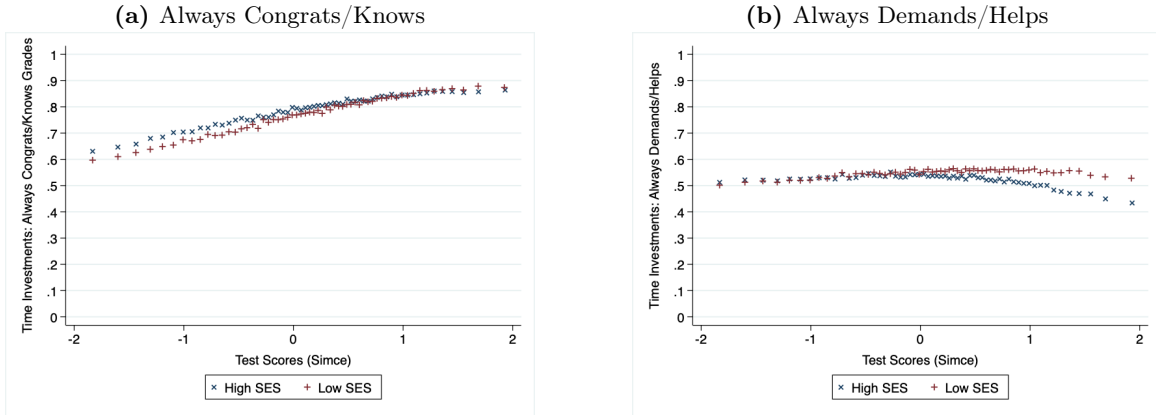
**Figure B.4:** Time Investments and Test Scores



Note: The graphs in [Figure B.4](#) plot the mean of the y-axis variables within equal sized bins of SIMCE test scores (in standard deviation units) in 4th grade, with a sample of approximately 460K observations. The y-axis variables are our measures of parental time investments.

In [Figure B.5](#) we show the same correlations after grouping these two groups of parental investments, by socioeconomic status. The graphs in [Figure B.5](#) show that the general pattern of correlations holds for both high and low socioeconomic status, with a slightly more marked u-shaped profile for high SES parents and students. Overall, graphs in [Figure B.4](#) and [Figure B.5](#) show that in the raw data, some parental time investments are correlated with test scores and others not, and that the patterns behaves similarly by socioeconomic group.

**Figure B.5:** Time Investments and Test Scores by SES



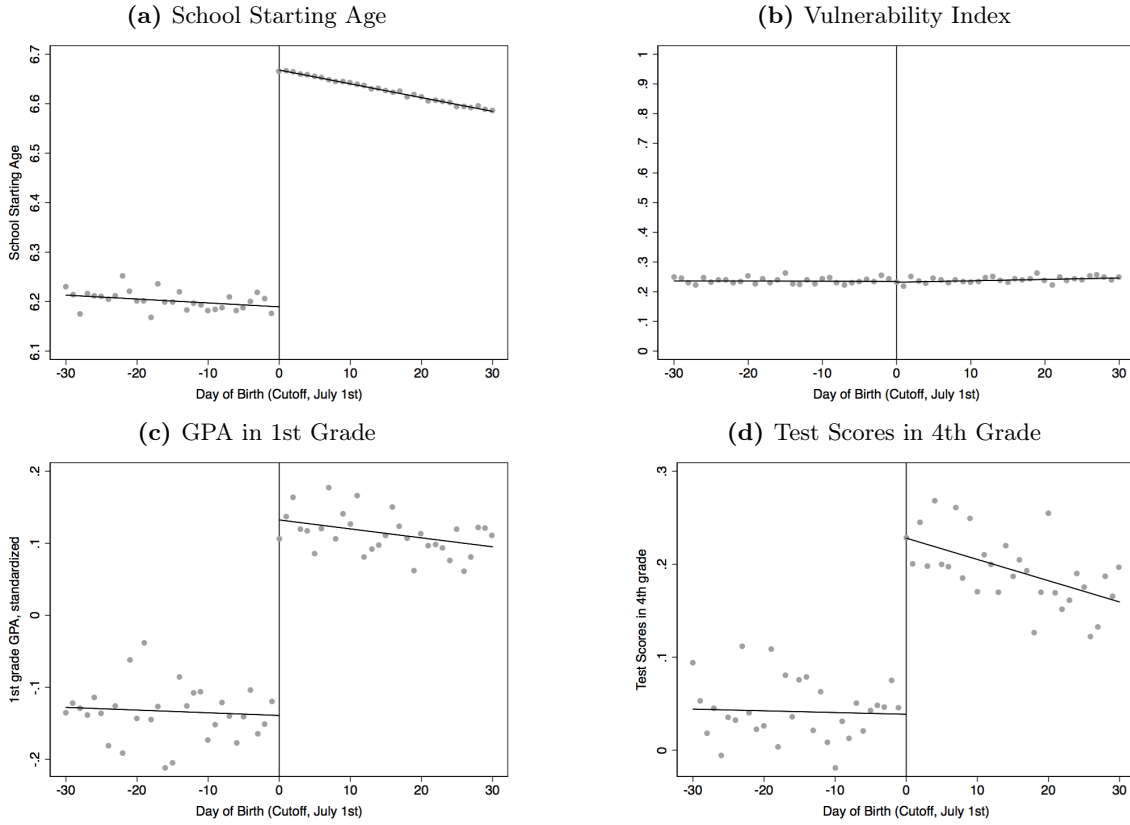
Note: The graphs in [Figure B.5](#) plot the mean of the y-axis variables within equal sized bins of SIMCE test scores (in standard deviation units) in 4th grade, with approximately 250K observations each. The y-axis variables are the average of our measures of parental time investments.

### Effects on the Survey Sample

Having data on first graders in years 2008 to 2010 allows us to exploit two discontinuities, using data for children born in June and July in 2002 and 2003 (analogously as explained in our research design in [Figure 3](#)). Similarly to our sample for financial investments we are left with approximately 50K observations to test effects on time and teacher investments.

[Figure B.6](#) and [Table B.2](#) show that results for this sample are similar to our working sample in the main text. There is a jump of about half a year in school starting age near the July 1 threshold, while the behavior of the vulnerability index near the cutoff is smooth. In terms of outcomes, July-born students have higher GPAs ( $0.26\sigma$ ) in first grade and higher test scores ( $0.18\sigma$ ) in the fourth grade, very similar to what we found in our working samples.

**Figure B.6:** School Starting Age, In-school Results and Vulnerability Index



Note: The graphs in Figure B.6 plot the mean of the y-axis variable within day of birth, and fit estimated lines using all the underlying data, allowing for different slopes on each side of the cutoff. Each day of birth contains about 2K observations.

**Table B.2:** Results for the Sample with Time Investments

	(1)	(2)	(3)	(4)
	Age at Entry	Vulnerability Index	GPA in 1st Grade	Test Scores in 4th Grade
$\widehat{\alpha}_1$	0.480*** (0.006)	-0.000 (0.000)	0.263*** (0.014)	0.181*** (0.013)
June Mean	6.202	0.235	-0.133	0.040
Observations	47,646	47,646	47,646	47,646

Notes: Table B.2 show the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) for first graders on age at school entry, vulnerability index and in-school outcomes. These are first grade GPA (GPA1, standardized within school and grade) and the Language-Math score in 4th grade. Robust standard errors (in parentheses) are clustered by day of birth.

### B.3 Factor Indexes

In this subsection we provide details of the composite score computation for financial and time investments. We compute a ‘factor index’ for each type of investment using principal components, which reduces the dimensionality of the investment measures to one composite score. For each index we performed the rotation of the loading matrix using the varimax method to produce the orthogonal factor.

Tables B.3 and B.4 summarize the results for each type of investment. The eigenvalues reported in Table B.3 indicate the total variance accounted by each factor. According to the Kaiser criterion we retain the factor with eigenvalues equal or higher than 1. The percentage of variance explained by the factors is 48% and 34% for financial and time investments, respectively.

The factor loadings in Table B.4 show the importance of each variable contributing the retained factor, while the uniqueness indicates the variance of each variable not shared with other variables in the overall factor model. The higher the loading (and the lower the uniqueness) the more relevant is the variable in defining the factor dimensionality.

**Table B.3:** Factor Analyses: Eigenvalues

Factor	Financial Investments	Time Investments
Factor 1	1.911	1.718
Factor 2	0.837	0.939
Factor 3	0.771	0.938
Factor 4	0.480	0.738
Factor 5	.	0.668

Notes: Table B.3 reports the eigenvalues of factor analyses conducted using principal components, with a rotation of the loading matrix performed with the varimax Kaiser criterion (Kaiser 1958). Column 1 shows the results for financial investments and column 2 does the same for time investments. According to the Kaiser criterion we retain the factor with eigenvalues equal or higher than 1 for each case.

**Table B.4:** Factor Analyses: Loadings and Uniqueness

Variable	Financial Investments		Variable	Time Investments	
	Loading	Uniqueness		Loading	Uniqueness
Computer	0.805	0.352	Congrats Grades	0.636	0.596
Internet	0.753	0.433	Knows Grades	0.547	0.701
High Spending	0.548	0.700	Demands Grades	0.369	0.864
More 10 Books	0.629	0.604	Helps Study	0.656	0.569
.	.	.	Helps Homework	0.669	0.552

Notes: Table B.4 reports the factor loadings and uniqueness each variable contributing the retained factor in the overall factor model. The higher the loading (and the lower the uniqueness) the more relevant is the variable in defining the factor dimensionality.

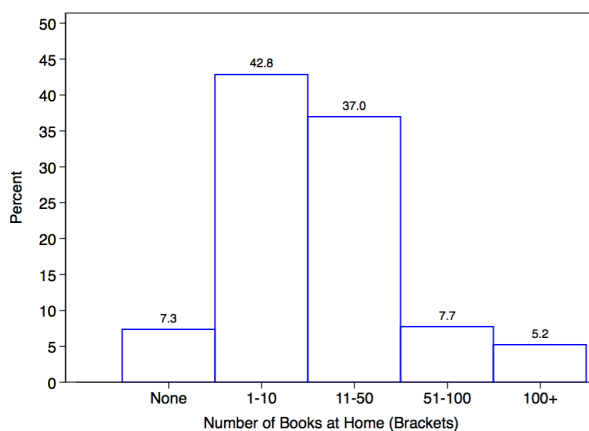
## B.4 Robustness to the Grouping of Answers

### Number of Books

Parents provide information on the number of books they own by brackets. [Figure B.7](#) shows the distribution of their answers by bracket. A 7% of parents report having no books, 43% report 1-10, 37% say 11-50, and then 7.7% report to have 51-100 books, with a 5.2% reporting more than 100 books. About half of parents report to have more than 10 books, which is the variable we use in the main text as a proxy of having books.

[Table B.5](#) shows that our results are robust to a number of ways of defining the *books* variable. Column (1) shows results on the number of books imputed as the middle value within each category (we used 120 books for the last category). Column (2) presents the results on the variable as it comes in the data (ranging from 0 to 4). Columns (3), (4) and (5) show the results on a dummy for more than 0, 10 and 50 books. The only variable where there are no effects is the 50 books category, but 90% of parents report to have less than 50 books. Other than that, all other variables show an effect on having books.

**Figure B.7:** Number of Books at Home



Note: The graphs in [Figure B.7](#) shows the distribution of the categorical variable ‘Number of Books at Home’, reported by parents in SIMCE surveys. The sample size is N=51,818.

**Table B.5:** Results for Books Grouping

	(1)	(2)	(3)	(4)	(5)
	Imp. Number of Books	Bracket of Books	More than 0 Books	More than 10 Books	More than 50 Books
$\widehat{\alpha}_1$	1.298** (0.510)	0.042*** (0.014)	0.013*** (0.004)	0.037*** (0.008)	-0.006 (0.005)
June Mean	36.248	1.588	0.917	0.478	0.137
Effect Size	0.036	0.027	0.014	0.078	-0.041
Observations	51,818	51,818	51,818	51,818	51,818

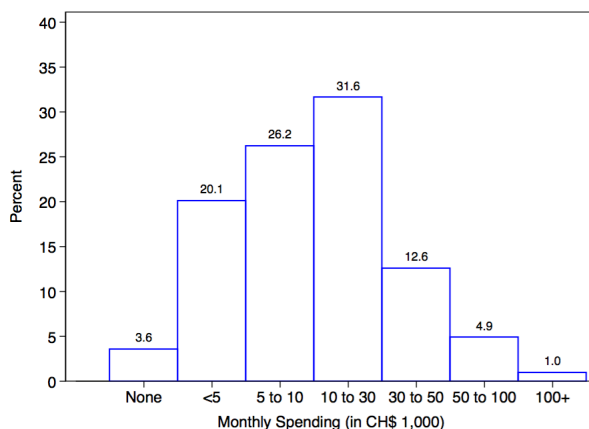
Notes: [Table B.5](#) show the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) for first graders on different ways of presenting the ‘Books at Home’ variable. Robust standard errors (in parentheses) are clustered by day of birth.

## Spending Categories

Parents provide information on their monthly spending on educational inputs, by brackets. [Figure B.8](#) shows the distribution of their answers by bracket. A 4% of parents report no spending, 20% report less than 5K Chilean pesos (\$CH), 26% say 5-10, 32% report 10-30, 13% report 30-50, and then 5% report to spend 50-100, with a 1% reporting more than 100\$CH. About half of parents report to spend than 10 \$CH, which is the variable we use in the main text as a proxy for spending.

[Table B.6](#) shows that our results are robust to a number of ways of defining the *spending* variable. Column (1) shows results on spending with values imputed as the middle value within each category (we used 120 \$CH for the last category). Column (2) presents the results on the variable as it comes in the data (ranging from 0 to 6). Columns (3) to (6) show the results on a dummy for more than 5K, 10K, 30K and 50K of \$CH. With the exception of the first dummy, all other variables show an effect on spending, with sizable effect sizes.

**Figure B.8:** Spending Categories



Note: The graphs in [Figure B.8](#) shows the distribution of the categorical variable ‘Monthly Spending on Educational Inputs’, reported by parents in SIMCE surveys. The sample size is N=51,818.

**Table B.6:** Results for Spending Grouping

	(1) Imp. Amount of Spending	(2) Bracket of Spending	(3) More 5K Sp	(4) More 10K Sp	(5) More 30K Sp	(6) More 50K Sp
$\widehat{\alpha}_1$	723.596** (294.659)	0.049** (0.019)	0.009 (0.007)	0.022*** (0.007)	0.013** (0.006)	0.006* (0.003)
June Mean	15793.682	2.424	0.757	0.478	0.165	0.052
Effect Size	0.046	0.020	0.011	0.047	0.077	0.111
Observations	51,818	51,818	51,818	51,818	51,818	51,818

Notes: [Table B.6](#) show the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) for first graders on different ways of presenting the ‘Monthly Spending on Educational Inputs’ variable. Robust standard errors (in parentheses) are clustered by day of birth.

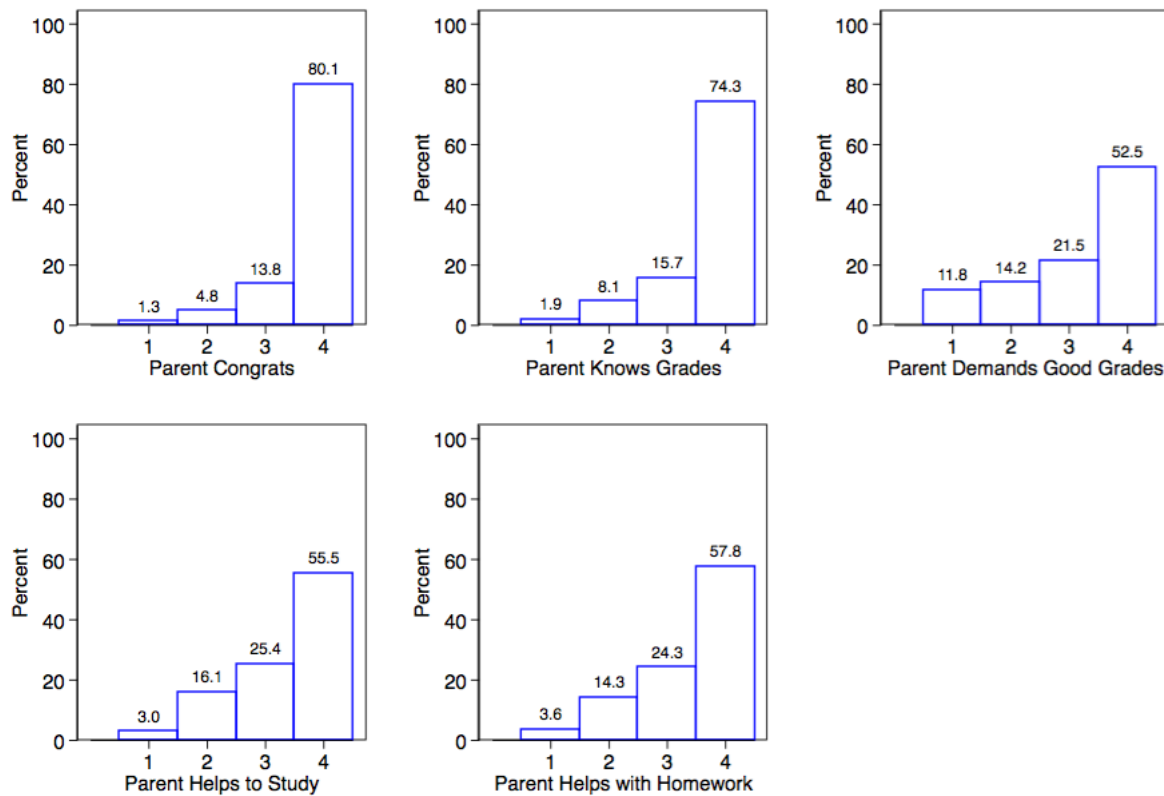
## Time Investment Categories

Students are asked about the time spent with their parents on educational activities. In particular, children report in a 1-4 Likert scale whether their parents help them study or with their homework, help them understand difficult subjects, whether parents know their grades, and whether parents demand improving grades. Available answers for each item are on the scale of ‘Never’, ‘Sometimes’, ‘Most of the time’, and ‘Always’.

Figure B.9 shows the distribution of their answers by question. Most of the students answer always for each question, ranging from 80% to 56%. This is why generate variables that equal one if the child answered that her parent does each activity ‘Always’, and zero otherwise.

Table B.7 shows that our results are robust to the way we use students answers. The first Panel shows the results for the answers in a 1-4 range, the second Panel shows the results grouping variables in the categories Often and Always, and the last panel shows results as in the main text. Effect sizes in almost all cases never higher than 2% and similar across tables.

**Figure B.9:** Time Investment Categories



1: Never, 2: Sometimes, 3: Often, 4: Always

Note: The graphs in Figure B.9 shows the distribution of the categorical variables of time investment reported by parents in SIMCE surveys. The sample size is N=47,646.



**Table B.7:** Results for Time Investment Grouping

<b>Panel 1</b>	(1)	(2)	(3)	(4)	(5)
	Congrats for good grades (from 1-4)	Knows my grades (from 1-4)	Demands good grades (from 1-4)	Helps to study (from 1-4)	Helps with homework (from 1-4)
$\widehat{\alpha}_1$	0.001 (0.010)	0.036** (0.015)	0.028 (0.025)	0.010 (0.015)	0.078*** (0.017)
June Mean	3.735	3.628	3.139	3.339	3.341
Effect Size	0.000	0.010	0.009	0.003	0.023
Observations	47,646	47,646	47,646	47,646	47,646

<b>Panel 2</b>	(1)	(2)	(3)	(4)	(5)
	Congrats for good grades (Often & Always)	Knows my grades (Often & Always)	Demands good grades (Often & Always)	Helps to study (Often & Always)	Helps with homework (Often & Always)
$\widehat{\alpha}_1$	0.010** (0.004)	0.021*** (0.006)	0.012 (0.008)	0.005 (0.006)	0.046*** (0.009)
June Mean	0.938	0.896	0.734	0.811	0.808
Effect Size	0.010	0.024	0.017	0.006	0.057
Observations	47,646	47,646	47,646	47,646	47,646

<b>Panel 3</b>	(1)	(2)	(3)	(4)	(5)
	Congrats for good grades (Always)	Knows my grades (Always)	Demands good grades (Always)	Helps to study (Always)	Helps with homework (Always)
$\widehat{\alpha}_1$	-0.010 (0.006)	0.010 (0.010)	0.001 (0.012)	0.001 (0.009)	0.018** (0.009)
June Mean	0.810	0.752	0.526	0.559	0.569
Effect Size	-0.012	0.013	0.002	0.001	0.032
Observations	47,646	47,646	47,646	47,646	47,646

Notes: [Table B.7](#) show the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) for first graders on different ways of presenting the ‘Monthly Spending on Educational Inputs’ variable. Robust standard errors (in parentheses) are clustered by day of birth.

## B.5 Effects on Raw Measures of Parental Investments

We also implemented a robustness exercise using the measures of parental time and material investments by themselves.

**Time Investments:** The estimates on raw measures are precise zeros, i.e., coefficients of tiny magnitude and precisely estimated. [Table B.8](#) shows the results using the raw time investments (answers with four possibilities). [Figure B.10](#) shows the graphs for the raw measures of time investments.

**Table B.8:** Results for Time Investments Measured in a 1 to 4 Scale

<b>Panel 1</b>	(1) Congrats for good grades (from 1-4)	(2) Knows my grades (from 1-4)	(3) Demands good grades (from 1-4)	(4) Helps to study (from 1-4)	(5) Helps with homework (from 1-4)
$\widehat{\alpha}_1$	0.001 (0.010)	0.036** (0.015)	0.028 (0.025)	0.010 (0.015)	0.078*** (0.017)
June Mean	3.735	3.628	3.139	3.339	3.341
Effect Size	0.000	0.010	0.009	0.003	0.023
Observations	47,646	47,646	47,646	47,646	47,646

Notes: The table shows the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) for first graders on measures of time investments, in a 1 to 4 scale. Robust standard errors (in parentheses) are clustered by day of birth.

**Material Investments:** The estimates on raw measures of material investments are statistically significant and of sizable magnitude. [Table B.9](#) shows the results using the individual measures of material investments in columns 3 to 6. In Appendix B.4 we also show that the effects are robust to different ways of defining the books and spending variables. [Figure B.11](#) show the graphs for the raw measures of material investments.

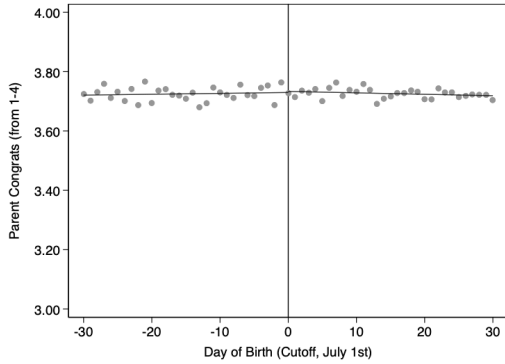
**Table B.9:** Results for Material Investments

	(1) Average Index	(2) Factor Index	(3) Computer at Home	(4) Internet at Home	(5) More 10 Books	(6) High Spending
$\widehat{\alpha}_1$	0.034*** (0.005)	0.108*** (0.016)	0.043*** (0.008)	0.032*** (0.006)	0.037*** (0.008)	0.022*** (0.007)
June Mean	0.379	-0.070	0.404	0.158	0.478	0.478
Effect Size	0.107	0.110	0.107	0.204	0.078	0.047
Observations	51,818	51,818	51,818	51,818	51,818	51,818

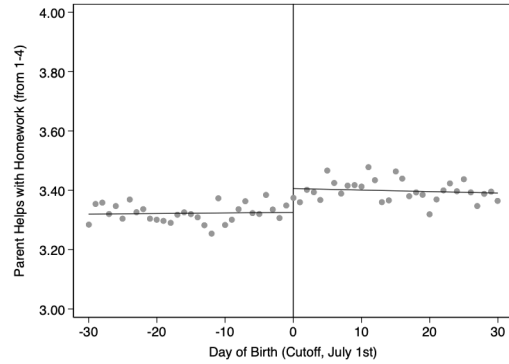
Notes: [Table B.9](#) show the coefficient  $\widehat{\alpha}_1$  estimated from the equation (1) for first graders on measures of material investments. Robust standard errors (in parentheses) are clustered by day of birth.

**Figure B.10:** Effects on Raw Time Investment Measures

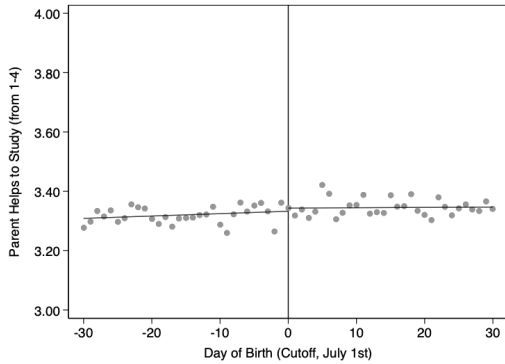
(a) Congrats for good grades (from 1-4): 0.001 (0.010)



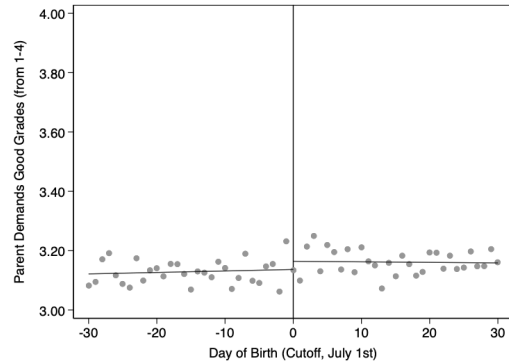
(b) Knows my grades (from 1-4): 0.036 (0.015)



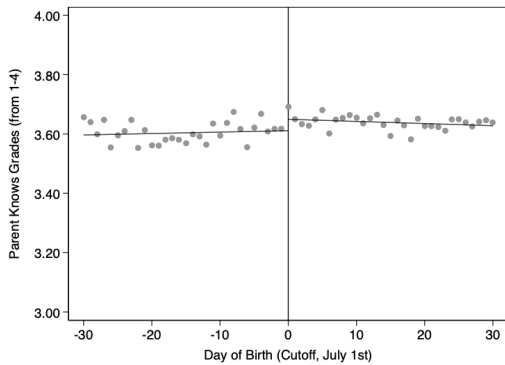
(c) Demands good grades (from 1-4): 0.028 (0.025)



(d) Helps to study (from 1-4): 0.010 (0.015)

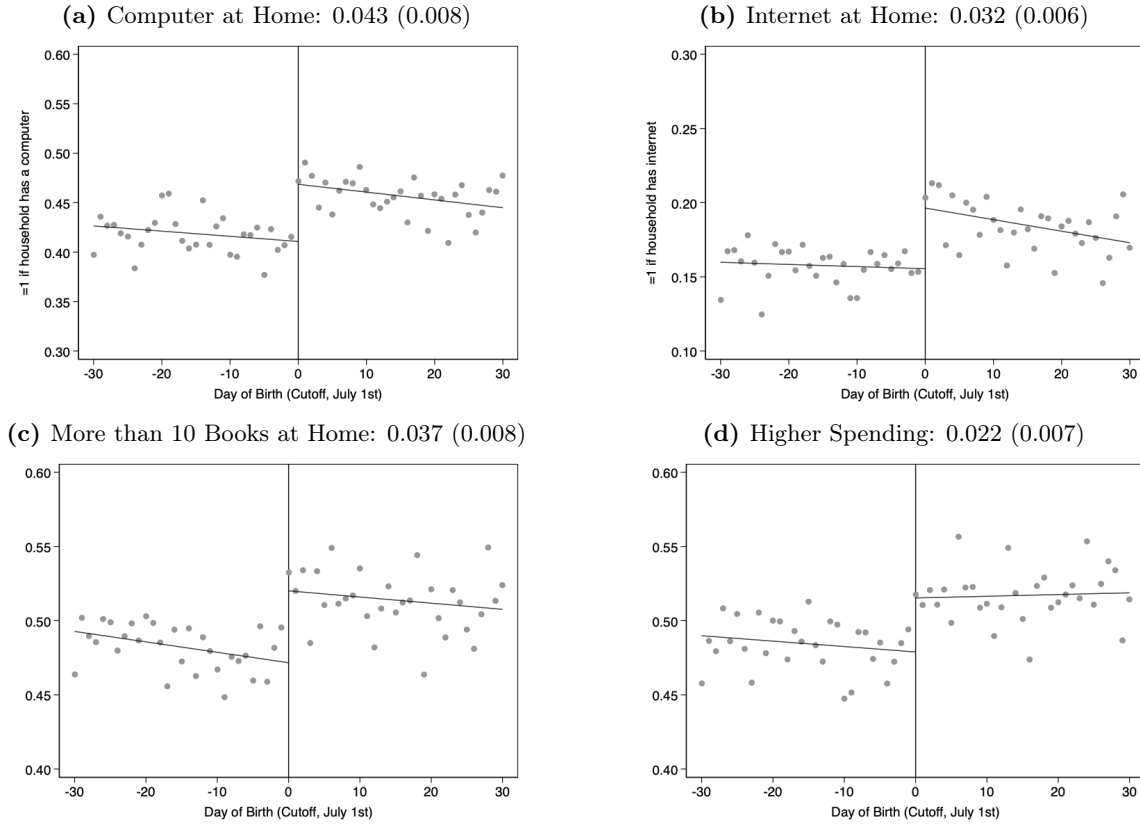


(e) Helps with homework: 0.078 (0.017)



Note: The graphs in [Figure B.10](#) plot the mean of the y-axis variable within day of birth, and fit estimated lines using all the underlying data, allowing for different slopes on each side of the cutoff. Each day of birth contains about 2K observations. The heading of each graph reports the estimated coefficient at the July 1st threshold, with corresponding standard errors in parentheses.

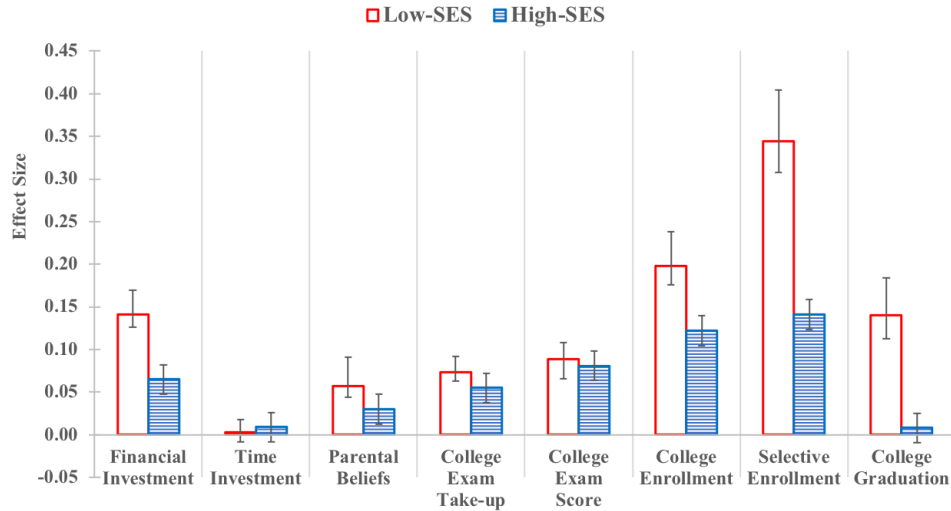
**Figure B.11: Effects on Raw Financial Investment Measures**



Note: The graphs in [Figure B.11](#) plot the mean of the y-axis variable within day of birth, and fit estimated lines using all the underlying data, allowing for different slopes on each side of the cutoff. Each day of birth contains about 2K observations. The heading of each graph reports the estimated coefficient at the July 1st threshold, with corresponding standard errors in parentheses.

## C Appendix: Additional Results

Figure C.1: Effect Sizes by Low-High Socioeconomic Status



Note: Figure C.1 plots the effect sizes on parental investments, beliefs and children long run outcomes. Financial and time investments are each measured by the average index described in subsection 3.2. Parental beliefs are measured as college completion expectations. Long run outcomes consist on the college entrance exam take-up and scores, college enrollment (overall and at selective institutions), and college graduation.

**Table C.1: Effect Sizes, by Quartiles of Socioeconomic Status**

	(1) Low SES	(2) Med-Low SES	(3) Med-High SES	(4) High SES	(5) Difference (1)-(4)
<b>Panel 1: Financial Investments</b>					
Average Index	0.189*** (0.0372)	0.113*** (0.0330)	0.071*** (0.0274)	0.062*** (0.0155)	0.127*** ( 0.040)
Computer at Home	0.305*** (0.0882)	0.157*** (0.0573)	0.123*** (0.0442)	0.033* (0.0176)	0.272*** ( 0.090)
Internet at Home	0.597** (0.2472)	0.454*** (0.1480)	0.131 (0.0920)	0.155*** (0.0407)	0.442* ( 0.251)
More than 10 Books at Home	0.143*** (0.0405)	0.097* (0.0506)	0.043 (0.0352)	0.066*** (0.0222)	0.077* ( 0.046)
High Spending	0.128** (0.0595)	0.034 (0.0374)	0.036 (0.0274)	0.039* (0.0218)	0.089 ( 0.063)
<b>Panel 2: Time Investments</b>					
Average Index	0.004 (0.0194)	0.002 (0.0157)	0.012 (0.0151)	0.025* (0.0144)	-0.021 ( 0.024)
Congrats Grades	0.047*** (0.0170)	0.006 (0.0194)	0.026* (0.0133)	0.013 (0.0150)	0.034 ( 0.023)
Knows Grades	0.019 (0.0244)	0.019 (0.0221)	0.026 (0.0175)	0.022 (0.0176)	-0.003 ( 0.030)
Demands Good Grades	0.087** (0.0432)	0.048 (0.0333)	0.025 (0.0347)	0.008 (0.0408)	0.079 ( 0.059)
Helps to Study	0.007 (0.0401)	0.001 (0.0299)	0.045 (0.0395)	0.048 (0.0434)	-0.041 ( 0.059)
Helps with Homework	0.042 (0.0372)	0.025 (0.0247)	0.001 (0.0316)	0.056** (0.0253)	-0.014 ( 0.045)
<b>Panel 3: Parental Beliefs</b>					
College Expectation	0.073 (0.0663)	0.048 (0.0393)	0.038* (0.0217)	0.027* (0.0144)	0.046 ( 0.068)
Grad School Expectation	0.429** (0.2086)	0.167 (0.1174)	0.165* (0.0957)	0.038 (0.0762)	0.391* ( 0.222)
Institute Expectation	0.043 (0.0440)	0.057** (0.0265)	0.005 (0.0164)	0.009 (0.0116)	0.034 ( 0.046)
<b>Panel 4: Long Run Outcomes</b>					
Takes PSU Exam	0.094*** (0.0307)	0.057** (0.0273)	0.072*** (0.0174)	0.042*** (0.0100)	0.052 ( 0.032)
College Enrollment	0.273*** (0.0680)	0.148*** (0.0525)	0.151*** (0.0403)	0.106*** (0.0255)	0.167** ( 0.073)
Selective College Enrollment	0.374*** (0.1001)	0.325*** (0.0713)	0.159*** (0.0612)	0.129*** (0.0419)	0.245** ( 0.109)
College Graduation	0.236*** (0.0792)	0.069 (0.0563)	0.072 (0.0522)	0.027 (0.0335)	0.209** ( 0.086)

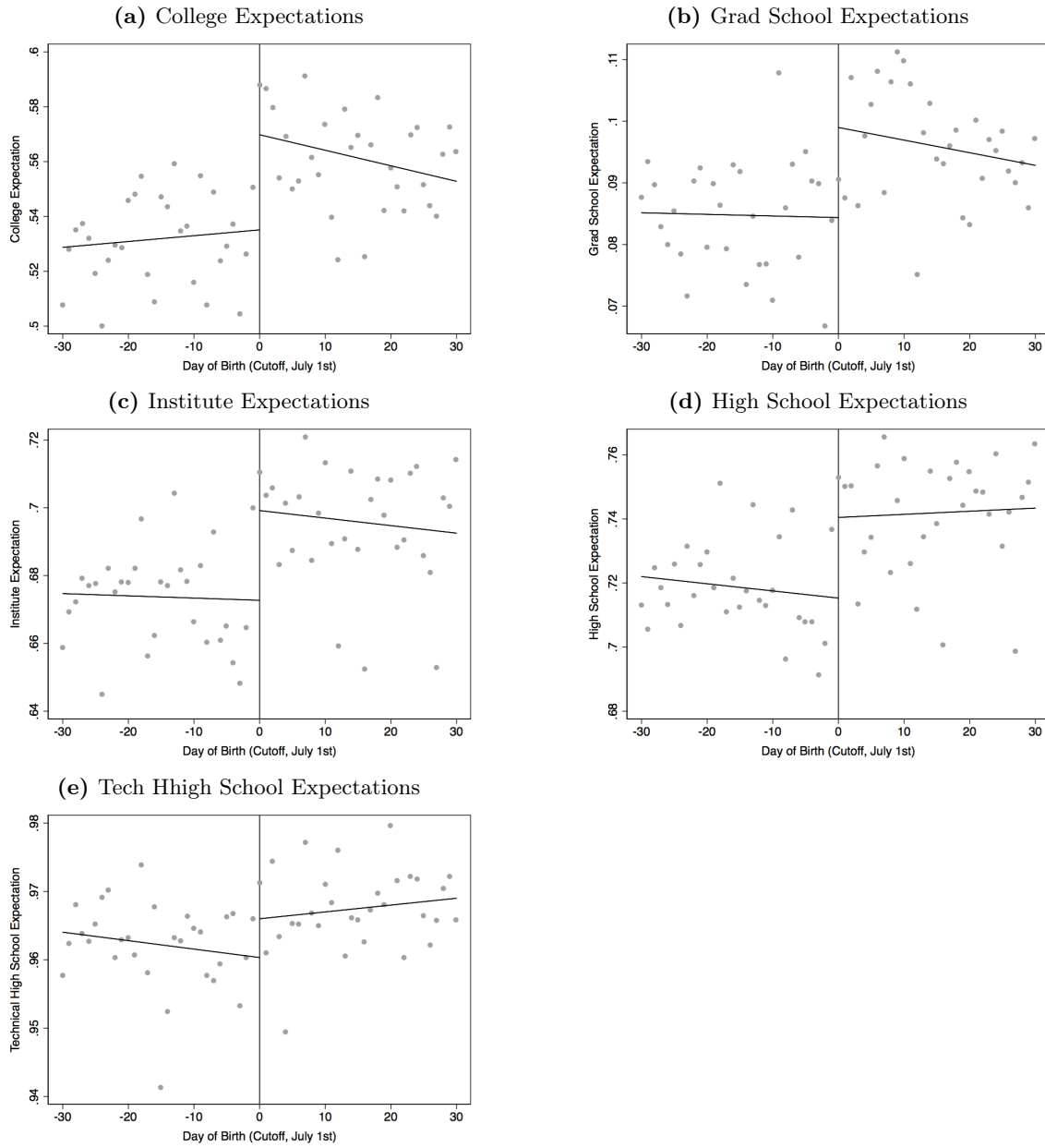
Notes: [Table C.1](#) presents separate estimates of Equation (1) reported as effect sizes, on parental investments, beliefs, and children long run outcomes (in rows) by quartiles of the school vulnerability index (in columns 1-4). We label each quartile as low SES, med-low SES, med-high SES, and high SES, in columns (1)-(4). Column (5) reports the difference in effect size between the low and high SES groups. Standard errors are computed using the delta method.

**Table C.2:** Effect Sizes, by Low-High Socioeconomic Status

Variable	(1) Low SES	(2) High SES	(3) Difference (1) - (2)
<b>Panel 1: Financial Investments</b>			
Average Index	0.141*** (0.0285)	0.065*** (0.0151)	0.076** (0.0323)
Computer at Home	0.207*** (0.0512)	0.068*** (0.0214)	0.139** (0.0555)
Internet at Home	0.482*** (0.1336)	0.148*** (0.0429)	0.334** (0.1403)
More than 10 Books at Home	0.116*** (0.0363)	0.056*** (0.0150)	0.060 (0.0393)
High Spending	0.070** (0.0308)	0.036** (0.0172)	0.034 (0.0353)
<b>Panel 2: Time Investments</b>			
Average Index	0.003 (0.0144)	0.009 (0.0110)	-0.006 (0.0181)
Congrats Grades	0.022* (0.0126)	0.003 (0.0099)	0.019 (0.0160)
Knows Grades	0.001 (0.0185)	0.024* (0.0141)	-0.023 (0.0233)
Demands Good Grades	0.018 (0.0280)	0.016 (0.0303)	0.002 (0.0413)
Helps to Study	0.004 (0.0275)	0.005 (0.0271)	-0.001 (0.0386)
Helps with Homework	0.033 (0.0213)	0.033* (0.0191)	0.000 (0.0286)
<b>Panel 3: Parental Beliefs</b>			
Grad School Expectation	0.267*** (0.0997)	0.087 (0.0616)	0.180 (0.1172)
College Expectation	0.057* (0.0337)	0.030** (0.0128)	0.027 (0.0360)
Institute Expectation	0.051** (0.0249)	0.002 (0.0086)	0.049* (0.0263)
<b>Panel 4: Long Run Outcomes</b>			
Takes PSU Exam	0.073*** (0.0187)	0.055*** (0.0101)	0.018 (0.0213)
College Enrollment	0.198*** (0.0398)	0.122*** (0.0225)	0.076* (0.0457)
Selective College Enrollment	0.344*** (0.0605)	0.141*** (0.0365)	0.203*** (0.0707)
College Graduation	0.140*** (0.0442)	0.008 (0.0272)	0.132** (0.0519)

Notes: [Table C.2](#) presents separate estimates of Equation (1) reported as effect sizes, on parental investments, beliefs, and children long run outcomes (in rows) by categories of school vulnerability index (in columns). We classify all observations below the median of the index as low SES, and all above as high SES). Column (3) reports the difference in effect size between the low and high SES groups. Standard errors are computed using the delta method.

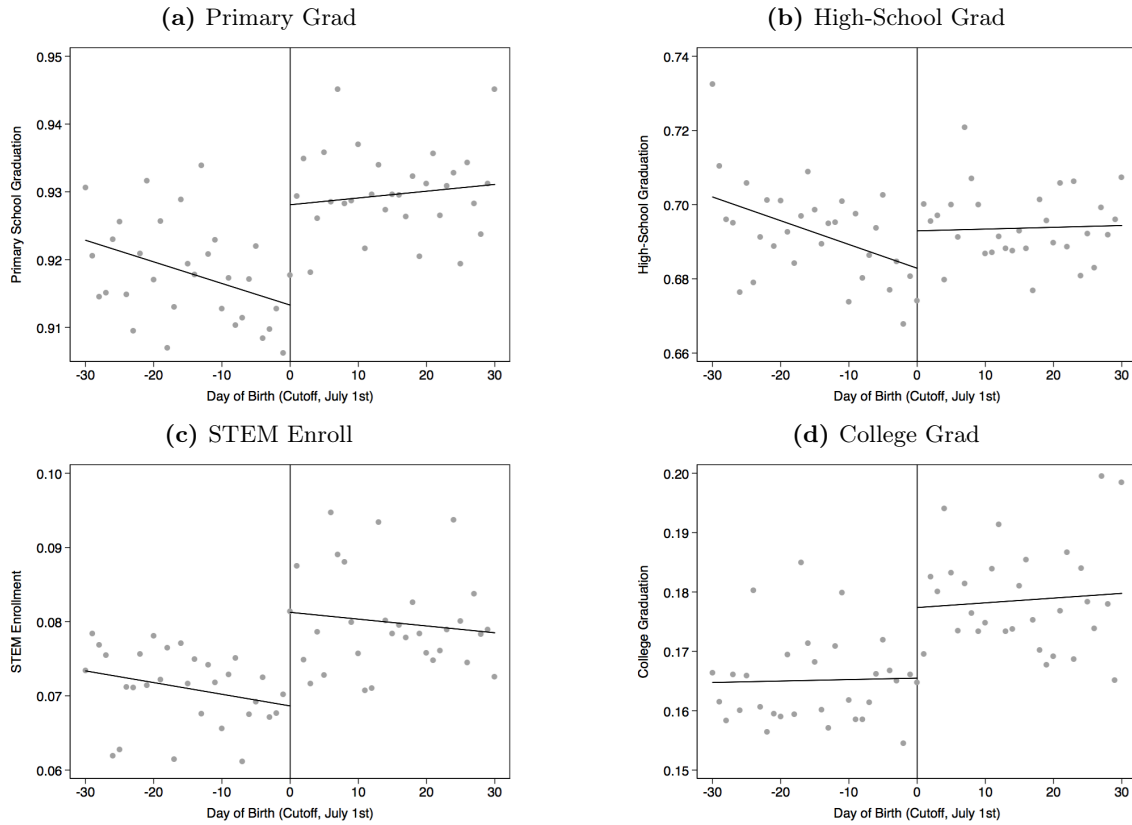
**Figure C.2:** Effects on Parental Beliefs



Note: The figures in [Figure C.2](#) are graphical analog to estimates in [Table 4](#). Each graph plots the mean of the y-axis variable within day of birth, and fit estimated lines using all the underlying data, allowing for different slopes on each side of the cutoffs. The dependent variables are described in [subsection 3.2](#).



**Figure C.3:** Additional Effects on Long Run Outcomes



Note: The figures in [Figure C.3](#) are graphical analog to estimates in [Table 5](#) . Each graph plots the mean of the y-axis variable within day of birth, and fit estimated lines using all the underlying data, allowing for different slopes on each side of the cutoffs. The dependent variables are described in the main text and in [Table 5](#).

## C.1 On Non-compliance and Interpretation of our Results

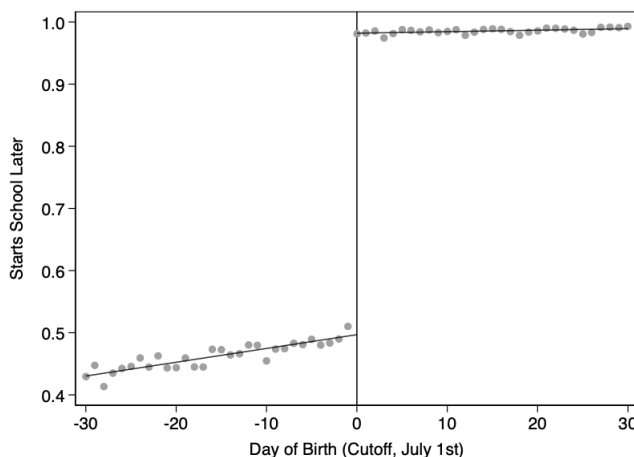
In the main text we focus on intent-to-treat estimates, which by definition are not adjusted by non-compliance rates. We interpret our results as the effect arising from the existence of the school entry rule (analog to being ‘offered’ a treatment) rather than to receiving the treatment itself. As we explain in the main text, we have several reasons supporting this methodological decision.<sup>20</sup>

With that caveat in mind, we think that describing non-compliance help to understand how the school entry rules work in practice in our setup. Below, we document the fraction of students complying (or not) with the cutoff, classifying them as on-time entrants, red-shirters and green-shirters. We also characterize the students in these groups using the covariates in our sample.

**Non-compliance** : The first takeaway is that most of the non-compliance is due to red-shirting. We classify students as on-time students, redshirter or greenshirters. The on-time students are those born in June who started early, or those born in July who started late. The redshirter are those born in June who started school later, and the greenshirters were born in July but enrolled early.

Figure C.4 shows the fraction of students who start later by date of birth. To the left of the cutoff, almost half of June born children do not comply with the rule and delay enrollment (they are the redshirter). To the right of the cutoff, almost all July born children comply with the rule and delay enrollment. The fraction of those born in July who enroll early (greenshirters) is extremely small (about 2%), which is consistent with the enforcement of the rule, which we detail next.

Figure C.4: Starting School Later



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<sup>20</sup>First, taking non-compliance into account would involve computing a LATE, for which we need additional assumptions to hold (e.g., exclusion restriction and monotonicity). Dhuey et al. (2019) provide an eloquent description of these topics. We expand on these issues in the Methods Section. Second, our ITT estimates approximate a lower bound of the effects. The reason is that with a binary instrument (being assigned to start early or late due to the rule), the denominator in the Local Average Treatment effect is always lower than 1. Then the LATE would be mechanically higher than the reduced-form estimate, and thus we see our ITT estimates as conservative.

Table C.3 characterizes on-time students, redshirter and greenshirkers. The main takeaway is that redshirkers tend to come from better socioeconomic backgrounds, which is similar to what has been found in the U.S. <sup>21</sup>

**Table C.3:** Characterizing On-time Students, Redshirkers and Greenshirkers

Variable	(1)	(2)	(3)	(4)
	On-Time	June Born Redshirter	On-Time	July Born Greenshirter
Father's Schooling	10.77	11.05	11.01	10.80
Mother's Schooling	10.72	10.94	10.93	10.75
Girl	0.51	0.46	0.50	0.47
Class Size	28.52	31.55	30.66	28.83
School Vulnerability (0-100)	32.66	27.57	29.32	32.13
School in Capital Region	0.31	0.44	0.37	0.41
Rural School	0.17	0.11	0.14	0.18
Public School	0.55	0.51	0.52	0.50
Voucher School	0.45	0.49	0.48	0.50
Observations	31,858	27,363	57,646	842
Fraction	0.27	0.23	0.49	0.01

## Enforcement of the School-entry Rules

How are school-entry rules enforced in practice? Principals at both at voucher and public schools rely on an important 129 page document called [Circular 1](#). The [Circular 1](#) is essentially a handbook that summarizes all duties, rules to follow, and deadlines that are necessary to be recognized by the National Law as schools and receive the funds.

The government makes no difference between voucher and public schools when overseeing and auditing schools through a National Education Board (NBE or *Superintendencia de Educacion*). The NBE implements some random checks to enforce the Law. This procedure happens in both public and voucher schools because both receive funding in the form of a per-student amount.

Enforcing the school-entry is one of the easiest tasks for the NBE because schools are required to have all children's birth certificates and indicate at which grade they are enrolled. Principals are fined and eventually laid off if they enroll children that are too young to start school (greenshirkers) but there is no penalty for delaying school (redshirking). This is consistent with the data showing almost no greenshirkers but important fraction of redshirkers in schools.

<sup>21</sup>See, e.g, [Bassok and Reardon \(2013\)](#).

## D Appendix: Mediation Analysis

### D.1. Model of Mediation

We consider the following linear model, closely following the notation in Fagereng et al (2021):

$$Y_z = \kappa_z + \underbrace{\sum_{j \in J_p} \alpha_z^j \theta_z^j}_{\text{measured mediators}} + \underbrace{\sum_{j \in J/J_p} \alpha_z^j \theta_z^j}_{\text{unmeasured mediators}} + X' \beta_z + \tilde{\varepsilon}_z \quad (\text{D.1})$$

where  $z$  indexes the child's school starting status (which in our case takes two values, early or late). The Equation (D.1) specifies the potential outcome  $Y_z$  as a function of observed mediator factors (those belonging to set  $J_p$ ), unobserved mediator factors (those not belonging to set  $J_p$ ), a vector  $X$  of predetermined variables used in our main estimations (such as child gender, socioeconomic status, class size, etc. and a smooth function of date of birth) and an idiosyncratic error term  $\tilde{\varepsilon}_z$ .

With unmeasured mediators absorbed in the error term and the intercept, equation (D.1) becomes:

$$Y_z = \tau_z + \sum_{j \in J_p} \alpha_z^j \theta_z^j + X' \beta_z + \varepsilon_z \quad (\text{D.2})$$

Where  $\varepsilon_z = \tilde{\varepsilon}_z + \sum_{j \in J/J_p} \gamma_z^j (\theta_z^j - E(\theta_z^j))$  is a mean-zero error term, and  $\tau_z = \kappa_z + \sum_{j \in J/J_p} \gamma_z^j E(\theta_z^j)$ .

The coefficients associated to mediators and control characteristics are specified in the following way:

$$\alpha_z^p = \alpha_0^p + \alpha^p \cdot z \quad \beta_z = \beta_0 + \beta \cdot z \quad \tau_z = \tau_0 + \tau \cdot z \quad (\text{D.3})$$

For identification of  $(\alpha_0^p, \alpha^p, \beta_0, \beta)$  we need to impose the assumption that unmeasured mediators are uncorrelated with both  $X$  and the measured mediator variables, as shown in Heckman and Pinto (2015). To further simplify the model, Fagereng et al (2021) assume no interactions between treatment and mediators or control characteristics, i.e.  $\alpha^p = \beta = 0$ . This last assumption does not change their results, and does not change ours. We show estimates both imposing and relaxing this assumption in the next subsection.

The last component of the mediation model includes specifying the mediator variables as a function of the school starting status ( $z$ ) and the vector of characteristics  $X$  as:

$$\theta_z^j = \mu_0^j + X' \mu_1^j + \mu_2^j \cdot z + \eta^j \quad j \in J_p \quad (\text{D.4})$$

Where  $\eta^j$  is a mean zero error term. Therefore, the average causal effect of starting school later ( $z'$ ) versus early ( $z$ ) can be decomposed into a direct and an indirect effect as:

$$E(Y_{z'} - Y_z) = (z' - z)\tau + \sum_{j \in J_p} \alpha_0^j E(\theta_{z'}^j - \theta_z^j) = \underbrace{(z' - z)\tau}_{\text{Direct effect}} + \underbrace{\sum_{j \in J_p} \alpha_0^j (z' - z) \mu_2^j}_{\text{Indirect effect}} \quad (\text{D.5})$$

The computation of the direct and indirect effect involves estimating  $(\tau, \alpha_0^j, \mu_2^j)$ . To obtain estimates of  $\tau$  and  $\alpha_0^j$  we estimate:

$$Y = \tau_0 + Z\tau + \sum_{j \in J_p} \alpha_0^j \theta^j + X'\beta_0 + \varepsilon \quad (\text{D.6})$$

where the variable  $Z$  is equal to one if child is born in July and is equal to zero if child is born in June of the same year. To estimate  $\mu_2^j$  we estimate the linear model for the observed mediator variables with each mediator as dependent variable and  $X$  and  $Z$  as regressors. We present our results below.

## D.2. Mediation Results

**Main Results.** Panel A in [Table D.1](#) presents the estimates from equation (D.6), assuming  $\alpha^p = \beta = 0$ . We use college enrollment as our outcome, and parental financial investments and beliefs (defined as in our main text) as observed mediators.<sup>1</sup>

The results show that the mediator variables are strong predictors of college enrollment, ( $\widehat{\alpha}_0^i$  and  $\widehat{\alpha}_0^b$  in the first two rows of [Table D.1](#)) and that the association between school entry and college enrollment remains significant, after conditioning on the mediator variables ( $\widehat{\tau}$ , in the third row).

Panel B in [Table D.1](#) presents the estimates for the  $\mu_2^j$ 's. These estimates are consistent with financial investments and beliefs responding positively to signals about the child's ability.

**Table D.1:** Coefficients from Linear Potential Outcome Equation

	Coefficient	Standard Error
Panel A		
$\widehat{\alpha}_0^i$	0.0548	(0.00235)
$\widehat{\alpha}_0^b$	0.1540	(0.00387)
$\widehat{\tau}$	0.0168	(0.00547)
Panel B		
$\widehat{\mu}_2^i$	0.108	(0.016)
$\widehat{\mu}_2^b$	0.021	(0.007)
Observations	51,818	

Notes: Panel A presents the estimates from equation (D.6), assuming  $\alpha^p = \beta = 0$ . Panel B reports the estimates for the  $\mu_2^j$ 's in the linear model for the observed mediators variables, with each mediator as dependent variable. We use timely college enrollment as our outcome, and parental financial investments and beliefs (defined as in our main text) as observed mediators.

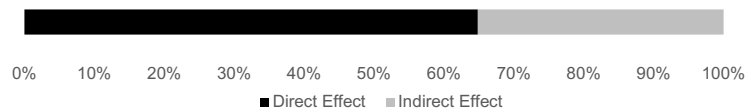
<sup>1</sup>Parental time investments have zero effect on our long run outcomes as we showed in the main text, and are therefore ruled out as mediators.

We use these coefficients to decompose the long run effect into direct and indirect effects as described in equation (D.5). We plot the results in Figure D.1.

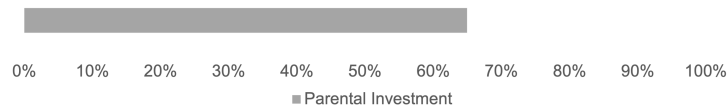
Panel (a) shows that 66% of the total effect is explained by direct effects of starting school later, while 34% of treatment effects are mediated by beliefs and financial investments. As a benchmark, Fagereng et al (2021) find that their mediators (parental wealth transfers being the most important) explain about 37% of the average causal effect from assignment to wealthier parents on children’s accumulation of wealth. In panel (b) we show that approximately two thirds of the mediated (indirect) effect in our setup is explained by financial investment of parents.

**Figure D.1:** Decomposition of Causal Effect on College Enrollment

(a) Direct and Indirect (Mediated) Effects as a Share of the Causal Effects



(b) Parental Investments as a Share of the Indirect (Mediated) Effect

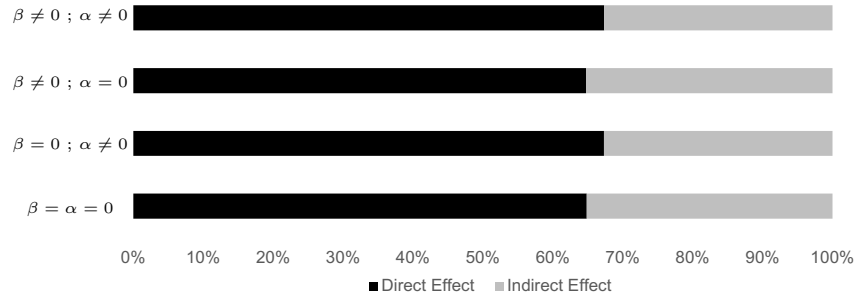


Note: Panel (a) in Figure D.1 decomposes the causal effect of starting school later on college enrollment into shares of indirect and direct effects, as illustrated in equation (D.5). Panel (b) shows how much of the indirect effect can be attributed to financial investments by parents.

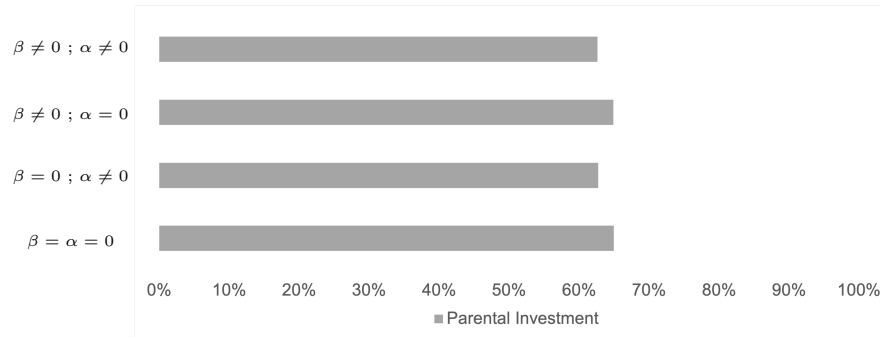
**Robustness.** We now show that the previous results are robust to relaxing the  $\alpha^p = \beta = 0$  assumption. Panel (a) in Figure D.2 shows that the shares of direct and indirect effects remain stable under different assumptions for  $\alpha^p$  and  $\beta$ . Panel (b) shows that the share of parental investments as a share of the indirect effect is also robust to relaxing the  $\alpha^p = \beta = 0$  assumption. Consistently, Table D.2 reports that the estimates of  $(\tau, \alpha_0^j, \mu_2^j)$  remain stable across different restrictions for  $\alpha^p$  and  $\beta$ .

**Figure D.2:** Decomposition of Causal Effect on College Enrollment

(a) Direct and Indirect (Mediated) Effects as a Share of the Causal Effects



(b) Parental Investments as a Share of the Indirect (Mediated) Effect



Note: Figure D.2 replicates Figure D.1 imposing different restrictions on  $\alpha^p$  and  $\beta$ .

**Table D.2:** Robustness: Results under Different Restrictions on  $\alpha^p$  and  $\beta$

	(1)	(2)	(3)	(4)
$\widehat{\alpha}_0^i$	0.0548*** (0.00235)	0.0548*** (0.00235)	0.0492*** (0.00298)	0.0490*** (0.00312)
$\widehat{\alpha}_0^b$	0.154*** (0.00387)	0.154*** (0.00389)	0.152*** (0.00493)	0.152*** (0.00509)
$\widehat{\tau}$	0.0168** (0.00547)	0.0275 (0.0154)	0.0142* (0.00614)	0.0166 (0.0160)
Observations	51,818	51,818	51,818	51,818
Restrictions	$\alpha^p = \beta = 0$	$\alpha^p = 0 ; \beta \neq 0$	$\alpha^p \neq 0 ; \beta = 0$	$\alpha^p \neq 0 ; \beta \neq 0$

Notes: Table D.2 replicates Panel (A) of Table D.1 in column (1) and imposes different restrictions on  $\alpha^p$  and  $\beta$  in columns (2) to (4).

## E Appendix: Children within Same Families

In this section we study multiple children within the same families. We implement a family fixed effects strategy, along the lines of Dhuey, Figlio, Karbownik and Roth (JPAM, 2019), restricting the sample to families where we observe at least two siblings in our data. Then, we further require that these siblings are the first two in the family and that both are born in either June or July.

### Data

The Ministry of Education (MINEDUC) shared with us a dataset with family ID for all students in school between years 2005 to 2014.

*Analytical Sample.* We keep the first two siblings within each family, born in either July or June in years 1996 to 1998, which correspond to the birth cohorts in our main analysis. The estimating samples remain large, varying in size by the availability of the outcomes.

From survey data, we have information on parental investments for 4,604 children (2,302 families) and on beliefs and test scores for 5,320 children (2,660 families)<sup>2</sup>. From administrative records, we have data on college attendance for 18,632 (9,316 families).

### Specification

With these data we estimate the same sibling fixed effects specification as in Dhuey, Figlio, Karbownik and Roth (JPAM, 2019):

$$y_{ij} = \alpha_j + \alpha_1 Z_{ij} + \alpha_2 X_{ij} + \mu_{ij} \tag{E.1}$$

Where all X variables are the same as in the main text, and are now also indexed by a family subscript  $j$ . The Z variable is an indicator for being born in July, and the family fixed effect  $\alpha_j$  accounts for observed and unobserved characteristics that are the same across different siblings within families. We also include order of birth, sex, and year of birth in  $X_{ij}$ . As such, the identifying variation now compares two siblings born in months at each side of the cutoff. We cluster standard errors by family.

### Results

Overall, the results show that controlling for family characteristics that are fixed across siblings does not change our point estimates in a substantial way. These findings suggest that our main results are not driven by family characteristics. Table E.1 reports the sibling fixed effects results (in even columns) compared to our main estimates (in odd columns).

The results from the sibling fixed effects strategy show that July-born children are 4.8 pp more likely to have a computer at home (4.3 pp in our main results), and 4.2 pp more likely to have an Internet connection (3.7 in our main results). In addition, results show that parents are 2.6 pp more

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<sup>2</sup>The availability depends on whether the exam was implemented in the year each sibling attended 4th grade.



likely to believe that their child will complete college (2.1 in our main results). In this case the magnitudes are similar but we lose some precision when using sibling fixed effects.<sup>3</sup>

The last columns show that when using sibling fixed effects July-born children score  $0.201\sigma$  in tests scores at fourth grade ( $0.207\sigma$  in our main results) and are 2.5 pp more likely to attend college on time (3.7 pp in our main results).

**Table E.1:** Robustness of Main Results to Sibling Fixed Effects

	Computer at Home		Internet at Home		College Expectation		Test Scores in 4th Grade		College Enroll	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\widehat{\alpha}_1$	0.043*** (0.008)	0.048*** (0.017)	0.032*** (0.006)	0.042** (0.017)	0.021*** (0.007)	0.026 (0.018)	0.207*** (0.010)	0.201*** (0.030)	0.037*** (0.005)	0.025*** (0.009)
Observations	51,818	4,604	51,818	4,604	51,818	5,320	117,709	5,320	117,709	18,632
Siblings FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Even columns in [Table E.1](#) show the coefficient  $\widehat{\alpha}_1$  estimated from equation (E.1) on measures of parental financial investment and expectations (beliefs). Financial investments and expectations are having computer and Internet connection at home, and expectations refer to whether the caregiver believes the child would complete college in the future. Odd columns report the same estimate but for our main specifications. Robust standard errors (in parentheses) are clustered at the family level. All specifications control for order of birth, sex, and cohort of birth.

<sup>3</sup>This is in part due to lower sample size, and also to lower ‘within-unit’ variability, as written by Lee and Lemieux (JEL, 2010), page 338: “*The inclusion of individual dummy variables may lead to an increase in the variance of the RD estimator for another reason. If there is little ‘within-unit’ variability in treatment status, then the variation in the main variable of interest (treatment after partialling out the individual heterogeneity) may be quite small.*”

## F Appendix: Human Capital Accumulation Model

We present a conceptual framework that describes the mapping of early childhood shocks and parental investments into a child's future human capital, building on multiple studies from the related literature (e.g., Almond et al., 2018, Boneva and Rauh, 2018, Cunha et al., 2010, and Francesconi and Heckman, 2016). In our model parents have beliefs about their child's ability and can make different types of investments (e.g., spending additional time on educational activities or investing additional money on school-related items). Since the choice of the production function might govern the response to early childhood shocks (Almond et al., 2018), we do not presuppose a particular functional form for preferences or technology relating human capital to later outcomes. This allows different investments to vary in magnitude and sign as a response to early shocks.

We consider a simple model with two periods, where the first period is childhood and the second period is child  $i$ 's young adulthood.

Child  $i$ 's human capital in the second period is determined by the following production technology

$$h_{2i} = h(\theta_{0i}, I_{1i}^m, I_{1i}^t, \zeta_{1i}), \quad (\text{F.1})$$

where  $\theta_{0i}$  represents endowed skills,  $I_{1i}^m$  are monetary investments made by parents of child  $i$  (such as school-related expenditures) in period 1,  $I_{1i}^t$  is child  $i$ 's parent time investment (such as mentoring activities) in period 1, and  $\zeta_{1i}$  is a shock during childhood (e.g., a skill advantage in the first grade). We assume that  $h(\cdot)$  is differentiable, monotone, weakly increasing, and concave in  $I_{1i}^m, I_{1i}^t$ .

Parents have an expectation about the level of human capital that their child will achieve in adulthood,  $\widetilde{h}_{2i}$ , which depends on their beliefs about child  $i$ 's skill endowment,  $\widetilde{\theta}_{0i}$ ; parents' investments in their child during the childhood period,  $I_{1i}^m, I_{1i}^t$ ; and the early shock faced by their child. We introduce these beliefs to point out that parents decide to invest considering their child's expected human capital in adulthood, which may differ from the human capital that they finally acquire ( $h_{2i}$ ). Importantly, it may be the case that the shock  $\zeta_{1i}$  does not change the skill endowment of child  $i$ ,  $\theta_{0i}$ , but does change parents' beliefs about it. Parents' perceived child's future human capital can be written as

$$\widetilde{h}_{2i} = h(\widetilde{\theta}_{0i}, I_{1i}^m, I_{1i}^t, \zeta_{1i}). \quad (\text{F.2})$$

During the childhood period, parent  $i$  allocates leisure time  $L_{1i}$  to child time investment,  $I_{1i}^t$ , and own leisure time,  $l_{1i}$ , so that  $L_{1i} = I_{1i}^t + l_{1i}$ . She also chooses how to allocate available money,  $M_{1i}$ , into consumption,  $C_{1i}$ , and monetary investment in children,  $I_{1i}^m$ . Therefore she faces time and budget constraints given by

$$L_{i1} = I_{1i}^t + l_{i1} \quad (\text{F.3})$$

$$M_{1i} = Y_{1i} + w(T - L_{i1}) = C_{1i} + p_I I_{1i}^m, \quad (\text{F.4})$$

where  $Y_{1i}$  is non-labor income,  $w$  denotes wage in the labor market,  $T$  is fixed and represents time available during the day, and  $p_I$  is the unit price of monetary investment (e.g., books, computer),

with the price of consumption normalized to one. Allowing parents to have preferences on their own leisure time, consumption, and expected child's human capital in adulthood, their maximization problem becomes

$$\max_{I_{1i}^m, I_{1i}^t} U(l_{1i}, C_{1i}, \widetilde{h}_{2i}) \quad \text{s.t. (F.2), (F.3), and (F.4);}$$

i.e., the parent chooses different types of investment levels to maximize utility subject to the production technology, budget, and time constraints. The optimal investment strategies in period 1 for the parent of child  $i$  and type of investment  $k$  are given by

$$I_{1i}^{*k} = I^k(\widetilde{\theta}_{0i}, \zeta_{1i}, p_I, Y, w) \quad \text{for } k = m, t. \quad (\text{F.5})$$

Given these optimal investment decisions, the effect of an early shock on human capital in the next period can be decomposed as

$$\underbrace{\frac{\delta h_{2i}^*}{\delta \zeta_{1i}}}_A = \underbrace{\frac{\delta h(\cdot)}{\delta \zeta_{1i}}}_B + \underbrace{\frac{\delta h(\cdot)}{\delta I_{1i}^{*m}} \times \frac{\delta I_{1i}^{*m}}{\delta \zeta_{1i}}}_C + \underbrace{\frac{\delta h(\cdot)}{\delta I_{1i}^{*t}} \times \frac{\delta I_{1i}^{*t}}{\delta \zeta_{1i}}}_D. \quad (\text{F.6})$$

The total effect,  $A$ , equals to a direct effect of an early shock,  $B$ , which can be mitigated or reinforced through behavioral effects of different investment decisions,  $C$  and  $D$ . Given that we assume that human capital is weakly increasing in investments,  $\frac{\delta h(\cdot)}{\delta I_{1i}^{*k}} \geq 0$ , the sign of  $C$  and  $D$  is determined by how parental investments respond,  $\frac{\delta I_{1i}^{*k}}{\delta \zeta_{1i}}$ .

We define a reinforcing investment decision as one that increases investment as a response to a positive shock,  $\frac{\delta I_{1i}^{*k}}{\delta \zeta_{1i}} > 0$ , while a compensating strategy consists in parents increasing investment as a response to a negative shock,  $\frac{\delta I_{1i}^{*k}}{\delta \zeta_{1i}} < 0$ .

Parents might respond to shocks differently by type of investment. We hypothesize that the response would differ by the productivity of each investment given the shock and socioeconomic background of the family. For instance, following a negative shock, parents might compensate by investing more time with the child, which is arguably more productive and affordable than buying a computer if the child is lagging behind. These responses imply that  $\frac{\delta I_{1i}^{*m}}{\delta \zeta_{1i}} = 0$  and  $\frac{\delta I_{1i}^{*t}}{\delta \zeta_{1i}} > 0$ . On

the other hand, a positive shock  $\frac{\delta \widetilde{h}_{2i}^*}{\delta \zeta_{1i}} > 0$  may trigger parents' monetary investment, like buying a computer, but not additional mentoring time (because the child is already performing well), so that  $\frac{\delta I_{1i}^{*1}}{\delta \zeta_{1i}} > 0$  and  $\frac{\delta I_{1i}^{*2}}{\delta \zeta_{1i}} = 0$ . Our rich data and research strategy allows us to test these hypotheses in our empirical analysis.