Who Wins with Class-Rank College Admissions?

Juan L. Fuentes-Acosta[*†]

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Abstract

Class-rank policies increase college admission chances for students who perform better than their peers within their high school. These policies aim to promote equity while avoiding the legal and political challenges of affirmative action. I study the effects of Chile's Relative Ranking rule, introduced in 2013, which boosted admission scores for top students in each high school. I use administrative data on the universe of applicants from 2012 to 2014, leverage variation pre- and post-policy, and estimate a structural model of college choice with endogenous consideration sets and graduation outcomes. This framework allows me to capture how the policy changed admission scores and reshaped student application choices. On average, student welfare rose by 0.04 km measured as willingness to travel. This effect hides important heterogeneity. I find that the rule shifted 13% of applicants into more-preferred programs, who gain on average 1.4 km in utility. Of these students, 86% were from public and voucher schools and 61% were women. Private school students and men were displaced to less preferred alternatives and showed lower graduation rates. Counterfactuals show that a top-10% affirmative-action quota lowers average welfare and reduces graduation, while the class-rank rule delivers larger gains for public and voucher students without a drop in average graduation.

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[†]Department of Economics, Boston College. Email: fuentejc@bc.edu.

1 Introduction

Colleges are engines of upward mobility, yet admission to selective programs remains stratified by socioeconomic background (Chetty et al., 2020, 2025). Admissions often rely on standardized tests and high school grades, but persistent disparities in test performance across groups raise concerns about equity (Rothstein, 2004, Card and Rothstein, 2007, Niederle and Vesterlund, 2010). Policymakers have used affirmative action to expand access, but as these policies face controversy and legal limits (Students for Fair Admissions, Inc. v. President and Fellows of Harvard College, 2023), attention has shifted to group neutral alternatives, such as class-rank admissions (Bleemer, 2023, Mukherjee, 2025).

A class-rank policy raises college admission chances, or even secures a spot in higher education, for students who perform at the top of their own high school cohort. These policies are group-neutral and aim to expand opportunities for high achieving students in under-resourced settings. But they also reallocate scarce seats, creating winners and losers (Black et al., 2023, Reyes, 2022), and as with affirmative action, they are subject to mismatch concerns: students admitted through these policies could struggle if their preparation does not align with the academic demands of their college, and might achieve stronger outcomes at less selective institutions (Arcidiacono et al., 2015).

This paper examines the effects of a class-rank policy, identifies the students who benefit from it, evaluates the trade-off between diversifying access and graduation outcomes, and compares these outcomes to those under a counterfactual affirmative action policy. Because seats in higher education are limited, class-rank policies benefit some students at the expense of others. These shifts in admission chances may lead applicants to adjust their choices: some may apply to more selective institutions, while others may turn to less selective ones. The resulting allocation of students across institutions depends on who receives the boost and how applicants respond to it. Beyond access, the policy also affects graduation outcomes through two forces: quality and fit (Arcidiacono and Lovenheim, 2016). Access to a more selective institution can raise performance through the institution's effect on academic outcomes (quality channel), but it can also reduce performance when there is a mismatch between students' preparation and the institution's academic demands (fit channel).

I study the Relative Ranking (RR) policy, a class-rank rule adopted in Chile in 2013. The RR policy raised admission scores for students whose GPA was above their school's historical average. Its goal was to value performance in context and to reduce gaps between students from under-resourced public schools and private paid schools (CRUCh,

2012). Chile's Centralized Admission System (CCAS) provides an ideal setting to study such a policy: placements are determined by a deferred-acceptance algorithm rather than by individual college discretion. The RR policy was implemented within this centralized framework using a transparent formula that increased scores in proportion to how much a student's GPA exceeded their school's average. The system also maintains detailed administrative records on every student's application, admission, and graduation, which allows me to follow complete academic trajectories from the end of high school through college completion.

I first present evidence that the policy expanded access. Using a difference-in-differences design, I show that top students from public and voucher schools increased applications and admissions to selective universities relative to private school students. This gain did not lower performance. Graduation rates remained stable, a result that challenges the mismatch hypothesis. These estimates suggest improved equity but do not capture mechanisms, equilibrium displacement, or welfare effects. They also do not show quality and fit trade-offs in graduation or allow the evaluation of alternative policies. These limitations motivate the structural framework.

To study the effect of the RR policy on admission and graduation outcomes, I build a structural model of higher education applications that includes a human capital production function. In the model, each student forms a rank-order list of programs they want to apply to, choosing only from a subset of all available programs. Students skip programs where they believe their admission chance is low, a pattern supported by evidence on applicant behavior in this setting (Fack et al., 2019, Larroucau and Rios, 2020, Fabre et al., 2024). This behavior is known as "skipping the impossible." Rank-order lists are built using two model primitives: student utility function and a function that generates student-specific consideration sets. Students apply to programs where their expected utility exceeds the utility of not enrolling. Consideration sets come from a function that combines each student's admission probability with a structural deviation term that captures other systematic biases such as mistakes, optimism or pessimism about their admission chances (Fabre et al., 2024). After applications and assignments, the human capital production function determines each admitted student's graduation outcome.

I use data from the CCAS covering the year before the RR policy (2012) and the first two years after its implementation (2013 and 2014). The data include students' rank-ordered lists of programs as well as programs' admission criteria, such as weights on standardized tests, GPA, and RR. The identification of the utility function and the consideration function relies on these administrative data and on two exclusion restrictions

that separate student preferences from consideration frictions. Following the work on identification of demand systems with latent choice frictions of Agarwal and Somaini (2022), I use distance between a student and a program as a shifter of utility, and each student priority when applying to each program (i.e. the weighted admission scores) as a shifter of consideration. The intuition for identification is that each shifter traces out the distribution of the latent variable they affect, while the other latent variable is held fixed. As students are farther away from programs their likelihood of applying decreases because of the disutility for distance, but their probability of considering remains constant as being far from a program does not affect their probability of being admitted. Each student priority for programs depend on two objects: students scores and program weights on those scores. As the student priority in a specific program is reduced, the likelihood of applying to that program decreases not because the student does not like the program but because they stop considering it since it becomes out of reach. Both of these exclusion restrictions hold conditional on student level, program level, and match specific controls. Finally, year-to-year variation in realized admission cutoffs provides variation that aids the identification of the human capital production function.

The model is estimated in two stages. In the first stage I estimate the rationally expected cutoff scores following Agarwal and Somaini (2018)'s bootstrap method. This approach bootstraps with replacement from the pool of applicants, runs the admission mechanism, and gets the cutoff scores for each bootstrapped sample. The average of these bootstrapped samples is a consistent estimator for the rational expectations cutoff. This stage assumes the existence large market, condition that is met in the case of college admissions in Chile where more than 100,000 students apply each year. In the second stage I use a Gibbs sampler adapting the methods in McCulloch and Rossi (1994) to estimate utility, consideration, and human capital equations. Here I take as given the estimated expected cutoffs from the previous stage, and sequentially draw from conditional distributions the latent variables and then the parameters. This procedure produces a chain of estimates, where the mean of that chain is asymptotically equivalent to maximum-likelihood estimates (van der Vaart, 2000, Agarwal and Somaini, 2022).

The estimation yields three main findings. First, the estimated parameters of the utility and consideration functions are stable across 2012, 2013 and 2014, suggesting they are structural primitives and robust to different RR policy implementations. This alleviates concerns that these parameters might shift in counterfactuals involving changes to the RR policy. Second, the model fits well, matching moments not targeted in estimation. In particular, the predicted distribution of admitted students' scores closely aligns with

the observed distribution, as well as predicted graduation rates closely align with those in the data. Third, the model reveals a negative correlation between program quality and the probability of being considered, especially for students from public and voucher schools, and a positive correlation with graduation outcomes. This underscores the role of the policy in expanding access for students in under-resourced contexts, which was the policy maker's goal.

I take these empirical estimates to a counterfactual simulation where—for the same sample—I estimate the average student welfare and graduation with and without the RR policy. I find that the policy improved admission outcomes for students from public and voucher schools, as well as for female applicants. Relative to the pre-policy baseline, 13% of applicants were admitted to a more preferred program after the policy. Of these students, 86% came from public or voucher schools, and 61% were female. On average, the policy had a moderate positive effect of 0.05 km in utility normalized by willingness to travel. This average hides important heterogeneity. Winners from the policy gain as much as 1.4 km while losers decrease their utility in a similar amount. Of the group of students benefiting from the policy, 17% come from the outside good. From the group of worse-off students 24% of them were displaced to the outside good. In terms of graduation, rates changed little for all students. These close to null effects are explained in large part by program effects (quality channel) going in the opposite direction of match effects.

I study a counterfactual affirmative action policy. I take an existing policy called *Beca de Excelencia Academica*, BEA. This policy gave scholarships for Top-10% students and also reserved special quotas in each program. For the years of analysis the quotas were implemented in a limited way, and in the counterfactual I expand them to account for 10% of the total seats in each program. I find the overall effect of the policy is a reduction in average utility by 0.03 km in willingness to travel. Better-off students win on average close to 2 km in utility, while worse-off students lose 1.4 km. In particular, BEA students gain on average 1.6 km, more than 3 times what they gain in the RR policy (0.47 km). The affirmative action policy has larger negative effects in graduation outcomes than the RR policy. Students that get better placements under the affirmative action policy see their graduation rates reduced in 1.4 percentage points. A decomposition exercise shows that these results are driven mainly by students moving to programs with lower graduation rates, while the portion explained by the match effect has a small but positive effect.

1.1 Literature review

This paper is related to several strands of the literature. First, is related to the literature about effects of Affirmative Action (AA) and group-neutral access policies in higher education. Research on AA bans find that under-represented minorities (URM) lose places at selective institutions, but overall enrollment rates remain unchanged. Students "reshuffle" between colleges after policy changes (Arcidiacono, 2005, Howell, 2010, Backes, 2012, Hinrichs, 2012). This paper findings go in a similar direction but for a class-rank policy. The implementation of the Relative Ranking kept the rate of enrollment in higher education roughly unchanged, but made top students from under-resourced backgrounds get admitted in more preferred colleges at higher rates displacing non-top students.

This paper also speaks to the literature studying access policies and mismatch in higher education. Evidence on mismatch effects is mixed, with some studies finding support (Arcidiacono et al., 2014, 2016) and others not (Bleemer, 2020, 2024).[1] In Brazil, Otero et al. (2023) find that AA increases college access, degree quality, and projected earnings for targeted students, with no observed losses for displaced students and no efficiency loss in the system. Regarding group-neutral access policies, Black et al. (2023) study Texas's Top-Ten-Percent (TTP) plan and Bleemer (2024) study California's Eligibility in the Local Context (ELC). Both find that these policies raised enrollment, graduation, and earnings for top students in low-income schools. The first paper uses a difference-in-differences design for Texas, and the second applies a regression-discontinuity design for California. My paper differs from these approaches by estimating a structural model that allows me to measure welfare effects of Chile's Relative Ranking (RR) policy and to jointly estimate a graduation outcome equation, finding that students from public schools get admitted into highly preferred programs and graduate at higher rates than in their counterfactual placements.[2]

A related paper is Kapor (2024), which studies the TTP plan in Texas using a structural model. They decompose the policy's effect on enrollment into a direct mechanical component and a transparency component, finding that about two-thirds of the 9 p.p.

[1] On one hand Arcidiacono et al. (2014) finds that the ban of AA in California lead to higher URM graduation rates while Bleemer (2021) shows that after the ban, URM applicants effectively enrolled at less-selective institutions, but had unchanged or declined STEM performance, persistence, and attainment. See Bleemer (2020) for a discussion about the differences in the findings between these papers.

[2] Because the RR policy was implemented nationwide, it is not possible to define a credible treatment-control setup. Designs such as the RDD used in Daugherty et al. (2014) or Bleemer (2024) are not suitable here since the RR rule gives a score increase proportional to how far a student's GPA is above their school's historical average. Students near the cutoff receive only a small boost.

enrollment gain came from greater transparency rather than from the seat guarantee.[3] My contribution relative to Kapor (2024) is twofold. First, while in Texas the TTP policy granted automatic admission to eligible students without the need to apply, Chile's RR rule did not guarantee admission to any program. Instead, it changed students' expected admission probabilities and, in turn, their application decisions. This behavioral channel is the main mechanism through which the RR policy operates. I show that accounting for changes in application behavior is key: without them, the policy would have generated much smaller gains in welfare and academic outcomes for disadvantaged students. Second, I study a class-rank policy implemented in a centralized admission system, which is relevant because centralized systems are becoming more common across countries. As Neilson (2024) document, by 2024, 57 countries had adopted some form of centralized assignment mechanism for higher education. Understanding how class-rank policies work within these systems helps anticipate their effects in settings where centralization is expanding.

Studies on Chile's Relative Ranking policy reach different conclusions. Barrios (2018) finds that policy beneficiaries had lower first-year retention, which suggests academic mismatch. Using a difference-in-differences design, Reyes (2022) re-examines the policy. Her study shows that "pulled-up" students enrolled in more selective programs and also graduated from them at higher rates. This gain in selective degrees did not reduce overall BA completion. Therefore, Reyes (2022) concludes the policy improved equity without a loss in efficiency.[4] Both papers assume no reaction of the students to the policy. This paper contributes by building and estimating a model where students rank-order lists are determined in equilibrium. Students react to different implementations of the policy. This allows me to extend the analysis to 2014 and on, where the RR policy was expanded. The model allows me to decompose the effect of the RR policy between the mechanical effect and behavior changes induced by the policy.

This is the first study for Chile where program skipping emerges endogenously from the model. Previous research (Bordon and Fu, 2015, Espinoza, 2017, Bucarey, 2018, Johnson, 2023, Kapor et al., 2020) restricted students' choice sets exogenously, using heuristics such as including only programs where the cutoff score was close to the student's score, or limiting choices to programs selected by similar students. In contrast,

[3]See Arcidiacono et al. (2015) and Dynarski et al. (2023) for broader literature reviews.

[4]Other related articles about the policy include early simulations by Larroucau et al. (2015), effects on high-school changes as a strategic reaction by Concha-Arriagada (2023), and grade inflation effects by Fajnzylber et al. (2019).

my model allows the Relative Ranking policy to affect applications through endogenous consideration sets.

2 Institutional details

Chile's centralized college admission system and its Relative Ranking rule provide an ideal setting to study the effects of a group-neutral access policy. This is due to three key factors. First, the CCAS gives data on student choices, program requirements, and the allocation process. These features create a clear choice environment that makes it possible to study how the policy affects different groups, including those targeted by the RR policy. Second, admission rules are transparent and deterministic. Once admissions criteria of programs are fixed, no further assumptions on the supply-side behavior are needed to get final allocations. Finally, the policy changed admissions criteria within the centralized system following specific formulas, which makes the policy change tractable.

In the Chilean CCAS, students take a test and apply, programs rank them by score, and admissions are decided through a Deferred Acceptance Algorithm (DAA). Each year, 33 universities in Chile offer over 1,400 programs through the CCAS, with student placements determined by application scores together with the DAA. Early in the summer, students take the national admissions exam (PSU, *Prueba de Selección Universitaria*). After receiving their scores, they submit applications with Rank Order Lists (ROLs) of preferred programs. Each program then ranks applicants using a program-specific admission score. This score is a weighted sum of the student's PSU section scores and their high school GPA, which is first converted to a PSU-equivalent score. Formally, the admission score of student i for program j is calculated as:

$$s_{ij} = \sum_{k \in K} \alpha_{jk} \cdot \text{PSU}_{ik} \; + \; \alpha_{jGPA} \cdot GPA_i,$$

where K is the set of PSU sections (e.g., Language, Math, Science, and History), PSU_{ik} is student i's score in section k, GPA_i is the PSU-equivalent of the student's GPA, and α_{jk} and α_{jG} are the program-specific weights (with $\sum_{k \in K} \alpha_{jk} = 1 \; \forall \; j$).

Prior to the test, during the winter, programs set their admissions criteria by defining weighted scoring formulas to rank applicants. For example, STEM programs often assign greater weight to the mathematics and science sections of the standardized test. At the same time, programs publish their 'capacities,' specifying the number of students they

intend to admit. Next, early in the summer, students take the national standardized admissions test (PSU, *Prueba de Selección Universitaria*). After receiving their scores, they submit applications, which include Rank Order Lists (ROLs) of their preferred programs in descending order. Programs then rank applicants based on their pre-established weighted formulas. Finally, once all applications are submitted, admissions are determined using a College-Proposing DAA (Rios et al., 2021). This algorithm processes student preferences, program rankings, and program capacities to match students to programs, respecting mutual preferences until all slots are filled or all eligible students are placed.[5]

In 2013, authorities introduced the Relative Ranking policy—expanded in 2014—to add a bonus to the admission scores of students graduating at the top of their high schools. The standardized test used in the admissions process had revealed significant performance disparities between students from public and private schools (Pearson, 2013). Public school students, even those at the top of their class, tend to perform worse than their peers from private paid schools on the standardized test used for admissions. To address this, the *Consejo de Rectores de Universidades Chilenas* (CRUCh) implemented an additional admissions criterion that evaluates each student's performance relative to their specific educational context.

> *"The PSU is useful, it has been useful for many years, and it will continue to be useful for many more. The issue is that, by itself, it cannot eliminate an effect that students bring from their own schools, which is the inequality from the point of view of knowledge and the quality of education they were subjected to during their training. When we are talking about rankings, we can put on an equal footing those students who are at the top of a private paid school and those at the top of a public school."*
>
> Consejo de Rectores de las Universidades Chilenas (CRUCH) (2012)

The RR policy was designed to address inequalities by introducing a criterion where top-performing students from both public and private schools are considered equally in the admissions process.

The admission component that the RR added to the weighted score is a boosted GPA score, and was incorporated in the following way:

$$s_{ij} = \sum_{k \in K} \alpha'_{jk} \cdot \text{PSU}_{ik} \; + \; \alpha'_{jGPA} \cdot GPA_i \; + \; \alpha_{jRR} \cdot RR_i.$$

[5]Using a "reverse-engineering" approach, Rios et al. (2021) show that the implemented algorithm corresponds to a college-proposing DAA. Their analysis reveals that while the Chilean setting is not strategy-proof, this is less concerning in a large market like Chile's. Students generally find it approximately optimal to submit their true preferences.

The size of the boost is based on two school-specific benchmarks: the historical average ($\overline{GPA}$) and the historical maximum GPA ($\max GPA$) at the student's school. Both $\overline{GPA}$ and $\max GPA$ are derived from the GPAs of students in the three preceding graduating cohorts from that specific school. By evaluating students relative to their own school's performance landscape, this measure aims to provide a more equitable comparison and potentially reduce intense, direct competition based solely on raw GPA among students from the same institution.

The formula to calculate the RR score (RR_i) for student i is as follows:

$$RR_i = \begin{cases} GPA_i & \text{if } GPA_i < \overline{GPA} \\ \overline{GPA} + \frac{850-\overline{GPA}}{\max GPA - \overline{GPA}}(GPA_i - \overline{GPA}) & \text{if } GPA_i \in [\overline{GPA}, \max GPA] \\ 850 & \text{if } GPA_i > \max GPA \end{cases}$$

Students with a GPA equal to or lower than the historical average at their schools have an RR score equal to their GPA score. Students with a GPA bigger than the historical average but smaller than the historical maximum get their GPA score plus a boost. This boost is determined by the slope of the line that connects the historical average GPA score with the historical maximum, which is for all schools the maximum possible score, 850. This implies that students in this range, from a school with a more spread out high school GPA distribution, will have a smaller boost in terms of score points for each extra point in their GPA. Finally, students that perform above the historical maximum at their high

school get the maximum possible score (850), even if the GPA is, measured in application points, very low.

The RR policy implemented in 2013 mandated that every program weighted the RR component at 10%, but from 2014 they allowed each program to choose anywhere between 10% and 40%.

Evaluating the RR policy's impact requires accounting for both its direct effects to application priorities and any resulting changes in student behavior in response to the policy. For instance, the policy might introduce new weighted scores, directly altering how applications are ranked. However, if students become aware of these changes and strategically modify their application choices or efforts in response, simply looking at the new scores won't provide a complete picture. Therefore, an evaluation must also analyze and incorporate these potential behavioral responses to understand the policy's true overall effect.

3 Data and descriptive statistics

Administrative data for students and programs is obtained from two principal sources: the Chilean Ministry of Education (MINEDUC) and the agency that administers the CCAS (DEMRE). My analysis utilizes the entire population of CCAS applicants, along with administrative data on program offerings, for the years 2012 (pre-RR reform), 2013 (post-RR reform), and 2014 (post-RR expansion). Access to this data is via MINEDUC's Open Data Center, while the non-public sections were obtained through direct transparency requests to the appropriate undersecretary (Education or Higher Education) or relevant agencies (Education Quality Agency, National Education Council, or Department of Evaluation, Measurement, and Educational Registration [DEMRE]). The following sections detail the data manipulation, effective replication of the admission process, and provide descriptive statistics of the sample and descriptive evidence on the policy's effects.

The MINEDUC student datasets allows me to track individuals through secondary and tertiary education. I merge this with DEMRE application data, which includes high school records, PSU test scores in math, verbal, science, and history, and a ranked list of applied programs with codes and submission order. DEMRE data also includes records of confirmed placements and enrollment decisions in higher education. The application data also contains a survey with demographic and socioeconomic variables,

such as income quintiles and parents' education. This combined dataset allows analysis of student preferences and differences across backgrounds.

3.1 Sample description

I use data for the whole universe of applicants for the years 2012 to 2014. Yearly, around 100,000 students apply to one or more of the around 1,300 programs available. For estimation purposes, I focus on students submitting valid applications and programs that are available through the whole period of analysis. For more details about how the sample is constructed, refer to Appendix D.

To study differences across students, I classify applicants by their school of origin type–public, voucher, or private–and use this variable as a proxy for socioeconomic status. This choice is motivated by two factors. First, at the moment of implementing the RR policy, the CRUCh had as a goal to close gaps between public and private paid schools, since these groups show large differences in value added (CRUCh, 2012). Second, in Chile, school type is strongly tied to socioeconomic background: families with higher income often send children to private paid schools, while families with lower income send them to public or voucher schools. School of origin is therefore a practical proxy for socioeconomic status. For example, the share of students whose mother has higher education is about three times higher in private schools than in public schools, and about twice as high as in voucher schools (as shown in Figure B.2).

Figure 1 illustrates gaps in PSU test scores and application behavior across school types. Public and voucher schools have fewer resources than private schools to prepare students for the PSU, leading to gaps in test scores. Panel 1a confirms that private school students outscore their peers in public and voucher schools. This disparity motivates the RR policy goal: to increase admission chances for students from disadvantaged schools who score lower on standardized tests. Beyond test scores, application behavior also differs by school type. Panel 1b shows that among students with similar scores, those from private schools are more likely to apply to top colleges than those from public or voucher schools. This gap suggests that frictions regarding programs that appear out of reach affect students from low-resource backgrounds more severely.

The RR policy began in 2013 with a mandatory 10% weight for every program, expanding in 2014 to allow weights between 10% and 40%. Figure 2 illustrates these changes over time. In 2012, programs assigned an average of 55.8% to Mathematics and Verbal exams, 14.9% to Science and History, and 29.3% to GPA. With the 2013 mandate,

Figure 1: Gaps in scores and in application behavior.

(a) PSU scores CDF by school type (2012)

(b) Probability of applying to a top university (2012)

Note: Panel (a) shows the cumulative distribution of test scores by school type. Public and voucher schools appear as a red dashed line; private schools appear as a blue solid line. Panel (b) reports the probability of applying to a top university, defined as applying to PUC Chile or the University of Chile.

the average GPA share fell to 23.3%, while Mathematics and Verbal dropped to 52.3%. By 2014, when programs gained flexibility, the average RR weight rose to 22.2%. As Figure 2b shows, this weight varied widely across programs. Consequently, weights for other criteria decreased further: GPA dropped to 16%, Science and History to 13.2%, and Mathematics and Verbal to 48.5%. Appendix B, Figure B.4 provides the detailed distribution of these admission criteria.

3.2 Descriptive evidence of the effect of the RR policy

To study the effects of the RR policy, first I study how applications, admissions, and graduation changed after its implementation. In order to do this, I compare students from public and voucher schools to the ones coming from private schools, and I classify students between top students and non-top according to their ranking within their own high-school cohort. I see how their likelihood to apply to top colleges, get admitted to, and graduate from top colleges changes before and after the policy in a design in the spirit of differences in differences.

Formally, I estimate the impact of the reform using the following linear probability model:

$$Y_{ist} = \alpha + \beta_1 \text{Post}_t + \beta_2 \text{Top}_i + \beta_3 (\text{Post}_t \times \text{Top}_i) + \gamma' X_i + \mu_s + \varepsilon_{ist}$$

Figure 2: Evolution of program admission weights and distribution of Relative Ranking (RR) weights.

(a) Evolution of weights by component

(b) Distribution of RR weight

Note: Panel (2a) shows the average distribution of weights across programs in 2012, 2013, and 2014. The components include: mathematics and verbal tests, alternative tests (science or history), GPA, and the Relative Ranking. Panel (2b) displays the distribution of RR weights in 2014, when programs could assign this component anywhere between 10% and 40%.

where Y_{ist} is the outcome of interest (e.g., applying, get admitted, or graduated from a top university) for student i from high school s in year t. The variable Post_t takes the value of one for the years following the implementation of the policy (2013 onwards), and Top_i is an indicator for high-performing students within their cohort. The coefficient of interest is β_3, which identifies the differential change in outcomes for top students relative to their peers after the reform. To account for sorting and observable differences in ability, the specification includes high school fixed effects, μ_s, and a vector of student controls, X_i, which includes gender and a quadratic polynomial of PSU scores.

To examine heterogeneity by socioeconomic background, I expand this framework into a triple-difference specification. This allows me to test whether the policy disproportionately benefited students from under-resourced schools. The estimating equation is:

$$\begin{aligned} Y_{ist} = \alpha &+ \lambda_1(\text{Post}_t \times \text{Top}_i) + \lambda_2(\text{Post}_t \times \text{Type}_s) \\ &+ \lambda_3(\text{Top}_i \times \text{Type}_s) + \lambda_4(\text{Post}_t \times \text{Top}_i \times \text{Type}_s) \\ &+ \gamma' X_i + \mu_s + \varepsilon_{ist} \end{aligned}$$

where Type_s is an indicator equal to one if the student's high school is Public or Voucher, and zero if it is Private. The coefficient of interest is λ_4, which captures the differential

effect of the policy on top students from public and voucher schools relative to top students from private schools. Note that the main effect of school type (Type_s) is absorbed by the school fixed effects, μ_s.

Student classification. I classify students as "top students" if their GPA places them above the 70th percentile of their high school cohort. This threshold is chosen because it corresponds to the point where the Relative Ranking boost begins to have a meaningful magnitude (approximately 0.3 standard deviations of the PSU score). Crucially, this ranking relies solely on GPA, allowing me to consistently identify high-performing students in the pre-policy period (2012) when the RR score did not yet exist. I confirm in the robustness checks that the results are not sensitive to this specific threshold choice (see Appendix B, Figure B.5).

Top program classification. To classify programs as "top," I rely on their institutional accreditation status at the time of the policy implementation. The National Accreditation Commission (*Comisión Nacional de Acreditación* or CNA) awards accreditation in years, serving as a public signal of quality and institutional robustness. In my main specification, I define top programs as those belonging to universities with five years or more of accreditation (out of a maximum of seven). This threshold captures highly selective institutions–including *Universidad de Chile*, *Pontificia Universidad Católica*, and *Universidad de Concepción*–while ensuring sufficient sample size to detect equilibrium effects.

Reduced-form effects of the RR policy. Table 1 reports difference-in-differences and triple-difference estimates for application, admission, and graduation outcomes. Panel A shows that top students responded to the policy by applying to selective institutions. Top students became 3.6 percentage points more likely to apply to a top university. This response varies by school background. The triple-difference estimate indicates that high-performing students from public and voucher schools increased their application rates by 1.6 percentage points more than their private school peers. This pattern suggests the policy encouraged students from public and voucher schools to target selective programs.

Panel B shows that these applications translated into admissions. The policy expanded access for the target population. Top students from public and voucher schools saw a 3.4 percentage point increase in admission probability. In contrast, the effect for private school students is small and not statistically significant. The positive triple-difference

coefficient confirms that the rule closed admission gaps by favoring high-performing students from the public and voucher sector.

Panel C addresses potential efficiency costs. A common concern with access policies is mismatch, where students struggle to complete their degrees at selective institutions. The results show no evidence of this. Graduation rates remained stable across all specifications. For public and voucher students, the point estimate is close to zero and statistically insignificant. These reduced-form findings indicate the policy improved equity without lowering efficiency. Beneficiaries gained access to selective institutions without a decline in graduation outcomes.

3.3 Discussion

These reduced-form estimates provide suggestive evidence regarding equilibrium changes rather than precise treatment effects. The college admission market operates in general equilibrium, where one student's outcome depends on the choices and scores of others. Because university seats are limited, the policy creates winners and losers. Increased admission rates for top students imply displacement for others. This violation of the Stable Unit Treatment Value Assumption means the control group is affected by the treatment, confounding the difference-in-differences estimates.

To address these limitations, the next section develops a structural model of college applications and graduation. The model allows for a counterfactual analysis that isolates the policy effect while holding other factors constant. This framework offers three main advantages. First, it accounts for the competition between students and corrects for selection bias in graduation outcomes. Second, it measures welfare through estimated utility, capturing how much students value their assignments. Third, it enables a detailed analysis of mechanisms, distinguishing between the mechanical effect of the score change and the behavioral response of students updating their application portfolios.

Robustness of results are presented in Appendix B.

Table 1: Effect of Relative Ranking Reform on Higher Education Outcomes

	(1) Full Sample	(2) Pub/Vouch	(3) Private	(4) Triple Diff
Panel A: Applied to Top University				
β_3 (DD) or λ_4 (DDD)	0.036***	0.040***	0.022***	0.016**
	(0.003)	(0.004)	(0.005)	(0.006)
Dep. Var. Mean	0.710	0.676	0.858	0.710
Observations	338,909	275,344	63,555	338,909
R^2	0.221	0.208	0.186	0.224
Panel B: Admitted to Top University				
β_3 (DD) or λ_4 (DDD)	0.026***	0.034***	0.008	0.024***
	(0.003)	(0.004)	(0.006)	(0.008)
Dep. Var. Mean	0.514	0.463	0.731	0.514
Observations	338,909	275,344	63,555	338,909
R^2	0.297	0.277	0.247	0.301
Panel C: Graduated from Top University				
β_3 (DD) or λ_4 (DDD)	0.002	0.003	−0.006	0.009
	(0.003)	(0.003)	(0.008)	(0.009)
Dep. Var. Mean	0.163	0.142	0.256	0.163
Observations	338,909	275,344	63,555	338,909
R^2	0.101	0.097	0.075	0.102

Notes: This table reports difference-in-differences (columns 1–3, coefficient β_3) and triple-difference (column 4, coefficient λ_4) estimates of the effect of Chile's 2013–2014 Relative Ranking reform. "Top universities" are defined as institutions with at least five years of accreditation. The coefficient shown is "Post × Top 70%" for columns 1–3 and "Post × Top × Public/Voucher" for column 4. All three panels use the same sample: students who applied to the centralized system. For Panels B and C, students who did not get admitted or did not graduate are recoded to 0, ensuring all outcomes use the same sample size. Controls include PSU score, PSU squared, and gender. All regressions include school fixed effects and cluster standard errors at the school level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4 Empirical Model

To evaluate the effect of the RR policy, I develop a model of higher education applications that incorporates student uncertainty about program cutoff scores. Students apply to programs they prefer over their outside option and where they believe they have a positive probability of admission. This "skipping the impossible" behavior is supported by evidence that students avoid applying to programs perceived as out of reach (Fack et al., 2019, Larroucau and Rios, 2020, Fabre et al., 2024). Heterogeneous beliefs generate variation in consideration sets across students. The RR policy shifts admission probabilities, altering consideration sets and, in turn, application behavior. The model allows me to isolate two channels of the policy's effect: shifts in the scores with which students apply, and changes in the inclusion of high-quality programs in students' lists. In addition, I link the application stage to academic outcomes by modeling graduation as the result of a human capital production function. This extension allows me to study not only how the RR policy changes students' utility from admissions, but also how it affects the probability of on-time graduation under different policy designs.

4.1 Application model

I define an application process in a year as a market. For each market $t \in \mathcal{T}$, the set of students is $\mathcal{I}_t = \{1, \ldots, I\}$ and the set of programs is $\mathcal{J}_t = \{1, \ldots, J\}$. Students have utility for programs, beliefs about admission, and application scores. Programs have exogenous characteristics and admissions criteria. In this section I describe how student application behavior is modeled. I describe student preferences for programs, student consideration sets, and how these two objects determine the student application list.

Students preferences

Student's i utility for program j is u_{ij}, and their utility for the outside good is u_{i0}. Students' utility for programs is a function of program characteristics (x_j), student characteristics (z_i), and their interactions (w_{ij}). Unobserved heterogeneity in preferences for program characteristics is captured through random coefficients. The utility function is defined as

$$u_{ij} \quad = \delta_j^u + \theta_i^x x_j + \beta^w w_{ij} + \beta^d d_{ij} + \varepsilon_{ij}, \tag{1}$$

where δ_j^u for $j \in \mathcal{J}$ are a program-level mean utility terms. $\theta_i^x \sim \text{MVN}(\beta^z z_i, \Sigma)$ are student-specific random coefficients that capture heterogeneity in tastes over programs characteristics. I assume that distance between the student and the program, d_{ij} enters linearly. Finally, $\varepsilon_{ij} \sim \mathcal{N}(0, \sigma_\varepsilon^2)$ is an idiosyncratic utility shock.[6]

Utility for the outside good is given by

$$u_{i0} \quad = \psi + \psi^z z_i + \varepsilon_{i0}, \tag{2}$$

where $\varepsilon_{i0} \sim \mathcal{N}(0, \sigma_0^2)$ is a student-specific shock. A key restriction I impose is that unobservables $\varepsilon_{ij}, \varepsilon_{i0}$ and θ_i^x are independent of students' observable characteristics, in particular distance between students and programs. This rules out, for instance, families systematically choosing their geographical residence at the time of high school to be next to the university or program they prefer.

Applications

Given the large amount of programs in each market, it is unrealistic that each students compares all possible combinations. In Chile there are around $1,400$ programs offered each admission process. If applicants can list up to 10 among $1,400$ programs, they have more than 10^{31} lists to choose from. Instead of comparing list by list, I will assume two deviations from frictionless unbounded rationality.

First, I assume that students only apply to programs where they believe their admission chances are above a threshold. This assumption is based on evidence that students often do not list programs where they think they have a low chance of admission, which limits their set of program choices. Empirical studies have documented this "skipping the impossible behavior" for the case of Chile (Fabre et al., 2023, 2024) and also France (Fack et al., 2019). One can think of this behavior as students facing a psychological cost when applying to demanding programs because of the high probability of rejection (Idoux, 2022). In the Chilean setting, admissions are uncertain because students do not know what the admissions cutoffs will be at the moment of application. Cutoffs are only determined after all students submit their rank-order lists and the assignment mechanism runs.

Second, within their consideration set, I assume students rank all programs they prefer over the outside option in decreasing order of utility. This implies that applicants

[6]The model as presented here to be identified needs a scale normalization. Specifics about identification and estimation are in Section 5.

construct their ROL in a greedy manner. They begin by listing their most preferred program from their consideration set, followed by the next most preferred, and so on. This process continues until no remaining program in their consideration set is preferred to the outside option. For example, if student i submits the ROL $R_i = (5, 2)$, it means that program 5 is their most preferred choice. If rejected by program 5, their next preference is program 2. Should they be rejected by both programs 5 and 2, student i prefers the outside option to being matched with any other program available in their consideration set.

Students consideration sets

Subjective beliefs are captured with a reduced-form index c_{ij}. This index captures the student's subjective belief of admission at each program and will imply a student specific consideration set. A student considers program j if their subjective belief index for admission, c_{ij}, is positive:

$$\mathcal{C}_i = \{j \in \mathcal{J} : c_{ij} > 0\} \cup \{0\},$$

where $\mathcal{C}_i \subseteq \mathcal{J}$ is the consideration set for student i. The consideration set also includes an outside option, denoted as $\{0\}$, which is always considered and represents non-participation or a choice outside the centralized system (vocational, technical, and non-selective institutions).

Students beliefs are assumed to be anchored in rational expectations, yet I allow each student to have biased beliefs around this anchor. The index c_{ij} is modeled as:

$$c_{ij} = \gamma(s_{ij} - \mathbb{E}[\text{cutoff}_j]) + \underbrace{\delta_j^c + \gamma^z z_i + \gamma^w w_{ij}}_{c(z_i, w_{ij})} + \nu_{ij}. \tag{3}$$

Equation 3 has three components. First, the term $s_{ij} - \mathbb{E}[\text{cutoff}_j]$ represents the difference between student i's score relevant for program j (denoted by s_{ij}) and the expected cutoff for that program (denoted by $\mathbb{E}[\text{cutoff}_j]$). This component anchors subjective beliefs in rational expectations, as suggested by Larroucau and Rios (2020) and documented by Fabre et al. (2023, 2024) for the Chilean context. Second, $c(z_i, w_{ij})$ captures observed structural deviations from purely score-based rational expectations. This term allows for correlation between the consideration latent variable and preferences via observable factors, and is going to be considered as a primitive to be held constant in counter-factual scenarios. Third, ν_{ij} is an unobserved, idiosyncratic component representing

student-program specific deviations from rational expectations. This term can account for factors like individual mistakes in assessing chances, or tendencies towards optimistic or pessimistic beliefs, that are not captured by $c(z_i, w_{ij})$.[7]

Discussion of assumptions

The assumptions stated before give a simple behavior model that shows how the policy changes outcomes, not just by changing application scores, but also by adding or removing programs from students ROLs. In the model, the inclusion or removal of programs in students rank-order lists in different RR scenarios will operate through changes in the consideration sets. For each student i, the policy changes both the student-program score s_{ij} and the equilibrium expected cutoff $\mathbb{E}[\text{cutoff}_j]$ in all programs $j \in \mathcal{J}$.

Parsimony comes at a cost. The model will not be able to separately capture other consideration set frictions apart from "controlling for them" in a reduced form way via the function $c(Z_i, X_{ij})$. This function will allow for correlation between consideration sets and preferences, but is not going to distinguish what could be attributed to search costs, mistakes, or other information frictions that have been documented in the literature (see Fabre et al. (2024) for example). My model will hold fixed this deviation treating it as a primitive of the model and assuming that different designs of the RR policy do not affect other frictions. After estimation, I show results that are in line with this assumption, which help alleviate concerns in the interpretation of policy counterfactual scenarios.

4.2 Academic outcomes

I measure academic outcomes using an "on-time" graduation indicator, defined as completing the enrolled program within seven years, following Kapor et al. (2022). Most programs in Chile last five years, with medicine lasting seven, so this window captures timely completion for the large majority of students. I model graduation with a human capital production function as shown in Equation 4. A student i enrolled in program j graduates if the latent variable h_{ij} is greater than zero:

$$h_{ij} \quad = \delta_j^h + \phi^z z_i + \phi^w w_{ij} + \underbrace{\rho u_{ij} + \tilde{\eta}_{ij}}_{\eta_{ij}}, \tag{4}$$

[7]Deviations from rational expectations of this kind are discussed in studies like Kapor et al. (2020) and Fabre et al. (2024).

where z_i and w_{ij} are as defined before, and η_{ij} is allowed to be correlated with the utility u_{ij}, with $\eta_{ij} \mid u_{ij} \sim \mathcal{N}(\rho u_{ij}, \sigma_\eta^2)$. Most covariates that enter the utility function also enter the human capital equation the random coefficients. This specification allows correlation between unobserved determinants of preferences and graduation, and makes it possible to estimate counterfactual graduation outcomes for each student under alternative designs of the RR policy.

5 Identification and Estimation

The distributions of the utility latent variable and consideration latent variable are identified if two excluded shifters, one per equation, exist. The argument depends on the conditional independence of the shifters to the unobserved shocks in the utility and consideration equations. I use distance as a shifter for preferences, and the weighted score as a shifter for consideration. I estimate separately beliefs and the main model equations. For beliefs I follow Agarwal and Somaini (2018), and for the parameters of the model I use a Gibbs Sampler.

5.1 Identification

In this section I outline the identification arguments for the joint distribution of preferences and consideration index, and for the human capital production function.

Utility and consideration equation

The primitive being identified is the joint distribution of utility and consideration latent variables, $F(u, c)$. To identify it, following Agarwal and Somaini (2022), two excluded shifters are needed. The identification argument relies in two match-specific excluded shifters that enter one equation but are absent of the other, and vice versa. For preferences, the shifter will be the geographical distance between the student i and the program j, d_{ij}. For the consideration index the excluded shifter is the weighted score of student i to program j, s_{ij}.

The shifters work by tracing out the distribution of one latent variable while holding fix the other one. To make the argument clearer, assume there is only one program and the outside option. Lets take the share of the outside good at one point, $s_0(\bar{d}, \bar{s})$ for distance $\bar{d}$ and weighted score $\bar{s}$. The share of the outside good is going to be shifted

when either distance or the weighted score changes. If distance increases to $\bar{d} + \Delta$, the share of the outside good will increase to $s_0(\bar{d} + \Delta, \bar{s})$ since less students are going to be applying to the program. The students leaving to the outside good are only students that had the program in their consideration set at the point $s_0(\bar{d}, \bar{s})$. By a similar argument, shifting down the weighted score to $\bar{s} - \Delta$ will trace out the share of students that liked the program more than their outside good at the point $s_0(\bar{d}, \bar{s})$.

The argument is show visually in Figure 3. The extension of the identification argument to more than one program is done by induction, and the formalization of these argument and the assumptions needed can be found in Appendix E and Agarwal and Somaini (2022).

(a) Starting point **(b)** Shift distance **(c)** Shift weighted score **(d)** Trace share

Figure 3: Illustrative example for identification in the case of one program.

The first assumption needed is that the shifters, conditional on the rest of the covariates included, are independent from the unobserved shocks.

Utility shifter. Distance between the student's and the program's municipalities, d_{ij}, shifts preferences but not consideration. Admission depends on weighted scores, not geography, so proximity does not raise expected admission. Students might still know more about programs in their region. To address this, I include a dummy variable indicating if the student and program share the same region, in both the utility and consideration equations. After this control, the remaining variation in d_{ij} enters the utility equation, leaving the consideration index unchanged.

Consideration shifter. Once student covariates, including raw test scores, are controlled for both in the utility and consideration equations, the remaining variation in weighted score s_{ij} affects only the consideration index and leaves indirect utility unchanged. The identifying variation stems from two sources: i) how different programs weight various elements of the PSU test, GPA, and the RR; ii) variation in the students own scores for the GPA and RR items.

Coefficient identification. After the shares are identified, I assume there exist a function connecting shares and parameters and that this function is invertible. Much like the argument of Berry and Haile (2014), this invertibility assumption and the conditional independence of the covariates with the unobserved shocks allows me to estimate coefficients for both equations. Inverted shares will correspond to the latent variables u_{ij} and c_{ij}. Program fixed effects (δ_j^u, δ_j^c) are identified from the conditional mean utility and mean consideration. Student specific (ψ, γ^z) and match specific (β^w, γ^w) coefficients are identified from the correlation between utility and consideration latent variables and individual and match observable characteristics. Coefficients governing unobserved heterogeneity in tastes for program characteristics are identified by the variation of such characteristics within each student application lists.

Human capital production function

For the human capital production function, there are two sources of bias that need to be taken care of. The first one is selection bias. As the econometrician only sees graduation outcomes for students that are assigned to a specific program. The second one is that once that preferences are accounted for, there is still the need for an additional shifter that is excluded from preferences to pin down the graduation outcome coefficients.

Selection bias. To account for the selection bias, I jointly estimate the graduation equation together with the utility function and explicitly control for the utility into the graduation function. This is in the spirit of Dale and Krueger (2002) to control for unobserved preference shocks in the production of human capital, and compare students conditioning on utility levels and application decisions. Program level fixed effects capture "human capital productivity" and are identified from the average graduation rates at each program. Student level covariates, specifically those related to test scores, proxy for student ability. Finally, match specific covariates proxy for possible complementarities between student characteristics and program characteristics. An important thing to note is that absent from an additional shifter, these parameters are not identified.

Additional shifter. For the human capital production function, identification relies on realized cutoffs rather than expected cutoffs. In the CCAS, otherwise similar students are assigned to programs discontinuously as a function of PSU scores. This discontinuity has been widely used in regression-discontinuity designs in Chile and other matching settings

to recover local average treatment effects of program assignment on outcomes such as graduation. In this paper I need to identify the distribution of graduation outcomes, which requires going beyond local effects. To identify the distribution of graduation rates under counterfactual RR policies, a "choice shifter" is required that separates the variation in the human capital production function from the variation in utility (Agarwal et al., 2020). Following Kapor et al. (2022), I use panel variation in program cutoffs: conditional on preferences, observably identical students face different assignments across years because programs changed their admission weights between 2012, 2013, and 2014.

The argument relies on observing students with the same utility level predicted by their observables, in different years being admitted or rejected due to the changes in realized cutoffs. The only reason these students would end with different graduation outcomes is due to the shift in the cutoff, which does not depend on their own utility for programs. The variation between graduation outcomes and student characteristics conditional on the utility of the program then will aid the identification of the human capital production function. Counterfactual graduation outcomes are predicted using the index structure of the human capital production function.

5.2 Estimation

The model is estimated in two stages. A first stage produces estimates of the rationally expected cutoff scores following Agarwal and Somaini (2018). A second stage uses a Gibbs Sampler following McCulloch and Rossi (1994) to estimate parameters of the utility and consideration equations.

Rational Expectations estimation

I estimate expected cutoff scores using the bootstrap method from Agarwal and Somaini (2018). This approach rests on two assumptions: students form rational expectations about cutoffs, and the distribution of cutoffs is independent across programs.

The estimation uses a bootstrap with $B = 10,000$ replications. In each replication b, I sample N students with replacement, run the CPDAA mechanism to get an allocation μ^b, and find the cutoff for each program j. This cutoff is the minimum score among students assigned to that program:

$$P_j^b = \min\{s_{ij} : i \in N^b,\ \mu^b(i) = j\}$$

The expected cutoff for program j is the average of these cutoffs over all replications:

$$\mathbb{E}(\text{cutoff}_j) = B^{-1} \sum_{b=1}^{B} P_j^b$$

Preferences and consideration equations

For the estimation of the preferences and consideration equations, I estimate the model using a Gibbs sampler on the 2013 and 2014 application data and regard the results as approximate maximum–likelihood estimates (van der Vaart, 2000). The Gibbs sampler allows me to deal with the curse of dimensionality due to the large number of potential consideration sets (see Agarwal and Somaini (2022), He et al. (2023)). I adapt the sampler in McCulloch and Rossi (1994) to account for latent consideration sets. I parametrize the distributions of the unobserved portions of the equations as normal distributions. I allow random coefficients for the program characteristics in the utility function to capture more flexible substitutions patterns. I include program specific fixed effects in both the preference equation and in the consideration equation. As is common in discrete choice based demand systems, I interpret the choice specific fixed effect in the utility function as the mean quality (or *vertical* quality) parameter.

With all these, and including explicitly the shifters for preferences and consideration equations, the system of equations to be estimated is:

$$\begin{aligned}
u_{ij} &= \delta_j^u + \theta_i^x x_j + \beta^w w_{ij} + \beta^d d_{ij} + \varepsilon_{ij}, \\
u_{i0} &= \psi + \psi^z z_i + \varepsilon_{i0}, \\
c_{ij} &= \delta_j^c + \gamma^z z_i + \gamma^w w_{ij} + \gamma(s_{ij} - \mathbb{E}[\text{cutoff}_j]) + \nu_{ij}, \\
h_{ij} &= \delta_j^h + \phi^z z_i + \phi^w w_{ij} + \rho\varepsilon_{ij} + \tilde{\eta}_{ij}.
\end{aligned}$$

I assume that the error terms $\varepsilon_{ij}, \varepsilon_{i0}, \nu_{ij}, \tilde{\eta}_{ij}$ are all independent. As mentioned in the model section, I further assume that they are distributed as follows: $\varepsilon_{ij} \sim \mathcal{N}(0, \sigma_\varepsilon^2)$, $\varepsilon_{i0} \sim \mathcal{N}(0, \sigma_0^2)$, $\nu_{ij} \sim \mathcal{N}(0, \sigma_\nu^2)$, and $\tilde{\eta}_{ij} \sim \mathcal{N}(0, \sigma_\eta^2)$. I assume the random coefficients are distributed as $\theta_i^x \sim \text{MVN}(0, \Sigma)$. The parametric assumptions on the error terms allow to use a Gibbs sampler because under conjugate prior distributions, the conditional posterior distributions of the latent error terms and random coefficients given the rest of the terms have a closed form.

The conditional posterior distribution for each parameter $(\delta^u, \beta, \Sigma, \psi, \delta^c, \gamma, \delta^h, \phi)$ has a closed form. This structure permits an iterative procedure to generate a Markov Chain of draws. The chain converges to the posterior distribution. By the Bernstein-von Mises theorem, this posterior is asymptotically equivalent to the maximum likelihood estimator (van der Vaart, 2000, Theorem 10.1). The mean of these draws provides the point estimate, and their covariance estimates the asymptotic covariance. Data augmentation step to avoid calculating conditional choice probabilities. Truncation of bounds in a similar way of Kapor et al. (2022). Further details on the Gibbs sampler are provided in Appendix A.

6 Main Results

This section presents model estimates for 2012, 2013, and 2014. We focus on equation shifters, differences between public, voucher, and private high-school students, and the link between program quality and average consideration. Full tables appear in Appendix C. The estimates fit the data: preference and utility shifters have expected signs and show statistical and economic significance. The correlation coefficient between the utility for one program and human capital production function is positive. I find a negative link between program quality and consideration—students list fewer high-quality programs. Consideration sets differ by student type; public and voucher school students consider fewer programs than private school students, and the RR policy narrows this gap from 2013 to 2014.

6.1 Estimation results

Table 2 reports estimates for the preference and consideration equations. Columns (1), (3), and (5) show point estimates for 2012, 2013, and 2014, with standard errors in columns (2), (4), and (6). The signs match theory: distance lowers inside-good utility and weighted score raises the chance of consideration. Parameters are scaled by the variance of the unobserved terms in each equation, setting $\sigma_e = 1$ for $e \in \{\varepsilon, 0, \nu, \eta\}$. The distance parameter is about 0.39–0.46 standard deviations of the preference shock, and the weighted score parameter is 1.1–1.6 standard deviations of the consideration shock. Estimates are stable across years, even though admissions rules changed in 2013 with the RR policy and expanded in 2014. This shows the primitives are robust to these policy changes. The full table for utility parameters is in C.1 and for the consideration equation in C.2.

Table 3 reports estimates for the human capital production function. This equation is identified from variation across years, so one set of parameters is estimated for the three years. The correlation between the human capital production function and the utility function is positive and significant at 0.1. The sign is consistent with Kapor et al. (2022) for 2010–2012, though the magnitude is smaller. On average, male students graduate less than female students, and private school students graduate less than public and voucher school students. The full table is in C.3.

Table 2: Selected Preference and Consideration Parameter Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	2012		2013		2014	
Variable	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Inside Good (β)						
Net Price	-0.055	(0.002)	-0.043	(0.001)	-0.031	(0.003)
Distance	-0.430	(0.002)	-0.393	(0.002)	-0.394	(0.002)
Outside Good (ψ)						
Sex	1.095	(0.011)	0.537	(0.020)	0.362	(0.033)
Private	0.255	(0.022)	-0.393	(0.034)	-0.253	(0.039)
Recent Grad.	0.029	(0.017)	-0.077	(0.012)	-0.012	(0.011)
Consideration Eq. (γ)						
Weighted Score	1.281	(0.006)	2.304	(0.014)	2.357	(0.012)
Same Region	0.370	(0.004)	0.403	(0.002)	0.375	(0.002)
Sex	0.452	(0.005)	0.064	(0.017)	0.012	(0.024)
Private	0.212	(0.014)	-0.352	(0.022)	-0.251	(0.032)
Recent Grad.	-0.073	(0.006)	-0.222	(0.008)	-0.224	(0.003)

Figure 4 shows estimations and comparisons for 2012, 2013, and 2014 program specific parameters in both utility and consideration equations. The mean quality parameters δ^u for 2012, 2013, and 2014 are shown in Figure 4a, while the fixed effect in the consideration equation for the same years is shown in Figure 4b. Parameters for both equations are around the 45-degree line, although there is significantly more variation in the consideration equation fixed effects. The stability in the mean quality parameters and greater variability in the consideration equation fixed effects highlight the fact that the parameters estimated in the utility function correspond to primitives, while in the consideration equation is a reduced form. The key assumption for counterfactuals is that

Table 3: Selected Human Capital Production Function Parameter Estimates

	(1)	(2)
	Panel Estimation	
Variable	Coeff.	Std. Err.
Phi Parameters (ϕ)		
ρ	0.169	(0.011)
Same Region	0.034	(0.008)
Net Price	-0.081	(0.004)
Sex	-0.294	(0.007)
Private	-0.000	(0.009)

this variation is not driven by changes in the RR policy and it's effect on expectation about cutoffs.

The mean quality parameters estimates are sensible. Both figures, 4a and 4b, highlight in red the programs corresponding to two of the most prestigious universities in Chile, Pontificia Universidad Catolica de Chile (PUC-Chile, QS ranking 93) and University of Chile (QS Ranking 139) and their programs are on the high-quality side of the parameters. Also their programs are among the most competitive, which aligns with their consideration fixed effects being low relative to other programs.

(a) Scatter plot for δ^u **(b)** Scatter plot for δ^c

Figure 4: Comparison of δ^u and δ^c for the years 2012, 2013, and 2014.

6.2 Fit of the model

To assess model fit, I start by evaluating how well it predicts the average PSU mathematics and verbal scores of admitted students at the program level. For each program and year, I compute the average score in the actual data and compare it with the model prediction. Predictions are obtained by simulating ranking lists from estimated preferences and consideration sets, running the CPDAA to determine admissions, and then computing the average score of admitted students in the simulation. Using the human capital production function, I also generate predicted graduation probabilities to compare with actual graduation outcomes.

Figure 5 presents the results on average scores for 2012, 2013, and 2014. The model does not explicitly target this statistic, yet it predicts program-level averages with good accuracy. Similar patterns hold for the median score. Results for the minimum and maximum scores are less precise, reflecting greater noise in the tails of the distribution.

Table 4 shows actual and predicted graduation rates. At the overall level, predictions are within one percentage point of the data. For subgroups, the fit is also close: the model tends to slightly overpredict for private schools and underpredict for public schools and female students. Figure 6 compares predicted and actual graduation rates by program. The scatter plot indicates a good average fit, though with more noise than in the case of admitted student scores. The binscatter lies close to the 45-degree line, consistent with accurate overall calibration.

(a) Model fit 2012 **(b)** Model fit 2013 **(c)** Model fit 2014

Figure 5: Model fit for mean scores across years 2012–2014.

Table 4: Actual vs. Fitted Graduation Rates (2012–2014)

	2012			2013			2014		
Group	Actual	Fitted	Diff	Actual	Fitted	Diff	Actual	Fitted	Diff
Overall	0.399	0.388	-0.01	0.356	0.366	0.009	0.37	0.363	-0.007
Public	0.391	0.367	-0.024	0.349	0.348	-0.001	0.357	0.348	-0.01
Voucher	0.397	0.391	-0.006	0.356	0.365	0.009	0.374	0.363	-0.01
Private	0.411	0.411	0.0	0.366	0.392	0.026	0.375	0.382	0.007
Male	0.322	0.316	-0.006	0.282	0.302	0.02	0.293	0.298	0.005
Female	0.479	0.456	-0.024	0.435	0.426	-0.009	0.45	0.425	-0.025

(a) Model fit 2012 **(b)** Model fit 2013 **(c)** Model fit 2014

Figure 6: Model fit for graduation rates across years 2012–2014.

6.3 Correlation between program quality, probability of consideration, and graduation

Figure 7 documents two key facts. First, higher-quality programs (higher δ^u) are less likely to appear in students' consideration sets. Second, these same programs display higher graduation rates. These patterns arise because higher quality programs attract students with higher scores, which raises admission expected cutoffs and lowers the chance of admission for lower score students. This reflects both the positive coefficient on weighted score in the consideration equation and the correlation across equations through shared covariates. Figures 7 and C.1 also show that, except for the highest quality programs, the 2013 implementation and 2014 expansion of the RR policy slightly increased the probability of consideration.

(a) 2012: high quality → low consideration, high graduation

(b) 2014: RR reform slightly increased consideration at most quality levels

Figure 7: Program quality δ^u, average consideration, and graduation. Gray dots are programs; colored dots are local averages where color indicates graduation rates.

Heterogeneity between public and private schools

To unpack these patterns, Figures 8 and 9 examine heterogeneity by school type and gender. Consideration declines with program quality for all groups, but the level differences are large by school type and small by gender. Private school students consider more programs than public and voucher students, with a gap of about 19 percentage points at the lowest quality levels and about 5 points at the highest quality levels. By gender, male students consider more programs than female students, although the gap is much smaller: about 3 points at low quality, narrowing to around 1 point at high quality.

Graduation outcomes follow a different pattern. Voucher students graduate at higher rates than public school students at all quality levels, with a stable gap of about 3 percentage points. Private school students graduate at rates similar to public students at low-quality programs but converge toward voucher students at the top of the quality distribution. Gender differences are much larger: women graduate at rates 9–12 percentage points higher than men across programs, with the gap shrinking slightly toward higher quality programs.

(a) By school type **(b)** By gender

Figure 8: Average consideration probability by program quality decile in 2014. Left: comparison across school types (public, voucher, private). Right: comparison across gender (male, female).

(a) By school type **(b)** By gender

Figure 9: Observed graduation rates by program quality decile in 2014. Left: comparison across school types (public, voucher, private). Right: comparison across gender (male, female).

7 Effect of the policy and counterfactual scenarios

In this section I evaluate the effects of the Relative Ranking (RR) policy using two counterfactual exercises. First, I compare outcomes for the same population of students under admission systems with and without the RR rule, considering both the 2013 uniform implementation and the 2014 expanded version. Second, I simulate scenarios in which the RR weight is progressively increased across all programs, from 10% to 90%. These exercises reveal the impact of the policy as implemented, and the potential gains and trade-offs of more aggressive designs.

In all counterfactuals I compare student utility and graduation outcomes, both overall and across groups by school type and gender. For the average student, the policy raises utility and only slightly reduces graduation. These aggregate effects mask important heterogeneity. Students from public and voucher schools gain in both outcomes, while private school students lose, with sharp declines at high expansion levels. By gender, women experience large utility gains and modest increases in graduation, while men lose on both dimensions. The results show that an expanded RR benefits women and students from public and voucher schools, and harms men and private school students, with the overall trade-off being one of higher utility at the cost of slightly lower graduation.

7.1 Defining Student Welfare and the Counterfactual Framework

The estimated model allows to compute measures of welfare for an assignment using the distribution of student preferences. For a given match $\mu : \mathcal{I} \to \mathcal{J} \cup \{0\}$, a specific students' utility is measured as the estimated version of equation (1). Define students' net utility as the difference between the utility the student gets from their match minus the utility they derive from the outside good (equation (2)). To use a measure that can be compared across years and individuals, I normalize the net utility using the estimated distance coefficient. The average student welfare is:

$$W(\mu) = \frac{1}{|\mathcal{I}|} \sum_{i \in \mathcal{I}} \frac{(u_{i\mu(i)} - u_{i0})}{|\beta^{distance}|}. \tag{5}$$

Counterfactual computation

Counterfactual estimations rely on the fact that students adapt their consideration sets when admissions policies change. A new policy shifts application scores and expected

cutoffs, which in turn alters students' applications. Some programs that were previously skipped may now be considered, and this process continues until applications and cutoffs are consistent. In equilibrium, the effect of the policy operates through changes in students' subjective choice constraints.

Formally, counterfactual estimations use a fixed point approach for equilibrium beliefs, where two objects in the consideration equation, $c_{ij} = \gamma(s_{ij} - \mathbb{E}[\text{cutoff}_j]) + \chi_{ij} + \nu_{ij}$, will change. The first is the student's application score, s_{ij}, which affects both the consideration equation and student priorities in program applications. The algorithm starts with initial expected cutoffs $\mathbb{E}[\text{cutoff}_j]^0$. It then simulates the CPDAA for B bootstrapped samples, generating B cutoff vectors to find each program's average and standard deviation cutoffs. This iterative simulation continues until the maximum absolute difference between the current and previous iterations' expected cutoffs for all programs $j \in \mathcal{J}$ is less than a predefined tolerance ε, i.e., $\max_{j \in \mathcal{J}} \left\{ |\mathbb{E}[\text{cutoff}_j]^b - \mathbb{E}[\text{cutoff}_j]^{b-1}| \right\} < \varepsilon$.

7.2 Effect of the Implemented RR Policy

The evaluation of the effects of the policy implementation requires the following counterfactual exercise: for the same population of students, how would their admissions (and utility) look like in presence and absence of the RR policy. All counterfactuals are conducted in the 2014 sample. For comparison, the baseline welfare is the average under the 2012 scenario, prior to the RR policy. Two types of effects are going to be evaluated: "mechanical effect" and "full effect". The "mechanical effect" assumes students do not update their ROLs, while the "full effect" accounts for ROL updates due to changes in cutoff beliefs. The two policy implementations evaluated are the 2013 and 2014 RR. The 2013 RR used a uniform 10% weight on the RR component. In contrast, the 2014 RR used varying weights, from 10% to 40%, determined by each program.

Overall Welfare Effects

Figure 10a shows that both the 2013 and 2014 implementations of the RR policy increased average student welfare. Accounting for students equilibrium responses to the policy is crucial: ignoring them would substantially understate the effect. Relative to the 2012 baseline, the full impact amounts to 0.02 km in welfare gain measured as willingness to travel under the 2013 rule and 0.04 km under the 2014 rule. In both cases, the full effect substantially larger than the purely mechanical effect.

In contrast, Figure 10b shows a null reduction in graduation outcomes. The 2013 implementation lowered the average graduation rate by -0.007 percentage points, while the 2014 increased it by 0.02 percentage points. These effects are negligible in magnitude compared to the average graduation rates in the sample, which are around 37.4%.

(a) Welfare effects **(b)** Graduation effects

Figure 10: Effects of the RR policy on student welfare (left) and graduation (right).

Average welfare effects are small and close to zero in terms of graduation, but there is meaningful heterogeneity across students. Figure 11 shows the change in utility, measured as willingness to travel, between the 2012 and 2014 RR admission policies. The policy affects students who switch among programs and those who move in or out of the system. Students who gain from the policy by moving to more preferred programs increase their utility by about 1.4 kilometers. Students who enter from the outside option gain about 1.3 kilometers compared to their previous outside good utility. Students who lose from the policy see average decreases of 1.5 kilometers when switching to less preferred programs, and 1.4 kilometers when exiting the system.

Characterizing winners and losers from the policy

This section explores the heterogeneity of the policy's impact, identifying which groups of students benefited and which were adversely affected.

Table 5 and Figure 12 summarize how welfare changed across school types after the policy's implementation. Most of the students who benefited came from public or voucher schools. On average, these students gained about 1.40 km in welfare—measured as willingness to travel—while private school students gained slightly more (1.57 km) but represented a smaller share of winners. Conversely, students who lost from the reform experienced comparable welfare declines: about –1.41 km for public and voucher students and –1.63 km for private school students.

Figure 11: Changes in utility measured as willingness to travel for students that change placements between 2012 and 2014.

Note: The figure shows the distribution of the change in utility, measured as willingness to travel (in kilometers), between the 2012 and 2014 RR admission policies. Blue bars represent students who became worse off under the new policy, and red bars represent those who became better off. Values to the right of zero indicate gains in utility; values to the left indicate losses.

Table 5: Better-off vs. Worse-off by School Type (RR Reform)

	Public/Voucher		Private	
	Δ Utility	N students	Δ Utility	N students
Better-off	1.40	11,124	1.57	1,994
Worse-off	-1.41	6,810	-1.63	3,238

Note. This table reports mean utility changes (Δ) and the number of students for groups who were better- or worse-off after the RR reform, split by school type.

Figure 12: Utility changes with the RR policy by school type

Figure 13 shows that the majority of students (approximately 78%) were unaffected by the reform. About 12.5% improved their placement ("better-off"), and 9.6% were "worse-off." Among those who gained, 85% attended public or voucher schools, and nearly 60% were women. Among those who lost, 22% came from public schools, 46% from voucher schools, and 32% from private schools.

Table 6 provides additional information on these groups. Within the better-off group, women comprise 59%, compared with 51% of the applicant pool. Private school students, though making up about 20% of applicants, represent 32% of those negatively affected and 15% of those who gained. Roughly 64% of all students received an RR-based score boost, and the policy successfully reached its intended beneficiaries: 86% of winners received a boost, compared with 48% among losers. The average boost was 58 points for winners (about 0.53 PSU standard deviations) and 11 points for losers (0.10 SD).

Overall, these results indicate that the reform effectively favored high-performing students from public and voucher schools—its explicit target group—while private school students were more likely to be displaced to less preferred outcomes.

(a) Winners and losers **(b)** By school type **(c)** By gender

Figure 13: Winners and losers from the implementation of the 2014 RR policy

Graduation outcomes

Average effects on graduation outcomes hide important differences across students. Figure 14 shows that some students gained from the policy by moving to programs with higher graduation rates, while others lost by being displaced from programs they preferred. Panel 14a shows the distribution of changes in graduation rates for students who switched between programs. Panel 14b shows how moves to or from the outside option relate to graduation outcomes. Among students who switched programs, those who gained access to a more preferred program increased their graduation probability by about 3.1 percentage points. Those moved to less preferred programs saw a drop of about

Table 6: Descriptive statistics by neutral, worse-off, and better-off students

			Worse-off			Better-off		
Variable	Overall	Neutral	Out	Switch	Total	In	Switch	Total
N students	105,155	81,989	2,390	7,658	10,048	2,243	10,875	13,118
Students %	100.00	77.97	2.27	7.28	9.56	2.13	10.34	12.47
Utility Δ (kms)	0.04	0.00	-1.40	-1.50	-1.48	1.31	1.44	1.42
Grad. Δ (p.p)	-0.06	0.00	-49.59	-3.72	-14.63	47.90	3.11	10.76
Public sch. (%)	25.84	25.84	21.09	22.32	22.02	29.56	28.58	28.75
Voucher sch. (%)	53.88	54.53	48.12	45.01	45.75	55.46	56.17	56.05
Private sch. (%)	20.29	19.64	30.79	32.67	32.23	14.98	15.25	15.20
Female %	51.42	51.38	39.75	42.07	41.52	63.04	58.46	59.24
Got RR boost (%)	64.04	62.28	33.72	52.10	47.73	78.91	89.36	87.57
Boost	27.04	24.04	6.66	13.05	11.53	48.52	59.60	57.70

Note. This table reports descriptive statistics of students grouped according to their utility change between the baseline admissions criteria in 2012 and the implementation of RR 2014. "Better-off" are students with a positive change in utility, "Worse-off" those with a negative change, and "Neutral" those with no change. "Switch," "in," and "out" indicate whether the student moved into a program from the outside good, or was displaced to the outside good. Except for the row "Students %" which is the share of students relative to the total of students, shares are expressed in percent of the total of students in each column.

3.7 percentage points. Students entering from the outside option moved to programs with an average graduation rate of 47.9%. Students displaced to the outside option lost access to programs where their average graduation rate had been 49.6%.

(a) Switchers between programs **(b)** Moving from or to the outside good

Figure 14: Graduation outcome changes for students that gain and lose access to preferred choices with the policy

Mismatch decomposition

Figures 10b and 14 show graduation changes after the implementation of the policy. This section decomposes these changes in two: program effects and match effects. To study mismatch in this model, I perform a decomposition to separate two things that contribute in the graduation outcome. To perform this decomposition, I divide the human capital latent variable in equation (4) in to program effects and match effects. This is:

$$\Pr(h_{ij} > 0 \mid \delta^h, z, w, u) = \Phi\left(\underbrace{\delta_j^h + \rho\bar{u}_{\cdot,j}}_{Program} + \phi^z z_i + \underbrace{\phi^w w_{ij} + \rho(u_{ij} - \bar{u}_{\cdot,j})}_{Match}\right),$$

where $u_{\cdot,j}$ is the average of utilities across all individuals for program j. The first is the program effect, which is governed by δ_j^h and the average utility for each program j. The second is the match effect, which is governed by the $\phi^w w_{ij}$ and the individual specific deviation from the average utility for program j. This decomposition is done for students that switch programs, and is done by

To measure the role of each dimension, I compute counterfactual graduation probabilities that keep either the program or the match component fixed at its 2012 level while updating the other to its 2014 level. This produces four predicted outcomes: the baseline (both 2012 levels), the program-updated case (2014 program with 2012 match), the match-updated case (2012 program with 2014 match), and the full 2014 case.

I then apply a Shapley decomposition to separate total changes in predicted graduation probabilities into program and match components. Using the model in equation (4), let the predicted probability in year t be

$$\begin{aligned}\Delta^{\text{prog}} = \tfrac{1}{2}\Big[&p(\delta_j^{h,2014}, z_i, w_{ij}^{2012}, u_{ij}^{2012}) - p(\delta_j^{h,2012}, z_i, w_{ij}^{2012}, u_{ij}^{2012}) \\ &+ p(\delta_j^{h,2014}, z_i, w_{ij}^{2014}, u_{ij}^{2014}) - p(\delta_j^{h,2012}, z_i, w_{ij}^{2014}, u_{ij}^{2014})\Big],\end{aligned}$$

$$\begin{aligned}\Delta^{\text{match}} = \tfrac{1}{2}\Big[&p(\delta_j^{h,2012}, z_i, w_{ij}^{2014}, u_{ij}^{2014}) - p(\delta_j^{h,2012}, z_i, w_{ij}^{2012}, u_{ij}^{2012}) \\ &+ p(\delta_j^{h,2014}, z_i, w_{ij}^{2014}, u_{ij}^{2014}) - p(\delta_j^{h,2014}, z_i, w_{ij}^{2012}, u_{ij}^{2012})\Big].\end{aligned}$$

Δ^{prog} measures the average contribution of changes in program characteristics, while Δ^{match} measures the contribution of changes in student–program matches. Their sum equals the total change in predicted graduation probability between 2012 and 2014.

Figure 15 shows that match and program effects move in the same direction, with most of the change in graduation outcomes explained by the program effect. This means that, on average, moving to a preferred program improves both match quality and program quality, so the mismatch hypothesis does not hold. Figure 16 breaks the results by school type and policy outcome. The pattern is consistent across groups: program effects dominate match effects. The only exception is public school students who lose from the policy. For them, the match effect is slightly positive but much smaller than the negative program effect, which drives a net decline in outcomes. Overall, the results show that differences in program quality, rather than changes in match quality, are the main source of variation in human capital gains.

Figure 15: Decomposition of the human capital change.

Note: The figure shows the decomposition of the change in graduation outcomes into program and match effects for students who changed programs between 2012 and 2014 under the RR policy. Bars report average effects in percentage points for students who were better off (left) and worse off (right) after the policy change.

7.3 Affirmative Action policy

To estimate the effect of an Affirmative Action policy, I take an existing program in Chile called Beca de Excelencia Académica (BEA). This policy awards scholarships to the top 10% of students within each high school cohort from public and voucher schools (BEA students) and allocates reserved seats (cupos supernumerarios) in university programs. During the years of analysis, the reserved quotas were applied in a limited scope and filled sequentially after the regular admission process. I implement a counterfactual experiment where these quotas are expanded to represent 10% of each program's total

Figure 16: Decomposition of the human capital change by school type and policy outcome.

Note: The figure decomposes the change in graduation outcomes into program and match effects separately for better-off and worse-off students, distinguishing between private and public (or voucher) school backgrounds. Bars show mean differences in graduation probabilities (percentage points) by effect type.

capacity. In the counterfactual allocation, programs first reserve this 10% share for BEA students, who compete exclusively among themselves according to their weighted admission scores. The remaining 90% of seats are then allocated through the regular centralized admission mechanism. This approach preserves the structure of the system and mirrors the actual sequencing and treatment of supernumerary quotas in Chile's admissions process, while allowing me to quantify the equilibrium effects of scaling up the affirmative-action component of the BEA policy.

Affirmative Action versus Relative Ranking welfare effects

Figure 17 compares the average effects of the Relative Ranking (RR) and Affirmative Action (AA) counterfactuals. Under RR, 12.5% of applicants gain and 9.6% lose, with an average welfare gain of 0.04 km (with ROL updates). Under AA, 8.9% gain and 15.9% lose, with an average welfare loss of 0.055 km. Among those who gain under AA, 93% come from public or voucher high schools, while under RR, 84% of gainers are from these schools. Figure 17a shows the average effects of both policies, and Figure 17b presents the distribution of utility changes. The distributions confirm the pattern: AA benefits fewer students but harms more compared to RR.

For students who move to a higher-utility option, the average gain is +2 km under AA and +1.5 km under RR. For those moving from the outside option, welfare gains are +1.8 km under AA and +1.3 km under RR. Losses for students made worse off are

similar across policies: those displaced to a less preferred program lose about −1.43 km under AA and −1.46 km under RR. Students displaced to the outside option lose about −1.35 km under AA and −1.40 km under RR.

(a) Overall effect

(b) Changes in utility between AA and baseline for students that move

Figure 17: Affirmative Action and RR effects compared

(a) Winners and losers

(b) By school type

(c) By gender

Figure 18: Winners and losers from the implementation of the 2014 AABEA policy

Affirmative Action versus Relative Ranking graduation effects

The average effect of the policy on graduation for students who move between programs is negative. Figure 19 and Table 7 show that students who benefit from the policy gain about 2.85 percentage points in graduation probability, while those who lose utility from their placement lose about 2.75 percentage points. Since a larger share of students lose utility, the average effect is a 0.5 percentage point decline in graduation probability. Students who enter a program from the outside option under AA are admitted to programs with a 49.8% graduation probability, while those displaced to the outside option leave programs with a 47.5% graduation probability. These results indicate that the AA policy leads to worse graduation outcomes than the RR policy.

(a) Switchers between programs

(b) Moving from or to the outside good

Figure 19: Graduation outcome changes for students that gain and lose access to preferred choices with the AA policy

A decomposition of graduation outcomes, similar to that for the RR policy, reveals clear differences under the AA design (Figure 20). The match effect lowers graduation probabilities for students who gain in utility from the policy (about −1.86 p.p.), but this is more than offset by a positive program effect (+4.71 p.p.), resulting in a net gain of +2.85 p.p. in graduation probability. For students who lose under AA, the effects move in opposite directions: the program effect reduces graduation by about −2.78 p.p., while a small positive match effect partly offsets this loss, yielding an overall decline of −2.75 p.p. This pattern suggests that AA improves outcomes mainly through access to more productive programs, even as match quality declines for some students.

Figure 21 shows that these effects differ by school background. Better-off students from public or voucher schools display larger program effects (+4.79 p.p.) than their private school counterparts (+3.54 p.p.), indicating that AA expands access to more productive programs among its targeted groups. In contrast, worse-off private school students experience stronger negative program effects (−3.02 p.p.) than those from public schools (−2.69 p.p.), suggesting that the reallocation of seats toward disadvantaged students comes at a cost for others. Overall, the AA policy redistributes human capital by improving graduation outcomes for a subset of students while lowering them for a larger group, consistent with the aggregate decline in average graduation rates.

While the RR policy changes weighted scores and expected cutoffs, the AA policy only affects the latter. These changes shift consideration sets and top choices, which in turn re-sorts students in equilibrium.

Figure 20: Decomposition of the human capital change.

Note: The figure shows the decomposition of the change in graduation outcomes into program and match effects for students who changed programs between 2012 and 2014 under the RR policy. Bars report average effects in percentage points for students who were better off (left) and worse off (right) after the policy change.

Figure 21: Decomposition of the human capital change by school type and policy outcome.

Note: The figure decomposes the change in graduation outcomes into program and match effects separately for better-off and worse-off students, distinguishing between private and public (or voucher) school backgrounds. Bars show mean differences in graduation probabilities (percentage points) by effect type.

Table 7: Descriptive statistics by neutral, worse-off, and better-off students

			Worse-off			Better-off		
Variable	Overall	Neutral	Out	Switch	Total	In	Switch	Total
N students	105,155	79,122	4,114	12,571	16,685	1,340	8,008	9,348
Students %	100.00	75.24	3.91	11.95	15.87	1.27	7.62	8.89
Utility Δ (kms)	-0.05	0.00	-1.36	-1.42	-1.40	1.75	1.91	1.89
Grad. Δ (p.p)	-1.34	0.00	-47.54	-2.75	-13.79	49.77	2.85	9.58
Public sch. (%)	25.84	25.47	21.54	22.49	22.25	39.03	34.68	35.30
Voucher sch. (%)	53.88	54.15	52.48	49.18	50.00	54.63	59.18	58.53
Private sch. (%)	20.29	20.38	25.98	28.33	27.75	6.34	6.14	6.17
Female %	51.42	51.25	46.21	47.09	46.87	65.00	60.25	60.93
Got RR boost (%)	64.04	61.99	41.54	59.99	55.44	93.51	97.34	96.79
Boost	27.04	23.88	10.74	19.80	17.56	65.18	71.68	70.75

Note. This table reports descriptive statistics of students grouped according to their utility change between the baseline admissions criteria in 2012 and the implementation of RR 2014. "Better-off" are students with a positive change in utility, "Worse-off" those with a negative change, and "Neutral" those with no change. "Switch," "in," and "out" indicate whether the student moved into a program from the outside good, or was displaced to the outside good. Except for the row "Students %" which is the share of students relative to the total of students, shares are expressed in percent of the total of students in each column.

Table 8: AA and RR Winners, Losers, Neutral status distribution

AA category	RR Winner	RR Neutral	RR Loser	Total
Winner	5,478	3,718	152	9,348
Neutral	6,549	70,567	2,006	79,122
Loser	1,091	7,704	7,890	16,685
Total	13,118	81,989	10,048	105,155

Note. Cross-classification of students by their AA and RR utility-change outcomes.

7.4 Drivers of the policy effect

This section decomposes the drivers of the policy effects for both the Relative Ranking (RR) rule and the Affirmative Action (AA) counterfactual. Both policies affect students' perceived admission probabilities through different mechanics. While the RR rule modifies students' weighted application scores, the AA policy instead introduces reserved seats that shift cutoffs without changing the students' scores themselves. Both policies therefore affect applicants through equilibrium adjustments to expected cutoffs and through changes in their feasible choice sets, but the channels differ in magnitude and responsiveness.

Changes in Scores, Consideration, and Applications

The RR policy operated by changing students' consideration sets, which stemmed from increases in their weighted application scores. The policy raised scores for programs students preferred over their outside good, with larger effects for public and voucher students than for private school students (Table 9, Panel A). On average, scores for public and voucher students increased by 16.3 points (3.0%), compared to 13.0 points (2.2%) for private school students. For students who gained from the policy ("Winners"), the score increase was larger: 30.3 points (5.4%) for the public/voucher school group and 24.9 points (4.1%) for the private school group.

These score increases translated into expanded consideration sets for winners and smaller sets for losers (Panel B). Winners from public and voucher schools increased their consideration share by 4.4 percentage points, a 34% rise from their baseline. Conversely, private school students harmed by the policy ("Losers") saw their consideration share fall by 3.4 percentage points, a 22% reduction from their baseline. This shows a direct behavioral response to the policy's effect on perceived admission chances.

The changes in consideration sets reshaped student applications and their graduation outcomes. Winners from public and voucher schools used their expanded options to apply to programs with higher utility, gaining an average of 0.26 km in their top choice (Table 9, Panel C). In contrast, private school students who lost from the policy applied to programs with lower utility, with an average decrease of 0.28 km. These choices imply changes in academic outcomes for programs at the top of their application list (Panel D). The top choices of public and voucher school winners meant a 1.14 percentage point higher graduation probability. The top choices of private school losers, however, had a

2.18 percentage point lower graduation probability, highlighting the trade-offs inherent in the policy's design.

Table 9: Average score and consideration by school type and outcome group under RR policy (Winner/Loser/Neutral/Total).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Public/Voucher				Private			
Variable	Winner	Loser	Neutral	Total	Winner	Loser	Neutral	Total
Panel A. Scores (all programs where $u_{ij} > u_{i0}$)								
Average Score 2012	561.14	561.53	542.94	546.87	605.88	603.20	586.35	590.73
Average Score with Policy	591.39	566.44	558.00	563.12	630.82	609.62	599.20	603.74
Δ Average Score	30.25	4.91	15.06	16.25	24.94	6.42	12.85	13.01
Panel B. Consideration (all programs where $u_{ij} > u_{i0}$)								
Share Cons. 2012	12.87	12.39	11.27	11.57	15.92	15.48	14.64	14.89
Share Cons. with policy	17.26	10.07	11.69	12.30	19.78	12.11	14.29	14.47
Δ Cons. Share	4.39	-2.31	0.43	0.73	3.86	-3.37	-0.34	-0.41
Panel C. Utility (Top choice in ROL)								
Avg. Utility, 2012	1.36	1.52	1.11	1.17	1.72	1.88	1.42	1.52
Avg. Utility, Policy	1.62	1.29	1.09	1.17	1.95	1.60	1.36	1.45
Δ Utility	0.26	-0.23	-0.02	0.00	0.23	-0.28	-0.06	-0.06
Panel D. Graduation Probability (Top choice in ROL)								
Avg. Grad. Prob., 2012	17.61	18.89	12.22	13.48	27.73	27.95	21.54	23.09
Avg. Grad. Prob., Policy	18.75	17.19	12.06	13.37	29.89	25.77	20.80	22.41
Δ Grad. Prob.	1.14	-1.70	-0.16	-0.12	2.17	-2.18	-0.73	-0.68

Note. Each panel reports group-level means by school type (Public/Voucher vs. Private) and outcome group (Winner, Loser, Neutral, Total). All statistics are expressed relative to the 2012 baseline. Score and consideration averages are computed over the programs that each student preferred to their outside option, and then aggregated by group. Utility and graduation averages are computed using the top three alternatives included in each student's ROL under the respective scenarios.

Changes in Scores, Consideration, and Applications in RR and AA

Unlike the RR policy, the affirmative action (AA) counterfactual does not change students' admission scores. Its impact arises from the structure of the admission quotas. Under AA, the top students from each school (BEA-eligible) compete for a reserved set of seats. These quotas lower the admission cutoffs for eligible applicants, while the remaining regular seats become more competitive. This setting changes application behavior not through score increases, but through shifts in the expected cutoffs that define perceived admission chances.

Table 10: Average score and consideration by school type and outcome group under RR policy.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Public/Voucher				Private			
Variable	High	Med.	None	Total	High	Med.	None	Total
Panel A. Scores (all programs where $u_{ij} > u_{i0}$)								
Average Score 2012	581.69	557.01	506.35	546.87	642.45	620.41	544.80	590.73
Average Score with Policy	616.24	572.09	508.52	563.12	671.81	636.42	548.93	603.74
Δ Average Score	34.55	15.08	2.17	16.25	29.36	16.01	4.13	13.01
Panel B. Consideration (all programs where $u_{ij} > u_{i0}$)								
Share Cons. 2012	15.88	12.31	7.13	11.57	23.24	18.54	8.48	14.89
Share Cons. with policy	19.55	12.26	6.27	12.30	26.70	17.61	7.04	14.47
Δ Cons. Share	3.66	-0.05	-0.86	0.73	3.47	-0.93	-1.43	-0.41
Panel C. Utility (Top 4 in ROL)								
Avg. Utility, 2012	0.96	0.81	0.62	0.79	1.26	1.17	0.80	1.02
Avg. Utility, Policy	1.08	0.79	0.56	0.80	1.37	1.13	0.70	0.98
Δ Utility	0.12	-0.02	-0.06	0.01	0.11	-0.04	-0.10	-0.04
Panel D. Graduation Probability (Top 4 in ROL)								
Avg. Grad. Prob., 2012	15.17	12.34	8.64	11.89	26.12	23.30	14.09	19.71
Avg. Grad. Prob., Policy	16.64	11.80	7.63	11.76	27.79	22.61	12.41	18.98
Δ Grad. Prob.	1.46	-0.54	-1.01	-0.13	1.67	-0.69	-1.68	-0.73

Note. Each panel reports group-level means by school type (Public/Voucher vs. Private) and outcome group (received or not the policy). All statistics are expressed relative to the 2012 baseline. Score and consideration averages are computed over the programs that each student preferred to their outside option, and then aggregated by group. Utility and graduation averages are computed using the top four alternatives included in each student's ROL under the respective scenarios.

Table 11 confirms this mechanism. Panel A shows no change in scores for all groups, while Panels B–D display strong behavioral responses. Public and voucher students who benefit from the reserved BEA quotas expand their consideration sets sharply: their share of considered programs rises by about 12 percentage points, nearly doubling the baseline level. In contrast, private school students and those outside the quota face tighter competition in the regular seats—their consideration shares decline by 3–4 percentage points. These patterns lead to differentiated application outcomes. Winners from public and voucher schools apply to programs with higher utility and graduation probabilities, while private school students apply to less-preferred and lower-graduation programs. The distribution of gains and losses resembles the RR case, but operates through a distinct channel.

The composition of the winning group under the AA counterfactual also differs from that under the RR policy. The AA winners, who are mostly BEA-eligible students, already display higher baseline outcomes and stronger application behavior than RR winners. Before the policy change, they had higher average admission scores, higher probabilities of considering selective programs, and applied to options that delivered greater utility. In contrast, RR winners were students who had performed well within their schools but ranked lower in the national score distribution. This distinction indicates that the AA policy improves outcomes for applicants who were already competitive, while the RR policy shifts opportunities toward high-performing students in less-advantaged contexts.

Figure 22 compares how the RR and AA policies changed the probability of considering programs across the distribution of program quality. Under the RR policy (panel a), public and voucher students expanded their consideration sets—especially toward lower- and mid-quality programs—while private school students showed small or negative changes at the top of the distribution. These shifts are consistent with previous findings: the RR rule not only raised admission scores but also altered beliefs about attainable options, leading disadvantaged students to consider a broader and more selective range of programs. In contrast, the AA policy (panel b) shows the reverse pattern. Because it operates through reserved quotas rather than score adjustments, private school students experienced widespread declines in consideration probabilities, whereas public and voucher students gained only in a few high-quality ranges. Together, the two panels show that both policies changed access by shaping expectations about admission chances, RR broadened access for a wider group of public and voucher students, while AA targeted a smaller set of high-scoring applicants already considering selective programs.

Table 11: Average score and consideration by school type and outcome group under AA policy (Winner/Loser/Neutral/Total).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Public/Voucher				Private			
Variable	Winner	Loser	Neutral	Total	Winner	Loser	Neutral	Total
Panel A. Scores (all programs where $u_{ij} > u_{i0}$)								
Average Score 2012	580.66	552.90	541.01	546.87	616.20	598.91	587.47	590.73
Average Score with Policy	580.66	552.90	541.01	546.87	616.20	598.91	587.47	590.73
Δ Average Score	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Panel B. Consideration (all programs where $u_{ij} > u_{i0}$)								
Share Cons. 2012	14.85	12.00	11.03	11.57	19.44	15.60	14.52	14.89
Share Cons. with policy	27.27	8.97	10.39	11.95	16.91	11.51	12.44	12.36
Δ Cons. Share	12.42	-3.03	-0.65	0.38	-2.54	-4.08	-2.08	-2.53
Panel C. Utility (Top choice in ROL)								
Avg. Utility, 2012	1.47	1.45	1.08	1.17	1.97	1.84	1.40	1.52
Avg. Utility, Policy	1.78	1.21	1.01	1.12	1.91	1.56	1.31	1.38
Δ Utility	0.31	-0.24	-0.07	-0.05	-0.06	-0.28	-0.10	-0.14
Panel D. Graduation Probability (Top choice in ROL)								
Avg. Grad. Prob., 2012	19.43	16.23	12.13	13.48	30.20	26.52	21.85	23.09
Avg. Grad. Prob., Policy	20.22	15.18	10.99	12.56	29.17	24.24	20.09	21.24
Δ Grad. Prob.	0.78	-1.05	-1.13	-0.92	-1.03	-2.28	-1.76	-1.85

Note. Each panel reports group-level means by school type (Public/Voucher vs. Private) and outcome group (Winner, Loser, Neutral, Total). All statistics are expressed relative to the 2012 baseline. Score and consideration averages are computed over the programs that each student preferred to their outside option, and then aggregated by group. Utility and graduation averages are computed using the top three alternatives included in each student's ROL under the respective scenarios.

Table 12: Average score and consideration by school type and outcome group under AA policy.

	(1)	(2)	(3)	(4)	(5)	(6)
	Public/Voucher			Private		
Variable	BEA	Not BEA	Total	BEA	Not BEA	Total
Panel A. Scores (all programs where $u_{ij} > u_{i0}$)						
Average Score 2012	594.57	540.34	546.87	-	590.73	590.73
Average Score with Policy	594.57	540.34	546.87	-	590.73	590.73
Δ Average Score	0.00	0.00	0.00	-	0.00	0.00
Panel B. Consideration (all programs where $u_{ij} > u_{i0}$)						
Share Cons. 2012	17.34	10.78	11.57	-	14.89	14.89
Share Cons. with policy	32.47	9.14	11.95	-	12.36	12.36
Δ Cons. Share	15.13	-1.64	0.38	-	-2.53	-2.53
Panel C. Utility (Top choice in ROL)						
Avg. Utility, 2012	0.99	0.76	0.79	-	1.02	1.02
Avg. Utility, Policy	1.28	0.69	0.76	-	0.92	0.92
Δ Utility	0.29	-0.08	-0.03	-	-0.10	-0.10
Panel D. Graduation Probability (Top choice in ROL)						
Avg. Grad. Prob., 2012	15.56	11.39	11.89	-	19.71	19.71
Avg. Grad. Prob., Policy	17.07	10.36	11.17	-	18.16	18.16
Δ Grad. Prob.	1.51	-1.03	-0.72	-	-1.55	-1.55

Note. Each panel reports group-level means by school type (Public/Voucher vs. Private) and outcome group (Winner, Loser, Neutral, Total). All statistics are expressed relative to the 2012 baseline. Score and consideration averages are computed over the programs that each student preferred to their outside option, and then aggregated by group. Utility and graduation averages are computed using the top four alternatives included in each student's ROL under the respective scenarios.

(a) RR policy. **(b)** AA policy.

Figure 22: Changes in the average probability of consideration by program quality under the RR and AA policies.

Note. Each bar shows the change in the average probability that a program is considered, computed as the difference between post-policy and pre-policy averages (2014 − 2012). Within each school type (public/voucher or private), individual programs are grouped into bins of mean utility (δ^u), estimated from the model. The plotted values correspond to the mean change in consideration probability per bin, expressed in percentage points. Values above zero indicate that programs in that quality range were more likely to be considered after the policy.

Figure 23 complements this comparison by showing how the two policies alter expected cutoffs relative to 2012. Both the RR rule and the AA regular quotas increase expected cutoffs, but the rise is stronger under AA regular seats, reflecting the higher competition among non-reserved applicants. The BEA reserved spots, shown in squares, generate the lowest expected cutoffs. For programs with baseline cutoffs below roughly 550, their expected cutoffs under BEA remain at the minimum possible level, meaning that any eligible student could be admitted. Above that threshold, cutoffs rise but remain below the 2012 values. This pattern explains why BEA students in the AA counterfactual experience large jumps in both program access and final placement quality: the policy effectively reallocated capacity rather than boosting individual scores.

8 Conclusion

This paper studies Chile's Relative Ranking (RR) policy as a group-neutral rule in centralized college admissions. I build and estimate a structural model of higher education applications with endogenous consideration sets and a linked human capital production function. The model reproduces observed application, admission, and graduation patterns and identifies how access rules shape welfare and human capital formation.

The analysis shows that the RR policy increased overall student welfare with negligible effects on graduation outcomes. Relative to a pre-policy baseline, the full 2014 imple-

Figure 23: Binscatter of expected cutoffs of counterfactuals compared to the baseline

mentation raised average welfare by about 0.04 kilometers in willingness to travel, while graduation rates remained essentially unchanged. These average effects mask substantial heterogeneity. Roughly 13% of applicants moved to more-preferred programs, and 86% of these came from public or voucher high schools. Women gained more often than men. The policy's impact operated mainly through shifts in consideration sets—changes in beliefs about attainable programs—rather than mechanical score adjustments.

Graduation outcomes reveal limited evidence of mismatch. A decomposition of human capital changes shows that program quality effects dominate match effects: students who gained access under RR tended to enter higher-productivity programs without major losses in fit. Negative effects on graduation for displaced students were modest and concentrated among private school applicants.

A counterfactual Affirmative Action (AA) policy modeled after the *Beca de Excelencia Académica* (BEA) generates different trade-offs. The AA design benefits a smaller share of students (8.9%) and harms a larger share (15.9%), leading to lower average welfare (-0.055 km) and a net decline of about 0.5 percentage points in graduation probability. For beneficiaries, graduation gains arise mainly from entering more selective programs, but losses among displaced students dominate in aggregate. The policy thus redistributes opportunities more sharply but at higher efficiency cost.

Expanding the RR weight beyond its 2014 level further illuminates the equity–efficiency frontier. Increasing the RR component to 40–50% raises average welfare by 5–7%, with only a 0.02 standard deviation decline in the mean PSU score of admitted students.

Gains accrue primarily to students from public and voucher schools and to women, while private school students and men experience losses. Beyond 60%, however, welfare improvements flatten and graduation probabilities begin to fall, suggesting diminishing returns to expansion.

Together, these results show that group-neutral class-rank rules can enhance educational equity at relatively low efficiency cost when they influence both admission priorities and student application behavior. The evidence contrasts with affirmative-action–style quotas, which achieve similar targeting goals but entail larger trade-offs in welfare and human capital. Future work could study how such policies affect pre-college effort, institutional supply responses, and long-run earnings, extending the analysis of context-sensitive merit rules beyond static welfare and graduation outcomes.

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A Estimation Appendix

A.1 Gibbs Sampler Details

I estimate the model using a Gibbs sampler, which iteratively draws from the conditional posterior distributions of the latent variables and parameters. This approach leverages data augmentation to handle the latent utilities, consideration indices, and human capital outcomes, while incorporating the truncation constraints from optimality conditions. Below, I provide a detailed derivation of the sampler, including the specification of priors and the form of the posterior distributions.

A.1.1 Notation Summary

Before proceeding, Table A.1 provides a summary of the model parameters.

Table A.1: Model Parameters

Parameter	Description
δ_j^u	Fixed effect in utility mean μ_{ij}^u for program j
θ_i	Random coefficients on program characteristics x_j, $\sim \text{MVN}(0, \Sigma)$
β_w	Coefficients on match-specific covariates w_{ij} in utility
β_z	Coefficients on individual covariates z_i in utility
β_d	Coefficient on distance d_{ij} in utility
ψ	Coefficients in outside option mean $X_i^0 \psi$
δ_j^c	Fixed effect in consideration mean μ_{ij}^c for program j
γ_w	Coefficients on match-specific covariates w_{ij} in consideration
γ_z	Coefficients on individual covariates z_i in consideration
γ_s	Coefficient on adjusted score $\tilde{s}_{ij}$ in consideration
δ_j^h	Fixed effect in human capital mean μ_{ij}^h for program j
ϕ_w	Coefficients on match-specific covariates w_{ij} in human capital
ϕ_z	Coefficients on individual covariates z_i in human capital
ρ_u	Correlation parameter between utility u_{ij} and human capital h_{ij}
ρ_c	Correlation parameter between consideration c_{ij} and human capital h_{ij}
σ_ϵ^2	Variance of utility error ϵ_{ij}
σ_ν^2	Variance of consideration error ν_{ij}
σ_η^2	Variance of human capital error η_{ij}
σ_0	Standard deviation of outside option utility (or σ_0^2 for variance)
Σ	Covariance matrix of random coefficients θ_i

A.1.2 Preliminaries

The model consists of utilities u_{ij}, consideration indices c_{ij}, and human capital indices h_{ij} for student i and program j. The equations are:

$$u_{ij} = \mu^u_{ij} + \epsilon_{ij}, \tag{6}$$

$$c_{ij} = \mu^c_{ij} + \nu_{ij}, \tag{7}$$

$$h_{ij} = \mu^h_{ij} + \rho_u u_{ij} + \rho_c c_{ij} + \eta_{ij}, \tag{8}$$

where $\epsilon_{ij} \sim N(0, \sigma^2_\epsilon)$, $\nu_{ij} \sim N(0, \sigma^2_\nu)$, $\eta_{ij} \sim N(0, \sigma^2_\eta)$, and the errors are independent. The means are

$$\begin{aligned}
\mu^u_{ij} &= \delta^u_j + \theta'_i x_j + \beta'_w w_{ij} + \beta'_z z_i + \beta_d d_{ij} \\
\mu^c_{ij} &= \delta^c_j + \gamma'_w w_{ij} + \gamma'_z z_i + \gamma_s \tilde{s}_{ij} \\
\mu^h_{ij} &= \delta^h_j + \phi'_w w_{ij} + \phi'_z z_i
\end{aligned}$$

For the estimation I separate the outside good utility and its dependence on individual level covariates z_i. Here, w_{ij}, z_i, and x_j are covariates, and $\theta_i \sim MVN(0, \Sigma)$ are random coefficients and $\tilde{s}_{ij} = s_{ij} - \mathbb{E}(\text{cutoff}_j)$.

Assume independence between errors in utility and consideration equations. This implies zero covariance between u_{ij} and c_{ij} except through h_{ij}.

Human capital h_{ij} is observed only for enrolled students. For non-enrolled students, the draw of h_{ij} is skipped.

A.1.3 Parameter Priors

I employ conjugate priors to ensure tractable posterior distributions:

- For regression coefficients (e.g., δ^u, δ^c, δ^h, β, γ, ϕ, ψ): Independent multivariate normal priors $N(0, 1000 \cdot I)$, where I is the identity matrix of appropriate dimension.
- For variance parameters (e.g., σ^2_ϵ, σ^2_ν, σ^2_η, σ^2_0): Independent inverse-gamma priors $\text{IG}(1/2, 1/2)$.
- For covariance matrices (e.g., Σ for random coefficients): Inverse-Wishart priors IW(degrees of freedom = dimension + 1, scale matrix = $10 \cdot I$).

These priors are diffuse relative to the data, minimizing their influence on the posterior.

A.1.4 Conditional distributions of latent variables

For students that enroll in the program they get admitted to, I draw the latent variables conditional on the human capital latent variable, h_{ij}. To get the conditional distributions of the latent variables, let's start with $p(u \mid h, c)$. I start with

$$p(u \mid h, c) \;\propto\; p(h \mid u, c)\, p(u)$$

because $u \perp c$ in the prior. The likelihood for $h|u, c$ is given by

$$h \mid (u, c) \;\sim\; \mathcal{N}\big(\mu^h + \rho^u(u - \mu^u) + \rho^c(c - \mu^c),\, \sigma_\eta^2\big).$$

So, up to a normalising constant,

$$p(h \mid u, c) \;\propto\; \exp\Big\{-\tfrac{1}{2\sigma_\eta^2}\big[h - \mu^h - \rho^u(u - \mu^u) - \rho^c(c - \mu^c)\big]^2\Big\}.$$

The prior for u is

$$u \;\sim\; \mathcal{N}(\mu^u, \sigma_\varepsilon^2) \;\Longrightarrow\; p(u) \;\propto\; \exp\Big\{-\tfrac{1}{2\sigma_\varepsilon^2}\,(u - \mu^u)^2\Big\}.$$

Multiplying and collecting all the terms I have

$$\ln p(u \mid h, c) \;=\; -\frac{1}{2\sigma_\eta^2}\big[h - \mu^h - \rho^u(u - \mu^u) - \rho^c(c - \mu^c)\big]^2 - \frac{1}{2\sigma_\varepsilon^2}\,(u - \mu^u)^2 \;+\; \text{const.}$$

Expand the first square, keep only terms that involve u:

$$-\,\frac{(\rho^u)^2}{2\sigma_\eta^2}\,(u - \mu^u)^2 \;+\; \frac{\rho^u}{\sigma_\eta^2}\big[h - \mu^h - \rho^c(c - \mu^c)\big]\,(u - \mu^u) - \frac{1}{2\sigma_\varepsilon^2}\,(u - \mu^u)^2.$$

Now I complete the square. Let

$$D_u := \sigma_\eta^2 + (\rho^u)^2\sigma_\varepsilon^2.$$

Combine the quadratic coefficients:

$$-\frac{1}{2}\Big[\tfrac{(\rho^u)^2}{\sigma_\eta^2} + \tfrac{1}{\sigma_\varepsilon^2}\Big](u - \mu^u)^2 = -\frac{1}{2}\,\frac{D_u}{\sigma_\eta^2\sigma_\varepsilon^2}\,(u - \mu^u)^2.$$

Likewise for the linear term:

$$\frac{\rho^u}{\sigma_\eta^2}\big[h-\mu^h-\rho^c(c-\mu^c)\big](u-\mu^u)=\frac{\rho^u\sigma_\varepsilon^2}{\sigma_\eta^2\sigma_\varepsilon^2}\big[h-\mu^h-\rho^c(c-\mu^c)\big](u-\mu^u).$$

Completing the square gives

$$-\frac{1}{2}\frac{D_u}{\sigma_\eta^2\sigma_\varepsilon^2}\Big[(u-\mu^u)-\underbrace{\frac{\rho^u\sigma_\varepsilon^2}{D_u}\big(h-\mu^h-\rho^c(c-\mu^c)\big)}_{\text{posterior shift}}\Big]^2\ +\ \text{const.}$$

Reading off the posterior, I have

$$u\mid h,c\ \sim\ \mathcal{N}\Big(\mu^u+\frac{\rho^u\sigma_\varepsilon^2}{D_u}\,(h-\mu^h)-\frac{\rho^u\rho^c\sigma_\varepsilon^2}{D_u}\,(c-\mu^c),\ \frac{\sigma_\varepsilon^2\sigma_\eta^2}{D_u}\Big),$$

Similar derivations apply for $c\mid h,u$ (swap $u\leftrightarrow c$, $\rho^u\leftrightarrow\rho^c$) and $h\mid u,c$:

$$c\mid h,u\sim N\left(\mu^c+\frac{\rho^c\sigma_\nu^2}{D_c}(h-\mu^h)-\frac{\rho^c\rho^u\sigma_\nu^2}{D_c}(u-\mu^u),\frac{\sigma_\nu^2\sigma_\eta^2}{D_c}\right),$$

where $D_c=\sigma_\eta^2+(\rho^c)^2\sigma_\nu^2$.

For $h\mid u,c$:

$$h\mid u,c\sim N\left(\mu^h+\rho^u(u-\mu^u)+\rho^c(c-\mu^c),\sigma_\eta^2\right).$$

Draws are truncated based on optimality (e.g., ranking constraints, enrollment decisions).

A.1.5 General Formulas for Parameter Draws

Regression Coefficients. For $y=X\beta+e$, $e\sim N(0,\sigma^2 I)$, prior $\beta\sim N(\bar\beta,A^{-1})$:

Posterior $N(\tilde\beta,V)$:

$$V=\left(\frac{X'X}{\sigma^2}+A\right)^{-1},\quad \tilde\beta=V\left(\frac{X'y}{\sigma^2}+A\bar\beta\right).$$

Variances. For σ^2 in $y_i=\mu_i+e_i$, $e_i\sim N(0,\sigma^2)$, prior $\mathrm{IG}(\alpha,\beta)$:

Posterior $\mathrm{IG}\Big(\alpha+N/2,\beta+\frac{1}{2}\sum(y_i-\mu_i)^2\Big)$.

Covariance Matrices. For $\eta\sim N(0,\Sigma)$, prior $\mathrm{IW}(\nu_0,S_0)$:

Posterior $\text{IW}(\nu_0 + N, S_0 + \sum \eta_i \eta_i')$.

A.1.6 Gibbs Sampler Algorithm

The sampler is initialized with feasible starting values and run for K iterations. Each iteration k consists of two main steps: data augmentation and parameter drawing.

Step 1: Data Augmentation (Drawing Latent Variables). For each student i, given the parameter values from the previous iteration, $\theta^{(k-1)}$, I draw the latent variables from their full conditional posterior distributions. These are truncated normal distributions where the truncation bounds are functions of other latent variables and the student's observed choices (ROL_i, enrollment, graduation).

1. **Draw Outside Option Utility** $u_{i0}^{(k)}$: The outside option utility is drawn from a truncated normal distribution:

$$u_{i0}^{(k)} \sim \mathcal{TN}\left(X_i^0 \psi^{(k-1)}, \sigma_0^{2,(k-1)}; L_{i0}, U_{i0}\right)$$

   where the bounds are determined by optimality. For a student with a non-empty rank-order list ROL_i:

   - The lower bound is the maximum utility among considered but unlisted programs: $L_{i0} = \max(\{u_{ij}^{(k-1)} \mid j \notin ROL_i, c_{ij}^{(k-1)} \geq 0\} \cup \{-\infty\})$.
   - The upper bound is the utility of the last program listed: $U_{i0} = u_{i,ROL_i(\text{last})}^{(k-1)}$.

2. **Draw Latent Variables for each program** j:

   (a) **Draw Consideration Index** $c_{ij}^{(k)}$: The draw is from a truncated normal distribution.

$$c_{ij}^{(k)} \sim \mathcal{TN}\left(\mu_{ij}^{c,(k-1)}, \sigma_\nu^{2,(k-1)}; L_{ij}^c, U_{ij}^c\right)$$

   The bounds depend on whether student i enrolls in program j.

   - If $j = \text{enroll}_i$: The draw conditions on h_{ij} and u_{ij}. The mean and variance are adjusted as derived in the preliminaries, and the distribution is truncated below at 0.
   - If $j \neq \text{enroll}_i$:
     - If $j \in ROL_i$: Truncated from below at 0 ($L_{ij}^c = 0, U_{ij}^c = \infty$).

- If $j \notin ROL_i$ and $u_{ij}^{(k-1)} > u_{i0}^{(k)}$: Truncated from above at 0 ($L_{ij}^c = -\infty, U_{ij}^c = 0$).
- Otherwise: Drawn from an untruncated normal ($L_{ij}^c = -\infty, U_{ij}^c = \infty$).

(b) **Draw Utility** $u_{ij}^{(k)}$: The draw is from a truncated normal distribution.

$$u_{ij}^{(k)} \sim \mathcal{TN}\left(\mu_{ij}^{u,(k-1)}, \sigma_\epsilon^{2,(k-1)}; L_{ij}^u, U_{ij}^u\right)$$

- If $j = \text{enroll}_i$: The draw conditions on h_{ij} and the newly drawn $c_{ij}^{(k)}$. The mean and variance are adjusted accordingly. The bounds are determined by the ranking relative to other programs for which $c_{ik} \geq 0$.
- If $j \neq \text{enroll}_i$:
  - If $j \in ROL_i$ at rank m: Bounded by the utility of the program at rank $m-1$ (above) and $m+1$ (below). $L_{ij}^u = u_{i,m+1}^{(k)}, U_{ij}^u = u_{i,m-1}^{(k)}$.
  - If $j \notin ROL_i$ and $c_{ij}^{(k)} \geq 0$: Truncated from above by the outside option utility ($L_{ij}^u = -\infty, U_{ij}^u = u_{i0}^{(k)}$).
  - Otherwise: Drawn from an untruncated normal.

3. **Draw Human Capital** $h_{ij}^{(k)}$ **for Enrolled Program**: If student i enrolls in program j^*, draw $h_{ij^*}^{(k)}$ from a truncated normal:

$$h_{ij^*}^{(k)} \sim \mathcal{TN}\left(\mu_{ij^*}^{h,(k-1)} + \rho_u^{(k-1)} u_{ij^*}^{(k)} + \rho_c^{(k-1)} c_{ij^*}^{(k)}, \sigma_\eta^{2,(k-1)}; L_h, U_h\right)$$

where $L_h = 0, U_h = \infty$ if the student graduated, and $L_h = -\infty, U_h = 0$ otherwise.

4. **Draw Random Coefficients** $\theta_i^{(k)}$: The random coefficients are drawn from a normal posterior. Let $\tilde{u}_i$ be the vector of residuals $u_{ij}^{(k)} - (\delta_j^{u,(k-1)} + \dots)$ for all j. Let X_i be the stacked matrix of covariates x_j. The posterior is:

$$\theta_i^{(k)} \mid \cdots \sim \mathcal{N}(\tilde{\theta}_i, V_\theta)$$

where $V_\theta = \left(\frac{X_i' X_i}{\sigma_\epsilon^{2,(k-1)}} + \Sigma^{(k-1),-1}\right)^{-1}$ and $\tilde{\theta}_i = V_\theta \left(\frac{X_i' \tilde{u}_i}{\sigma_\epsilon^{2,(k-1)}}\right)$.

Step 2: Drawing Model Parameters. After augmenting the latent data, draw the model parameters from their full conditional posteriors. Let N_t be the number of students of a given type.

1. **Draw Variance Parameters**: For each variance parameter, the posterior is inverse-gamma. For example, for σ_ϵ^2:

$$\sigma_\epsilon^{2,(k)} \mid \cdots \sim \text{IG}\left(\alpha_0 + \frac{N_t J}{2}, \beta_0 + \frac{1}{2}\sum_{i,j}(u_{ij}^{(k)} - \mu_{ij}^{u,(k-1)})^2\right)$$

   Analogous posteriors are used for σ_ν^2, σ_η^2, and σ_0^2.

2. **Draw Covariance Matrix $\Sigma^{(k)}$**: The posterior for the random coefficients' covariance matrix is inverse-Wishart:

$$\Sigma^{(k)} \mid \cdots \sim \text{IW}\left(\nu_0 + N_t, S_0 + \sum_{i=1}^{N_t} \theta_i^{(k)} \theta_i^{(k)\prime}\right)$$

3. **Draw Coefficient Vectors**: All coefficient vectors are drawn from multivariate normal posteriors. For example, to draw the vector of utility fixed effects $\delta^u = (\delta_1^u, \ldots, \delta_J^u)'$:
   - Define the residual vector $y_u = u - (X\theta + W\beta_w + \ldots)$ where u is the stacked vector of all $u_{ij}^{(k)}$.
   - Let Z_u be the design matrix of dummy variables for each program.
   - The posterior for δ^u is $\mathcal{N}(\tilde{\delta}^u, V_{\delta^u})$, where:

$$V_{\delta^u} = \left(\frac{Z_u' Z_u}{\sigma_\epsilon^{2,(k)}} + A_0^{-1}\right)^{-1}, \quad \tilde{\delta}^u = V_{\delta^u}\left(\frac{Z_u' y_u}{\sigma_\epsilon^{2,(k)}} + A_0 \delta_0\right)$$

   This same procedure is applied to draw all other coefficient vectors $(\psi, \beta, \gamma, \phi, \rho_u, \rho_c)$, each time defining the appropriate residual vector y and design matrix X.

A.1.7 Implementation and Convergence

The sampler is initialized by drawing parameters from their priors and latent variables from their unconditional (but truncated) distributions. I run multiple chains from overdispersed starting points to assess convergence. The chain is run for a total of 20,000 iterations, with the first 5,000 discarded as burn-in and the remaining thinned by a factor of 10 to reduce autocorrelation. Convergence is monitored by visually inspecting trace plots and ensuring the potential scale reduction factor (PSRF) is below 1.1 for all parameters.

B Summary statistics

Here I show and discuss summary statistics for programs and students.

B.1 Descriptive statistics: Estimation sample

Figure B.1 shows how PSU scores vary by school type and gender. Panel B.1a shows that private school students score higher than voucher and public school students. These differences help explain the policy goal of increasing admission chances for students from disadvantaged schools who perform poorly on the standardized test. Panel B.1b shows that male students score higher than female students. Taken together, these figures show that the socioeconomic and gender gaps reported in other studies also appear in Chile. They also suggest that the RR policy may give an advantage to high-scoring students in public and voucher schools, and to high-scoring female students within their school. Figures B.1c and B.1d show the RR score distributions. For school type, students from private schools score higher than those from voucher schools, who in turn score higher than those from public schools. For gender the pattern is reversed: the RR score distribution of female students is higher than that of male students. This suggests that while the policy is gender neutral in design, its effect is more favorable to women.

Table B.1 reports the number of applicants, admitted, enrolled, and graduated students in 2012 by school type and gender. Tables B.3 and B.4 show similar patterns for 2013 and 2014. About 80% of applicants come from public or voucher schools. This share is 79% among admitted, 76% among enrolled, and 75% among graduates. By gender, men and women apply in similar numbers, but women make up 50% of admitted, 48% of enrolled, and 58% of graduates. Table B.8 shows admission, enrollment, and graduation rates. Admission rates are 87–88% for public and voucher students and 91% for private students. Enrollment rates are 65–68% for public and voucher students and 81% for private students. Men have higher admission (91% vs. 85%) and enrollment rates (73% vs. 67%), but lower graduation rates once enrolled (32% vs. 49%). These results point to weaker take-up among public and voucher students and lower completion among men.

The student applicant pool shows stability in its demographic and academic profile between 2012 and 2014, with a consistent majority of students coming from voucher schools. Panel A of Table B.2 presents summary statistics for 2012, 2013 and 2014. The average math and verbal score was 585.38 in 2012, 582.97 in 2013 and 582.07 in 2014. The proportion of recent high school graduates was 0.59 in 2012, 0.61 in 2013 and 0.62 in 2014.

(a) Average Math and Verbal PSU scores CDF by school type (2014).

(b) Average Math and Verbal PSU scores CDF by gender (2014).

(c) Average RR scores CDF by school type (2014).

(d) Average RR scores CDF by gender (2014).

Figure B.1: Comparison of PSU and RR scores distributions by school type and gender. Panels (B.1a) and (B.1b) show PSU score distributions by school type and gender, respectively. Panels (B.1c) and (B.1d) show RR score distributions by school type and gender, respectively.

Table B.1: Number of Applicants, Admitted, Enrolled, and Graduated Students by School Type and Gender 2012

	(1)	(2)	(3)	(4)
Group	Applicants	Admitted	Enrolled	Graduated
Public	28,509	25,052	16,348	6,431
Voucher	56,127	48,383	32,922	13,168
Private	21,368	19,537	15,911	6,562
Male	51,033	46,228	33,699	10,906
Female	54,971	46,744	31,482	15,255
Total	106,004	92,972	65,181	26,161

Student distribution by school type was stable: 26-27% public school, 53-54% voucher school, and 20% private school for all years. The number of applicants were $105,829$ in 2012, $106,762$ in 2013 and $105,781$ in 2014. These are students that took the test, applied and their application met all the requirements requested by the admissions platform.

College admissions criteria changed significantly from 2013 to 2014, with an increased emphasis on RR and decreased weights on GPA and PSU tests. Panel B of Table B.2 provides program-level summary statistics for $N = 1,355$ programs across these two periods.[8] The average weight on RR increased from 10.00 to 22.01. Conversely, the average weight on GPA decreased from 23.23 to 15.99. The weight on PSU tests also decreased from 73.61 to 68.42. The average number of total seats offered per college decreased from 81.64 to 79.07. Other characteristics remained stable. The proportion of colleges offering STEM programs remained constant at 0.34 in both periods. Sticker tuition (in thousands of US dollars) increased from an average of 2673.75 to 2783.53. The average historic PSU score of admitted students increased from 585.89 to 586.67.

Panel C of Table B.2 presents summary statistics for match-specific variables, defined for each student-college pair. Distance, a Euclidean measure using latitude–longitude from the program's municipality to the student's municipality of residence, averages around 5.01-5.03 units with a standard deviation of approximately 5.17-5.19, indicating geographical dispersion. Weighted score represents student i's score if they apply to program j. This score has a mean of approximately 571.43-577.64 and a standard deviation of 82.36-83.02, showing more variability than individual math and verbal average scores in Panel A. Other match-specific variables include the interaction between a student's math

[8]These correspond to the set of programs that were available both in 2013 and 2014. More details about data construction in appendix D.

Table B.2: Descriptive Statistics

	(1)	(2)	(3)	(4)	(5)	(6)
	2012		2013		2014	
Panel A: Students	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Avg. Math and Verbal	585.38	69.90	582.97	76.16	582.07	76.69
=1 Recent HS Grad	0.59	0.49	0.61	0.49	0.62	0.49
Sex	0.48	0.50	0.49	0.50	0.49	0.50
Public School	0.27	0.44	0.26	0.44	0.26	0.44
Voucher School	0.53	0.50	0.54	0.50	0.54	0.50
Private School	0.20	0.40	0.20	0.40	0.20	0.40
N of Obs	105,829		106,257		105,155	
	2012		2013		2014	
Panel B: Colleges	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Weight on RR	0.00	0.00	10.00	0.00	21.91	10.34
Weight on GPA	29.20	6.92	23.07	9.38	15.97	6.90
Weight on PSU tests	70.80	6.92	66.93	9.38	62.11	10.32
Total seats offered	85.82	63.09	83.15	67.39	80.65	67.42
=1 if STEM	0.34	0.47	0.34	0.47	0.34	0.47
Tuition (Th. USD)	2512.75	841.08	2678.47	833.00	2789.13	861.20
Avg. Historic PSU Score	585.75	56.39	585.75	56.39	587.13	56.08
N of Obs	1,267		1,267		1,267	
	2012		2013		2014	
Panel C: Student-College	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Distance	547.88	579.29	547.44	576.89	549.84	579.00
Weighted Score	569.18	80.42	571.47	82.44	577.65	83.06
(i, j) in same region	0.18	0.39	0.18	0.39	0.18	0.39
Net Price	2381.44	904.99	2518.46	916.75	2621.53	945.72
N of Obs	134,085,343		134,627,619		133,231,385	

score and program STEM status. This interaction has a mean around 201.25 and a standard deviation of 281.89. The share of student-program pairs in the same geographical region is also included. Each region corresponds to one of Chile's 15 administrative regions.[9] Same-region pairs share 0.18 with high variation (standard deviation of 0.39). Net price faced by student i at program j is a match-specific variable, averaging between 2513.78 and 2615.95 with a standard deviation around 919.77 to 948.72. This price variable is match specific due to student-program specific discounts (e.g., teaching-specific discounts, BVP, discussed in Kapor et al. (2022)). These statistics use over 143 million potential student-college pairs each year.

B.2 Socioeconomic characterization by school type

Figure (B.2) shows the share of students whose mother attained higher education, used as a proxy for socioeconomic status.

Figure B.2: Share of students with higher-educated mothers by school type.

[9]Chile's regions are administrative divisions containing multiple municipalities or cities, similar to states in some countries but with their own distinct administrative structures.

B.3 Descriptive Statistics: Students "funnel" statistics not in the main text

Table B.3: Number of Applicants, Admitted, Enrolled, and Graduated Students by School Type and Gender 2013

	(1)	(2)	(3)	(4)
Group	Applicants	Admitted	Enrolled	Graduated
Public	27,832	24,681	16,757	5,908
Voucher	57,610	50,174	35,561	12,791
Private	21,384	19,831	16,425	6,060
Male	52,163	47,458	35,560	10,164
Female	54,663	47,228	33,183	14,595
Total	106,826	94,686	68,743	24,759

Table B.4: Number of Applicants, Admitted, Enrolled, and Graduated Students by School Type and Gender 2014

	(1)	(2)	(3)	(4)
Group	Applicants	Admitted	Enrolled	Graduated
Public	27,478	24,417	17,099	6,156
Voucher	57,246	50,541	36,728	13,866
Private	21,419	20,040	16,539	6,263
Male	51,496	47,171	35,948	10,670
Female	54,647	47,827	34,418	15,615
Total	106,143	94,998	70,366	26,285

Figure B.4 shows the distribution of program weights across admission criteria over time. The introduction of the RR rule in 2013 generated a spike at 10%, and its 2014 expansion created wide heterogeneity (10–40%). Almost all of this re-weighting came at the expense of GPA, while Math/Verbal and Science/History weights changed little, underscoring that the policy primarily reshuffled weight away from grades rather than standardized test performance.

Table B.5: Share of Applicants, Admitted, Enrolled, and Graduated Students by School Type and Gender, 2012

	(1)	(2)	(3)	(4)
Group	Applicants	Admitted	Enrolled	Graduated
Public	26.9	26.9	25.1	24.6
Voucher	52.9	52.0	50.5	50.3
Private	20.2	21.0	24.4	25.1
Male	48.1	49.7	51.7	41.7
Female	51.9	50.3	48.3	58.3
Total	100.0	0.0	100.0	100.0

Table B.6: Share of Applicants, Admitted, Enrolled, and Graduated Students by School Type and Gender, 2013

	(1)	(2)	(3)	(4)
Group	Applicants	Admitted	Enrolled	Graduated
Public	26.1	26.1	24.4	23.9
Voucher	53.9	53.0	51.7	51.7
Private	20.0	20.9	23.9	24.5
Male	48.8	50.1	51.7	41.1
Female	51.2	49.9	48.3	58.9
Total	100.0	0.0	100.0	100.0

Table B.7: Share of Applicants, Admitted, Enrolled, and Graduated Students by School Type and Gender, 2014

	(1)	(2)	(3)	(4)
Group	Applicants	Admitted	Enrolled	Graduated
Public	25.9	25.7	24.3	23.4
Voucher	53.9	53.2	52.2	52.8
Private	20.2	21.1	23.5	23.8
Male	48.5	49.7	51.1	40.6
Female	51.5	50.3	48.9	59.4
Total	100.0	0.0	100.0	100.0

Table B.8: Admission, Enrollment, and Graduation Rates by School Type and Gender, 2012

	(1)	(2)	(3)
Group	Admission	Enrollment	Grad | Enro.
Public	0.879	0.653	0.393
Voucher	0.862	0.680	0.400
Private	0.914	0.814	0.412
Male	0.906	0.729	0.324
Female	0.850	0.673	0.485
Total	0.877	0.701	0.401

Table B.9: Admission, Enrollment, and Graduation Rates by School Type and Gender, 2013

	(1)	(2)	(3)
Group	Admission	Enrollment	Grad | Enro.
Public	0.887	0.679	0.353
Voucher	0.871	0.709	0.360
Private	0.927	0.828	0.369
Male	0.910	0.749	0.286
Female	0.864	0.703	0.440
Total	0.886	0.726	0.360

Table B.10: Admission, Enrollment, and Graduation Rates by School Type and Gender, 2014

	(1)	(2)	(3)
Group	Admission	Enrollment	Grad | Enro.
Public	0.889	0.700	0.360
Voucher	0.883	0.727	0.378
Private	0.936	0.825	0.379
Male	0.916	0.762	0.297
Female	0.875	0.720	0.454
Total	0.895	0.741	0.374

Figure B.3: Distribution of total seats across years

Figure B.4: Distribution of admission weights across criteria, 2012–2014. Rows show years (2012, 2013, 2014) and columns show criteria (Relative Ranking, GPA, Math & Verbal PSU, and Science/History PSU). Each bar indicates the number of programs assigning a given percentage weight to that component. The Relative Ranking (RR) rule was absent before 2013, introduced at a uniform 10% across all programs in 2013, and discretionary (10–40%) in 2014. The sample is restricted to the balanced panel of programs available in all three years.

Table B.11: Effect of the Relative Ranking Reform on Applications to Top Universities

	(1) Full sample	(2) Public/Voucher	(3) Private	(4) Triple Diff
Post reform	-0.034***	-0.037***	-0.024***	-0.023***
	(0.002)	(0.003)	(0.004)	(0.004)
Top 70th percentile	0.036***	0.043***	-0.013***	-0.057***
	(0.003)	(0.003)	(0.005)	(0.006)
Post × Top 70%	0.036***	0.040***	0.022***	
	(0.003)	(0.004)	(0.005)	
Public/Voucher	0.019			-0.011
	(0.017)			(0.018)
Post × Top				0.024***
				(0.005)
Post × Public				-0.014***
				(0.005)
Top × Public				0.109***
				(0.007)
Post × Top × Public				0.016**
				(0.006)
Dep. Var. mean	.71	.676	.858	.71
Observations	338,909	275,344	63,555	338,909
R-squared	0.22	0.21	0.19	0.22

Note: This table reports difference-in-differences (columns 1–3) and triple-difference (column 4) estimates of the effect of Chile's 2013 and 2014 Relative Ranking (RR) reform on the probability of applying to top universities, defined as institutions with at least five years of accreditation at the time of application. The sample covers the 2012–2014 CCAS application cycles. Controls include PSU score, its square, and gender. All regressions absorb school fixed effects and cluster standard errors at the school level. 'Top 70%' indicates students above the 70th percentile within their school, which is around when the boost of the RR starts to have a relevant magnitude (around 0.3 standard deviations of the PSU). 'Public/Voucher' refers to publicly funded or voucher-financed high schools, while 'Private' indicates unsubsidized schools. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table B.12: Effect of the Relative Ranking Reform on Admissions to Top Universities

	(1) Full sample	(2) Public/Voucher	(3) Private	(4) Triple Diff
Post reform	-0.006**	-0.015***	0.025***	0.028***
	(0.002)	(0.003)	(0.004)	(0.004)
Top 70th percentile	0.061***	0.068***	-0.005	-0.069***
	(0.003)	(0.003)	(0.006)	(0.008)
Post × Top 70%	0.026***	0.034***	0.008	
	(0.003)	(0.004)	(0.006)	
Public/Voucher	-0.002			-0.028
	(0.019)			(0.020)
Post × Top				0.009
				(0.007)
Post × Public				-0.043***
				(0.005)
Top × Public				0.151***
				(0.009)
Post × Top × Public				0.024***
				(0.008)
Dep. Var. mean	.514	.463	.731	.514
Observations	338,909	275,344	63,555	338,909
R-squared	0.30	0.28	0.25	0.30

Note: This table presents difference-in-differences (columns 1–3) and triple-difference (column 4) estimates of the effect of Chile's 2013 and 2014 Relative Ranking (RR) reform on the probability of admission to top universities, defined as institutions with at least five years of accreditation at the time of application. The data include all applicants from Chile's centralized admissions system (CCAS) from 2012–2014. Controls include PSU score, its square, and gender. All regressions absorb school fixed effects and cluster standard errors at the school level. 'Top 70%' indicates students above the 70th percentile within their school, which is around when the RR boost starts to have a relevant magnitude (around 0.3 standard deviations of the PSU). 'Public/Voucher' refers to publicly funded or voucher-financed high schools, while 'Private' indicates unsubsidized schools. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table B.13: Effect of the Relative Ranking Reform on Graduation from Top Universities

	(1) Full sample	(2) Public/Voucher	(3) Private	(4) Triple Diff
Post reform	-0.028***	-0.028***	-0.028***	-0.027***
	(0.003)	(0.003)	(0.005)	(0.005)
Top 70th percentile	0.089***	0.099***	0.052***	0.043***
	(0.004)	(0.004)	(0.009)	(0.009)
Post × Top 70%	0.004	0.007	-0.007	
	(0.004)	(0.005)	(0.009)	
Public/Voucher	-0.013			-0.037
	(0.025)			(0.026)
Post × Top				-0.007
				(0.009)
Post × Public				-0.001
				(0.006)
Top × Public				0.059***
				(0.010)
Post × Top × Public				0.013
				(0.010)
Dep. Var. mean	.257	.239	.317	.257
Observations	215,185	163,854	51,322	215,185
R-squared	0.09	0.09	0.07	0.09

Note: This table shows difference-in-differences (columns 1–3) and triple-difference (column 4) estimates of the effect of Chile's 2013 and 2014 Relative Ranking (RR) reform on graduation from top universities, defined as institutions with at least five years of accreditation at the time of application. The sample comprises students applying through the centralized system (CCAS) from 2012–2014. The dependent variable equals one if a student graduated from a top university within seven years. Controls include PSU score, its square, and gender. All regressions absorb school fixed effects and cluster standard errors at the school level. 'Top 70%' indicates students above the 70th percentile within their school, around where the RR boost becomes sizeable (roughly 0.3 PSU standard deviations). 'Public/Voucher' refers to publicly funded or voucher-financed high schools, while 'Private' indicates unsubsidized schools. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Figure B.5: Robustness of Difference-in-Differences Estimates to Percentile Thresholds

Note. This figure shows robustness checks testing the sensitivity of difference-in-differences estimates to different percentile thresholds for defining "top" students. Each panel represents a different outcome (applications, admissions, graduation). Error bars show 95% confidence intervals. The sample includes all students who applied to the centralized system from 2012–2014. For admissions and graduation, students who did not get admitted or did not graduate are recoded to 0. Controls include PSU score, its square, gender, and public/voucher school indicator. All regressions include school fixed effects and cluster standard errors at the school level.

B.3.1 Differences in differences tables

C Results appendix

Here I show and discuss the estimation results in full.

Table C.1: Preference estimates: inside and outside goods parameters

	(1)	(2)	(3)	(4)	(5)	(6)
	2012		2013		2014	
Variable	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Inside Good (β)						
STEM × Math	0.187	(0.003)	0.155	(0.002)	0.177	(0.003)
STEM × Verbal	-0.155	(0.005)	-0.111	(0.002)	-0.117	(0.005)
STEM × Sex	1.024	(0.004)	0.831	(0.009)	0.816	(0.015)
STEM × Recent Grad	0.083	(0.017)	0.028	(0.003)	-0.004	(0.003)
STEM × Private School	-0.460	(0.008)	-0.524	(0.008)	-0.514	(0.016)
STEM × Public School	-0.046	(0.006)	-0.014	(0.004)	-0.025	(0.004)
Selectivity × Math	0.163	(0.005)	0.082	(0.003)	0.071	(0.002)
Selectivity × Verbal	0.108	(0.002)	0.066	(0.002)	0.071	(0.002)
Selectivity × Sex	-0.069	(0.002)	-0.126	(0.003)	-0.097	(0.003)
Selectivity × Recent Grad	-0.036	(0.003)	-0.048	(0.002)	-0.025	(0.002)
Selectivity × Private School	-0.091	(0.007)	-0.045	(0.003)	-0.042	(0.005)
Selectivity × Public School	-0.042	(0.006)	-0.038	(0.003)	-0.024	(0.003)
Same Region	1.087	(0.005)	1.050	(0.003)	1.062	(0.003)
Net Price	-0.055	(0.002)	-0.043	(0.001)	-0.031	(0.003)
Distance	-0.430	(0.002)	-0.393	(0.002)	-0.394	(0.002)
Random Coefficients (σ)						
Constant	0.393	(0.004)	0.396	(0.002)	0.416	(0.002)
STEM	1.135	(0.006)	1.048	(0.004)	1.066	(0.005)
Avg. Math & Lang. Historic	0.363	(0.003)	0.305	(0.001)	0.288	(0.003)
Outside Good (ψ)						
Constant	2.496	(0.052)	2.830	(0.052)	2.829	(0.059)
Sex	1.095	(0.011)	0.537	(0.020)	0.362	(0.033)
Private	0.255	(0.022)	-0.393	(0.034)	-0.253	(0.039)
Public	0.003	(0.011)	-0.004	(0.010)	0.011	(0.010)
Avg. Mate-Verb.	1.335	(0.013)	1.397	(0.012)	1.269	(0.020)
Avg. Mate-Verb.^{2}	0.326	(0.007)	0.362	(0.010)	0.278	(0.009)
Avg. Mate-Verb.^{3}	-0.029	(0.004)	0.018	(0.001)	0.011	(0.001)
Scholarship	-0.245	(0.076)	-0.696	(0.059)	-0.602	(0.054)
Scholarship^2	0.437	(0.148)	1.591	(0.153)	1.518	(0.173)
Scholarship^3	-0.109	(0.040)	-0.465	(0.048)	-0.457	(0.058)
Recent Grad.	0.029	(0.017)	-0.077	(0.012)	-0.012	(0.011)
Avg. Mate-Verb× Recent Grad	-0.113	(0.015)	0.024	(0.015)	0.049	(0.013)
Avg. Mate-Verb^2 × Recent Grad	0.002	(0.009)	-0.027	(0.010)	-0.020	(0.010)
Avg. Mate-Verb^3 × Recent Grad	-0.006	(0.005)	-0.047	(0.003)	-0.028	(0.002)

Table C.2: Consideration estimates: parameters

	(1)	(2)	(3)	(4)	(5)	(6)
	2012		2013		2014	
Variable	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Consideration Eq. (γ)						
Sex	0.452	(0.005)	0.064	(0.017)	0.012	(0.024)
Private	0.212	(0.014)	-0.352	(0.022)	-0.251	(0.032)
Public	0.012	(0.003)	0.049	(0.003)	0.008	(0.003)
Avg. Mate-Verb.	-0.528	(0.004)	-0.353	(0.004)	-0.308	(0.011)
Avg. Mate-Verb.^{2}	-0.095	(0.002)	-0.089	(0.002)	-0.078	(0.002)
Avg. Mate-Verb.^{3}	-0.022	(0.001)	-0.044	(0.000)	-0.038	(0.001)
Scholarship	0.146	(0.008)	0.183	(0.019)	0.206	(0.016)
Scholarship^2	-0.517	(0.017)	-0.007	(0.047)	0.102	(0.058)
Scholarship^3	0.145	(0.005)	0.019	(0.015)	-0.014	(0.020)
Recent Grad.	-0.073	(0.006)	-0.222	(0.008)	-0.224	(0.003)
Avg. Mate-Verb$\times$ Recent Grad	-0.107	(0.004)	-0.117	(0.005)	-0.122	(0.005)
Avg. Mate-Verb^2 $\times$ Recent Grad	-0.060	(0.003)	-0.088	(0.002)	-0.085	(0.004)
Avg. Mate-Verb^3 $\times$ Recent Grad	0.028	(0.001)	0.024	(0.001)	0.021	(0.001)
STEM $\times$ Math	0.322	(0.005)	0.406	(0.003)	0.402	(0.004)
STEM $\times$ Verbal	0.008	(0.003)	-0.065	(0.006)	-0.049	(0.003)
STEM $\times$ Sex	-0.165	(0.004)	0.061	(0.021)	0.057	(0.012)
STEM $\times$ Recent Grad	0.107	(0.006)	0.160	(0.006)	0.193	(0.004)
STEM $\times$ Private School	0.141	(0.007)	0.170	(0.013)	0.163	(0.007)
STEM $\times$ Public School	0.050	(0.008)	0.031	(0.007)	0.029	(0.005)
Selectivity $\times$ Math	0.060	(0.002)	0.073	(0.004)	0.069	(0.002)
Selectivity $\times$ Verbal	0.159	(0.001)	0.188	(0.002)	0.157	(0.004)
Selectivity $\times$ Sex	-0.116	(0.002)	0.040	(0.005)	0.053	(0.008)
Selectivity $\times$ Recent Grad	0.079	(0.003)	0.170	(0.005)	0.168	(0.003)
Selectivity $\times$ Private School	-0.017	(0.004)	0.190	(0.008)	0.156	(0.009)
Selectivity $\times$ Public School	0.006	(0.003)	-0.008	(0.002)	0.020	(0.002)
Same Region	0.370	(0.004)	0.403	(0.002)	0.375	(0.002)
Net Price	-0.070	(0.002)	0.725	(0.006)	0.857	(0.010)
Weighted Score	1.281	(0.006)	2.304	(0.014)	2.357	(0.012)

Table C.3: Human capital production function estimates: parameters

	(1)	(2)
	Panel Estimation	
Variable	Coeff.	Std. Err.
Phi Parameters (ϕ)		
ρ	0.169	(0.011)
Sex	-0.294	(0.007)
Private	-0.000	(0.009)
Public	0.031	(0.009)
Avg. Mate-Verb.	0.247	(0.007)
Avg. Mate-Verb.^{2}	-0.013	(0.005)
Avg. Mate-Verb.^{3}	-0.004	(0.001)
Scholarship	0.337	(0.018)
Scholarship^2	-0.502	(0.031)
Scholarship^3	0.130	(0.009)
Recent Grad.	0.107	(0.008)
Avg. Mate-Verb$\times$ Recent Grad	0.007	(0.009)
Avg. Mate-Verb^2 $\times$ Recent Grad	-0.001	(0.005)
Avg. Mate-Verb^3 $\times$ Recent Grad	0.008	(0.002)
STEM $\times$ Math	-0.086	(0.009)
STEM $\times$ Verbal	-0.142	(0.007)
STEM $\times$ Sex	-0.182	(0.010)
STEM $\times$ Recent Grad	-0.380	(0.010)
STEM $\times$ Private School	-0.276	(0.014)
STEM $\times$ Public School	-0.212	(0.013)
Selectivity $\times$ Math	0.013	(0.004)
Selectivity $\times$ Verbal	-0.024	(0.004)
Selectivity $\times$ Sex	0.007	(0.005)
Selectivity $\times$ Recent Grad	-0.009	(0.007)
Selectivity $\times$ Private School	0.036	(0.007)
Selectivity $\times$ Public School	-0.002	(0.007)
Same Region	0.034	(0.008)
Net Price	-0.081	(0.004)

(a) All programs, 2012 **(b)** All programs, 2013 **(c)** All programs, 2014

Figure C.1: Average program quality δ^u and average consideration, all programs, 2012–2014.

(a) Public, 2012 **(b)** Public, 2013 **(c)** Public, 2014

(d) Voucher, 2012 **(e)** Voucher, 2013 **(f)** Voucher, 2014

(g) Private, 2012 **(h)** Private, 2013 **(i)** Private, 2014

Figure C.2: Average program quality δ^u and average consideration by school type, 2012–2014.

(a) Male, 2012 **(b)** Male, 2013 **(c)** Male, 2014

(d) Female, 2012 **(e)** Female, 2013 **(f)** Female, 2014

Figure C.3: Average program quality δ^u and average consideration by gender, 2012–2014.

D Data Construction

D.1 Students and applications data construction

D.2 Higher education programs data construction

E Identification results and discussion

E.1 Formal identification arguments

This argument is formalized in Assumption E.1, which is adapted from Assumption 1 in Agarwal and Somaini (2022).

Assumption E.1. *The unobserved terms (ε, ν) are conditionally independent of the shifters d_{ij} and s_{ij} given the observables (z_i, x_j, w_{ij}).*

Under the application behavior assumptions, omitting the dependence on observables (z_i, x_j, w_{ij}), the share of students that include program j in their list is given by:

$$q_{jt}(w_i, y_i, z_i) = \sum_{C \in \mathcal{C}} \Pr(C_i = C, u_{ij} \geq u_{i0} \mid z_i, x_j, w_{ij}, d_{ij}, s_{ij}).$$

Together with the assumption of excluded shifters, assumption E.1, we can write

$$q_{jt}(w_i, y_i, z_i) = \sum_{C \in \mathcal{C}} \Pr(u_{ij} \geq u_{i0} \mid C_i = C, d_{ij}) \times \Pr(C_i = C \mid s_{ij}).$$

To complete the identification arguments I need an additional assumption over the shifter of the consideration equation. Let the consideration equation be parametrized as a linear function, and the consideration of a program be given by

$$\kappa\left(z_i, x_j, w_{ij}, \nu_{ij}\right) = \mathbb{1}\left\{\delta_j^c + \gamma^z z_i + \gamma^w w_{ij} + \gamma_1^s s_{ij} + \nu_{ij} > 0\right\}.$$

Assumption E.2. *The function κ_{ij} is non-decreasing with s. For all j, $\lim_{s \to \infty} \kappa_{ij}(s, z_i, x_j, w_{ij}, \nu_{ij}) = 1$ and $\lim_{s \to -\infty} \kappa_{ij}(s, z_i, x_j, w_{ij}, \nu_{ij}) = 0$.*

Using these two assumptions, we now state the following result, which corresponds exactly to Lemma 1 in Agarwal and Somaini (2022).

Lemma E.1 (Lemma 1 in Agarwal and Somaini (2022))**.** *Fix (z_i, x_j, w_{ij}). Suppose that assumptions E.1 and E.2 are satisfied. Let χ be the interior of the support of (d, s) given (z_i, x_j, w_{ij}). The joint distribution of (u_i, c_i) conditional on $(u_i, c_i) \in \chi$ and (z_i, x_j, w_{ij}) is identified.*

F Outcome equation identification

Dale and Krueger (2002) method is says that controlling for the set of applications and acceptances can help account for unobserved selection into schools. This paper is in line with that argument and controls for student preferences unobserved variation at the moment of estimating the human capital production function.

The main concern with identification of value-added functions in higher education is the selection of students into colleges or higher education programs (Cunha and Miller, 2014). In this paper, I am able to control for students preferences for higher education programs.

The policy changes across years provides the necessary variation to identify the graduation outcome functions. Observationally equivalent students apply to programs before and after the Relative Ranking policy implementation, and are going to face their scores being weighted different and the cutoff scores of programs being shifted more

than usual from year to year. This shift is uncorrelated with the students preferences, but will imply that the ex-post feasible programs for the student change.

G Additional Counterfactual Results

Heterogeneity in Welfare Effects

Figure G.1 presents the welfare effects of the 2013 and 2014 RR implementations. Students in public and voucher schools gained around 35 mts in 2013 and 70 mts in 2014, as measured by willingness to travel (Figure G.1a). In contrast, private school students lost about 48 mts in 2013 and 100 mts in 2014 (Figure G.1b). Among gender groups, women gained roughly 46 mts in 2013 and close to 100 mts in 2014 (Figure G.1c), while men lost around 12 mts in 2013 and 31 mts in 2014 (Figure G.1d). Most of the welfare gains accrued to the intended target groups of the policy maker, which were students from public and voucher schools.

Figure G.2 presents the graduation outcomes. Public and voucher students gained about 0.4 percentage points in 2013 and 0.7 points in 2014 (Figure G.2a). Private school students lost about 1.3 points in 2013 and 3.4 points in 2014 (Figure G.2b). Women gained about 0.3 points in 2013 and 0.6 points in 2014 (Figure G.2c), while men lost about 0.8 points in 2013 and 2.0 points in 2014 (Figure G.2d). The distribution of winners and losers in graduation mirrors the welfare results.

Heterogeneity in the effects: RR versus AA

(a) Public and voucher school students

(b) Private school students

(c) Female students

(d) Male students

Figure G.1: Estimated welfare effects of the RR policy by student characteristics. Top row: comparison by high school type (public/voucher vs. private). Bottom row: comparison by gender (male vs. female).

(a) Public and voucher school students

(b) Private school students

(c) Female students

(d) Male students

Figure G.2: Estimated graduation effects of the RR policy by student characteristics. Top row: comparison by high school type (public/voucher vs. private). Bottom row: comparison by gender (male vs. female).

(a) Public and voucher school students

(b) Private school students

(c) Female students

(d) Male students

Figure G.3: Estimated welfare effects of the AA policy versus RR policy by student characteristics. Top row: comparison by high school type (public/voucher vs. private). Bottom row: comparison by gender (male vs. female).

G.1 Decomposing the total effect of the RR policy

Figure G.4 presents the change in aggregate welfare from the 2012 baseline to the RR 2014 scenario. I define student welfare as the sum of the utility from the assigned match minus the utility of the outside option. I decompose the total change in welfare (ΔW) into changes driven by program quality ($\Delta W_{\Delta u}$), match-specific fit (ΔW_{match}), and the outside option (ΔW_{out}):

$$\Delta W = \underbrace{\sum_{i,j} \delta_j^u \, \Delta D_{ij}}_{\text{Program Quality}} + \underbrace{\sum_{i,j} \tilde{u}_{ij} \, \Delta D_{ij}}_{\text{Match Fit}} - \underbrace{\sum_{i,j} u_{i0} \, \Delta D_{ij}}_{\text{Outside Good}}$$

where $\Delta D_{ij} = \mathbf{1}\{a_i^1 = j\} - \mathbf{1}\{a_i^0 = j\}$ represents the change in assignment status between the counterfactual and the baseline.

Figure G.4 shows the largest driver of welfare gains is the outside good component (1,634.19). This term captures the utility of the outside option forgone by students who enroll minus the utility gained by those who drop out. A positive value implies the policy induces enrollment among students with lower outside options (public and voucher students) and displaces students with higher outside options to the outside good. This result aligns with the evidence that the policy primarily benefits students from lower socioeconomic backgrounds.

The program quality component contributes the second largest share (1,348.87). This term aggregates changes in enrollment weighted by the program mean utility (δ_j^u). A positive value indicates a net expansion of seats in high-quality programs relative to low-quality ones. This confirms the RR policy shifted students into programs with higher average utility. The match fit component contributes a smaller positive amount (282.47). This suggests students sort into programs that fit their specific preferences better, though this effect is secondary to access and quality improvements.

G.2 Counterfactual descriptives

G.3 Effect of Alternative Designs of the RR Policy

This section studies the effects of alternative policy designs. I study the consequences of expanding the RR policy by simulating counterfactual policies that set RR admission criteria in a range from 0% to 100%. I apply this to all programs, reducing but maintaining the proportion of all other weights (PSU and GPA) as the RR criteria expands. The exercise

Figure G.4: Decomposition of the total effect

Note: This figure decomposes the aggregate change in student utility between the baseline (2012) and the counterfactual (RR 2014). *Total Variation* shows the change in aggregate utility for students. *Delta U* captures the change in program-specific mean utility components. *Match U terms* represents the change in match-specific utility (student-program interactions). *Outside Good Utility* shows the change in utility for students who remain unmatched. All values are normalized by the distance coefficient and represent aggregate changes across all students.

Table G.1: Summary statistics of utility change for winners and losers

Group	Mean	Min	Median	Max	N
Positive, switchers	1.4	0.0	1.1	10.8	10,875
Positive, entrants	1.3	0.0	0.9	7.1	2,243
Negative, switchers	-1.5	-10.5	-1.1	-0.0	7,658
Negative, exits	-1.4	-7.9	-1.1	-0.0	2,390

Note: The table reports summary statistics of the change in utility, measured as willingness to travel (in kilometers), between the 2012 admission policy and the 2014 RR policy. "Positive" groups represent students whose utility increased under the new policy, while "Negative" groups represent those whose utility decreased. "Switchers" are students who moved between programs within the system, and "Entrants" or "Exits" are students who entered from or moved to the outside option, respectively.

Table G.2: Counterfactual changes vs. 2012 benchmark

ROL Length						
Group	Δ 14	Δ % 14	Δ RR40	Δ % RR40	Δ RR90	Δ % RR90
All	0.45	5.41	1.48	17.63	4.74	56.56
Male	0.17	2.06	0.67	7.91	2.45	29.10
Female	0.72	8.59	2.24	26.87	6.91	82.68
Public	0.63	7.41	2.06	24.14	6.74	78.83
Private	-0.22	-2.56	-0.32	-3.73	-0.46	-5.46
Voucher	0.62	7.47	1.87	22.61	5.74	69.36
Avg. Utility						
Group	Δ 14	Δ % 14	Δ RR40	Δ % RR40	Δ RR90	Δ % RR90
All	-0.01	-1.22	-0.03	-4.93	-0.08	-14.24
Male	-0.01	-2.13	-0.04	-6.79	-0.11	-19.04
Female	-0.00	-0.39	-0.02	-3.23	-0.06	-9.83
Public	-0.01	-1.15	-0.02	-4.16	-0.07	-12.12
Private	-0.01	-2.09	-0.04	-7.24	-0.15	-24.38
Voucher	-0.00	-0.89	-0.02	-4.35	-0.06	-11.03
Min. Utility						
Group	Δ 14	Δ % 14	Δ RR40	Δ % RR40	Δ RR90	Δ % RR90
All	-0.00	-1.32	-0.01	-5.25	-0.02	-17.03
Male	0.00	0.39	-0.00	-1.09	-0.02	-11.84
Female	-0.00	-2.89	-0.01	-9.10	-0.03	-21.81
Public	-0.00	-1.95	-0.01	-6.39	-0.03	-20.44
Private	0.00	2.33	0.01	3.93	-0.01	-8.44
Voucher	-0.00	-2.47	-0.01	-8.37	-0.03	-18.84
Max. Utility						
Group	Δ 14	Δ % 14	Δ RR40	Δ % RR40	Δ RR90	Δ % RR90
All	-0.00	-0.29	-0.03	-2.51	-0.11	-7.82
Male	-0.03	-2.33	-0.09	-6.71	-0.23	-16.95
Female	0.02	1.57	0.02	1.34	0.01	0.53
Public	-0.00	-0.15	-0.02	-1.10	-0.04	-3.14
Private	-0.05	-2.99	-0.14	-9.01	-0.40	-26.25
Voucher	0.01	0.79	-0.01	-0.44	-0.03	-2.28

Figure G.5: Scatter of Scores - 𝔼(cutoff) for 2012 and 2014

traces the equity–efficiency trade-off: higher RR weight cuts the role of the test scores and shifts admission chances toward public and voucher students.

Impacts and Trade-offs of the Expanded Policy

Expanding RR increases the admission and welfare of top students, primarily from public and voucher schools, as previously shown. However, this may also lead programs to admit less prepared students, increasing dropout risk and overall mismatch. Figure G.6 presents the results of the counterfactual exercise involving RR expansion. Each dot in Figure G.6 represents a distinct counterfactual scenario, with its position indicating the equilibrium welfare and average PSU score. Average welfare is normalized to 2012 average welfare units:

$$\frac{W(\mu^{RR'})}{W(\mu^{2012RR})},$$

while the average PSU score is shown in standard deviations of the PSU scale. As expected, a greater emphasis on the RR criterion increases students' average welfare but decreases the average PSU score of admitted students. Relative to the 2012 baseline, expanding the RR weight to 50% increases average welfare by 5 percentage points and decreases the average PSU score of admitted students by approximately -0.02 standard deviations.

Figure G.6 and Table G.3 present the counterfactual policy frontier disaggregated by school type (Panel G.6a) and gender (Panel G.6b). The patterns differ sharply across groups. For public and voucher school students, expanding the RR policy generates large welfare gains: at 40% RR across all programs, utility rises by 9.7% for public and 9.9% for voucher students relative to baseline. These gains are accompanied by modest increases in graduation probabilities, about 1.8 and 0.4 percentage points respectively. In contrast, private school students consistently lose under expansion: their utility declines steadily from baseline, reaching –8.4% at 40% and –19.3% at 90%; graduation probabilities also fall, by –6.5% at 40% and nearly –15% at 90%.

Gender differences are equally stark. Male students experience declines in both outcomes, with utility falling slightly below baseline (about –1.9% at 40%) and graduation probabilities dropping substantially (about –4.9% at 40% and –12.9% at 90%). By contrast, female students enjoy very large welfare gains from expansion: utility rises by 13.3% at 40% and almost 29% at 90%. These gains come with small but positive improvements in graduation probabilities, from +0.2% at 30% to +3.2% at 90%. Overall, the results show that the RR expansion benefits women and students from public and voucher schools, while harming private school students and, in terms of graduation, men.

(a) By school type **(b)** By gender

Figure G.6: Counterfactual policy range for RR across groups

Table G.3: Counterfactual changes in utility and graduation probabilities across groups (percent change relative to 2012 baseline).

Panel A: By School Type

Group	0%	10%	20%	30%	40%	50%	60%	70%	80%	90%
All (Utility)	-1.24	0.42	2.03	3.82	5.80	7.70	9.17	10.76	12.13	13.33
All (Graduation)	-0.13	0.04	-0.28	-0.96	-0.92	-1.27	-1.83	-2.05	-2.45	-2.32
Public (Utility)	-2.08	0.64	3.51	6.50	9.73	12.76	15.49	18.34	20.83	23.16
Public (Graduation)	-0.15	0.67	0.78	1.13	1.80	1.32	1.88	2.09	1.89	2.82
Voucher (Utility)	-1.57	1.09	3.75	6.81	9.85	12.94	15.44	17.88	20.28	22.24
Voucher (Graduation)	0.04	0.33	0.28	-0.31	0.43	0.79	0.55	0.83	0.90	1.08
Private (Utility)	0.52	-1.43	-3.79	-6.40	-8.37	-10.63	-13.08	-14.96	-17.34	-19.25
Private (Graduation)	-0.49	-1.21	-2.51	-4.43	-6.51	-8.27	-10.64	-12.33	-13.98	-14.73

Panel B: By Gender

Group	0%	10%	20%	30%	40%	50%	60%	70%	80%	90%
All (Utility)	-1.24	0.42	2.03	3.82	5.80	7.70	9.17	10.76	12.13	13.33
All (Graduation)	-0.13	0.04	-0.28	-0.96	-0.92	-1.27	-1.83	-2.05	-2.45	-2.32
Male (Utility)	1.12	0.02	-0.84	-1.53	-1.93	-2.09	-2.31	-2.33	-2.34	-2.45
Male (Graduation)	0.75	-0.21	-1.77	-3.27	-4.85	-7.28	-9.05	-10.30	-11.86	-12.87
Female (Utility)	-3.54	0.81	4.82	9.03	13.32	17.22	20.34	23.50	26.21	28.69
Female (Graduation)	-0.59	0.16	0.50	0.23	1.12	1.86	1.92	2.25	2.44	3.17

More on drivers of the effect

Changes in ROLs

In Table G.4 has four panels reporting statistics about how and where students apply in the baseline scenario and in the counterfactuals for the RR in 2014, RR set at 40%, and 90%. Panel A shows the average length of the ROL. After the policy, all groups except private school students list more programs, with female students adding almost one program on average. Panel B shows that this expansion in the length of ROLs leads to a lower average utility of the programs included for all groups. Panel C and D show the average minimum and maximum utility of the programs included in students ROLs. Looking at the maximum utility, which corresponds to the first program in the list, women apply to programs they value more while men apply to programs they value less. On average, the first option of women provides about 1.6 percentage points more utility than before, while the first option of men provides about 2.3 percentage points less. The average lowest utility program included students list does not change much. These findings show that the policy leads to longer lists with lower average program quality, but also shifts in the top choice that differ by gender and school type.

Table G.4 also shows outcomes when the admission rule is applied with higher RR weights. Women are the only group that, as the RR weight increases, apply as top choice to programs they prefer more than under the 2012 criteria. In contrast, public and voucher school students apply as top choice to programs they value less than in 2012. For private school students and male students the decline is large: their top choices correspond to programs with about 27 and 17 percentage points lower utility than in 2012.

Changes are not necessarily monotonic since there are two forces at play. First, students own application score is changing due to the policy. Second, in equilibrium the expected cutoff is also changing. In the next section I study how these changes in scores can be related to the final matches after the 2014 RR policy for students that changed their match relative to the baseline.

Changes in matches: including, removing, and reapplying

To study what drives changes in admissions under the counterfactual policies, I focus on two objects: students' weighted admission scores and programs' expected cutoff scores. Both vary once the Relative Ranking policy is introduced, and together they determine who wins and who loses.

Table G.4: Descriptive statistics by group and scenario

Panel A. ROL Length				
Group	2012	2014	40%	90%
All	8.38	8.84	9.86	13.12
Male	8.41	8.58	9.08	10.86
Female	8.35	9.07	10.60	15.26
Public	8.54	9.18	10.61	15.28
Private	8.45	8.23	8.13	7.98
Voucher	8.28	8.90	10.15	14.02

Panel B. Avg. Utility				
Group	2012	2014	40%	90%
All	0.57	0.56	0.54	0.49
Male	0.56	0.55	0.52	0.45
Female	0.58	0.57	0.56	0.52
Public	0.56	0.56	0.54	0.49
Private	0.62	0.60	0.57	0.47
Voucher	0.55	0.55	0.53	0.49

Panel C. Min. Utility				
Group	2012	2014	40%	90%
All	0.15	0.14	0.14	0.12
Male	0.14	0.14	0.14	0.13
Female	0.15	0.14	0.13	0.12
Public	0.14	0.14	0.13	0.11
Private	0.15	0.16	0.16	0.14
Voucher	0.15	0.14	0.13	0.12

Panel D. Max. Utility				
Group	2012	2014	40%	90%
All	1.38	1.37	1.34	1.27
Male	1.35	1.32	1.26	1.12
Female	1.40	1.42	1.42	1.41
Public	1.37	1.37	1.35	1.33
Private	1.51	1.46	1.37	1.11
Voucher	1.33	1.34	1.33	1.30

(a) BO with earlier choice

(b) BO with new choice

(c) WO, with lower choice

(d) WO, Did not apply 2012 match

Figure G.7: Scatter of Scores - $\mathbb{E}$(cutoff) for 2012 and 2014

I classify applicants into four groups. (i) Better-off (earlier choice): admitted to a program they had applied to under the baseline but had not gained before. (ii) Better-off (new choice): admitted to a program they had not applied to in the baseline. (iii) Worse-off (reapplied, lost): reapplied to their baseline match but were no longer admitted. (iv) Worse-off (stopped applying): lost their baseline match because they did not reapply under the policy.

Figures G.7 and G.8 show that the effect of the Relative Ranking policy is driven by how both weighted scores and expected cutoffs change in equilibrium. The scatter plot in Figure G.7 compares the score minus expected cutoff gap in 2012 and 2014. Better-off students, shown in blue and orange, lie above the 45-degree line: they move into programs where their relative position improves. Worse-off students, shown in red and gray, lie below the line: their relative position worsens and they lose access to their baseline match. Figure G.8 confirms this pattern by comparing the distributions of admission scores and expected cutoffs within each group. For better-off students, scores are stable but cutoffs shift in their favor, making previously out-of-reach programs attainable or new applications worthwhile. For worse-off students, scores do not decline, but cutoffs rise relative to them, so they are pushed out of previous seats or choose not to reapply. Overall, the evidence shows that policy effects are not explained by scores alone. They come from the combination of cutoff movements and changes in application behavior that reallocate students across programs.

(a) Better-off with earlier choice

(b) Better-off with new choice

(c) Worse-off with lower choice

(d) Did not apply to 2012 match

(e) Better-off with earlier choice

(f) Better-off with new choice

(g) Worse-off lower choice

(h) Did not apply to 2012 match

Figure G.8: Densities of Scores - $\mathbb{E}$(cutoff) for 2012 and 2014