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Early Skill Gap Effects on Long-Run Outcomes and Parental Investments

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Abstract

This paper examines the effects of skill advantages at age six on different types of parental investments, and long-run outcomes 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 higher in-school performance and college entrance scores, and sizable effects on college attendance and enrollment at more selective institutions, in particular for low-income children. Our findings suggest that parental time investments are neutral to early skills gaps, while monetary investments are reinforcing and likely to be mediating the long-run effects.

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

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

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1 Introduction

In developing and emerging countries, human capital is the most important asset that children acquire to escape poverty in the long run. Yet, several studies show that children's human capital in these same countries is particularly vulnerable to early life shocks (see, e.g., Bharadwaj et al., 2013), with lasting effects on adult outcomes (Almond et al., 2018). Investments by parents can mediate these long term consequences and there is growing interest in understanding how parents' behavior respond to positive or negative shocks on their children.[1] Parental investments have been linked to 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, 2018; Bharadwaj et al., 2018).

Despite the growing literature in this area, several questions are in need of more evidence. These questions are related to whether parents' choices of investments vary by type (e.g., cash or time inputs), how responses exacerbate (if at all) long term effects on their children, and whether other agents around a child's life, like teachers, react to the disadvantages that children face in early life. These questions become more relevant in emerging economies, where children have less resources to cope with early life disparities.

This paper examines the effects of skill gaps generated by school entry rules at age 6 on long-run outcomes up to age 27 in a middle-income country and explores the role of different types of investments in children's future human capital. We first show how skill-advantaged children perform compared to their peers during the first years of school. Second, we show how different parental investments and teacher behavior respond to these gaps in school performance. Third, we show effects on educational outcomes twenty years after initial skill differences. Finally, we interpret our findings within a human capital accumulation framework that connects parental responses to 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. We show in Figure 1 that these age differences translate into large disparities in a host of skills measured just before school entry.[2] We exploit this variation in skills at entry combining twenty years of Chilean administrative micro-data in a regression discontinuity design. We supplement the administrative records with survey data containing information on parental and teacher investments reported by parents and students. Leveraging large sample sizes, we are also able to estimate precisely how our results vary by socioeconomic background.

We find that children who start school with higher ability perform better in several in-school out-

[1] 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., Almond and Mazumder (2013), Doepke and Zilibotti (2017), and Francesconi and Heckman (2016).

[2] 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).

comes like GPA and test scores (by about 0.20 standard deviations), measured at the same grade. We then measure long-run results at the same age and find higher college entrance scores, and higher college enrollment rates, overall (effect size of 14 percent) and at more selective programs (19 percent). In addition, all of our effects are more pronounced for low-income children. The magnitude of our estimates is sizable as compared to the results from the related literature, most of them for developed countries. For instance, Dhuey et al. (2017) report effect sizes of 2.5 and 8 percent, for overall and selective college enrollment, respectively. Moreover, we document that our estimated effect on college enrollment is within those found by the early childhood interventions literature (Elango et al., 2015), suggesting that policy shocks on early skills in developing countries can be as important as programs designed to bolster children's abilities.

We explore the role of parental investments by estimating effects on multiple measures reflecting resources spent on educational assets, beliefs, and involvement with their children, four years after school entry. We argue that while the gaps in skills at entry would be undone if adjusted by age, parents, teachers and children themselves observe and react based on the unadjusted in-school differences. Our results show that children with higher ability at school entry are exposed later on to larger financial investments, like money spent on school-related items or number of books and whether they have a computer at home. Parents have stronger beliefs about their educational future as well, measured by college completion expectations. On the other hand, our results show that parental time investments, like helping with study or homework, and parental involvement measures, like knowing their children's grades, tend to be neutral to differential skills in the first grade.

We further test for investments made by teachers. Using students' reports about teacher behavior in class we find that skill-advantaged children report that teachers solve exercises in class more often, while less-advantaged children indicate that their teachers check their work more often. Although small in magnitude, we interpret these effects as suggestive evidence that teachers behave differently with children of different abilities.

Our paper contributes to the literature of early life disparities and long term outcomes in three ways. First, we contribute with novel evidence on how parents and teachers respond using data on several types of investments.[3] Our results suggest that investment strategies can vary by type and agent. 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 at the same time, other important investments –monetary– can respond differentially to early disparities.[4] In addition, recent research shows that investments might also be formed by parental beliefs about returns. Some recent examples of this research are Biroli et al. (2018), Boneva and Rauh (2018), Dizon-Ross (2019) and Attanasio et al. (2019a). Related to this literature, our results can be interpreted as parents' reaction to signals of their child's

[3]Some examples recent research on parental investments are Adhvaryu and Nyshadham (2016), Akee et al. (2015), Baker and Milligan (2016), Breining et al. (2015), Gelber and Isen (2013), Hsin (2012), Rosales-Rueda (2014), and Yi et al. (2015).

[4]Bharadwaj et al. (2018) find a negative *correlation* between children birth weight and time investments, though that correlation disappears within twins.

ability after observing children's relative performance in the first years of school. Our findings on parents' expectations suggest that beliefs may be an additional mechanism that triggers financial investments.

Our evidence on teacher behavior is also novel in that most of the related literature studies teacher effects on students outcomes, instead of causal effects on teacher behavior. To our knowledge, the closest strand of the literature are studies on teacher expectations and biases on student outcomes (Burgess and Greaves, 2013; Lavy and Sand, 2018; Papageorge et al., 2018). Also, our data on parental involvement and teacher behavior is based on children's survey reports rather than on self-reported parental responses, which may be subject to social desirability bias. In all, we believe that these findings add an empirical thread to the literature on parent and teacher behavior, and child development.

Second, we track students for a long period of time –twenty years– measuring outcomes repeatedly in-between. Few studies on long-term effects of early life disparities 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. (2006), Elango et al. (2015) and Beuermann and Jackson (2018) highlight that effects might fade out in the medium term but emerge in the long run. We measure relevant outcomes at age 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). Also, our sample sizes permit to estimate precise small or null effects, and are particularly useful to study heterogeneous effects by socioeconomic status (SES). Other studies that have examined SES differences on long-run outcomes usually use data from developed countries where initial disparities might be relatively less important compared to Chile, a middle income country with a particularly segregated school system (Hsieh and Urquiola, 2006). For instance, while we uncover important heterogeneity, Black et al. (2011) for Norway and Dhuey et al. (2019) for Florida find small to null differences by SES. Our results show that early life disparities in developing countries can be as determinant as popular early childhood interventions in the US.

Third, we measure long-run outcomes at the same age, and therefore our main results are free of the age-at-test effect, which is persistent in the related literature.[5] In addition, our results are free of interactions with other laws such as a minimum age to drop out from school like in the US. These rules make late enrollees more likely to drop out from school early which affects later outcomes such as college attendance in a different direction (e.g., Cook and Kang, 2016, Deming and Dynarski, 2008, Dobkin and Ferreira, 2010, Hemelt and Rosen, 2016, and Tan, 2017).

Overall, our results emphasize that early skill gaps can have sizable and heterogeneous consequences in adulthood in developing countries and present novel evidence on how responses vary by the type of investment made in children. Still, the extent to which investments can be treated as

[5]Some examples of this literature are Attar and Cohen-Zada (2018), Bedard and Dhuey (2006), Datar (2006), Elder and Lubotsky (2009), Fletcher and Kim (2016), Foureaux Koppensteiner (2018), McEwan and Shapiro (2008),Nam (2014), Peña (2017), Puhani and Weber (2008), and Smith (2010). For other outcomes unrelated to in-school performance, see, for example, Anderson et al. (2011), Cook and Kang (2016), Dee and Sievertsen (2018), and Landersø et al. (2016). Examples of studies on long-run outcomes are Black et al. (2011), Cascio and Schanzenbach (2016), Dobkin and Ferreira (2010), Fredriksson and Öckert (2013), Kawaguchi (2011), and Larsen and Solli (2017).

mediators is suggestive, as we would need an additional source of exogenous variation to identify a causal effect of investments on outcomes (Almond and Mazumder, 2013). Recent studies that aim at disentangling interactions between different interventions and investments are Attanasio et al. (2019b), Duque et al. (2018), Johnson and Jackson (2017), Malamud et al. (2016), and Rossin-Slater and Wüst (2019). Developing a research agenda that studies several types of parental investments and their return by socioeconomic background would deepen our understanding of mediators of early shocks on inequality in adulthood outcomes.

The remaining sections of the paper are organized as follows. In Section 2, we introduce a simple model of human capital accumulation and outline our empirical strategy. In Section 3, we describe our data, while Section 4 describes 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 those 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 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 technology of production:

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

where θ_{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 ζ_{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 ability 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 ζ_{1i} does not change the actual endowment of child i θ_{0i}, but acts through the parent beliefs about it. Parents' perceived child's future human capital can be written as

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

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

$$L_{i1} = I_{1i}^t + l_{i1} \tag{3}$$

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

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

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

i.e., the parent chooses different types of investment levels to maximize utility subject to the perceived technology of production, 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

$$I_{1i}^{*k} = I^k(\widetilde{\theta_{0i}}, \zeta_{1i}, p_I, Y, w) \quad \textit{for} \quad k = m, t. \tag{5}$$

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

$$\underbrace{\frac{\delta h_{2i}^*}{\delta \zeta_{1i}}}_{A} = \underbrace{\frac{\delta h(\cdot)}{\delta \zeta_{1i}}}_{B} + \underbrace{\frac{\delta h(\cdot)}{\delta I_{1i}^{*m}} \times \frac{\delta I_{1i}^{*m}}{\delta \zeta_{1i}}}_{C} + \underbrace{\frac{\delta h(\cdot)}{\delta I_{1i}^{*t}} \times \frac{\delta I_{1i}^{*t}}{\delta \zeta_{1i}}}_{D}. \tag{6}$$

The total effect, A, can be decomposed in 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 implies that the parent increases investment as a response to a negative shock, $\frac{\delta I_{1i}^{*k}}{\delta \zeta_{1i}} < 0$.

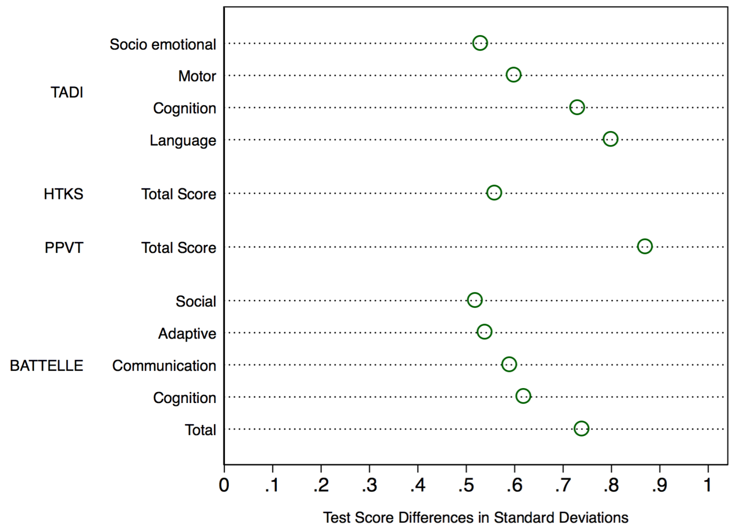
Parents might respond to shocks differently by type of investment. We hypothesize that the response would differ by the productivity of each investment given the shock and socioeconomic background of the family. For instance, following a negative shock, parents might compensate by investing more time 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 doing good), 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. 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 cognitive tests, measured just before starting school. The age differences translate into skill gaps, measured in standard deviation units (σ) that range from 0.52σ to 0.86σ on a battery of tests commonly used in the early childhood literature (e.g., Rubio-Codina et al., 2016).

Figure 1: Baseline Differences in Cognitive Tests and Parental Investments



Note: Figure 1 plots differences in a host of cognitive tests between July and June born children, measured just before children start their respective 1^{st} grade. Due to the birth date cutoff rule, July-born children are a year older than June-born children at school entry. The y-axis shows the measure of different cognitive test and subjects measured. In particular, the BATTELLE test corresponds to the Battelle Developmental Inventory for Young Children; the HTKS test to the Head Toes Knees Shoulders, the PPVT test to the Peabody Picture Vocabulary Test, and 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 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.[6] 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

$$Y_i = \alpha_0 + \alpha_1 Z_i + f(B_i) + \alpha_2 X_i + \mu_i. \tag{1}$$

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 in the same year. $f(B_i)$ is a function that interacts birth date, B_i, with Z_i to allow for different

[6] Chile's academic year runs from March to December. The official enrollment cutoff for the first grade is April 1, but in practice the Ministry of Education allows schools to implement cutoff dates as late as July 1. The data shows that this late cutoff date was the most commonly used by Chilean schools in practice. See for example McEwan and Shapiro (2008).

slopes on each side of the cutoff. μ_i represents the error term that we cluster within birth date. We also include a set of predetermined variables as controls in X_i, such as child gender, SES, class size, school rurality, and type of school. In practice, these control variables have very little effect on our RD estimates and serve mainly to improve precision. We also add year of birth indicators to control for secular trends common to all children.

Our parameter of interest is α_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 we would need additional assumptions to hold,[7] and even if they do, our reduced-form results are still conservative estimates as "naive" two-stage least squares (LATE) estimates would increase the magnitude our estimated effects. As we describe in the following section, 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.

3 Data

We use administrative data 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 is an administrative dataset, maintained by the Ministry of Education (MINEDUC), 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 2018. The 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 and surveys administered in the fourth grade, which we describe below.

We combine these data with three additional sources of information. The first dataset 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

[7] For example, Barua and Lang (2016) suggest that instrumenting for school entry age may violate the monotonicity assumption, and we would need to defend that the exclusion restriction holds in this setup. See Jones (2015) for a discussion on this topic.

admitted to most of the universities in the country.[8] The second and third sources consist of further administrative micro-data from MINEDUC for years 2007 to 2018, with individual records for the population of students who enrolled in, and graduated from, higher education institutions by type of programs (e.g., selective, non-selective) and area of study.

3.2 Parent and Student Surveys

The SIMCE is Chile's System of Education Quality Measurement or *Sistema de Medición de la Calidad de la Educación* in Spanish. The SIMCE consists of a test scores in math and language subjects, accompanied by parent and student surveys, and has been administered to fourth graders every year since 2005. We use two surveys accompanying the SIMCE which cover over about three quarters of the whole student population in fourth grade. 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 financial resources they spend every month on their child's education. These last two variables are reported in brackets, and we dichotomize them in our analysis later on. In particular we define a "higher spending" variable that takes value one for the half of respondents who report to spend more and a "ten or more books" variable because ten books is the median of the distribution. Parents also report their expectations on their child's educational attainment in the future (e.g., "What do you think will be the highest level of education reached by your child in the future?").

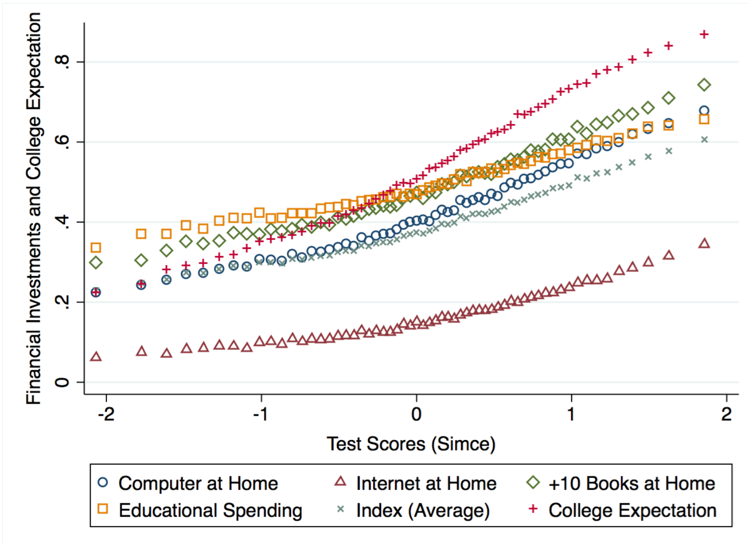
In a separate survey, students are asked about parental engagement and teacher behavior. In particular, children report in a Likert scale whether their parents help them study or work on homework, help them understand difficult subjects, know their grades, and demand they improve their grades. Available answers for each item are on the scale of "Never," "Sometimes," "Most of the time," and "Always." In our analysis later on, we use a binary indicator equal to one if the child answers that her parent does each activity "Most of the time" or "Always" and zero otherwise, though results are robust to the choice of how to group answers. The same survey asks students about their teacher's behavior, with answers reported in the same scale. In particular, students are asked whether the teacher reviews and solves homework in class, explains subjects when asked, explains until students understand, explains the grading schemes in class, and explains how to solve problem sets from textbooks.

Figure 2 and Figure 3 show non-parametric plots of the raw data relating SIMCE test scores (math-language average, in standard deviation units) to parental monetary and time investments, respectively. Each graph is constructed with about 500,000 observations, and we provide further detail on these graphs in Appendix B. Figure 2 shows that parental financial investments and college expectations are positively correlated with test scores, while Figure 3 shows a positive correlation for two measures of parental involvement (whether parents always know and congratulate children for their grades) and a flatter, slightly u-shaped correlation for three other measures

[8]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.

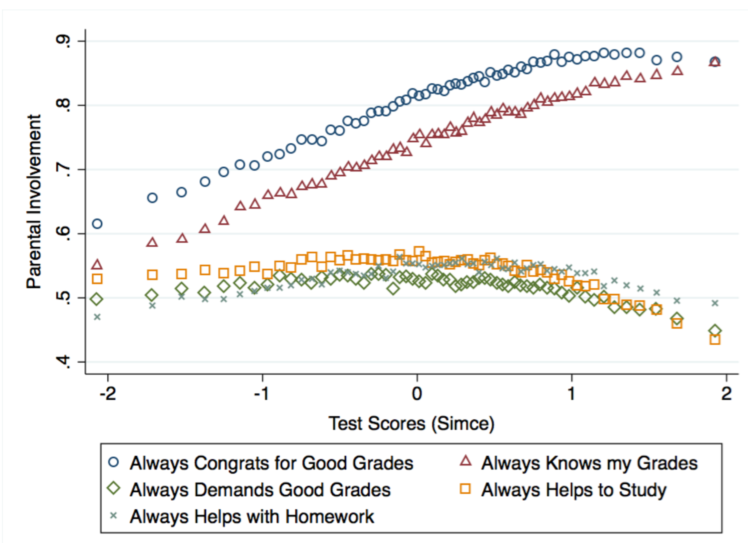
(whether parents demand good grades, help with homework, and help them study).

Figure 2: Financial Investments, College Expectations and Test Scores



Note: Figure 2 plots the mean of the y-axis variables within equal sized bins of SIMCE test scores (math-language average, in standard deviation units) in 4th grade, for a sample of approximately 500K observations. The y-axis variables are all binary and represent our measures of parental monetary investments (having computer, Internet connection and more than ten books at home, spending above the median in educational items and an the average of those four variables in an Index) and whether parents think their child will attend college in the future.

Figure 3: Time Investments and Test Scores



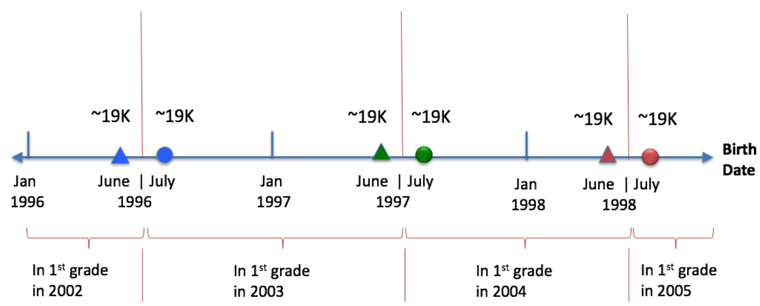
Note: Figure 3 plots the mean of the y-axis variables within equal sized bins of SIMCE test scores (math-language average, in standard deviation units) in 4th grade, for a sample of approximately 460K observations. The y-axis variables represent our measures of parental time investments. Each variable is a binary indicator equal to one if the child answers that her parent does each activity “Most of the time” or “Always” and zero otherwise.

3.3 Working Sample

We study two sets of student cohorts. The first cohorts corresponds to first graders in years 2002 to 2005, and the second cohorts consist of eighth graders in the same years. 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).

We build two working samples. 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 4 shows our research design for first graders. Those born in July just missed the cutoff date and therefore would start the first grade 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. This setup allows us to control for potential idiosyncratic cohort differences and also provides enhanced statistical power for our analysis.

Figure 4: Research Design for 1st Graders



Note: Figure 4 illustrates our research design for first graders. About 19K children were born in either June or July in years 1996 to 1998. Those born in June from year T start the first grade in year T+6, while July-born children start in year T+7.

We observe children once they are in first grade, and ideally we would like to have birth records to avoid attrition between birth and first grade enrollment. In addition, if attrition were 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 the 7 percent of children (who enroll in private schools), then we get 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.

The data permit us to measure outcomes up to age 20 for first graders because the youngest first graders were born in 1998 and we have data up to year 2018. Analogously, the younger eighth graders were born in 1991, and hence we can follow them until they are 27 years old in 2018.

3.4 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 we 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 of the 12 months of the year should account for 8.3 percent of the births.

The covariates situate our data in the context of a middle-income developing country. For instance, the average parental schooling is close to 11 years, which is less than the 12 years needed to get a high school diploma. The levels of schooling in Chile are higher today,[9] 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 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). The index considers poverty of students and risk of dropout as factors, as detailed in JUNAEB (2019). 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, with the remaining 49 percent of students attending school in urban areas outside the Metropolitan Region. Finally, 52 percent of first graders and 57 percent of eight graders attend public schools, with the remaining fraction attending voucher schools.[10]

[9] 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).

[10] Public schools are both publicly funded and administered. Voucher schools receive public funding but are privately managed, similar to charter schools in the US.

4 Results

We start by discussing average effects, with a brief description of results on in-school outcomes to focus then on longer run results. Next we describe our findings by SES. We end with the effects on parental investments and teacher behaviors, and interpret the results within our economic conceptual framework.

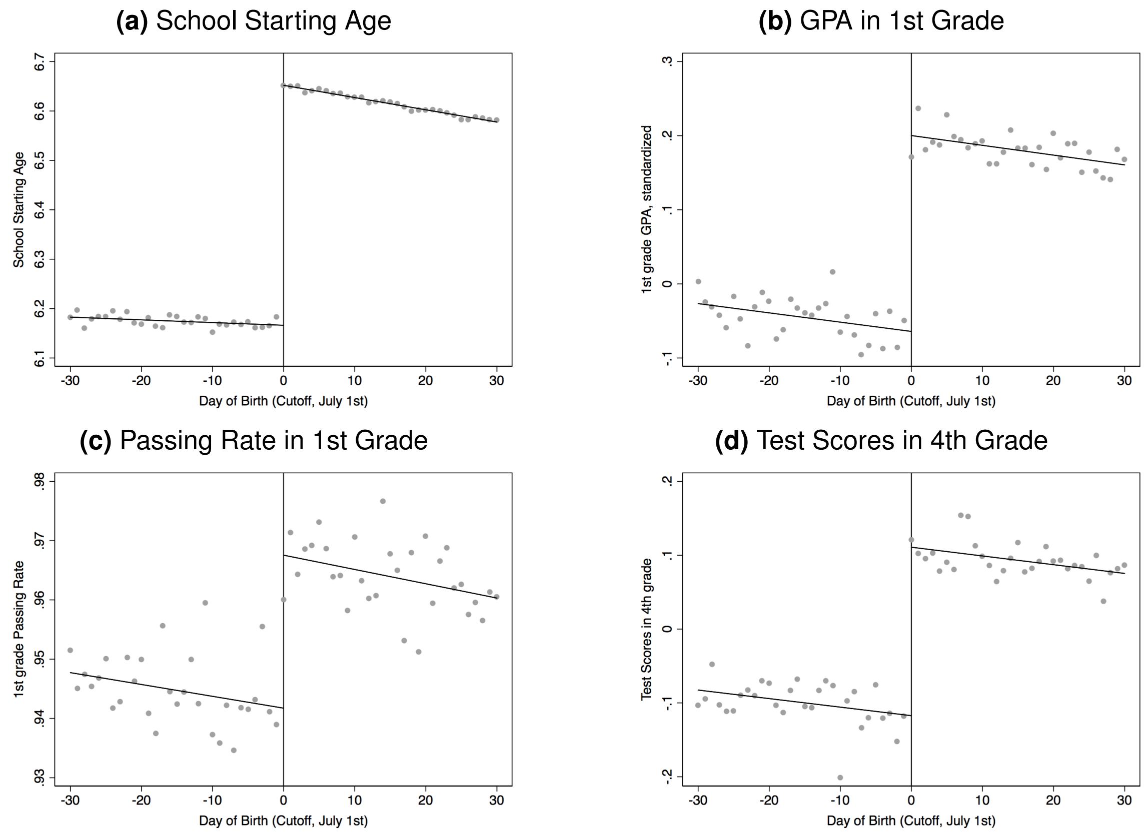
4.1 Average Effects

In Figure 5 and Table 2 we present in-school results for our sample of first graders. July-born children start the first grade 0.48 years older than their June-born counterparts (see Figure 5a) and so are more likely to enjoy a skill advantage over those who start younger, as discussed previously and depicted in Figure 1. Figures 5b, 5c, and 5d show that a skill advantage in the first grade translates into higher GPAs (0.26σ) and higher passing rates (2.4 percentage points (pp)) in the first grade and higher test scores (0.21σ) in the fourth grade.

These positive results on in-school outcomes are consistent with the findings from the related literature. As some studies have highlighted (e.g., Cascio and Schanzenbach, 2016, Black et al., 2011), these in-school effects are composed by an age effect and the effect given by the age of measurement (an 'age-at-test' effect).[11] While for short run effects these components are non-separable, our data allows to rule out the age-at-test effect for longer run outcomes, as we describe below.

[11] Some studies further decompose age at test as absolute, and relative to other test takers. See, e.g., Pena (2017); Pena and Duckworth (2018).

Figure 5: School Starting Age and In-school Results



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. Each day of birth contains about 2K observations. Table 2 shows the results from the estimation of equation (1) for these outcomes.

We construct long-run outcomes that are not subject to 'age at measurement' effects measuring them at the same age later on. In particular, we measure outcomes up to age 20 for first graders and 27 for eighth graders, allowing several years after the exact time in which students should have completed school had they progressed with no interruptions. In addition, our estimates do not interact with minimum dropout age laws as there is none in Chile. In other contexts, like the US, older starters complete less education at the minimum dropout age due to the school-leaving age rule (e.g., Angrist and Krueger, 1991, Cook and Kang, 2016, and Dobkin and Ferreira, 2010).

Given the absence of a minimum dropout age we do not expect to find sizable effects on school completion rates. Indeed Table 3, columns (2) and (3), show precise zero effects on primary school graduation (effect size of 1 percent) and high school completion (effect size of 0.6 percent).

In contrast, we expected to find effects on college related outcomes since higher education enrollment is not mandatory. We first look at the probability of taking the national college entrance exam, which proxies for whether students want to pursue higher education beyond the 12th grade. Figure 6a and column (3) in Table 3 show that July-born students are 6 percent (3.6 pp over a mean of 59 percent) more likely to take the test up at age 20. This is a good measure of ever taking the entrance exam because each year, a very small fraction of all test-takers (less than 5 percent) take the test after turning 20 years old.

Moreover, students with an early skill advantage score 0.07σ higher, as shown in Figure 6b and in column (4) of Table 3. 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.

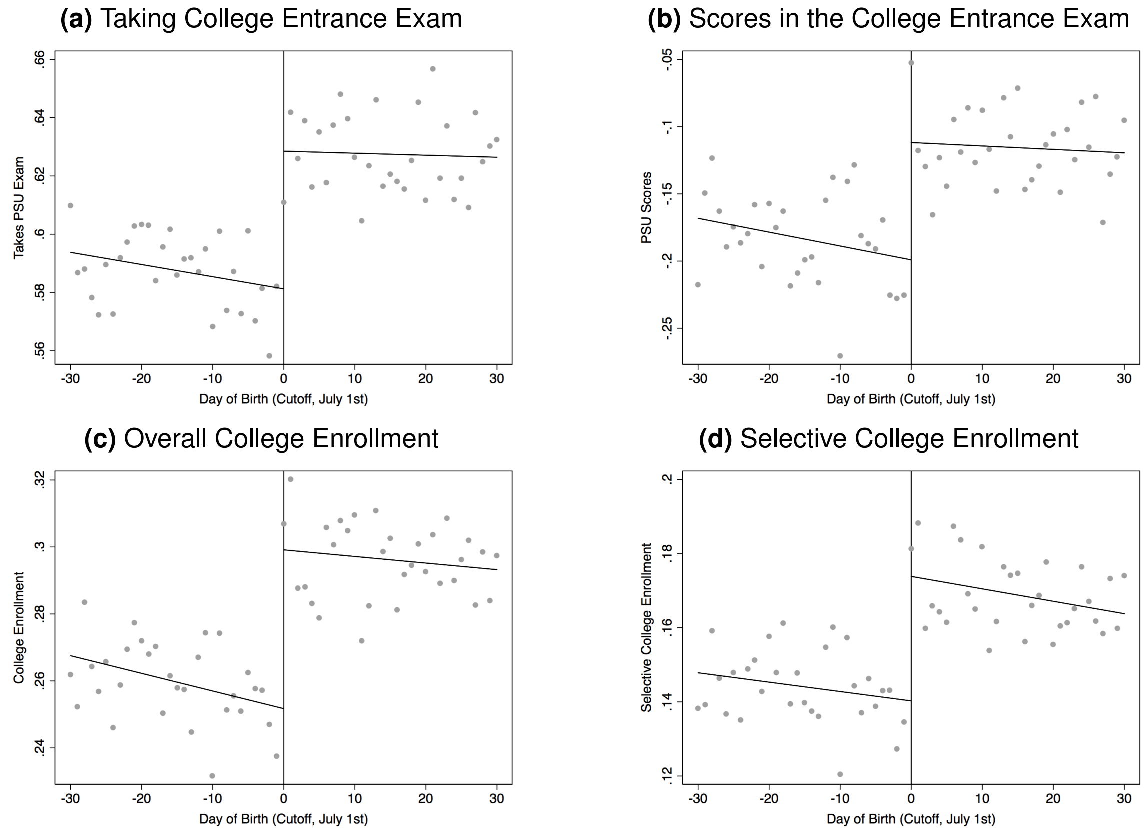
The next results show indeed effects on multiple measures of college enrollment, reported in columns (5) to (8) in Table 3. We find a 14 percent increase (3.7 pp over a mean of 26 percent) in college enrollment (see Figure 6c) at any university for July-born students, which is consistent with the positive effects on the likelihood of being a test-taker. Meanwhile, the effects on enrollment at more selective[12] universities (19 percent; 2.8 pp over a mean of 14 percent, see Figure 6d) and STEM programs (14 percent; 1 pp over a mean of 7 percent) are consistent with higher PSU scores.

We find the magnitude of these estimates to be sizable compared to small or null effects found for similar and recent studies in developed countries. For instance, with data from a particular school district in Florida, Dhuey et al. (2017) find effect sizes of a similar skill gap of 2 percent on college enrollment, while Cascio and Schanzenbach (2016) find no effects on taking the college entrance exams in the US. Other earlier studies finding relatively small or null effects on college entrance and take-up of college entrance tests are Dobkin and Ferreira (2010) and Lincove and Painter (2006).

We finally use our sample of eighth graders to estimate effects on college graduation later on. We find a precise zero effect, shown in the last column of Table 3. On average, a share of 14 percent of both June- and July-born students obtain a college degree by age 27. The rates are similar to back of the envelope computations from official reports by MINEDUC on higher education completion rates (MINEDUC, 2019).

[12] The selective universities are non-profit institutions that existed before 1982, grouped in the Council of Rectors of the Universities of Chile (CRUCH), and they receive the higher scoring students of the country.

Figure 6: Longer Run Results



Note: The graphs in Figure 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.

4.2 Skill Advantage and Socioeconomic Background

In our conceptual framework we hypothesize that the effects of an early skill advantage might vary by the socioeconomic background of the family, as investments by parents are determined by income and other characteristics. In this section we show that is the case. For college related outcomes, point estimates are similar by SES but effect sizes (i.e., point estimate in relation to the mean) are especially higher for the more disadvantaged students.

As described in Section 3, our data contain measures of parental schooling and school vulnerability, which we use as proxies for SES. Parental schooling is available for a subsample, and results are almost identical to using school vulnerability, so we use the latter to describe our analysis. In addition, it is well documented that differences between students in Chile come from between, rather than within, school characteristics (Mizala et al., 2007; Santiago et al., 2017), which provides additional support to use the school-level measure.

We present results for lower and higher SES groups in panels A and B of Table 4. Students belong to the lower SES group if their school has a vulnerability index above the median of the national index distribution, and they belong to the higher SES group otherwise. Our results remain similar if we split the sample in different ways, as we show in Appendix B.

The estimates for primary school and high school completion remain close to precise zeros for both groups, as we show in columns (1) and (2) in panels A and B. Effect sizes are smaller than 2 percent. For the remaining outcomes in Table 4, the overall pattern shows similar point estimates but higher effect sizes for lower SES students.

July-born children have a probability of taking the college entrance exam that is higher by 7 percent (3.4 pp over 46 percent) and 5 percent (3.8 pp over 72 percent) for the lower and higher SES group, respectively. The test scores increase similarly by 0.08σ and 0.07σ for each group, but younger students in the lower SES group had a much lower baseline score, –0.45σ versus 0.00σ for the high SES students. This result implies that we should see a relatively larger effect on college enrollment for lower SES students.

Consistently, we find that college enrollment increases 19 percent for the lower SES students with a skill advantage (3 pp over 15 percent) and 12 percent for more affluent students (5 pp over 38 percent). Moreover, the results show that lower SES students are 32 percent more likely to enroll in selective colleges (3 pp over 9 percent), while the same effect size for their higher SES counterparts is about half of that (14 percent, or 3 pp over 21 percent). Effects on enrollment in STEM programs are small for lower SES students (half a pp over a mean of 4 percent), which reflects that these programs require high scores for admission. The effect for the higher SES students is 2 percent over an 11 percent mean.

Finally, the results also show that the null average effect on college graduation in Table 4 was masking an economically important effect size of 13 percent for the low SES group (a precisely estimated pp increase over a mean of 7 percent) and a precise zero for the higher SES group. These more affluent students already graduate from college at a rate of 21 percent, equivalent to a three-fold difference over their low SES counterparts.

Overall, the findings by SES show that the long-run effects of early skill gaps are relatively larger for low SES students. In contrast, Dhuey et al. (2019) for Florida in the US and Black et al. (2011) for Norway, report small to null differences by SES. In the next section we document effects on parental and teacher behaviors, which could play a role as potential mechanisms behind the persistent effects of early skill gaps on long-run outcomes.

4.3 Effects on Parental Investments, Parental Beliefs, and Teacher Behavior

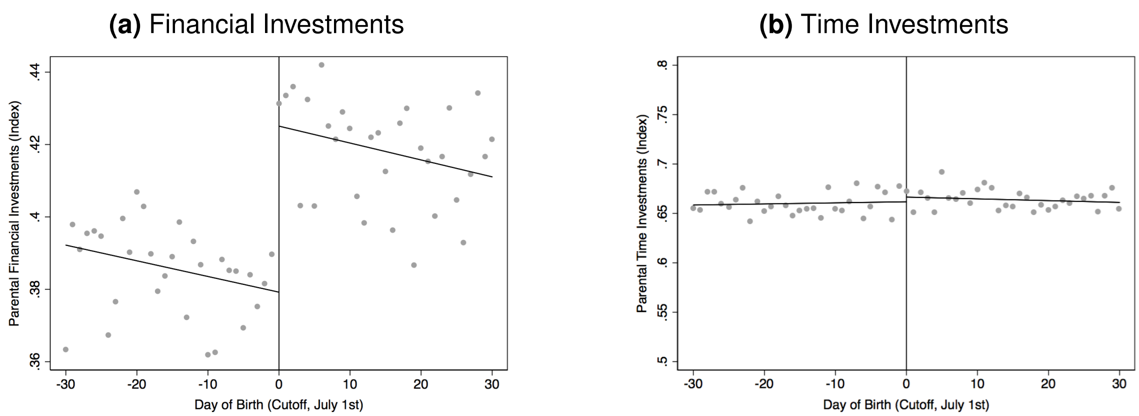
In this section we examine effects on a host of parental and teacher behaviors reported by parents and students when children are in the fourth grade. By then, parents have already received information on their child's performance over four years. Survey data from SIMCE show that 74 percent (panel B of Table 5) of students report that their parents know their grades, which means that parents of July-born children are mostly aware that they perform relatively well, while parents of June-born children also know that their child is performing relatively worse.

Overall, our results indicate that parents of July-born children spend more resources on monetary investments, while June- and July-born children report no differences in time investments made by their parents. 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.

Average Responses. We first examine parental investments on educational goods and related spending in panel A from Table 5. We find that parents of July-born children report higher investments of this sort. Columns (1) to (3) show that their children are 10 percent (4.3 pp over 42 percent) more likely to have a computer at home, 20 percent (3.2 pp over 16 percent) more likely to have an Internet connection, and 8 percent (3.7 pp over 48 percent) more likely to have ten or more books at home.

In addition to investing in more educational assets, parents also report spending more money on school-related items on a monthly basis. In particular, they are 5 percent (2.2 pp over 49 percent) more likely to spend above the median of our sample (column (4)) every month. We average all previous outcomes and plot the results in Figure 7a and column (5). According to this measure, July-born children receive approximately 10 percent of additional monetary resources (3.4 pp over 39 percent). In addition, column (6) of panel A shows that parents are 4 percent (2.1 pp over 53 percent) more likely to believe that their child will attend college in the future. This result is consistent with parental beliefs positively responding to signals about their child's ability endowment.

Figure 7: Parental Investments



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.

We examine parental responses in terms of time investments in panel B of Table 5, with information reported by the children. Overall, our findings suggest that parents do not invest time differentially between July- and June-born children. We find precise zero effects in whether parents congratulate their child for good grades (mean of 81% for both groups of children) and whether they know their grades (74%). This is despite the strong positive correlation between these variables and test scores that we showed in Figure 3. We also find precise zeros in whether parents demand good grades (mean of 52% for both groups) and whether they help with study (55%), and a small positive effect on whether parents help with homework (2pp over a mean of 57%).

Next, we examine students' perception of teacher behavior in different class activities. Panel C of Table 5 shows the results, which are suggestive of small effects on teacher behavior for children with different abilities.

In particular, June-born children are 3 percent more likely to report that their teacher checks whether they did their homework (column 1), while students had the initial skill advantage (July-born children) are more likely to report that teachers explain course topics to the class (2 percent, column 3), solve problems (5 percent, column 4), and homework in class (3 percent, column 6).

Analyses by Socioeconomic Status. In Table 6 we document whether parental investments, beliefs, and teacher behavior react differentially by SES. The results show that parental monetary investments and expectations of college attainment are more intensive for lower SES students, while time investments and teaching behaviors tend to be similar across socioeconomic status.

Panel A shows that effect sizes for parental monetary investments are twice or three times as large for the lower SES children. In particular, effect sizes of having computer, Internet and more than ten books at home are 19, 44, and 11 percent for the more disadvantaged students and 7, 15, and 6 percent for their more affluent counterparts. In terms of educational spending, we find that effect sizes of investing highly in school-related items are 7 percent for low SES and 3.5 percent for the high SES group of students. In addition, parents from a low SES are 6 percent more likely to believe that their child will attend college, while the relative change in beliefs for parents in the high SES group is 3 percent.

Panel B in Table 6 indicates that higher and lower SES groups exhibit time investments that are neutral to early skill gaps, with precise zero estimates for each variable. In Panel C, effects on the perception of teacher behavior is also similar regardless of SES status.

4.4 Discussion

Our findings show that entering school with a skill advantage positively affects children's school performance during the first four years of school. Subsequently, in fourth grade parents make a greater investment in school-related items when their child performs better in school, which may widen the gap created by the initial differences in children's skill stock. Several years later, the results show that skill-advantaged students are more likely to attend college and enter selective higher education institutions.

Going back to our conceptual framework, our empirical results show that the long-run effects of an early childhood shock are positive so that in Equation 6 the component A is positive and in some cases large, relative to what others have found for a similar shock and outcomes. The sign of our objects on parental responses indicates that monetary investments reinforce the shock (i.e., C is positive) and time-intensive investments are neutral (i.e., D is zero). This suggests that long-term effects of an early skill gap, A, are being explained by a direct effect and reinforcing financial investments from parents, $B + C$. In

To fully understand the causal path by which these investments compensate or reinforce early shocks, one would need to take into account that investments may have changed the education production function and therefore may be themselves endogenous in that characteristics of parents who respond to these shocks may be different from those who remain neutral. Therefore, to identify the interaction between shocks and investments would also require an exogenous variation on investments. Examples of recent papers studying these type of interactions are Duque et al. (2018), Johnson and Jackson (2017), Malamud et al. (2016), and Rossin-Slater and Wüst (2019). 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. (2019b) and Jervis (2017).

Finally, we find useful to compare our main effect (of 14.2 percent) on college attendance to some of the popular early childhood interventions in the literature. 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 using their entire sample. Likewise, Ludwig and Miller (2007) find that Head Start increases the likelihood of attending college in five percent. Deming (2009) shows that the same program increases the probability of attending college in ten percent approximately.[13] Anderson (2008) shows that participants of the Perry Preschool Program are 21 percent more likely to attend any college.[14] Our estimated effect on college enrollment is within those found by the early childhood interventions literature, suggesting that policy shocks on early skills in developing countries can be as important as programs designed to bolster children's abilities.

5 Conclusion

This paper studies parental and teacher investments, and long-term effects of having an early skill advantage at school entry in a middle-income country. Combining rich administrative records with an RD design, we find lasting effects after twenty years. Students with an early skill advantage end up being more likely to enroll in college and at selective programs, with larger effects for low SES students. These results add to the literature by emphasizing that early skill gaps can have sizable and heterogeneous consequences in adulthood in developing countries.

In addition, our results are also relevant beyond the context of developing countries because we present novel evidence on teacher behaviors and parental responses by the type of investment they make in their children. We find that teachers behave differentially according to children's skill. Parents of children with the early skill advantage reinforce the positive effects with monetary investments, while time investments are neutral to differences in child's ability or performance. Moreover, reinforcing financial investments are more pronounced for the group of children with lower SES backgrounds.

[13] Deming (2009) report their results in percentage points. We translate the percentage point increase to percentage change using the control mean reported by Ludwig and Miller (2007) who study the same program and outcome using Census data.

[14] Anderson (2008) reports results separately by gender, which we average to compute the overall effect.

Our findings emphasize the importance of developing a research agenda that studies several types of parental investments and their return by socioeconomic background. Evidence from different contexts would deepen our understanding of parental responses as mediators of early shocks on inequality in adulthood outcomes.

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Tables

Table 1: Summary Statistics

Variable	(1) 1st Graders Sample	(2) All	(3) 8th Graders Sample	(4) All
Born in June	0.50	0.08	0.50	0.08
Born in July	0.50	0.08	0.50	0.08
Father's Schooling	10.95	10.89	N.A.	N.A.
	(3.61)	(3.63)		
Mother's Schooling	10.88	10.82	N.A.	N.A.
	(3.52)	(3.53)		
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

Notes: Table 1 shows the mean of each variable in the rows, with standard deviation in parentheses for non-dichotomic variables. The first two columns describe students in first grade in 2002 to 2005. Columns (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 variables 'Born in June (July)' take value 1 if the student was born in June (July). The measures of parental schooling refer to years of completed education and come from SIMCE surveys with 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 rest of the covariates come from administrative data and have zero missing values. The school vulnerability index measures percentage of students receiving free or reduced price lunch.

Table 2: School Starting Age and In-School Outcomes

	(1) Age at Entry	(2) GPA in 1st Grade	(3) Pass Rate in 1st Grade	(4) Test Scores in 4th Grade
$\widehat{\alpha_1}$	0.482***	0.263***	0.024***	0.209***
	(0.003)	(0.012)	(0.003)	(0.011)
June Mean	6.175	-0.045	0.945	-0.099
Effect Size	0.078	5.916	0.025	-
Observations	117,709	117,709	117,709	101,915

Notes: Table 2 show the coefficient $\widehat{\alpha_1}$ estimated from the equation (1) for first graders on age at school entry, and in-school outcomes. These are first grade GPA (GPA1, standardized within school and grade) and pass rate (Pass1), and the Language-Math score in 4th grade. Robust standard errors (in parentheses) are clustered by day of birth.

Table 3: Main Outcomes

	(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
$\widehat{\alpha_1}$	0.013***	0.004	0.036***	0.072***	0.037***	0.028***	0.010***	0.003
	(0.003)	(0.005)	(0.006)	(0.014)	(0.005)	(0.004)	(0.003)	(0.004)
June Mean	0.918	0.693	0.588	-0.183	0.260	0.144	0.071	0.139
Effect Size	0.014	0.006	0.062	-	0.143	0.194	0.138	0.025
Observations	117,709	117,709	117,709	71,509	117,709	117,709	117,709	111,664

Table 4: Main Outcomes, by Socioeconomic Status

Panel 1: Lower SES

	(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
$\widehat{\alpha_1}$	0.016***	-0.001	0.034***	0.076***	0.028***	0.027***	0.005*	0.009***
	(0.005)	(0.009)	(0.009)	(0.016)	(0.006)	(0.005)	(0.003)	(0.003)
June Mean	0.891	0.627	0.464	-0.453	0.147	0.085	0.038	0.068
Effect Size	0.018	0.001	0.073	-	0.193	0.316	0.120	0.127
Observations	58,846	58,846	58,846	28,423	58,846	58,846	58,846	55,841

Panel 2: Higher SES

	(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
$\widehat{\alpha_1}$	0.009**	0.009	0.038***	0.070***	0.045***	0.028***	0.015***	-0.003
	(0.004)	(0.007)	(0.007)	(0.020)	(0.008)	(0.007)	(0.005)	(0.006)
June Mean	0.946	0.761	0.716	-0.001	0.377	0.205	0.106	0.212
Effect Size	0.010	0.012	0.054	-	0.119	0.137	0.146	0.013
Observations	58,863	58,863	58,863	43,086	58,863	58,863	58,863	55,823

Notes: Table 3 and Table 4 show the coefficient $\widehat{\alpha_1}$ estimated from the equation (1) on a host of eight dependent variables. For reference, we include the June-mean, and the 'Effect Size', computed as $\widehat{\alpha_1}/$(June Mean) for binary outcomes, in both Tables. Table 3 presents results for our sample of first graders in columns (1) to (7) and for eight graders in column (8). Table 4 shows effects on the same outcomes for Low-SES in Panel 1 and High-SES in Panel 2. 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.

Table 5: Parental Investments and Teacher Behavior, 4th Grade

Panel A: Parental Financial Investments and Beliefs

	(1) Computer at Home	(2) Internet at Home	(3) More 10 Books	(4) High Spending	(5) Average (1)-(4)	(6) College Expectation
$\widehat{\alpha_1}$	0.043***	0.032***	0.037***	0.022***	0.034***	0.021***
	(0.008)	(0.006)	(0.008)	(0.007)	(0.005)	(0.007)
June Mean	0.419	0.158	0.483	0.485	0.386	0.532
Effect Size	0.104	0.204	0.078	0.046	0.088	0.039
Observations	51,818	51,818	51,818	51,818	51,818	51,818

Panel B: Parental Time Investments: 'My parent...'

	(1) ...congrats for good grades	(2) ...knows my grades	(3) ...demands good grades	(4) ...helps to study	(5) ...helps with homework	(6) Average1 (1)-(2)	(7) Average2 (3)-(5)
$\widehat{\alpha_1}$	-0.010	0.010	0.001	0.001	0.018**	0.000	0.007
	(0.006)	(0.010)	(0.012)	(0.009)	(0.009)	(0.006)	(0.008)
June Mean	0.806	0.738	0.524	0.552	0.569	0.772	0.548
Effect Size	-0.012	0.013	0.002	0.001	0.032	0.000	0.012
Observations	47,646	47,646	47,646	47,646	47,646	47,646	47,646

Panel C: Teacher Behavior: 'My teacher...'

	(1) checks homework	(2) explains if asked	(3) explains for everybody	(4) solves homework	(5) solves exams	(6) solves exercises	(7) Average (2)-(3)	(8) Average (4)-(6)
$\widehat{\alpha_1}$	-0.021***	0.008	0.015*	0.031***	-0.009	0.016**	0.012	0.013**
	(0.007)	(0.008)	(0.008)	(0.010)	(0.006)	(0.008)	(0.007)	(0.005)
June Mean	0.711	0.711	0.684	0.607	0.500	0.637	0.697	0.581
Effect Size	-0.030	0.011	0.022	0.051	-0.018	0.025	0.017	0.022
Observations	51,879	51,879	51,879	51,879	51,879	51,879	51,879	51,879

Notes: Table 5 shows the coefficient $\widehat{\alpha_1}$ estimated from the equation (1) on measures of parental financial investment and expectations (Panel A), parental time investments (Panel B) and teacher behavior (Panel C). The financial investments and expectations in Panel A are: PC - having computer (column (1)) and Internet connection at home (column (2)), spending in school-related items over the median (column (3)), number of books at home above the median (more than ten books) and whether the child would complete college in column (5). The parental investments in Panel B come from answers from the child to questions like 'does your parent know your grades?' 'does your parent help you when the material is difficult?'. The teacher behaviors from Panel C also come from answers from the child to questions like 'does the teacher check your homework?' 'does the teacher solve the problem-set during the class?' 'does the teacher explain properly?'. All estimations include cohort fixed effects and controls for child gender, class size, school vulnerability, school rurality and type of school (public, voucher or private). Robust standard errors (in parentheses) are clustered by day of birth.

Table 6: Parental Investments and Teacher Behavior by SES

Panel A: Parental Financial Investments and Beliefs

Lower SES

	(1) Computer at Home	(2) Internet at Home	(3) More 10 Books	(4) High Spending	(5) Average (1)-(4)	(6) College Expectation
$\widehat{\alpha_1}$	0.047***	0.023***	0.040***	0.022**	0.033***	0.021*
	(0.012)	(0.006)	(0.012)	(0.010)	(0.007)	(0.012)
June Mean	0.251	0.052	0.355	0.317	0.244	0.364
Effect Size	0.188	0.441	0.113	0.070	0.136	0.057
Observations	25,899	25,899	25,899	25,899	25,899	25,899

Higher SES

	(1)	(2)	(3)	(4)	(5)	(6)
$\widehat{\alpha_1}$	0.039***	0.040***	0.034***	0.023**	0.034***	0.021***
	(0.012)	(0.011)	(0.009)	(0.011)	(0.008)	(0.009)
June Mean	0.588	0.265	0.612	0.653	0.530	0.701
Effect Size	0.067	0.151	0.056	0.035	0.065	0.030
Observations	25,919	25,919	25,919	25,919	25,919	25,919

Panel B: Parental Time Investments: 'My parent...'

Lower SES

	(1) ...congrats for good grades	(2) ...knows my grades	(3) ...demands good grades	(4) ...helps to study	(5) ...helps with homework	(6) Average1 (1)-(2)	(7) Average2 (3)-(5)
$\widehat{\alpha_1}$	-0.018*	-0.001	0.010	-0.002	0.019	-0.010	0.009
	(0.010)	(0.013)	(0.014)	(0.015)	(0.012)	(0.009)	(0.011)
June Mean	0.786	0.720	0.533	0.555	0.564	0.753	0.551
Effect Size	-0.023	-0.001	0.019	-0.003	0.034	-0.013	0.017
Observations	24,683	24,683	24,683	24,683	24,683	24,683	24,683

Higher SES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\widehat{\alpha_1}$	0.002	0.017	-0.015	-0.006	0.013	0.009	-0.003
	(0.008)	(0.011)	(0.016)	(0.012)	(0.010)	(0.007)	(0.009)
June Mean	0.831	0.760	0.500	0.528	0.566	0.796	0.531
Effect Size	0.002	0.022	-0.031	-0.012	0.024	0.011	-0.005
Observations	27,196	27,196	27,196	27,196	27,196	27,196	27,196

Panel C: Teacher Behavior: 'My teacher...'

Lower SES

	(1) checks homework	(2) explains if asked	(3) explains for everybody	(4) solves homework	(5) solves exams	(6) solves exercises	(7) Average (2)-(3)	(8) Average (4)-(6)
$\widehat{\alpha_1}$	-0.019*	0.014	0.026*	0.030**	-0.018	0.022	0.020*	0.011
	(0.011)	(0.010)	(0.014)	(0.012)	(0.011)	(0.014)	(0.010)	(0.009)
June Mean	0.740	0.705	0.683	0.604	0.505	0.633	0.694	0.580
Effect Size	-0.026	0.020	0.037	0.049	-0.035	0.035	0.029	0.020
Observations	24,683	24,683	24,683	24,683	24,683	24,683	24,683	24,683

Higher SES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\widehat{\alpha_1}$	-0.022*	0.003	0.005	0.032**	-0.001	0.011	0.004	0.014*
	(0.012)	(0.010)	(0.010)	(0.014)	(0.011)	(0.010)	(0.009)	(0.008)
June Mean	0.685	0.716	0.684	0.609	0.495	0.641	0.700	0.582
Effect Size	-0.032	0.004	0.008	0.053	-0.002	0.018	0.006	0.024
Observations	27,196	27,196	27,196	27,196	27,196	27,196	27,196	27,196

Notes: Table 6 estimates effects on the same outcomes from Table 5, but splitting the sample between Low and High SES at each Panel A, B and C. All estimations include cohort fixed effects and controls for child gender, class size, school vulnerability, school rurality and type of school (public, voucher or private). Robust standard errors (in parentheses) are clustered by day of birth.

Appendices

A Appendix: Robustness

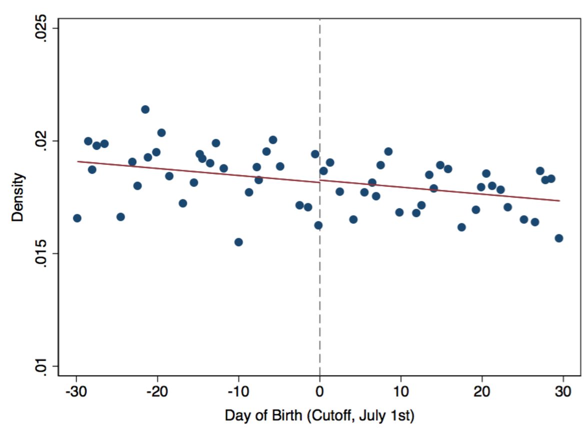
A.1 Density of Running Variable

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 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	0.000	0.000	0.000	0.001	-0.000	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(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

Our research design mimics a local experiment where children are exogenously (to potential outcomes) allocated to either being born in June or July. 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. shows the results of estimating equation (1) using each covariate in Table 1 as dependent variable. One important point to keep in mind is that we have much statistical power given our big sample sizes and therefore some point estimates are significant. However, the magnitudes are small enough to interpret those coefficient as precise zeros (effect sizes are never higher than 0.02) and moreover, our main estimates remain practically unchanged when we add covariates. In Table A.3 we show that point estimates change in the third decimal point.

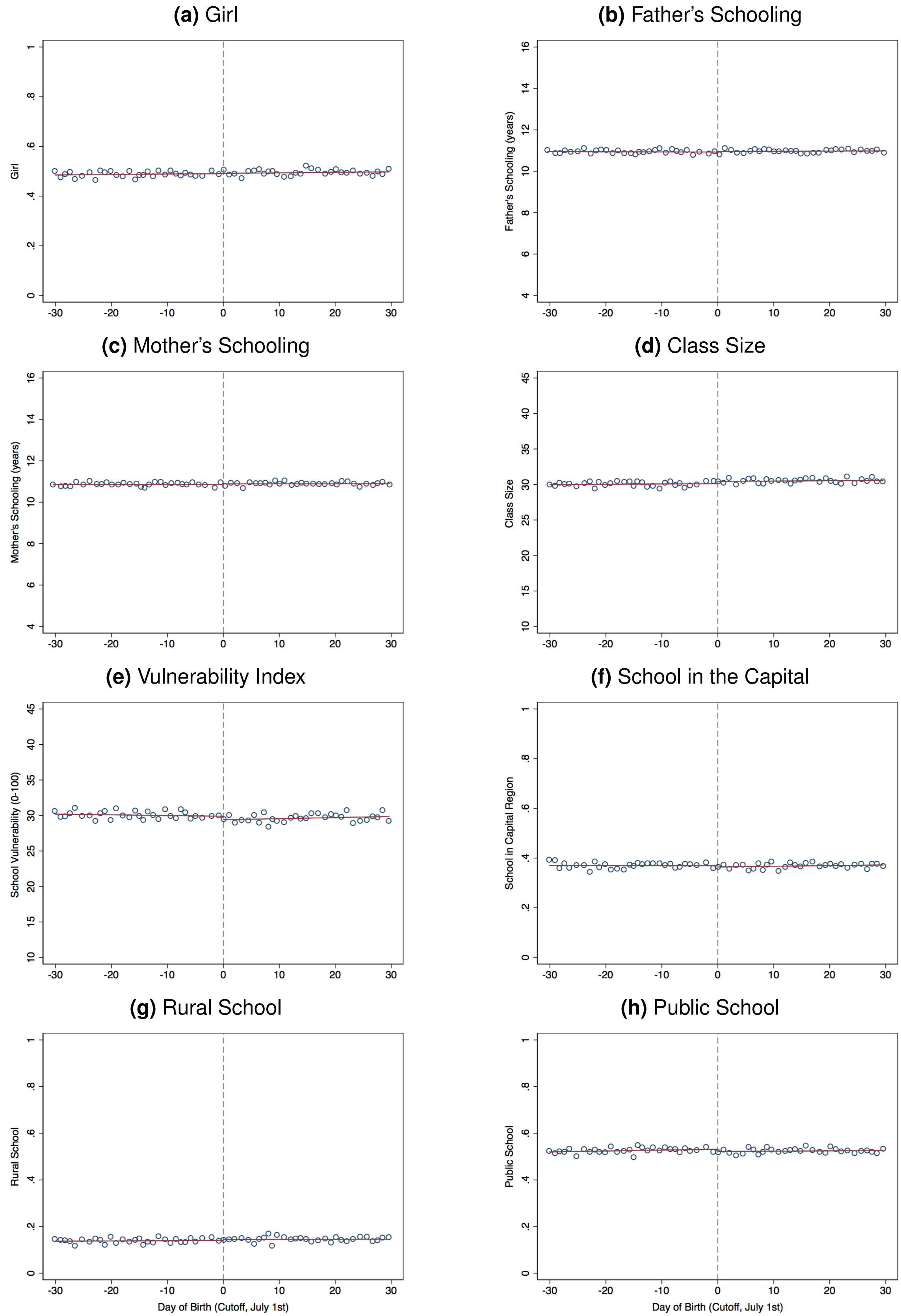
We complement these results with a graphical illustration for every covariate in Figure A.2, which provide further evidence of a smooth behavior at the July 1 cutoff.

Table A.2: Covariates Smoothness

	(1) Girl	(2) Father's Schooling	(3) Mother's Schooling	(4) Class Size	(5) IVE Index	(6) Capital Region	(7) Rural School	(8) Public School
$\widehat{\alpha_1}$	0.002	0.052	0.009	0.531	-0.003	-0.005	0.003	-0.011
	(0.006)	(0.041)	(0.041)	(0.102)	(0.002)	(0.005)	(0.003)	(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



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. The y-axis variables are described in Table 1¡v

Table A.3: Robustness

		Primary Grad	High-School Grad	PSU Exam	PSU Score	College Enroll	Selective Enroll	STEM Enroll	College Grad
(A)	$\widehat{\alpha_1}$	0.01	0.01	0.04	0.08	0.05	0.03	0.01	0.01
		(0.00)	(0.01)	(0.01)	(0.02)	(0.01)	(0.00)	(0.00)	(0.00)
(B)	$\widehat{\alpha_1}$	0.01	0.01	0.04	0.08	0.04	0.03	0.01	0.01
		(0.00)	(0.01)	(0.01)	(0.02)	(0.01)	(0.00)	(0.00)	(0.00)
(C)	$\widehat{\alpha_1}$	0.01	0.00	0.04	0.07	0.04	0.03	0.01	0.00
		(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
(D)	$\widehat{\alpha_1}$	0.01	0.00	0.03	0.07	0.04	0.03	0.01	0.00
		(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)

Notes: In row (A) we include cohort fixed effects, weekends,and holidays; in row (B) we include controls in (A) plus the Demographics (Female, Class Size, Rural School, School in Capital Region); in row (C) we include controls in (B) plus the Vulnerability Index; in row (D) we include controls in (C) plus the Public School

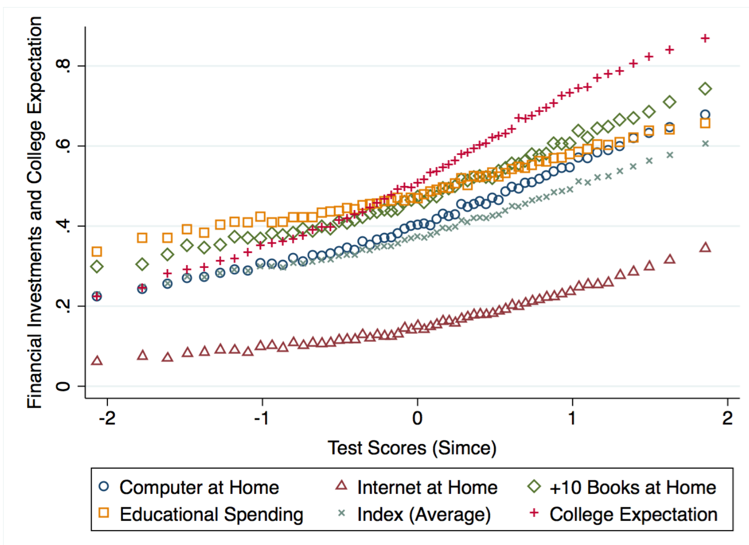
B Appendix: Investments

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 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.

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[15] illustrative of the positive correlation between parental financial investments, college expectations and children's test scores.

Figure B.1: Financial Investments, College Expectations and Test Scores

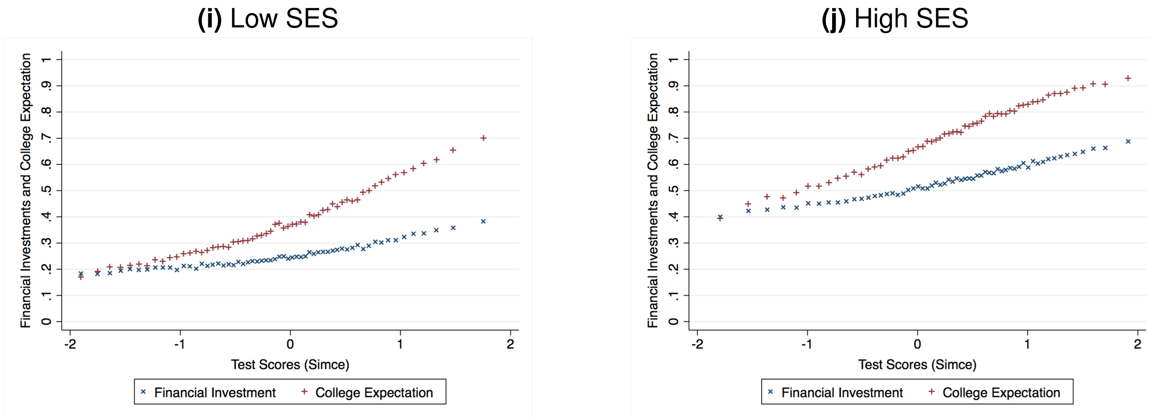


Note: The graphs in Figure B.1 plot the mean of the y-axis variables within equal sized bins of SIMCE test scores (in standard deviation units) in 4th grade, with a sample of approximately 500K observations. The y-axis variables are our measures of parental financial investments (having computer, internet 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) and whether parents think their child will attend college in the future.

Figure B.2 shows how that positive correlation persists by socioeconomic status, showing even a steeper gradient for college expectations in the lower SES group. 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.

[15]From the 987,264 observations in our results analysis we end up with about 500K observations, because data is available for 3 out of four cohorts, and then survey response is 75%.

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



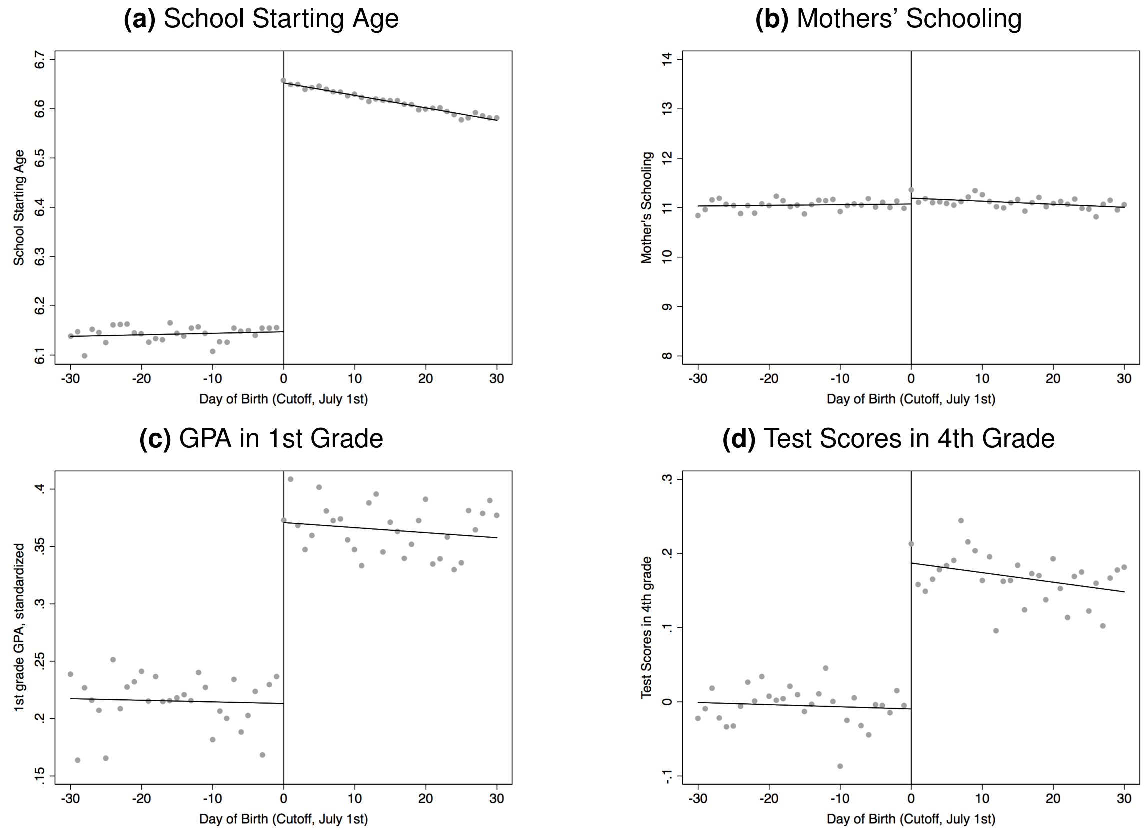
Note: The graphs in Figure B.2 plot the mean of the y-axis variables within equal sized bins of SIMCE test scores (in standard deviation units) in 4th grade, with approximately 250K observations each. The y-axis variables are the average of our measures of parental financial investments (having computer, 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 4). 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 (0.16σ) in first grade and higher test scores (0.18σ) in the fourth grade, very similar to what we found in our working samples.

Figure B.3: School Starting Age, Mothers' Schooling and In-school Results



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) Age at Entry	(2) Mother's Schooling	(3) GPA in 1st Grade	(4) Test Scores in 4th Grade
$\widehat{\alpha_1}$	0.501	-0.005	0.162	0.178
	(0.005)	(0.045)	(0.012)	(0.013)
June Mean	6.143	11.054	0.215	-0.005
Effect Size	0.082	0.000	0.753	35.055
Observations	51,818	51,492	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, mothers' schooling 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.

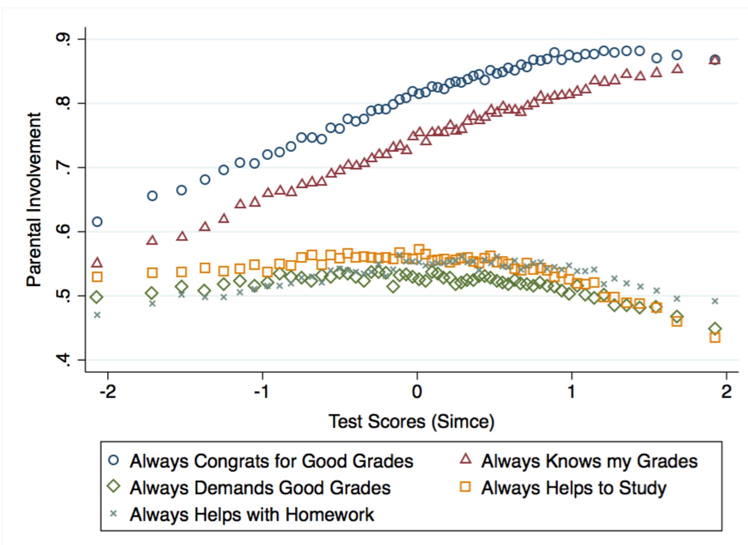
Parental Time Investments and Teacher Involvement

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-third, leaving us with a the dataset of 460K observations

Parental Time Investments

We first 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

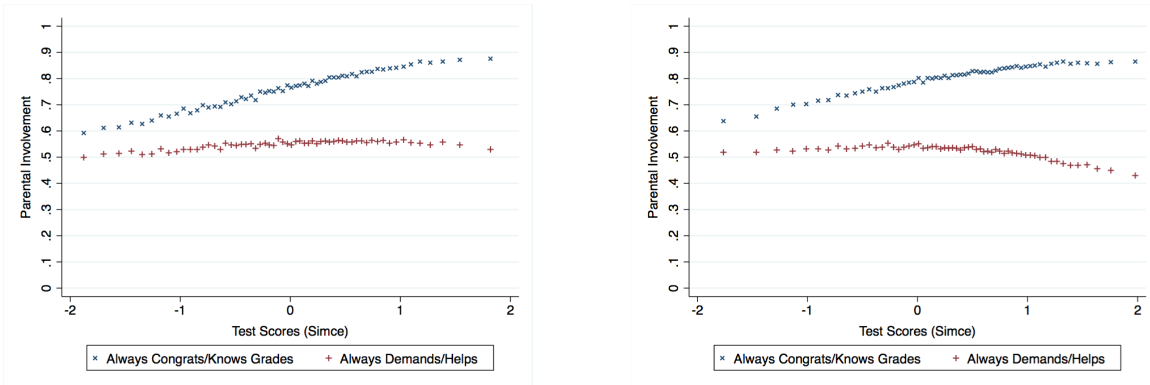


Note: The graphs in Figure B.4 plot the mean of the y-axis variables within equal sized bins of SIMCE test scores (in standard deviation units) in 4th grade, with a sample of approximately 460K observations. The y-axis variables are our measures of parental time investments.

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 display a similar correlation by socioeconomic status. 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

(a) Low SES **(b)** High 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.

Teacher Involvement

We use the surveys to examine measures of teacher involvement as reported by students. Figure B.6 shows the raw data relating different measures of teacher involvement to SIMCE test scores (in standard deviation units).

The non-parametric plot in Figure B.6 shows different degrees of correlation. There is a steeper gradient for whether students report that their teachers always explains again if somebody asks, and if the teachers always explain until everybody is on board. A less steep gradient appears for whether teachers solve material in class (exercises from textbooks, homework and exams). Finally there is a flat slope (and even negative for higher levels of test scores) for whether the teacher checks homework for everyone in class.

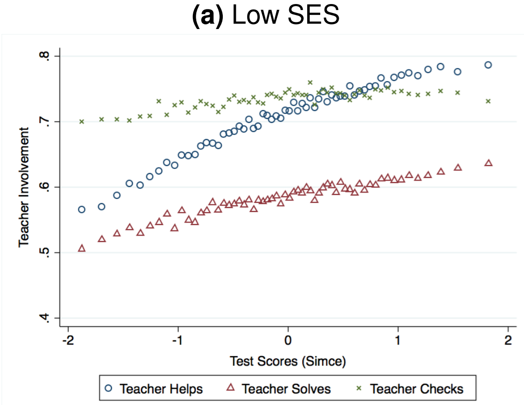
Figure B.6: Time Investments and Test Scores

Note: The graphs in Figure B.6 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.

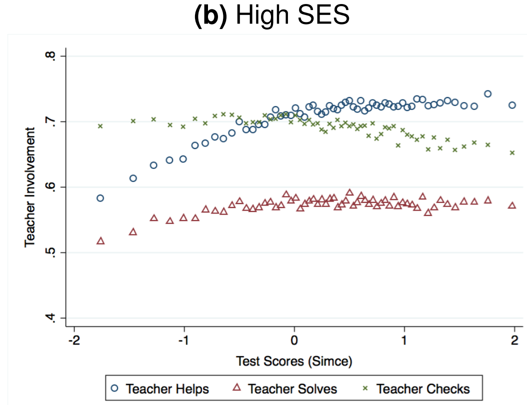
In Figure B.7 we show the same correlations by socioeconomic status, after grouping teacher involvement in whether teacher helps (the two measures with steeper slope), solves (the three measures with less steep slope), or checks. The graphs in Figure B.7 display a higher correlation between the measures of teacher involvement for lower SES students. Overall, graphs in Figure B.6 and Figure B.7 show that in the raw data whether teacher is deemed helpful by students is correlated positively with test scores, and that the patterns seems more pronounced for students from the lower socioeconomic group.

Figure B.7: Teacher Involvement and Test Scores by SES

(a) Low SES



(b) High SES



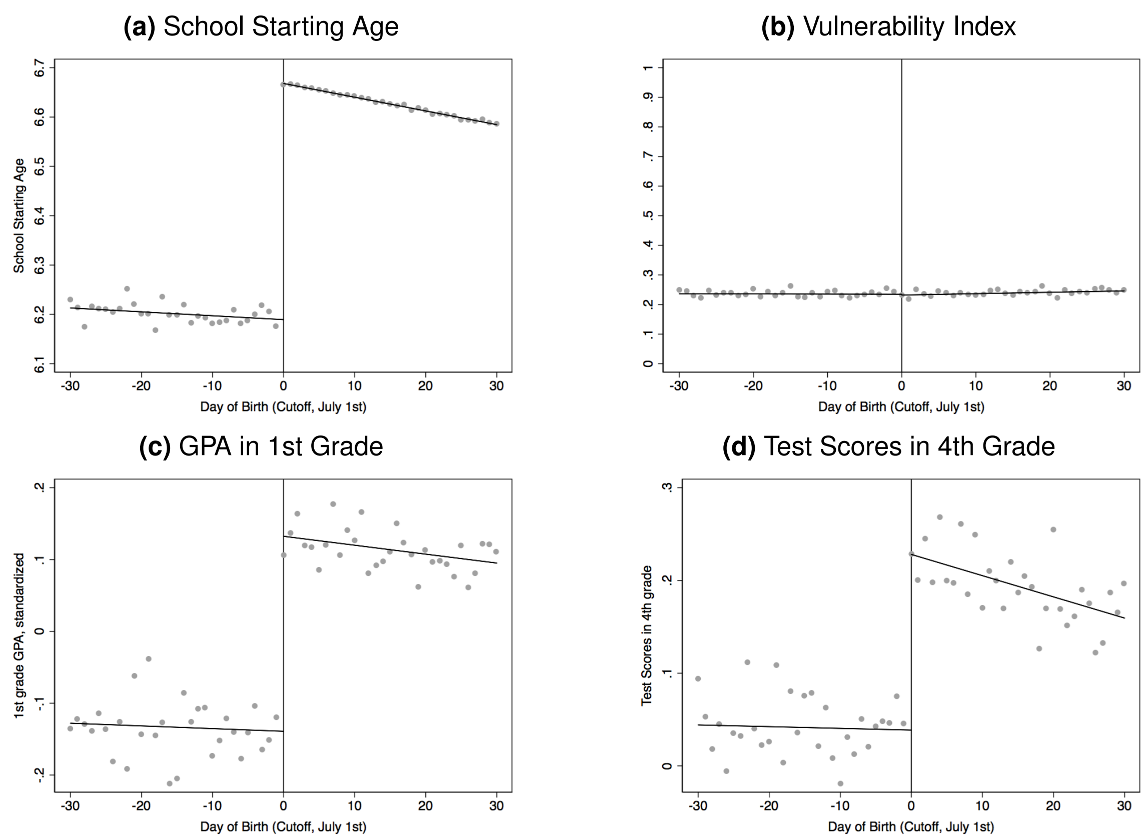
Note: The graphs in Figure B.7 plot the mean of the y-axis variables within equal sized bins of SIMCE test scores (in standard deviation units) in 4th grade, with approximately 250K observations each. The y-axis variables are the average of our measures of parental time investments.

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 4). Similarly to our sample for financial investments we are left with approximately 50K observations to test effects on time and teacher investments.

Figure B.8 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 vulnerability index is smooth. In terms of outcomes, July-born students have higher GPAs (0.26σ) in first grade and higher test scores (0.18σ) in the fourth grade, very similar to what we found in our working samples.

Figure B.8: School Starting Age, Mothers' Schooling and In-school Results



Note: The graphs in Figure B.8 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) Age at Entry	(2) Vulnerability Index	(3) GPA in 1st Grade	(4) Test Scores in 4th Grade	(5) Parent Congrats Knows	(6) Parent Demands Helps	(7) Teacher Helps	(8) Teacher Solves	(9) Teacher Checks
$\widehat{\alpha_1}$	0.480	-0.000	0.263	0.183	0.000	0.007	0.014	0.012	-0.021
	(0.006)	(0.000)	(0.014)	(0.014)	(0.006)	(0.008)	(0.008)	(0.005)	(0.008)
June Mean	6.202	0.236	-0.133	0.042	0.772	0.548	0.703	0.585	0.718
Effect Size	0.077	-0.000	-1.975	4.415	0.000	0.012	0.020	0.021	-0.029
Observations	47,646	47,646	47,646	46,077	47,646	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.

C Appendix: Additional Results

Appendix for Average Effects

Table B.3: In-school Outcomes: 1st Grade to High-School Completion

	(1) SSA	(2) GPA1	(3) Pass1	(4) TakesSimce4	(5) Scores4	(6) OnTime8	(7) Late8	(8) OnTime12	(9) Late12
$\widehat{\alpha_1}$	0.482	0.263	0.024	0.017	0.209	0.067	0.011	0.061	0.004
	(0.003)	(0.012)	(0.003)	(0.004)	(0.011)	(0.004)	(0.003)	(0.005)	(0.005)
June Mean	6.175	-0.045	0.945	0.857	-0.099	0.719	0.915	0.510	0.693
Observations	117,709	117,709	117,709	117,709	101,915	117,709	117,709	117,709	117,709

Table B.4: Long Run Outcomes: Transition to Higher Ed., Enrollment and Graduation

	(1) SSA	(2) OnTime12	(3) Late12	(4) PSU Exam	(5) PSU Score	(6) College Enrollment	(7) Tech School Enrollment	(8) College Grad	(9) Tech School Grad
$\widehat{\alpha_1}$	0.441	0.012	0.006	0.018	0.043	0.010	-0.003	0.003	-0.003
	(0.005)	(0.006)	(0.006)	(0.006)	(0.012)	(0.005)	(0.005)	(0.004)	(0.003)
June Mean	6.211	0.651	0.690	0.631	-0.229	0.310	0.250	0.139	0.122
Observations	111,664	111,664	111,664	111,664	71,860	111,664	111,664	111,664	111,664

Notes: Tables B.3 and B.4 show the coefficient $\widehat{\alpha_1}$ estimated from the equation (1) for our cohorts of 1st and 8th graders respectively. In both Tables column (1) shows the coefficient reflecting the difference on school starting age (SSA) at the July 1 cutoff. The next columns (2) to (9) show results for a host of in-school outcomes and longer run outcomes respectively. The in-school outcomes are first grade GPA (GPA1, standardized within school and grade) and pass rate (Pass1), whether students took the fourth grade standardized exam (TakesSimce4) and the Language-Math score if they did (Scores4), two measures of primary school completion at grade 8th (on time or within three years of the expected graduation date), and the same for high-school completion. The long run outcomes are: high school completion either on time (OnTime12) or within three years (Late12), likelihood of taking the college entrance exam (PSU Exam), and the PSU score if they did; enrollment in college or technical schools, all of them within 5 years of high school graduation; and finally, college and technical school graduation within 8 years of high school graduation (age 27-28). 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.

Appendix for In-School, Heterogeneous Effects

Table B.5: In-school Outcomes, by Socioeconomic Status

Panel 1: Low-SES

	(1) SSA	(2) GPA1	(3) Pass1	(4) TakesSimce4	(5) Scores4	(6) OnTime8	(7) Late8	(8) OnTime12	(9) Late12
$\widehat{\alpha_1}$	0.531	0.273	0.029	0.018	0.233	0.071	0.014	0.054	-0.001
	(0.004)	(0.018)	(0.004)	(0.006)	(0.017)	(0.007)	(0.005)	(0.009)	(0.009)
June Mean	6.129	-0.057	0.923	0.825	-0.338	0.664	0.887	0.442	0.627
$\widehat{\alpha_1}$/(June Mean)									
Observations	0.087	-4.763	0.032	0.022	-0.690	0.107	0.016	0.123	-0.001
N	58,846	58,846	58,846	58,846	49,171	58,846	58,846	58,846	58,846

Panel 2: High-SES

	(1) SSA	(2) GPA1	(3) Pass1	(4) TakesSimce4	(5) Scores4	(6) OnTime8	(7) Late8	(8) OnTime12	(9) Late12
$\widehat{\alpha_1}$	0.434	0.255	0.019	0.017	0.185	0.063	0.008	0.067	0.009
	(0.005)	(0.016)	(0.003)	(0.006)	(0.016)	(0.006)	(0.004)	(0.007)	(0.007)
June Mean	6.222	-0.031	0.968	0.889	0.131	0.775	0.944	0.579	0.761
$\widehat{\alpha_1}$/(June Mean)									
Observations	0.070	-8.129	0.020	0.019	1.416	0.081	0.009	0.116	0.012
N	58,863	58,863	58,863	58,863	52,744	58,863	58,863	58,863	58,863

Panel 3: Low Parental Schooling

	(1) SSA	(2) GPA1	(3) Pass1	(4) TakesSimce4	(5) Scores4	(6) OnTime8	(7) Late8	(8) OnTime12	(9) Late12
$\widehat{\alpha_1}$	0.509	0.283	0.028	0.000	0.210	0.068	0.006	0.057	0.000
	(0.006)	(0.015)	(0.003)	(.)	(0.018)	(0.008)	(0.006)	(0.010)	(0.006)
June Mean	6.153	-0.019	0.951	1.000	-0.338	0.719	0.935	0.483	0.672
$\widehat{\alpha_1}$/(June Mean)	0.083	-14.880	0.030	0.000	-0.620	0.094	0.006	0.117	0.000
Observations	48,430	48,430	48,430	48,430	48,430	48,430	48,430	48,430	48,430

Panel 4: High Parental Schooling

	(1) SSA	(2) GPA1	(3) Pass1	(4) TakesSimce4	(5) Scores4	(6) OnTime8	(7) Late8	(8) OnTime12	(9) Late12
$\widehat{\alpha_1}$	0.458	0.210	0.013	0.000	0.181	0.036	0.002	0.041	-0.013
	(0.007)	(0.016)	(0.002)	(.)	(0.016)	(0.005)	(0.003)	(0.009)	(0.008)
June Mean	6.189	0.149	0.981	1.000	0.222	0.838	0.973	0.645	0.823
$\widehat{\alpha_1}$/(June Mean)	0.074	1.413	0.014	0.000	0.814	0.043	0.002	0.063	-0.016
Observations	41,629	41,629	41,629	41,629	41,629	41,629	41,629	41,629	41,629

Notes: Table B.5 show the coefficient $\widehat{\alpha_1}$ estimated from the equation (1) on in-school outcomes by different definitions of socioeconomic status. Panels 1 and 2 are the result of splitting the sample of 117,709 according to whether the school was above or below the median of the vulnerability index. Students in schools with low SES (high vulnerability) have a mean of 47% of vulnerability, while those in high SES (low vulnerability) have a mean of 12%. Panels 3 and 4 split the sample by whether parents have on average less then high school diploma, or more, respectively. The average parental schooling for the first group is 8.5 years, while for the second group is 13.6 years. In all panels column (1) shows the coefficient reflecting the difference on school starting age (SSA) at the July 1 cutoff. The next columns, (2) to (9) show results for a the same host of in-school outcomes described in Tables B.3. 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.

Appendix for Long Run, Heterogeneous Effects

Table B.6: Longer Run Outcomes, by Socioeconomic Status

Panel 1: Low-SES

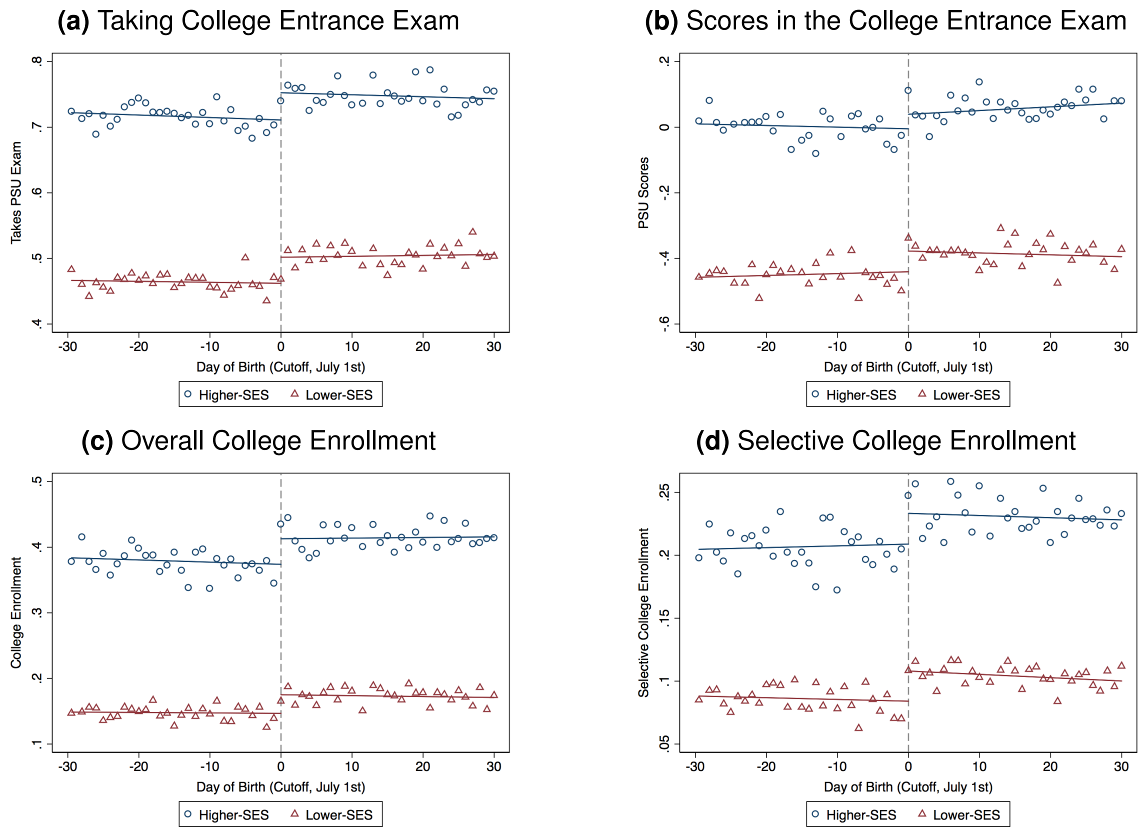
	(1) SSA	(2) OnTime12	(3) Late12	(4) PSU Exam	(5) PSU Score	(6) College Enrollment	(7) Tech School Enrollment	(8) College Grad	(9) Tech School Grad
$\widehat{\alpha_1}$	0.459	0.018	0.009	0.024	0.066	0.010	-0.001	0.009	-0.002
	(0.008)	(0.007)	(0.007)	(0.008)	(0.015)	(0.005)	(0.006)	(0.003)	(0.005)
June Mean	6.197	0.591	0.631	0.490	-0.556	0.160	0.240	0.068	0.118
Observations	55,841	55,841	55,841	55,841	27,826	55,841	55,841	55,841	55,841

Panel 2: High-SES

	(1) SSA	(2) OnTime12	(3) Late12	(4) PSU Exam	(5) PSU Score	(6) College Enrollment	(7) Tech School Enrollment	(8) College Grad	(9) Tech School Grad
$\widehat{\alpha_1}$	0.422	0.006	0.002	0.011	0.026	0.008	-0.003	-0.003	-0.004
	(0.006)	(0.008)	(0.008)	(0.007)	(0.015)	(0.008)	(0.008)	(0.006)	(0.005)
June Mean	6.225	0.713	0.751	0.778	-0.015	0.466	0.260	0.212	0.127
Observations	55,823	55,823	55,823	55,823	44,034	55,823	55,823	55,823	55,823

Notes: Results in Table B.6 show the coefficient $\widehat{\alpha_1}$ estimated from the equation (1) on long run outcomes by the school vulnerability index. Panels 1 and 2 are the result of splitting the sample of 111,664 according to whether the school was above or below the median of the vulnerability index. Students in schools with low SES (high vulnerability) have a mean of 45% of vulnerability, while those in high SES (low vulnerability) have a mean of 11%. There was no survey measuring parental schooling for these students, but results in Table B.5 suggest that the school-level SES index behaves similarly to the parental schooling analysis. In both panels column (1) shows the coefficient reflecting the difference on school starting age (SSA) at the July 1 cutoff. The next columns, (2) to (9) show results for a the same host of longer run outcomes described in Tables B.4. 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.

Figure B.9: Longer Run Results by Socioeconomic Status



Note: The graphs in Figure B.9 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.