

Assessing the Family-Wide Impact of Delayed Primary School Enrollment: The Effects of an Additional Year Without Childcare

Carmen Quezada[†]
University of Toronto

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Abstract

This paper examines the impact of delayed school entry on families, particularly focusing on how an additional year of childcare responsibilities affects both mothers and their children's siblings. Using a natural experiment created by school entry cutoffs in Chile, I find that mothers reduce their labor market participation by approximately 2% when caregiving is extended. This reduction persists until the child turns 20, and there is a corresponding decrease in monthly income, although this is offset by small increases in government transfers. The negative effects also extend to younger siblings, who experience lower GPAs, standardized test scores, and post-secondary enrollment rates. In contrast, older siblings and fathers are not significantly affected. These results suggest that time constraints on mothers, driven by limited access to formal preschool care, may contribute to the observed negative outcomes.

Keywords: Education, Family, Spillovers

JEL Codes: I21, J13, J22, I28, J24

[†]carmen.quezada@mail.utoronto.ca

1 Introduction

Women represent 50% of the world’s population, yet their labor force participation and income remain far from equitable. This imbalance is evident not only in the general workforce but also among parents. Research has shown that these disparities often emerge after the birth of a child and continue long afterward (Miller (2011), Angelov et al. (2016), Adda et al. (2017), Lundborg et al. (2017), Kleven et al. (2019), Gallen et al. (2023)). However, the reasons for this persistence are not well understood. Schools, which have long provided essential skills to children and an implicit childcare subsidy to parents, may be one relevant factor. This paper sheds light on this relationship by presenting new evidence on how parental labor supply is influenced by the timing and accessibility of schooling. To that end, I leverage a natural experiment to carefully examine how caring for a child for an additional period, close to school age, affects the family — not only estimating the effect on parents but also on their children’s siblings.

To explore this question, I exploit the discontinuity in school starting age generated by school entry rules, which dictate the age at which students begin their formal education, to estimate the exogenous effect of needing to caregiver a child an additional year prior to formal schooling. By comparing families where the focal child — the child subject to the cutoff rules, was born in the days before and after the cutoff, I can observe the effects of starting school a year later. A child born after the cutoff has to wait an additional year to begin first grade, increasing the childcare burden on the family for that extra year.

This added childcare responsibility does not only affect parents’ labor supply; it reshapes the way parental time, a finite yet crucial resource, is allocated within the family. When a child’s school entry is delayed, parents, particularly mothers, must often redistribute their time, balancing caregiving duties with other children’s developmental needs and the demands of work. Research indicates that parental time is one of the most significant investments in child development, accounting for over two-thirds of family spending on children under 12 (Caucutt et al. (2020)). More time spent with children has been shown to enhance their cognitive and non-cognitive skills (Del Boca et al. (2014), Attanasio et al. (2020), Milovanska-Farrington (2021)), while less time has been associated with declines in these areas (J. Price (2008), Bibler (2020)). Consequently, a prolonged childcare period for one child can indirectly impact siblings, as parents divide their time to meet competing demands within the family.

To study these dynamics, I use an exceptionally rich administrative dataset from Chile that links individual family members across multiple dimensions. My primary sample consists of all children attending public or voucher schools, representing 93% of total enrollment, who started first grade between 2002 and 2007. For these students, I examine educational and early fertility outcomes of older and younger siblings, as well as parental labor market participation and government assistance. The longitudinal structure of the data enables a thorough examination of both short-term and medium-to-long-term effects on those various family outcomes. In particular, the data provides valuable insights into parental labor outcomes, allowing me to observe participation in both formal and informal markets. This distinction is essential, as informal employment is a significant feature

of labor markets in developing countries, especially for women, who experience higher rates of informality compared to men (SEDLAC (2014), Otobe (2017)).

I find that caregiving responsibilities at an older age impact mothers, whereas fathers remain unaffected. Specifically, a later school starting age has a negative effect on mothers' participation in the labor market. Mothers reduce their labor supply by 3.2% in the formal market and by 1.8% when considering both formal and informal markets combined. This suggests that while the informal market often serves as a buffer—offering lower access barriers, more flexible work arrangements, and typically shorter working hours (Gasparini and Tornarolli (2009), Perry (2007), Berniell et al. (2021))—additional caregiving responsibilities still reduce mothers' availability for work across all job sectors. Importantly, this effect persists until the child turns 20, indicating a long-term impact on mothers' employment patterns. In addition to reduced labor supply, I also observe a similar decline in mothers' monthly income when analyzing formal and informal markets together. However, a slight increase in government assistance transfers may help to partially offset the financial strain caused by this income reduction, providing some level of relief for affected families.

To ensure that these effects are indeed a consequence of school starting age rather than other external factors, I created a secondary sample of students starting first grade from 2008 to 2020. This sample allows me to access formal labor market outcomes for parents at the time their children are just beginning school, filling a gap in the primary sample, which lacks this early-stage labor market information. The results from this secondary sample align with the primary findings: fathers show no significant changes in either participation or income, while mothers experience a notable reduction in labor participation. Specifically, I observe nearly a 2% reduction in mothers' labor participation and a 2.8% decrease in their income, reinforcing the initial results and indicating a consistent, lasting impact of delayed school entry on maternal labor outcomes.

These caregiving demands extend beyond mothers, influencing younger siblings of children who delay school entry. The negative impacts on younger siblings persist throughout their schooling, as measured by average GPA, standardized test scores, and post-secondary enrollment. On average, younger siblings score 3% of a standard deviation lower in GPA and test scores, and they are 7% more likely to repeat a grade. These adverse effects continue into post-secondary education, with younger siblings being 10% less likely to enroll in post-secondary institutions. In contrast, the effects on older siblings are negative but not statistically significant. Importantly, these results remain robust when accounting for siblings' and focal characteristics, as well as for different measures of post-secondary access, whether immediately following high school graduation or at any later point.

I suggest that the mechanism behind the negative effects on mothers and younger siblings is either the lack of formal preschool care or social norms that encourage mothers to stay home for childcare. When a child remains at home for an extra year instead of attending school, two outcomes arise: first, the mother cannot enter the workforce, and second, her remaining children must compete for her time, leaving less time available for younger siblings. To test this hypothesis, I split the sample between mothers who are heads of their households and those who are not. Since

a mother who is the head of the household is typically the primary breadwinner, she is less likely to stay home for childcare. Therefore, I expect to see a smaller impact on outcomes for her children. Indeed, I find that when the mother is the head of the household, the negative effects on younger siblings decrease. In contrast, the effects are more pronounced when the mother is not the head of the household. This suggests that when mothers are not household heads and are more likely affected by this childcare responsibility, the negative effects on younger siblings intensify, pointing to a reduction in parental time as a likely mechanism behind these results.

My contributions to the literature focus on two main areas. The first area addresses the gendered effects of schooling on parental labor market outcomes, specifically how school schedules and availability impact mothers' and fathers' work participation differently. While my findings reveal almost no measurable impact on fathers' employment, they show significant effects for mothers, underscoring the unique relationship between school timing and maternal labor supply, as discussed in studies by Duchini and Van Effenterre (2024), Hansen et al. (2024), and B. M. Price and Wasserman (2024), and the relationship between school availability and maternal labor supply, as seen in Martinez and Peticara (2017), Padilla-Romo and Cabrera-Hernández (2019), and Berthelon et al. (2024). I contribute to this literature in two key ways. First, I analyze the effects of a widely adopted policy, one that holds broad relevance across different contexts and societies. Second, I incorporate an analysis of informal labor markets, which is especially important in developing countries—not only because a large portion of individuals work in this sector, but also because informal jobs offer amenities that are particularly valuable to mothers, such as flexibility and shorter hours. This setup reveals the tradeoff between these amenities and the precarious nature of informal work, characterized by lower wage growth and a lack of social protection, which may help to explain the persistent wage gap between parents after the birth of a child.

The second area of my contribution pertains to the family-level spillover effects of educational policies. My work expands and complements existing research on family spillovers, an area explored by Landersø et al. (2020) for the Danish population and by Karbownik and Özek (2021) for a large district in the state of Florida. These studies have examined how school starting age and entry rules affect family dynamics in various ways. Building on this foundation, my research provides external validity by demonstrating the impact of these policies in a context that mirrors the conditions faced by most countries, particularly in terms of limited access to formal childcare. This broader perspective helps to generalize findings on how school entry policies indirectly shape family outcomes, from employment patterns to the distribution of parental time, highlighting the essential role of schools—from early childhood education onwards—as a crucial support system that enables mothers to participate in the workforce.

This paper proceeds as follows: Section 2 the empirical strategy, and the internal validity of the estimates. Section 3 describes the data, 4 the main results. Section 5 tests some of the mechanisms that could explain the findings. Section 6 summarizes the findings and concludes.

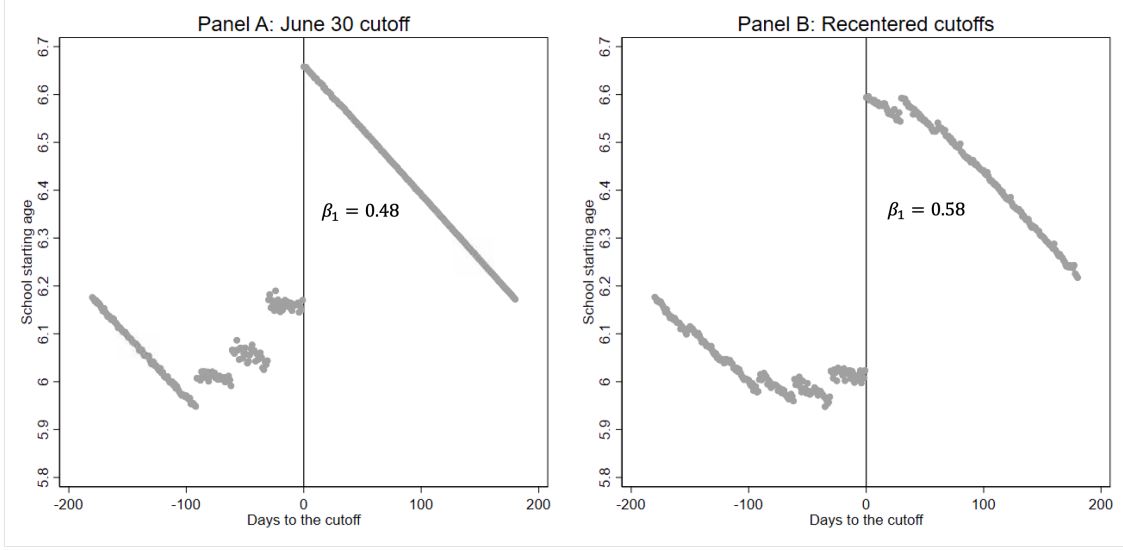
2 Identification and Empirical Strategy

The identification of the effects of enrollment age relies on comparing the outcomes of students (and their families) who were born on or just after the cutoff dates with those of students born just before the cutoffs. The causal interpretation of these comparisons depends on the assumption that birth dates near the cutoffs are random, similar to a localized randomized experiment. This identification strategy relies on the assumption that the specific timing of births around the cutoffs does not lead to significant differences in unobserved factors that could influence student outcomes.

Chilean law requires prospective students to be six years old by March 31st of the academic year to begin school, though it allows schools to admit students until June 30th.¹ This flexibility led to observe sharp increases in enrollment in April, May, June, and July (see McEwan and Shapiro (2008) for initial documentation). Since the most prominent discontinuity occurred in July, researchers focusing on the Chilean context limited their analysis to the July cutoff. I opted to identify each school's cutoff to make the school starting age analysis more precise and reduce the misclassification of students and cutoffs. While I provide a detailed explanation in the Appendix A.1, the process involves calculating the proportion of students born in each month-year pair for every school and academic year. The empirical cutoff corresponds to the month with the highest proportion of students born within 12 consecutive months. This approach is conservative, assigning July 1st as the cutoff to each school year pair when any month-year pair within the 12 months has no students born. As a result, the empirical cutoff still reveals increases in multiples of 30 days from the cutoff, although these increases are much smaller than those with the July 1st cutoff. Moreover, the increase in school starting age at the empirical cutoff is 20% larger than at the July 1st cutoff, suggesting a better classification of students around the cutoff.

¹This law changed in 2017, making March 31st a firm deadline. However, in practice, full enforcement of the law began in 2019.

Figure 1: First grade enrollment age using July 1st cutoff vs using empirical cutoffs



Note: Panel A displays the average school starting age for children born each day where the cutoff date is July 1st, while in Panel B, empirical cutoffs are calculated by each school and year.

The empirical approach capitalizes on Chile’s birth date cutoff regulations, stipulating that aspiring students not yet six years old by the cutoff of their school should start their studies in the following one. To explore this, I employ a regression discontinuity (RD) design, leveraging precise birth dates for children born before and after the cutoff. This allows me to compare outcomes among children born just days apart but entering school with markedly different maturity levels.

The assumptions underpinning the identification process align with standard practices for RD designs. I declare that no other alterations taking place at the cutoff point could introduce confounding factors into our analysis. The initial equation I use for estimation is as follows:

$$Y_{ij} = \beta_0 + \beta_1 \text{BornAfter}_j + \beta_2 \text{DaystoCutoff}_j + \beta_3 \text{DaystoCutoff}_j \times \text{BornAfter}_j + \mathbf{X}'_{ij} \beta_4 + \varepsilon_{ij} \quad (1)$$

where Y is the outcome of interest for individual i , which could be a parent, a sibling, or the same focal student j ; The variable BornAfter_i takes on a value of one if the focal student j was born on or after the cutoff and zero otherwise; DaystoCutoff is the running variable, which is defined as the day of birth relative to the cutoff; \mathbf{X} is a vector of covariates depending on the individual analyzed, and ε is the error term. The variables DaystoCutoff and BornAfter are interacted to allow for differential slopes on either side of the cutoff. I apply a triangular kernel and follow the robust data-driven method proposed by Calonico et al. (2014) to select an optimal bandwidth. Robust standard errors clustered by distance to the cutoff are reported. β_1 represents the key coefficient and reflects the intent-to-treat effects of the school starting age on the different outcomes. I limit my analysis to these reduced-form effects and avoid “scaling up” the estimates by using the starting age threshold as an instrument, as doing so would require the additional assumptions of the LATE framework to be valid.

2.1 Validity of the research design

In the proposed empirical framework, β_1 is designed to estimate the causal effects of the focal child's, either older or younger, ineligibility to start school earlier. This estimation is valid as long as predetermined factors remain continuous at the school-entry cutoff. In other words, no other changes should occur at the threshold that might confound the analysis. Tables 1, 2 and 3 present the estimated discontinuities in variables that are expected to remain smooth around the cutoff for the focal student's, parents', and sibling's school-entry dates.

Table 1 presents the estimated discontinuities in four variables that should be continuous around the focal school-entry cutoff date. Of the 4 estimates presented, only one is statistically significant at conventional levels, and none of the estimates exceed 2 percent of the dependent variable mean. The only significant coefficient is the probability of attending a Public School, which is lower for students born after the cutoff. However, the implied imbalance is small and equal to 1.4%. Specifically, 51% of students born before the cutoff attend public schools, compared to 49.6% of those born after the cutoff.

Table 1: Discontinuities in the focal student background characteristics

	(1) Female	(2) Public	(3) Rural School	(4) Class size
Born after school-entry cutoff	-0.0058 (0.004)	-0.007* (0.004)	-0.002 (0.003)	0.118 (0.106)
Mean before cutoff	0.492	0.510	0.133	33.8
Implied imbalance	-0.011	-0.014	-0.017	0.003
Bandwidth	65.99	46.67	60.7	38.44
Observations	406,190	286,124	374,524	237,180

Notes: This table shows the result of local linear regressions with the optimal bandwidth of the robust data-driven method proposed by Calonico et al. (2014) and triangular kernel for reduced-form estimates based on Equation 1. Outcome variables are an indicator if the focal student is female, an indicator if the focal student attends a public school, an indicator if the focal student attends a rural school, and the class size. There are no discontinuities in background characteristics for the focal student, except in the probability of attending a public school. However, the implied imbalance is small. Robust standard errors are clustered at the running variable at the daily level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Next, Table 2 presents the estimated discontinuities in five variables that should remain continuous around the focal school-entry cutoff date. Panel A shows the spillover balance from the focal child to the mother (i.e., the discontinuity in the mother's characteristics at the child's school-entry cutoff date). In contrast, Panel B shows the spillover balance from the focal child to the father. Some estimates are statistically significant, particularly Child Order (the order of the focal child among their siblings), which is significant for both mothers and fathers. However, the imbalances are small. For mothers, the imbalance is 1.7%, and for fathers, it is 0.1%. This suggests that for mothers, the mean order of a focal child born before the cutoff is 2.07, compared to 2.15 for

a child born after the cutoff. Another variable with a discontinuity is the mother’s age at birth, where mothers of children born before the cutoff were, on average, 27.16 years old, while those born after the cutoff were 27.32 years old—around two months older. More importantly, there are no discontinuities in the number of children, marital status, or, for fathers only, age at birth. When analyzing mechanisms, I will use the variable “head of household,” which is equal to one if the mother is the primary breadwinner; there are no discontinuities in this variable either. Crucially, none of the estimates exceed 2% of the dependent variable’s mean.

Table 2: Discontinuities in parents background characteristics

	(1) Number of children	(2) Married	(3) Age at birth	(4) Child order	(5) Head of the household
Panel A: Mothers					
Focal child born after school-entry cutoff	0.0056 (0.010)	-0.0014 (0.0042)	0.167** (0.065)	0.035*** (0.01)	-0.001 (0.004)
Mean before the cutoff	2.80	0.55	27.16	2.07	0.605
Implied imbalance	0.002	-0.0024	0.006	0.0171	-0.003
Bandwidth	52.1	59.25	69.76	83.9	75.6
Observations	274,375	311,723	414,432	501,342	437,183
Panel B: Fathers					
Focal child born after school-entry cutoff	-0.0001 (0.0117)	-0.004 (0.0047)	-0.02 (0.08)	0.02* (0.011)	- -
Mean before the cutoff	2.95	0.611	29.45	2.07	-
Implied imbalance	-0.00004	-0.007	-0.0007	0.001	-
Bandwidth	62.2	60.54	45.42	67.6	-
Observations	293,427	284,539	246,614	367,847	-

Notes: Local linear regression with the optimal bandwidth of the robust data-driven method proposed by Calonico et al. (2014) and triangular kernel for reduced-form estimates based on Equation 1. The sample in Panel A is based on all registered mothers at birth, and the sample in Panel B is based on all the fathers registered at the birth of the focal student. Multiple observations per individual may exist if there is more than one focal student in the sample. In this study, 7.31% of families may have multiple focal students, consistent with findings in the literature (Landersø et al. (2020) also reports 7% multiple focal students). Outcome variables are the number of children, if the parent is married, the age of the parent at birth, and focal child order. Additionally, for mothers, there is an additional variable that will be used for the mechanism section that shows the probability of being head of the household. The implied imbalances in age at birth are equivalent to two months. The differences in child order are statistically significant but economically small. Robust standard errors are clustered at the running variable at the daily level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3 presents the estimated discontinuities in four variables—gender, birth month, birth year, and age difference—that should remain continuous around the focal school-entry cutoff date. Panel A shows the spillover balance from the focal child to the younger sibling (i.e., the discontinuity in the younger sibling’s characteristics at the focal sibling’s school-entry cutoff date), while Panel B shows the spillover balance from the focal child to the older sibling. All estimates, except for the age difference (in years), are not statistically significant. The implied imbalance in age difference for younger siblings is -1.5%, while for older siblings, it is 1.95%. This difference suggests that the younger sibling of a focal child born before the cutoff has an age difference of 2.88 years, compared to 2.84 years for siblings born after the cutoff—about 15 days. For older siblings, the age difference with the focal child is 2.89 years if the focal child is born before the cutoff, while this difference increases to 2.95 years if the focal child is born after the cutoff—roughly a 20-day difference. Importantly, none of the estimates exceed 2% of the dependent variable’s mean.

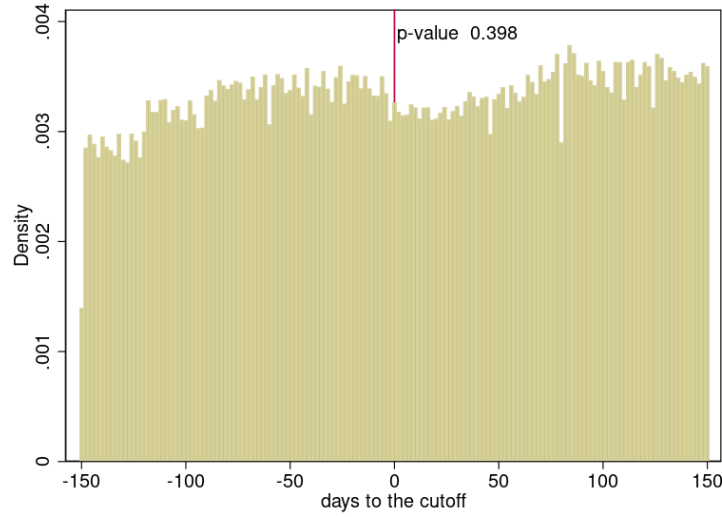
Table 3: Discontinuities siblings' background characteristics

	(1) Female	(2) Birth Month	(3) Birth Year	(4) Age Difference
Panel A: Effects in younger siblings				
Focal child born after school-entry cutoff	0.009 (0.007)	-0.04 (0.058)	-0.009 (0.048)	-0.042** (0.021)
Mean before the cutoff	0.487	6.48	2001	2.88
Implied imbalance	0.015	0.006	0.000	-0.015
Bandwidth	58.21	64.22	72.99	53.22
Observations	95,661	105,361	118,705	87,217
Panel B: Effects in older siblings				
Focal child born after school-entry cutoff	-0.001 (0.007)	0.006 (0.043)	-0.036 (0.050)	0.056*** (0.017)
Mean before the cutoff	0.487	6.6	1995	2.89
Implied imbalance	-0.003	0.0008	0.000	0.0195
Bandwidth	75.06	3073.7	65.81	47
Observations	142,010	138,164	122,642	88,723

Notes: Local linear regression with the optimal bandwidth of the robust data-driven method proposed by Calonico et al. (2014) and triangular kernel for reduced-form estimates based on Equation 1. The output is the average test score of the SIMCE tests that students write in Grades 4, 6, 8, and 10. The sample in Panel A is based in all children for whom I observe two adjacent siblings with at least one GPA observation in Grades 1-8 for the younger child and where the older sibling is a focal child. Sample in Panel B is based on all children for whom I observe two adjacent siblings with at least one GPA observation in Grades 1-8 for the older child and where the younger sibling is a focal child. Single observation per individual in all columns. Outcome variables are if a sibling is female, birth month, birth year, and age difference. The implied imbalance in age difference in Panel A is equivalent to 15 days and in Panel B to 20 days. Robust standard errors are clustered at running variable at daily level. *** p<0.01, ** p<0.05, * p<0.1.

Finally, I examine potential manipulation by applying a nonparametric test for discontinuity in the distribution of students born on either side of the eligibility cutoff, as outlined by Cattaneo et al. (2018). As shown in Figure 2, I cannot reject the hypothesis of no discontinuity in the distribution density at the cutoff, and the p-value is 0.398.

Figure 2: Birth density relative to the cutoff



Note: Figure 2 plot the density of observation by each day in the data. The red line indicate the cutoff, while p-value is based on density test, run at daily level proposed by Cattaneo et al. (2018)

3 Data

I use confidential administrative datasets provided by several ministries in Chile, managed through the Integrated Social Registry of the Ministry of Social Development and Family (MDS). All data are at the census level, except for the Social Registry of Households, which covers between 70% and 90% of the population.

My primary sample includes all students from public and charter schools,² who started first grade between 2002 and 2007. I define a focal student as an individual born around the cutoff, with a bandwidth determined using the method outlined by Calonico et al. (2014). I construct the parent sample using the Vital Records and Cache datasets, which include biological or adoptive parents (0.07% of children in Chile are adopted, and I cannot identify if a child is adopted). The first dataset links children with their parents and reports the child's gender and date of birth, while the second provides the parents' gender and date of birth. For the sibling sample, I restrict the data to siblings of focal students born within 100 days of the cutoff, and I estimate sibling outcomes using the same data-driven bandwidth mechanism as before.

I apply two filters in the family spillover analysis. First, I include only the closest older and younger siblings when I analyze sibling spillovers. In other words, I only look at consecutive siblings from the focal children point of view. Second, I allow families to have more than one focal student. For example, a child born on June 2nd, 1996, who started first grade in 2002, and their sibling, born on July 5th, 1999, who started first grade in 2007, are both considered focal students. In this study, 7.31% of families may have multiple focal students, consistent with findings in the literature

²93% of students in Chile attend public or charter schools.

(Landersø et al. (2020) also reports 7% multiple focal students).

I derive educational outcomes from the datasets student performance records, standardized test results (SIMCE), college admission process (DEMRE), and MDS Education. GPA and attendance data come from the student performance database, spanning from 2002 to 2022. Standardized test scores come from SIMCE (the acronym for “Measurement System for Learning Outcomes”, in Spanish), a centralized test designed to evaluate learning based on the national curriculum. This study uses SIMCE test results from 4th, 6th, 8th, and 10th grades, covering the period from 2002 to 2022. Indicators of college test-taking, college admission test scores³, and post-secondary enrollment come from DEMRE, covering 2018 to 2022. Finally, I obtained data on years of education and post-secondary graduation from the student performance and MDS Education datasets, which also track post-secondary years of schooling, and the highest education level attained between 2017 and 2022.

Income information comes from the Chilean Pension Supervisor and the Social Registry of Households (known as the Social Protection Form until 2016) datasets. The first source provides information on formal income and the number of months worked each year. In contrast, the second documents self-reported employment status and total monthly income⁴. The monthly income variable captures the earnings of formal workers and informal workers who issue invoices. Information from the Chilean Pension Supervisor spans the period from 2014 to 2022. Self-reported employment information spans from 2014 to 2022, while total monthly income is available from 2016 to 2022.⁵ Access to these labor market data is crucial, as developing countries like Chile have high rates of informal employment. With 30% of the population working in informal sectors, comparing formal and informal labor markets provides external validity to my results.

I construct fertility outcomes using the Vital Records and Cache datasets. Teenage parenthood is defined as becoming a parent before turning nineteen. Unlike in other countries, male teenage parenthood is not highly underreported in Chile. Of all teenage births in this study, only 6.1% lack an identified father, compared to 5.1% for non-teenage births.

I use the Beneficios RSH dataset, created internally by MDS, to gather information on government transfers and the amount received.

Since income information is only available from 2014 onwards, I created a secondary sample to examine parents’ labor outcomes at children’s younger ages, including all public and charter school students who started first grade between 2008 and 2020. Finally, I use the variable “head of the household” to examine heterogeneous effects in mothers from the Social Registry of Households from 2014 to 2022.

Because income data is only available from 2014 onwards, I created a secondary sample to examine parents’ labor outcomes when children are younger. This sample includes all public and

³The exam is administered at the conclusion of the final year of high school and is a prerequisite for admission to most universities in the country

⁴The total monthly income variable is created using inputs from the Internal Revenue Service, the Chilean Pension Supervisor, and the Unemployment Fund Administrator. The Unemployment Fund Administrator’s data offers a broader range of observed income due to its higher earnings ceiling but excludes public sector workers, who account for 12% of the workforce (16% of women).

⁵The total monthly income variable was constructed with administrative records beginning in 2016; prior to that, the Social Protection Form only contained self-reported income.

charter school students who started first grade between 2008 and 2020. Finally, to analyze the heterogeneous effects of school starting age on mothers, I use the “head of the household” variable from the Social Registry of Households between 2014 and 2022.

4 Results

4.1 Effects on parents

4.1.1 Labor outcomes

This section examines how a child’s school-starting age affects parents’ labor market participation and income. Within each category, I present the results for both the overall market (formal and informal), and the formal market alone⁶ for mothers and fathers, separately.

My primary sample of study corresponds to focal students who started school between 2002 and 2007 and their families. Since I only have information on income and participation starting in 2014, the effects shown in Table 4 correspond to pooled effects on participation and income when the focal child is between 14 and 20 years old. To ensure that the observed effects are not driven by shocks unrelated to school starting age, I supplement the analysis with a secondary sample of students who started school between 2008 and 2020 and their families. The results presented in Figure 3 show that the effects are driven by the school starting age.

The first finding from Table 4 is that a child’s school starting age affects mothers’ and fathers’ labor outcomes differently. While mothers experience a decrease in participation and income, fathers show no significant effects. The negative effects on mothers appears across all types of labor markets, i.e., in both formal and informal markets combined, as well as in the formal market alone. However, the 2.4% reduction in income is only observed when considering the formal and informal markets together; there is no significant effect on income within the formal market alone.

⁶Information of participation in any sector comes from the self-reported employment status on the Social Registry of Households. This variable can under report individuals who work in absolute informality, in other words, individuals who do not declare their economic activity. Another type of informal workers, such as freelance or self-employed individuals who emit vouchers and fill out tax returns, are contemplated in this category and won’t be considered as under reported since they know that the MDS can identify their income, and lying would decrease the probability of receiving social assistance.

Table 4: Participation and income effects on parents

	Participation		Monthly Income	
	(1) Any sector	(2) Formal	(3) Any sector	(4) Formal
Panel A: Mothers				
Focal child born after school-entry cutoff	-0.010** (0.005)	-0.012*** (0.003)	-6,047** (2,717)	7,205 (5,211)
Mean before cutoff	0.56	0.327	252,913	67,805
Effect Size	-0.018	-0.0362	-0.024	0.0106
Bandwidth	61.33	43.08	73.2	66.48
Observations	1,699,124	2,362,469	1,160,689	1,181,604
Panel B: Fathers				
Focal child born after school-entry cutoff	-0.003 (0.003)	-0.005 (0.004)	11,139 (7,349)	3,083 (5,063)
Mean before cutoff	0.875	0.557	508,067	553,245
Effect size	-0.004	-0.00978	0.002	0.00557
Bandwidth	71.69	55.05	41.25	35.14
Observations	1,611,797	2,899,383	1,104,506	1,845,764

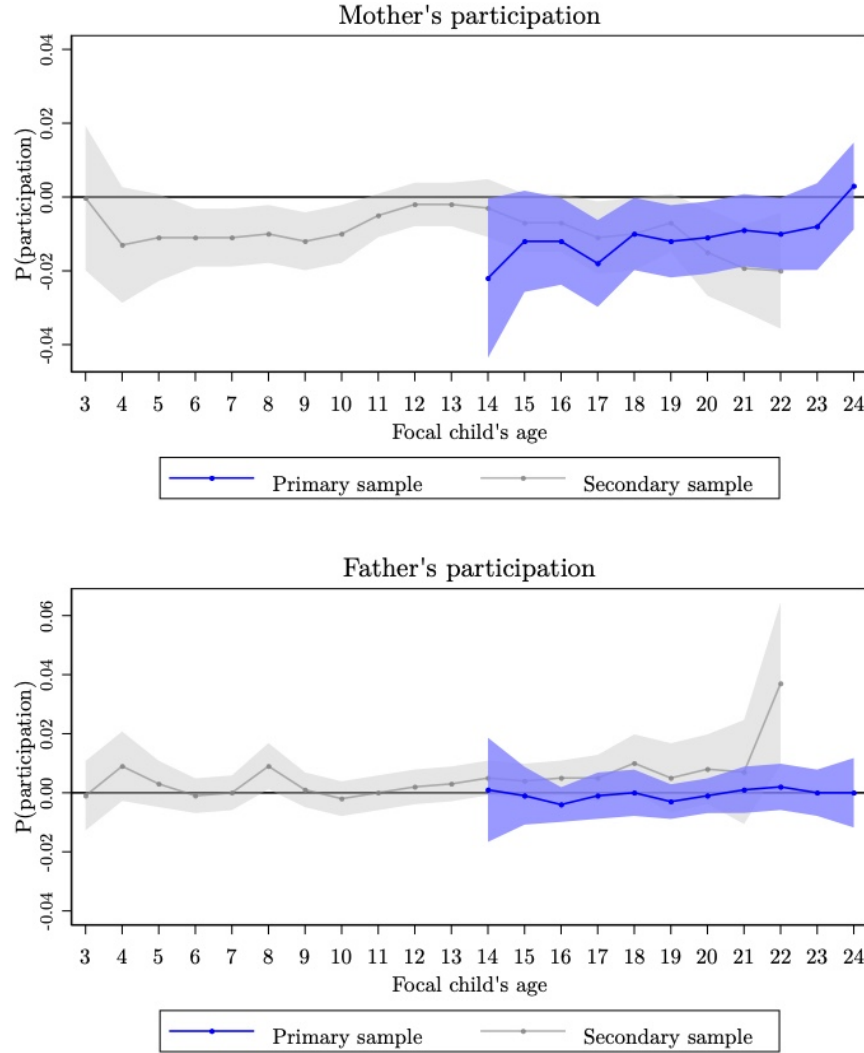
Note: All estimations include cohort fixed effects, year fixed effect, and control for student gender, class size, school rurality and type of school (public or charter), age of mother at any given year, and number of children. Robust standard errors (in parentheses) are clustered by the distance in days to the cutoff. Income is reported in 2023 real Chilean pesos. Calendar years 2014 to 2022. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5: Participation and Income effects in the formal market including secondary sample

	Primary Sample 2002-2007			Secondary Sample 2008-2020		
	(1) P(working)	(2) Positive Income	(3) Income	(4) P(working)	(5) Positive Income	(6) Income
Panel A: Mothers						
Focal child born after the cutoff	-0.012*** (0.003)	7,205 (5,211)	-5,430 (3,549)	-0.007*** (0.002)	-6,656 (4,898)	-7,456*** (2,745)
Mean before cutoff	0.327	67,805	221,200	0.355	744,375	264,194
Effect size	-0.0362	0.0106	-0.0245	-0.019	-0.00894	-0.0282
Bandwidth	43.08	66.48	49.41	37.22	41.86	36.76
Observations	2,362,469	1,181,604	2,689,536	8,016,378	3,136,355	7,800,970
Panel B: Fathers						
Focal child born after the cutoff	-0.005 (0.004)	13,278 (8,101)	3,083 (5,063)	0.002 (0.003)	166 (5,098)	1,286 (5,609)
Mean before cutoff	0.557	992,550	553,245	0.625	1,026,385	640,295
Effect size	-0.00978	0.0134	0.00557	0.00277	0.000162	0.00201
Bandwidth	55.05	35.67	35.14	59.05	40.73	42.27
Observations	2,899,383	1,025,357	1,845,764	12,425,910	5,239,684	8,820,044

Note: All estimations include cohort fixed effects and control for student gender, class size, school rurality, and type of school (public or charter), and mothers' age at any given year and number of children. Robust standard errors (in parentheses) are clustered by the distance in days to the cutoff.

Figure 3: Participation in any labor market by age of the focal child



Note: This combined plot shows the parents' participation in any market (formal and informal markets combined) by the age of the focal child. The gray series corresponds to a secondary sample of focal children starting school between 2008 and 2020. The purple/blue line corresponds to the primary sample: focal children starting school between 2002 and 2007. All estimations include cohort fixed effects and control for child's gender, class size, school rurality, type of school (public or charter), and parent's age at any given year and number of children. Robust standard errors (in parentheses) are clustered by the number of days to the cutoff.

4.1.2 Government assistance

The previous section showed that school starting age negatively impacts mothers' labor outcomes, and a natural question to answer is whether some of these adverse effects can be partially offset by an increased likelihood of receiving government assistance or by larger transfers. In Table 6, I present the effects of the focal child's school starting age on the probability of receiving government aid, the size of the transfer, and the number of programs in which the parent is enrolled.

As in the previous section, I find that a child's school-starting age affects mothers and fathers differently. Panel A of Table 6 shows that mothers of children born after the school-entry cutoff

date are not significantly different from their counterparts in terms of the probability of receiving government assistance. However, they receive larger annual transfers—about 2% more—and are enrolled in slightly more programs. Panel B reveals that, although fathers have a higher probability of receiving government assistance, there is no significant difference in the amount of transfers they receive or the number of programs in which they are enrolled.

Table 6: Government Assistance: Mothers and Fathers

	Government assistance		
	(1)	(2)	(3)
	P(Gov. help)	Transfer (\$ CLP)	N. of programs
Panel A: Mothers			
Focal child born after school-entry cutoff	0.001 (0.004)	16,336* (9,802)	0.024* (0.015)
Observations	2,263,797	770,360	970,839
Mean before cutoff	0.520	788,750	2
Effect Size	0.002	0.023	0.012
Bandwidth	71.55	49.85	58.48
Panel B: Fathers			
Focal child born after school-entry cutoff	0.007** (0.003)	-2,536 (21,790)	-0.009 (0.012)
Observations	2,441,622	500,648	289,929
Mean before cutoff	0.227	513,027	1
Effect Size	0.029	-0.005	-0.009
Bandwidth	71.95	75.94	37.98

Notes: This table shows the results of a local linear regression with the optimal bandwidth of the robust data-driven method proposed by Calonico et al. (2014) and triangular kernel for reduced-form estimates of Equation 1 on different forms of government assistance, for mothers and fathers separately. Column 1 corresponds to the probability of receiving any kind of government help. That help could be in goods or direct transfers. The dependent variable of Column 2 is the average annual amount of transfer in 2023 real Chilean Pesos. The results of Column 3 corresponds to the number of programs that a parent get effectively enrolled. All the estimations include the following controls: focal children cohort fixed effects, year fixed effect, and control for student gender, class size, school rurality and type of school (public or charter), age of the parent at any given year, and number of children of each parent at each point in time. Years analyzed: 2014 to 2019. *** p<0.01, ** p<0.05, * p<0.1.

4.2 Effects on siblings

4.2.1 Educational outcomes

Table 7 presents the main results for average GPA, standardized within school and grade, in primary school (across Grades 1-8). Following the tables' structure of Karbownik and Özek (2021), Panel A shows spillover effects from the focal student to the younger one (the dependent variable is the younger sibling's average GPA, and the running variable is the focal child's birthdate relative to the school-entry cutoff). Panel B examines spillovers to older siblings (the dependent variable corresponds to the older child's averaged GPA, and the running variable is the focal child's birthdate relative to the school-entry cutoff). Columns 1-3 present reduced-form results. Column 1 includes no controls, while Column 2 introduces individual characteristics for the sibling (those for whom I am measuring effects), such as an indicator for female sibling, indicators for Grades 1-8, an indicator for birth year, and an indicator for birth month. Column 3 builds on the controls of Column 2 by

adding controls of the focal student: the focal school starting age cohort (cutoff-cohort level with a single indicator for both children born before and after cutoff in a given cohort, for example, 2002), focal being a female and the age difference in years between siblings.

I observe negative and significant spillovers from older to younger siblings measured by average GPA. Specifically in Table 7, the results of Panel A indicate that students with older siblings born after the school-entry cutoff have a lower average GPA by about 2.8 to 3.0 percent of a standard deviation, compared to those with older siblings born before the cutoff. These results are robust to the inclusion of sibling and focal controls. On the other hand, Panel B shows no effects on the average GPA for older siblings.

Table 7: Mean GPA in Primary School: Spillovers to Siblings

	(1)	(2)	(3)
	Mean GPA primary school		
Panel A: Spillovers to younger siblings			
Focal child born after school-entry cutoff	-0.028** (0.013)	-0.025** (0.012)	-0.030** (0.013)
Mean before the cutoff	-0.006	-0.006	-0.002
Bandwidth	38	41	38
Observations	61,051	65,819	61,071
Sibling controls		X	X
Focal controls			X
Panel B: Spillovers to older siblings			
Focal child born after school-entry cutoff	0.005 (0.019)	0.001 (0.017)	-0.005 (0.016)
Mean before the cutoff	0.057	0.058	0.057
Bandwidth	32	32	33
Observations	60,205	60,205	60,205
Sibling controls		X	X
Focal controls			X

Notes: This table shows the results of a local linear regression with the optimal bandwidth of the robust data-driven method proposed by Calonico et al. (2014) and triangular kernel for reduced-form estimates of Equation 1 on mean GPA of primary school (Grade 1- Grade 8, standardized within school, grade and class). Column 1 does not include any controls. Column 2 controls for individual covariates of child for whom I measure the outcome. Column 3 further includes individual controls of the focal child from the pair. Siblings controls are: indicator for female student, indicators for Grades 1-8, indicators for birth year and an indicator for birth month. Focal controls are indicators for sibling school starting cohorts (cutoff-cohort level with a single indicator for both children born before and after cutoff in a given cohort, for example, 2002), indicator for a female, and age difference between siblings in years. *** p<0.01, ** p<0.05, * p<0.1

To ensure the results are robust to different measures of academic performance, I look at the effects of the focal student's school starting age on siblings using scores from the SIMCE exam, a centralized and standardized test designed to assess learning based on the national curriculum. Students take the SIMCE exam depending on a specific combination of grade and year. While all

students in the sample took the Grade 4 exam, not all of them took the exams in Grades 6, 8, and 10. Older siblings, in particular, have no estimates for Grade 6 SIMCE because that exam was administered almost every year starting in 2013. By this time, most older siblings were already in higher grades.

Table 8 presents the estimates of the effects on test scores by grade for siblings. To avoid visual clutter, I only present the results with siblings and focal-student controls included, but the version with each control added gradually can be found in Appendix A.1. The results indicate that the effects remain negative, with test scores between 5 percent of a standard deviation lower in grade 10 and 7.7 percent of a standard deviation lower in Grade 8 for those with older siblings born after the cutoff. These findings indicate that the effects are not only robust across different measures of academic performance but also persist into high school.

Table 8: Sibling Spillovers in Test Scores by grade

	Test scores			
	(1) Grade 4	(2) Grade 6	(3) Grade 8	(4) Grade 10
Panel A: Spillovers to younger siblings				
Focal child born after school-entry cutoff	-0.041*** (0.016)	-0.058** (0.025)	-0.077*** (0.029)	-0.050* (0.026)
Mean before the cutoff	-0.183	-0.198	-0.146	-0.106
Bandwidth	33	30	27	28
Observations	46,646	20,612	22,985	24,215
Sibling controls	X	X	X	X
Focal controls	X	X	X	X
Panel B: Spillovers to older siblings				
Focal child born after school-entry cutoff	-0.032 (0.030)		-0.010 (0.034)	0.037 (0.036)
Mean before the cutoff	-0.142	-	-0.119	-0.117
Bandwidth	27	-	26	19
Observations	26,954	-	17,668	15,522
Sibling controls	X	-	X	X
Focal controls	X	-	X	X

Notes: I use a local linear regression with the optimal bandwidth from the robust data-driven method proposed by Calonico et al. (2014) and a triangular kernel to estimate the reduced-form results based on Equation 1. The dependent variable is each test score from the SIMCE exams taken by students in Grades 4, 6, 8, and 10. Because the specific exam a student takes depends on the year and grade they are in, no older siblings took the Grade 6 SIMCE exam, so I excluded that estimation in Panel B. The sample in Panel A includes all children for whom we observe two adjacent siblings, with at least one SIMCE test score for the younger sibling, where the older sibling is the focal child. The sample in Panel B includes all children for whom we observe two adjacent siblings, with at least one SIMCE test score for the older sibling, where the younger sibling is the focal child. There is one observation per individual in all columns. If a student took the same test more than once (e.g., due to grade repetition), I kept only the first attempt. Unlike previous tables of sibling spillovers, to avoid visual cluttering, I only reported the results that included siblings and focal student controls. However, the full table can be found in Appendix A.1 Robust standard errors are clustered by the running variable at the daily level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Another important indicator of school performance is grade retention. Grade retention can be seen as a result of poor performance, or it may contribute to future underperformance (Valbuena et al. (2021)). Table 9 presents the effects on grade retention, showing a positive impact on the likelihood of being held back, ranging from 5.2 to 7.5 percent of a standard deviation for younger siblings, with no significant effects for older siblings.

Table 9: P(Grade Retention): Spillovers to Siblings

	(1)	(2)	(3)
	P(Grade Retention)		
Panel A: Older to younger			
Focal child born after school-entry cutoff	0.020*	0.018*	0.028***
	(0.011)	(0.011)	(0.001)
Mean before the cutoff	0.377	0.378	0.38
Effect size	0.052	0.047	0.075
Bandwidth	40.1	39.44	39.5
Observations	33,313	32,487	32,487
Own controls		X	X
Sibling controls			X
Panel B: Younger to older			
Focal child born after school-entry cutoff	0.005	0.003	0.012
	(0.009)	(0.009)	(0.009)
Mean before the cutoff	0.394	0.393	0.393
Effect size	0.011	0.006	0.028
Bandwidth	32.7	31.7	31.6
Observations	38,833	32,804	32,804
Own controls		X	X
Sibling controls			X

Notes: I use a local linear regression with the optimal bandwidth from the robust data-driven method proposed by Calonico et al. (2014) and a triangular kernel to estimate the reduced-form results based on Equation 1. The dependent variable is the probability of repeating a grade for all siblings who completed 1-12 education. Column 1 does not include any controls. Column 2 controls for individual covariates of the child for whom I measure the outcome. Column 3 further includes individual controls of the focal child from the pair. Own controls are: indicator for female student, indicators for birth year, indicators for birth month. Sibling controls are indicators for sibling school starting cohorts (cutoff-cohort level with a single indicator for both children born before and after cutoff in a given cohort, for example, 2002), indicator for sibling being a female, and age difference between siblings in years. *** p<0.01, ** p<0.05, * p<0.1

After presenting the in-school effects on siblings, I examine whether these effects extend beyond high school. Table 10 displays the results for three out-of-school outcomes: Taking the college entrance exam, Entrance exam scores, and Post-secondary enrollment. Instead of measuring the effects at a certain age (e.g., 20 years old), I look at the outcome in the year siblings graduate from high school⁷. I find no significant effects on the probability of taking the college entrance exam, except for a negative effect on older siblings, though this result is not robust across different specifications. Additionally, I find no effects on the entrance exam scores for either group of siblings. However, I find significant effects on the probability of enrollment in a post-secondary institution. School starting age affects younger siblings, decreasing the likelihood of enrolment by about 9.6%

⁷However, I also ran estimations based on whether the student took the college entrance exam at any point after graduating from high school, and the results remain the same. These results are available upon request

to 12.8%. For older siblings, there is a positive effect on enrollment, ranging from 7% to 11%, although the results are not robust for all specifications.

Table 10: Post-secondary schooling effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	P(College entrance exam)			Entrance exam score			P(Enrollment)		
Panel A: Spillovers to younger siblings									
Focal child born after school-entry cutoff	-0.006 (0.019)	-0.006 (0.019)	-0.009 (0.019)	-0.016 (0.042)	-0.013 (0.042)	-0.033 (0.042)	-0.067*** (0.019)	-0.065*** (0.018)	-0.074*** (0.019)
Mean before the cutoff	0.690	0.693	0.69	-0.17	-0.171	-0.17	0.33	0.33	0.44
Effect size	-0.008	-0.008	-0.013				-0.202	-0.197	-0.094
Bandwidth	35.1	35	35.1	29.6	28.9	29.1	24.5	25	25.75
Observations	21,960	21,278	21,960	12,450	12,002	12,450	10,275	10,694	10,694
Sibling controls		X	X		X	X		X	X
Focal controls			X			X			X
Panel B: Spillovers to older siblings									
Focal child born after school-entry cutoff	-0.006 (0.012)	-0.004 (0.012)	-0.027** (0.013)	0.057 (0.048)	0.048 (0.048)	-0.002 (0.047)	0.005 (0.015)	0.002 (0.014)	-0.0002 (0.014)
Mean before the cutoff	0.67	0.669	0.669	-0.208	-0.208	-0.208	0.229	0.228	0.227
Effect size	-0.009	-0.006	-0.04				0.0213	0.007	-0.001
Bandwidth	37.7	36.4	36.3	29	29.37	29.4	39.3	38.5	38.4
Observations	33,032	32,108	32,108	17,288	17,288	17,289	23,148	22,572	22,572
Sibling controls		X	X		X	X		X	X
Focal controls			X			X			X

Notes: I use a local linear regression with the optimal bandwidth from the robust data-driven method proposed by Calonico et al. (2014) and a triangular kernel to estimate the reduced-form results based on Equation 1. The dependent variable from Columns 1 to 3 is taking the college entrance exam; Columns 4 to 6 is the Entrance Exam Score, and Columns 7 to 9 is the Enrollment in a post-secondary institution. Columns 1, 4, and 7 do not include controls. Columns 2, 5, and 8 controls for individual covariates of the child for whom I measure the outcome. Columns 3, 6, and 9 further include individual controls of the focal child from the pair. Own controls are indicator for female student, indicators for birth year, and indicators for birth month. Sibling controls are indicators for sibling school starting cohorts (cutoff-cohort level with a single indicator for both children born before and after cutoff in a given cohort, for example, 2002), indicator for sibling being a female, and age difference between siblings in years. *** p<0.01, ** p<0.05, * p<0.1

4.2.2 Fertility outcomes

To conclude the analysis of sibling spillovers, I examine the effects of the focal student's school starting age on siblings' early fertility. In particular, Table 11 presents the effects on siblings' teen parenthood. As in the previous tables, Panel A shows the spillover effects from the focal student to the younger sibling (the dependent variable is the younger sibling's probability of becoming a teen parent, and the running variable is the focal child's birthdate relative to the school-entry cutoff). Panel B examines spillovers to older siblings (the dependent variable corresponds to the older sibling's probability of becoming a teen parent, and the running variable is the focal child's birthdate relative to the school-entry cutoff).

I divide the sample in each panel into female and male siblings to investigate whether there is any gender-related heterogeneity in the effects. Columns (1), (4), and (7) include no controls; Columns (2), (5), and (8) add sibling controls: an indicator for birth year, an indicator for birth

month (Column 2 also includes an indicator for whether the sibling is female). Columns (3), (6), and (9) build on the previous controls by adding focal student controls: the focal student's school starting age cohort (cutoff-cohort level with a single indicator for both children born before and after the cutoff in a given cohort, for example, 2002), whether the focal child is female, and the age difference in years between siblings.

I find that the focal student's starting age does not affect either younger or older siblings' probabilities of becoming a teen parent. These results are robust across different specifications, such as examining female and male siblings' outcomes separately and incorporating various controls.

Table 11: Siblings teen parenthood spillovers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All			Sibling Female			Sibling Male		
Panel A: Spillovers to younger siblings									
Focal child born after school-entry cutoff	0.00132 (0.00257)	0.0015 (0.00253)	0.00226 (0.0026)	0.00115 (0.00397)	0.00112 (0.00397)	0.00115 (0.00397)	0.00153 (0.00282)	0.00152 (0.00282)	0.00153 (0.00282)
Mean before the cutoff	0.0378	0.038	0.038	0.0548	0.0546	0.0548	0.02179	0.0217	0.0217
Effect size	0.035	0.0392	0.06	0.0454	0.0454	0.0454	0.134	0.134	0.134
Bandwidth	60.01	59.63	60.39	59.867	60.145	59.867	64.038	64.048	64.389
Observations	206,961	203,667	206,961	99,753	101,356	99,753	112,525	112,525	112,525
Sibling controls		X	X		X	X		X	X
Focal controls			X			X			X
Panel B: Spillovers to older siblings									
Focal child born after school-entry cutoff	0.00349 (0.0037)	0.00345 (0.00362)	0.00349 (0.0037)	0.0065 (0.0047)	0.0065 (0.0047)	0.0065 (0.0047)	-0.00208 (0.00389)	-0.00208 (0.00389)	-0.00208 (0.00389)
Mean before the cutoff	0.096	0.0959	0.0959	0.133	0.1333	0.1333	0.0604	0.060	0.060
Effect size	0.0364	0.036	0.0344	0.0493	0.0493	0.0493	0.0349	0.0349	0.0349
Bandwidth	59	58.41	58.51	71	71.12	71.22	54	53.97	54.07
Observations	203,717	203,717	203,717	121,476	121,476	121,476	95,383	97,151	97,151
Sibling controls		X	X		X	X		X	X
Focal controls			X			X			X

Note: All estimations include sibling controls and focal controls as indicated in each column. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

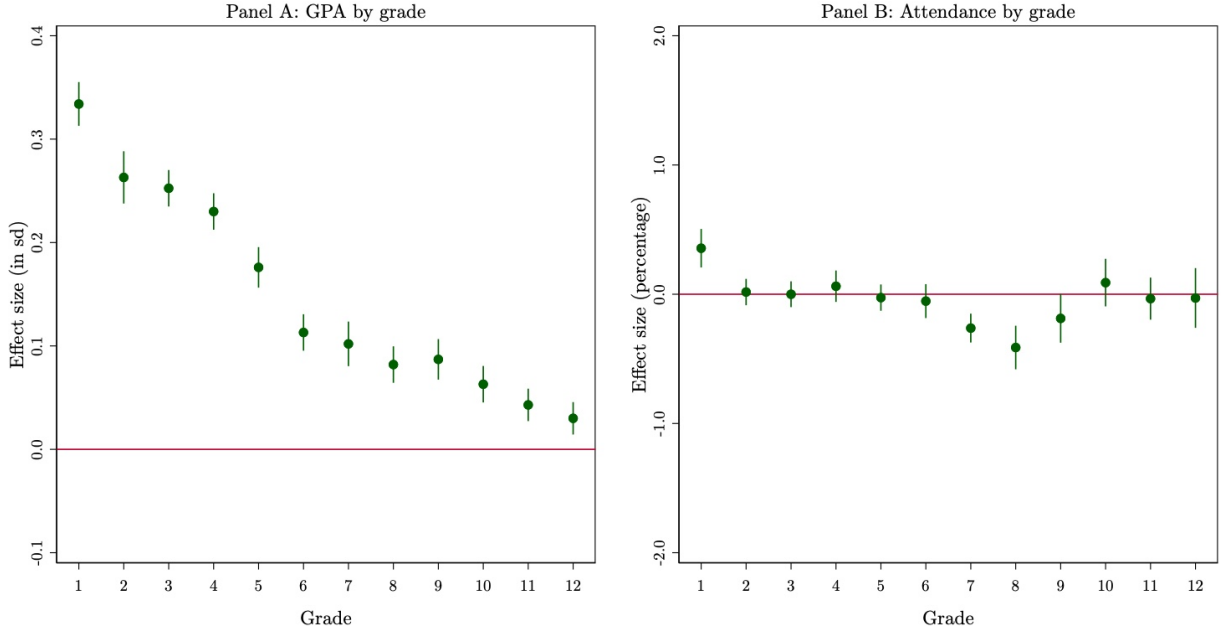
4.3 Effects on focal students

4.3.1 In school effects

Figure 1 shows that students born on or after the cutoff start first grade 0.58 years older (about seven months) than those born before the cutoff. This age difference increases the likelihood of having a skill advantage over younger peers. Panel A of Figure 4 illustrates this. Figure 4 displays the impact of school starting age by grade on two important outcomes: GPA and attendance. These outcomes can be observed year by year, making them particularly useful for measuring effects over time in a more granular way. The first remark is that the effects of SSA on GPA are consistently positive and statistically significant. The effect in Grade 1 is 33% of a standard deviation, which is substantial but aligned with the literature. However, these effects fade over time, and by Grade 12, the effect is reduced to one-tenth of the initial value, reaching 3% of a standard deviation. A

different trend is observed for attendance. Effects are only significant in Grade 1 (positive) and Grades 7, 8, and 9 (negative). The point estimates are small, and since the mean attendance across grades is above 90, these results suggest that the effects of the school starting age on attendance are negligible. If anything, the significant effects in those years likely reflect a precise zero.

Figure 4: School outcomes by grade



Note: This table presents the results of a local linear regression with the optimal bandwidth of the robust data-driven method proposed by Calonico et al. (2014), with a triangular kernel for reduced-form estimates of Equation 1 on two different outcomes. Panel A displays the results for GPA from Grade 1 to Grade 12 (standardized within school, grade, and class), reported in standard deviations. Panel B shows the effect on attendance by grade (in percentages, with the mean of attendance by grade is approximately 91%). The controls applied include indicators for female, public school attendance, rural school attendance, and the number of students in the classroom.

Another well-explored outcome of school starting age is its effect on test scores. Table 12 presents the effects on test scores for Grades 4, 8, and 10. These results show a similar pattern to those for GPA: the strongest effect is always observed when students are younger. For all students, the effect on SIMCE Grade 4 is 19% of a standard deviation, while in Grade 10, it is nearly 10% of a standard deviation. The effect is consistently larger for females than for males, though the difference between the two coefficients is not statistically significant.

Table 12: Test Scores by Gender

	Test scores		
	(1)	(2)	(3)
	Grade 4	Grade 8	Grade 10
Panel A: All students			
Born after school-entry cutoff	0.191*** (0.008)	0.094*** (0.011)	0.097*** (0.009)
Mean before cutoff	-0.159	-0.139	-0.143
Bandwidth	60	42	74
Observations	328,071	128,658	300,664
Panel B: Female students			
Born after school-entry cutoff	0.199*** (0.011)	0.102*** (0.018)	0.090*** (0.012)
Mean before cutoff	-0.132	-0.123	-0.105
Bandwidth	61	46	60
Observations	164,748	71,426	121,100
Panel C: Male students			
Born after school-entry cutoff	0.180*** (0.012)	0.082*** (0.015)	0.105*** (0.014)
Mean before cutoff	-0.186	-0.155	-0.138
Bandwidth	69	58	79
Observations	193,981	92,266	161,701

Notes: I use a local linear regression with the optimal bandwidth from the robust data-driven method proposed by Calonico et al. (2014) and a triangular kernel to estimate the reduced-form results based on Equation 1. The dependent variable is each test score from the SIMCE exams that students took in Grades 4, 8, and 10. The sample in Panel A includes all focal students (females and males) for whom at least one SIMCE test score is available. The samples in Panel B and Panel C include females and males, respectively. Each column represents one observation per individual. If a student took the same test more than once (e.g., due to grade repetition), I only kept the first attempt. All estimations control for cohort fixed effects, student gender, class size, school rurality, and school type (public or charter). Robust standard errors (in parentheses) are clustered by the running variable at the daily level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Having documented the in-school effects, I now examine how the SSA impacts educational outcomes as students grow older, particularly as they transition out of school. Table 13 summarizes the most important outcomes related to this transition.

Columns 1 and 2 provide alternative measures of grade retention. Column 1 shows the probability that a student completes 12 years of education within 12 years. Complementarily, Column 2 reports the number of years it takes a student to finish grades 1-12. The results indicate that relatively older students are almost 12% more likely to complete their mandatory schooling in 12 years. On average, older students take 12.36 years to complete grades 1-12, compared to 12.49 for

younger students.

Column 3 presents the results on the probability of dropping out after 12 years in school. Although Chile does not have a compulsory schooling law that allows students to drop out after a certain age, I observe a 20% increase in the probability of dropping out for relatively older students. This result can be explained for two reasons: First, dropping out is rare -only 3% of students drop out after 12 years at school. Because the mean is so low, even point estimates as small as 0.006 provide substantial effect sizes. Second, and more importantly, while relatively older students tend to perform better, they also reach the age of majority (18 years old) before their younger counterparts, which may influence their decision to drop out.

The next set of results, Columns 4 to 7, focuses on post-secondary education. Column 4 shows the effect on the probability of taking the college entrance exam. This outcome measures the year the student graduates from high school, and shows a 2.7 percentage point increase, translating to a 4% higher probability of taking the college admissions test. Additionally, older students perform better on the test, scoring 5.3% of a standard deviation higher. They are also more likely to enroll in post-secondary education. However, they are 12.5% less likely to graduate from college by age 25 compared to younger students. This may be due to the fact that, since they started school at a relatively older age, they are more likely to take longer to complete their college education.

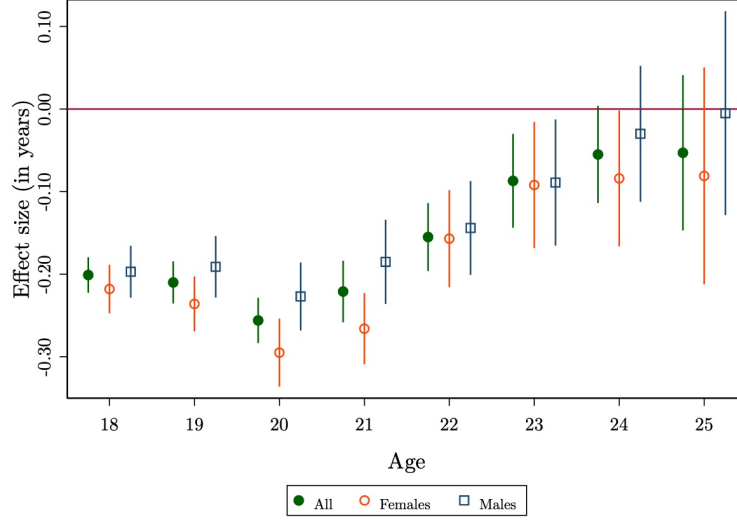
Table 13: Transition Out of School Effects

	(1) P(1-12 education in 12 years)	(2) Number years to complete 1-12 education	(3) P(dropping out after 12 years in school)	(4) P(taking college exam)	(5) College admission test score	(6) Enrollment tertiary education	(7) P(College graduate by age 25)
Born after school-entry cutoff	0.070*** (0.004)	-0.121*** (0.008)	0.006*** (0.001)	0.027*** (0.003)	0.053*** (0.009)	0.0164*** (0.004)	-0.035*** (0.004)
Mean before cutoff	0.594	12.49	0.03	0.684	-0.158	0.276	0.278
Effect size	0.117	-0.01	0.199	0.04	-	0.059	-0.125
Bandwidth	25.73	29.2	70.69	72.84	45.14	64.6	70
Observations	181,622	186,689	509,546	396,345	216,326	406,583	286,477

Notes: I use a local linear regression with the optimal bandwidth from the robust data-driven method proposed by Calonico et al. (2014) and a triangular kernel to estimate the reduced-form results based on Equation 1. The dependent variable in Column 1 is the Probability of completing 1-12 education in 12 years, meaning the probability of not repeating a grade. Column 2 is the number of years the student took to complete grades 1-12. Column 3 is the probability of dropping out after being in school for 12 years (starting from Grade 1). Column 4 represents the probability of taking the college entrance exam, conditional on graduating from high school. Column 5 is the college admission test score (standardized by year). Column 6 shows enrollment in any post-secondary institution, and Column 7 is the probability of being a college graduate by age 25. All estimations control for cohort fixed effects, student gender, class size, school rurality, and school type (public or charter). Robust standard errors (in parentheses) are clustered by the running variable at the daily level. *** p<0.01, ** p<0.05, * p<0.1.

A more nuanced analysis of educational outcomes is displayed in Figure 5, which shows the effect of SSA on the number of years of education by gender of the focal student aged 18 and older. I observe that relatively older students consistently show fewer years of education, with the effect size peaking at age 20, where they have 2% fewer years of education. However, the effect size gradually decreases to a negligible 0.4% by age 25.

Figure 5: Effect on number of years of education by gender, for students aged 18 and older



Notes: I use a local linear regression with the optimal bandwidth from the robust data-driven method proposed by Calonico et al. (2014) and a triangular kernel to estimate the reduced-form results based on Equation 1. The dependent variable on this figure is the number of years of education conditional on the age of the student. The green solid circles represent all students (females and males); the orange hollow circles represent females, and the blue hollow squares represent males. All estimations control for cohort fixed effects, student gender, class size, school rurality, and school type (public or charter). Robust standard errors (in parentheses) are clustered by the running variable at the daily level.

4.3.2 Early labor outcomes effects

Following the discussion on educational outcomes in Subsection 4.3.1, I now examine early labor outcomes, particularly participation and real monthly income. Table 14 displays the average effects on labor outcomes between the ages of 18 and 26. The table summarizes the effects on participation in the formal market, positive income (i.e. conditional on participation), and income (real monthly income each year, assigning a value of zero for any period without recorded participation). Panel A includes all individuals who worked at least once during the analyzed period, while Panel B presents a more restricted sample, requiring individuals to have worked more than six months each year⁸. The results in Panel A show that being born after the school-entry cutoff decreases the probability of participating in the formal market by about 5.6% and reduces income by 8%. Additionally, conditional on participation, these students earn about 3.9% lower salaries. In the more restricted sample of Panel B, which ensures employment is not temporary, the effects are similar in sign, but larger in magnitude: being born after the school-entry cutoff decreases the probability of participating in the formal market by about 9%, reduces income by about 4%, and conditional on participation, the effect on income is a 9.5% reduction.

⁸I use this definition as a proxy for a “full time job”, as part-time jobs are almost non-existent in Chile. Additionally, it is uncommon for students to work while studying; most students focus solely on their education since most of them live with their parents while pursuing post-secondary education. Finally, Teaching Assistance (TA) work is paid through fee-for-service invoices and it is not subject to pension contribution, meaning this type of work does not appear in the Chilean Pension Supervisor data. In other words, I won’t be misclassifying TAs’ as workers, since TAs do not appear in that dataset. Note of caution: Since 2019, workers who issue fee-for-service invoices are subject to pension contributions. However, the amount received for TA work is so low (less than 10% of the minimum wage) that it falls under the exempted category.

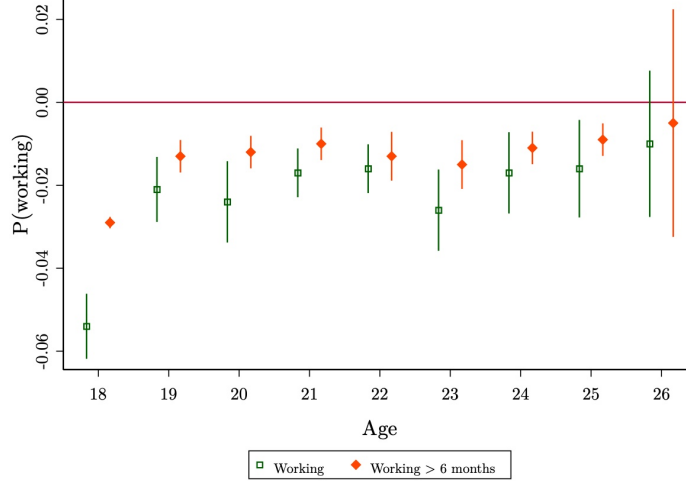
Table 14: Effects of Being Born After School-Entry Cutoff on Labor Outcomes

	Panel A: Working			Panel B: Working more than 6 months in a year		
	Participation	Positive income	Income	Participation	Positive income	Income
	(1)	(2013 Chilean pesos)	(2013 Chilean pesos)	(4)	(2013 Chilean pesos)	(2013 Chilean pesos)
	(1)	(2)	(3)	(4)	(5)	(6)
Focal child born after school-entry cutoff	-0.028*** (0.003)	-17,150*** (2,228)	-17,678*** (1,715)	-0.022*** -0.003	-20,310*** (3,738)	-18,761*** (1,796)
Mean before cutoff	0.501	441,171	221,159	0.252	532,719	197,774
Effect Size	-0.056	-0.039	-0.080	-0.0876	-0.0381	-0.0949
Bandwidth	35.13	75.31	46.26	46.39	67.91	57.77
Observations	1,497,092	1,577,944	1,967,669	1,316,864	800,606	1,934,660

Notes: I use a local linear regression with the optimal bandwidth from the robust data-driven method proposed by Calonico et al. (2014) and a triangular kernel to estimate the reduced-form results based on Equation 1. This table presents the average effects on labor outcomes between the ages 18 and 26, summarizing the effects on participation in the formal market, positive income (i.e., conditional on participation), and total income (real monthly income each year, assigning a value of zero for any period without recorded participation). Panel A includes all individuals who worked at least once during the analyzed period, while Panel B includes a more restricted sample, requiring individuals to have worked more than six months each year (I use this definition as a proxy for a “full-time job”, as part-time jobs are almost non-existent in Chile). All estimations include cohort-fixed effects, year-fixed effects, and controls for student gender, class size, school rurality, and school type (public or charter). Robust standard errors (in parentheses) are clustered by the distance in days to the cutoff. Participation and income data come from the Chilean Pension Supervisor from 2014 to 2022. Income is reported in 2023 real Chilean pesos.

Figure 6 provides a more in-depth analysis of participation outcomes. The chart displays the probability of participation in the formal market by age, between 18 and 26, distinguishing between the same individuals in Panel A and Panel B. The green hollow squares represent all individuals who worked at least once during the analyzed period, while the orange diamonds represent a more restricted sample, requiring individuals to have worked more than six months each year. The results show that the effect on labor market participation is consistently negative for students born after the cutoff, and these effects only fade when the student turns 26. It also shows that the point estimates are not statistically different between the two samples. However, these results should be interpreted with caution. As shown in Table 15, the means of participation differ between Panel A and Panel B—participation in Panel B is much lower than in Panel A—resulting in effect sizes for this alternative version of “Full-time worker” being at least twice as large.

Figure 6: Participation in the formal labor market



Notes: I use a local linear regression with the optimal bandwidth from the robust data-driven method proposed by Calonico et al. (2014) and a triangular kernel to estimate the reduced-form results based on Equation 1. The dependent variable on this figure is participation in the formal labor market conditional on the student's age between 18 and 26. The green hollow squares represent all individuals who worked at least once during the analyzed period, while orange diamonds represent a more restricted sample, requiring individuals to have worked more than six months each year (I use this definition as a proxy for a "full-time job", as part-time jobs are almost non-existent in Chile). All estimations include cohort-fixed effects, year-fixed effects, and controls for student gender, class size, school rurality, and school type (public or charter). Robust standard errors are clustered by the distance in days to the cutoff. Participation and income data come from the Chilean Pension Supervisor from 2014 to 2022.

Table 15: Probability of Working by Age

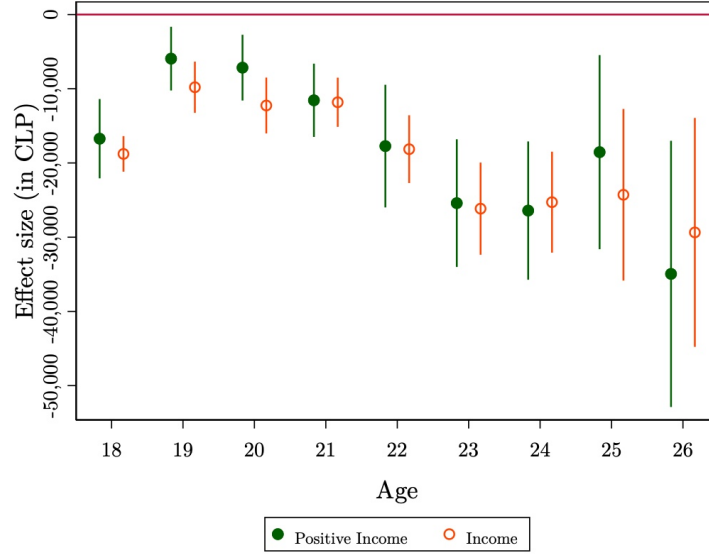
	Probability of working by age								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	18	19	20	21	22	23	24	25	26
Panel A: Working									
Born after school-entry cutoff	-0.054*** (0.004)	-0.021*** (0.004)	-0.024*** (0.005)	-0.017*** (0.003)	-0.016*** (0.003)	-0.026*** (0.005)	-0.017*** (0.005)	-0.016*** (0.006)	-0.01 (0.009)
Mean before cutoff	0.299	0.44	0.47	0.51	0.54	0.58	0.62	0.67	0.675
Effect size	-0.179	-0.048	-0.051	-0.034	-0.03	-0.044	-0.027	-0.024	-0.015
Bandwidth	36	45	36	47	66	66	70	46	
Observations	213,214	274,266	219,416	280,222	404,229	324,294	222,523	174,504	57,430
Panel B: Working more than 6 months in a year									
Born after school-entry cutoff	-0.029*** (0.0007)	-0.013*** (0.002)	-0.012*** (0.002)	-0.010*** (0.002)	-0.013*** (0.003)	-0.015*** (0.003)	-0.011*** (0.002)	-0.009*** (0.002)	-0.005 (0.014)
Mean before cutoff	0.01	0.058	0.1	0.136	0.171	0.173	0.157	0.123	0.14
Effect size	-2.9	-0.228	-0.117	-0.075	-0.08	-0.086	-0.073	-0.072	-0.036
Bandwidth	51	48	73	54	58	71	53	47	48
Observations	311,595	292,837	447,248	329,782	354,935	434,936	323,742	348,693	62,837

Notes: I use a local linear regression with the optimal bandwidth from the robust data-driven method proposed by Calonico et al. (2014) and a triangular kernel to estimate the reduced-form results based on Equation 1. This table presents the average effects on labor outcomes between the ages 18 and 26, summarizing the effects on participation in the formal market. Panel A includes all individuals who worked at least once during the analyzed period, while Panel B includes a more restricted sample, requiring individuals to have worked more than six months each year (I use this definition as a proxy for a "full-time job", as part-time jobs are almost non-existent in Chile). All estimations include cohort-fixed effects, year-fixed effects, and controls for student gender, class size, school rurality, and school type (public or charter). Robust standard errors (in parentheses) are clustered by the distance in days to the cutoff. Participation and income data come from the Chilean Pension Supervisor from 2014 to 2022.

Finally, Figure 7 illustrates the effects on formal labor market income for "full-time" workers. It shows that the effect sizes in real Chilean Pesos (base year 2023) are negative for all ages and that the effect size increases over time. These effects grow not only because average income increases with age, but also because the effect size itself does not diminish over time. Excluding age 18 from the analysis (as relatively older students might still be studying), the effect size at age 19 is a 1.8% reduction in positive income, which increases to 5% by the time these students reach 26 years old.

However, when incorporating zeroes from nonparticipants, the effect remains relatively around 6%, except for age 18, where the effect size is -22%, and age 23, where it is -9%.

Figure 7: Effects on labor market income full time workers



Notes: I use a local linear regression with the optimal bandwidth from the robust data-driven method proposed by Calonico et al. (2014) and a triangular kernel to estimate the reduced-form results based on Equation 1. The dependent variable in this figure is monthly income (in real Chilean pesos year 2023), conditional on the student's age between 18 and 26. The green circles represent positive income (i.e., conditional on working), while the orange hollow circles represent income, assigning a value of zero for any age without recorded participation. All estimations include cohort-fixed effects, year-fixed effects, and controls for student gender, class size, school rurality, and school type (public or charter). Robust standard errors are clustered by the distance in days to the cutoff. Participation and income data come from the Chilean Pension Supervisor from 2014 to 2022.

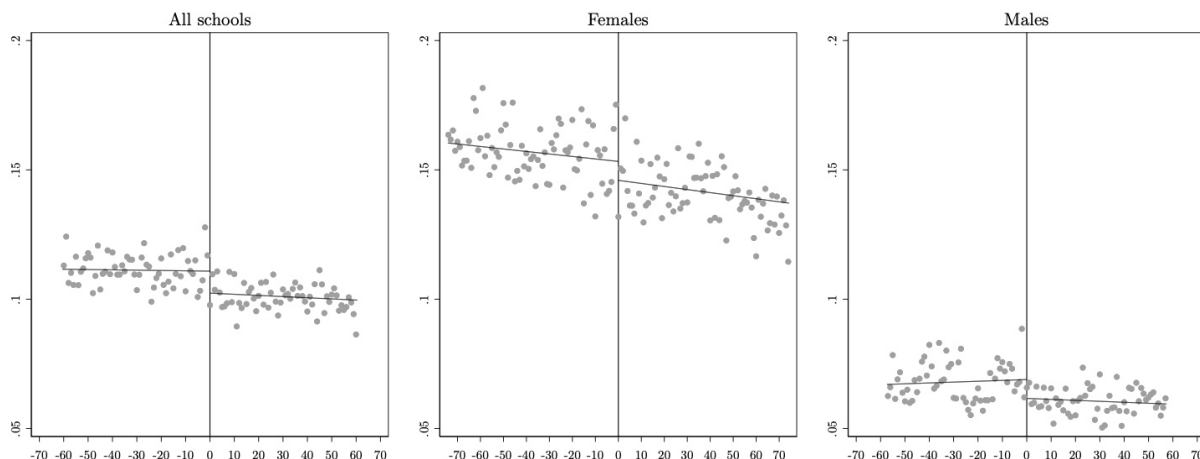
4.3.3 Fertility

One of the least explored outcomes, but one with long-lasting negative effects, is teenage parenthood. Teenage parenthood is defined as becoming a parent at 19 years old or younger. Table 16 shows that being born after the cutoff decreases the probability of becoming a teen parent. Although the point estimates are smaller than one percentage point, the effect sizes are substantial. For the pooled sample (females and males), the effect is a reduction of 8.2%; for females, it is a reduction of 5.6%, and for males, it is a reduction of 12.1%. Figure 8 provides a visual representation of these results. Robustness checks confirming the results using different bandwidths (Table A.2), local linear regression (Table A.3), and minimum school starting age—defined as the child's age if she started school the first year allowed by law—as an instrument for actual SSA (Table A.4) are presented in the Appendix A.3.1.

Table 16: Effect of Being Born After School-entry Cutoff on Teen Parenthood by Gender

	(1)	(2)	(3)
	All	Females	Males
Born after school-entry cutoff	-0.009*** (0.003)	-0.009* (0.005)	-0.008*** (0.003)
Mean before cutoff	0.111	0.157	0.068
Effect Size	-0.082	-0.056	-0.121
Bandwidth	60.6	74.2	57.4
Observations	366,855	222,384	177,671

Note: All estimations include cohort fixed effects, year fixed effects, and control for student gender, class size, school rurality, and type of school (public or charter). Robust standard errors (in parentheses) are clustered by the distance in days to the cutoff. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure 8: Teen parenthood

Notes: I use a local linear regression with the optimal bandwidth from the robust data-driven method proposed by Calonico et al. (2014) and a triangular kernel to estimate the reduced-form results based on Equation 1. The dependent variable on this figure is the probability of becoming a parent before turning 20. Panel All schools correspond to females and males together, Panel Females correspond to female students, and Panel Males correspond to male students. All estimations control for cohort fixed effects, student gender, class size, school rurality, and school type (public or charter). Robust standard errors (in parentheses) are clustered by the running variable at the daily level.

5 Suggestive mechanisms

In this paper, I showed that a child born after the cutoff reduces the mother's labor supply and negatively impacts the educational outcomes of younger siblings. In this section, I propose two complementary mechanisms that may explain these results. The first mechanism — time spent with the mother — suggests that parental resources are limited, and the increased childcare demands of the focal child force the mother to redistribute her time, which was initially devoted entirely to the younger children. As a result, the younger children experience a reduction in time spent with their mother, while the older ones receive more attention. If this were the case, the time spent

with parents could be a key factor explaining the negative effect on younger siblings. To partially test this hypothesis, I divide my sample into two groups: mothers who are more likely to stay at home and could be exposed to this mechanism and those who cannot, showing that even if not statically significant differences, children of the first set of mothers have larger differences between siblings than ones of the second set of mothers. The second mechanism — parental responses to children’s abilities — suggests that parents could reinforce, rather than compensate for, differences in their children’s abilities. If this were the case, the differences we observe between siblings may come from the parents’ perception that the focal child born after the cutoff is more capable due to better academic performance. As a result, parents invest more in that child at the expense of the younger siblings, leading to the observed results.

Finally, in the last part of this section, I rule out grandmother’s participation in childrearing as a plausible explanation for the effects on mothers’ labor market outcomes.

5.1 Mother’s time spent with younger children

Time is a key parental investment. Caucutt et al. (2020) shows that over two-thirds of family spending on child development for children under 12 is made through parental time investments. More time spent with children improves their cognitive and non-cognitive abilities (Del Boca et al. (2014), Attanasio et al. (2020), Milovanska-Farrington (2021)), and less time with children is associated with the opposite effects (J. Price (2008), Bibler (2020)). In this subsection, I propose that a reduction in time spent with younger children drives the negative results on educational outcomes. A mother has a finite amount of time to dedicate to her children, and a shock that increases childcare costs — such as having to care for the focal children who couldn’t start first grade that year — forces her to distribute her time between the two children, benefiting the focal student at the expense of the younger one.

To explore this hypothesis, I divide my sample into two groups: mothers who are more likely to stay at home because of this shock and could be exposed to this mechanism, and those who cannot. I expect the time mechanism to be more pronounced in families where mothers stay at home compared to those where they do not. The best proxy for creating these two groups is to divide the sample between mothers who are the head of the household and those who are not. I assume that a mother who is the head of the household cannot afford to stay at home since she is the primary breadwinner. Despite this status being self-reported in the Social Protection Form, it is unlikely that a mother would call herself the head of the household if she were not, since Chile has very traditional norms regarding gender equality (OECD (2023)). What’s more, the Ministry of Social Development (MDS) defines “The term [female] *Head of Household* refers to a woman who is economically active, has family responsibilities, and is the primary financial provider for the household.” (MDS (2007)).

How the increase in childcare responsibilities affects mothers and their children is illustrated in Figure 17. Section (A) shows that if a mother is the head of the household (HH), regardless of the date of birth of her focal children, she cannot afford to stay at home. Consequently, in comparison

to families not subject to this shock (i.e., where the focal child is born before the cutoff), the amount of time spent with their children will not change. On the other hand, if a mother is not HH, she may be pushed out of the job market to take care of the focal children who did not meet the school entry cutoff. Sections (B) and (C) illustrate the effects on children compared to families where the focal child was born before the cutoff. When a mother is HH, she does not leave the job market due to this shock, so there will be no additional time with either child. As a result, if the time mechanism is driving the positive test scores in focal siblings and negative test scores in younger siblings, and we assume that that mechanism is restricted here, we should expect younger and focal siblings to present results closer to the children born before the cutoff. In other words, younger siblings would show less negative results, and focal siblings would show less positive results.

In contrast, mothers who are not HH and who stay at home as a result of this shock will reduce the time spent with the younger sibling to allocate more time to the focal child, who must stay at home for an extra year. Therefore, the additional time given to the focal child will likely increase their test scores, and the reduced time experienced by the younger sibling will magnify the gap compared to children of families where the focal child was born before the cutoff. That is to say, I expect the effects on these children to be more negative for younger siblings and more positive for focal siblings.

Table 17: Suggestive mechanism of mother’s time effect on younger children performance

	Effects on children with respect families focal born before cutoff					
	(A) Mothers		(B) Time with mother		(C) Expected effects on test scores	
	Can afford to stay at home?	Change time with children?	Younger sibling	Focal sibling	Younger sibling	Focal Sibling
Head of the household (HH)	No	No change	No change	No change	Less negative	Less positive
Not head of the household (No HH)	Maybe	More time	Less time	More time	More negative	More positive

Tables 18 and 19 show the effects on test scores by mothers’ HH status for focal children and siblings, respectively. Although the differences are not statistically significant, they illustrate the mechanisms mentioned earlier. With respect to the focal student, Table 18 shows that the point estimates for focal students when the mother is the head of the household are smaller (columns (1)-(3) present the effects for the pooled sample as a benchmark). Similarly, the effects are larger when the mother is not the head of the household. Table 19 follows this pattern as well: the effects for younger siblings are attenuated (closer to zero) when the mother is the head of the household, whereas the effects are more negative when she is not.

Overall, while the evidence is not conclusive, the hypothesis that a mother’s time spent with children affects their school performance remains compelling and could benefit from further research.

Table 18: Effects of SSA cutoffs on focal student test scores by moms' head of the household status

	Pooled			Mom head of the household			Mom not head of the household		
	(1) Grade 4	(2) Grade 8	(3) Grade 10	(4) Grade 4	(5) Grade 8	(6) Grade 10	(7) Grade 4	(8) Grade 8	(9) Grade 10
Born after school-entry cutoff	0.191*** (0.008)	0.094*** (0.011)	0.097*** (0.009)	0.179*** (0.008)	0.080*** (0.014)	0.090*** (0.012)	0.214*** (0.010)	0.111*** (0.018)	0.105*** (0.013)
Mean before cutoff	-0.159	-0.139	-0.143	-0.166	-0.148	-0.144	-0.181	-0.157	-0.175
Bandwidth	60	42	74	58.55	65.88	66.19	63.75	40.43	80.03
Observations	328,071	128,658	300,664	186,797	118,037	151,063	131,259	47,570	127,038

Note: All estimations include cohort fixed effects, year fixed effect and control for student gender, class size, school rurality and type of school (public or charter). Robust standard errors (in parentheses) are clustered by the distance in days to the cutoff. ***p<0.01, **p<0.05, *p<0.1

Table 19: Sibling Spillovers in Test Scores by Household Head Status

	Pooled				Mom is head of the household				Mom is NOT head of the household			
	(1) Grade 4	(2) Grade 6	(3) Grade 8	(4) Grade 10	(5) Grade 4	(6) Grade 6	(7) Grade 8	(8) Grade 10	(9) Grade 4	(10) Grade 6	(11) Grade 8	(12) Grade 10
Panel A: Spillovers to younger siblings												
Focal child born after school-entry cutoff	-0.044*** (0.017)	-0.048* (0.026)	-0.069** (0.029)	-0.047 (0.030)	-0.040* (0.021)	-0.030 (0.034)	-0.032 (0.036)	-0.047 (0.038)	-0.060* (0.034)	-0.057 (0.047)	-0.154*** (0.052)	-0.051 (0.054)
Mean before the cutoff	-0.196	-0.213	-0.163	-0.123	-0.205	-0.230	-0.119	-0.119	-0.181	-0.188	-0.157	-0.124
Bandwidth	31.6	31.52	27.42	28.82	37.4	30.4	26.4	31.9	32.5	37.8	31.4	31.9
Observations	42,535	20,637	23,116	23,449	31,229	12,305	13,370	17,220	17,009	9,664	7,885	10,572
Panel B: Spillovers to older siblings												
Focal child born after school-entry cutoff	-0.017 (0.029)		0.004 (0.031)	0.039 (0.033)	0.004 (0.030)		-0.011 (0.035)	0.111*** (0.037)	-0.094* (0.054)		-0.012 (0.058)	-0.053 (0.057)
Mean before the cutoff	-0.162		-0.133	-0.131	-0.16		-0.117	-0.126	-0.172		-0.157	-0.129
Bandwidth	25.7		24.25	19	37		36.9	18.27	22.5		24.11	23.9
Observations	25,096		16,431	15,885	21,494		9,385	8,619	8,890		6,687	8,133

Notes: The dependent variable is each test score from the SIMCE exams taken by students in Grades 4, 6, 8, and 10. Since the specific exam a student takes depends on their grade and the year, I excluded the Grade 6 SIMCE exam from the estimation in Panel B because almost no older siblings took it. The sample in Panel A includes all children for whom we observe two adjacent siblings, with at least one SIMCE test score for the younger sibling, where the older sibling is the focal child. In Panel B, the sample includes all children for whom we observe two adjacent siblings, with at least one SIMCE test score for the older sibling, where the younger sibling is the focal child. Each column represents one observation per individual. If a student took the same test more than once (e.g., due to grade repetition), I retained only the first attempt. Unlike previous tables of sibling spillovers, I reported only the results that include both sibling and focal student controls to avoid visual clutter. Columns (1) to (4) show the effects for all students with head of household information, which may differ from 8 for this reason. Columns (5) to (8) show the results for siblings whose mother is the head of the household, while columns (9) to (12) show the results for siblings whose mother is not the head of the household. Robust standard errors are clustered by the running variable at the daily level. *** p<0.01, ** p<0.05, * p<0.1.

5.2 Parental responses to children's ability

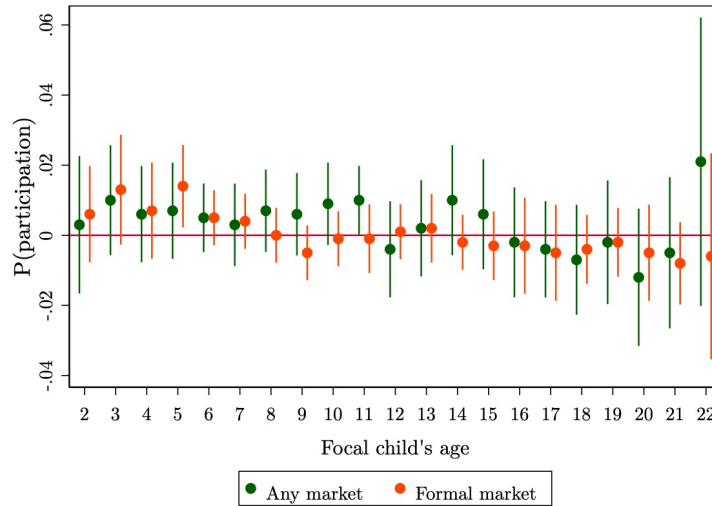
An alternative explanation for the negative spillovers observed in younger siblings comes from the idea that parental involvement varies among children within the same family (G. S. Becker (1992), G. S. Becker and Tomes (1976), Behrman et al. (1982), Griliches (1979)), with G. Becker (1975) suggesting that parents tend to invest more in the human capital of the more able child. In this paper, I find that a later school starting age has positive in-school effects for focal children but negative spillover effects for their younger siblings. If parents adopt a reinforcement approach (as opposed to compensating for differences in siblings' abilities), their investment may respond to differences in school performance, reinforcing the focal children because they perform better academically. While I cannot test this hypothesis directly, there is evidence from Celhay and Gallegos (2022) indicating that parents of students born after the cutoff have higher educational

expectations than those who start school younger. (It is important to note that the authors compare different students—those born before and after the cutoff—rather than siblings.) Moreover, Wang et al. (2022) show that parents’ beliefs about their children’s abilities are upwardly biased. If parents follow a reinforcement strategy and hold these biased beliefs, they may overinvest in the “more able” children, perpetuating the differences throughout the children’s entire schooling, as evidenced in this study.

5.3 Grandmother’s help with children

Having identified some potential mechanisms explaining the effects on younger siblings, I will now turn to a broader analysis of what might be explaining the effects on mothers’ labor market participation. A plausible hypothesis is that grandmothers are helping with childrearing. To test this, I look at grandmothers’ labor market participation in both the formal and informal markets. If grandmothers were helping some mothers and not others — for example, mothers of focal students born before the cutoff and not the others — I would expect those grandmothers to leave the labor market around the time their focal grandchildren reach school-starting age. Since the economic evidence shows that grandmothers quit working to help their daughters raise their children (see Karademir et al. (2024), Gørtz et al. (2023)), I should observe something similar in the data. Figure 9 shows that this is not the case. In short, this subsection demonstrates that grandmother help with childrearing is not different between families of focal children born before or after the cutoff.

Figure 9: Effects on labor market participation maternal grandmother



Notes: I use a local linear regression with the optimal bandwidth from the robust data-driven method proposed by Calonico et al. (2014) and a triangular kernel to estimate the reduced-form results based on Equation 1. The dependent variable in this figure is participation in any market (deep green) and formal market (orange), conditional on the student’s age between 2 and 22. All estimations include cohort-fixed effects, year-fixed effects, and controls for student gender, class size, school rurality, and school type (public or charter). Robust standard errors are clustered by the distance in days to the cutoff. Participation in any labor market comes from the Social Registry of Households, and participation in the formal market comes from the Chilean Pension Supervisor from 2014 to 2022.

6 Conclusion

This study provides new insights into the impact of increased childcare responsibilities on both mothers and their children. While previous literature has focused on the financial and time costs of parenting, this research adds to the understanding by showing how an unexpected extension of caregiving—specifically due to delayed school entry—affects not only mothers’ labor market participation but also the academic performance of their younger children. The findings indicate that mothers reduce their labor supply and experience lower income when their children are required to stay home for an additional year before starting school. More importantly, this additional caregiving burden has long-term negative effects on younger siblings’ educational outcomes, including lower GPA and test scores, as well as a reduced likelihood of post-secondary enrollment.

This study highlights the critical role of maternal time as a mechanism driving these outcomes. When mothers are not the head of the household, and thus more likely to stay home, the negative effects on younger siblings are more pronounced. In contrast, these effects are mitigated when the mother is the primary breadwinner. These results suggest that the lack of formal preschool care or societal pressures for mothers to take on additional caregiving responsibilities may contribute to these adverse outcomes.

This research makes significant contributions to three key areas of literature: the child penalty, childcare, and child development. By demonstrating that childcare burdens extend beyond the early years and affect the entire family, it underscores the need for policies that support working mothers and provide more equitable access to childcare. Furthermore, the study reinforces the importance of time spent with children as a critical factor in their long-term success, making a compelling case for further exploration of how family dynamics shape educational and labor market outcomes.

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A Appendix

A.1 Empirical Cutoffs

I follow the steps below to identify the cutoff that each school faces each year:

1. I keep only 1st graders attending regular schools, excluding those in special education and adult schools.
2. I retain only students who turn 5, 6, 7, or 8 years old during the academic year in analysis. This group accounts for 99.1% of the 1st-grade students in this study.
For example, in the 2002 academic year, I keep students born between 1994 and 1997.
3. For each *school* and *academic year*:
 - (a) I calculate the total number of students.
For example, 150 students started 1st grade in school $id = 2q4e$ in 2002.
 - (b) I compute the total number of students born in the 48-month-year pairs.
For example, in 2002, the first month-year pair is Jan-94, and the 48th is Dec-97.
For example, in school $id = 2q4e$ in 2002, one student was born in Jan-94, 10 in May-95, 3 in Jan-96, and one in Feb-97.
 - (c) I calculate the proportion of students born in each month-year pair.
For example, from school $id = 2q4e$ in 2002, the proportion of students born in Jan-94 is 0.007, in May-95 is 0.067, in Jan-96 is 0.02, and 0.007 in Feb-97.
 - (d) I identify the highest proportion of students born in the sum of 12 consecutive months. Since I consider four birth cohorts (those turning 5, 6, 7, and 8), I calculate a total of 37 sums of 12 consecutive months.
For example, in 2002, the first value for the sum of 12 consecutive months represents the total proportion of students born between Jan-1994 and Dec-1994. The second proportion covers those born between Feb-1994 and Jan-1995, and so on.
Note: If the proportion of students born in any month-year pair within these 12 consecutive months is zero, I replace the total sum for that period with a missing value.
 - (e) I set the empirical cutoff for that school as the month with the highest total proportion. In practice, I recode this as the first day of the following month for simplicity.
Note: In 8% of schools, more than one maximum proportion occurs. In such cases, Stata selects the first maximum by default and assigns it as the cutoff date.
 - (f) I determine the cutoffs for students who turn 6 the year they start 1st grade using this formula. The same cutoff applies to students turning 7.
 - (g) If the empirical cutoff for a school year is a missing value because the proportion of students born in any month-year pair is zero, I assign July 1st as the cutoff for that school for that academic year.

A.2 Sibling's spillovers

Table A.1: Test Scores: Spillovers to Siblings by Grade

	Grade 4			Grade 6			Grade 8			Grade 10		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Spillovers to younger siblings												
Focal child born after school-entry cutoff	-0.027* (0.016)	-0.025* (0.015)	-0.041*** (0.016)	-0.051** (0.025)	-0.048** (0.024)	-0.058** (0.025)	-0.071*** (0.0267)	-0.066** (0.0287)	-0.077*** (0.029)	-0.061** (0.026)	-0.061** (0.026)	-0.050* (0.026)
Mean before the cutoff	-0.184	-0.183	-0.184	-0.198	-0.198	-0.145	-0.146	-0.145	-0.106	-0.104	-0.106	-0.106
Bandwidth	32	33	33	30	30	27	28	26	28	27	28	28
Observations	43,753	45,226	46,646	20,612	20,612	22,014	22,985	22,985	23,318	24,215	24,215	24,215
Sibling controls		X	X		X	X		X	X		X	X
Focal controls			X			X			X			X
Panel B: Spillovers to older siblings												
Focal child born after school-entry cutoff	-0.002 (0.031)	0.000 (0.030)	-0.032 (0.030)	-	-	-	0.036 (0.035)	0.034 (0.035)	-0.010 (0.034)	0.062* (0.036)	0.063* (0.036)	0.037 (0.036)
Mean before the cutoff	-0.14	-0.142	-0.142	-	-	-	-0.19	-0.119	-0.119	-0.113	-0.113	-0.117
Bandwidth	28	27	27	-	-	-	22	26	26	19	19	19
Observations	28,021	26,954	26,954	-	-	-	17,668	17,668	16,440	16,440	15,522	15,522
Sibling controls		X	X					X	X		X	X
Focal controls			X						X			X

Notes: I use local linear regression with the optimal bandwidth from the robust data-driven method proposed by Calonico et al. (2014) and a triangular kernel to estimate the reduced-form results based on Equation 1. The dependent variable is each test score from the SIMCE exams taken by students in Grades 4, 6, 8, and 10. Because the specific exam a student takes depends on the year and grade they are in, no older siblings took the Grade 6 SIMCE exam, so I excluded that estimation in Panel B. The sample in Panel A includes all children for whom we observe two adjacent siblings, with at least one SIMCE test score for the younger sibling, where the older sibling is the focal child. The sample in Panel B includes all children for whom we observe two adjacent siblings, with at least one SIMCE test score for the older sibling, where the younger sibling is the focal child. There is one observation per individual in all columns. If a student took the same test more than once (e.g., due to grade repetition), I kept only the first attempt. Columns 1, 4, 7, and 10 do not include any controls. Columns 2, 5, 8, and 11 control for individual covariates of the child for whom I measure the outcome. Columns 3, 6, 9, and 10 further include individual controls of the focal child from the pair. Own controls are an indicator for female student, indicators for birth year, and indicators for birth month. Sibling controls are indicators for sibling school starting cohorts (cutoff-cohort level with a single indicator for both children born before and after cutoff in a given cohort, for example, 2002), indicator for sibling being a female, and age difference between siblings in years. Robust standard errors are clustered by the running variable at the daily level. *** p<0.01, ** p<0.05, * p<0.1.

A.3 Focal student

A.3.1 Teenage parenthood

Table A.2: Effects of school-entry cutoffs on teenage parenthood across different bandwidths

	Bandwidth				
	(1) Optimal	(2) 30	(3) 20	(4) 10	(5) 5
Panel A: All students					
Focal child born after school-entry cutoff	-0.009*** (0.003)	-0.008*** (0.004)	-0.009*** (0.005)	-0.016*** (0.006)	-0.031*** (0.004)
Mean before cutoff	0.115	0.114	0.114	0.113	0.113
Effect Size	-0.078	-0.070	-0.079	-0.142	-0.274
Observations	228,634	117,565	77,421	37,347	17,287
Panel B: Female students					
Focal child born after school-entry cutoff	-0.009* (0.005)	-0.005 (0.009)	-0.024* (0.014)	-0.062*** (0.008)	-0.057*** (0.006)
Mean before cutoff	0.157	0.161	0.16	0.156	0.156
Effect Size	-0.057	-0.031	-0.150	-0.397	-0.365
Observations	222,384	57,767	38,223	18,405	8,513
Panel C: Male students					
Focal child born after school-entry cutoff	-0.009** (0.004)	-0.011** (0.005)	-0.009 (0.007)	-0.007 (0.011)	-0.004 (0.013)
Mean before cutoff	0.070	0.069	0.070	0.072	0.072
Effect Size	-0.129	-0.159	-0.129	-0.097	-0.056
Observations	112,202	57,978	39,198	18,942	8,774

Note: Optimal bandwidth for All students is 60.5, Females 74.2, and Males 57.3. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.3: Effects of school-entry cutoffs on teenage parenthood across different bandwidths using Local Linear Regression

	Bandwidth				
	(1) 30	(2) 20	(3) 10	(4) 5	(5)
Panel A: All students					
Focal child born after school-entry cutoff	-0.007*** (0.002)	-0.009** (0.004)	-0.010** (0.005)	-0.012* (0.007)	-0.027*** (0.008)
Mean before cutoff	0.11	0.11	0.11	0.11	0.11
Effect Size	-0.06	-0.08	-0.09	-0.11	-0.24
Effective observations	366,855	183,104	122,726	62,178	31,974
Panel B: Female students					
Focal child born after school-entry cutoff	-0.005 (0.003)	-0.006 (0.006)	-0.008 (0.008)	-0.018* (0.010)	-0.042*** (0.008)
Mean before cutoff	0.16	0.16	0.15	0.15	0.15
Effect Size	-0.03	-0.04	-0.05	-0.12	-0.28
Effective observations	222,384	89,899	60,460	30,570	15,632
Panel C: Male students					
Focal child born after school-entry cutoff	-0.007*** (0.002)	-0.011*** (0.003)	-0.011** (0.004)	-0.006 (0.007)	-0.012 (0.013)
Mean before cutoff	0.07	0.07	0.07	0.07	0.07
Effect Size	-0.10	-0.16	-0.16	-0.08	-0.17
Effective observations	177,671	93,205	62,266	31,608	16,342

Note: All estimations include cohort fixed effects, year fixed effect and control for student gender, class size, school rurality, and type of school (public or charter). Robust standard errors (in parentheses) are clustered by the distance in days to the cutoff. Optimal bandwidth for All students is 60.5, Females 74.2, and Males 57.3. *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Two-Stage Regression Results: Minimum School Starting Age and School Starting Age

	All		
	All (1)	Females (2)	Males (3)
First Stage			
Minimum school starting age	0.728*** (0.011)	0.745*** (0.011)	0.711*** (0.012)
Second Stage			
School starting age	-0.004*** (0.000)	-0.005*** (0.000)	-0.002*** (0.000)
Mean sample	0.104	0.146	0.063
Effect Size	-0.0340	-0.0310	-0.0390
F-statistic	4,325	5,019	3,617
Observations	1,138,381	557,167	581,214

Note: All estimations include cohort fixed effects, year fixed effect, and control for student gender, class size, school rurality, and type of school (public or charter). Robust standard errors (in parentheses) are clustered by the distance in days to the cutoff. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.