Group-Neutral College Access and Student Welfare: Evidence from Chile

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PRELIMINARY AND INCOMPLETE

Abstract

Group-neutral admission policies aim to promote equity without the political costs of affirmative action, yet their success depends on how disadvantaged students respond to them. I study the welfare effects of Chile's Relative Ranking (RR) rule, which raises college admission scores for the top students at every high school. Using administrative data on the universe of applicants, I estimate a structural model of college choice where the policy alters students' beliefs about admission, thereby changing the set of schools they consider. I find the policy increased average student welfare by 1.5%, concentrating gains among students from public and voucher schools while reducing welfare for students from private schools. Accounting for students' behavioral response is important, as it amplifies the policy's effect by a factor of five relative to the mechanical effect alone. 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 sharply stratified by socioeconomic background (Chetty et al., 2020). Policymakers have attempted to address this inequality through access policies such as affirmative action, but these have proven highly controversial (Students for Fair Admissions, Inc. v. President and Fellows of Harvard College, 2023). Group-neutral alternatives, such as class-rank plans, have gained attention in response (Bleemer, 2023, Mukherjee, 2025). These policies boost admission chances for each high school's top performers, reallocating seats and generating winners and losers—potentially trading off equity and efficiency. Their impact on disadvantaged students hinges on two mechanisms: (i) the direct change in their admission chances and (mechanical effect) (ii) the adjustment in their application behavior due to the policy (behavioral effect). Quantifying both channels is important to evaluate whether class-rank rules are a good alternative to affirmative action.

This paper studies the effects of the Relative Ranking policy (RR policy) implemented in Chile in 2013. The RR policy is a type of class-rank policy, and worked by increasing the college admission probabilities of top-of-their-class high school students by boosting their admission scores. By implementing the RR policy, the policy-maker intended to *level the playing field* for students at the top of their class that came from low value added public schools with those that came from high value added private paid schools. I study the effects of this policy, identify who are the winners and losers, and ask whether the policy actually benefited the group of students the policy maker intended to help. I estimate a structural model and simulate counterfactual allocations to compare the utility each student would receive under admission systems with and without the RR policy, allowing me to measure the policy's welfare effects. Moreover, I study alternative designs of the policy and explore the severity of an equity-efficiency trade-off when expanding the RR component.

I find that the policy increased students' welfare, measured as the expected utility from the programs to which they are admitted. On average, students experienced a 1.5 percentage point gain in utility relative to the pre-policy baseline. The policy benefited students from public and voucher schools while displacing private-school students from their preferred options. Although the average gains and losses were similar in magnitude, the net welfare effect is positive because public and voucher students account for over two-thirds of admitted students. Furthermore, I show that an alternative design that expands the policy homogeneously to all programs would increase average welfare by

9 percentage points, with only a modest decline in the average test scores of admitted students.

Chile's centralized college admission system (CCAS) offers an ideal setting to study class-rank admission policies due to its transparency, clear policy implementation, and rich administrative data. First, the CCAS operates transparently: universities publish seat numbers and their admissions criteria, students take tests and submit a list of program preferences, and a deferred-acceptance algorithm (DAA) determines final allocations based on these preferences, scores, and programs admissions criteria. Second, the Relative Ranking policy was introduced in a straightforward way, providing a score boost proportional to how much a student's GPA exceeded their high school's historical GPA average, with no boost for those below average. Finally, extensive administrative data from the Chilean Ministry of Education (MINEDUC) and DEMRE (*Departamento de Evaluación, Medición y Registro Educacional*) is available, including student records for around 100,000 yearly applicants detailing ranked applications, test scores, enrollment decisions, and academic history, all linked by a masked identifier. Program records list over 1,350 programs, including seat counts, admission rules, tuition, and accreditation. Together, these datasets enable the replication of admission offers for every applicant before and after the reform.

Using the CCAS data, I can observe the decisions on both sides of the market before and after the Relative Ranking reform. In 2013 the RR weight was fixed at 10% for every program, while in 2014 each program could choose any value between 10% and 40%, generating rich variation in the intensity of the policy across programs. The RR weight, along with the weights of the other standardized tests, defines each program's admission criteria by determining the weighted score used to rank students.[1] The DAA final allocation is characterized by a vector of cutoff scores where applicants with weighted scores above the programs cutoff are admitted. As the DAA runs after the all applications are submitted, applicants do not know the cutoff score at each program in the application stage, and admissions remain uncertain until the DAA is run. Earlier studies show that when a program looks unattainable, many students leave it off their list. By raising admission probabilities of top students, the RR reform can thus alter both the priority of applicants (mechanical effect) as well as the rank-order lists they submit (behavioral effect).

[1]The same student in an admission process has different weighted scores when applying to different programs. Also, two students with the same (unweighted) test scores applying to the same program face different entry chances under different policy scenarios.

Estimating the policy effect requires observing the same student applying in both pre- and post-policy scenarios. The challenge arises due to a classical problem in 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, I develop a structural model of higher education applications. In the model, students apply to programs they prefer over their outside option and where they believe they have a positive chance of admission. This "skipping the impossible" behavior is supported by evidence that students avoid programs perceived as out of reach (Fack et al., 2019, Larroucau and Rios, 2020, Fabre et al., 2024). I capture this behavior with student-specific consideration sets which are constructed around students subjective beliefs about cutoffs. The RR policy will change weighted scores and admission probabilities, which in turn affects consideration sets and application behavior. Students' beliefs are assumed to be anchored in rational expectations about cutoffs, which adjust in equilibrium to reflect the aggregate response to the policy.

The identification of the model relies on administrative data of students' rank-ordered lists of programs and two exclusion restrictions to separate preferences from consideration frictions. I follow the work on identification of demand systems with latent choice frictions of Agarwal and Somaini (2022), and use distance and weighted scores as shifters for preferences and consideration. While distance negatively affects a student's utility from attending a program, I argue 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 argue 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

[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.

weighted score comes from the way different programs weight different tests, GPA, and RR, which is exogenous to the student.

The model is estimated in two stages. In the first stage I estimate the rationally expected cutoff scores following Agarwal and Somaini (2018) 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 preferences and consideration 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 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. The stability also implies that the reduced-form deviation in the consideration equation remains invariant to the policy design, with effects operating through updates in expected cutoffs. 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. Third, the model reveals a negative correlation between program quality and the probability of being considered, especially for students from public and voucher schools. 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 with and without the RR policy. I find that the policy leads to a net positive impact on student welfare, resulting in a 1.5% increase relative to the baseline benchmark in the absence of the policy. I find that incorporating the belief update in the welfare impact estimations is important, and not accounting for it would result in downward biased estimations of the effect of the policy of 80% ($\Delta^{+}0.31$ p.p. of the baseline welfare instead of $\Delta^{+}1.5$ p.p.). The policy benefited primarily students from public and voucher schools, and crowded out from their preferred options students from private schools.

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

[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).

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 (RR) 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.

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 (CCAS), combined with its Relative Ranking (RR) policy, offers an ideal context to evaluate the welfare effects of a group-neutral, access-oriented reform. This is due to two key factors. First, the CCAS provides rich administrative data on both student preferences and program requirements, along with a transparent allocation mechanism. Second, as detailed in this section, the institutional framework creates a well-defined choice environment that allows me to disentangle

[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).

the forces driving the policy's effects across different student groups, including those intended to be targeted by the RR policy.

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 admission scores and the DAA. First, 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.*

[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.

When we are talking about rankings, we can put on an equal footing those students who are at the top of a private paid school and those at the top of a public school."

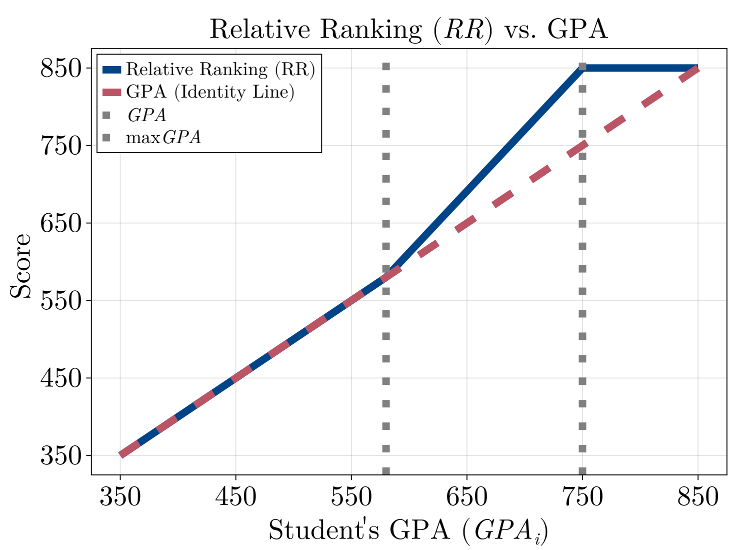
Consejo de Rectores de las Universidades Chilenas (CRUCH) (2012)

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

The admission component that the RR added to the weighted score is a boosted GPA score. 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 Descriptive statistics

The student applicant pool shows stability in its demographic and academic profile between 2013 and 2014, with a consistent majority of students coming from voucher schools. Panel A of Table 1 presents summary statistics for 2013 and 2014. The average math and verbal score was 582.78 in 2013 and 581.81 in 2014. The proportion of recent high school graduates was 0.61 in 2013 and 0.62 in 2014. Student distribution by school type was: public school (0.26), voucher school (0.54), and private school (0.20) for both years. The number of observations was $106,762$ in 2013 and $105,781$ in 2014.

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 1 provides program-level summary statistics for $N = 1,355$ programs across these two periods.[7] The average weight on RR increased from 10.00 to 22.01. Conversely, the average weight on GPA decreased from 23.23 to 15.99. The weight on PSU tests also decreased from 73.61 to 68.42. The average number of total seats offered per college decreased from 81.64 to 79.07. Other characteristics remained stable. The proportion of colleges offering STEM programs remained constant at 0.34 in both periods. Sticker tuition (in thousands of US dollars) increased from an average of 2673.75 to 2783.53. The average historic PSU score of admitted students increased from 585.89 to 586.67.

Panel C of Table 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

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

Table 1: Descriptive Statistics

	(1)	(2)	(3)	(4)
	2013		2014	
Panel A: Students	Mean	Std. Dev.	Mean	Std. Dev.
Avg. Math and Verbal Score	582.78	76.16	581.81	76.75
=1 Recent HS Grad	0.61	0.49	0.62	0.49
Sex	0.49	0.50	0.49	0.50
Public School	0.26	0.44	0.26	0.44
Voucher School	0.54	0.50	0.54	0.50
Private School	0.20	0.40	0.20	0.40
Number of observations	106,762		105,781	
	2013		2014	
Panel B: Colleges	Mean	Std. Dev.	Mean	Std. Dev.
Weight on RR	10.00	0.00	22.01	10.40
Weight on GPA	23.23	9.45	15.99	6.84
Weight on PSU tests	73.61	12.27	68.42	13.83
Total seats offered	81.64	65.85	79.07	65.93
=1 if STEM	0.34	0.48	0.34	0.48
Sticker Tuition (Th. USD)	2673.75	836.85	2783.53	864.83
Avg. Historic PSU Score	585.89	55.74	586.67	56.25
Number of observations	1,355		1,355	
	2013		2014	
Panel C: Student-College	Mean	Std. Dev.	Mean	Std. Dev.
Distance	5.01	5.17	5.03	5.19
Weighted Score	571.43	82.36	577.64	83.02
STEM $\times$ Math	201.25	281.89	201.03	281.73
(i, j) in same region	0.18	0.39	0.18	0.39
Net Price	2513.78	919.77	2615.95	948.72
Number of observations	144,662,510		143,333,255	

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 score and program STEM status. This interaction has a mean around 201.25 and a standard deviation of 281.89. The share of student-program pairs in the same geographical region is also included. Each region corresponds to one of Chile's 15 administrative regions.[8] Same-region pairs share 0.18 with high variation (standard deviation of 0.39). Net price faced by student i at program j is a match-specific variable, averaging between 2513.78 and 2615.95 with a standard deviation around 919.77 to 948.72. This price variable is match specific due to student-program specific discounts (e.g., teaching-specific discounts, BVP, discussed in Kapor et al. (2022)). These statistics use over 143 million potential student-college pairs each year.

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.

4.1 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

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

exogenous characteristics and admissions criteria. In this section I describe how student application behavior is modeled. I describe student preferences for programs, student consideration sets, and how these two objects determine the student application list.

Students preferences

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

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

where δ_j^u for $j \in \mathcal{J}$ are a program-level mean utility terms. $\theta_i^x \sim \text{MVN}(\beta^z z_i, \Sigma)$ are student-specific random coefficients that capture heterogeneity in tastes over programs characteristics. I assume that distance between the student and the program, d_{ij} enters linearly. Finally, $\varepsilon_{ij} \sim \mathcal{N}(0, \sigma_\varepsilon^2)$ is an idiosyncratic utility shock.[9]

Utility for the outside good is given by

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

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

Applications

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

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

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 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 $c(z_i, w_{ij})$.[10]

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

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

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.

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. The 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.[11] To make progress, I further assume that in counterfactuals, the deviation from the rationally expected cutoff is invariant to the RR policy. I provide evidence later that this assumption is reasonable. The model as is, is identified within

[11] 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.

a market t. This means we can test whether the estimated coefficients for assumed primitives are stable across different implementations of the RR policy. I show evidence in this line.

5.2 Estimation

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

Rational Expectations estimation

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

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

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

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

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

Preferences and consideration equations

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

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

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

I assume that the error terms $\varepsilon_{ij}, \varepsilon_{i0}, \nu_{ij}$ are all independent. I further assume that they are distributed as follows: $\varepsilon_{ij} \sim \mathcal{N}(0, \sigma_\varepsilon^2)$, $\varepsilon_{i0} \sim \mathcal{N}(0, \sigma_0^2)$, and $\nu_{ij} \sim \mathcal{N}(0, \sigma_\nu^2)$. 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 $(\delta^u, \beta, \Sigma, \delta^c, \gamma)$ 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.

6 Main Results

This section presents model estimates for 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. We 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

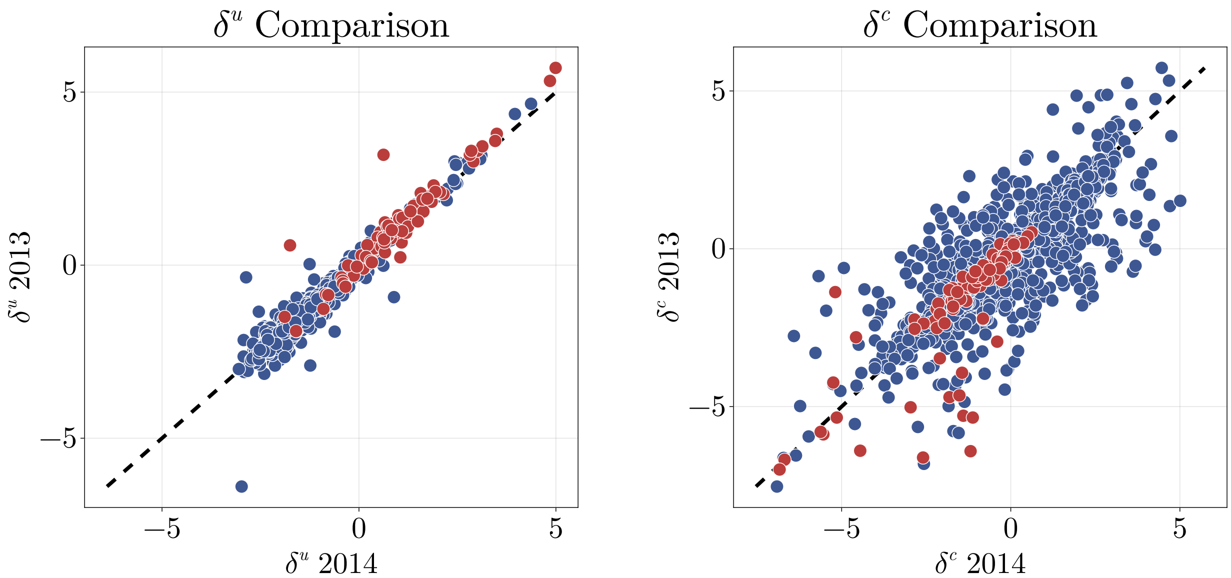
Table 2 reports selected estimates for the preference and consideration equations. Columns (1) and (3) show point estimates for 2013 and 2014, respectively, with standard errors in columns (2) and (4). All parameter signs align with theory: distance lowers inside-good utility and weighted score raises the probability of consideration. The size of the parameters are significant in magnitude relative to the scale normalization. Distance parameter is around 0.35 standard deviations of the unobserved preference shock, while the weighted score parameter is 2.5 standard deviations of the unobserved consideration shock. The estimated parameters remain stable across both years, despite significant changes in the 2013 to 2014 RR policy. This suggests the estimated primitives are robust to policy shifts.

Figure 1 shows estimations and comparisons for 2013 and 2014 program specific parameters in both utility and consideration equations. The mean quality parameters δ^u for 2013 and 2014 are shown in Figure 1a, while the fixed effect in the consideration equation for the same years is shown in Figure 1b. 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. In Appendix C I show evidence supporting this assumption.

The mean quality parameters estimates are sensible. Both figures, 1a and 1b, 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.

Table 2: Selected Preference and Consideration Parameter Estimates

	(1)	(2)	(3)	(4)
Inside Good (β)	2013		2014	
Variable	Coefficient	Std. Error	Coefficient	Std. Error
Net Price	-0.084	(0.008)	-0.081	(0.008)
Distance	-0.356	(0.003)	-0.341	(0.003)
Outside Good (ψ)	2013		2014	
Variable	Coefficient	Std. Error	Coefficient	Std. Error
Sex	-0.307	(0.013)	-0.281	(0.011)
Private	0.545	(0.018)	0.514	(0.016)
Recent Grad.	0.018	(0.015)	0.094	(0.014)
Consideration Eq. (γ)	2013		2014	
Variable	Coefficient	Std. Error	Coefficient	Std. Error
Weighted Score	2.342	(0.023)	2.357	(0.029)
STEM-Math	0.304	(0.006)	0.268	(0.006)
Same Region	0.387	(0.006)	0.347	(0.006)
Net Price	0.880	(0.014)	0.948	(0.018)
Sex	-0.110	(0.005)	-0.057	(0.005)
Private	0.210	(0.008)	0.216	(0.006)
Recent Grad.	0.022	(0.006)	0.071	(0.006)



(a) Scatter plot for δ^u

(b) Scatter plot for δ^c

Figure 1: Comparison of δ^u and δ^c for the years 2013 and 2014.

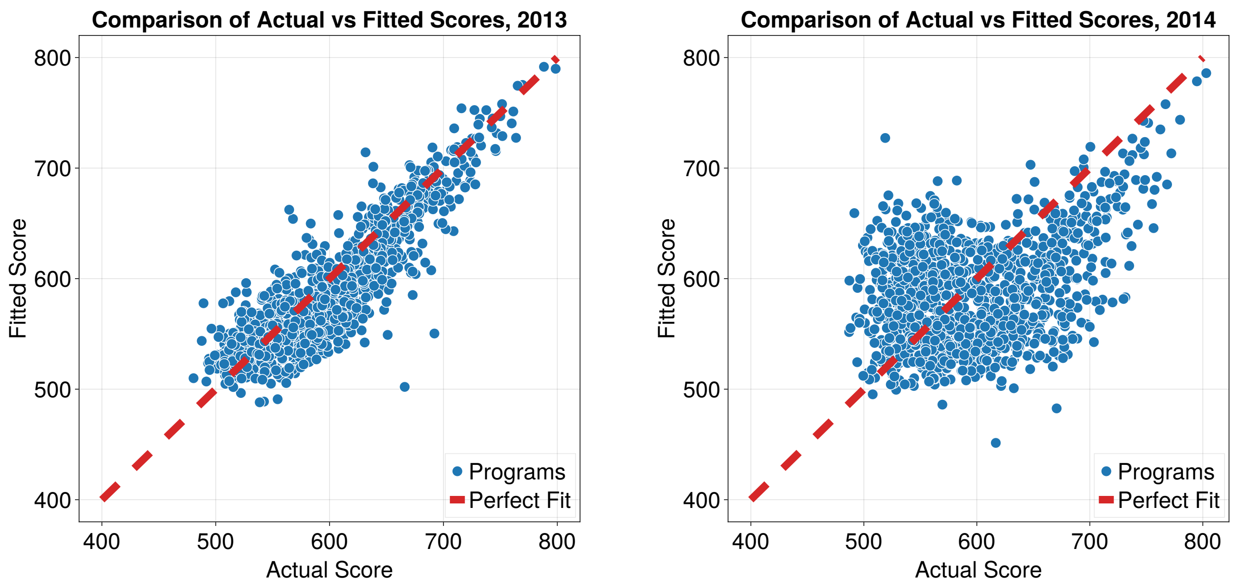
Note: TBD.

6.2 Fit of the model

To assess model fit, I initially evaluate its ability to predict the average PSU scores in mathematics and verbal exams for admitted students at the program level. To this end, for each program in each year, I first compute the average score of the admitted students in the actual data. Then, I take my estimates and simulate the admission process. I first simulate ROLs using the estimates for preferences and consideration sets. With the predicted ROLs I run the CPDAA and get admissions. Finally, I compute the average score for the admitted students in the simulation.

Figure 2 shows the model fit for year 2013 (2a) and 2014 (2b). Apart from the estimated preferences and consideration sets, there is nothing in my model that specifically targets the average score. Nevertheless, the model seems to predict in a reasonable way the program level average score. This is also the case for other statistics, such as the median score, minimum score, maximum score, but for the last two results are significantly more noisy.

Fit results are good, considering that in my model there is nothing targeting the length or composition of the ROL apart from preferences and belief-driven consideration sets. My model deals with the inclusion or exclusion of programs at the "top" of the students' list with the consideration equation, and with the inclusion or exclusion of programs at the "bottom" with the individual specific outside good utility.



(a) Model fit 2013

(b) Model fit 2014

Figure 2: Model fit

6.3 Negative correlation between quality and average consideration

Figure 3 shows the correlation between program quality, δ^u, and program's average probability of consideration. There is a negative correlation between quality and probability of consideration. Higher quality programs are demanded more by students with higher scores, which in turn causes the expected threshold of those programs to be higher, reducing the admission likelihood for lower score students. Figure 3 shows that on average, for all levels of quality except for the very high ones, the 2014 RR policy increases slightly the probability of consideration of programs.

Heterogeneity between public and private schools

On average, public and voucher school students consider less programs than private school students at any level of program quality. This is shown in Figure 4, which on panel 4a depicts the correlation for public and voucher school students, and in panel 4b for private school students. This is explained partially because private school students

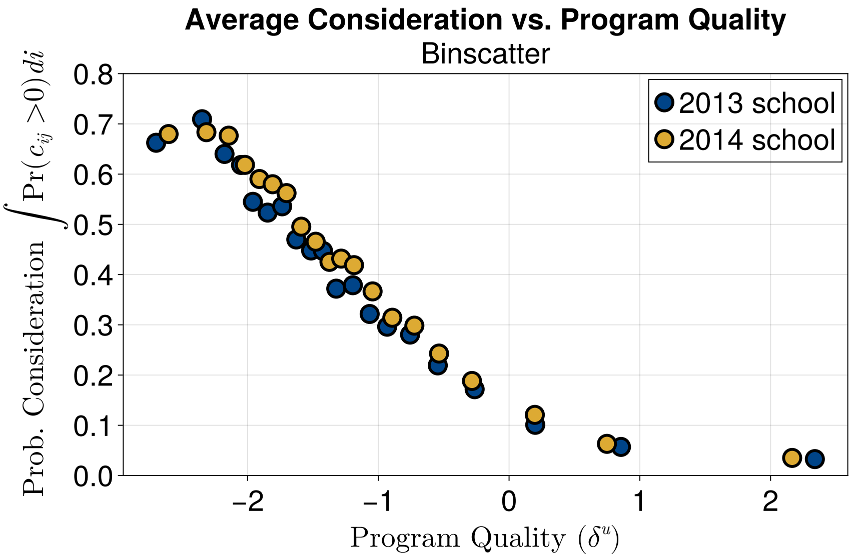
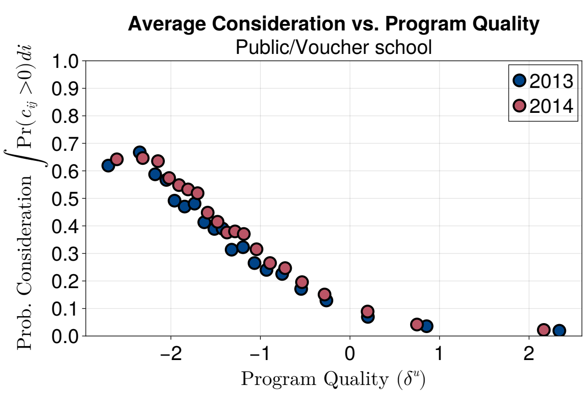
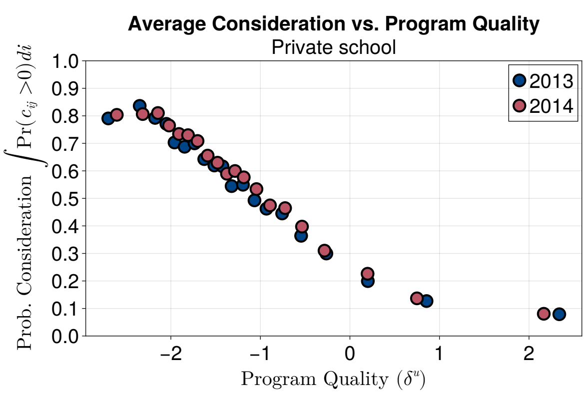


Figure 3: Average program quality δ^u and average consideration for the years 2013 and 2014.

tend to score higher in the standardized admission test, but even after controlling for that there is a significant gap as previously shown in Table 2, where the coefficient on the binary variable of private school is positive in the consideration equation. Differences captured in this reduced form consideration equation could be attributed to differences in information acquisition by school, targeted advertising from colleges to private school students, among other reasons.



(a) Public and voucher



(b) Private

Figure 4: Program quality and average consideration by school type

7 Effect of the policy and counterfactual scenarios

In this section I discuss two counterfactual scenarios: first, for the same population of students I evaluate the impact of the policy; and then I implement counterfactual policies. I compare students utility in all the different counterfactual scenarios. The policy had a positive effect on student utility. The overall effect of the policy was an increase of 1.5% in baseline student welfare. 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 interpreted in dollar terms, I normalize the net utility using the estimated price coefficient. The average student welfare is:

$$W(\mu) = \frac{1}{|\mathcal{I}|} \sum_{i \in \mathcal{I}} \frac{(u_{i\mu(i)} - u_{i0})}{\beta^{price}}. \tag{4}$$

Counterfactual computation

Counterfactual estimations use a fixed point approach for equilibrium beliefs, where two objects in the consideration equation, $c_{ij} = \gamma(s_{ij} - \mathbb{E}[cutoff_j]) + \chi_{ij} + \nu_{ij}$, 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 $\mathbb{E}[cutoff_j]^0$. 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., $\max_{j\in\mathcal{J}} \left\{ |\mathbb{E}[cutoff_j]^b - \mathbb{E}[cutoff_j]^{b-1}| \right\} < \varepsilon$.

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 5 shows the 2013 and 2014 policy implementations positively affected average student welfare. Accounting for belief updates is important; not considering them would bias the impact downward. The full effect is an additional 0.74% of the baseline welfare for the 2013 implementation and 1.5% for the 2014 implementation. For both years, the full effect is between 5$\times$ and 6$\times$ the mechanical effect.

In Figure 6 we see the effects of the policy by student high-school type. Public and voucher school students benefit from the policy. Their welfare increases by 1.3% with the 2013 rule and 2.5% with the 2014 rule, relative to the 2012 benchmark. Private school students' welfare decreases by 1% and 3.05% for the 2013 and 2014 rules, respectively. The policy primarily affects private school students through a loss of application priority. This can be seen as the full effect of the policy for them is roughly the same as the mechanical effect alone. Public and voucher school students gain priority, and this effect is amplified

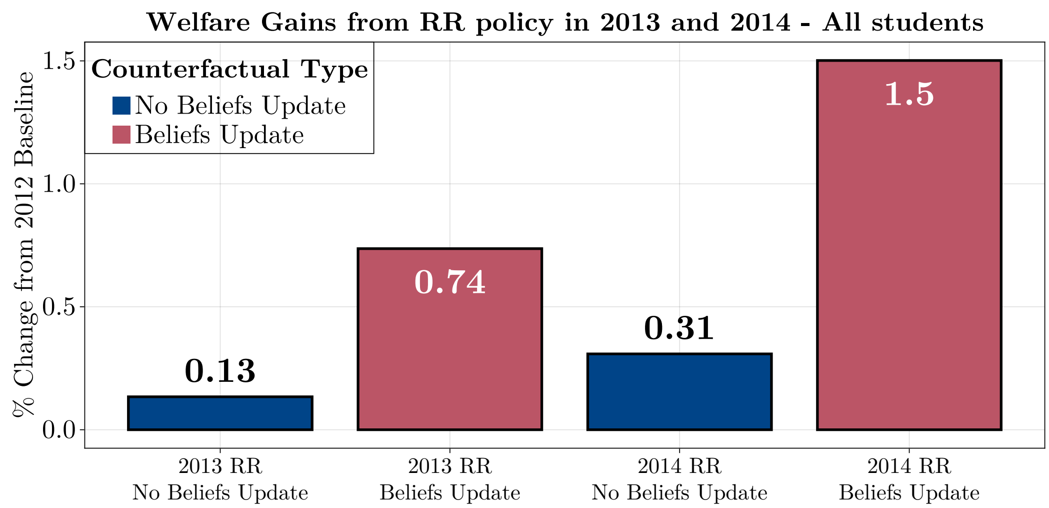
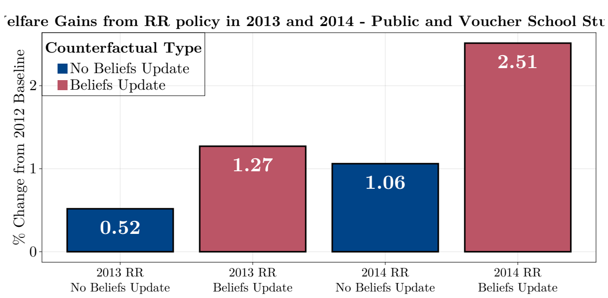
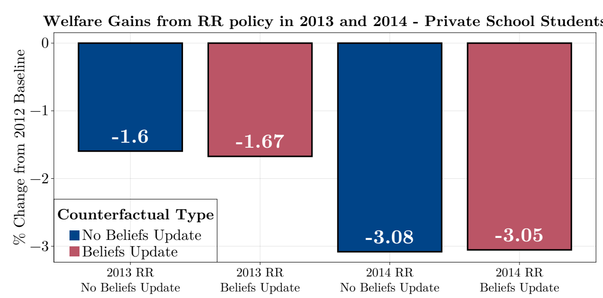


Figure 5: Welfare Effect of the RR policy

because they now include highly preferred programs, which they previously avoided due to low admission expectations.



(a) Public and voucher school students



(b) Private school students

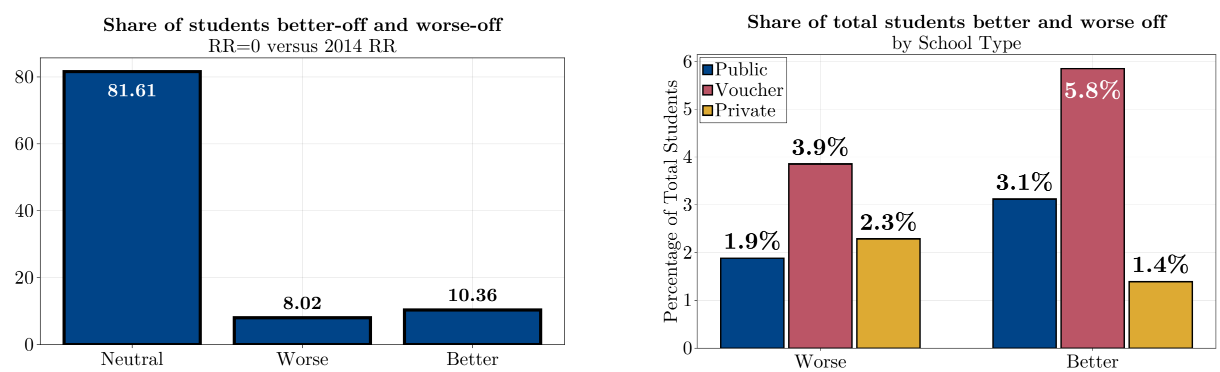
Figure 6: Welfare Effects by High-school Type

These results are in line to what Reyes (2022) finds for the 2013 implementation, where their paper finds that pulled-up students (those who are better-off with the policy) are more likely to enroll in the program they get admitted to after the policy. I am able to evaluate the effects for the whole sample of students—not just pulled-up and pushed-down students—and find not only that there is a positive impact on average, but that the role of beliefs updating is significant in mediating the effect of the policy.

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 7 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 3 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) Winners and losers share by school type

Figure 7: Winners and losers from the implementation of the 2014 RR policy

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

Table 3: Descriptive statistics by neutral, worse-off, and better-off students

Variable	Overall	Neutral	Worse-off	Better-off
Number of students	106,804	87,168	8,570	11,066
Female share	0.514	0.515	0.407	0.591
Public school share	0.257	0.254	0.235	0.301
Voucher school share	0.536	0.538	0.480	0.565
Private school share	0.201	0.201	0.285	0.134
Share that received boost	0.635	0.621	0.473	0.868
Average boost received	26.820	24.382	11.718	57.723
Average utility change	0.157	0.000	-5.869	6.064

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 8 presents the results of the counterfactual exercise involving RR expansion. Each dot in Figure 8 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 9 presents the same analysis as Figure 8, but disaggregated by school type. As previously shown in the descriptive statistics, a significant score gap exists between public and private school students. For private school students, no trade-off exists; as

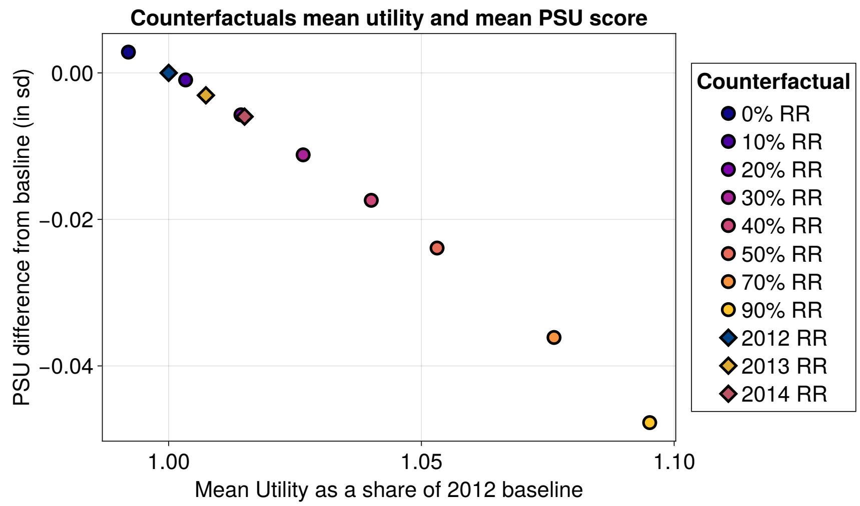


Figure 8: Counterfactual policy range for RR

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 40% across all programs significantly exceed those from the 2014 implementation, where programs could set RR criteria between 10% and 40%. The corresponding decrease in admitted students' PSU scores is limited and offset by the substantial welfare increase.

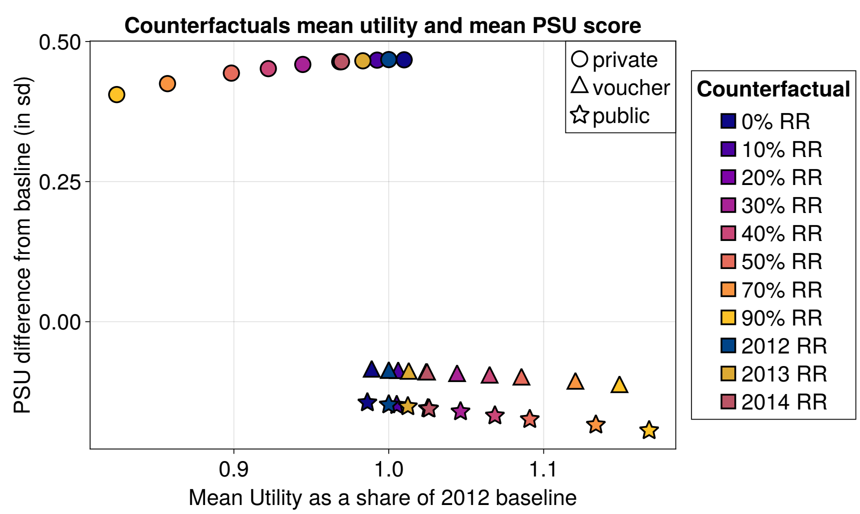


Figure 9: Counterfactual policy range for RR by school type

7.4 Drivers of the policy effect

Consideration sets

Outside good utility differences

7.5 Discussion of Key Assumptions and Limitations

Increasing significantly the RR could make students less incentivized to actually prepare for the PSU, which makes this counterfactual conclusions a "lower-bound" of the potential effect of the RR in decreasing the average admitted student pre-college ability.

Average PSU score is not an exact measure of student quality and the predictive power for graduation rates is limited [see Carril and Boehm paper Boehm and Carril (2024)]

8 Conclusion

This paper quantifies the welfare effects of Chile's Relative Ranking (RR) policy using a structural model that separates preferences from consideration frictions and embeds belief-driven application behavior under cutoff uncertainty. The model fits key out-of-sample moments, remains stable across different RR implementations, and shows that higher program quality is systematically less considered, especially by public and voucher students.

The RR policy was on average welfare enhancing and targeted the intended population. The 2014 RR rule raises average student welfare by 1.5% relative to a no-RR benchmark. Roughly 90% of the gains accrue to students from public and voucher schools, while private-school students lose 3.0 percentage points. Belief updates amplify the policy's impact fivefold; ignoring them would understate the effect to 0.3 percentage points. Counterfactual expansions reveal a limited equity–efficiency trade-off: weighting RR at 50% lifts overall welfare by 5 percentage points—and by 9 points for public and voucher students—while lowering the mean PSU score of admitted students by only 0.02 standard deviations.

These findings highlight three policy lessons. First, group-neutral rules can improve equity without large quality losses when they raise both admission priority and perceived attainability. Second, behavioral responses are central; reforms that overlook belief formation risk large mis-measurement of welfare changes. Third, moderate increases in

RR weights appear welfare-enhancing, but more aggressive expansions should be coupled with academic support to mitigate potential mismatch.

Two limitations remain. The analysis stops at enrollment; future work should trace graduation and labor-market outcomes to confirm long-run welfare gains. In addition, very high RR weights could weaken incentives to prepare for the PSU. Extending the model to allow endogenous effort and incorporating post-entry performance data would refine the equity–efficiency calculus and guide optimal policy design.

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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 4 provides a summary of the model parameters.

Table 4: 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:

$$u_{ij} = \mu^u_{ij} + \epsilon_{ij}, \tag{5}$$

$$c_{ij} = \mu^c_{ij} + \nu_{ij}, \tag{6}$$

$$h_{ij} = \mu^h_{ij} + \rho_u u_{ij} + \rho_c c_{ij} + \eta_{ij}, \tag{7}$$

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

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

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:

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

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

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

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

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

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

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

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

A.1.6 Gibbs Sampler Algorithm

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

   Analogous posteriors are used for σ_ν^2, σ_η^2, and σ_0^2.
2. **Draw Covariance Matrix** $\Sigma^{(k)}$: The posterior for the random coefficients' covariance matrix is inverse-Wishart:

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

3. **Draw Coefficient Vectors**: All coefficient vectors are drawn from multivariate normal posteriors. For example, to draw the vector of utility fixed effects $\delta^u = (\delta_1^u, \dots, \delta_J^u)'$:
   - Define the residual vector $y_u = u - (X\theta + W\beta_w + \dots)$ 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.

C Results appendix

Here I show and discuss the estimation results in full.

Table 5: Preference estimates: inside and outside goods parameters

	(1)	(2)	(3)	(4)
Inside Good (β)	2013		2014	
Variable	Coefficient	Std. Error	Coefficient	Std. Error
STEM × Math	0.329	(0.007)	0.281	(0.005)
Same Region	0.975	(0.004)	0.984	(0.004)
Net Price	-0.084	(0.008)	-0.081	(0.008)
Distance	-0.356	(0.003)	-0.341	(0.003)
Random Coefficients (σ)				
STEM	0.015	(0.000)	0.015	(0.000)
Avg. Math & Lang. Score	0.014	(0.000)	0.015	(0.000)
Outside Good (ψ)	2013		2014	
Variable	Coefficient	Std. Error	Coefficient	Std. Error
Constant	3.365	(0.081)	3.473	(0.082)
Sex	-0.307	(0.013)	-0.281	(0.011)
Private	0.545	(0.018)	0.514	(0.016)
Public	-0.051	(0.013)	0.015	(0.013)
Avg. Mate Lyc.	1.679	(0.016)	1.519	(0.014)
Avg. Mate Lyc.^{2}	0.222	(0.009)	0.201	(0.009)
Avg. Mate Lyc.^{3}	-0.002	(0.001)	-0.001	(0.001)
Scholarship	-0.279	(0.068)	-0.279	(0.063)
Scholarship^2	0.371	(0.179)	0.573	(0.184)
Scholarship^3	-0.064	(0.056)	-0.140	(0.060)
Recent Grad.	0.018	(0.015)	0.094	(0.014)
Avg. Mate × Recent	0.042	(0.018)	0.085	(0.017)
Avg. Mate^2 × Recent	-0.044	(0.013)	-0.055	(0.014)
Avg. Mate^3 × Recent	-0.013	(0.006)	-0.011	(0.006)

Table 6: Consideration estimates: parameters

	(1)	(2)	(3)	(4)
Consideration Eq. (γ)	2013		2014	
Variable	Coefficient	Std. Error	Coefficient	Std. Error
Sex	-0.110	(0.005)	-0.057	(0.005)
Private	0.210	(0.008)	0.216	(0.006)
Public	-0.020	(0.004)	-0.008	(0.005)
Avg Mate Lyc	-0.055	(0.005)	0.005	(0.005)
Avg Mate Lyc^2	-0.012	(0.003)	-0.002	(0.003)
Avg Mate Lyc^3	-0.015	(0.000)	-0.015	(0.000)
Scholarship	0.470	(0.021)	0.493	(0.022)
Scholarship^2	-0.654	(0.037)	-0.633	(0.053)
Scholarship^3	0.230	(0.012)	0.226	(0.018)
Recent Grad.	0.022	(0.006)	0.071	(0.006)
Avg Mate × Recent	0.008	(0.006)	-0.002	(0.007)
Avg Mate^2 × Recent	-0.111	(0.005)	-0.164	(0.006)
Avg Mate^3 × Recent	0.027	(0.002)	0.043	(0.003)
STEM-Math	0.304	(0.006)	0.268	(0.006)
Same Region	0.387	(0.006)	0.347	(0.006)
Net Price	0.880	(0.014)	0.948	(0.018)
Weighted Score	2.342	(0.023)	2.357	(0.029)

D Data Construction