Who Wins with Class-Rank College Admissions?

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Abstract

Class-rank admission policies aim to promote equity without the political costs of affirmative action. I study the effects on admission and graduation outcomes of Chile's Relative Ranking rule, a class-rank policy which raises college admission scores for the top students at every high school. Using administrative data on the universe of applicants from 2012 to 2014, I estimate a structural model of college choice, endogenous consideration sets, and graduation outcomes, where the policy changes where students apply in equilibrium. The rule led 13% of applicants to enter a more-preferred program, with gains concentrated in students from public and voucher schools and women. Average welfare rose by 2.1% compared to a scenario without the rule, while private school students and men faced losses. Accounting for students' endogenous response is important, as it amplifies the policy's effect by a factor of ten relative to the mechanical effect alone. Graduation outcomes changed little for the average student, but fell for private school students and men. Counterfactuals show that expanding the policy further enhances equity with only a minimal trade-off in the average test scores of admitted students.

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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 class-rank admissions (Bleemer, 2023, Mukherjee, 2025). These policies are group-neutral, they increase admission chances for top students within each high school, and intend 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), and as affirmative action, present trade-offs between quality and fit (Arcidiacono and Lovenheim, 2016).

The goal of this paper is to examine the effects of a class-rank policy, identify the students who benefit from it, and assess the trade-offs of expanding such a policy to the point of replacing test based admissions. A central feature of class-rank policies is that they alter admissions criteria in a group-neutral manner. In other words, their impact is not confined to a specific demographic. Any student who performs above average within their school but has relatively weak test scores may also benefit. This study investigates whether such policies improve admission outcomes for groups with well-documented test score disparities and unequal college access, such as those defined by socioeconomic status (Camara and Schmidt, 1999, Dixon-Román et al., 2013, Chetty et al., 2025) and gender (Bedard and Cho, 2010, Niederle and Vesterlund, 2010, Bordón et al., 2020). Additionally, I analyze the policy's impact on graduation outcomes for these groups and consider whether concerns about "mismatch," frequently raised in discussions of access-oriented policies (Arcidiacono et al., 2015), are relevant in the context of the class-rank system evaluated here.

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 aim was to value performance in context and reduce gaps between students from low value-added public schools and high value-added private schools (CRUCh, 2012). Chile's Centralized Admission System (CCAS) is well suited to study such policies. It is one of the world's oldest centralized admission systems using the

Deferred-Acceptance (DAA) algorithm.[1] Universities publish seats and criteria for each program, students state preferences after standardized tests, and a DAA allocates seats. The RR policy relied on a tractable formula that increased scores in proportion to how much a student's GPA exceeded the school's average. Administrative data from the Ministry of Education (MINEDUC) and DEMRE (*Departamento de Evaluación, Medición y Registro Educacional*) make it possible to follow both admission and graduation outcomes.

Estimating the policy effect on any outcome faces the classical challenge of causal inference: I observe each student either before or after the implementation of the policy. Taking a student application to college pre-policy and see how admission changes with the post-policy weights will not capture the causal effect of the policy. This approach would assume that the student does not respond to the change in admissions criteria.[2] If the policy makes certain programs more attractive to top-ranked students—programs they would not have considered otherwise—ignoring this behavioral response could bias estimates downward by holding application lists fixed.

To estimate the effect of the RR policy on admission and graduation outcomes, I develop a structural model of higher-education applications that embeds a human capital production function. The policy operates through two channels: (i) a mechanical effect from changes in admission scores, and (ii) a behavioral effect from changes in application choices. Students apply to programs they prefer over their outside option and where they believe they have a positive chance of admission, a behavior consistent with evidence that applicants avoid programs perceived as out of reach (Fack et al., 2019, Larroucau and Rios, 2020, Fabre et al., 2024). I capture this with student-specific consideration sets, which reflect beliefs about program cutoffs. These beliefs are anchored in rational expectations that adjust in equilibrium to reflect the aggregate response to the policy. Admissions are then determined by a deferred-acceptance algorithm, after which human capital production links admission to graduation outcomes.

I use data from the CCAS covering the year before the RR policy (2012) and the first two years after its implementation (2013–2014). The data include students' rank-ordered lists of programs as well as programs' admission criteria, such as weights on standardized

[1]The centralized admission process began in 1967. It combined an admission test, similar to the SAT, with a single-offer assignment system that used the deferred acceptance algorithm. According to Neilson (2024), by 2024, 57 countries used some form of centralized assignment mechanism for higher education.

[2]Previous research has conducted descriptive analysis implementing strategies that hold fix students applications and change the admissions criteria (Larroucau et al., 2015, Barrios, 2018). The only causal analysis done has studied only the first year of the implementation of the RR policy, leveraging the fast and arguably non expected way it was implemented (Reyes, 2022). This is later discussed in the literature review section.

tests, GPA, and RR. The identification of the model 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 weighted admission scores as a shifter of consideration. While distance negatively affects a student's utility from attending a program, I assume it does not affect whether the program is considered as long as I condition for factors affecting awareness and information. This is, distance is conditionally independent to probability of being admitted. Furthermore, I assume that the student-program specific weighted score only affects consideration and not preferences. Students preferences for programs academic content, which could be correlated with scores, is captured when I condition on students raw scores. The variation left in the weighted score comes from the way different programs weight different tests, GPA, and RR, which is exogenous to the student. 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. 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 as doing maximum-likelihood estimation (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 these students, 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 increased admission and graduation 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, beneficiaries gained 4–5 percentage points in utility from their placements. In terms of graduation, rates changed little for public or voucher school students and for female students. In contrast, they fell by 3.4 percentage points for private school students and 2 percentage points for male students. Overall, the policy raised average student welfare while leaving average graduation outcomes largely unchanged.

I study a counterfactual scenario where I expand the policy in an aggressive way, and explore the role of a potential equity-efficiency trade-off. The implementation and expansion of the policy does not show significant detriments in the quality of admitted students, measured as average PSU score. For private school students, no trade-off exists; as the RR policy expands, their welfare decreases, and admitted private school students have lower average PSU scores. Conversely, for public and voucher school students, the welfare gains from expanding the policy to 50% across all programs lead to an average welfare gain of 9 percentage points of the baseline welfare. The average loss relative to the baseline in admitted student average PSU score is -0.02 standard deviations of the PSU scale. This points to an existent but limited equity-efficiency trade-off.

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).[3] 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.[4] In this paper, I use a structural model to provide evidence of effects on welfare of a group-neutral policy. The structural model allows me to quantify welfare changes, and identify winners and losers of the policy. Moreover, it allows me to disentangle the extent to which the "skipping the impossible" behavior mediates outcomes of the policy.

Studies on Chile's Relative Ranking policy reach different conclusions. Barrios (2018) finds that policy beneficiaries had lower first-year retention, which suggests academic mismatch. Using a difference-in-differences design, Reyes (2022) re-examines the policy. Her study shows that "pulled-up" students enrolled in more selective programs and also graduated from them at higher rates. This gain in selective degrees did not reduce overall BA completion. Therefore, Reyes (2022) concludes the policy improved equity without a loss in efficiency.[5] 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.

[3]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.

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

[5]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 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. This score is a weighted sum of the student's PSU section scores and their high school GPA, which is first converted to a PSU-equivalent score. Formally, the admission score of student i for program j is calculated as:

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

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

Prior to the test, during the winter, programs set their admissions criteria by defining weighted scoring formulas to rank applicants. For example, STEM programs often assign greater weight to the mathematics and science sections of the standardized test. At the same time, programs publish their 'capacities,' specifying the number of students they intend to admit. Next, early in the summer, students take the national standardized admissions test (PSU, *Prueba de Selección Universitaria*). After receiving their scores, they submit applications, which include Rank Order Lists (ROLs) of their preferred programs in descending order. Programs then rank applicants based on their pre-established weighted formulas. Finally, once all applications are submitted, admissions are determined using a College-Proposing DAA (Rios et al., 2021). This algorithm processes student preferences, program rankings, and program capacities to match students to programs, respecting mutual preferences until all slots are filled or all eligible students are placed.[6]

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

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

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

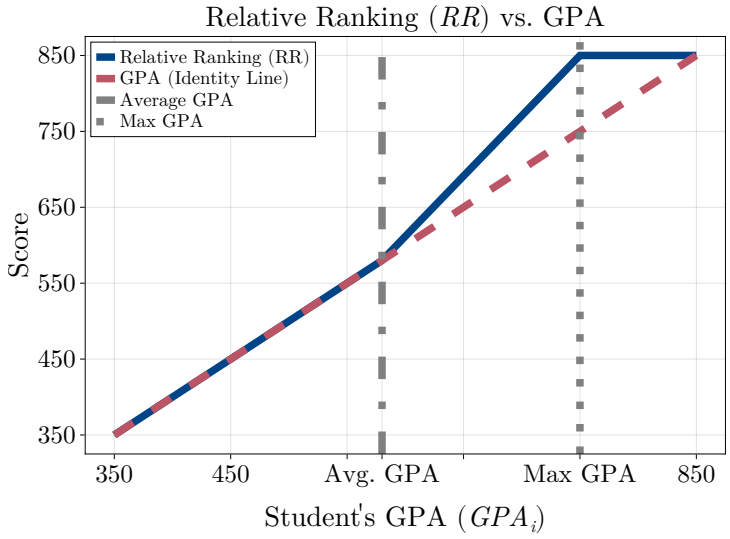
The admission component that the RR added to the weighted score is a boosted GPA score, and was incorporated in the following way:

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

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.

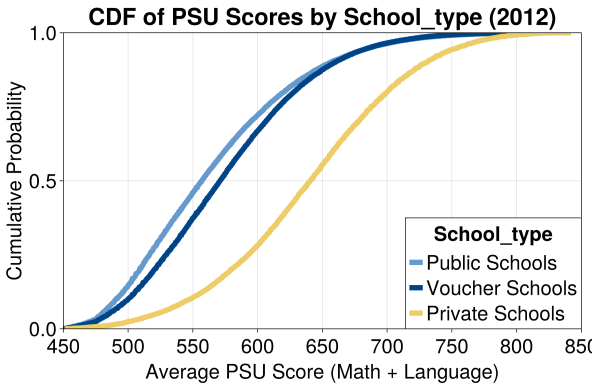
3.1 Sample description

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.

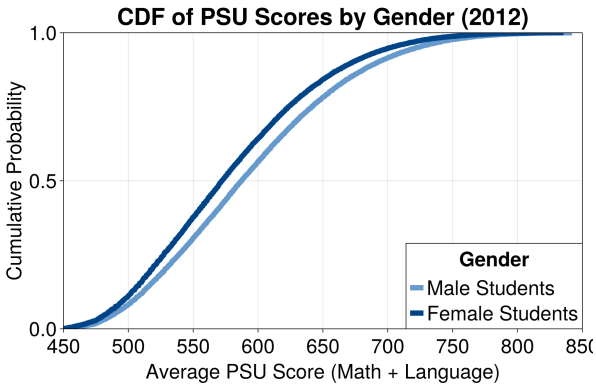
To study differences across students, I classify applicants by their school of origin type–public, voucher, or private–and use this variable as a proxy for socioeconomic status. This choice is motivated by two factors. First, at the moment of implementing the RR policy, the CRUCh had as a goal to close 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 socioeconomic background and PSU scores vary across school type and gender. Panel 1a shows that the score distribution of private school students stochastically dominates those of public and voucher school students. These patterns help explain the policy goal: to increase admission chances for students from disadvantaged backgrounds who do not perform well on the standardized test. Panel 1b shows that male students stochastically dominate the female students distri-

bution of scores. Both figures confirm that socioeconomic and gender gaps documented elsewhere are also observed in the Chilean context. Moreover, they suggest that the RR policy may help top students in public and voucher schools as well as top female students within their high school.



(a) Average Math and Verbal PSU scores CDF by school type (2012).



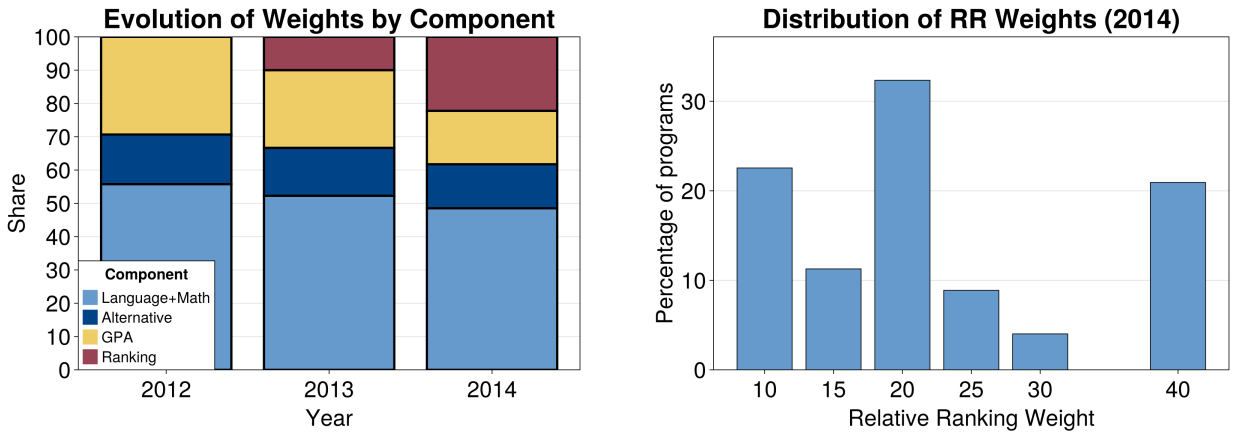
(b) Average Math and Verbal PSU scores CDF by school type (2012).

Figure 1: Socioeconomic composition and PSU scores by school type. Panel (1a) shows the distribution by school type. Panel (1b) shows the distribution by gender.

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



(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%.

rates. Admission rates are 87–88% for public and voucher students and 91% for private students. Enrollment rates are 65–68% for public and voucher students and 81% for private students. Men have higher admission (91% vs. 85%) and enrollment rates (73% vs. 67%), but lower graduation rates once enrolled (32% vs. 49%). These results point to weaker take-up among public and voucher students and lower completion among men.

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.[7]

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

[7]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 $c(z_i, w_{ij})$.[8]

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:

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

[8]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 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}.

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.

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 how different programs weight various elements of the PSU test, GPA, and the RR.

This argument is formalized in Assumption 5.1, which is adapted from Assumption 1 in Agarwal and Somaini (2022).

Assumption 5.1. *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 5.1, 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.

Assumption 5.2. *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 5.1 (Lemma 1 in Agarwal and Somaini (2022))**.** *Fix (z_i, x_j, w_{ij}). Suppose that assumptions 5.1 and 5.2 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.*

Proof. See appendix A.1 in Agarwal and Somaini (2022). □

This result shows that a reduced form version of the consideration probability is identified. Unfortunately, for counterfactuals this is not enough. Since we are using an approximation, the parameters may change when s_{ij} varies, making counterfactual

predictions unreliable.[9] To make progress, I assume that in counterfactuals the deviation from the rationally expected cutoff is invariant to the RR policy. Since the utility and consideration equations are identified from cross-sectional variation, I can test this assumption. The model is identified within a market t, so for each year I can estimate a set of parameters and compare them. This allows me to check if the estimated coefficients for the assumed primitives stay the same across different RR policy settings. In the results section I provide evidence on this point.

Human capital production function

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 (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.

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.

[9] This can be the case when

- The weighted score enters both the belief about probability of admission of the students and the awareness of the student for different programs.
- The weighted score enters non-linearly in the index governing the believed probability of admission. In that case, we would be doing a version of a linear approximation to the index.

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 $\tilde{\eta}_{ij} \sim \mathcal{N}(0, \sigma_{\tilde{\eta}}^2)$. 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.

Figure 3 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 3a, while the fixed effect in the consideration equation for the same years is shown in Figure 3b. 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 this variation is not driven by changes in the RR policy and it's effect on expectation about cutoffs.

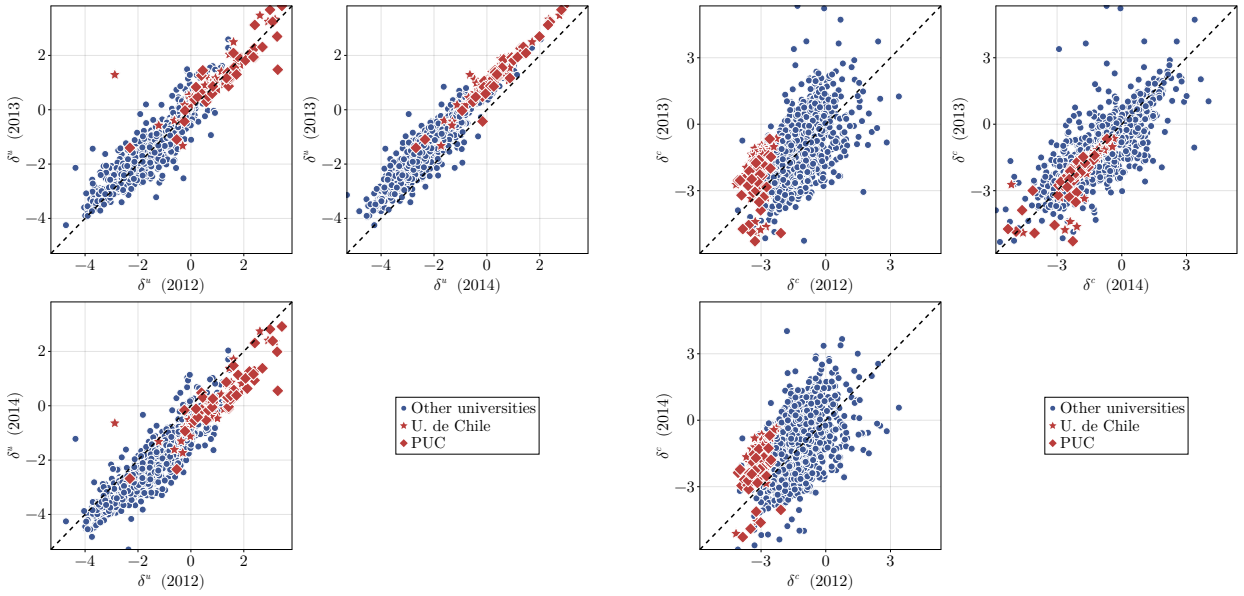
Table 2: Selected Preference and Consideration Parameter Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	2012		2013		2014	
Variable	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Inside Good (β)						
Net Price	-0.067	(0.010)	-0.031	(0.005)	-0.041	(0.006)
Distance	-0.467	(0.019)	-0.420	(0.013)	-0.393	(0.021)
Outside Good (ψ)						
Sex	1.098	(0.098)	0.670	(0.062)	0.533	(0.054)
Private	0.195	(0.029)	0.013	(0.069)	0.516	(0.086)
Recent Grad.	-0.104	(0.031)	-0.175	(0.087)	-0.078	(0.082)
Consideration Eq. (γ)						
Weighted Score	1.116	(0.037)	1.570	(0.286)	1.636	(0.307)
Same Region	0.396	(0.015)	0.357	(0.028)	0.323	(0.038)
Sex	0.415	(0.040)	0.104	(0.042)	0.090	(0.052)
Private	0.143	(0.017)	-0.010	(0.020)	0.316	(0.014)
Recent Grad.	-0.078	(0.006)	-0.139	(0.020)	-0.128	(0.029)

Table 3: Selected Human Capital Production Function Parameter Estimates

	(1)	(2)
	Panel Estimation	
Variable	Coeff.	Std. Err.
Phi Parameters (ϕ)		
ρ	0.093	(0.010)
Same Region	0.125	(0.012)
Net Price	-0.044	(0.017)
Sex	-0.274	(0.013)
Private	-0.067	(0.017)

The mean quality parameters estimates are sensible. Both figures, 3a and 3b, 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 3: Comparison of δ^u and δ^c for the years 2012, 2013, and 2014.

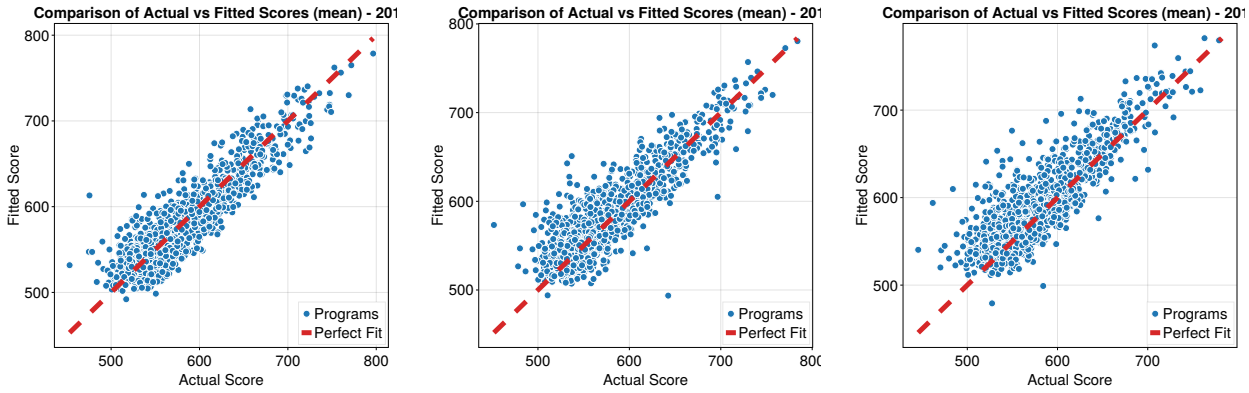
6.2 Fit of the model

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 4 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 5 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.

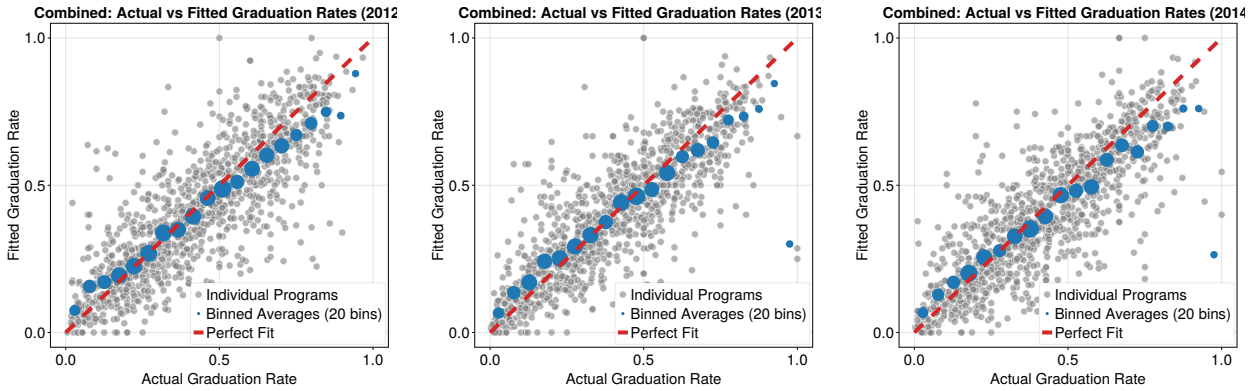


(a) Model fit 2012 **(b)** Model fit 2013 **(c)** Model fit 2014

Figure 4: 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 5: Model fit for graduation rates across years 2012–2014.

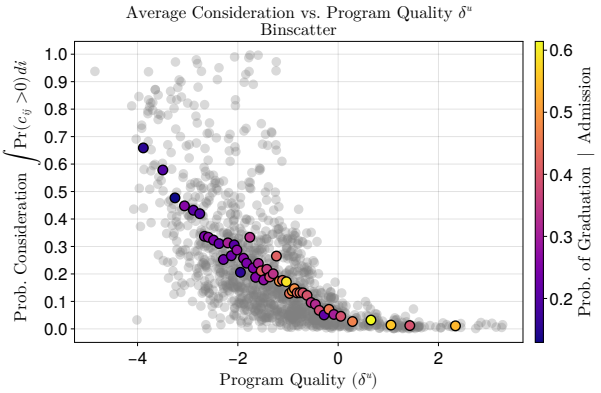
6.3 Correlation between program quality, probability of consideration, and graduation

Figure 6 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 6 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.

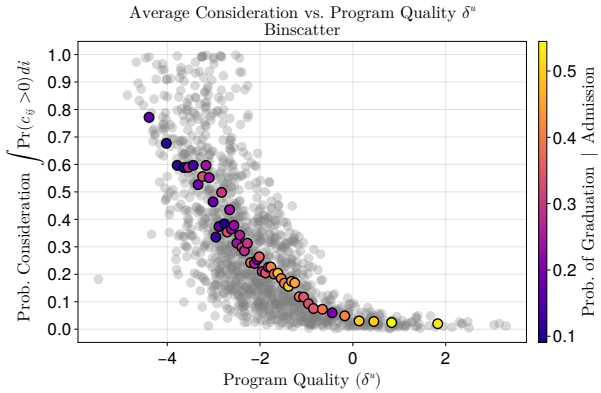
Heterogeneity between public and private schools

To unpack these patterns, Figures 7 and 8 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 per-



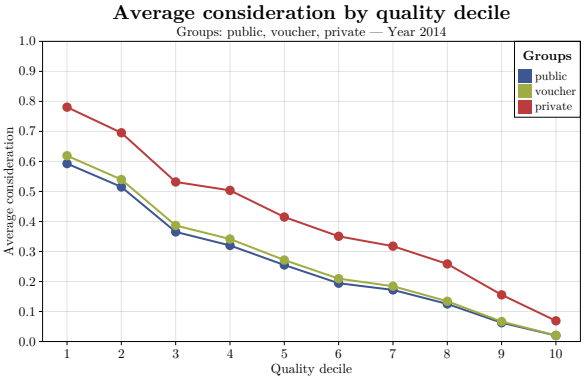
(a) 2012: high quality → low consideration, high graduation



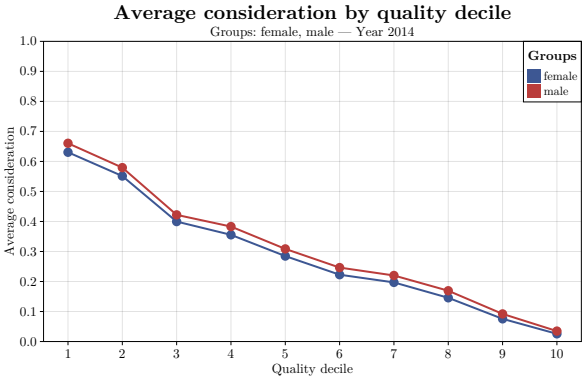
(b) 2014: RR reform slightly increased consideration at most quality levels

Figure 6: Program quality δ^u, average consideration, and graduation. Gray dots are programs; colored dots are local averages where color indicates graduation rates.

centage 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.

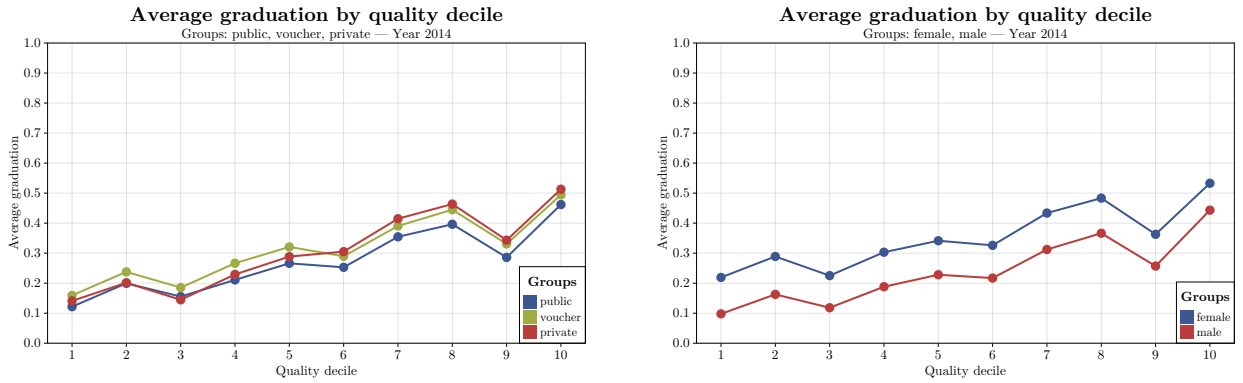


(a) By school type



(b) By gender

Figure 7: 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 8: 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 policy's actual impact and the potential gains and trade-offs of more aggressive designs.

In all counterfactuals I compare students utility and graduation outcomes, overall and between groups by school type and gender. The policy had a positive effect on student utility. The overall effect of the 2014 policy was an increase of 2.1% over the baseline student welfare (welfare under no policy scenario). Among students who benefited from the policy's implementation, 90% came from voucher and public schools. Conversely, private school students, on average, experienced a 3 percentage point decrease from their average pre-policy utility. Finally, expanding the policy to a RR of 50% homogeneously across all programs decreased admitted student test scores by approximately 0.02 PSU standard deviations, while increasing overall welfare by 5 percentage points. This indicates an existing, albeit limited, trade-off between student quality and student utility.

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. The welfare change, ΔW, is calculated as:

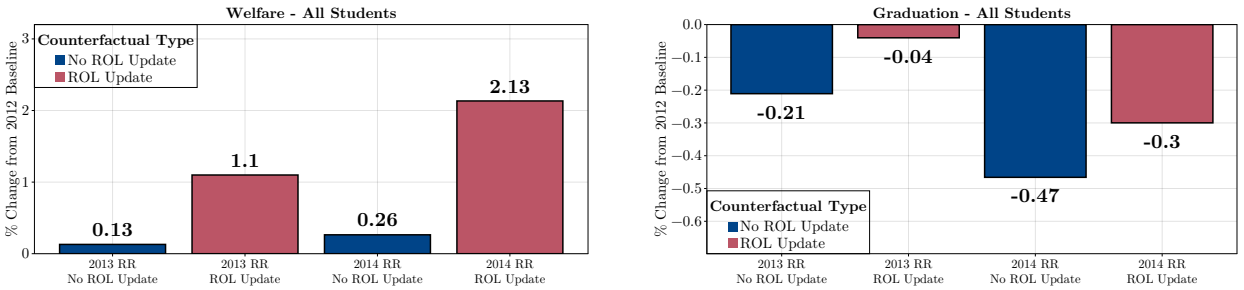
$$\Delta W = \frac{W(\mu^{RR'}) - W(\mu^{2012})}{W(\mu^{2012})}.$$

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 9a 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 a 1.1% welfare gain under the 2013 rule and 2.1% under the 2014 rule. In both cases, the full effect is about ten times larger than the purely mechanical effect.

In contrast, Figure 9b shows a small reduction in graduation outcomes. The 2013 implementation lowered the average graduation rate by -0.04 percentage points relative to the baseline, while the 2014 version lowered it by -0.3 percentage points. Although negative, these effects are very small in magnitude.



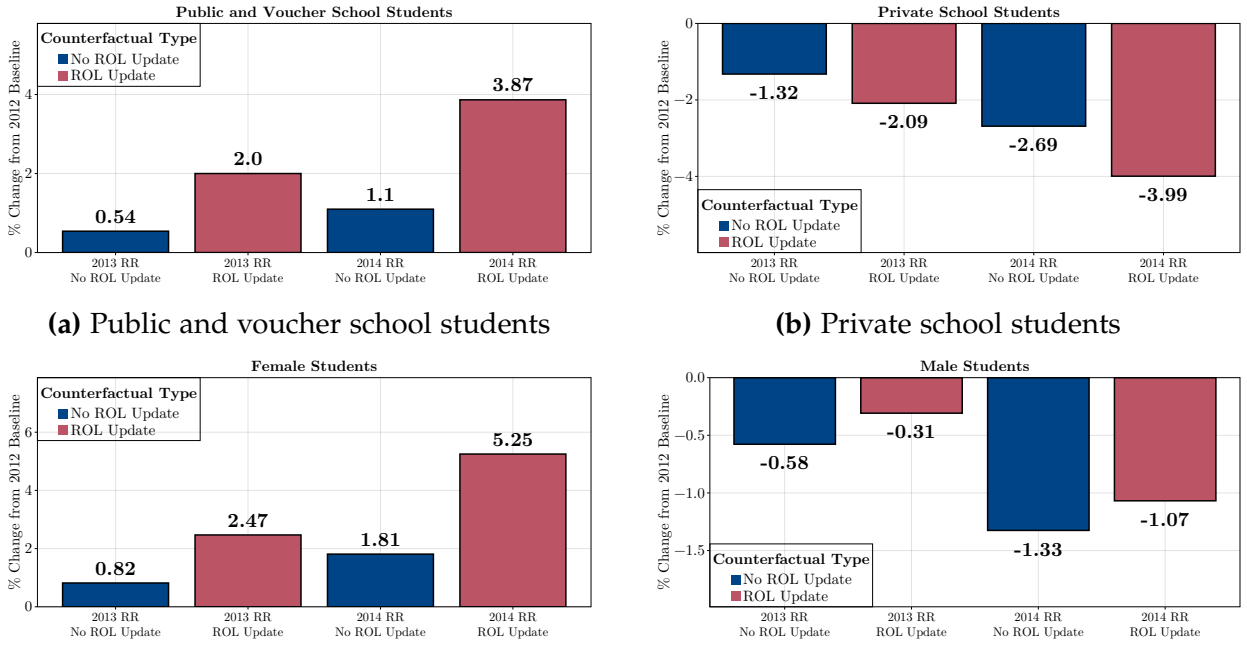
(a) Welfare effects **(b)** Graduation effects

Figure 9: Effects of the RR policy on student welfare (left) and graduation (right).

Heterogeneity in Welfare Effects

Figure 10 shows the welfare effects of the 2013 and 2014 RR implementations. Public and voucher students benefited, gaining about 2.0 percentage points in 2013 and 3.9 points in 2014 (Figure 10a). Private school students lost about 2.1 points in 2013 and 4.0 points in 2014 (Figure 10b). Women gained about 2.5 points in 2013 and 5.3 points in 2014 (Figure 10c), while men lost about 0.3 points in 2013 and 1.1 points in 2014 (Figure 10d). Most of the welfare gains accrued to the intended target groups.

Figure 11 presents the graduation outcomes. Public and voucher students gained about 0.4 percentage points in 2013 and 0.7 points in 2014 (Figure 11a). Private school students lost about 1.3 points in 2013 and 3.4 points in 2014 (Figure 11b). Women gained about 0.3 points in 2013 and 0.6 points in 2014 (Figure 11c), while men lost about 0.8 points in 2013 and 2.0 points in 2014 (Figure 11d). The distribution of winners and losers in graduation mirrors the welfare results.



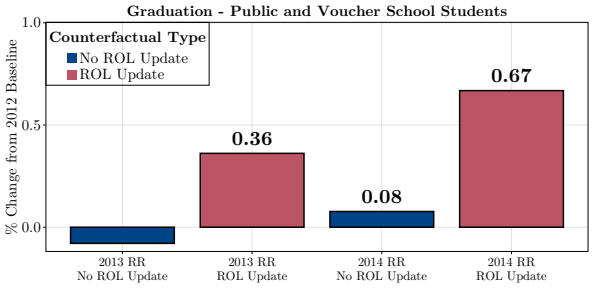
(a) Public and voucher school students

(b) Private school students

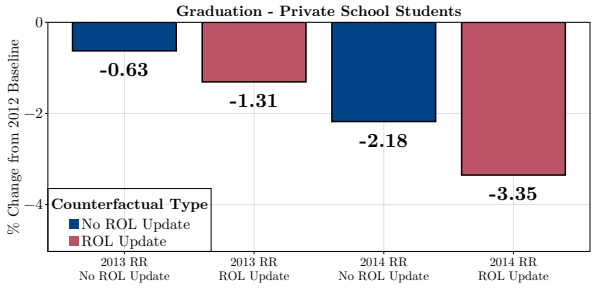
(c) Female students

(d) Male students

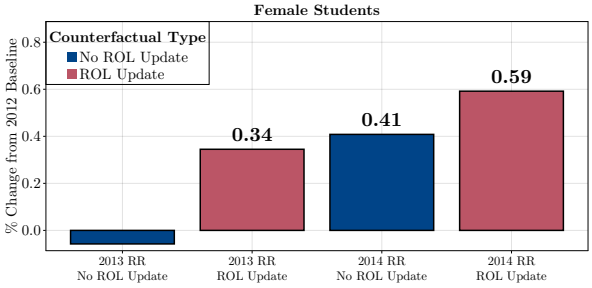
Figure 10: Estimated welfare effects of the RR policy by student characteristics. Top row: comparison by high school type (public/voucher vs. private). Bottom row: comparison by gender (male vs. female).



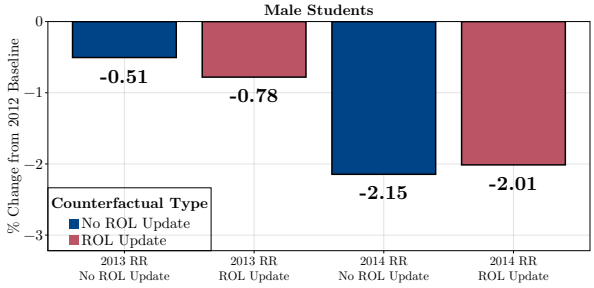
(a) Public and voucher school students



(b) Private school students



(c) Female students



(d) Male students

Figure 11: 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).

7.3 Effect of Alternative Designs of the RR Policy

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

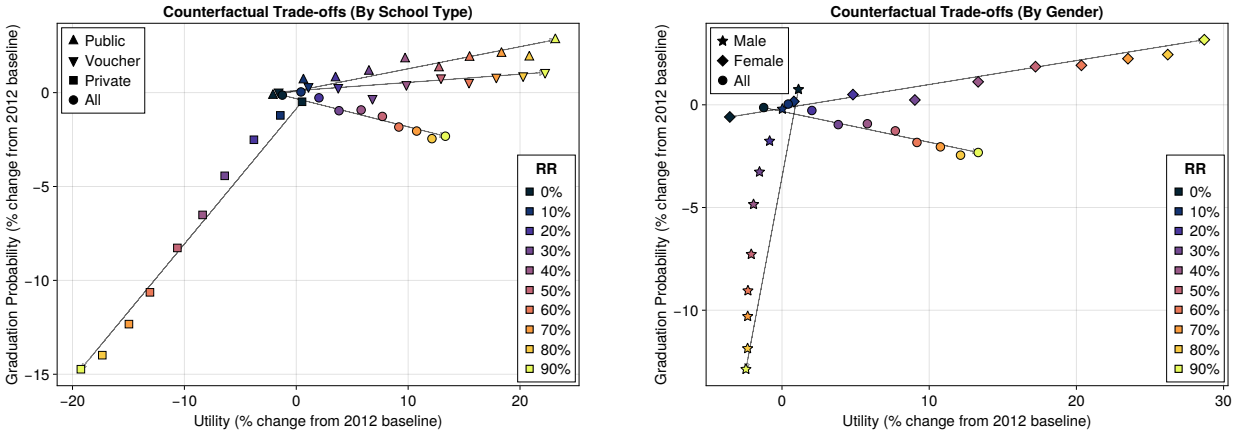
Expanding RR increases the admission and welfare of top students, primarily from public and voucher schools, as previously shown. However, this may also lead programs to admit less prepared students, increasing dropout risk and overall mismatch. Figure 12 presents the results of the counterfactual exercise involving RR expansion. Each dot in Figure 12 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 12 and Table E.2 present the counterfactual policy frontier disaggregated by school type (Panel 12a) and gender (Panel 12b). 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 12: Counterfactual policy range for RR across groups

7.4 Drivers of the policy effect

Changes in ROLs

In Table 5 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 5 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.

Table 5: Descriptive statistics by group and scenario

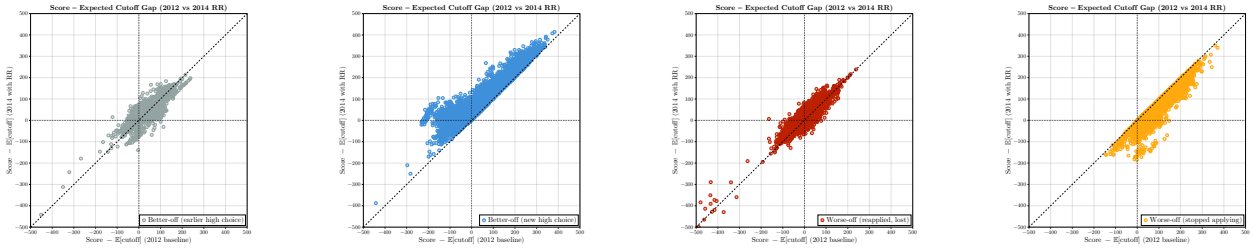
Panel A. ROL Length				
Group	2012	2014	40%	90%
All	8.38	8.84	9.86	13.12
Male	8.41	8.58	9.08	10.86
Female	8.35	9.07	10.60	15.26
Public	8.54	9.18	10.61	15.28
Private	8.45	8.23	8.13	7.98
Voucher	8.28	8.90	10.15	14.02

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

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.



(a) BO with earlier choice

(b) BO with new choice

(c) WO, with lower choice

(d) WO, Did not apply 2012 match

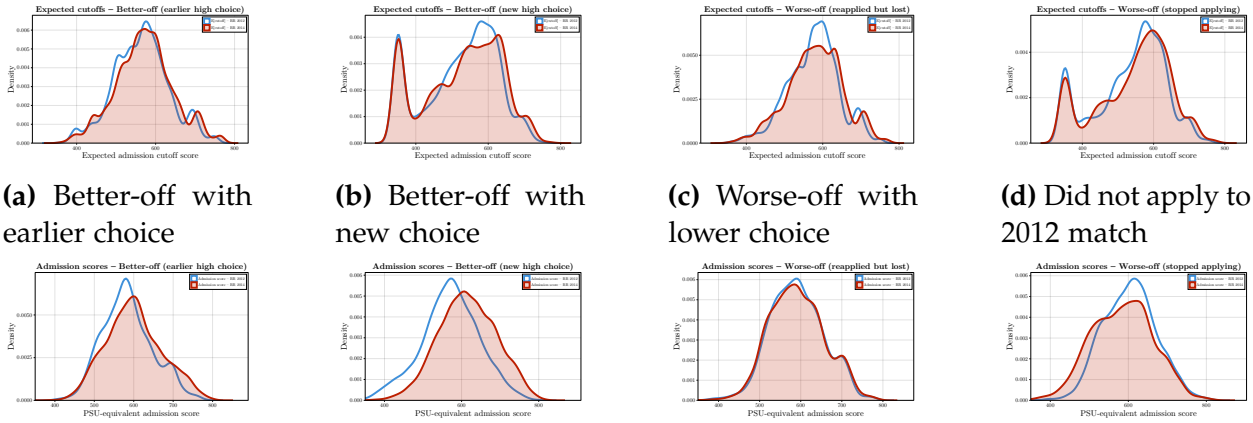
Figure 13: Scatter of Scores - $\mathbb{E}$(cutoff) for 2012 and 2014

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 (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 13 and 14 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 13 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 14 confirms this pattern by comparing the distributions of admission scores and expected cutoffs within each group. For better-off students, scores are stable but cutoffs shift in their favor, making previously out-of-reach programs attainable or new applications worthwhile. For worse-off students, scores do not decline, but cutoffs rise relative to them, so they are pushed out of previous seats or choose not to reapply. Overall, the evidence shows that policy effects are not explained by scores alone. They come from the combination of cutoff movements and changes in application behavior that reallocate students across programs.



(a) Better-off with earlier choice

(b) Better-off with new choice

(c) Worse-off with lower choice

(d) Did not apply to 2012 match

(e) Better-off with earlier choice

(f) Better-off with new choice

(g) Worse-off lower choice

(h) Did not apply to 2012 match

Figure 14: Densities of Scores - $\mathbb{E}$(cutoff) for 2012 and 2014

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.

Table A.1: Model Parameters

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

A.1.2 Preliminaries

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

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

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

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

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

$$\begin{aligned}
\mu^u_{ij} &= \delta^u_j + \theta_i' x_j + \beta_w' w_{ij} + \beta_z' z_i + \beta_d d_{ij} \\
\mu^c_{ij} &= \delta^c_j + \gamma_w' w_{ij} + \gamma_z' z_i + \gamma_s \tilde{s}_{ij} \\
\mu^h_{ij} &= \delta^h_j + \phi_w' w_{ij} + \phi_z' z_i
\end{aligned}$$

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

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

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

A.1.3 Parameter Priors

I employ conjugate priors to ensure tractable posterior distributions:

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

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

A.1.4 Conditional distributions of latent variables

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

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

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

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

So, up to a normalising constant,

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

The prior for u is

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

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 $\text{IG}(\alpha,\beta)$:

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

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

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

A.1.6 Gibbs Sampler Algorithm

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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.[10] 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

[10] 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.[11] 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.

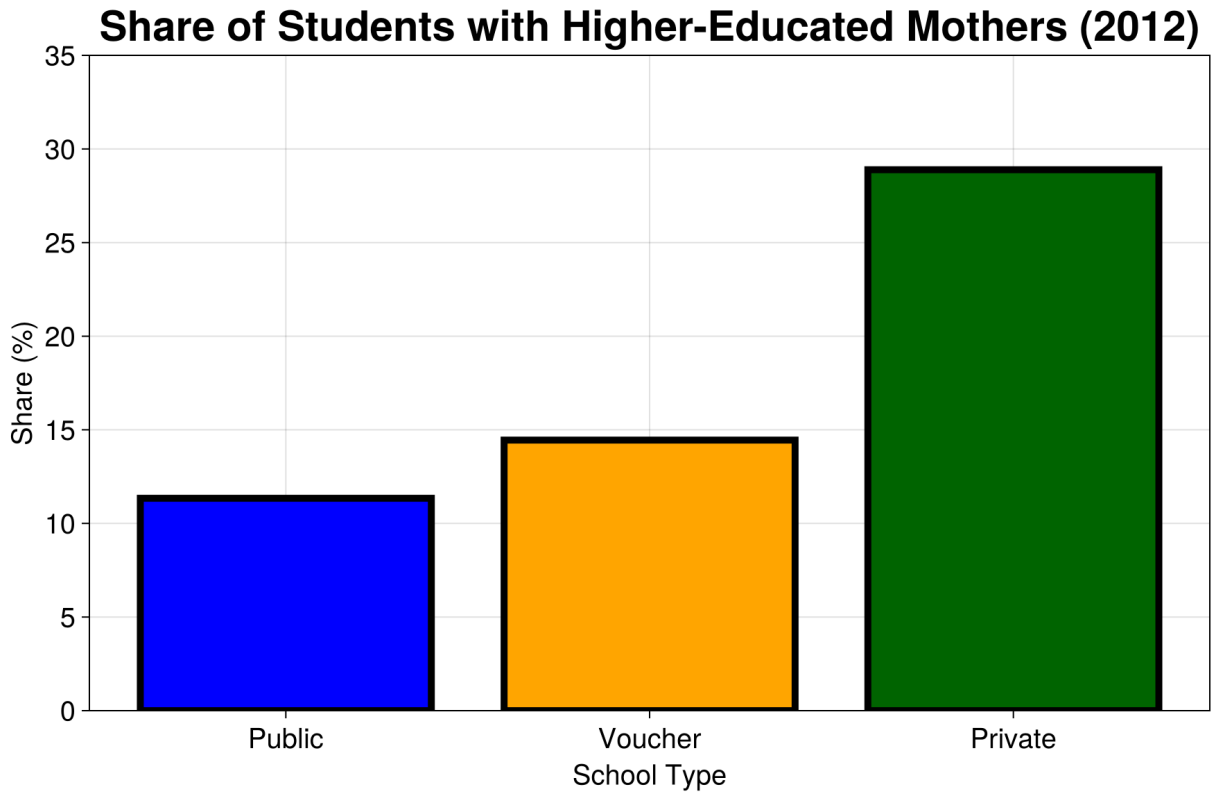


Figure B.1: Share of students with higher-educated mothers by school type.

[11] 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

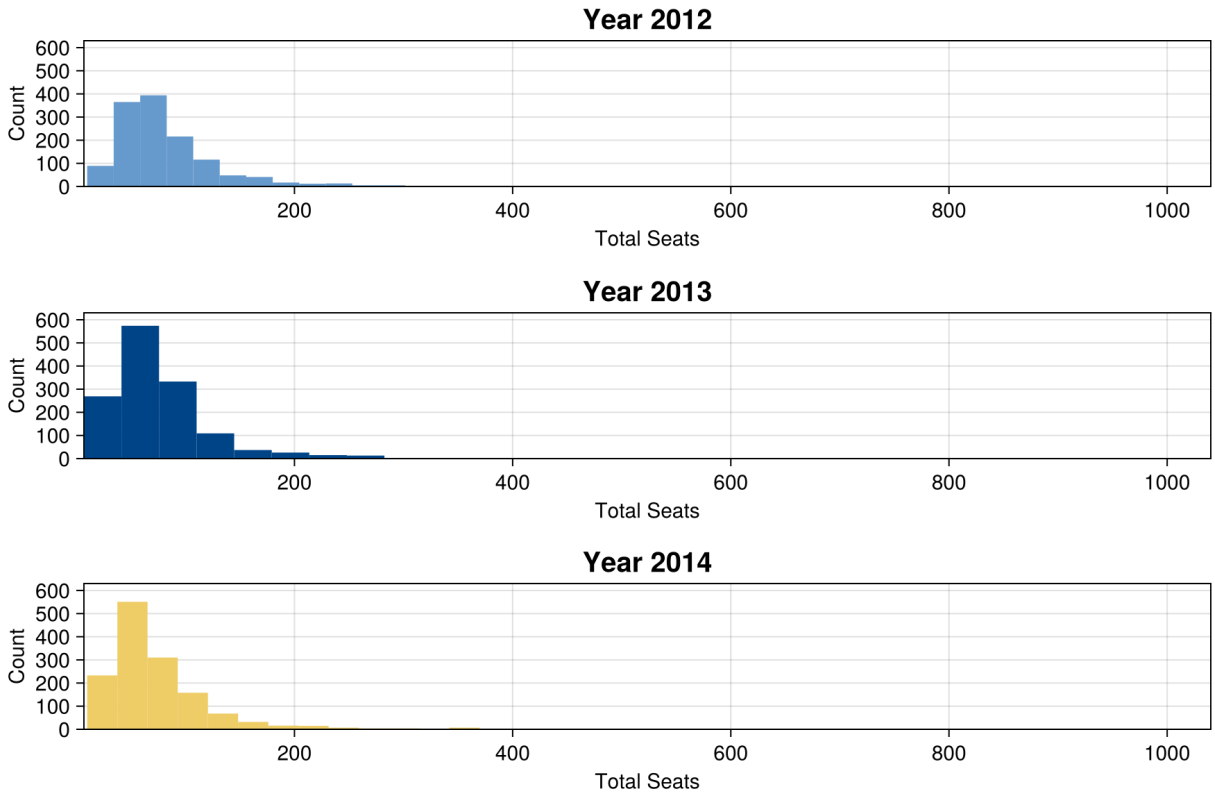


Figure B.2: Distribution of total seats across years

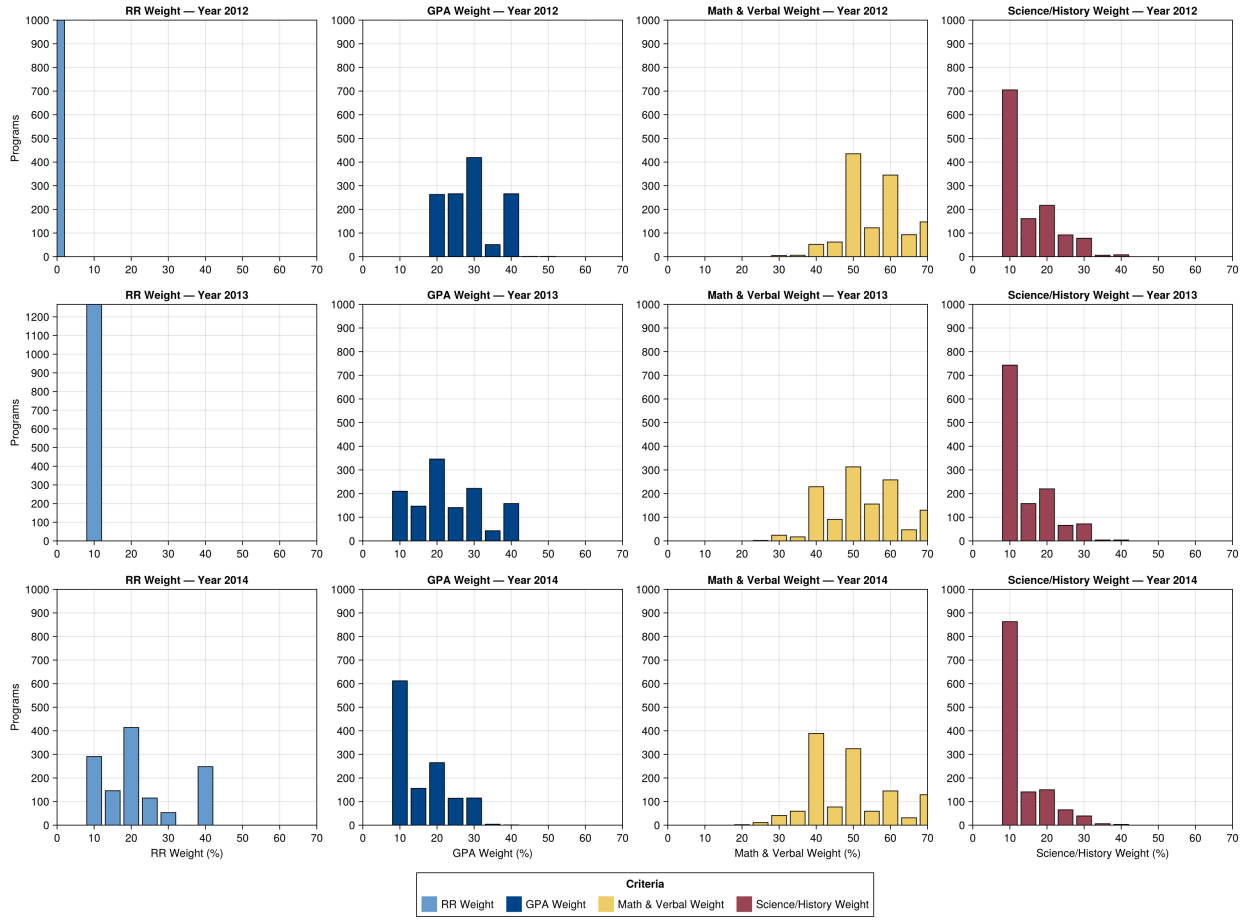


Figure B.3: Distribution of admission weights across criteria, 2012–2014. Rows show years (2012, 2013, 2014) and columns show criteria (Relative Ranking, GPA, Math & Verbal PSU, and Science/History PSU). Each bar indicates the number of programs assigning a given percentage weight to that component.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

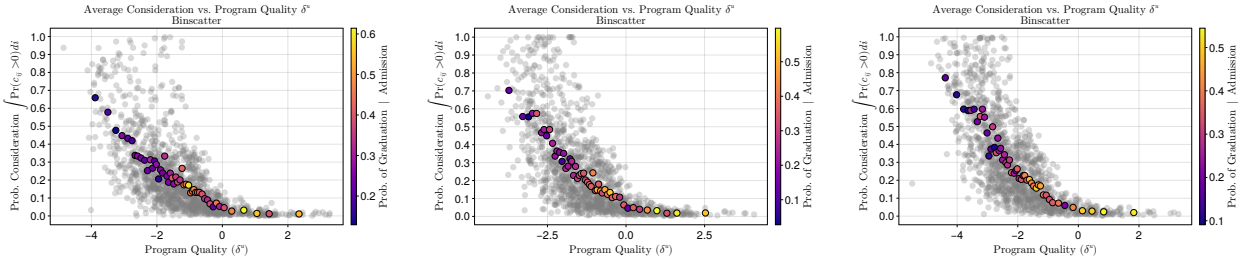
	(1)	(2)	(3)	(4)	(5)	(6)
	2012		2013		2014	
Variable	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Inside Good (β)						
STEM × Math	0.242	(0.018)	0.246	(0.023)	0.203	(0.014)
STEM × Verbal	-0.175	(0.009)	-0.134	(0.012)	-0.120	(0.014)
STEM × Sex	0.926	(0.142)	0.886	(0.125)	0.791	(0.114)
STEM × Recent Grad	-0.122	(0.045)	-0.099	(0.051)	-0.106	(0.027)
STEM × Private School	-0.542	(0.015)	-0.526	(0.022)	-0.382	(0.020)
STEM × Public School	-0.167	(0.027)	-0.118	(0.033)	-0.122	(0.020)
Selectivity × Math	0.189	(0.009)	0.125	(0.019)	0.093	(0.016)
Selectivity × Verbal	0.080	(0.017)	0.088	(0.004)	0.074	(0.007)
Selectivity × Sex	-0.057	(0.010)	-0.155	(0.006)	-0.134	(0.007)
Selectivity × Recent Grad	-0.038	(0.011)	-0.088	(0.014)	-0.078	(0.015)
Selectivity × Private School	-0.097	(0.017)	-0.076	(0.017)	-0.101	(0.015)
Selectivity × Public School	-0.037	(0.011)	-0.037	(0.005)	-0.043	(0.006)
Same Region	1.104	(0.014)	1.132	(0.006)	1.118	(0.008)
Net Price	-0.067	(0.010)	-0.031	(0.005)	-0.041	(0.006)
Distance	-0.467	(0.019)	-0.420	(0.013)	-0.393	(0.021)
Random Coefficients (σ)						
Constant	0.452	(0.029)	0.454	(0.023)	0.419	(0.054)
STEM	1.236	(0.076)	1.165	(0.055)	1.059	(0.109)
Avg. Math & Lang. Historic	0.402	(0.028)	0.380	(0.008)	0.308	(0.024)
Outside Good (ψ)						
Constant	2.670	(0.143)	3.463	(0.220)	1.543	(0.147)
Sex	1.098	(0.098)	0.670	(0.062)	0.533	(0.054)
Private	0.195	(0.029)	0.013	(0.069)	0.516	(0.086)
Public	-0.084	(0.029)	-0.224	(0.083)	-0.208	(0.083)
Avg. Mate-Verb.	1.303	(0.029)	1.506	(0.025)	1.370	(0.074)
Avg. Mate-Verb.^{2}	0.306	(0.018)	0.401	(0.041)	0.271	(0.026)
Avg. Mate-Verb.^{3}	-0.042	(0.006)	0.021	(0.006)	0.007	(0.004)
Scholarship	-0.361	(0.111)	-0.691	(0.128)	-0.509	(0.123)
Scholarship^2	0.710	(0.210)	1.528	(0.354)	1.003	(0.367)
Scholarship^3	-0.187	(0.057)	-0.448	(0.112)	-0.275	(0.120)
Recent Grad.	-0.104	(0.031)	-0.175	(0.087)	-0.078	(0.082)
Avg. Mate-Verb× Recent Grad	-0.113	(0.023)	-0.004	(0.033)	0.020	(0.031)
Avg. Mate-Verb^2 × Recent Grad	0.033	(0.015)	0.028	(0.029)	0.030	(0.038)
Avg. Mate-Verb^3 × Recent Grad	-0.011	(0.008)	-0.077	(0.019)	-0.058	(0.017)

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	0.415	(0.040)	0.104	(0.042)	0.090	(0.052)
Private	0.143	(0.017)	-0.010	(0.020)	0.316	(0.014)
Public	-0.014	(0.007)	-0.037	(0.022)	-0.096	(0.030)
Avg. Mate-Verb.	-0.557	(0.011)	-0.186	(0.093)	-0.140	(0.057)
Avg. Mate-Verb.^{2}	-0.131	(0.009)	-0.094	(0.008)	-0.103	(0.009)
Avg. Mate-Verb.^{3}	-0.017	(0.002)	-0.034	(0.004)	-0.034	(0.003)
Scholarship	0.106	(0.027)	0.093	(0.091)	0.161	(0.116)
Scholarship^2	-0.392	(0.060)	-0.015	(0.126)	-0.163	(0.191)
Scholarship^3	0.110	(0.017)	0.018	(0.040)	0.074	(0.062)
Recent Grad.	-0.078	(0.006)	-0.139	(0.020)	-0.128	(0.029)
Avg. Mate-Verb$\times$ Recent Grad	-0.079	(0.008)	-0.148	(0.017)	-0.170	(0.016)
Avg. Mate-Verb^2 $\times$ Recent Grad	-0.039	(0.005)	-0.095	(0.017)	-0.110	(0.025)
Avg. Mate-Verb^3 $\times$ Recent Grad	0.019	(0.003)	0.044	(0.012)	0.052	(0.013)
STEM $\times$ Math	0.275	(0.015)	0.267	(0.021)	0.293	(0.025)
STEM $\times$ Verbal	0.015	(0.005)	-0.067	(0.014)	-0.070	(0.012)
STEM $\times$ Sex	-0.093	(0.053)	-0.083	(0.015)	-0.006	(0.022)
STEM $\times$ Recent Grad	0.150	(0.018)	0.143	(0.011)	0.170	(0.010)
STEM $\times$ Private School	0.141	(0.018)	0.072	(0.029)	0.031	(0.029)
STEM $\times$ Public School	0.075	(0.011)	0.026	(0.010)	0.036	(0.012)
Selectivity $\times$ Math	0.088	(0.010)	0.088	(0.003)	0.092	(0.009)
Selectivity $\times$ Verbal	0.193	(0.009)	0.177	(0.005)	0.155	(0.009)
Selectivity $\times$ Sex	-0.117	(0.015)	0.050	(0.024)	0.038	(0.026)
Selectivity $\times$ Recent Grad	0.070	(0.006)	0.171	(0.024)	0.175	(0.031)
Selectivity $\times$ Private School	0.004	(0.010)	0.084	(0.013)	-0.010	(0.020)
Selectivity $\times$ Public School	0.017	(0.006)	0.040	(0.018)	0.078	(0.019)
Same Region	0.396	(0.015)	0.357	(0.028)	0.323	(0.038)
Net Price	-0.049	(0.005)	0.469	(0.118)	0.558	(0.141)
Weighted Score	1.116	(0.037)	1.570	(0.286)	1.636	(0.307)

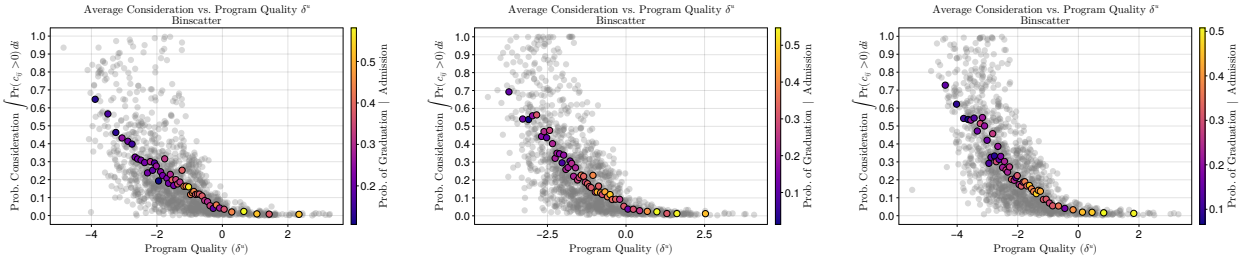
Table C.3: Human capital production function estimates: parameters

	(1)	(2)
	Panel Estimation	
Variable	Coeff.	Std. Err.
Phi Parameters (ϕ)		
ρ	0.093	(0.010)
Sex	-0.274	(0.013)
Private	-0.067	(0.017)
Public	-0.025	(0.013)
Avg. Mate-Verb.	0.235	(0.014)
Avg. Mate-Verb.^{2}	-0.019	(0.008)
Avg. Mate-Verb.^{3}	-0.006	(0.001)
Scholarship	0.512	(0.032)
Scholarship^2	-0.923	(0.050)
Scholarship^3	0.254	(0.014)
Recent Grad.	0.107	(0.013)
Avg. Mate-Verb$\times$ Recent Grad	0.011	(0.019)
Avg. Mate-Verb^2 $\times$ Recent Grad	-0.022	(0.011)
Avg. Mate-Verb^3 $\times$ Recent Grad	0.010	(0.005)
STEM $\times$ Math	0.273	(0.016)
STEM $\times$ Verbal	-0.137	(0.014)
STEM $\times$ Sex	-0.064	(0.023)
STEM $\times$ Recent Grad	-0.108	(0.021)
STEM $\times$ Private School	-0.078	(0.032)
STEM $\times$ Public School	-0.059	(0.025)
Selectivity $\times$ Math	0.010	(0.008)
Selectivity $\times$ Verbal	-0.037	(0.007)
Selectivity $\times$ Sex	0.031	(0.009)
Selectivity $\times$ Recent Grad	0.013	(0.013)
Selectivity $\times$ Private School	0.001	(0.012)
Selectivity $\times$ Public School	-0.016	(0.012)
Same Region	0.125	(0.012)
Net Price	-0.044	(0.017)

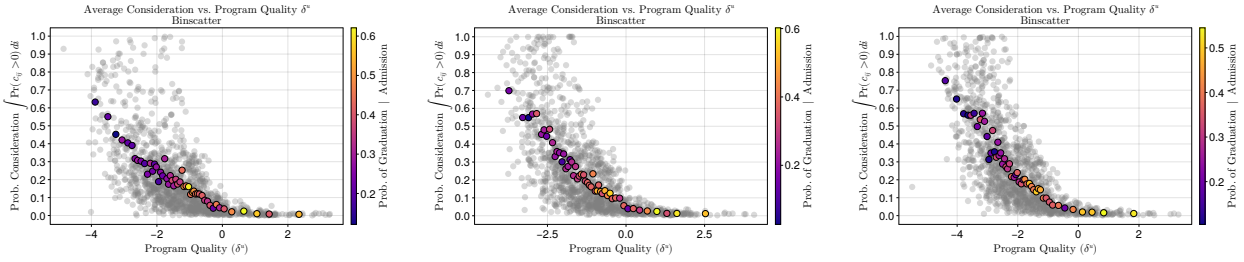


(a) All programs, 2012 **(b)** All programs, 2013 **(c)** All programs, 2014

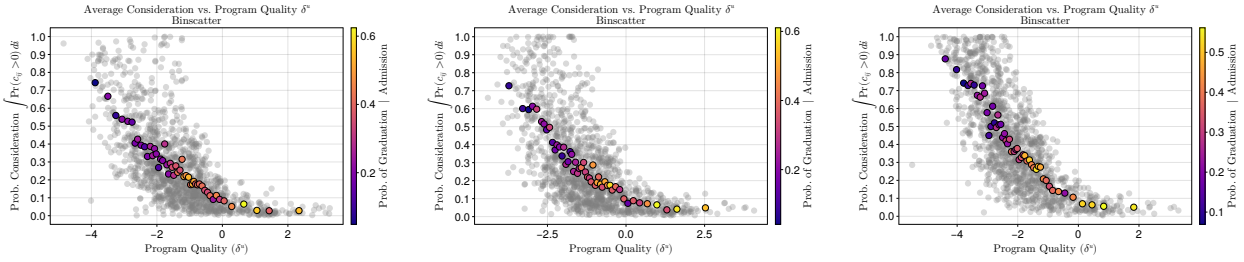
Figure C.1: Average program quality δ^u and average consideration, all programs, 2012–2014.



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

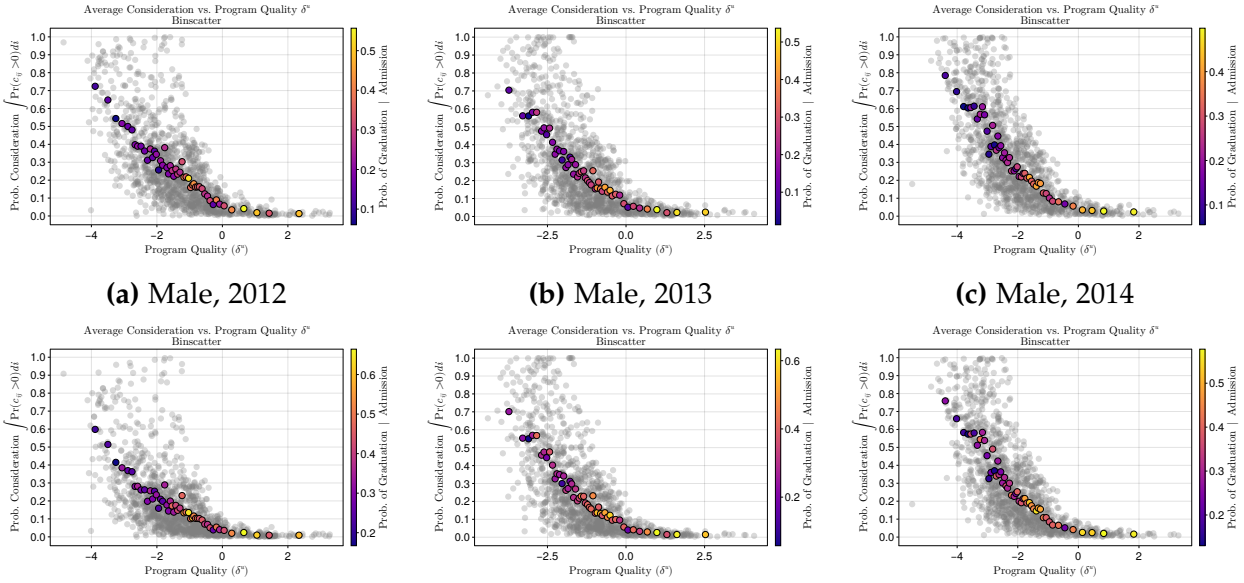


(d) Voucher, 2012 **(e)** Voucher, 2013 **(f)** Voucher, 2014



(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

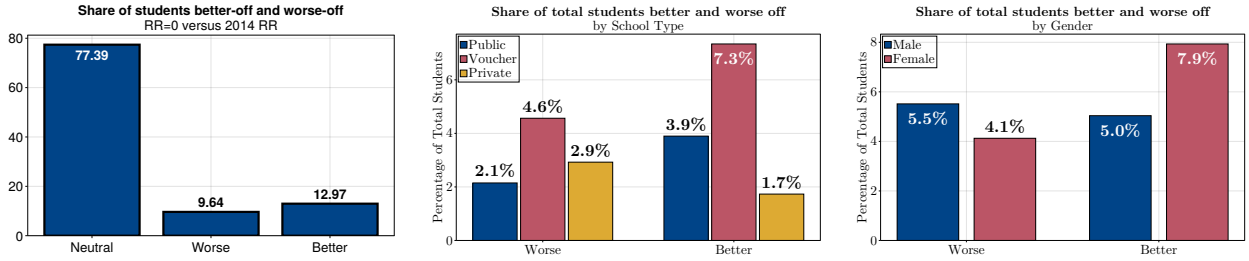
D.2 Higher education programs data construction

E Additional Counterfactual Results

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 E.1 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—80%—of students remain neutral, while 10.3% are better-off and 8% are worse off. Among the students that are better-off, 90% of them come from either public or voucher schools. Among the students that are worse-off 25% of them are from public schools, 50% come from voucher schools, and 25% from private schools.

In Table E.1 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, 86% received some boost and the average boost received was around 58 points (0.53 standard deviations of the PSU scale). The average utility change was of 6 thousand dollars. In the worse-off group, 40% of them were women, and while 47% of them received a boost due to the policy, the average boost received was of 11.7 points (0.1 standard deviations of the PSU score). The average utility change in the worse-off group was -5.8 thousand dollars.



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

Figure E.1: Winners and losers from the implementation of the 2014 RR policy

Table E.1: Descriptive statistics by neutral, worse-off, and better-off students

Variable	Overall	Neutral	Worse-off	Better-off
Number of students	105,155	81,378	10,137	13,640
Female share	0.514	0.509	0.428	0.612
Public school share	0.258	0.256	0.223	0.301
Voucher school share	0.539	0.542	0.474	0.566
Private school share	0.203	0.202	0.303	0.134
Share that received boost	0.640	0.621	0.475	0.882
Average boost received	27.045	23.820	11.500	57.837
Average utility change	0.047	0.000	-1.445	1.433

E.1 Counterfactual range tradeoffs table

E.2 Counterfactual descriptives

Table E.2: 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

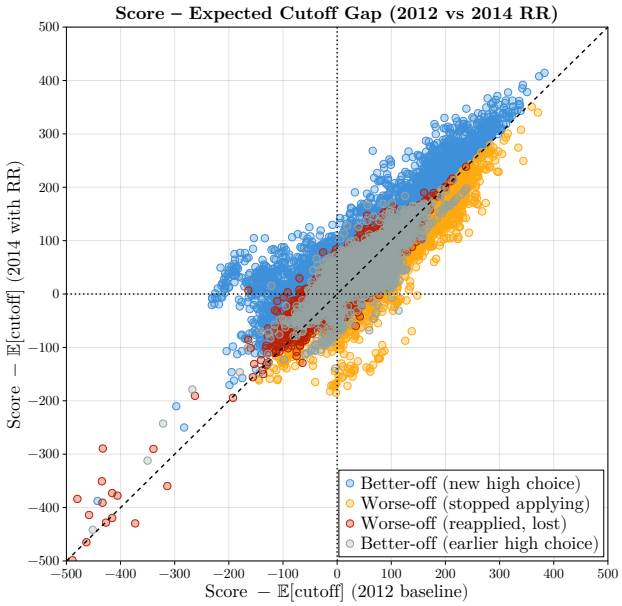


Figure E.2: Scatter of Scores - 𝔼(cutoff) for 2012 and 2014

Table E.3: 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