When Rankings Reward Age: College Access and the Maturity Advantage

Cristhian Molina*

April 3, 2026

[Preliminary draft, do not cite]

[Click Here for the Latest Version]

College admissions formulas determine which student characteristics are rewarded, but their interaction with pre-existing demographic advantages has received little attention. I study how Chile's 2013 ranking reform, which shifted admissions weight from absolute GPA toward within-school class rank, amplified the advantage that relatively older students hold over their younger classroom peers. Because older students earn higher grades due to a well-documented maturity advantage, they also rank higher within their school, and the reform increased the return to this relative position. Using a difference-in-discontinuities design, I find that the reform widened the application gap at the July 1 school entry cutoff by 3.8 percentage points (12 percent). The effect is concentrated among first-generation college aspirants from secondary-educated, higher-income families. However, marginal applicants are less likely to be admitted or to enroll, resulting in no net gain in college enrollment. The findings show that admissions formula design can amplify demographic advantages in college access even when the reform is motivated by equity objectives.

Keywords: School starting age, college admissions, relative age effect, education policy
JEL: I21, I23, J01, J13

*Department of Economics, University of Illinois at Urbana-Champaign. cmolina3@illinois.edu

1 Introduction

Two students sit in the same classroom, take the same courses, and graduate from the same school. They are, in almost every observable dimension, identical. Yet because they were born two days apart, one on each side of a school entry cutoff, the older student entered first grade a year later and spent an extra year developing before beginning formal schooling. By twelfth grade, that maturity advantage has translated into slightly higher grades (Bedard and Dhuey, 2006; Elder and Lubotsky, 2009). Whether this advantage matters for college access depends on how the admissions formula is designed. A formula that rewards absolute performance treats both students' grades the same way. A formula that rewards relative position within the school amplifies the gap, because the older student ranks higher among the very peers whose grades reflect the same curriculum and the same teachers. The difference is not in who has higher grades. It is in what those grades buy.

This paper studies whether admissions formula design can amplify the well-documented age-at-entry advantage in academic performance into an advantage in college access. While a large literature has established that school starting age affects grades, test scores, and educational attainment (Angrist and Krueger, 1992; Bedard and Dhuey, 2006; Black et al., 2011; Celhay and Gallegos, 2025; Elder and Lubotsky, 2009; Fredriksson and Öckert, 2014; McEwan and Shapiro, 2008), and that the advantage can persist into college access (Matta et al., 2016), less is known about how institutional features of the college admissions process mediate these effects. Admissions formulas translate academic performance into admission chances, and the specific way they do so determines which student characteristics are rewarded. If a formula change increases the weight on a dimension where older students hold an advantage, the formula amplifies that advantage even if the designer intended no such effect.

Chile's 2013 ranking reform provides a direct test of this mechanism. Before the reform, college admissions scores were computed from absolute GPA and standardized entrance exam scores, both compared against the national distribution of applicants. In 2013, the Council of Rectors introduced a within-school class ranking component (*puntaje ranking*), requiring programs to assign at least 10 percent of their admissions weight to a student's relative position in the GPA distribution of their own school. Because older students within a cohort tend to earn higher grades than their younger peers (McEwan and Shapiro, 2008), they also rank higher within their school. The reform created a direct, mechanical channel through which the age-at-entry advantage in grades feeds into the admissions formula. I exploit the timing of this reform using a difference-in-discontinuities design (Grembi et al., 2016), comparing the magnitude of the birthday discontinuity in college outcomes before and after 2013 at the July 1 school entry cutoff.

The ranking channel operates through academic performance. After the reform, older students near the July 1 cutoff earned significantly higher grades relative to their younger peers ($\hat{\delta} = 0.082$, $p = 0.037$), consistent with the ranking score increasing the return to within-school performance. This translated into a 3.8 percentage point increase in the application gap at the cutoff, equivalent to 12 percent of the pre-reform application rate. These additional applications, however, did not produce additional enrollments. Conditional enrollment among applicants fell by 7.0 percentage points, driven by marginal applicants who both target programs beyond their competitive reach and are more likely to give up enrollment when admitted. The net effect on unconditional enrollment is zero. The reform expanded who applies without expanding who enrolls.

The amplification is not uniform across families. Students from secondary-educated, higher-income households, with sufficient resources and information to act on a new signal, exhibit the largest response ($\hat{\delta} = 0.113$, $p = 0.006$). Before the reform, older students in this group were no more likely to apply than their younger peers. After the reform, they were substantially more likely to do so. College-educated families show no response because their children apply regardless. Primary-educated families show no response because the ranking signal alone does not overcome the larger barriers they face. The reform matters most at the margin of college participation.

This paper contributes to several literatures. A growing body of work studies the consequences of admissions formulas for equity and sorting (Concha-Arriagada, 2023; Cullen et al., 2013; Larroucau et al., 2015; Niu and Tienda, 2010), focusing on how formulas redistribute access across schools or socioeconomic groups. This paper shows that formula changes can also interact with characteristics the designer did not intend to reward, producing amplification rather than redistribution. The finding connects two literatures that have developed independently. The school starting age literature (Bedard and Dhuey, 2006; Black et al., 2011; Celhay and Gallegos, 2025; Fredriksson and Öckert, 2014; McEwan and Shapiro, 2008) documents that birth timing affects academic performance but has not examined how admissions institutions mediate this effect downstream. The ordinal rank literature

(Denning et al., 2023; Elsner and Isphording, 2017; Murphy and Weinhardt, 2020) shows that within-school rank shapes educational investment and long-run outcomes, but has studied rank primarily as an informal psychological mechanism operating through peer comparison and self-concept. Chile's reform formalizes rank into a policy instrument, and the results show that this formalization creates distributional consequences that the informal channel does not. Finally, the three-margin decomposition of application, admission, and enrollment reveals that a reform can expand who applies to college without expanding who enrolls. A growing literature on college application behavior documents that small changes in costs or information shift who applies (Hoxby and Avery, 2012; Pallais, 2015), but less attention has been paid to what happens to marginal applicants after they enter the admissions process. The enrollment decomposition highlights these downstream barriers.

The remainder of the paper proceeds as follows. Section 2 describes Chile's school entry rules, the college admissions system, and the 2013 ranking reform. Section 3 presents the data sources and sample construction. Section 4 develops the difference-in-discontinuities design. Section 5 reports the main results, including covariate balance, the enrollment decomposition, and socioeconomic heterogeneity. Section 6 concludes.

2 Institutional Background

2.1 School Entry Rules

Chile operates a mandatory education system spanning first grade through the completion of secondary school (grade 12). Students enroll in *educación básica* (grades 1 through 8) and then *educación media* (grades 9 through 12). Children must turn six years old on or before April 1 to begin first grade that academic year. This rule implies a minimum enrollment age of approximately 5.92 years, calculated as of March 1^{st} of the corresponding year.

Until 2017, however, a Ministry of Education decree allowed schools to adopt later cutoff dates, as late as July 1.[1] In practice, most schools in Chile used July 1 as their operational cutoff, with smaller shares adopting May 1 or June 1 (McEwan and Shapiro, 2008). A child born on July 2 would need

[1]In 2017, Decree 1718 eliminated this flexibility and enforced April 1 as the sole enrollment cutoff for all schools.

to wait an additional year to begin first grade, entering school at approximately age 6.67 rather than 5.67 by March 1^{st} of the corresponding year. This creates a discontinuity of up to one year in school starting age at each cutoff date, with the largest and most widely used discontinuity occurring on July 1. In practice, the observed discontinuity is approximately 0.4 years (Figure 1) because some parents of children born before July 1 choose to delay enrollment voluntarily, narrowing the gap relative to those born after the cutoff who are required to wait. This partial compliance motivates the use of intention-to-treat estimates throughout the paper rather than scaling by enrollment age.

Two features of the Chilean setting make it particularly useful for studying school starting age effects. First, the July 1 cutoff generates the largest first-stage discontinuity in enrollment age among the four cutoffs (approximately 0.4 years), providing sufficient statistical power to detect effects on downstream outcomes such as college applications. Second, Chile's Ministry of Education maintains comprehensive administrative records covering all schools nationally, and the college admissions process operates through a single centralized platform (DEMRE). Together, these allow us to track students throughout their entire educational history, from school enrollment to college application outcomes, using linked individual identifiers.

2.2 College Admissions

Admission to selective universities in Chile is managed through a centralized system coordinated by the Department of Evaluation, Measurement, and Educational Registry (DEMRE). Students can apply to up to nearly 2,000 programs hosted by 41 universities by submitting a rank-ordered list of up to ten preferred programs. Each program computes a composite admission score for every applicant as a weighted average of high school grade point average (NEM, *Notas de Enseñanza Media*), standardized entrance exam scores in mathematics and language, an optional test in either history or science, and, after 2013, a within-school class ranking score. Each program sets its own weights subject to regulatory floors established by the Council of Rectors (CRUCH), which require minimum weights on NEM, mathematics, language, and (after 2013) the ranking score.[2]

[2]The specific regulatory floors are 10 percent for NEM and 10 percent for the ranking score (post-2013). Mathematics and language must each receive positive weight, though the exact minimum varies by year. Every program must assign positive weight to at least one of the optional tests (history or science). See DEMRE's annual admissions guidelines for details.

The entrance exam, known as the *Prueba de Selección Universitaria* (PSU) during our study period, is taken by approximately 95 percent of secondary school graduates each year.[3] Scores are scaled to a distribution with a range of 150 to 850 and a mean and median of 500. After receiving their scores, students submit their preference list, and the centralized system runs a student-proposing deferred acceptance algorithm (DAA) to assign students to programs (Hastings et al., 2013). The algorithm considers each student's composite score for each listed program and matches them to their most preferred program in which their score exceeds the admission threshold. Each preference receives a disposition (selected, not selected, or superseded by a higher-ranked admission), and each student is matched to at most one program.

Once matched, students are expected to enroll in the assigned program. In practice, however, enrollment is not automatic. Some admitted students choose not to matriculate, opting instead for programs outside the centralized system, alternative educational paths, or entry into the labor force. This distinction between admission and enrollment becomes relevant when we examine the conditional enrollment margin in Section 5.3.

2.3 The 2013 Ranking Reform

Before 2013, the admissions formula rewarded high school performance exclusively through NEM, a measure of absolute grade point average. A student's GPA was compared against the full population of test-takers, not against peers within the same school. Under this system, the age-at-entry advantage operated through an indirect channel. Older students tended to earn higher grades due to their relative maturity in the classroom (Bedard and Dhuey, 2006; Black et al., 2011; Elder and Lubotsky, 2009; McEwan and Shapiro, 2008), but this advantage was diluted when compared with the national GPA distribution rather than with same-school peers.

In 2013, the Council of Rectors introduced a new component in the admissions formula called the *puntaje ranking* (ranking score).[4] The ranking score captures a student's relative position within the GPA distribution of graduates from the same school, averaged over the three most recent graduating

[3]The PSU was replaced by the *Prueba de Acceso a la Educación Superior* (PAES) starting in 2022, outside our sample period.

[4]This change was established through Agreement 24/2012 (CRUCH). See Larroucau et al. (2015) for a detailed description of the reform and its implementation.

cohorts. Programs were required to assign at least 10 percent of their composite admissions weight to this new component.

The reform introduced a direct, mechanical channel through which school starting age affects college admissions outcomes. A growing literature documents that within-school ordinal rank has independent effects on educational attainment, human capital investment, and long-run earnings (Denning et al., 2023; Elsner and Isphording, 2017; Murphy and Weinhardt, 2020). Because older students within a cohort tend to earn higher grades than their younger peers, they also rank higher within their school. Before 2013, this within-school advantage translated only into higher absolute GPA (NEM), which was compared nationally. After 2013, the advantage also feeds directly into the ranking score, which is explicitly relative to peers at the same school. The reform, therefore, increased the return to being relatively older within one's school cohort.

Crucially, the ranking score was funded by reallocating weight from other components. Programs reduced the weight assigned to NEM, mathematics, or verbal exam scores to accommodate the new 10 percent minimum for ranking.[5] This reallocation means the reform did not simply add a new dimension to the formula. It shifted the composition of what the formula rewards, tilting it toward within-school relative performance and away from absolute grades and standardized test scores (Boehm and Carril, 2024).

The identification strategy in this paper exploits the timing of this reform. If the age-at-entry advantage in college applications is amplified by the ranking channel, then the discontinuity in application rates at the July 1 birthday cutoff should increase after 2013 relative to the pre-reform period. The next sections describe the data and empirical framework used to test this prediction.

[5] On average, NEM weight fell by approximately 14 percentage points across programs, with smaller reductions in mathematics (3.6 pp) and verbal (3.6 pp) exam weights. See Appendix Table A.5 for the full decomposition.

3 Data

3.1 Sources and Sample Construction

We combine student-level administrative records from two institutions within Chile's Ministry of Education. The first source is the MINEDUC *Rendimiento* panel, which records every student's enrollment, grade level, school, and academic outcomes (pass, fail, transfer) at the end of each academic year. From this panel we observe exact birth dates, gender, year of enrollment in each grade, and school identifiers.

The second source is the DEMRE database, which contains the universe of entrance exam (PSU) registrants from 2004 through 2020. For each test-taker, we observe scores in all four tests, high school GPA (NEM), the ranking score (available from 2013 onward), and whether the student submitted an application to the centralized admissions system. DEMRE also collects a short survey at the time of PSU registration, from which we construct measures of parental education (primary, secondary, or college) and family income (low, middle, or high).

We link the two sources using a unique identification number at the student level. Our estimation sample consists of on-schedule, first-time PSU takers between 2010 and 2015 who attended urban, non-special-needs schools and have non-missing values for the key covariates.[6] The resulting sample contains 1,158,950 students, roughly evenly split between the pre-reform period (2010–2012) and the post-reform period (2013–2015).

We focus on a symmetric three-year window around the 2013 reform, with the post-reform period ending in 2015 to avoid contamination from the 2016 *Gratuidad* reform (free tuition policy), which altered the college application margin through a separate channel.

3.2 Key Variables

Running variable. The running variable is the day of birth within the calendar year, normalized as the distance (in days) from the July 1 cutoff. Students born on or after July 1 (positive values)

[6]On-schedule means the student took the PSU in the same year of their secondary school graduation, in order to enroll into college the following year. We restrict our analysis to first-time takers to avoid confounding due to students who retake the exam after a gap year.

are "treated" in the sense that they are induced to delay first-grade enrollment by one year, entering school at an older age. Following Celhay and Gallegos (2025), we estimate intention-to-treat effects throughout and do not scale our estimates by enrollment age, as doing so would require monotonicity and exclusion restriction assumptions that are difficult to defend in this setting.[7]

Outcomes. Our primary outcome is *college application*, an indicator equal to one if the student submitted at least one application through the centralized admissions system after taking the PSU. We also study *college enrollment*, defined as admission to and enrollment in a program, both unconditionally (among all PSU takers) and conditionally (among applicants only). The three-margin decomposition of application, unconditional enrollment, and conditional enrollment is central to the paper's argument. To investigate the enrollment margin further, we also construct indicators for whether an applicant was admitted to any listed program, whether they were admitted to their first-choice program, and whether they were admitted but chose not to enroll. These variables are derived from the per-preference disposition codes in the DEMRE application records.

GPA. We observe NEM (high school grade point average), normalized within each PSU cohort year to have mean zero and unit standard deviation. NEM captures the absolute academic performance channel that existed before the 2013 reform and serves as a mechanism check in the analysis.

Socioeconomic status. Parental education is classified into three mutually exclusive categories based on the highest level attained by either parent: primary only, secondary, and college. Family income is classified as low or high based on self-reported household income brackets from the DEMRE pre-test survey, where high combines the middle and upper brackets. These categories are predetermined relative to the student's academic outcomes.

[7] Appendix A describes the construction of the enrollment age measure used in Figure 1, which combines directly observed first-grade enrollment records with an approximation following McEwan and Shapiro (2008) for the remainder of the sample.

3.3 Descriptive Statistics

Table 1 presents summary statistics for students within a 15-day window on either side of the July 1 cutoff, separately by period (pre- and post-reform) and by position relative to the cutoff (younger and older at school entry).

Students born after the cutoff, who enter school at an older age, apply to college at higher rates than those born just before the cutoff in both periods. The gap, however, is noticeably larger after the reform (3.2 percentage points versus 2.3 percentage points pre-reform). This widening is consistent with the hypothesis that the introduction of the ranking score amplified the advantage of being relatively older within one's school cohort. Whether this raw difference reflects a causal effect of the reform, rather than sustained trends in college access, is the question the empirical strategy addresses.

Enrollment rates are also higher among older students in both periods. Normalized GPA follows the expected pattern in the literature on school starting age effects, with older students scoring slightly higher than younger students (0.054 and 0.076 standard deviations in the pre- and post-reform periods, respectively), consistent with a persistent maturity advantage in academic performance (Bedard and Dhuey, 2006; McEwan and Shapiro, 2008).

Predetermined covariates such as gender, parental education, and family income appear similar across the cutoff in both periods, as expected under the assumption that birth dates near July 1 are quasi-randomly assigned. The sample is roughly balanced by gender (48 percent male), approximately half of the students have parents with secondary education, and about a quarter have parents with college education.

4 Empirical Strategy

4.1 Difference-in-Discontinuities Design

The school entry cutoff at July 1 creates a sharp discontinuity in first-grade enrollment age (Figure 1). Students born on or after July 1 must wait an additional year to begin first grade, entering school approximately 0.4 years older than students born just before the cutoff. As described in Section 2.3, these older students carry a GPA advantage through secondary school that, after 2013, feeds directly into the ranking component of the college admissions formula.

To isolate the amplification caused by the 2013 ranking reform, we adopt a difference-in-discontinuities design following Grembi et al. (2016). The estimator compares the magnitude of the July 1 discontinuity before and after the reform:

$$\hat{\delta} = \hat{\beta}_{post} - \hat{\beta}_{pre} \tag{1}$$

where $\hat{\beta}_{pre}$ and $\hat{\beta}_{post}$ are separate RD estimates obtained from the pre-reform and post-reform subsamples, respectively. Because the two subsamples consist of non-overlapping cohorts, the standard error is computed as:

$$SE(\hat{\delta}) = \sqrt{SE(\hat{\beta}_{pre})^2 + SE(\hat{\beta}_{post})^2} \tag{2}$$

Each period-specific estimate is obtained using the bias-corrected local polynomial estimator of Calonico et al. (2014) with a local linear polynomial ($p = 1$), a triangular kernel, and MSE-optimal bandwidth selection. We report bias-corrected point estimates with robust standard errors throughout, following Cattaneo et al. (2020).[8]

The estimand δ captures the change in the age-at-entry advantage at the July 1 cutoff attributable to the reform. A positive δ for college application would indicate that the introduction of the ranking score widened the gap between older and younger students in their propensity to apply to college.

4.2 Identification

The difference-in-discontinuities design rests on two assumptions (Lee and Lemieux, 2010). The first is the standard continuity condition for regression discontinuity designs: potential outcomes must be continuous at the July 1 cutoff. This would be violated if families systematically manipulate birth timing to place children on the advantaged side of the cutoff. The second is that no other factor changed the magnitude of the July 1 discontinuity at exactly 2013, other than the ranking reform (Grembi et al., 2016).

[8]The conventional RD estimator has asymptotic bias. The bias-corrected estimator removes this bias using a pilot bandwidth, and the robust standard error accounts for the additional variance introduced by the correction. See Calonico et al. (2014) for details.

No manipulation of the running variable. Birth dates are determined well before parents could anticipate school entry cutoff rules with precision. Nevertheless, evidence from Chile shows a correlation between parental socioeconomic status and scheduled C-sections in private hospitals (Borrescio-Higa and Valdés, 2019), raising the possibility that higher-income parents may time births around cutoff dates. We examine the density of births around the July 1 cutoff using the test proposed by McCrary (2008) and find no visual evidence of bunching (Appendix Figure A.1). Moreover, the difference-in-discontinuities design provides additional protection against this concern. Any birth-timing manipulation would need to change differentially at 2013 to bias our estimates. Since the students in our sample were born approximately 17 to 18 years before the reform was announced, differential manipulation in response to the reform is not plausible.

No compositional changes at the threshold. A more direct test of the continuity assumption examines whether predetermined covariates jump discontinuously at the cutoff in a way that changed with the reform. We estimate the RD for each predetermined covariate separately in the pre- and post-reform periods, and compute the difference-in-discontinuities for each. The results of this exercise, presented in Section 5.2, confirm that no covariate exhibits a statistically significant differential change at the threshold.

4.3 Specification Choices

Our baseline specification follows the recommendations of Cattaneo et al. (2020) for regression discontinuity designs. We use a local linear polynomial with MSE-optimal bandwidth selection, a triangular kernel, and heteroskedasticity-consistent (HC1) standard errors. We include a set of predetermined covariates (gender, parental education, family income) to improve precision without affecting the bias-corrected point estimates (Calonico et al., 2014). As robustness checks, we consider alternative polynomial orders, bandwidth selection, and placebo cutoffs at non-July dates. These results are reported in Appendix Section D.

5 Results

5.1 The Reform Amplified College Applications

The ranking reform amplified the age-at-entry advantage in college applications. Table 2 presents RD estimates at the July 1 cutoff for the pre-reform and post-reform periods, along with the difference-in-discontinuities for each outcome. Figure 2 displays these estimates visually.

In the pre-reform period, older students were slightly more likely to apply to college than their younger peers, though the difference is small and not statistically significant ($\hat{\beta}_{pre} = 0.006$, $p = 0.61$). The post-reform period tells a different story. The application advantage for older students increased in magnitude and became highly significant ($\hat{\beta}_{post} = 0.044$, $p < 0.001$). The difference between these two estimates, $\hat{\delta} = 0.038$ ($p = 0.032$), indicates that the reform increased the application gap by 3.8 percentage points, equivalent to a 12 percent increase relative to the pre-reform application rate of 31 percent near the cutoff.

Unconditional enrollment shows a positive but insignificant RD in both periods ($\hat{\beta}_{pre} = 0.014$, $\hat{\beta}_{post} = 0.011$), with a difference-in-discontinuities close to zero ($\hat{\delta} = -0.003$, $p = 0.85$). The reform induced more applications from older students, but these additional applications did not translate into more enrollments on net.

The most striking contrast emerges in conditional enrollment. Among applicants, the pre-reform RD is positive but insignificant ($\hat{\beta}_{pre} = 0.033$), while the post-reform estimate turns negative and marginally significant ($\hat{\beta}_{post} = -0.037$, $p = 0.066$). This sign reversal produces a large and significant difference-in-discontinuities estimate of $\hat{\delta} = -0.070$ ($p = 0.032$), a decline of roughly 13 percent relative to a baseline conditional enrollment rate of 56 percent. The reform brought new applicants to the centralized system, but these marginal applicants were less likely to convert their applications into enrollments.

The next subsections investigate whether these results are driven by compositional changes at the threshold, the mechanisms underlying the decline in conditional enrollment, and whether the amplification in application behavior varies across socioeconomic groups.

5.2 Compositional Changes Do Not Drive the Result

For the application and enrollment results to be attributed to the ranking channel, the reform must have changed the behavior of students near the July 1 cutoff without changing who those students are. If the composition of students near the threshold shifted differentially after the reform, the observed increase in applications could partly reflect selection into the sample rather than a response to the admissions formula.

Figure 3 and Appendix Table A.1 present difference-in-discontinuities estimates for predetermined covariates and academic performance measures. The post-reform period shows some directional patterns in the individual RD estimates. Older students near the cutoff tend to come from slightly higher-income families, and their average PSU scores are marginally higher than in the pre-reform period. These patterns, however, are present in the pre-reform period as well, and the difference-in-discontinuities for each predetermined covariate (gender, parental education, family income, average PSU score) is statistically indistinguishable from zero, with p-values ranging from 0.11 to 0.84. A joint test of the null that all five predetermined covariate differences are simultaneously zero fails to reject ($\chi^2(5) = 5.58$, $p = 0.35$), and individual p-values remain insignificant after Bonferroni correction for multiple testing (minimum adjusted $p = 0.55$). The relevant test for the difference-in-discontinuities design is whether the change in covariate balance coincides with the reform, not whether each period exhibits perfect balance independently (Grembi et al., 2016). Moreover, in our baseline specification, we control for these covariates, absorbing any smooth compositional differences at the threshold (Calonico et al., 2014).

Concha-Arriagada (2023) documents that the ranking reform induced strategic school switching among 12^{th}-grade students seeking to improve their within-school rank. While this behavioral response changed the composition of students at some schools, there is no reason to believe it would differentially affect students born on opposite sides of the July 1 cutoff within the same school, as the switching decision depends on GPA relative to school peers rather than on birth timing.

GPA, unlike the predetermined covariates, is determined during schooling and can respond to the reform through behavioral channels. The difference-in-discontinuities for GPA is positive and statistically significant at the 5% ($\hat{\delta} = 0.082$, $p = 0.037$), indicating that, after the reform, older students earned higher grades than younger students. This is consistent with Fajnzylber et al. (2019), who document GPA inflation as a strategic response to the same ranking reform, and with Cascio and Schanzenbach (2007), who show that age-based advantages interact with peer rank in the classroom. The GPA shift does not threaten identification because GPA is downstream of school starting age and is not predetermined at birth. It reflects the reform increasing the return to high grades through the ranking channel, which is precisely the mechanism the paper studies.

5.3 Why Does Conditional Enrollment Fall?

The negative conditional enrollment result could arise through several channels. Using the detailed application records from DEMRE, we can investigate two of them. Marginal applicants, emboldened by the ranking boost, may target programs beyond their competitive reach and fail to gain admission. Alternatively, students who receive an offer may choose not to enroll, opting instead for alternatives outside the centralized system, such as vocational programs, non-CRUCH universities, or direct entry into the labor force. We refer to these channels as overreach and deterrence, respectively.

To distinguish them, we decompose the conditional enrollment margin into an admission component and a post-admission component. For each applicant, we observe whether the centralized matching algorithm selected them for at least one program in their preference list, and whether they ultimately enrolled. Figure A.3 displays the pre- and post-reform RD coefficients for each decomposition outcome, and Appendix Table A.6 reports the full estimates.

The pre-reform RD for admission to any listed program is slightly positive ($\hat{\beta}_{pre} = 0.022$), indicating that older applicants were somewhat more likely to be admitted before the reform. After the reform, this estimate turns negative ($\hat{\beta}_{post} = -0.024$), yielding a difference-in-discontinuities of -0.046 ($p = 0.103$). The pattern is similar for first-choice admission, where the post-reform estimate shifts from near zero to -0.055, with a difference-in-discontinuities of -0.054 ($p = 0.102$). These estimates sharpen when we narrow the bandwidth to focus on students closest to the cutoff (CER-optimal), with first-choice admission falling by 6.4 percentage points ($p < 0.10$). The quadratic specification attenuates both estimates toward zero, consistent with a local effect concentrated near the threshold.

The deterrence channel also shows directional evidence. Among admitted students, the probability of not enrolling shifted from $\hat{\beta}_{pre} = -0.018$ to $\hat{\beta}_{post} = 0.028$, with a difference-in-discontinuities of $+0.046$ ($p = 0.159$). The sign is stable across bandwidth choices but does not reach conventional significance.

Neither channel individually accounts for the full decline in conditional enrollment. Both point in the expected direction, and together they are consistent with the reform drawing marginal applicants who both aim higher than their competitive position warrants and are somewhat more likely to pursue alternatives when they do receive an offer. This pattern parallels the broader finding that marginal applicants induced into the college pipeline by information or cost changes often face barriers downstream (Hoxby and Avery, 2012; Pallais, 2015).

5.4 Which Families Respond to the Reform?

The amplification of the age-at-entry advantage is not uniform across socioeconomic groups. Table 3 and Figure 4 present difference-in-discontinuities estimates for college application across six cells defined by parental education (primary, secondary, college) and family income (low, high).

Panel A of Table 3 shows that students from primary-educated families exhibit positive but insignificant effects in both income groups ($\hat{\delta} = 0.041$ for low income, $\hat{\delta} = 0.032$ for high income). The ranking signal alone appears insufficient to overcome the larger barriers facing families where neither parent completed secondary school.

Panel B reveals where the reform effect concentrates. Among secondary-educated families, the low-income group shows a positive but insignificant estimate ($\hat{\delta} = 0.043$, $p = 0.22$). The high-income group, however, produces the largest and most precisely estimated effect in the table ($\hat{\delta} = 0.113$, $p = 0.006$). The pre-reform RD for this cell is slightly negative ($\hat{\beta}_{pre} = -0.036$), meaning that before the reform, older students from these families were not more likely to apply. After the reform, the estimate becomes strongly positive ($\hat{\beta}_{post} = 0.077$, $p = 0.004$). This sign reversal drives the difference-in-discontinuities.

Panel C confirms the null result for college-educated families. Both income groups produce estimates close to zero ($\hat{\delta} = -0.002$ for low income, $\hat{\delta} = -0.039$ for high income). These families' children were already likely to apply regardless of the ranking reform. This null finding is consistent with

Zimmerman (2019), who documents that in the Chilean context, the returns to elite college admission accrue primarily to students from advantaged backgrounds who were already on the margin of admission.

This pattern is consistent with the ranking channel operating most strongly among families at the margin of college aspiration, paralleling findings from rank-based admissions policies in other contexts (Cullen et al., 2013; Niu and Tienda, 2010). Students from secondary-educated, higher-income families occupy a specific position in the college access pipeline. Their parents did not attend college, so the student would be a first-generation applicant, but the family has sufficient resources to support the application process. Before the reform, the age-at-entry advantage in GPA existed but was diluted in the national NEM comparison. After the reform, the ranking score made this advantage locally visible within the student's own school, providing a concrete signal that the student is competitive relative to peers.

Figure 4 displays the difference-in-discontinuities estimates for each education-income category. The visual pattern reinforces the statistical result: only the secondary-educated, higher-income cell produces a precisely estimated positive effect, while all other cells straddle zero.

6 Conclusion

Admissions formula design can amplify pre-existing advantages in college access. Chile's 2013 ranking reform shifted weight within the university admissions formula from absolute GPA toward a within-school class ranking component. Because older students earn higher grades than their younger classroom peers, this reallocation increased the return to being relatively older within one's school cohort. Using a difference-in-discontinuities design, this paper shows that the reform widened the application gap at the July 1 school entry cutoff by 3.8 percentage points (12 percent), confirmed by a significant increase in the GPA discontinuity after the reform ($\hat{\delta} = 0.082$, $p = 0.037$). The reform did not change who has higher grades. It changed what those grades buy in the admissions process.

These additional applications did not translate into additional enrollments. Conditional enrollment among applicants fell by 7.0 percentage points, driven by marginal applicants who both overreach in program targeting and are more likely to give up enrollment when admitted. The amplification concentrates among students from secondary-educated, higher-income families ($\hat{\delta} = 0.113$, $p = 0.006$), with no effect among college-educated families whose children apply regardless or among primary-educated families facing larger barriers. The ranking signal matters most for families at the margin of college participation, where it shifts the perceived return to applying.

Three implications follow. First, admissions formulas are not neutral instruments. The ranking reform was designed to reward students who excel relative to their school context, yet it accidentally amplified a demographic advantage rooted in birth timing. Concha-Arriagada (2023) documents a parallel consequence of the same reform, as she finds that strategic school switching eroded approximately 30 percent of the intended redistribution. Formulas based on within-school relative performance appear particularly vulnerable to behavioral responses that undermine their equity objectives. Second, the application margin deserves attention as a distinct outcome in studies of college access. The three-margin decomposition reveals a large effect on applications but no net effect on enrollment, a finding invisible in enrollment-only analyses. Third, admissions reforms are most consequential not for the poorest or the most advantaged families, but for those in between, where a formula change alters the perceived competitiveness of their application.

Several limitations apply. Our sample conditions on PSU test-taking, so we do not observe whether the reform changed who takes the entrance exam. Celhay and Gallegos (2025) document a positive age-at-entry effect on PSU take-up for overlapping Chilean cohorts, suggesting our estimates capture the intensive margin among test-takers rather than the full effect. Because we estimate intention-to-treat effects, our estimates reflect the reduced-form policy impact of the cutoff rather than the per-year effect of delayed enrollment. The three-year window (2010–2015) avoids contamination from Chile's 2016 free tuition reform but limits our ability to study whether the amplification persists as schools and families adapt to the ranking component.

Table 1: Summary Statistics Around the July 1 Cutoff

	Pre-reform			**Post-reform**		
	Younger	Older	Diff	Younger	Older	Diff
Panel A: Outcome variables						
Applied	0.309	0.332	0.023	0.372	0.404	0.032
Enrolled	0.173	0.188	0.014	0.235	0.259	0.024
GPA score	-0.021	0.033	0.054	-0.030	0.045	0.076
Ranking score	—	—	—	-0.030	0.048	0.078
Panel B: Baseline covariates						
Male	0.474	0.485	0.011	0.471	0.476	0.005
Income: middle	0.311	0.313	0.002	0.392	0.397	0.004
Income: high	0.120	0.126	0.006	0.153	0.160	0.008
Educ: secondary	0.482	0.488	0.006	0.505	0.501	-0.004
Educ: college	0.230	0.234	0.004	0.251	0.257	0.006
N	23,457	25,428		22,999	24,819	

Note: Sample consists of on-schedule PSU first-takers in the July window born within ±15 days of the July 1 cutoff. Younger = born before July 1; Older = born on or after July 1. Diff = Older − Younger (unadjusted means). Ranking score shown for post-reform cohorts only (official DEMRE data). The difference-in-discontinuities estimate is recovered via rdrobust in Table 2.

Table 2: Effect of the Ranking Reform on College Outcomes

	(1)	**(2)**	**(3)**
	$\hat{\beta}_{pre}$	$\hat{\beta}_{post}$	**(2) − (1)**
Applied	0.006	0.044***	0.038**
	(0.012)	(0.013)	(0.018)
Enrolled	0.014	0.011	-0.003
	(0.010)	(0.011)	(0.015)
Enrolled (if applied)	0.033	-0.037*	-0.070**
	(0.024)	(0.022)	(0.032)
Covariates	Yes	Yes	
Bandwidth (days)	24	22	
N	300,201	294,671	

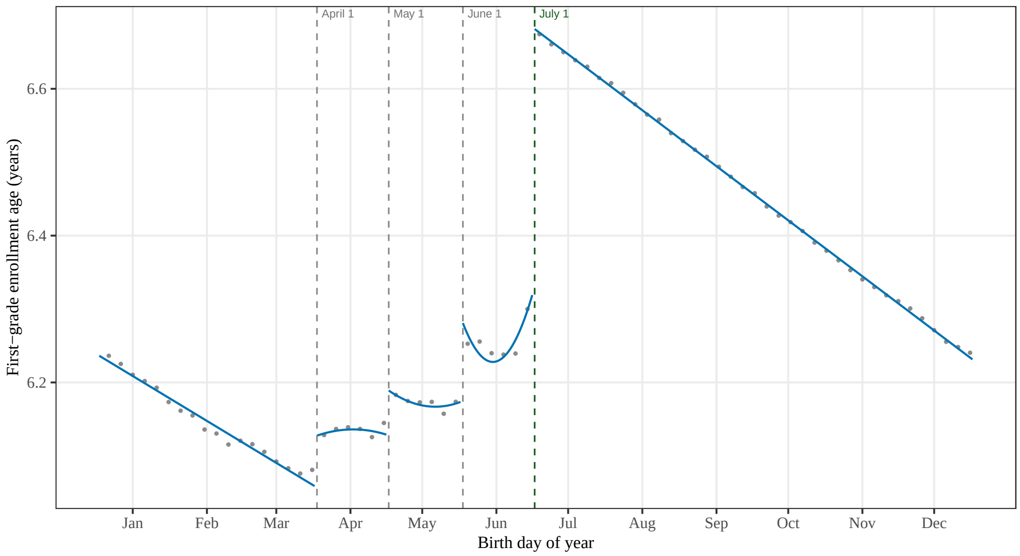
Note: Each row reports RD estimates at the July 1 school entry cutoff for the indicated outcome. Column (1) reports the pre-reform estimate, column (2) the post-reform estimate, and column (3) the difference-in-discontinuities. Enrolled (if applied) is estimated on the applicant subsample. Pre-reform means (younger group, within bandwidth): Applied = 0.309, Enrolled = 0.173, Enrolled (if applied) = 0.558. Covariates include gender, family income, and parental education. Estimator: bias-corrected local polynomial with robust standard errors (Calonico et al., 2014). Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Heterogeneous Reform Effects on College Application by Family Background

	(1)	(2)	(3)
	$\hat{\beta}_{pre}$	$\hat{\beta}_{post}$	(2) − (1)
Panel A: Primary educ. parents			
Low income	-0.019	0.023	0.041
	(0.020)	(0.025)	(0.032)
High income	0.062	0.094**	0.032
	(0.052)	(0.045)	(0.069)
Panel B: Secondary educ. parents			
Low income	0.022	0.065**	0.043
	(0.023)	(0.026)	(0.035)
High income	-0.036	0.077***	0.113***
	(0.031)	(0.027)	(0.041)
Panel C: College educ. parents			
Low income	-0.012	-0.014	-0.002
	(0.066)	(0.075)	(0.100)
High income	0.046	0.008	-0.039
	(0.032)	(0.029)	(0.043)

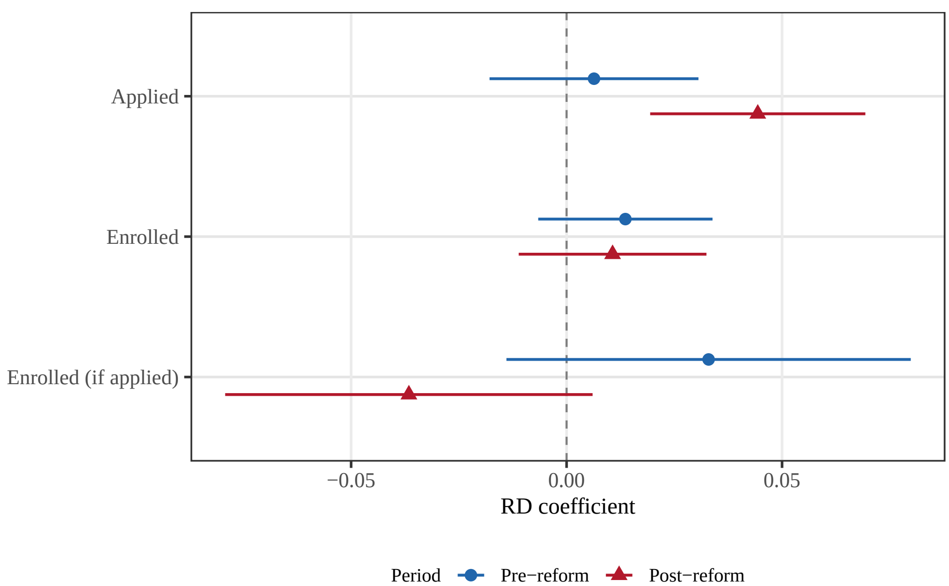
Note: Each row reports difference-in-discontinuities estimates for college application (=1) within the indicated education and income cell. Column (1) reports the pre-reform RD, column (2) the post-reform RD, and column (3) the difference-in-discontinuities. Education groups are mutually exclusive (highest parental attainment). Income is based on self-reported household brackets from the DEMRE survey. Cell sample sizes range from 10,674 to 84,646 (within MSE-optimal bandwidth). Covariate: gender (education and income dummies excluded within each cell). Estimation follows (Calonico et al., 2014). Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: First-stage: school starting age by day of birth



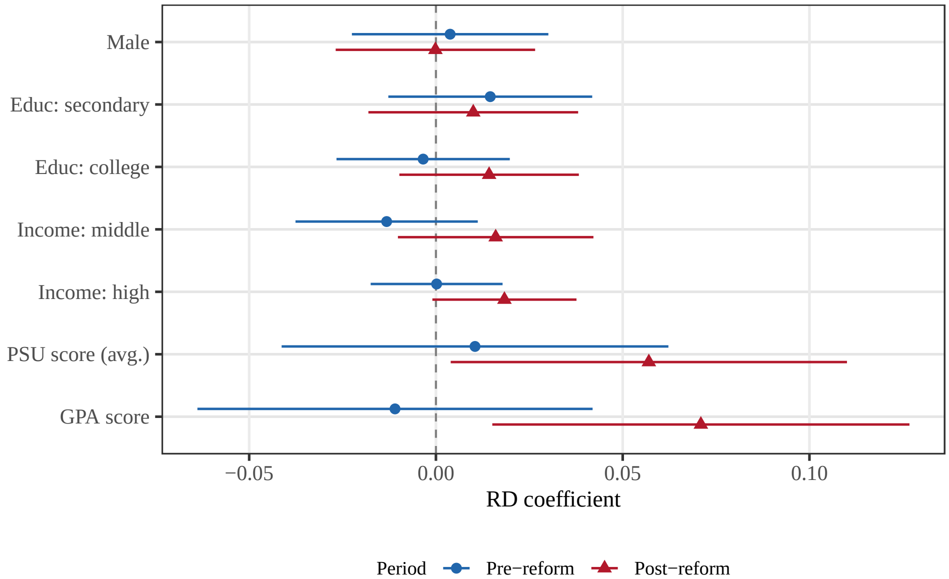
Note: This figure plots the relationship between day of birth in the calendar year and first-grade enrollment age. Each point represents the average enrollment age within a day-of-birth bin. Vertical lines mark the four school entry cutoffs: April 1, May 1, June 1, and July 1. The July 1 cutoff produces the largest discontinuity (approximately 0.4 years).

Figure 2: Pre-reform and post-reform RD estimates for main outcomes



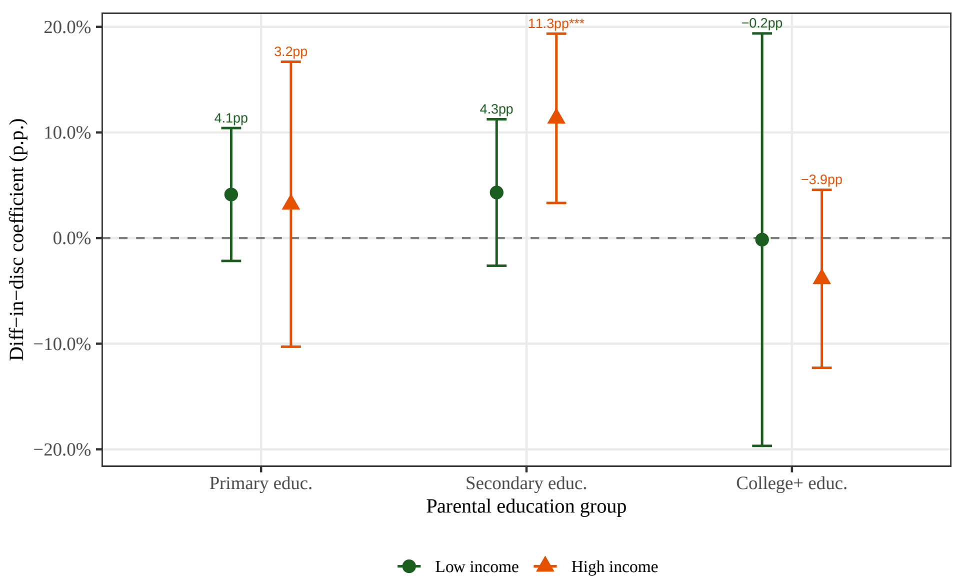
Note: This figure plots RD estimates at the July 1 cutoff for each main outcome, separately for the pre-reform and post-reform periods. College application and unconditional enrollment are estimated on the full sample of PSU takers. Conditional enrollment is estimated on the applicant subsample. Horizontal bars represent 95% confidence intervals. Estimation follows Calonico et al. (2014).

Figure 3: Covariate balance at the July 1 cutoff



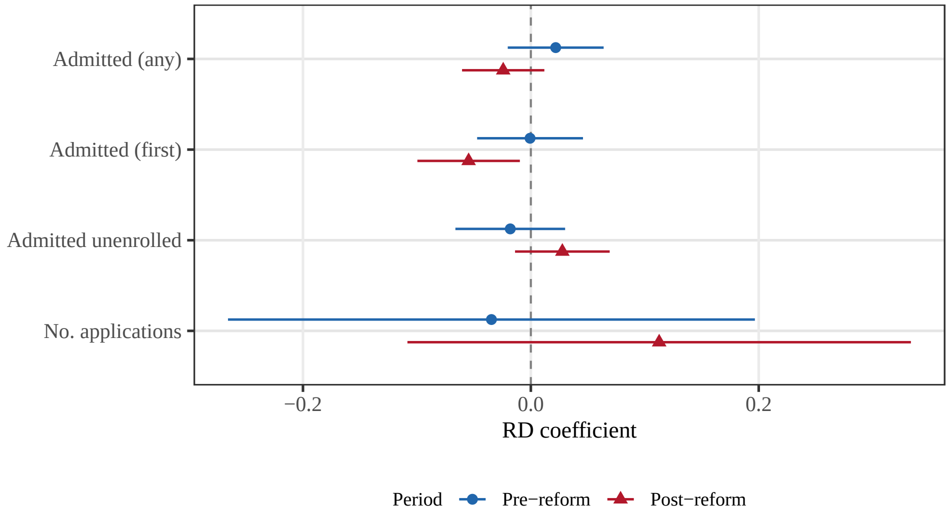
Note: This figure plots RD estimates at the July 1 cutoff for each predetermined covariate, separately for the pre-reform and post-reform periods. Covariates include gender, parental education (secondary, college), family income (middle, high), and normalized PSU scores (math, verbal). Horizontal bars represent 95% confidence intervals. Estimation follows Calonico et al. (2014).

Figure 4: Heterogeneous reform effects on college application by education and income



Note: This figure plots difference-in-discontinuities estimates for college application across six cells defined by parental education (primary, secondary, college) and family income (low, high). Vertical bars represent 95% confidence intervals. RD estimation follows Calonico et al. (2014), difference-in-discontinuities design follows Grembi et al. (2016).

Figure 5: Decomposition of conditional enrollment



Note: This figure plots pre-reform and post-reform RD coefficients at the July 1 cutoff for four outcomes measured among applicants. Horizontal bars represent 95% confidence intervals. Estimation follows Calonico et al. (2014).

References

Angrist, J. D. and Krueger, A. B. (1992). The Effect of Age at School Entry on Educational Attainment: An Application of Instrumental Variables with Moments from Two Samples. *Journal of the American Statistical Association*, 87(418):328–336. Publisher: Taylor & Francis _eprint: https://www.tandfonline.com/doi/pdf/10.1080/01621459.1992.10475212.

Bedard, K. and Dhuey, E. (2006). The Persistence of Early Childhood Maturity: International Evidence of Long-Run Age Effects. *The Quarterly Journal of Economics*, 121(4):1437–1472. Publisher: Oxford University Press.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2011). Too Young to Leave the Nest? The Effects of School Starting Age. *The Review of Economics and Statistics*, 93(2):455–467.

Boehm, E. and Carril, Á. (2024). College Admissions and Universities' Preferences for Students.

Borrescio-Higa, F. and Valdés, N. (2019). Publicly insured caesarean sections in private hospitals: a repeated cross-sectional analysis in Chile. *BMJ Open*, 9(4):e024241. Publisher: British Medical Journal Publishing Group Section: Health economics.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica*, 82(6):2295–2326.

Cascio, E. and Schanzenbach, D. W. (2007). First in the Class? Age and the Education Production Function.

Cattaneo, M. D., Titiunik, R., and Vazquez-Bare, G. (2020). Analysis of regression-discontinuity designs with multiple cutoffs or multiple scores. *The Stata Journal*, 20(4):866–891. Publisher: SAGE Publications.

Celhay, P. and Gallegos, S. (2025). Early Skill Effects on Parental Beliefs, Investments, and Children's Long-Run Outcomes. *Journal of Human Resources*, 60(2):371–399.

Concha-Arriagada, C. (2023). Should I Stay, or Should I Go? Strategic Responses to Improve College Admission Chances.

Cullen, J. B., Long, M. C., and Reback, R. (2013). Jockeying for position: Strategic high school choice under Texas' top ten percent plan. *Journal of Public Economics*, 97:32–48.

Denning, J. T., Murphy, R., and Weinhardt, F. (2023). Class Rank and Long-Run Outcomes. *The Review of Economics and Statistics*, 105(6):1426–1441.

Elder, T. E. and Lubotsky, D. H. (2009). Kindergarten Entrance Age and Children's Achievement: Impacts of State Policies, Family Background, and Peers. *Journal of Human Resources*, 44(3):641–683. Publisher: University of Wisconsin Press Section: Articles.

Elsner, B. and Isphording, I. E. (2017). A Big Fish in a Small Pond: Ability Rank and Human Capital Investment. *Journal of Labor Economics*, 35(3):787–828.

Fajnzylber, E., Lara, B., and León, T. (2019). Increased learning or GPA inflation? Evidence from GPA-based university admission in Chile. *Economics of Education Review*, 72:147–165.

Fredriksson, P. and Öckert, B. (2014). Life-cycle Effects of Age at School Start. *The Economic Journal*, 124(579):977–1004. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/ecoj.12047.

Grembi, V., Nannicini, T., and Troiano, U. (2016). Do Fiscal Rules Matter? *American Economic Journal: Applied Economics*, 8(3):1–30.

Hastings, J. S., Neilson, C. A., and Zimmerman, S. D. (2013). Are Some Degrees Worth More than Others? Evidence from college admission cutoffs in Chile.

Hoxby, C. M. and Avery, C. (2012). The Missing "One-Offs": The Hidden Supply of High-Achieving, Low Income Students.

Larroucau, T., Rios, I., and Mizala, A. (2015). The Effect of Including High School Grade Rankings in the Admission Process for Chilean Universities. *Pensamiento Educativo: Revista de Investigación Educacional Latinoamericana*, 52:95–118.

Lee, D. S. and Lemieux, T. (2010). Regression Discontinuity Designs in Economics. *Journal of Economic Literature*, 48(2):281–355.

Matta, R., Ribas, R. P., Sampaio, B., and Sampaio, G. R. (2016). The effect of age at school entry on college admission and earnings: a regression-discontinuity approach. *IZA Journal of Labor Economics*, 5(1):9.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, 142(2):698–714.

McEwan, P. and Shapiro, J. (2008). The Benefits of Delayed Primary School Enrollment: Discontinuity Estimates Using Exact Birth Dates. *Journal of Human Resources*, 43.

Murphy, R. and Weinhardt, F. (2020). Top of the class: The importance of ordinal rank. *The Review of Economic Studies*, 87(6):2777–2826.

Niu, S. X. and Tienda, M. (2010). The Impact of the Texas Top 10 Percent Law on College Enrollment: A Regression Discontinuity Approach. *Journal of Policy Analysis and Management: [the Journal of the Association for Public Policy Analysis and Management]*, 29(1):84–110.

Pallais, A. (2015). Small Differences That Matter: Mistakes in Applying to College. *Journal of Labor Economics*, 33(2):493–520.

Zimmerman, S. D. (2019). Elite Colleges and Upward Mobility to Top Jobs and Top Incomes. *American Economic Review*, 109(1):1–47.

A Cutoff Selection and Enrollment Age Construction

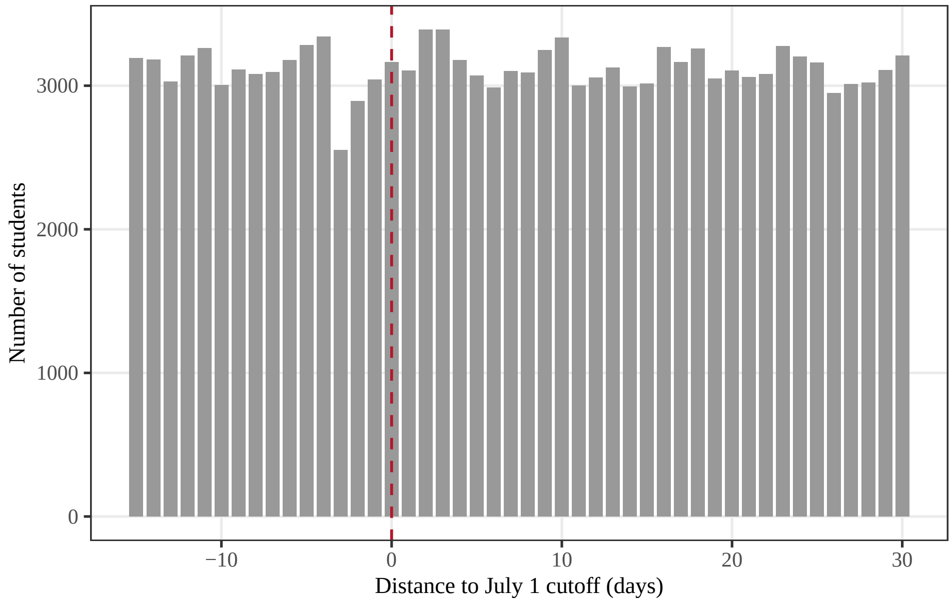
Chile operates four school entry cutoffs (April 1, May 1, June 1, and July 1), each generating a discontinuity in first-grade enrollment age. We focus on the July 1 cutoff because it produces the largest first-stage jump (approximately 0.4 years), reflecting the fact that most schools adopted this date as their operational entry rule. The April, May, and June cutoffs produce substantially smaller jumps (approximately 0.06, 0.06, and 0.11 years, respectively). A Cochran Q test of equality across the four cutoff-specific RD estimates rejects the null of homogeneity for college application ($Q = 18.2$, $df = 3$, $p < 0.001$), confirming that the July 1 effect is statistically distinct.

The enrollment age measure used in Figure 1 combines two sources. For approximately 27 percent of students in our sample (52 percent in the post-reform period), we directly observe the year of first-grade enrollment from the MINEDUC *Rendimiento* panel. For the remainder, we approximate school starting age as the student's graduation year minus eleven, correcting for observed grade repetitions following McEwan and Shapiro (2008). We validate this approximation against the directly observed measure on the overlap subsample ($r = 0.79$).

Following Celhay and Gallegos (2025), we estimate intention-to-treat effects throughout the paper and do not scale estimates by enrollment age. Doing so would require monotonicity and exclusion restriction assumptions that are difficult to defend when multiple cutoffs generate heterogeneous compliance rates.

B Density Test

Figure A.1: Density of births around the July 1 cutoff



Note: This figure plots the number of students by day of birth relative to the July 1 school entry cutoff. The dashed vertical line marks the cutoff. No visual evidence of bunching is detected (McCrary, 2008).

C Covariate Balance

Table A.1: Covariate Balance at the July 1 Cutoff

	(1) $\hat{\beta}_{pre}$	**(2)** $\hat{\beta}_{post}$	**(3)** **(2) − (1)**
Panel A: Predetermined covariates			
Male	0.004	-0.000	-0.004
	(0.013)	(0.014)	(0.019)
Educ: secondary	0.015	0.010	-0.005
	(0.014)	(0.014)	(0.020)
Educ: college	-0.003	0.014	0.018
	(0.012)	(0.012)	(0.017)
Income: middle	-0.013	0.016	0.029
	(0.012)	(0.013)	(0.018)
Income: high	0.000	0.018*	0.018
	(0.009)	(0.010)	(0.013)
Joint significance test			
$\chi^2(5)$			5.58
p-value			0.350
Panel B: Downstream academic measures			
PSU score (avg.)	0.010	0.057**	0.047
	(0.026)	(0.027)	(0.038)
GPA score	-0.011	0.071**	0.082**
	(0.027)	(0.028)	(0.039)

Note: Each row reports RD estimates at the July 1 cutoff. Column (1): pre-reform period. Column (2): post-reform period. Column (3): difference-in-discontinuities. Panel A reports predetermined covariates (determined before the student enters school). Panel B reports downstream academic measures that can respond to the reform. The joint χ^2 test evaluates whether all Panel A differences are simultaneously zero. Bonferroni-adjusted minimum p-value across Panel A: 0.55. Estimation follows (Calonico et al., 2014). Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Robustness

We subject the main difference-in-discontinuities estimates to four robustness checks. The results confirm that the application effect is robust to specification choices while the conditional enrollment effect is equally stable.

Polynomial order and bandwidth selection. Our baseline uses a local linear polynomial ($p = 1$) with MSE-optimal bandwidth. Table A.2 presents estimates under two alternative specifications: a local quadratic ($p = 2$) with MSE-optimal bandwidth, which uses a wider bandwidth and provides a more conservative test. The application estimate is 3.8 percentage points under the baseline and 2.2 percentage points under the quadratic. The attenuation under the quadratic is expected, as broader bandwidths include observations farther from the cutoff, diluting a treatment effect that is concentrated near the threshold. The conditional enrollment result follows the same pattern, with the linear specification producing a significant estimate (-7.0 pp) and the quadratic attenuating toward zero.

Table A.2: Robustness: Polynomial Order

	(1) $\hat{\boldsymbol{\beta}}_{\boldsymbol{pre}}$	**(2)** $\hat{\boldsymbol{\beta}}_{\boldsymbol{post}}$	**(3)** **(2)** − **(1)**
Panel A: Applied			
$p = 1$ (bw: 24 / 22)	0.006	0.044***	0.038**
	(0.012)	(0.013)	(0.018)
$p = 2$ (bw: 27 / 26)	0.014	0.036*	0.022
	(0.018)	(0.019)	(0.026)

Note: Each row reports difference-in-discontinuities estimates for college application under an alternative polynomial order. Bandwidth (bw) in days shown per specification. Covariates include gender, family income, and parental education. Estimation follows (Calonico et al., 2014). Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Alternative fixed bandwidths. Table A.3 reports estimates for college application at bandwidths of $0.5h$, $0.75h$, h (MSE-optimal), $1.5h$, and $2.0h$ days around the July 1 cutoff. The point estimate is stable across this range, and the confidence intervals overlap throughout. Narrower bandwidths produce larger point estimates with wider confidence intervals, consistent with a local effect.

Table A.3: Robustness: Bandwidth Sensitivity

	(1) $\hat{\beta}_{pre}$	(2) $\hat{\beta}_{post}$	(3) (2) − (1)
Panel A: Applied			
0.50×	0.007	0.052***	0.045*
	(0.016)	(0.018)	(0.024)
0.75×	0.010	0.044***	0.034*
	(0.013)	(0.014)	(0.019)
1.0× (baseline)	0.007	0.046***	0.039**
	(0.013)	(0.013)	(0.018)
1.50×	0.005	0.045***	0.039**
	(0.012)	(0.012)	(0.017)
2.00×	0.002	0.045***	0.043**
	(0.012)	(0.012)	(0.017)

Note: Each row reports difference-in-discontinuities estimates for college application at alternative bandwidths expressed as multiples of the MSE-optimal bandwidth (h_{pre} = 24, h_{post} = 22 days). Covariates include gender, family income, and parental education. Estimation follows (Calonico et al., 2014). Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Placebo cutoffs. If the result reflects the ranking reform rather than a generic change at the July 1 discontinuity, we should observe no amplification at dates where no school entry rule applies. Table A.4 tests three placebo cutoffs (October 1, May 15, and March 1) and confirms this prediction. The estimates are small, statistically insignificant, and of mixed sign. The amplification is specific to the July 1 cutoff where the age-at-entry advantage is largest.

No covariates. Our baseline specification includes a set of predetermined covariates (gender, parental education, family income) to improve precision. Removing all covariates yields a difference-in-discontinuities of 4.5 percentage points for college application ($p = 0.014$), slightly larger than the baseline estimate, confirming that covariate adjustment does not drive the result.

Table A.4: Robustness: Placebo Cutoffs

	(1) $\hat{\beta}_{pre}$	(2) $\hat{\beta}_{post}$	(3) (2) − (1)
Panel A: True cutoff (July 1)			
Baseline	0.006	0.044***	0.038**
	(0.012)	(0.013)	(0.018)
Panel B: Placebo cutoffs			
Oct 1 (day 274)	-0.014	-0.017*	-0.003
	(0.012)	(0.010)	(0.015)
May 15 (day 135)	-0.004	0.002	0.006
	(0.010)	(0.011)	(0.014)
Mar 1 (day 60)	0.005	-0.008	-0.013
	(0.011)	(0.010)	(0.015)

Note: Panel A reproduces the baseline estimate at the July 1 cutoff. Panel B reports estimates at placebo cutoff dates where no school entry rule applies. Placebo windows span ±60 days around each date. Covariates include gender, family income, and parental education. Estimation follows (Calonico et al., 2014). Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

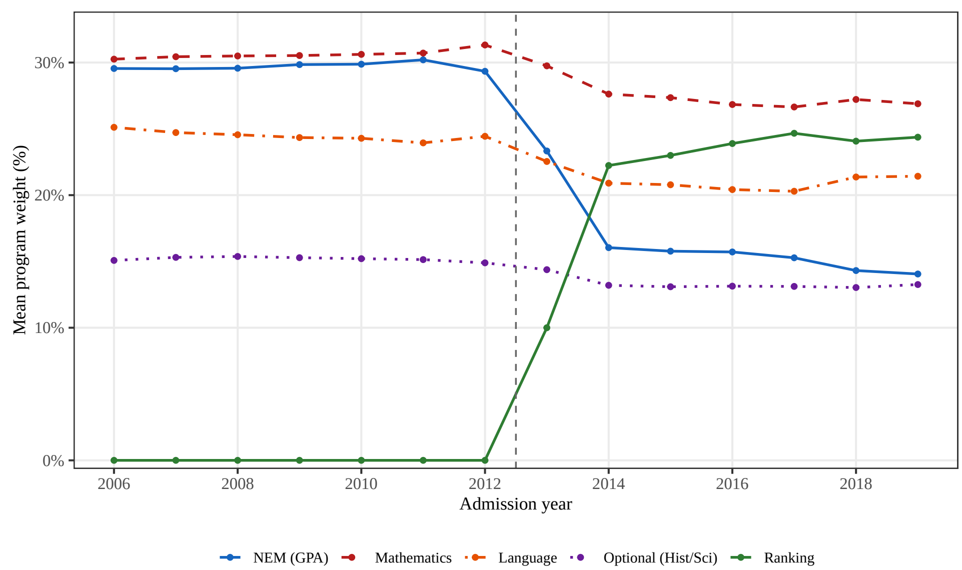
Table A.5: Admissions Weight Composition Before and After the Reform

	Pre-reform	**Post-reform**	**Change**
NEM (GPA)	29.7	16.2	-13.5
Mathematics	30.7	27.4	-3.2
Language	24.5	21.1	-3.4
Optional (Hist/Sci)	15.2	13.3	-1.9
Ranking (new)	0.0	22.0	+22.0
Total	100.0	100.0	
N (program × year)	6,988	10,638	

Note: Mean weight assigned to each admissions score component across all programs in the centralized system. Weights are on a 0–100 percentage point scale and sum to approximately 100. The ranking score was introduced in 2013, funded by reductions in other components. Optional is the maximum of history and science weights per program. Estimation follows (Calonico et al., 2014).

E Program-Level Weight Shift

Figure A.2: Admissions weight composition before and after the reform



Note: This figure plots the average weight assigned to each admissions score component across all programs in the centralized system. The ranking score was introduced in 2013, funded primarily by reductions in NEM weight. Five components sum to approximately 100 percentage points.

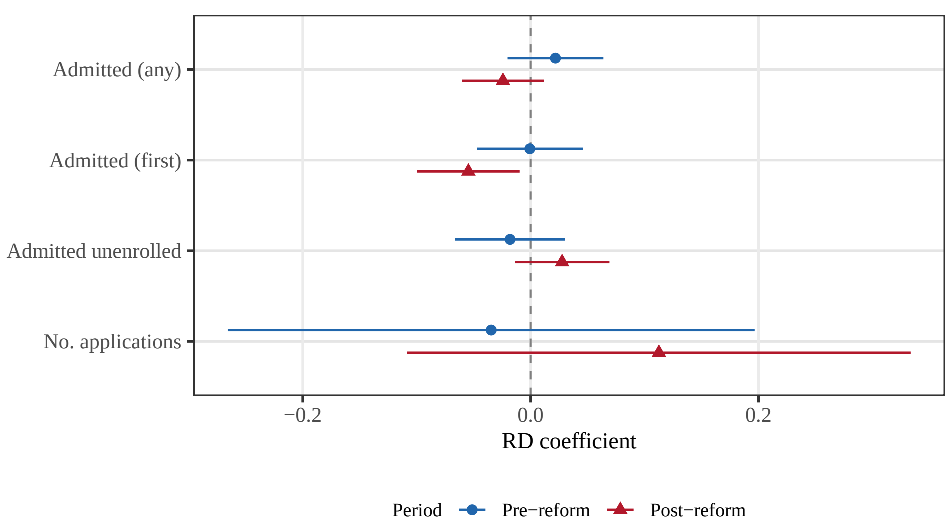
F Enrollment Decomposition

Table A.6: Decomposition of Conditional Enrollment

	(1) $\hat{\beta}_{pre}$	(2) $\hat{\beta}_{post}$	(3) (2) − (1)
Admitted (any)	0.022 (0.021)	-0.024 (0.018)	-0.046 (0.028)
Admitted (first)	-0.001 (0.024)	-0.055** (0.023)	-0.054 (0.033)
Admitted unenrolled	-0.018 (0.025)	0.028 (0.021)	0.046 (0.032)
No. applications	-0.035 (0.118)	0.113 (0.113)	0.147 (0.163)
N	98,264	116,977	

Note: Sample consists of applicants (PSU takers who submitted at least one preference). Admitted (any) indicates selection by the DAA for at least one listed program. Admitted (1st choice) indicates selection for the top-listed preference. Admitted unenrolled indicates selection but no matriculation in any centralized program. No. preferences is the count of programs listed in the application. Estimation follows (Calonico et al., 2014). Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

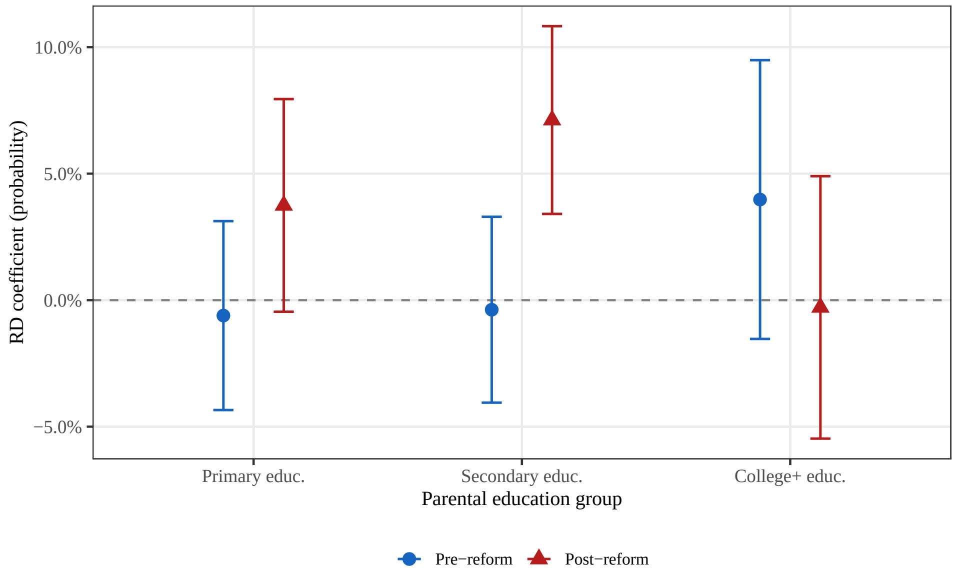
Figure A.3: Decomposition of conditional enrollment



Note: This figure plots pre-reform and post-reform RD coefficients at the July 1 cutoff for four outcomes measured among applicants. Horizontal bars represent 95% confidence intervals. Estimation follows Calonico et al. (2014).

G Additional SES Specifications

Figure A.4: Pre-reform and post-reform RD estimates by parental education



Note: This figure plots the pre-reform and post-reform RD coefficients at the July 1 cutoff for college application, separately by parental education group (primary, secondary, college). Vertical bars represent 95% confidence intervals. Estimation follows Calonico et al. (2014).