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 0.05 km measured as willingness to travel. This effect hides important heterogeneity. I find that the rule shifted 13% of applicants into more-preferred programs. Of these students, 90% were from public and voucher schools and 60% were women. Private school students and men were displaced to less preferred alternatives and showed lower graduation rates. Counterfactuals show that, compared to affirmative action, the class-rank rule delivers higher welfare for public school students and smaller reductions in graduation rates.

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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.

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). Evidence on mismatch effects is mixed, with some studies finding support (Arcidiacono et al., 2014) and others not (Bleemer, 2020).[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 rules, using a difference-in-differences framework Black et al. (2023) shows Texas's Top-Ten-Percent (TTP) plan raised enrollment, graduation, and early earnings for top students in low-income schools, without detectable harm to displaced peers. Using an regression-discontinuity design, Daugherty et al. (2014) shows that TTP beneficiaries substitute toward flagship public universities and away from private institutions. Kapor (2024) uses a structural model and decomposes the TTP effect on enrollment between direct mechanical effect and increased transparency effect. Their results attributes two-thirds of the TTP's 9 p.p. enrollment gain to the policy's added transparency rather than to its mechanical seat guarantee.[2]

This paper studies how a large-scale class-rank policy affects welfare and academic outcomes, and compares these effects to an affirmative action policy. Unlike Kapor (2024) and other studies of Top-N percent policies, students in my setting do not gain automatic admission by being among the top in their high school. Instead, top students change their application choices in response to the policy, introducing a new mechanism within class-rank policies. I use a structural model to identify the policy's drivers and measure welfare and graduation outcomes under different regimes. The model shows which groups gain or lose, how academic results change, and how the behavior of "skipping the impossible" shapes outcomes through endogenous applications. I also connect to the mismatch literature by embedding in the model the joint estimation of a human capital production function, which separates changes in graduation outcomes into institution-quality effects and match-specific effects.

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

[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] A comprehensive literature review can be found in Arcidiacono et al. (2015) and Dynarski et al. (2023).

a loss in efficiency.[3] 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, 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.

[3]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).

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:

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

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.[4]

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.

[4]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 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:

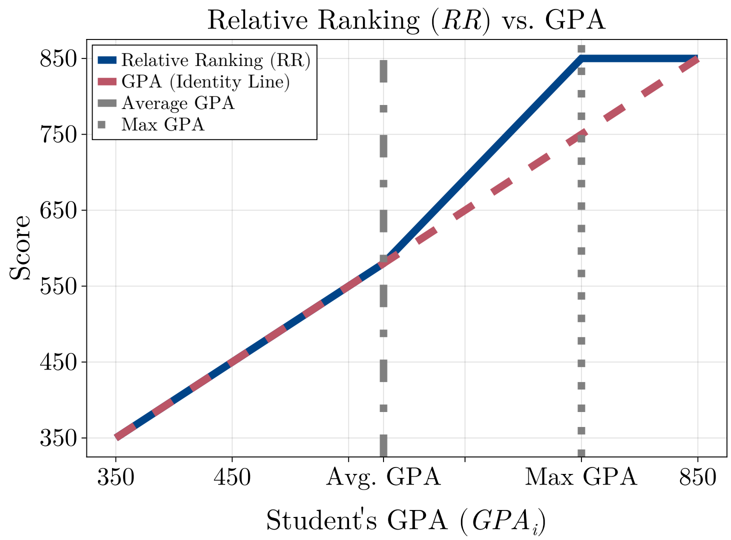
$$s_{ij} = \sum_{k \in K} \alpha'_{jk} \cdot \mathrm{PSU}_{ik} \; + \; \alpha'_{jGPA} \cdot GPA_i \; + \; \alpha_{jRR} \cdot RR_i.$$

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.

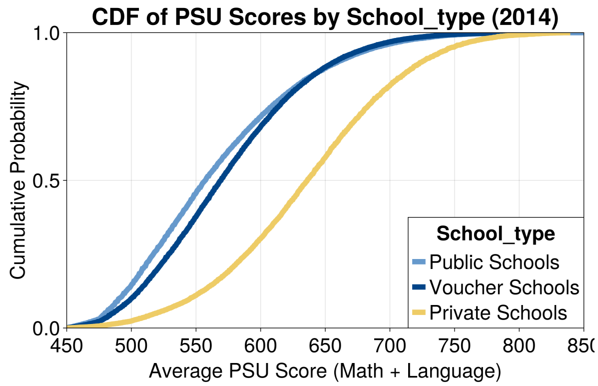
To study differences across students, I classify applicants by their school of origin type–public, voucher, or private–and use this variable as a proxy for socioeconomic status. This choice is motivated by two factors. First, at the moment of implementing the RR policy, the CRUCh had as a goal to close 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. Other evidence supports this. 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.1).

Building on this classification, Figure 1 shows how PSU scores vary by school type and gender. Panel 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 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 1c and 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.

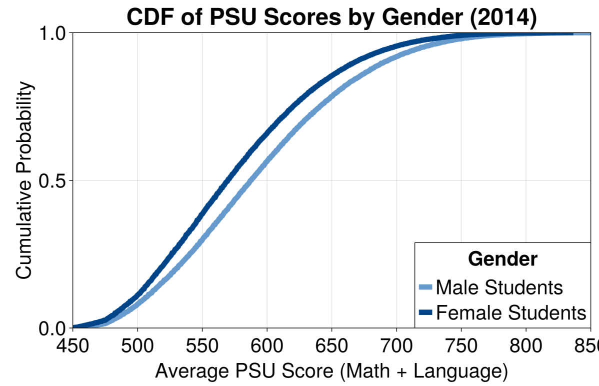
Figure 2 shows how admission weights changed over time and how programs distributed the Relative Ranking weight in 2014. In 2012, prior to the policy, programs gave on average 55.8% to Mathematics and Verbal exams, 14.9% to Science and History tests, and 29.3% to GPA. In 2013, after the policy required a 10% weight on RR, the share of GPA fell to 23.3% and Mathematics and Verbal to 52.3%. In 2014, when programs could assign between 10% and 40% to RR, the average weight on RR rose to 22.2%, with wide variation across programs, as Figure 2b shows. That year, GPA dropped to 16.0%, Science and History to 13.2%, and Mathematics and Verbal to 48.5%. A detailed Figure with the distribution of the different admissions criteria is available in Appendix B, Figure B.3.

3.2 Descriptive statistics and stylized facts

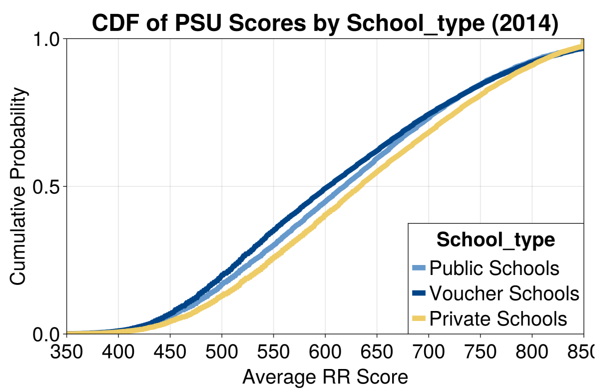
Table 1 reports the number of applicants, admitted, enrolled, and graduated students in 2012 by school type and gender. Tables B.2 and B.3 show similar patterns for 2013 and 2014. About 80% of applicants come from public or voucher schools. This share is 79% among admitted, 76% among enrolled, and 75% 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.7 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%



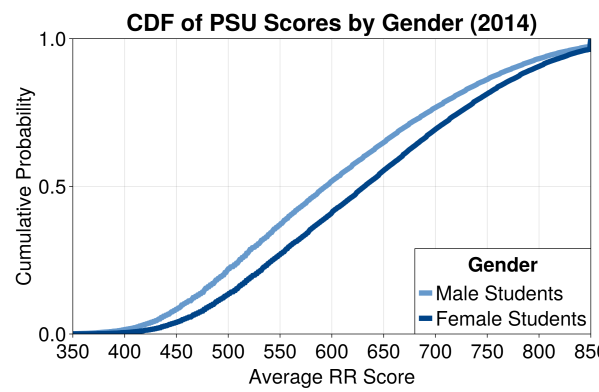
(a) Average Math and Verbal PSU scores CDF by school type (2014).



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

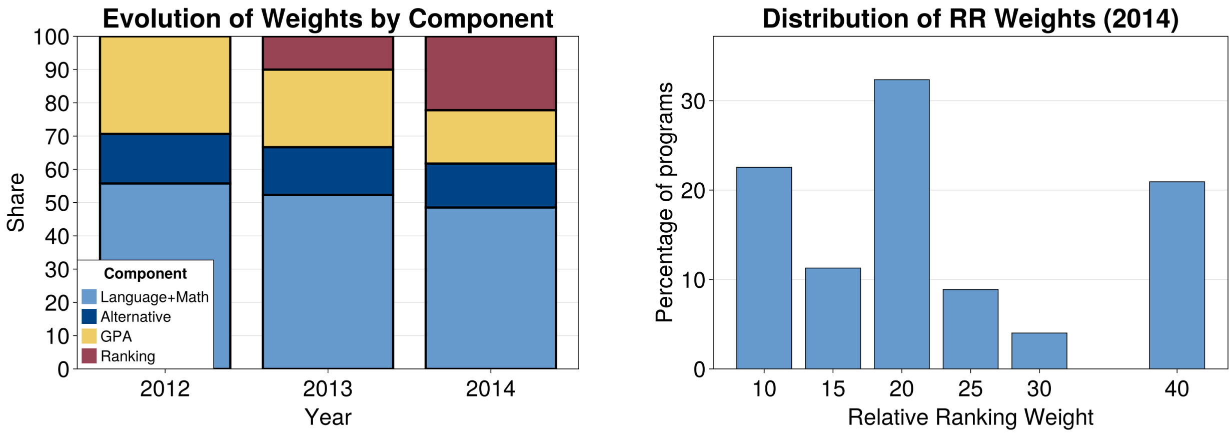


(c) Average RR scores CDF by school type (2014).



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

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



(a) Evolution of weights by component

(b) Distribution of RR weight

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

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.

Table 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

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

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

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.[5]

Utility for the outside good is given by

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

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

[5] 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 counterfactual 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 $c(z_i, w_{ij})$.[6]

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:

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

[6] 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 correlated with the preference shock ε_{ij}, with $\eta_{ij} \mid \varepsilon_{ij} \sim \mathcal{N}(\rho\varepsilon_{ij}, \sigma_\eta^2)$. Most covariates that enter the utility function also enter the human capital equation, except for distance and 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

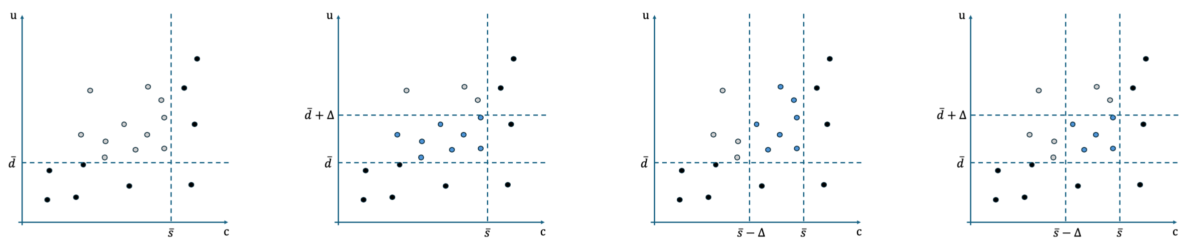
In this section I outline the identification arguments for the joint distribution of preferences and consideration index, and for the human capital production function.

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)

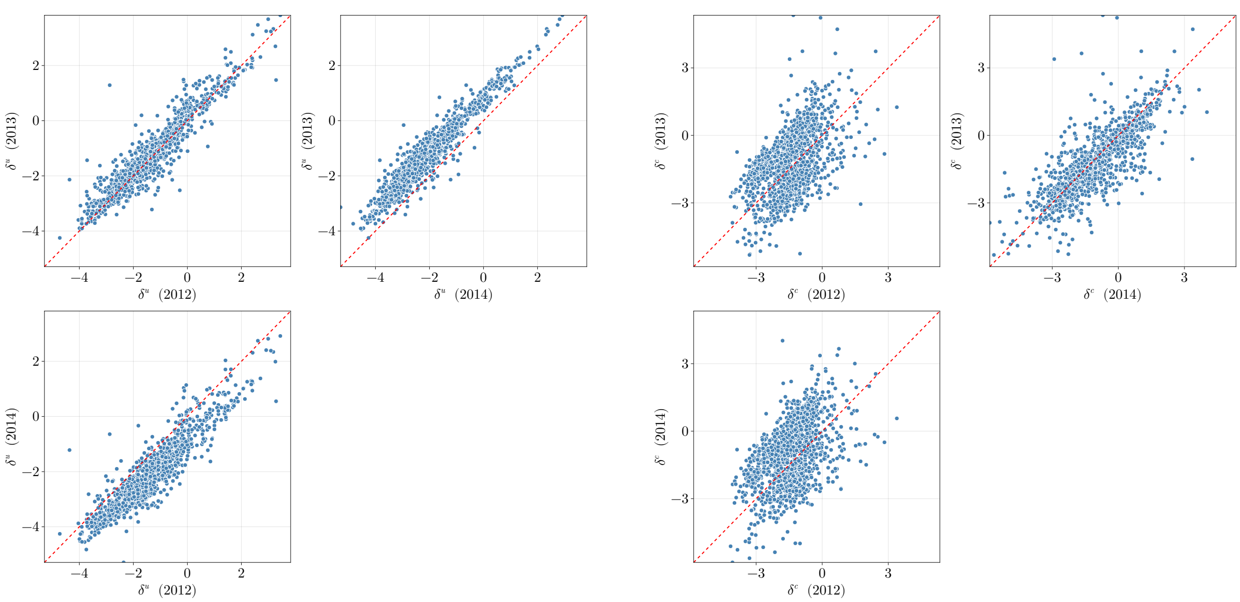
Figure 4 shows estimations and comparisons for 2012, 2013, and 2014 program specific parameters in both utility and consideration equations. The mean quality parameters δ^u for 2012, 2013, and 2014 are shown in Figure 4a, while the fixed effect in the consideration equation for the same years is shown 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

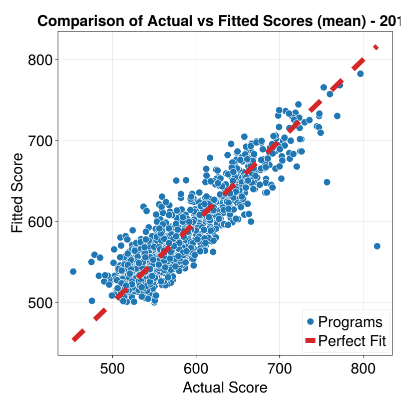
Figure 4: Comparison of δ^u and δ^c for the years 2012, 2013, and 2014.

6.2 Fit of the model

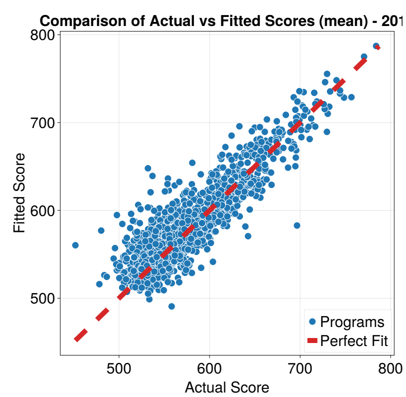
To assess model fit, I start by evaluating how well it predicts the average PSU mathematics and verbal scores of admitted students at the program level. For each program and year, I compute the average score in the actual data and compare it with the model prediction. Predictions are obtained by simulating 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.

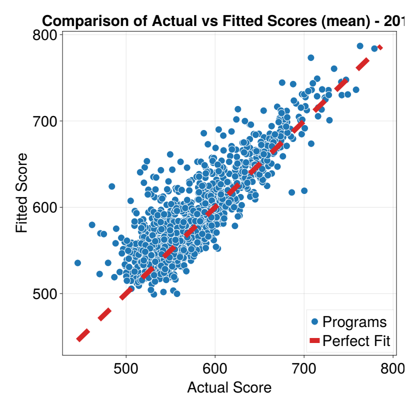
Table 4 shows actual and predicted graduation rates. At the overall level, predictions are within one percentage point of the data. For subgroups, the fit is also close: the model tends to slightly overpredict for private schools and underpredict for public schools and female students. Figure 6 compares 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

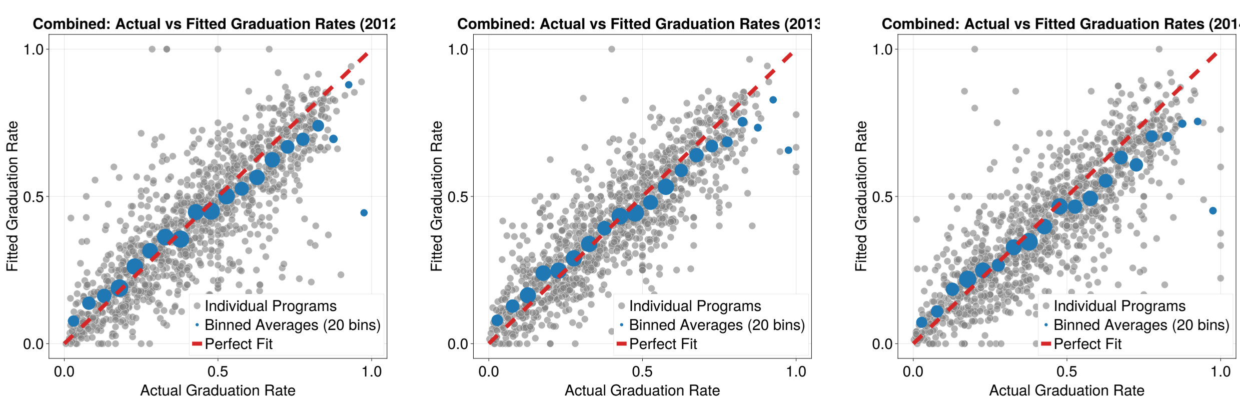


(c) Model fit 2014

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

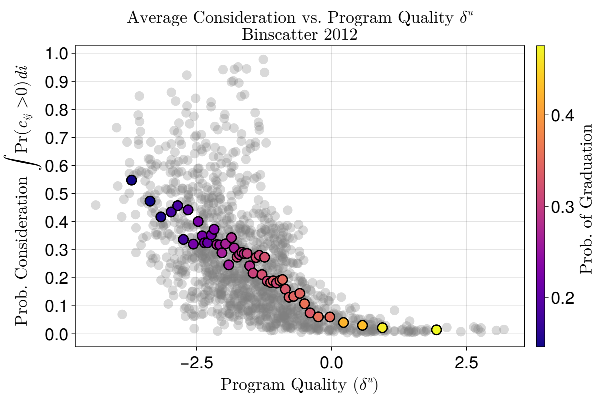


(a) Model fit 2012 **(b)** Model fit 2013 **(c)** Model fit 2014

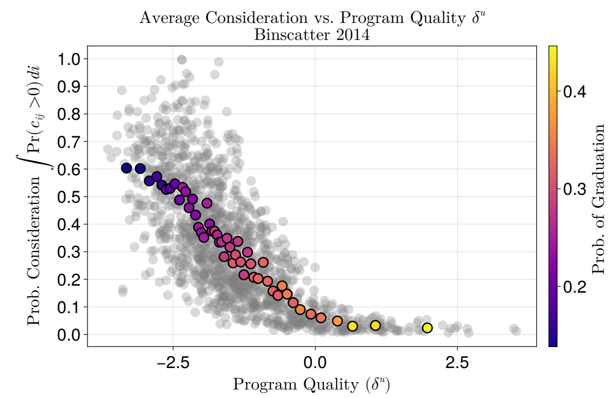
Figure 6: Model fit for graduation rates across years 2012–2014.

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



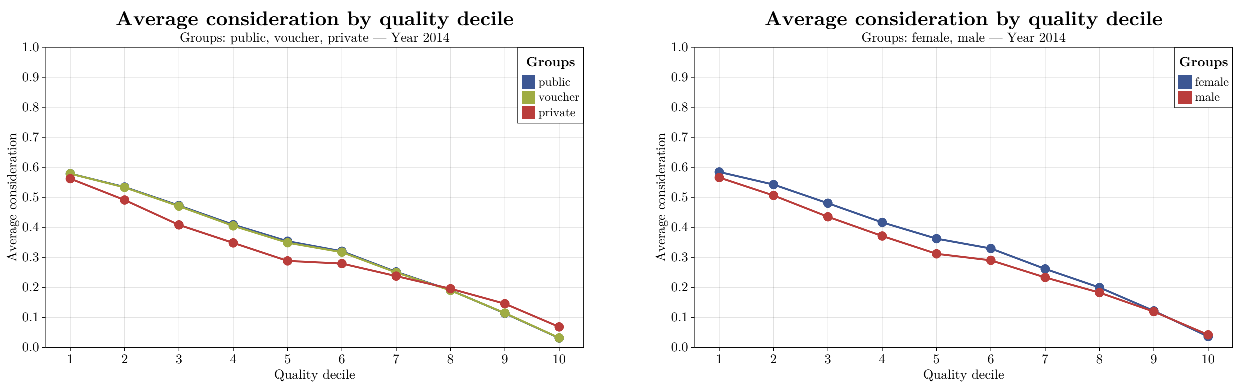
(b) 2014: RR reform slightly increased consideration at most quality levels

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.

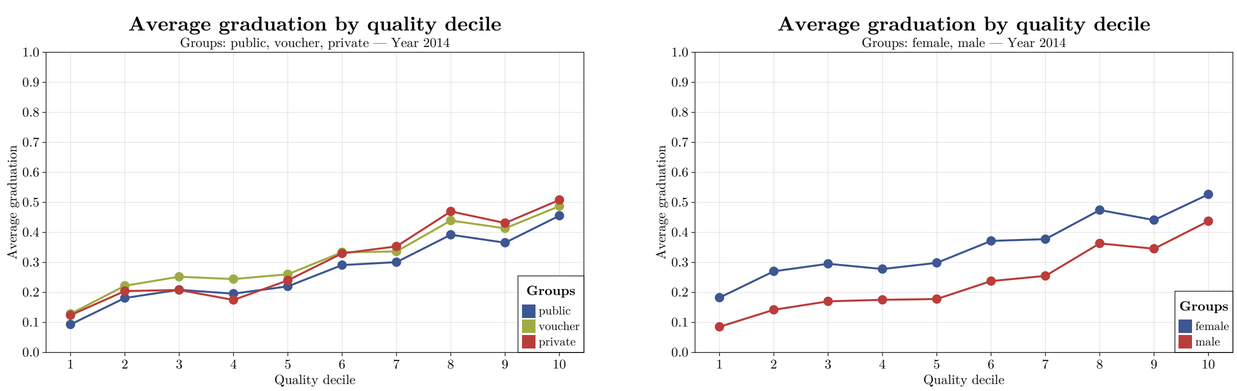
Graduation outcomes follow a different pattern. Voucher students graduate at higher rates than public school students at all quality levels, with a stable gap of about 3 percentage points. Private school students graduate at rates similar to public students at low-quality programs but converge toward 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., $\max_{j\in\mathcal{J}} \left\{|\mathbb{E}[\text{cutoff}_j]^b - \mathbb{E}[\text{cutoff}_j]^{b-1}|\right\} < \varepsilon$.

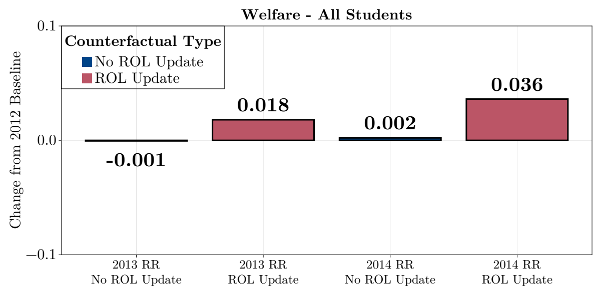
7.2 Effect of the Implemented RR Policy

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.

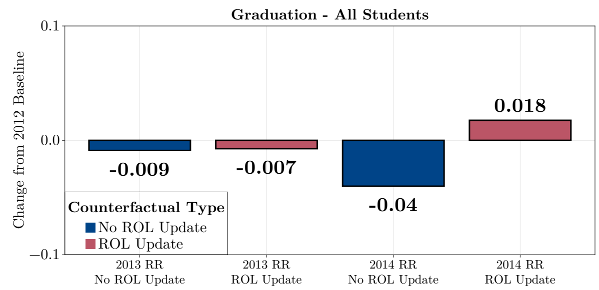
Overall Welfare Effects

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 36.5%.



(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. Figure 12 show the share of students that ended better-off, worse-off, and neutral after the implementation of the policy. This is the share of students that are estimated to be admitted to a higher utility, lower utility, or same program. The majority—78%—of students remain neutral, while 12.5% are better-off and 9.6% are worse off. Among the students that are better-off, 85% of them come from either public or voucher schools. Among the students that are worse-off 22% of them are from public schools, 46% come from voucher schools, and 32% from private schools. Almost 60% of the students that gain from the implementation of the policy are women.

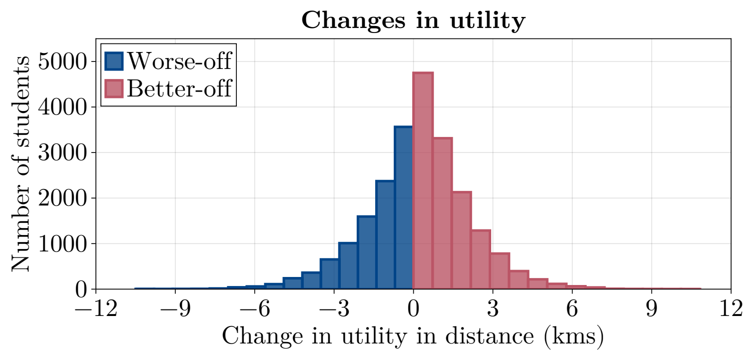
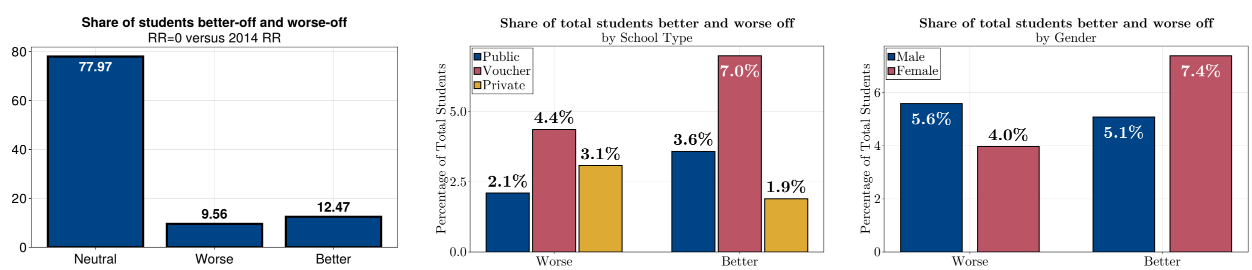


Figure 11: Changes in utility measured as willingness to travel for students that change placements between 2012 and 2014.
Note: The figure shows the distribution of the change in utility, measured as willingness to travel (in kilometers), between the 2012 and 2014 RR admission policies. Blue bars represent 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.

To put these numbers in perspective, in Table 5 I identify students that were benefited by the policy from those what were not, and provide more details about both groups. Within the better-off group, 59% are women, these being 51% of the total of applicants. While private school students correspond to 20% of the applicants, they conform 32% of the losers from the policy and 15% of the winners of the policy. Of the total of students, 64% received the boost and 86% from winners received some boost. The average score boost received was around 58 points for winners and 11 for losers (0.53 and 0.1 standard deviations of the PSU scale, respectively).



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

Figure 12: Winners and losers from the implementation of the 2014 RR policy

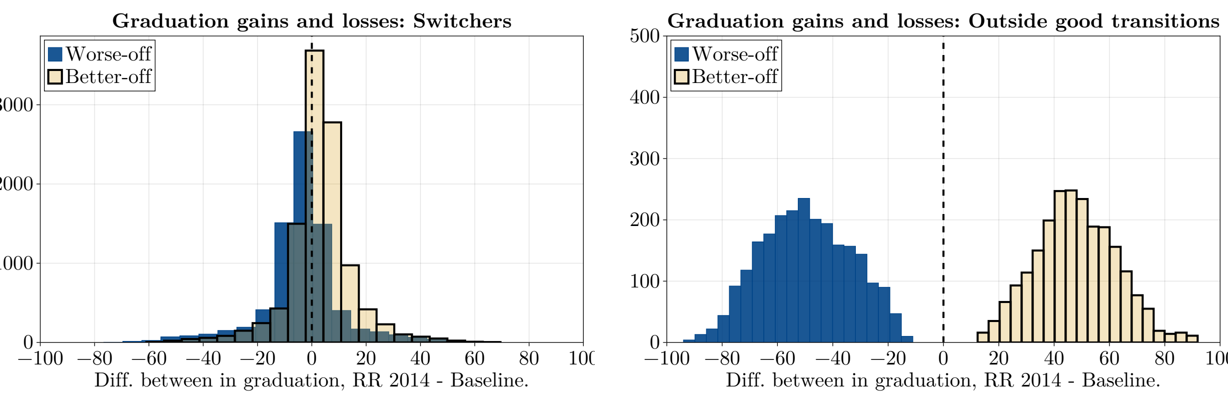
Table 5: Descriptive statistics by neutral, worse-off, and better-off students

Variable	Overall	Neutral	Worse-off			Better-off		
			Out	Switch	Total	In	Switch	Total
N students	105,155	81,989	2,390	7,658	10,048	2,243	10,875	13,118
Female %	51.4	51.4	39.7	42.1	41.5	63.0	58.5	59.2
Public sch. (%)	25.8	25.8	21.1	22.3	22.0	29.6	28.6	28.7
Voucher sch. (%)	53.9	54.5	48.1	45.0	45.8	55.5	56.2	56.1
Private sch. (%)	20.3	19.6	30.8	32.7	32.2	15.0	15.2	15.2
Got RR boost (%)	64.0	62.3	33.7	52.1	47.7	78.9	89.4	87.6
Boost	27.0	24.0	6.7	13.0	11.5	48.5	59.6	57.7
Utility Δ (kms)	0.0	0.0	-1.4	-1.5	-1.5	1.3	1.4	1.4
Grad. Δ (p.p)	-0.1	0.0	-49.6	-3.7	-14.6	47.9	3.1	10.8

Note. This table reports descriptive statistics of students grouped according to their utility change between 2012 and 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 or out of the official group between periods. Shares are expressed in percent; averages correspond to sample means.

Graduation outcomes

Average effects on graduation outcomes hide heterogeneity too. Figure 13a shows that students benefited by the policy move towards programs where they have higher graduation outcomes, while the opposite is true for students displaced from their preferred choices.



(a) Switchers between programs

(b) Moving from or to the outside good

Mismatch decomposition

Figures 10b and G.2 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. The first is the program effect, which is governed by δ_j^h in Equation (4) for each program j. The second is the match effect, which is governed by the $\phi^w w_{ij}$ in Equation (4) for each student i in program j. The decomposition exercise is shown in Figure 14.

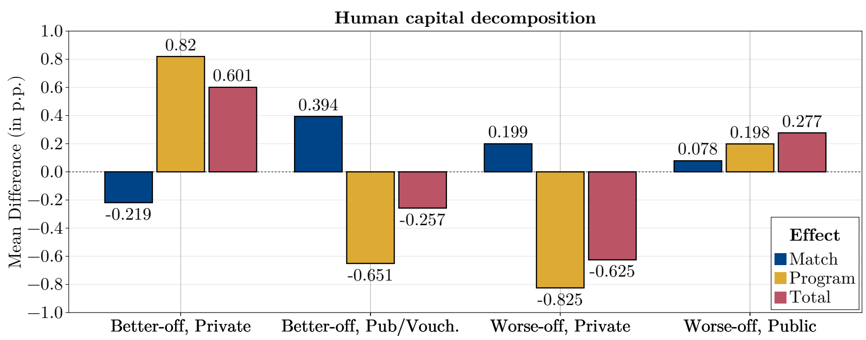


Figure 14: Decomposition of the human capital change

The decomposition exercise shows that except for public and voucher school students that are worse off, the program effect and the match effect go in opposite directions offsetting each other. This is, with the RR policy students that improve their placement move to programs with higher match value but lower overall program graduation rates.

7.3 Drivers of the policy effect

This section examines how the Relative Ranking policy changes applications and admissions. The analysis covers the 2014 RR weights, as well as counterfactual cases with RR weights set at 40% and 90%. First, it shows how students adjust their rank-ordered lists, including the number of programs they apply to and the program they place first. Then, it studies how these choices affect matches, distinguishing students who gain a better option from those who lose their baseline match. The results show that outcomes are driven by the interaction between changes in weighted scores and shifts in expected equilibrium cutoffs.

Changes in ROLs

In Table 6 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 6 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.

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

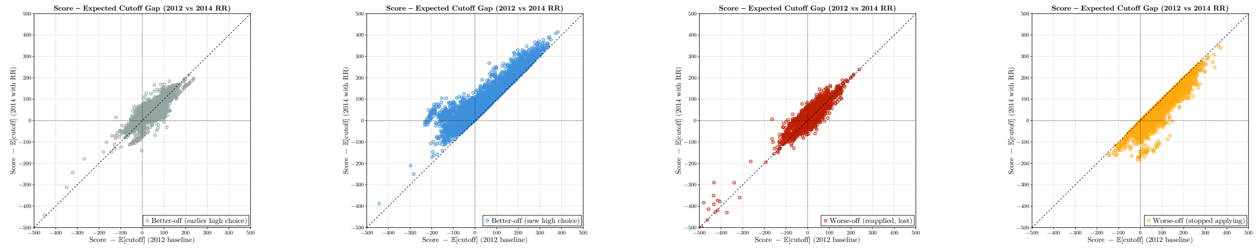
Table 6: Descriptive statistics by group and scenario

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

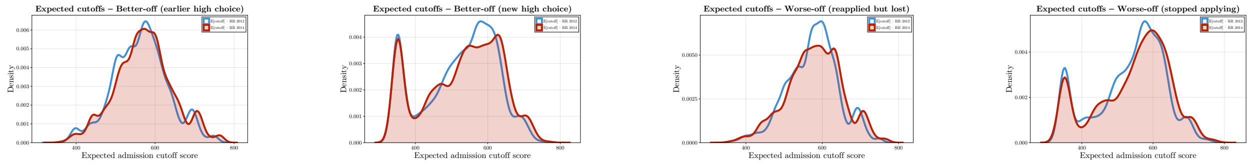
Figure 15: Scatter of Scores - $\mathbb{E}$(cutoff) for 2012 and 2014

(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 15 and 16 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 15 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 16 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.

7.4 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

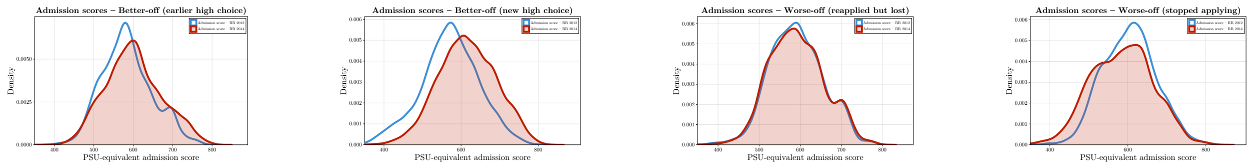


(a) Better-off with earlier choice

(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

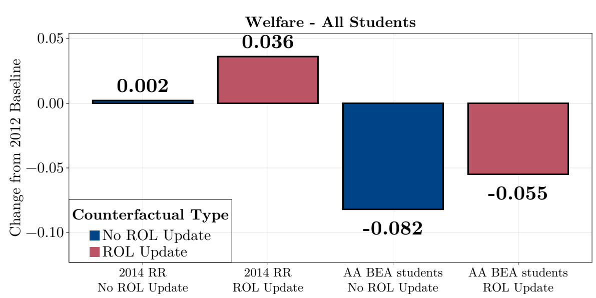
(f) Better-off with new choice

(g) Worse-off lower choice

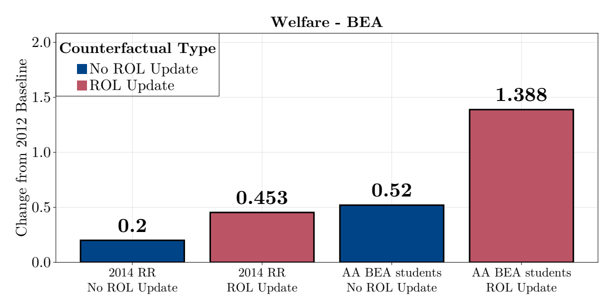
(h) Did not apply to 2012 match

Figure 16: Densities of Scores - $\mathbb{E}$(cutoff) for 2012 and 2014

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 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.



(a) Overall effect



(b) BEA students

Figure 17: Affirmative Action and RR effects compared

Under Relative Ranking (RR), about 13% of applicants are better-off and 9.6% are worse-off, for a small average welfare gain of about 0.05 km (with ROL updates). Under Affirmative Action (AA/BEA), about 10% are better-off and 16.3% are worse-off, for an

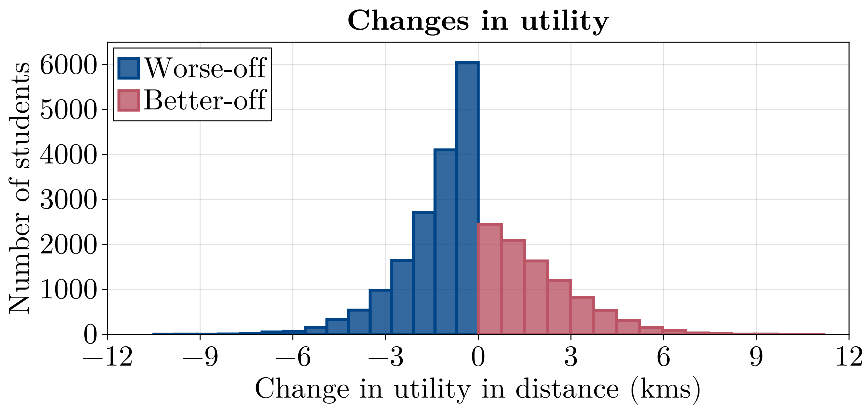


Figure 18: Overall effect

Figure 19: Changes in utility between AA and baseline for students that move

average loss of 0.03 km. Conditional on moving, gains for winners are larger under affirmative action. For students moving from a less preferred option to a higher utility one, the gain is +1.987 km under AA vs. +1.454 km under RR. For students moving from the outside good, the effect in welfare is +1.755 km vs. +1.333 km. Losses for worse-off movers are similar across policies: for students that get displaced to a less preferred program they lose about −1.43 relative to −1.46 km under RR. As for students displaced to the outside good, they lose admission to a program where they got about −1.35, relative to −1.40 km.

Under Relative Ranking (RR), about 13% of applicants are better-off and 9.6% are worse-off, for a small average welfare gain of about 0.05 km (with ROL updates). Under Affirmative Action (AA/BEA), about 10% are better-off and 16.3% are worse-off, for an average loss of 0.03 km. Conditional on moving, gains for winners are larger under affirmative action. For students moving from a less preferred option to a higher utility one, the gain is +1.987 km under AA vs. +1.454 km under RR. For students moving from the outside good, the effect in welfare is +1.755 km vs. +1.333 km. Losses for worse-off movers are similar across policies: for students that get displaced to a less preferred program they lose about −1.43 relative to −1.46 km under RR. As for students displaced to the outside good, they lose admission to a program where they got about −1.35, relative to −1.40 km.

Students who enter a program from the outside good increase their graduation probability by about +35 p.p. under both policies. Students displaced to the outside good see their graduation probability fall by about −36 to −37 p.p. The main difference is for students who move to a more preferred program. Under AA, their graduation falls by -1.389 p.p., while under RR it falls by just -0.141 p.p. Students displaced to a less preferred program show small changes in graduation: $+0.615$ p.p. under AA and $+0.006$ p.p. under RR. The AA policy benefits its target group. BEA students gain $+1.6$ km in welfare under AA, over three times their gain under RR ($+0.47$ km). They also represent a large share of students who move to a better program or enter college from the outside good.

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.

A decomposition of graduation outcomes shows the negative effect under AA for students who move to a more preferred program comes from a program effect. These students move to programs that have lower historical graduation rates. The match effect, which measures student-program fit, is small and positive. Under RR, these same forces are weaker, which explains the smaller changes in graduation. For both policies, large graduation swings are linked to students entering or leaving college, not to switches between programs. The main efficiency cost under AA comes from the mix of programs students select after cutoffs shift, not from worse fit within those programs.

	Better-off		Worse-off	
Affirmative Action	IN→IN	OG→IN	IN→IN	IN→OG
N	9,265	1,653	13,009	4,146
Δ Utility	1.987	1.755	-1.428	-1.346
Δ Grad	-1.389	35.346	0.615	-36.127

Relative Ranking	IN→IN	OG→IN	IN→IN	IN→OG
N	11,328	2,312	7,708	2,429
Δ Utility	1.454	1.333	-1.459	-1.399
Δ Grad	-0.141	35.735	0.006	-37.305

Two-thirds of better-off switchers and 55% of outside entrants are BEA. Graduation outcomes driven by program effect δ^h.

8 Conclusion

This paper studies Chile's Relative Ranking (RR) policy as a group-neutral rule in college admissions. I use a structural model of applications with endogenous consideration sets and link admissions to on-time graduation. The model matches key outcomes and its main parameters are stable across 2012–2014. It shows that students from public and voucher schools are less likely to consider high quality programs, even though those programs have higher graduation rates. The adjustment of beliefs about cutoffs is the main channel through which the RR policy has impact.

Relative to a 2012 baseline without RR, the 2014 policy raised average welfare by 2.1%. Thirteen percent of applicants moved to a more preferred program, with 86% from public or voucher schools and 61% women. Gains were about 4 to 5 percentage points per beneficiary. The mechanical effect alone was small (0.26% in 2014), while the full effect with updated beliefs was about ten times larger (2.13%). Welfare rose for public and voucher students (+3.9) and for women (+5.3), and fell for private school students (-4.0) and men (-1.1).

Graduation rates did not change much on average, but showed clear group differences. In 2014, rates rose for public and voucher students (+0.67) and for women (+0.59), and fell for private school students (-3.35) and men (-2.01). A counterfactual expansion of RR weights to 50% raised welfare by 5% overall and by 9% for public and voucher students, while lowering the mean PSU score of admitted students by only 0.02 standard deviations. For private school students both welfare and mean PSU fell as RR rose. These results show that group-neutral rules can improve equity with small efficiency costs when they change both admissions priorities and students' program choices. Future work should examine effort responses, supply adjustments, and longer-run 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.

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:

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

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

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

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

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

$$\begin{aligned}
\mu_{ij}^u &= \delta_j^u + \theta_i' x_j + \beta_w' w_{ij} + \beta_z' z_i + \beta_d d_{ij} \\
\mu_{ij}^c &= \delta_j^c + \gamma_w' w_{ij} + \gamma_z' z_i + \gamma_s \tilde{s}_{ij} \\
\mu_{ij}^h &= \delta_j^h + \phi_w' w_{ij} + \phi_z' z_i
\end{aligned}$$

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_ϵ^2, σ_ν^2, σ_η^2, σ_0^2): 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

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

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

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.

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

   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:

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

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

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.1 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. 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.1 provides program-level summary statistics for $N = 1,355$ programs across these two periods.[7] 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.1 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

[7] 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.1: 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.[8] 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.1) shows the share of students whose mother attained higher education, used as a proxy for socioeconomic status.

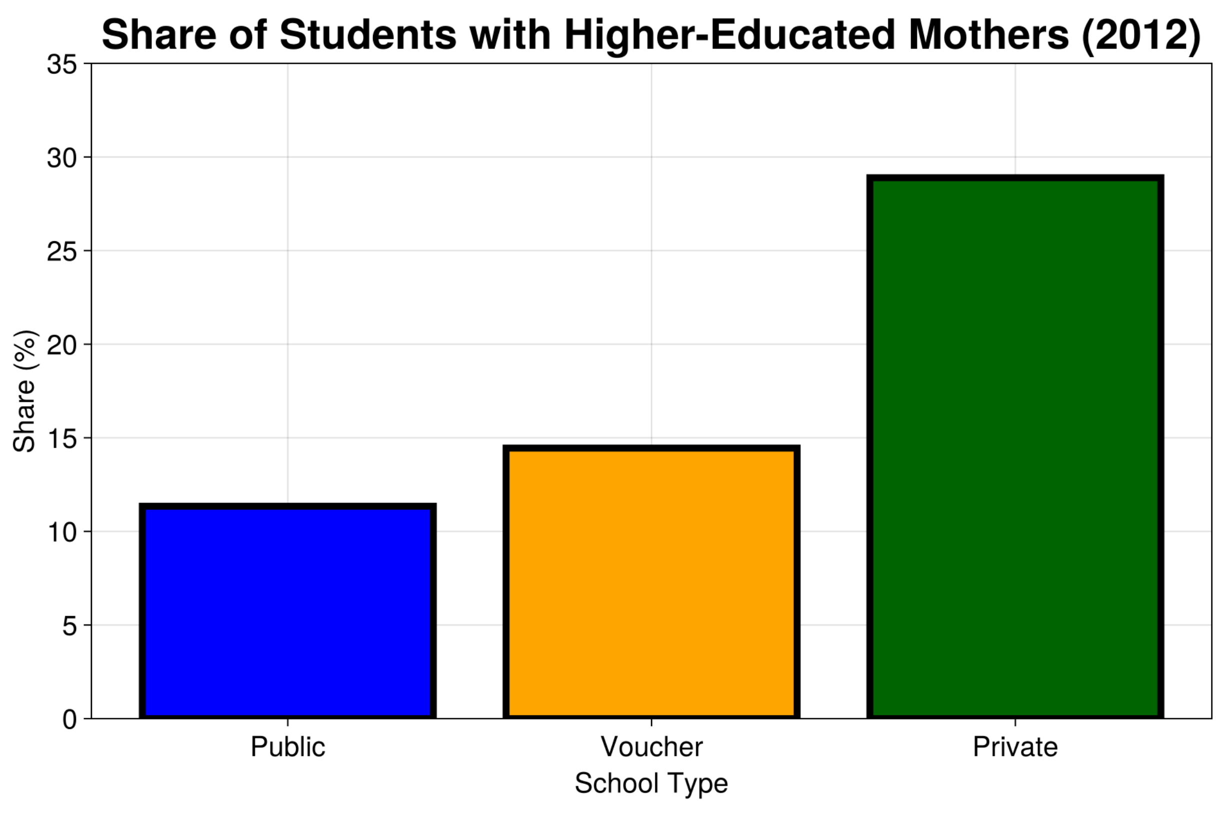


Figure B.1: Share of students with higher-educated mothers by school type.

[8]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.2: 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.3: 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.3 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.4: 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.5: 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.6: 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.7: 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.8: 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.9: 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

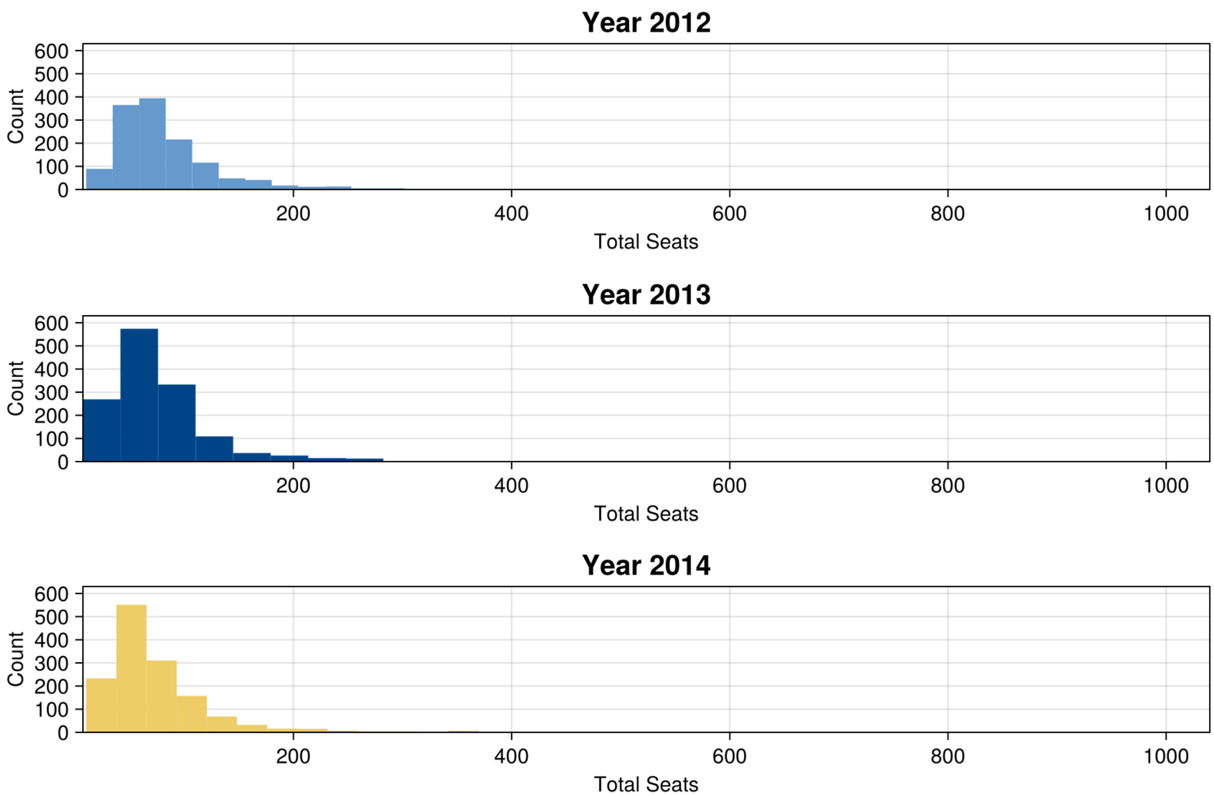


Figure B.2: Distribution of total seats across years

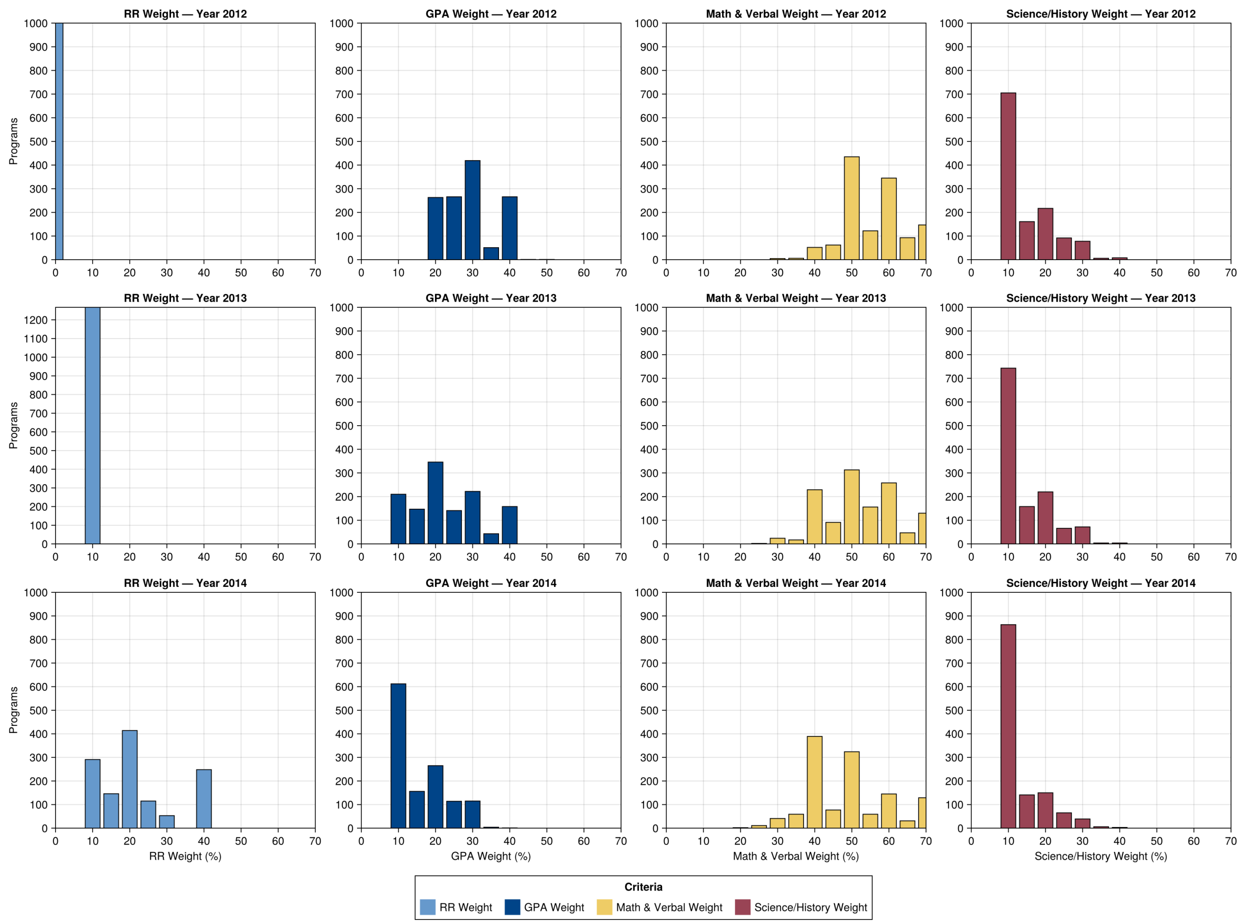


Figure B.3: Distribution of admission weights across criteria, 2012–2014. Rows show years (2012, 2013, 2014) and columns show criteria (Relative Ranking, GPA, Math & Verbal PSU, and Science/History PSU). Each bar indicates the number of programs assigning a given percentage weight to that component. 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.

C Results appendix

Here I show and discuss the estimation results in full.

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

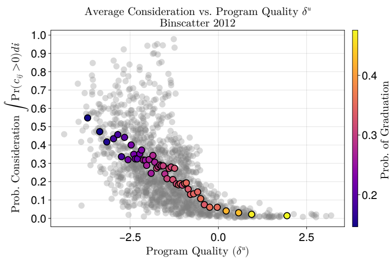
	(1)	(2)	(3)	(4)	(5)	(6)
	2012		2013		2014	
Variable	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Inside Good (β)						
STEM $\times$ Math	0.187	(0.003)	0.155	(0.002)	0.177	(0.003)
STEM $\times$ Verbal	-0.155	(0.005)	-0.111	(0.002)	-0.117	(0.005)
STEM $\times$ Sex	1.024	(0.004)	0.831	(0.009)	0.816	(0.015)
STEM $\times$ Recent Grad	0.083	(0.017)	0.028	(0.003)	-0.004	(0.003)
STEM $\times$ Private School	-0.460	(0.008)	-0.524	(0.008)	-0.514	(0.016)
STEM $\times$ Public School	-0.046	(0.006)	-0.014	(0.004)	-0.025	(0.004)
Selectivity $\times$ Math	0.163	(0.005)	0.082	(0.003)	0.071	(0.002)
Selectivity $\times$ Verbal	0.108	(0.002)	0.066	(0.002)	0.071	(0.002)
Selectivity $\times$ Sex	-0.069	(0.002)	-0.126	(0.003)	-0.097	(0.003)
Selectivity $\times$ Recent Grad	-0.036	(0.003)	-0.048	(0.002)	-0.025	(0.002)
Selectivity $\times$ Private School	-0.091	(0.007)	-0.045	(0.003)	-0.042	(0.005)
Selectivity $\times$ 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$\times$ Recent Grad	-0.113	(0.015)	0.024	(0.015)	0.049	(0.013)
Avg. Mate-Verb^2 $\times$ Recent Grad	0.002	(0.009)	-0.027	(0.010)	-0.020	(0.010)
Avg. Mate-Verb^3 $\times$ Recent Grad	-0.006	(0.005)	-0.047	(0.003)	-0.028	(0.002)

Table C.2: Consideration estimates: parameters

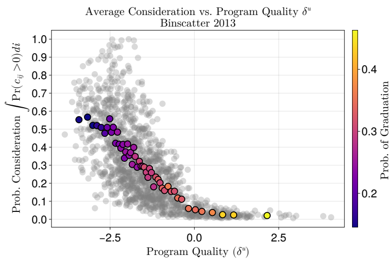
	(1)	(2)	(3)	(4)	(5)	(6)
	2012		2013		2014	
Variable	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Consideration Eq. (γ)						
Sex	0.452	(0.005)	0.064	(0.017)	0.012	(0.024)
Private	0.212	(0.014)	-0.352	(0.022)	-0.251	(0.032)
Public	0.012	(0.003)	0.049	(0.003)	0.008	(0.003)
Avg. Mate-Verb.	-0.528	(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

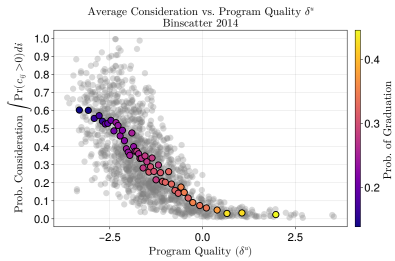
	(1)	(2)
	Panel Estimation	
Variable	Coeff.	Std. Err.
Phi Parameters (ϕ)		
ρ	0.169	(0.011)
Sex	-0.294	(0.007)
Private	-0.000	(0.009)
Public	0.031	(0.009)
Avg. Mate-Verb.	0.247	(0.007)
Avg. Mate-Verb.^{2}	-0.013	(0.005)
Avg. Mate-Verb.^{3}	-0.004	(0.001)
Scholarship	0.337	(0.018)
Scholarship^2	-0.502	(0.031)
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

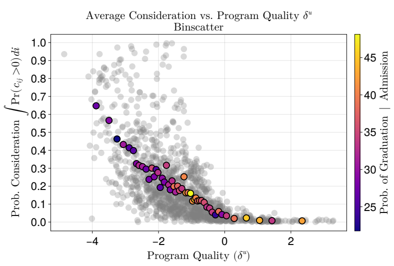


(b) All programs, 2013

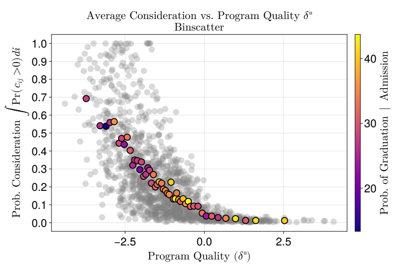


(c) All programs, 2014

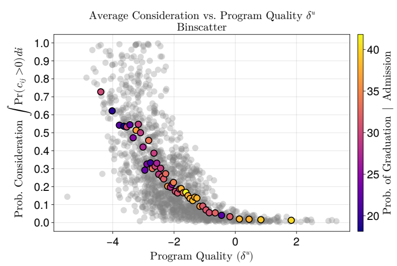
Figure C.1: Average program quality δ^u and average consideration, all programs, 2012–2014.



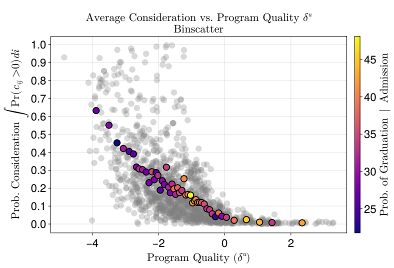
(a) Public, 2012



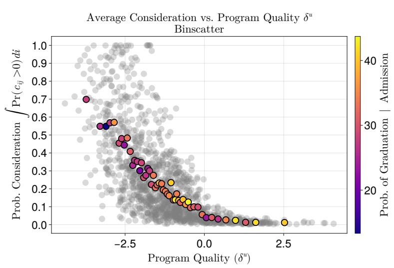
(b) Public, 2013



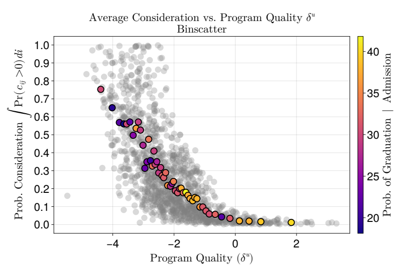
(c) Public, 2014



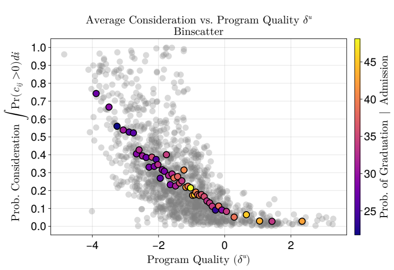
(d) Voucher, 2012



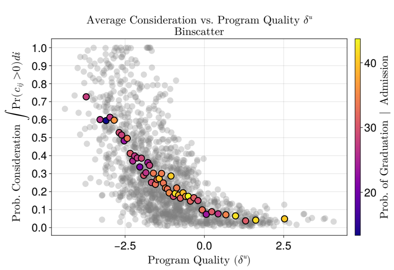
(e) Voucher, 2013



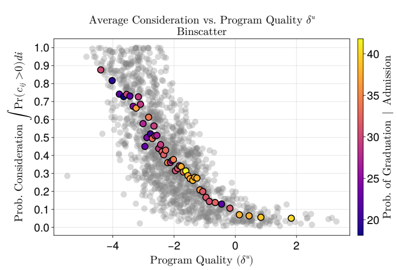
(f) Voucher, 2014



(g) Private, 2012

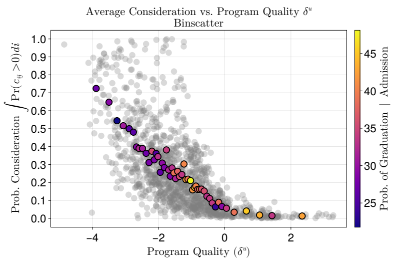


(h) Private, 2013

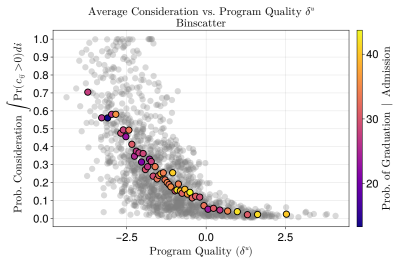


(i) Private, 2014

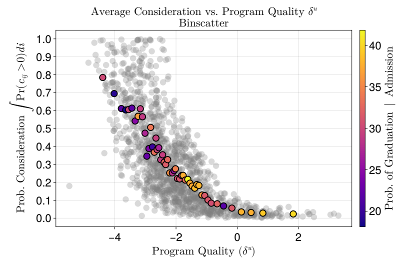
Figure C.2: Average program quality δ^u and average consideration by school type, 2012–2014.



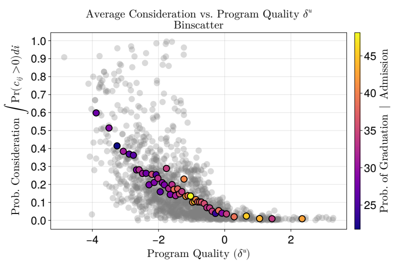
(a) Male, 2012



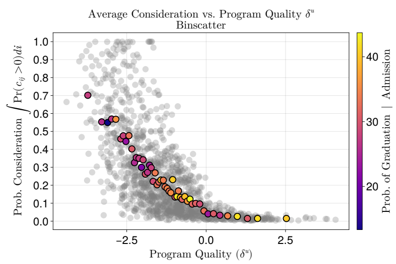
(b) Male, 2013



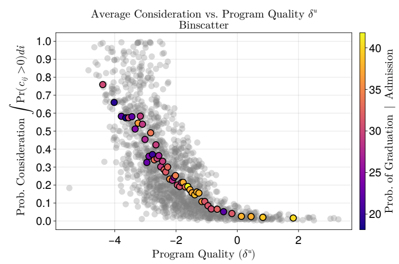
(c) Male, 2014



(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.

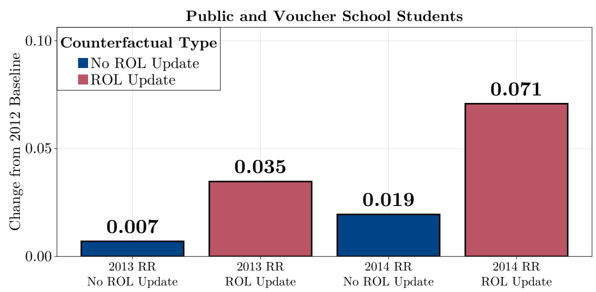
Figure G.2 presents the graduation outcomes. Public and voucher students gained about 0.4 percentage points in 2013 and 0.7 points in 2014 (Figure G.2a). Private school students lost about 1.3 points in 2013 and 3.4 points in 2014 (Figure G.2b). Women gained about 0.3 points in 2013 and 0.6 points in 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.

G.1 Counterfactual descriptives

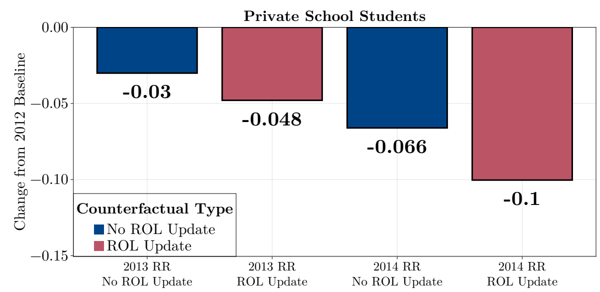
Table G.1: Summary statistics of utility change for winners and losers

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

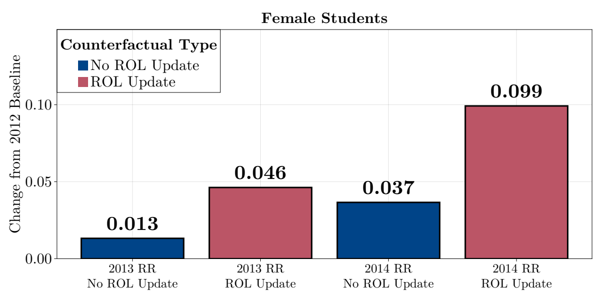
Note: The table reports summary statistics of the change in utility, measured as willingness to travel (in kilometers), between the 2012 admission policy and the 2014 RR policy. "Positive" groups represent students whose utility increased under the new policy, while "Negative" groups represent those 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.



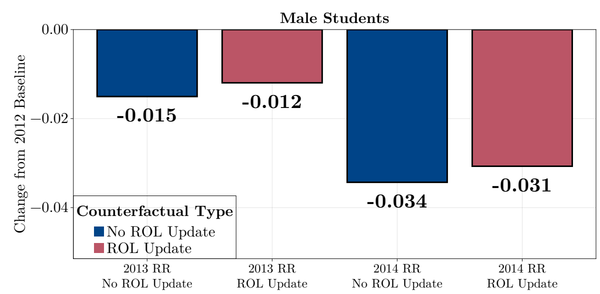
(a) Public and voucher school students



(b) Private school students

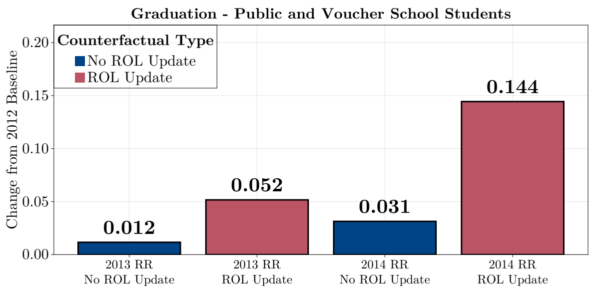


(c) Female students

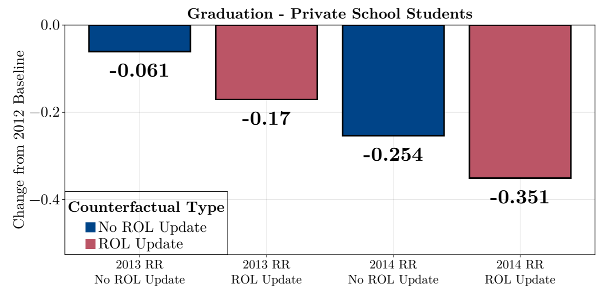


(d) Male students

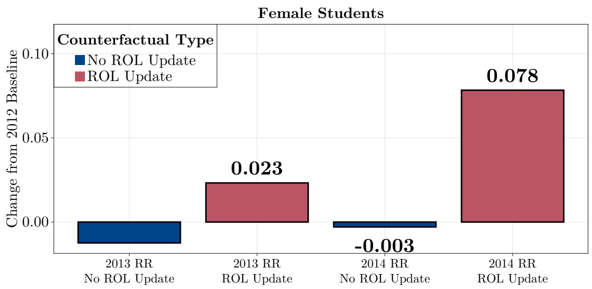
Figure G.1: Estimated welfare effects of the RR policy by student characteristics. Top row: comparison by high school type (public/voucher vs. private). Bottom row: comparison by gender (male vs. female).



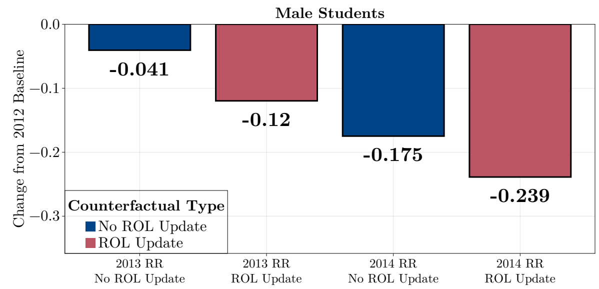
(a) Public and voucher school students



(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).

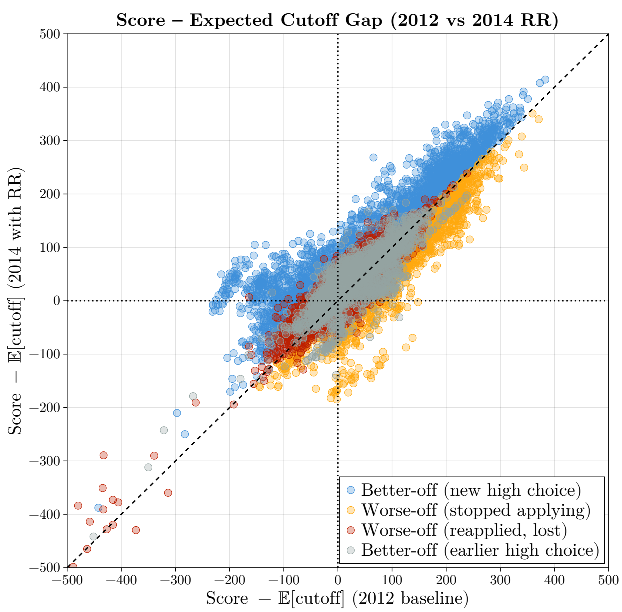


Figure G.3: Scatter of Scores - $\mathbb{E}$(cutoff) for 2012 and 2014

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

G.2 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 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

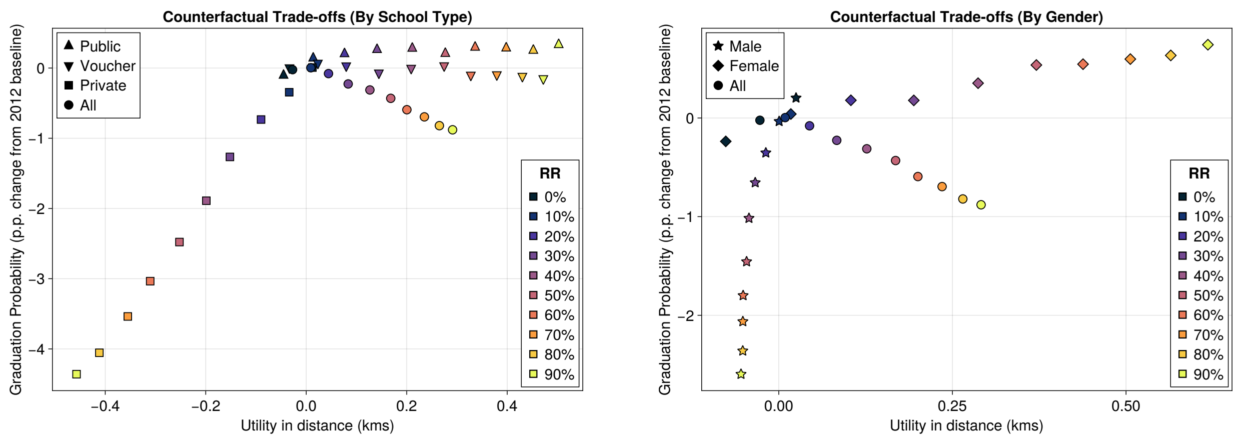
Expanding RR increases the admission and welfare of top students, primarily from public and voucher schools, as previously shown. However, this may also lead programs to admit less prepared students, increasing dropout risk and overall mismatch. Figure G.4 presents the results of the counterfactual exercise involving RR expansion. Each dot in Figure G.4 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.4 and Table G.3 present the counterfactual policy frontier disaggregated by school type (Panel G.4a) and gender (Panel G.4b). 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.4: 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