College Discretion in Admissions and Student Achievement

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Abstract

This paper studies the aggregate and distributional consequences of allowing colleges discretion over whom to admit. Two distinctive features of Chile's centralized assignment system allow us to make progress in identifying colleges' objectives. First, the admissions rules chosen by each college-major program are publicly observed. Second, a 2014 reform expanded programs' discretion by giving them the *option* to increase emphasis on high school class rank. Programs responded differently: many maintained their admissions rules, while those that placed more emphasis on class rank admitted more low-SES and female students. To quantify the importance of discretion beyond this reform, we estimate an equilibrium model of students' preferences for programs, programs' preferences for students, and their interaction in determining graduation. Match effects matter: the same program raises graduation more for some students than for others, allowing colleges to improve aggregate graduation by admitting students based on fit. Exploiting observed rule choices, we infer that programs value graduation but differ in how they trade it off against socioeconomic composition, consistent with their differential response to the reform. In counterfactuals that limit or expand discretion, lotteries reduce segregation but also graduation, while test-only admissions and full discretion increase segregation without raising graduation. The results support delegating some admissions authority to colleges, but with guardrails to ensure alignment with a policymaker's goals.

Keywords: college admissions, centralized assignment, higher education
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1 Introduction

Access to higher education raises lifetime earnings, with returns extending down to those who narrowly gain admission (Zimmerman 2014; Mountjoy 2026). However, college enrollment is stratified by socioeconomic status, with the potential to cement inequality across generations as much as to increase upward mobility (Chetty et al. 2020; Abramitzky et al. 2024; Barrios-Fernández, Neilson, and Zimmerman 2026). In the United States, where universities have wide discretion over whom to admit, colleges themselves contribute to this social stratification through their admissions processes (Chetty, Deming, and Friedman 2026), and policy debates regarding admissions are often about which student characteristics colleges are allowed to consider when allocating scarce seats (Arcidiacono and Lovenheim 2016; Bleemer and Rothstein 2026). A large and growing number of countries address these issues through transparent, centralized admissions (Neilson 2024), but even in these systems, policymakers need to decide how much authority to delegate to colleges in setting admissions priorities.

Whether discretion aligns with a policymaker's desired balance of equity and efficiency depends crucially on what colleges value and on how colleges and students interact to determine outcomes. Aside from elite colleges in the United States (e.g. Arcidiacono, Kinsler, and Ransom 2023), there is limited direct evidence on what colleges value and even less that quantifies their preferences using policy variation. Yet even if colleges' objectives are aligned with the policymaker's, the role for discretion to improve aggregate outcomes depends on the importance of student-college match, that is, whether the same program benefits some students more than others. Without match, reassigning a seat affects who benefits but does not improve overall outcomes (Abdulkadiroğlu, Pathak, and Walters 2026). However, evidence on the existence and structure of match remains mixed (e.g. Arcidiacono, Aucejo, and Hotz 2016; Black, Denning, and Rothstein 2023), let alone whether colleges factor it into their admissions decisions. Moreover, discretion is exercised in a market where students and colleges interact to determine enrollments, making its value an equilibrium question.

In this paper, we study the aggregate and distributional impacts of allowing colleges discretion in their admissions decisions. We exploit two unique features of Chile's centralized college admissions system: publicly observable admission rules and a reform that expanded discretion in admissions. Chile's system is characterized by partial discretion, where each program (a college-major pair) independently constructs an admissions index as a weighted sum of standardized tests and grades. These admission weights are publicly observable, and knowledge of the deferred acceptance (DA) algorithm allows us to accurately replicate assignments. We leverage a 2014 reform

that gave programs discretion over how much to weigh high school class rank (relative rank) in their admissions index. Together, these features enable us to infer programs' preferences for students from their observed admissions choices, including responses to the reform. To quantify the value of discretion beyond the reform, we estimate a model of students' preferences for programs, programs' preferences for students, and their interaction in determining graduation, accounting for equilibrium spillovers through the assignment mechanism. The model enables us to simulate the impacts of counterfactual admissions systems that change the extent of allowed discretion.

Our analysis proceeds in four steps. First, we analyze programs' choices to weigh high school class rank in response to the 2014 reform. Programs differed in how they used discretion to change the composition of their admitted students. About 23 percent of programs did not adopt a higher weight on relative rank, while 33 percent placed substantial weight on it. Programs that placed a higher weight on relative rank saw declines in test scores, but increases in the share of low-SES (+5.7pp; 15 percent) and female (+2.2pp; 4.5 percent) students. Notably, these effects arise despite the fact that programs cannot directly target non-academic characteristics like SES or gender in admissions.

Simulating the mechanism under alternative weights shows that many programs could have admitted more low-SES and female students using relative rank, but *chose* not to. Under observed applications, programs that used the minimum weight could have increased their low-SES share by 3.3pp (12 percent) and their female share by 4.2pp (9 percent) had they adopted the maximum weight. Moreover, adopting a higher weight came at little cost in outcomes. A difference-in-differences analysis based on simulated exposure to the policy shows little net effect on graduation among students whose admission changed under the post-reform rules, mirroring findings in the literature (e.g. Black, Denning, and Rothstein 2023). A similar within-program analysis of admitted students shows that those pulled in by the new weight choices perform about as well as those they displaced, except perhaps at programs placing the highest weight on relative rank. Given these limited detrimental effects on graduation, we interpret the differential adoption of a higher weight on relative rank as being most consistent with heterogeneous program preferences over students' academic and non-academic characteristics.

While informative, the reduced-form analysis is limited in its ability to address the key question of policy interest: what are the impacts on students of alternate admissions systems that expand or limit discretion? Answering this requires using the observed variation to forecast graduation at alternate assignments as well as measure student and program preferences, which determine counterfactual equilibrium allocations. Therefore, in the second part of our analysis, we begin tackling this question by estimating a model of student preferences and graduation out-

comes. In the model, students' payoffs and graduation probabilities depend on academic and non-academic observables as well as unobserved types that interact flexibly with program characteristics, including field of study, institution type, selectivity, and baseline demographic composition. This approach allows for dependence between students' application choices and potential outcomes, correcting for selection in the spirit of a control function approach (Abdulkadiroğlu et al. 2020), but without imposing strong distributional assumptions (Heckman and Singer 1984).[1] The model's extrapolations are disciplined by several sources of variation: observed rank-ordered lists, variation in admissions and cutoffs from centralized assignment, distance to programs, and shifts in access induced by the relative rank reform itself.[2] The model fits this variation well and replicates sorting patterns when re-simulating the assignment equilibrium, including in a year held out of estimation.

Our model indicates that the part of a program's causal effect that is student-specific — that is, student-program match — is an important determinant of graduation, creating a role for programs to improve aggregate outcomes by selecting students based on fit. Following Abdulkadiroğlu et al. (2020), we decompose predicted graduation into student ability, program average effects, and student-program match effects. Match explains 21 percent of the variance in predicted graduation across students, compared to 49 percent for student ability and 30 percent for program average effects. Match is both vertical, mattering at very selective programs and consistent with a role for student preparation, and horizontal, reflecting sorting across fields of study on comparative advantage consistent with differentiated student skills and interests (e.g. Kirkeboen, Leuven, and Mogstad 2016). There is positive sorting on gains in the sense of Roy (1951): students apply to and enroll in programs where they have higher match, patterns that are an empirical result and not imposed by the model's structure. Self-selection helps rationalize why the relative rank policy had few displacement effects even though match matters: the programs students are shifted between by the policy tend to be similar in match.

Third, we estimate programs' preferences for the academic and non-academic composition of their admitted students. We use the DA algorithm to simulate pools of admitted students under every alternative choice of weights, and use these menus of admitted students to infer programs' objectives. Programs have strong preferences for predicted graduation over and above test scores,

[1] Bonhomme, Lamadon, and Manresa (2022) show that discrete types can be cast as an approximation to continuous unobserved heterogeneity. See also Bonhomme and Manresa (2015) and Bonhomme, Lamadon, and Manresa (2019) for applications to panel data and Conlon, Mortimer, and Sarkis (2026) for an application to demand estimation.

[2] We combine self-revelation through rank-ordered lists (Dale and Krueger 2002, 2014; Abdulkadiroğlu et al. 2020; Mountjoy and Hickman 2021; Abdulkadiroğlu, Pathak, and Walters 2026), distance to programs (Card 1995; Walters 2018; Mountjoy 2022), variation from centralized assignment (Abdulkadiroğlu et al. 2022) including hundreds of admission cutoffs (Hastings, Neilson, and Zimmerman 2013; Kirkeboen, Leuven, and Mogstad 2016), and the variation induced by the reform (Black, Denning, and Rothstein 2023; Bleemer 2024; Barahona, Dobbin, and Otero 2026).

driven largely by a taste for match, indicating that programs tend to select students based on fit. This finding also reflects the importance of college discretion in mediating the effects of the relative rank policy — the lack of impacts on graduation is partially due to *programs' choices*. We also find that, all else equal, programs prefer to admit more low-SES students on average. However, these preferences are heterogeneous: colleges disagree on their admission objectives, with some estimated to prefer fewer low-SES students.

Fourth, we use the model to explore alternative admission designs, focusing on graduation, access, and segregation as key outcomes, and accounting for equilibrium adjustments through the mechanism. Shutting down discretion by moving to admissions based only on test scores slightly reduces graduation (-0.25pp) and reduces access for low-SES and female students. Moving instead to lottery-based admissions, as is common in K-12 settings, sharply reduces socioeconomic segregation (by 7.3 percent) but also lowers graduation by 1.9pp overall and 3.1pp among those assigned. These results underscore the importance of discretion — removing the ability for colleges to sort students destroys valuable matches, while relying on exams alone reduces access and is insufficient to improve student performance. However, a full-discretion counterfactual increases segregation (+6.7 percent) while yielding no gain in graduation. As programs already select strongly on graduation at baseline, Chile's system sits near the graduation frontier: even a likely infeasible benchmark counterfactual that assigns seats solely by expected graduation would only raise graduation by 0.5pp.

Our findings illustrate important policy tradeoffs in the design of college admissions. On the one hand, match matters: the same program benefits some students more than others, so discretion has the potential to raise aggregate graduation, and colleges are also likely better placed than a policymaker to assess student fit. On the other hand, college preferences are heterogeneous, meaning that at least some colleges will disagree with a policymaker's preference for equity. This tension explains why Chile's partial discretion performs well on both dimensions: it gives programs enough levers to select on fit while also restraining them in ways that limit adverse impacts on graduation and access. In general, the results of this paper support delegating some admissions authority to colleges, but imposing guardrails to ensure alignment with a policymaker's goals.

Contribution to the Literature This paper contributes to our understanding of the design of centralized assignment systems by focusing on the supply side of higher education, which the literature on centralized admissions largely treats as passive. Much work has estimated student demand in centralized systems (Abdulkadiroğlu, Agarwal, and Pathak 2017; Agarwal and So-

maini 2018; Fack, Grenet, and He 2019; Kapor, Neilson, and Zimmerman 2020; Vrioni 2024), including in Chile (Bordon and Fu 2015; Kapor, Karnani, and Neilson 2024; Larroucau and Rios 2025; Fuentes-Acosta 2026).[3] This literature generally treats the priorities that schools use to rank applicants as fixed, either examining a social planner's tradeoffs (e.g. Ellison and Pathak 2021) or quantifying the contribution of admission criteria to segregation (Idoux 2023). Little work studies the supply side as an active participant in setting these admission priorities. In this vein, our paper is closest to Agarwal (2015), who estimates medical programs' vertical preferences for students; Friedrich et al. (2025), who focus on identifying interdependent values in Danish medical school admissions; and Cheng and Weingarten (2026), who recover K-12 magnet schools' preferences for students to study a shift from decentralized to centralized, lottery-based admissions. We contribute by leveraging publicly observed admission rules to recover supply-side preferences across hundreds of programs at a national scale. The known mechanism lets us simulate programs' menus of admitted students, allowing us to model a program's selection of rules as a discrete choice problem with unobserved preference heterogeneity. We also exploit policy variation in the amount of discretion allowed within a centralized system.

Our paper is also related to the literature that models student applications and equilibrium in the college market. Models of applications, admissions, and enrollment have been used to study testing requirements (Borghesan 2023), tuition and financial aid (Epple, Romano, and Sieg 2006; Fu 2014; Fillmore 2023; Cook 2026), affirmative action (Arcidiacono 2005; Howell 2010), and percent plans (Kapor 2025). As colleges in these settings do not publish admission rules, these papers face a challenging problem: colleges' preferences over students must be recovered indirectly from admissions outcomes, which requires modeling strategic self-selection into the applicant pool, itself a function of admissions probabilities. Focusing on the Chilean setting allows us to make progress in three ways. First, we discipline a model of student graduation using observed rank-ordered lists and quasi-experimental variation from centralized assignment in an initial step that is separate from the estimation of colleges' objectives. Second, observed admission rules and the reform allow us to relax structure imposed on colleges' objectives, enabling us to recover program-specific unobserved tastes for students. Third, we can obtain counterfactual allocations and admitted classes directly by re-running the assignment algorithm.

Our paper also relates to the literature studying admission policies that affect college access, such as affirmative action, percent plans, and test-optional admissions. We contribute by high-

[3]Fuentes-Acosta (2026) estimates a model of applications, consideration sets, and graduation to evaluate the 2013 introduction of Chile's relative rank policy, holding programs' weights fixed. Our paper is complementary in that we use the policy to study the role of discretion in admissions. As in Kapor, Karnani, and Neilson (2024), we do not micro-found a model of strategic applications, since we assume assignments are stable with respect to true preferences, which suffices for our admissions simulations and counterfactuals.

lighting colleges' role in mediating these policies' impacts and by developing a framework to evaluate alternative admissions regimes beyond a single policy. Recent studies tend to find that students gaining access through these inclusive admissions policies benefit at rates at least as large as those of displaced students (Bleemer 2022; Black, Denning, and Rothstein 2023; Bleemer 2024; Barahona, Dobbin, and Otero 2026), including in Chile (Reyes 2022), often concluding that these policies can increase equity in access without losses in average outcomes. Our analysis of Chile's reform is consistent with these findings. However, how far this conclusion can extend depends on the structure of potential outcomes and match effects (Arcidiacono and Lovenheim 2016; Bleemer and Rothstein 2026), especially at margins not moved by policy-specific variation.[4] Rather than limiting ourselves to a single policy, we quantify match and student preferences in the spirit of structural models that incorporate self-selection on unobserved gains (Willis and Rosen 1979; Arcidiacono 2005; Carneiro, Heckman, and Vytlacil 2011; Heckman, Humphries, and Veramendi 2018; Walters 2018). Specifically, we estimate a model in which unobserved student types interact flexibly with program characteristics to shift both student utility and graduation outcomes (e.g. Keane and Wolpin 1997; Arcidiacono 2005). In contrast to Abdulkadiroğlu et al. (2020), who find that match effects play a limited role for high school test scores, we find that match matters for college graduation. This finding is also consistent with design-based evidence from a later Chilean policy (Carlana, Miglino, and Tincani 2024).[5] Moreover, we contribute by showing that match matters not only for student outcomes but also as a driver of college behavior, so that colleges themselves mediate policy impacts.[6]

Outline. Section 2 outlines data and motivating facts about programs' rule choices. Section 3 examines the role of college discretion in the impact of Chile's relative rank policy. Section 4 shows the results of our approach to modeling student applications and graduation outcomes. Section 5 discusses how we infer program preferences from rule choices. Section 6 presents the impacts of counterfactual college admissions systems. Finally, Section 7 concludes.

[4]Match can occur on multiple margins, all of which we seek to incorporate into our framework. Previous work has found that student preparation matters for returns to selective or STEM degrees (Arcidiacono, Aucejo, and Hotz 2016; Dillon and Smith 2020; Carlana, Miglino, and Tincani 2024) and that socioeconomic background matters for returns to elite degrees (Zimmerman 2019). Other research finds evidence for high returns for marginal students (Hoekstra 2009; Zimmerman 2014; Goodman, Hurwitz, and Smith 2017; Mountjoy 2026) and larger returns for disadvantaged groups (Dale and Krueger 2002; Bleemer 2022; Black, Denning, and Rothstein 2023; Bleemer 2024; Barahona, Dobbin, and Otero 2026). Relatedly, Kirkeboen, Leuven, and Mogstad (2016) provide evidence for selection into field of study based on comparative advantage. Other approaches find limited roles for match (Mountjoy and Hickman 2021).

[5]Carlana, Miglino, and Tincani (2024) study a later Chilean policy that guarantees admission to selective colleges for students in the top 15 percent of their class at disadvantaged high schools. They find that the policy benefits the average targeted student but that marginally eligible students see higher dropout rates and worse labor market outcomes, suggesting that match matters at different margins.

[6]Consistent with this, Antonovics and Backes (2014) find that public universities changed the admissions weight placed on academics relative to student background after California's affirmative action ban, partially offsetting its impacts.

2 Context and Data

Setting Higher education in Chile is offered by around 150 institutions, broadly categorized into (i) technical schooling centers, (ii) professional institutes, and (iii) universities awarding academic degrees (i.e., bachelor's, master's). Our focus is on undergraduate admissions at selective universities, which are determined through a centralized assignment system. Students submit rank-ordered lists (ROLs) of their preferences over *programs*, unique combinations of majors, universities, and location/campus.[7] After submission of preference lists, students are algorithmically and simultaneously assigned to programs through a college-proposing DA algorithm with flexible quotas (Rios et al. 2021).

The Chilean setting has several features that make it well-suited to study the role of discretion in admissions. First, admission priority is completely determined by the same fixed, well-defined requirements across all colleges: standardized college entry exams and high school performance. During our sample period, the entry exam was the *Prueba de Selección Universitaria* (PSU), consisting of subject tests in Math and Language/Verbal, and a choice of either Natural Sciences and/or Social Sciences. High school performance is evaluated through cumulative GPA. Admission priorities are completely determined by the combination of these scores.[8]

Second, programs are allowed to exercise *public* and partial discretion over their admission priorities. In fall of every year, programs report the number of available seats for admission to the upcoming academic year (March through December). At the same time, each program also publishes their choices of linear *weights* assigned to each test and GPA score, along with minimum required scores in specific tests. All requirements—including one of the optional Natural/Social Science tests—must be assigned a weight of at least 10 percent; GPA must constitute between 20 and 50 percent of the total weight. Subject to these constraints, programs are allowed to freely choose the priority given to test vs. non-test components, as well as each of the subject scores.[9] Figure 1 shows the distribution of total weight assigned to test scores in 2013 across colleges and programs.

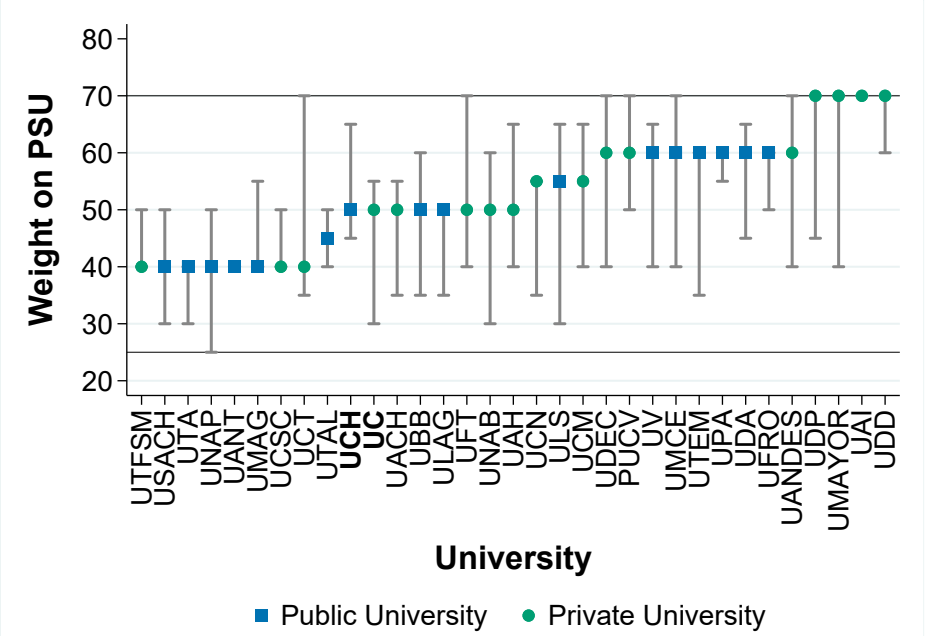
The linear weights and their heterogeneity across programs are the main objects of study in this paper. Weight choices express programs' preferences over students, as they determine the order in which applicants are ranked and ultimately assigned. Given student rank-ordered lists

[7] The Chilean college structure offers substantially less flexibility in switching programs than in the U.S. For the most part, students need to re-apply through the centralized system to gain admission to a different program.

[8] Exceptions include arts programs, rare sports scholarships, or preferential admission schemes, such as the *Beca de Excelencia Académica* (BEA) or the *Programa de Acompañamiento y Acceso Efectivo a la Educación Superior* (PACE, see Tincani, Kosse, and Miglino 2025). The scope of these programs is small relative to the total pool of admissions. For example, in our sample, BEA admissions make up about 5% of a program's slots on average, with a median of 3.7%.

[9] Weights are further restricted to be nonnegative multiples of 5.

Figure 1: Distribution of Test (PSU) Weights in 2013



Notes: This figure shows the empirical distribution of total weights assigned to exam scores (PSU) across universities. The University of Chile is marked as "UCH". Pontificia Universidad Católica de Chile is marked as "UC". Observations are grouped at the university level, ordered from lowest to highest, and distinguishing public from private institutions. The gray lines indicate the range across programs within each institution; the markers show each university's modal weight, colored by public or private status. See Appendix Figure A4 for the 2013 seat-weighted distribution of admission weights.

and program weights, the system assigns students to programs using a variation of the deferred acceptance algorithm in Gale and Shapley (1962). Each program filters applicants by minimum requirements, ranks the remainder by application scores, and accepts students in order until seats are filled. Students are assigned to the highest-ranked program at which they can gain admission. Given the system restricts selection to these numerical scores, programs must rely on correlations between metrics and other characteristics to influence outcomes such as the demographic composition of admitted students. We therefore expect considerations such as diversity preferences to translate directly into weight choices.

The partial discretion in the Chilean setting constitutes a "middle ground" when compared internationally. Countries such as Norway or China conduct admissions based on public, largely uniform criteria that apply equally across all institutions. In contrast, U.S. universities have discretion to weight non-academic factors substantially — legacy status, diversity, or attributes such as "fit" (Arcidiacono, Kinsler, and Ransom 2024) — and do not publicly disclose weights or cutoffs for individual requirements.[10] By allowing partial yet observable discretion, the Chilean context provides a unique setting to examine how college discretion shapes access to higher education.

[10]See Kirkeboen, Leuven, and Mogstad (2016) for details on the Norwegian case, Chen and Kesten (2017) and Jia and Li (2021) for the Chinese case, and Figure A1 (Bleemer 2024) for an example of UC Davis' 2002 "admissions protocol."

The GPA+ Policy In our analysis, we leverage the introduction of a new admission requirement during the 2013 enrollment cycle: the relative rank score (*Puntaje Ranking*). This measure captures students' relative performance within their high school by comparing their GPA to that of previous cohorts. Following Reyes (2022), we refer to this score as GPA+, since it is implemented as a "boost" over the base GPA score for above-average students. Details of the GPA+ calculation are shown in the Appendix (Figure A2).

The inclusion of the GPA+ requirement in the centralized system was carried out in two stages. Programs were first mandated to assign a fixed, 10 percent weight to GPA+ for the 2013 admission cycle. Starting in 2014, the weight was allowed to vary along with all other requirements. Given our focus on the role of discretion, we focus primarily on the 2014 admissions cycle. The design of the GPA+ measure resembles policies implemented in other contexts, such as the Texas Top 10 (Kapor 2025) or the Eligibility in the Local Context policy in California (Bleemer 2024). However, in the case of the relative rank score, Chilean programs retained *discretion* over how heavily to weight the measure—the main source of variation we exploit in this paper.

The introduction of the relative rank score was motivated by the desire to address inequalities in access to higher education (DIRCOM – Universidad de Chile 2012). The stated aim was to recognize students who demonstrated strong academic potential within their educational context. Figure 2 shows that standardized test scores strongly correlate with socioeconomic status, while the GPA+ distribution is fairly homogeneous across high- and low-SES students.[11] The GPA+ measure was therefore designed to increase admission of low-SES, high-achieving students without explicitly using socioeconomic background as a criterion.

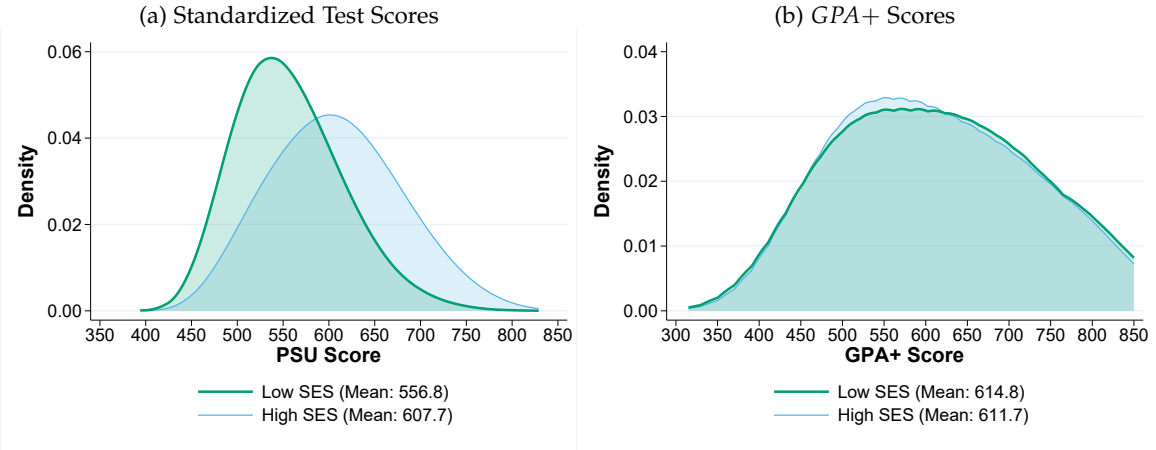
The policy was further supported by evidence that grades and within-cohort performance were strong predictors of college success (Kri et al. 2013; Soria-Barreto and Zúñiga-Jara 2014). Figure 3 shows the relationship between academic measures and graduation outcomes. In the spirit of Friedman et al. (2025), we isolate the predictive power of each academic measure net of observable confounders by controlling for correlated academic and demographic characteristics, and account for program composition by including program-year fixed effects.

All admission scores — exams, GPA, and GPA+ — are predictive of student outcomes, but with differing informativeness depending on a student's SES and their place in the score distribution. High-school grades are most predictive of academic success: a one-point increase in GPA predicts an 18.8pp increase in graduation for high-SES students and a 15.9pp increase for low-SES ones. Test scores are especially predictive at the bottom of the distribution and differ little across groups. High-school relative rank, by contrast, is significantly more informative for low-

[11] Appendix Figure A3 shows a slightly weaker correlation with gender for standardized test scores, and a stronger performance by female students in terms of the GPA+ measure.

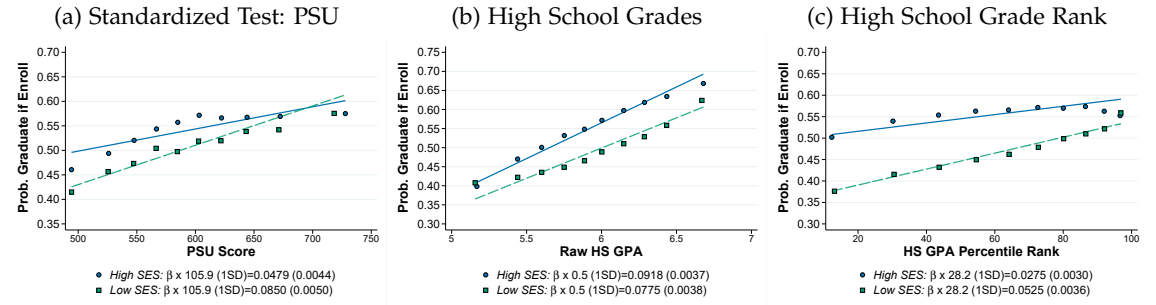
SES students. Moving from the bottom to the top of the class corresponds to a 19pp increase in graduation probability for low-SES students, almost double the 10pp increase for their high-SES peers. Therefore, a program's incentives in weighting each measure, including how it responds to GPA+ discretion, depend jointly on the specific measure as well as the margin of student.

Figure 2: Distribution of Admission Requirements by Socioeconomic Status



Notes: This figure shows the distribution of PSU scores and GPA+ scores by student demographic characteristics. The sample is applicants from the 2013 application cycle. We use one-third of a standard deviation as the bandwidth. See Figure A3 for density estimates by gender.

Figure 3: Graduation Gaps by Socioeconomic Status across Measures



Notes: Binned scatterplots of graduation against each academic measure, split by socioeconomic status, among enrollees in the 2012–2014 cycles, adjusting for the other academic inputs, the complementary demographic, private, and program-by-year fixed effects. Group-specific slopes and the level difference (evaluated at mean PSU) with program-clustered standard errors are reported in each panel.

Data Our analysis uses public, administrative data from the Chilean centralized admission system from 2012 to 2014.[12] For each year, we observe information on all students sitting the PSU exam, all applications to the centralized system, enrollment, and graduation outcomes for all students within the higher education system. On the program-side, we observe all programs offering

[12]We also use data from 2015 for out-of-sample validation.

admission through the centralized system and their submitted admission weights. We focus special attention on the 2013 and 2014 admission cycles, since they correspond to the introduction of the GPA+ discretion.

The student data come from *Departamento de Evaluación, Medición y Registro Educacional* (DEMRE), which oversees the national entry exam and centralized admissions. For each exam-taker, we observe demographic information (gender, age), application scores (GPA, GPA+, and all subject exams), and a unique high school identifier. We link this identifier to data from SIMCE, a battery of standardized tests administered periodically to all Chilean schools, which provides detailed information on each high school's location, type (public/private), and socioeconomic composition. We also observe students' rank-ordered application lists and outcomes at each program (accepted, waitlisted, admitted to a preferred program, or various types of invalid applications). These data allow us to precisely forecast how student allocation would change under different admission component weights.

Program-level data for all universities participating in the centralized system come from both DEMRE and the Ministry of Education. For each program, we observe basic characteristics (minimum duration, location, available seats, sticker prices) and, crucially, the weights assigned to each admission requirement. We refer to these throughout as admission requirement weights, score weights, or simply weights. These data allow us to study heterogeneity in program preferences and their interaction with the assignment mechanism.

Finally, we use enrollment and graduation data from the Ministry of Education, from 2012 through 2022. These data track outcomes at all higher education institutions, including those that do not participate in the centralized assignment system — technical centers, institutes, and non-selective colleges. We use these data to examine the relationship between student characteristics, weight choices, and program-level outcomes.

DA mechanism simulations. Our analysis relies on the assignment mechanism to simulate program admissions under different score weight combinations. This procedure requires predicting application scores at each program from supplied grades and exam scores, then clearing the market using a college-proposing deferred acceptance algorithm. We successfully replicate 99.5% of assignments from student rank-ordered lists, with the remaining gap consistent with unlisted admission criteria or minor reporting errors.

Sample restrictions. In 2014, 33 institutions participated in the centralized admission system, offering a combined total of about 1,400 programs accepting applications. For most of the program-focused analysis, we focus on 716 programs that are "active" in both the 2013 (the pre-discretion baseline) and 2014 enrollment cycles (post-discretion). We define a program as active in an appli-

cation cycle if (i) it enrolls at least one student, and (ii) it rejects at least one applicant, making the admissions threshold binding. These are the programs we expect to face meaningful trade-offs when selecting students. Table A1, discussed in the next section, displays summary statistics of key variables for the sample, broken out by programs' choice of $GPA+$ weight in 2014.

Summary Statistics. Table 1 shows summary statistics for the students in our sample across application cycles. In each year, over 100,000 students apply to about 1,400 programs. Low-SES students account for around 45 percent of applicants; roughly half of all applicants are female. Both these shares and other academic characteristics, such as raw GPA and within-HS class rank, remain stable across years. Characteristics of the applications, such as those submitting shortlists and those reapplying, are also similarly stable across years. Despite this stability, the GPA+ policy does affect students—in 2013 and 2014, just under 13 percent of all students would have a different assignment outcome had they operated under the other year's observed rules. However, while the GPA+ policy shifts students between programs, it appears not to have strong extensive margin effects, unlike other percent plans (Kapor 2025).

Around 90 percent of students are assigned every year by the centralized system. However, only around 73 percent of students enroll in their assigned option, increasing by 1.4pp between 2013 and 2014, reflecting some students turning down their assignment or others enrolling from the waitlist at other programs. Completion is also not assured: within 8 years of enrollment at a university, only about half of students receive a degree from that institution. Graduation rates increase slightly over time. As discussed below, this time trend appears unrelated to the GPA+ policy, likely instead reflecting decreased dropout and extensions related to COVID-19.

Table 1: Summary of Applicants by Cycle

	(1) 2012	(2) 2013	(3) 2014	(4) 2015
Female (%)	51.8	51.1	51.4	51.7
Low SES (%)	44.1	45.2	45.0	45.7
SEP Share (%)	13.2	23.3	26.4	25.8
Share Elite Public HS (%)	12.2	11.8	12.0	10.9
Share Exposed (%)		12.6	12.9	
PSU	585.2	584.7	583.6	583.1
Raw GPA	5.9	5.9	5.9	5.9
HS GPA Rank	60.9	60.9	61.2	61.0
HS Avg. PSU	538.3	537.5	536.6	535.2
Any Assignment (%)	87.7	88.6	89.5	86.5
Enrolled if assigned (%)	70.3	72.7	74.1	74.7
Share Enroll (Any DEMRE) (%)	71.9	74.9	76.5	74.5
Share Graduate (Enrolled Program) (%)	52.8	51.5	52.9	54.6
Avg. Number of Apps	4.8	4.4	4.2	4.3
Share Shortlist (%)	92.7	95.0	96.1	96.0
Share Reapply (%)	17.6	16.8	17.4	20.0
N (Students)	106,719	107,546	106,804	112,824
N (Programs)	1,335	1,393	1,419	1,422

Notes: Summary statistics for students submitting any application in each year from 2012 to 2015. Graduation is measured as receiving a degree from the university of initial enrollment within 8 years. Low SES indicates that the student's high school is classified in the bottom three of SIMCE's five socioeconomic groups. SEP indicates a socioeconomically vulnerable student who is eligible for a targeted school voucher under the *Subvención Escolar Preferencial* program in the application year or the year before (there is only partial coverage in 2012). See Neilson (2025) for more information. Elite Public HS indicates a non-private high school whose test takers averaged above 600 PSU points in any year from 2010 to 2014. Exposed students are those whose assignment differs under the other year's admission weights (see Section 3). HS GPA Rank is the within-school-cohort percentile of raw GPA; HS Avg. PSU is the leave-one-out mean PSU of test takers at the student's high school. Shortlist indicates applicants listing fewer than ten programs; Reapply indicates applying again in either of the next two cycles.

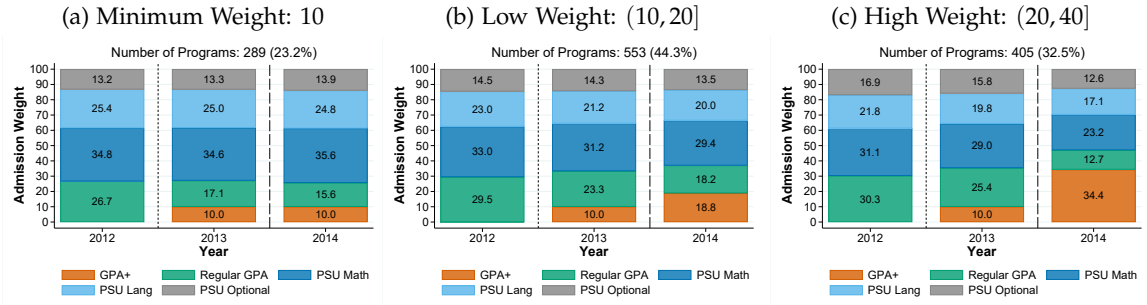
3 The Role of College Discretion in Chile's Relative Rank Policy

In this section, we show how programs used the discretion afforded by the GPA+ policy and how it impacted student composition. Programs did not uniformly respond to the GPA+ policy, and some of this differential response is explained by baseline observables. Discretion directly changed who was admitted to adopting programs, raising the share of low-SES and female students at programs that adopted high weights on relative rank. While students admitted at the margin were not stronger than the ones they replaced, and were weaker on average at the highest adopters, these compositional changes came at little aggregate cost to completion. We read these results as most consistent with colleges having heterogeneous preferences for different student characteristics, with at least some programs trading off student composition against academic quality.

Heterogeneous GPA+ Adoption Figure 4 shows the response to the GPA+ policy for programs active in each year from 2012 to 2014. Adoption was far from uniform: about 23 percent of programs remained at the statutory minimum of 10, while more than 30 percent set a weight above 20. We refer to the latter group as strong adopters or "high GPA+ programs."

The responses to the policy implied a sharp divergence in the emphasis placed on test versus non-test components across these groups. In 2012, programs placed about 70 percent of their weight on test scores. However, by 2014, high GPA+ programs reduced the weight on tests by nearly 20 points. In contrast, programs at the minimum left their test-score weight essentially unchanged, taking the mandated GPA+ component out of the weight previously assigned to regular GPA.

Figure 4: Program Weights by Year and 2014 $GPA+$ Weight

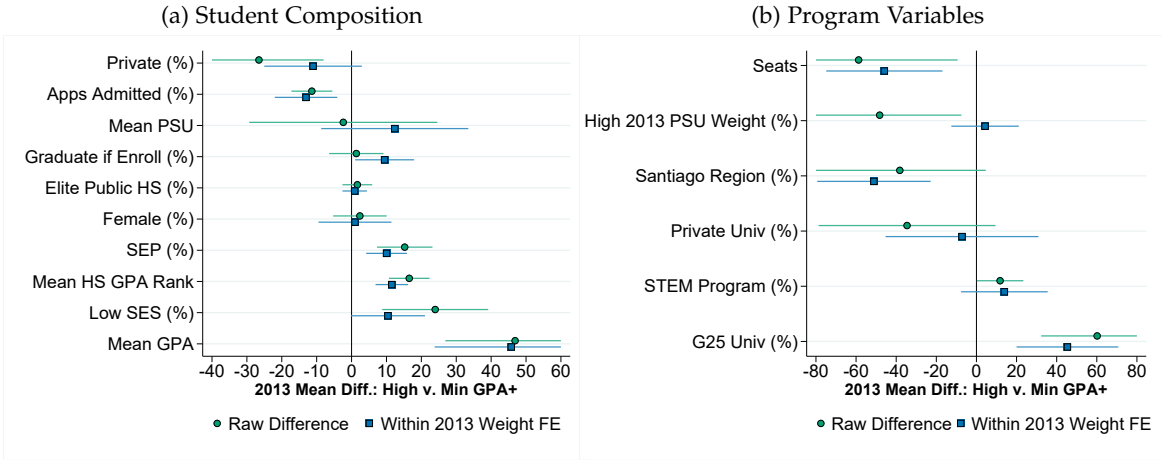


Notes: Sample is 1,247 DEMRE programs with at least one enrolled student in each year from 2012 to 2014. Programs are weighted by number of seats. "PSU Optional" is the elective science or history exam. "Minimum Weight" is defined as having $GPA+$ weight set to the statutory minimum of 10. "High Weight" is defined as setting a 2014 $GPA+$ weight strictly above 20, the median weight for programs above the statutory minimum (unweighted). "Low Weight" is defined as a 2014 $GPA+$ weight in $(10, 20]$.

Figure 5 shows that high 2014 GPA+ programs differed in their pre-discretion 2013 characteristics. Relative to programs at the minimum, high GPA+ programs are smaller, by 58.7 seats, and more selective, admitting 11.4pp fewer of the applicants they consider. Strong adopters are also less likely to be private, much more likely to belong to the traditional G25, less likely to have a campus in Santiago, and enroll a larger baseline share of low-SES students. Notably, these programs enroll students with nearly identical PSU exam scores but much higher GPA/GPA+ scores, even before the policy. Most differences persist within baseline weight cells, where high GPA+ programs also enroll students 9.47pp more likely to graduate. See Appendix Table A1 for precise summary means.[13]

[13] Appendix Table A2 regresses the 2014 weight jointly on baseline characteristics. Enrollee GPA, PSU, and the low-SES share remain predictive; most institutional characteristics do not once entered jointly. Baseline observables and weight cells together explain 53 percent of the variation in GPA+ adoption.

Figure 5: Differences in Sample Means by GPA+ Weight



Notes: Differences in means for high GPA+ adopters (defined as the 2014 GPA+ weight strictly above 20). All variables are computed among enrollees unless otherwise stated. "2013 Weight FE" control for coarsened PSU-GPA rule choices in 2013. All statistics and regressions are weighted by the number of enrollees in 2013. SEs are clustered by university. See Appendix Table A1 for precise point estimates.

Impacts of GPA+ Discretion on Student Composition The choice of weights directly changed the type of students admitted to each program. Figure 6 shows the relationship between GPA+ weight choice and changes in directly targeted academic characteristics. For each 2014 GPA+ weight, we plot the average 2013–2014 change in admitted students' GPA+, GPA, and PSU at programs choosing that weight.[14] We observe a clear tradeoff between test and non-test academic measures: increasing the weight on GPA+ led to strong declines in PSU exam scores. The pattern confirms that programs could, and did, move their admit pools to trade off test scores against grades.[15]

The key question is whether these observed impacts reflect a causal role for discretion to impact program composition through student selection. A potentially confounding effect of the policy is changes in application behavior. Students know their scores and observe programs' admission weights at the time of submitting applications. The observed patterns may therefore reflect changes in application patterns instead of admission priorities.[16] A secondary confounder involves equilibrium spillovers between programs: a program's admit pool depends on its competitors' choices. If competing programs adopt at similar rates, some of the observed effects may

[14] No program chose a weight of 35.

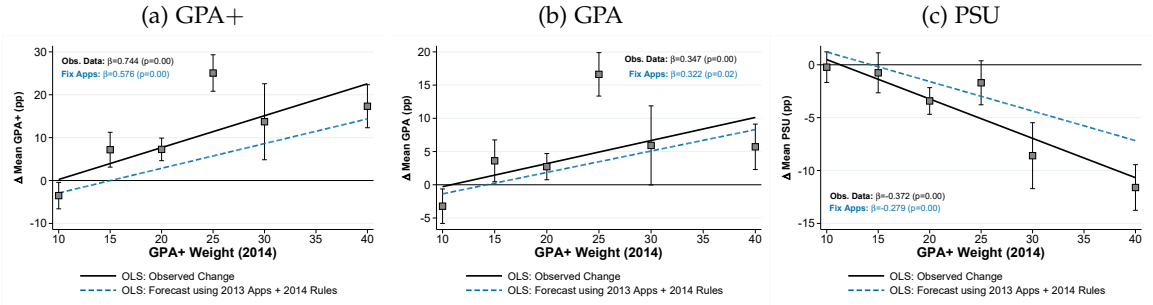
[15] Observed changes look almost identical when comparing the characteristics of enrolled versus admitted students. However, we focus on admitted students in this section as we can directly forecast changes in admit pools using the mechanism.

[16] While truthful preference revelation is optimal for students in deferred acceptance mechanisms, prior work on college admissions in Chile indicates that students are more likely to strategically apply to programs where they have a higher chance of assignment (Larroucau and Rios 2020; Larroucau et al. 2024). Reyes (2022) and Fuentes-Acosta (2026) show some evidence of strategic responses by students to admissions chances in the context of the GPA+ policy. Concha-Arriagada (2026) also shows that students respond to the policy by switching high schools.

be driven or offset by equilibrium forces.

To assess the importance of these mechanisms in driving the patterns in Figure 6, we leverage knowledge of the assignment mechanism to forecast the effect of the discretion policy, *keeping applications fixed to the baseline*. Specifically, we simulate the effect of programs implementing their chosen 2014 weights in 2013 — a year earlier than in the data — thereby shutting down the application-response channel. The blue/dashed lines in Figure 6 show that these forecasts mirror the observed changes in GPA+ and PSU. We provide a formal decomposition of the relationship between GPA+ and changes in student characteristics into the direct effects of discretion, indirect effects from equilibrium adjustments, and changes in applications in Appendix Table A3. While application changes and spillovers from competitors play a role, they largely offset each other, and the relationships between observed changes and GPA+ weight strengthen when removing these effects.[17] Changes in admitted student composition are therefore primarily a direct consequence of colleges' weight decisions affecting admission priorities.[18]

Figure 6: 2014 *GPA+* Weight and Changes in Academic Characteristics of Admitted Students



Notes: Each square shows the mean change in the indicated admissions characteristic by 2014 GPA+ weight, weighting programs by number of enrollments. Panel (a) shows changes in the boosted GPA measure (i.e., GPA+), Panel (b) shows the changes in the GPA measure, and Panel (c) shows changes in composite PSU exam scores. The solid black line reports the OLS relationship between 2014 GPA+ weight and changes in an average academic measure (corresponding to column (1) of Table A4). The blue dashed line shows predicted changes from simulations using 2013 applications, holding competing programs to their observed 2014 choices but shutting down changes in applications. Standard errors for OLS relationships are clustered at the university level. Error bars report 95 percent confidence intervals and are not adjusted for clustering. See text for more details.

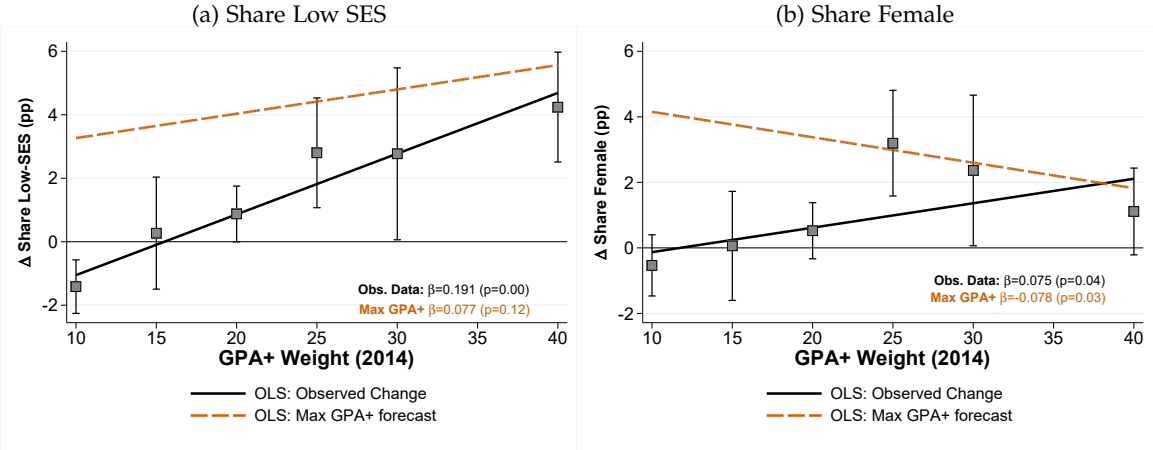
Figure 7 shows that discretion also impacted characteristics *not directly targeted* by admissions. Universities cannot select applicants based on demographic characteristics such as sex or socioeconomic status. Nevertheless, the shares of both female and low-SES students strongly increase at the programs that adopt higher GPA+ weights. The effect is particularly strong for the latter: moving from the minimum to the maximum GPA+ weight is associated with a 5.7pp increase in the share of low-SES students. This change represents a 15 percent increase over the average share in 2013 (38.3 percent). For female students, a higher GPA+ weight choice is associated with a 2.2pp

[17] Panel A of Appendix Table A3 decomposes the OLS relationship between GPA+ and changes in student characteristics, showing that unilateral program weight adjustments are the dominant drivers of changes.

[18] Appendix Table A4 shows that the relationships are robust to controlling for baseline observables, baseline weight fixed effects, and contemporaneous changes in other student characteristics.

increase in their share, or 4.5 percent over the baseline (48.7 percent). Appendix Table A4 shows that the low-SES pattern remains after accounting for correlated changes in student observables, consistent with a direct preference for socioeconomic composition.

Figure 7: 2014 *GPA+* Weight and Changes in Non-Targeted Characteristics of Admitted Students



Notes: Shows the change in the shares of low-SES and female students between 2013 and 2014 among admitted students, stratified by 2014 GPA+ weight. The orange/dashed line shows predicted changes from simulations in which each program deviates unilaterally from its observed choice to the maximum GPA+ weight in the 2014 equilibrium using 2014 applications. See notes to Figure 6.

We next show that these changes are driven by different program *choices*, not constraints from applicant pools: many programs could have admitted more low-SES and female students but *chose* not to. The orange dashed lines in Figure 7 show the forecast effect of forcing all programs to unilaterally adopt the maximum GPA+ rule.[19] Programs that picked the minimum GPA+ weight in 2014 could have increased their low-SES share by 3.3pp, 12 percent of their baseline share of 28 percent. Similarly, female representation would have increased by 4.2pp (9 percent) had these programs instead chosen the maximum GPA+ weight. Programs' choices, not constraints, largely determine the impact of the policy. This result underscores colleges' role as active participants in shaping their student composition and is suggestive of heterogeneous program tastes for students.

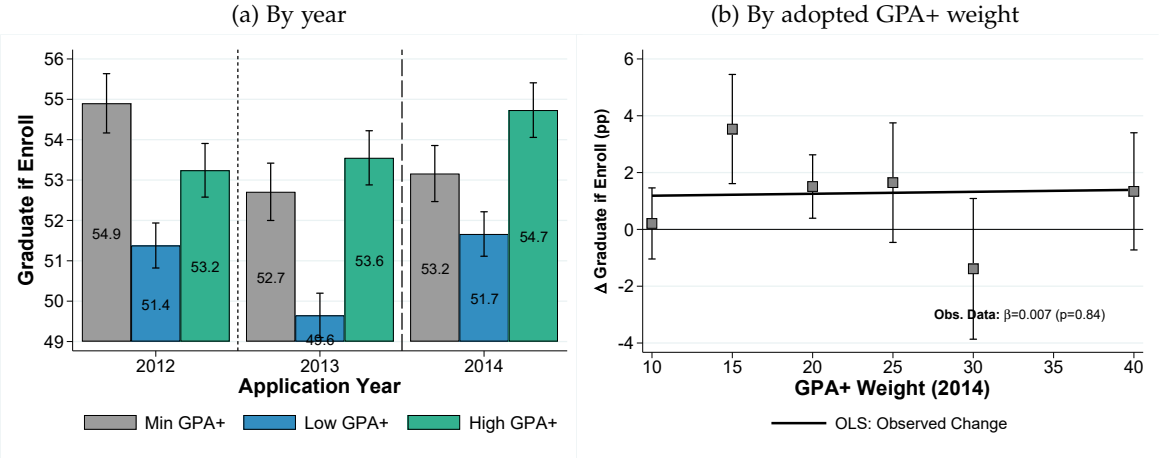
Impacts of GPA+ Discretion on Student Achievement

Aggregate Patterns. Was this increase in diversity costless in terms of student graduation? We first look at aggregate patterns, which show little movement in eight-year graduation around the policy in Figure 8. Figure 8(a) shows eight-year graduation rates for enrollees across all active programs, by application year and adoption level. Graduation rates rise slightly across the board in 2014. The increase is strongest for moderate adopters at +2.1pp, but is also present at the

[19] The orange/dashed line conducts the simulations within the 2014 equilibrium, holding fixed applications and competitor rule choices. When imposing a GPA+ weight of 40, we account for shifts in other weights by simply rescaling them to preserve their original ratios.

minimum (+0.5pp) and high end (+1.1pp) of GPA+ choices. Figure 8(b) shows the same 2013–2014 change by GPA+ weight choice for programs that are selective in both years. In general, the average change in graduation among enrollees appears unrelated to GPA+ adoption.[20]

Figure 8: Aggregate Changes in Graduation around the GPA+ Policy



Notes: Panel (a) shows eight-year graduation among enrollees for programs having enrollment in every year 2012–2014, by application year and 2014 GPA+ weight group. Error bars show 95 percent confidence intervals. Panel (b) restricts to programs selective in both 2013 and 2014, showing the 2013–2014 change in graduation among enrollees, stratifying by 2014 GPA+ weight. The OLS slope and university-clustered p-value of this relationship are shown in the legend.

Measuring GPA+ Exposure. To focus our analysis on the students affected by the policy, we use the mechanism to classify applicants as "pulled in" or "pushed out," following Black, Denning, and Rothstein (2023) and extending the analysis of the uniform 2013 introduction in Reyes (2022) to the 2014 discretion year. Our approach to identifying exposure to the policy based on reported applications is illustrated and described in detail in Figure 9.

We start by identifying the weights observed for all programs in 2013 (w_{2013}) and 2014 (w_{2014}). For applicants in both years, we compare observed assignments to those simulated under the *other* year's weight choices. Students "pulled in" by the policy are those who would be admitted to a program under the 2014 discretion weights, but rejected under the 2013 ones (purple in Figure 9). In the data, these are students admitted to a preferred option in 2014 but rejected in 2013. Similarly, students admitted under the 2013 weights but not the 2014 ones are "pushed out" (orange). The simulations also identify students whose admission to the target program is unaffected by the GPA+ policy.

We use two distinct designs to learn about the impact of the policy for students and programs. For students, we use comparisons *within* the pulled-in and pushed-out groups across years to

[20] Looking at the precise year of enrollment and graduation in our administrative data, it appears the overall increase in graduation rates in 2014 is related to decreased dropout during the COVID-19 pandemic, which results in increased graduation in the post-period.

examine the effect of being admitted to a more preferred option. For programs, we focus on comparisons *between* pulled-in and pushed-out students at that program to study the trade-offs induced by adoption of the GPA+ policy.

Student-Level Analysis. We first explore the effect of the policy on the outcomes of exposed students. The treatment is admission priority and operates through the assignment. Pulled-in students are assigned to a higher preference in their rank-ordered list, pushed-out ones to a program they ranked lower. We compare these groups across years to examine the impacts on enrollment and graduation.

Exposed students may be negatively selected, leading us to compare changes over time in these groups rather than levels. We analyze the policy variation in the following specification for student i's outcome in year t,

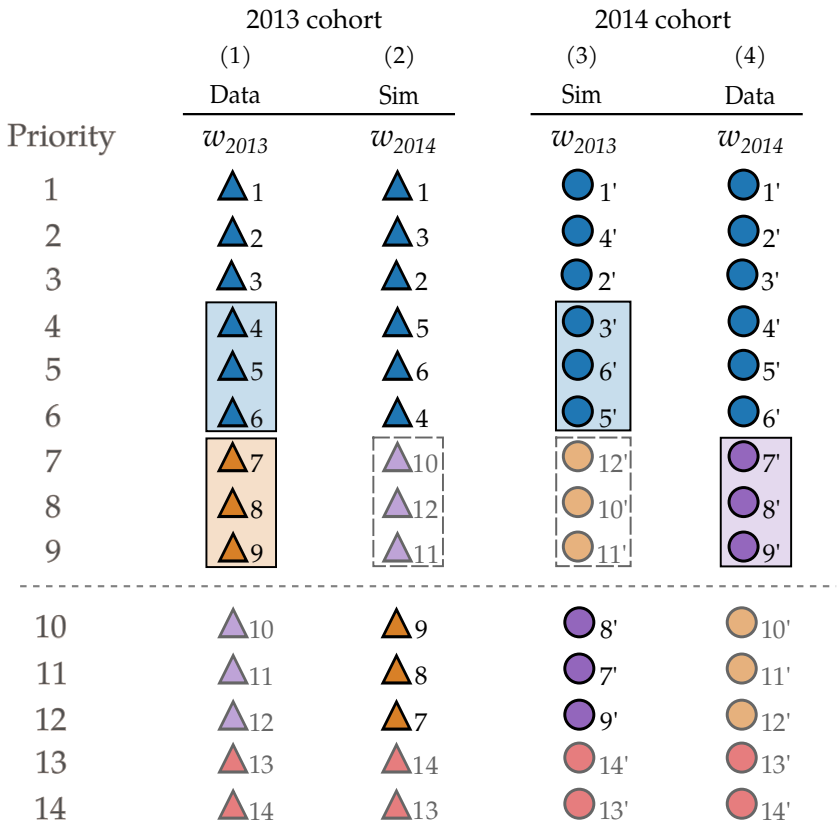
$$y_{it} = \alpha_1 \text{In}_{it} + \alpha_2 \text{Out}_{it} + \pi \, \text{Post}_t + \beta_1 \text{In}_{it} \times \text{Post}_t + \beta_2 \text{Out}_{it} \times \text{Post}_t + X_{it}'\gamma + \delta_{h(i)} + u_{it} \tag{3.1}$$

where In_{it} and Out_{it} indicate being pulled in or pushed out at the student's highest-ranked affected application under the full 2013-versus-2014 rule change, holding applications fixed.[21] Concretely, students with $\text{In}_{it} \times \text{Post}_t = 1$ are admitted to the target program in the data, while those with $\text{Out}_{it} \times \text{Post}_t = 1$ are rejected in the data. The coefficient π on Post_t captures the general time trend in the outcome, that is, the level shift net of observables seen in Figure 8(a). In order to restrict comparisons to observably similar students, we include controls X_{it} for PSU math and language scores, GPA, within-school rank, gender, private high school attendance, and high school fixed effects $\delta_{h(i)}$. Under parallel trends for students with the same observables and high school, β_1 and β_2 are the effects of the policy on the pulled-in and the pushed-out.

Table 2 shows nearly offsetting effects on pulled-in and pushed-out students. Students pulled in by the policy are more likely to enroll and graduate as a result, with gains about the same size as the losses of those pushed out. Students pulled in by the policy are 28.0 percentage points more likely to receive any assignment, while those pushed out are 21.4pp less likely to do so. This gap reflects differences in applications and fallback options between the two groups. The effect is carried over to enrollment, though the loss for pushed-out students (16.6pp) somewhat exceeds the gain for pulled-in students (13.5pp). Pulled-in students are more likely to graduate from any DEMRE program by 1.7pp and from any program they applied to in this year by 2.1pp. In contrast, pushed-out students see almost no change in graduation from any DEMRE program but are 1.8pp less likely to graduate from a program listed in their applications. The gap between

[21] A student pulled into a higher-ranked program is by construction "pulled out" out of a lower-ranked one. We classify pulled/pushed status by the highest-ranked affected program.

Figure 9: Difference-in-Differences Designs: Measuring Exposure



Notes: The figure illustrates our approach to identifying students affected by the GPA+ policy. The schematic depicts the effect of weight choices on the admitted pool into a particular program in the data. Each column corresponds to the considered applicant lists, ranking the same applicants under the 2013 or 2014 weight choices. For each cohort, one of these lists is observed in the data, the other is simulated. Triangles denote the 2013 cohort and circles the 2014 cohort. The dashed line marks the program's cutoff.

Students "pulled into" the target program by the change in admission weights from 2013 to 2014 are shown in purple. Students "pushed out" by the policy are shown in orange. Blue students are always admitted, red always rejected.

In the student difference-in-differences, we focus on the "full equilibrium" effects of all programs shifting their weight choices between 2013 and 2014. We are interested in how outcomes for the pulled-in and pushed-out students evolve due to gaining or losing admission to a program ranked higher in their application list. In the schematic, this design compares outcomes of counterfactually pulled-in students in 2013 (faded purple triangles, assigned to a less-preferred program) to those of pulled-in students in 2014 (solid purple circles, assigned to the more-preferred program). The analogous comparison applies to pushed-out students (solid and faded orange figures). To isolate confounding time-varying effects, we compare these differences to trends in observably similar, yet unaffected students.

In the program difference-in-differences, we focus on the "partial equilibrium" effects of a program's weight choices, holding their competitors' actions fixed. We are interested in how outcomes compare *between* students pulled into and pushed out of the same program. This comparison poses a missing-data problem: we only observe outcomes at that program for students admitted to it, i.e. those assigned under the program's admissions weights in the data (w_{2013} in 2013 and w_{2014} in 2014).

To circumvent this challenge, we compare the outcomes of pushed-out students in 2013 (solid orange triangles) to those of pulled-in students in 2014 (solid purple circles). To account for potential program-level changes between 2013 and 2014, we define control groups by identifying students who are admitted under both sets of admission weights, but are close to the admission cutoff (highlighted blue symbols). To further ensure these control groups are similar across both periods, we choose both of them using the ordering implied by 2013 weights.

these last two results indicates that some pushed-out students opt for reapplying in a later cycle, consistent with the evidence in Larroucau and Rios (2025).

Overall, and in line with Reyes (2022), we find combined effects on graduation that are indistinguishable from zero. The results mirror evidence from similar policies that gains to beneficiaries roughly offset losses to the displaced (Barahona, Dobbin, and Otero 2026) and that beneficiaries show little sign of mismatch (Black, Denning, and Rothstein 2023), in contrast to findings for STEM fields at selective U.S. universities (Arcidiacono, Aucejo, and Hotz 2016).

Table 2: Student-Level Pushed/Pulled Analysis

	(1) Assigned	(2) Enroll in Any App	(3) Grad from Any DEMRE	(4) Grad from Any App
Pulled In	-0.161***	-0.079***	-0.030***	-0.033***
	(0.006)	(0.006)	(0.006)	(0.006)
Pushed Out	0.109***	0.085***	-0.004	-0.004
	(0.003)	(0.006)	(0.006)	(0.007)
Post	0.005***	0.023***	0.018***	0.017***
	(0.002)	(0.002)	(0.002)	(0.002)
Post × Pulled In	0.280***	0.135***	0.017**	0.021***
	(0.007)	(0.008)	(0.008)	(0.008)
Post × Pushed Out	-0.214***	-0.166***	-0.008	-0.018**
	(0.006)	(0.008)	(0.009)	(0.009)
Combined: In+Out	0.066***	-0.032***	0.010	0.003
	(0.009)	(0.012)	(0.012)	(0.012)
Outcome Mean	0.890	0.718	0.480	0.431
R^2	0.096	0.138	0.155	0.139
N (Students)	213,477	213,477	213,477	213,477
N (High Schools)	3,122	3,122	3,122	3,122

Notes: Student-level DiD across the 2013 and 2014 cycles. Pulled In (Pushed Out) students are admitted (rejected) under the 2014 rules but rejected (admitted) under the 2013 rules, based on their highest-ranked affected application using the full change in all programs' rules with applications held fixed. All other applicants in the estimation sample serve as controls. Regressions include high school fixed effects and controls for PSU math and language scores, GPA, within-school rank, gender, and private high school dummies. SEs clustered at the high school level.

Program-Level Analysis. The previous analysis measures impacts of the policy on students relative to their counterfactual outcome, i.e., their next-best alternative. This comparison reflects aggregate trade-offs from the policy given the observed level of adoption, but it does not reflect an *individual program's* incentives when choosing its weights. Programs are likely to care about the quality of the students they themselves admit, in absolute terms, and not necessarily the students' relative gains from assignments.

To get at the program's tradeoffs, we analyze the impact of the policy on the types of students a program receives. Within program, we compare the admitted students exposed to the policy in each year: those pushed out under the 2013 rules and those pulled in under the 2014 rules. In

order to account for trends in graduation at the program level, we also include an equal number of marginal, always-admitted students in both years.[22] We analyze this variation with the following specification for the outcome of student i admitted to program j in year t,

$$y_{itj} = \alpha \text{Exposed}_{itj} + \beta \text{Exposed}_{itj} \times \text{Post}_t + \delta_{tj} + u_{itj}, \tag{3.2}$$

where Exposed_{itj} is an indicator for being an exposed (pulled-in/pushed-out) rather than a comparison student. δ_{tj} are program-year fixed effects that capture program-level trends in outcomes.[23] The coefficient of interest is β on $\text{Exposed}_{itj} \times \text{Post}_t$; it measures differences in outcomes for the pulled-in students in 2014 relative to the pushed-out students in 2013 that cannot be explained by program-level trends in the matched comparison group.

Table 3 shows the marginal trade-offs involved in adopting the GPA+ policy. Students pulled in by a program's weight choices are 5.4pp less likely to enroll and 4.2pp less likely to graduate from that program than those pushed out. Conditional on enrollment, we reject graduation gains above 1pp and losses beyond 6pp, suggesting students pulled in are not stronger than those they replaced. Notably, demographic and score controls attenuate estimates, indicating that observables explain much of the gap in enrollment and graduation.

We also find heterogeneous effects across programs' adoption levels. Columns (5) to (8) of Table 3 focus on high GPA+ adopters, showing more negative impacts for programs reaching deeper into the pool of applicants. Pulled-in students at these programs are 5.9pp less likely to enroll and 6.7pp less likely to graduate than those pushed out. Conditional on enrollment, pulled-in students graduate at a 5.5pp lower rate, an effect that survives the addition of controls (5.1pp).

Discussion Discretion in Chile's relative rank policy strongly impacted student composition across programs. The effect is driven by changes in admission priorities rather than application responses or spillovers from other programs. Increases in low-SES and female students do not seem to have come at a great cost to aggregate completion, and pulled-in students do not fare worse when compared to their pushed-out counterparts. However, pulled-in students appear to be weaker relative to a program's counterfactual admitted student, a pattern that is starker at the

[22] We define exposure here by switching the target program's own rule between years while holding competitors' rules fixed. Specifically, at each program-year, we count the number of exposed admitted students. We then rank always-admitted students by priority under the 2013 index, which is rule-invariant and so keeps the comparison group's characteristics balanced over time. The comparison group is the same number of always-admitted students with the lowest priority in each program-year.

[23] We include these to capture the potential effects of additional student supports or changes in academic standards that shift outcomes for all students in a program across years. In some specifications we also add student controls.

Table 3: Program-Level Difference-in-Differences Analysis

	All				High GPA+ Only			
	(1) Enroll	(2) Enroll	(3) Graduate	(4) Graduate	(5) Enroll	(6) Enroll	(7) Graduate	(8) Graduate
A. Unconditional								
Exposed	0.030***	-0.027**	-0.003	-0.018	0.041***	-0.016	0.008	-0.013
	(0.009)	(0.011)	(0.011)	(0.013)	(0.012)	(0.014)	(0.013)	(0.017)
Exposed × Post	-0.054***	0.017	-0.042***	-0.014	-0.059***	0.013	-0.067***	-0.032
	(0.015)	(0.016)	(0.015)	(0.017)	(0.019)	(0.022)	(0.019)	(0.024)
Outcome Mean	0.778	0.778	0.422	0.422	0.779	0.779	0.440	0.440
N (Students)	17,741	17,741	17,741	17,741	11,063	11,063	11,063	11,063
N (Programs)	483	483	483	483	216	216	216	216
B. Conditional on Enrollment								
Exposed			-0.022*	-0.009			-0.013	-0.007
			(0.012)	(0.015)			(0.014)	(0.019)
Exposed × Post			-0.025	-0.027			-0.055**	-0.051**
			(0.018)	(0.020)			(0.022)	(0.025)
Outcome Mean			0.541	0.541			0.566	0.566
N (Students)			13,811	13,811			8,616	8,616
N (Programs)			482	482			216	216
Add Controls		X		X		X		X

Notes: Sample is students admitted to selective programs in 2013 and 2014, who are exposed to the policy (admission outcome flips in the counterfactual year's rules) and a matched number of marginal always-admitted students. Programs are restricted to those having both exposed and comparison students in both years. Panel B conditions on enrollment in the admitted program. Columns (5) to (8) restrict to high-GPA+ programs. All regressions include program-by-year fixed effects. "Add Controls" adds gender, low-SES status, PSU, GPA, within-school rank, and high school mean PSU. SEs clustered at the program level.

higher GPA+ programs. In contrast, many other programs appear to leave increases in diversity on the table.

Overall, the patterns are consistent with programs facing a tradeoff between diversity and achievement. However, our estimates are specific to programs that respond to the policy by increasing their GPA+ weight. The results are therefore silent on what would have happened under alternate admission designs, including for programs that adopted the minimum required weight on the policy.

4 Student Application and Graduation Model

Answering what would have happened under alternate rules requires forecasting outcomes deeper into each program's applicant pool. To this end, in this section, we propose a model of students' application choices and graduation outcomes. The student model serves two purposes. First, it allows us to predict graduation at counterfactual assignments. This enables a decomposition of student outcomes into selection and heterogeneous causal effects, in the spirit of Heckman, Humphries, and Veramendi (2018) and Abdulkadiroğlu et al. (2020), quantifying the role of match. Second, the model allows us to forecast how students would reallocate when the programs avail-

able to them change under alternate admissions rules.

The central challenge is that we observe graduation for only one program at a time. Our framework reduces the dimensionality of the forecasting problem while incorporating rich interactions between student and program characteristics, including selection on unobserved gains. While this empirical approach involves extrapolation, it is disciplined by quasi-experimental variation and validated out of sample.

Conceptual Framework We begin by outlining the intuition behind our outcome decomposition, which allows us to quantify the role of student-program match. This illustrates the stakes of allowing for college discretion in admissions. As in Abdulkadiroğlu et al. (2020), student i's outcome at program j can be decomposed into student ability, program average effects, and match-specific factors, using the following accounting identity:

$$\Pr(\text{Grad})_{ij} = \underbrace{A_i}_{\text{Ability / Selection}} + \underbrace{ATE_j}_{\text{Program Average Effect}} + \underbrace{M_{ij}}_{\text{Match Effect}} . \tag{4.1}$$

Given knowledge of graduation probabilities at each program, the terms are computed as:

$$A_i = \frac{1}{|\mathcal{J}_t|} \sum_{j \in \mathcal{J}_t} \Pr(\text{Grad})_{ij} \tag{4.2}$$

$$ATE_j = \frac{1}{N_t} \sum_{i \in t} \Big(\Pr(\text{Grad})_{ij} - A_i \Big) \tag{4.3}$$

$$M_{ij} = \Pr(\text{Grad})_{ij} - A_i - ATE_j \tag{4.4}$$

The ability term A_i describes how likely a student i is to graduate from a program assigned at random, capturing classical selection bias (Griliches 1977). ATE_j is the average causal effect at a program j: how much program j is expected to affect graduation for the average student in the population. This component reflects a mix of program effectiveness, teaching practices, and academic standards, which are held fixed under counterfactual assignments. Finally, M_{ij} captures *match effects* or comparative advantage in graduation outcomes—how much the causal effect of program j differs across students i.

The role of match effects is key for assessing the importance of college discretion. If match is important, then colleges play an important role in rationing seats to students who stand to gain the most. Otherwise, college admissions are a zero-sum game, and there are only winners and losers with no efficiency gain. In our empirical treatment below, the match component is left generically unrestricted.

Empirical Framework Students are characterized by their observable covariates X_i, type q_i, and location loc_i. We model students using the 2012 to 2014 application cycles, denoted t, often left implicit. These observables X_i are indicators for being low SES, female, or from a large metro area (Santiago, Valparaíso, or Concepción), math and verbal PSU scores, high school GPA, within-high-school GPA percentile, high school average PSU score, and the baseline share of a student's high school enrolling in a DEMRE college. Based on their scores, students are limited in the feasible set of options to which they can apply, Ω_i, discussed more below.

Programs j are characterized by their college-area $g(j)$, location of their campus loc_j, and a vector of observable characteristics W_j. These program covariates W_j include indicators for being a private university, a Catholic university, indicators for broad area of study (business/law, art/humanities/social sciences, education, health; technology/science is excluded and absorbed in college-area main effects), program length in number of semesters, and average PSU, female share, and low-SES share among enrolled students in the pre-period (2010–2011). These compositional covariates capture time-invariant program features which are held fixed in counterfactuals. We are able to fit time-varying patterns related to GPA+ with these time-invariant controls.[24]

Our implementation allows for flexible interactions between these student and program characteristics. In particular, we model unobservable selection through a flexible mixture of *latent types*, each having exhaustive interactions with program covariates W_j. Discrete types can be viewed as an approximation to continuous unobserved heterogeneity (Bonhomme, Lamadon, and Manresa 2022). The key assumption underlying this framework is that a type q will capture *all* unobserved dependence between the choice and outcome equations. In other words, once we know an individual's type q, their observed choices are no longer informative about their outcomes. Of course, we do not observe an individual's type ex ante, and choices are informative of the latent type.

Type Priors. Prior to observing any endogenous outcomes or choices, the probability of student i with characteristics X_i being of type q is:

$$\Pi_q(X_i) = \frac{\exp\{\pi_0^q + X_i'\pi_X^q\}}{1 + \sum_{r=2}^{Q} \exp\{\pi_0^r + X_i'\pi_X^r\}}. \tag{4.5}$$

The type-specific intercepts π_0^q capture differences in the population shares of each type. The parameters π_X^q allow for unobservables (types) to flexibly correlate with observable student characteristics while preserving computational simplicity.[25] The coefficients for type $q = 1$ are nor-

[24] This leads us not to incorporate endogenous peer effects (e.g. Bordon and Fu 2015; Cheng and Weingarten 2026) or academic standards. If these were first order, we would expect the model to require some time-varying controls to fit the data.

[25] This specification relaxes the typical assumption of independence between observable and unobservable characteristics. In all equations, we also control for main effects in X_i, which are identified separately from the unobservables as long

malized to zero, as we can only identify relative differences in latent probabilities.

Student Utility. When applying to programs in cycle t, students can rank any option in their feasible set, a subset of all options in the year, $j \in \Omega_i \subset J_{t(i)}$. Following Kapor, Karnani, and Neilson (2024), we define a program j as (ex-ante) feasible if student i's program-specific admission index is near the realized cutoff. We assume applications within this "credible" bandwidth are truthfully ranked. We also exclude applications listed after a clear safety as not credible. This approach relaxes the assumption of full truth-telling and generalizes the stability-based approach of Fack, Grenet, and He (2019) to include more information from applications. This allows us to uncover students' true preferences without microfounding a mapping to observed portfolios, which can potentially be strategic application choices (e.g., Agarwal and Somaini 2018; Calsamiglia, Fu, and Güell 2020; Larroucau and Rios 2020). The key assumption is that the assignments from DA are stable, which is sufficient for counterfactual assignments. We show below that we fit sorting patterns in the data with this assumption, even in a year held out of estimation. See Appendix B.1 for details.

Student i of type q's utility from ranking program j is given by

$$U_{ijq} = \delta_{g(j)} + \mu_{iq} + W_j'\gamma_{iq}^W - \lambda(loc_i, loc_j) + \epsilon_{ijq}, \tag{4.6}$$

where $\delta_{g(j)}$ is the mean utility of programs in college-by-area group $g(j)$, one of 246 such groups, and ϵ_{ijq} is an EV(I) error.[26] The term μ_{iq} captures shifts in student utility for any on-platform option. This mean utility term includes intercepts for each student type q and year t, as well as linear, quadratic, and cross-interaction terms in the covariates X_i:[27]

$$\mu_{iq} = \mu_t + \mu_q + \sum_{k}^{|X_i|} \mu_k^X \cdot X_i^{(k)} + \sum_{k_1}^{|X_i|} \sum_{k_2 \geq k_1}^{|X_i|} \mu_{k_1,k_2}^{XX} \cdot X_i^{(k_1)} \cdot X_i^{(k_2)}. \tag{4.7}$$

as there is no collinearity between types and observables (e.g. type 1 is all men). For example, we could estimate our model conditional on covariate strata $X_i = x$ and all identification arguments would go through.

[26] We include indicators for 246 college-by-areas in both the choice and outcome equations. We use group effects rather than program fixed effects due to identification concerns related to sample size at a few small programs. In addition, we combine these effects among some small areas within universities to account for programs with few enrollments: university-by-area cells with fewer than 20 enrollments, 20 assignments, or 5 graduations in the estimation sample are pooled with a closely related area at the same university. As we include the constant in μ_i and control for private, Catholic, and field of study dummies in W_j, we normalize the following coefficients in $\delta_{g(j)}$ to zero: all programs from Universidad de Chile, Technology programs at Pontificia Universidad Católica de Chile, and Technology programs at Universidad de Concepción.

[27] Specifically, there are 9 linear effects, one for each element of X_i; 6 quadratics in the continuous X_i; and 36 interaction terms. Squares are included only for the continuous covariates, since $X^2 = X$ for the binary indicators. Year effects exclude 2014.

Individual tastes for program covariates l in W_j are given by

$$\gamma^W_{iq,l} = \gamma^W_{q,l} + \sum_{k=1}^{|X_i|} \gamma^W_{l,k} \cdot X_i^{(k)}, \tag{4.8}$$

so that each program covariate is interacted with a type-specific intercept $\gamma^W_{q,l}$ and with all covariates in X_i. The sensitivity to distance $\lambda(loc_i, loc_j)$ changes the *effective* choice set of an individual, in a similar way to admission cutoffs, and it plays a related role in identification. The disutility from distance is parametrized as

$$\lambda(loc_i, loc_j) = \lambda^{\text{Dist}} \cdot \text{Dist}_{ij} + \lambda^{\text{Out}} \cdot \text{Dist}_{ij} \cdot \left[1 - \text{InRegion}_{ij}\right], \tag{4.9}$$

where we allow different slopes based on whether the student is located in the same or a different region as the program.[28]

Graduation Outcomes. Graduation outcomes follow an identical functional form, with no exclusions other than distance. Student i of type q's graduation probability at program j is specified as

$$\Pr(\text{Grad})_{ijq} = \Lambda\left(\alpha_{g(j)} + \varphi_{iq} + W_j'\tau^W_{iq}\right), \tag{4.10}$$

where $\Lambda()$ is the logistic function, $\Lambda(x) = \frac{e^x}{1+e^x}$. As above, $\alpha_{g(j)}$ are 246 college-area fixed effects capturing different program-level graduation rates net of student composition. φ_{iq} captures the propensity for student i to graduate across all programs, while τ^W_{iq} captures how much graduation depends on interactions between student and program characteristics. In terms of functional forms, each has a direct counterpart in the choice model and is parametrized using the same variables: $\alpha_{g(j)}$ mirrors $\delta_{g(j)}$, φ_{iq} mirrors μ_{iq}, and τ^W_{iq} mirrors γ^W_{iq}. The utility and outcome indices are thus allowed to be arbitrarily correlated and do not impose behavioral assumptions like graduation-maximization or rational expectations. The fact that we measure selection on gains below is an empirical result.

Estimation Procedure The upshot of our structure is that, given a guess of parameters and observed data, we can use the likelihood structure to compute the posterior probability that a student is each type q using Bayes' rule. We use this property to jointly estimate the application, graduation, and prior parameters using the Expectation-Maximization (EM) algorithm to iteratively maximize the likelihood (Dempster, Laird, and Rubin 1977; Arcidiacono and Jones 2003), which includes the distribution of unobserved types. This procedure works by iterating between up-

[28] As this is defined at the comuna-level, programs and students in the same comuna have a distance of 0.

dating the current posterior probability of each individual's latent type, given current parameters and observed outcomes, and then sequentially optimizing each component of the likelihood. We provide additional details on the estimation routine in Appendix B.1.

Identification Our context and administrative data allow us to attain identification across a variety of margins. Identification of the framework implicitly draws on the same variation as several quasi-experimental designs: controlling for selection using application portfolios (Dale and Krueger 2002; Abdulkadiroğlu et al. 2020), admission cutoffs from centralized assignment (Hastings, Neilson, and Zimmerman 2013; Kirkeboen, Leuven, and Mogstad 2016), distance-based variation in access (Card 1995; Walters 2018; Mountjoy 2022), and shifts in access from the GPA+ policy itself (Reyes 2022; Black, Denning, and Rothstein 2023).

Combining these sources makes each more credible than it would be alone. For example, designs relying on application fixed effects (as in Dale and Krueger 2002) assume that students with the same choice set end up in different programs for idiosyncratic reasons unrelated to their potential outcomes. Variation in admissions cutoffs allows us to weaken this assumption by providing exogenous within-portfolio shifts in assigned options. Our revealed preference approach to student utility also informs what these applications reveal about outcomes.

While the assignment process generates sharp variation in these choice sets, the distance exclusion[29] can be thought of as generating additional smoother variation in *effective* choice sets, shifting otherwise similar students into different applications and consequently cutoff margins.[30] Finally, cutoffs and the GPA+ variation only generate local variation. Being able to observe students' counterfactual assignments through rank-ordered lists and including distance as a shifter informs extrapolations away from this marginal variation.

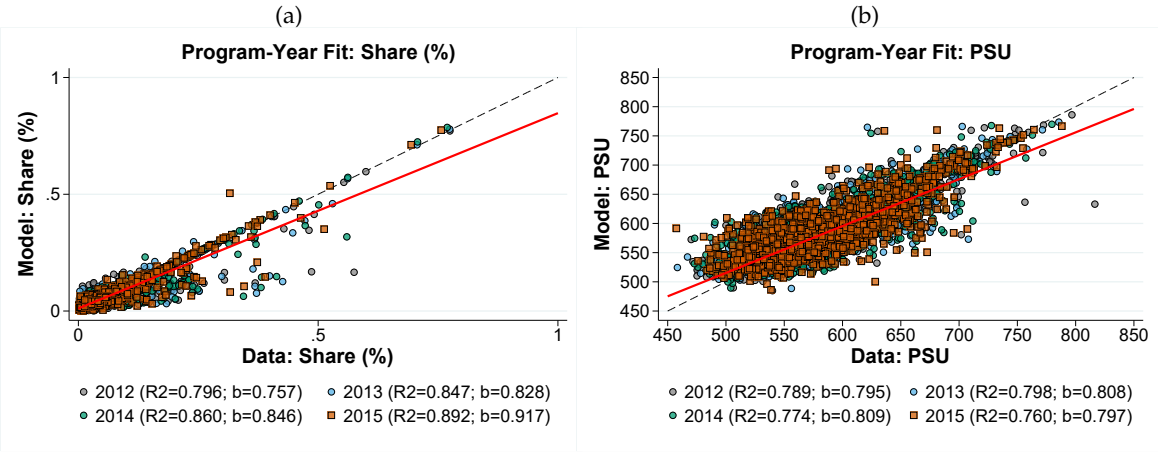
Fit and Validation Appendix Figure A5 provides extensive documentation of the model's fit, and we focus here on key patterns. First, Figure 10 shows that the model fits both student demand and sorting patterns down to the program level, rerunning the DA mechanism after drawing new student preferences at estimated parameters. This test asks the model to jointly fit all students'

[29] The argument here is identification at infinity (Heckman 1990; French and Taber 2011): a student will not attend an option no matter how desirable if they are far away enough and will be drawn into less desirable options located nearby. Concretely, a latent "STEM" type will enroll in an art program if they live close enough to it, identifying the graduation probability of a STEM type in an art program. In practice, the empirical model applies a low-dimensional approximation to this nonparametric argument, inferring gains from marginal variation in the excluded variable. See Carneiro, Heckman, and Vytlacil (2011) and Mountjoy (2022) for applications.

[30] A criticism of the distance instrument is that individuals of different latent abilities will locate near or far from options they desire, or colleges will open near families with a high propensity to send their children to college. We partially address this concern by including indicators for metro area alongside other rich observable controls, including characteristics of the high school, which directly capture any ability-distance sorting. What matters for identification is the *differential distance* between program types. We could in principle include comuna fixed effects and do not do so for computational simplicity.

preferences well enough to replicate the sorting equilibrium in the data. The model fits assignment shares, shown in Figure 10(a), as well as average PSU scores, shown in Figure 10(b). Finally, both patterns fit similarly well for 2015 (orange squares), which is held out of estimation, reassuring us of the model's ability to extrapolate to new admissions rules.

Figure 10: Fit of Student Demand Model: Full Simulation of Program-Level Shares and Means



Notes: Compares program-level assignment shares and average PSU scores in the data against the model. Each point is a program-year. Data along the x-axis are observed shares of all assignments and mean PSU scores among assigned students. Model counterparts are computed by running DA using each student's complete simulated ranking of programs, drawing new types (from the posterior) and preference shocks. The dashed black line is the 45-degree line and the solid red line is a seat-weighted OLS fit. The model is estimated on 2012 to 2014; 2015 points (squares) are out of sample.

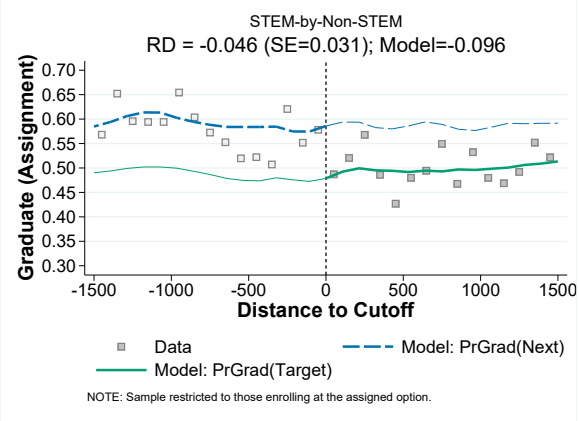
Second, we illustrate in a simplified example how information around admission cutoffs can help identify match effects. We show that marginal students likely differ in their returns to different options and that the model captures much of this variation. Figure 11 compares enrolling students at the margin of admission to STEM with a non-STEM fallback option (panel a) to those at the margin of a non-STEM admission with a STEM fallback option (panel b). Assuming an inability to precisely manipulate one's position relative to the realized cutoff, assignments are quasi-random and yield estimates of the causal effect on graduation. Stratifying by target and next area captures comparative advantage (Kirkeboen, Leuven, and Mogstad 2016): admission effects may change depending on which option is ranked first or the ordering of the options. In both cases, assignment to a STEM program lowers graduation by about 5-10pp. Under the assumption of homogeneous treatment effects (i.e., no match effects), the two discontinuities would be equal in magnitude and opposite in sign. Instead, the effects are not symmetric — STEM lowers graduation when it is ranked first (by 4.6pp) while falling back into STEM lowers graduation by 9.7pp. The patterns suggest that students who rank STEM first are positively selected on gains as well as levels.[31] We see this shift in levels clearly, as those marginally admitted to STEM with a non-STEM

[31]The null hypothesis of the difference being zero cannot be rejected at the size of the standard errors. We view this

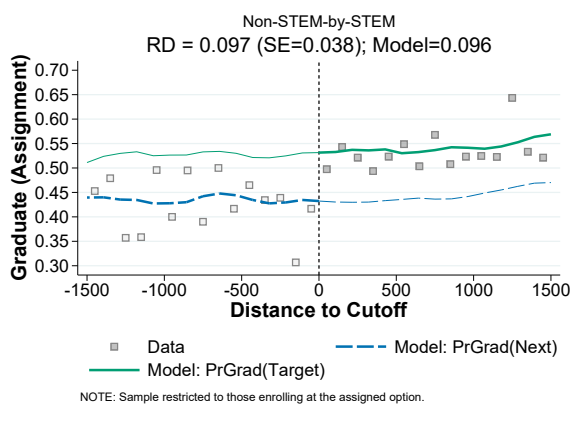
fallback have graduation rates around 50 percent, compared to about 45 percent for those who fall back into STEM from a non-STEM target. The model captures these levels and closely matches the discontinuity in panel (b) (+9.6pp versus +9.7pp), while it overstates the decline in panel (a) (−9.6pp versus −4.6pp). The lines in Figure 11 also show how the model extrapolates away from the cutoff, for example, by drawing out the counterfactual graduation for those rejected from the target had they been assigned (solid green below the cutoff).

Figure 11: Fit of Graduation around Admissions Cutoffs

(a) STEM target, non-STEM next option



(b) Non-STEM target, STEM next option



Notes: Regression discontinuity of graduation at the assigned program on the index distance to the target program's admission cutoff, among students who enroll at their assigned option. Squares are data in 100-point bins. The solid line is model-predicted graduation at the target program while the dashed line is model-predicted graduation at the next-best option. The estimated model and data RD effects (conditional on enrollees) are reported in the subtitle. Panel (a) shows applicants whose target is STEM and next option non-STEM, panel (b) the reverse.

Third, we assess the model's ability to fit variation around admission cutoffs and that related to the GPA+ policy. All told, the model's fit to this quasi-experimental variation, which shifts otherwise similar students into different programs, gives us confidence in using the model to explore counterfactual admissions that go beyond the observed data.[32] Figure 12(a) compares regression discontinuity estimates both in the data and produced by the model, stratifying by broad areas of the target and next available options, among those enrolling in their assigned program and having a next feasible option.[33] The model captures this variation well — we cannot reject that the slope of the line of best fit is one, indicating the model is forecast unbiased for these cutoff estimates in the spirit of Angrist et al. (2017). Figure 12(b) examines the GPA+ policy variation, showing graduation by year and 2014 GPA+ adoption group. The model understates

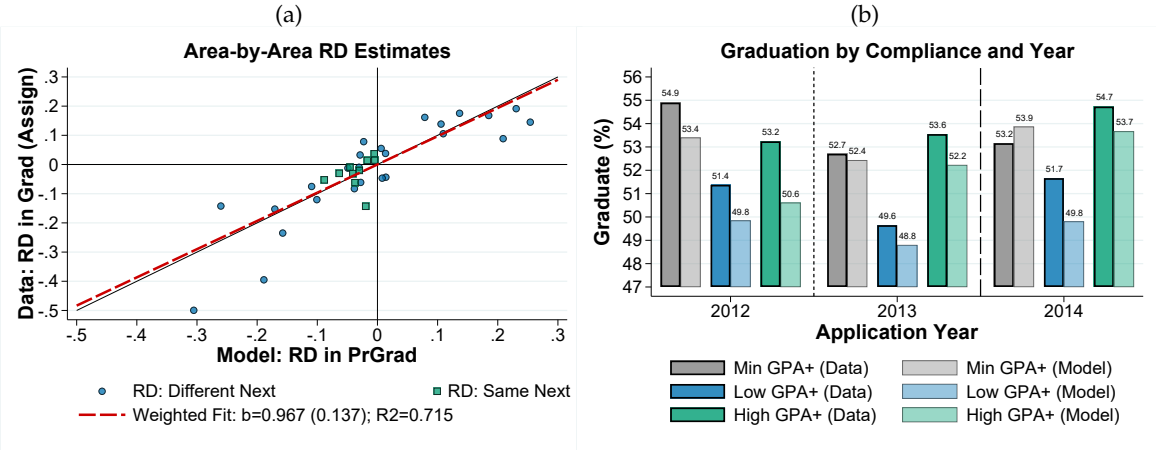
evidence as illustrative and suggestive of the mechanism, with a precise quantification incorporating information from a variety of cutoffs being done by the model.

[32] As we use maximum-likelihood estimation, this variation is implicitly used but not directly targeted.

[33] The estimates displayed in Figure 12 are conditional on enrollment. As such, they are not well-defined causal parameters, since they combine quasi-experimental shifts in assignment around the cutoff as well as enrollment bunching from enrolling more in a higher ranked option. However, this comparison remains a strong test of the model regardless as we need the model to capture both the causal and bunching impacts on identical samples around the cutoff.

the level of graduation for high adopters (by 2.6pp in 2012) but captures changes around the adoption of discretion in 2014. Graduation rates at high adopters rose between 2013 and 2014 by 1.5pp in the model and 1.1pp in the data.

Figure 12: Fit of Student Outcome Model: Quasi-experimental Variation



Notes: Shows the fit of the model around quasi-experimental variation. Panel (a) compares regression discontinuity estimates at admission cutoffs in the data and the model. Each point is a target-area × next-area estimate, pooling each student's first marginal application within 1,500 index points of the cutoff, among students with an admissible next option who enroll at their assigned program. Estimates are restricted to cells with more than 100 admitted and 100 rejected students. The data and model estimates are discontinuities in graduation (or predicted graduation) at the assigned program, from a linear specification on each side of the cutoff with standard errors clustered by program-year. Squares are cells where the target and next option share an area. The solid black line is the 45-degree line and the red dashed line is a linear fit weighted by the inverse variance of the data estimates. Panel (b) compares graduation rates among enrollees in the data and the model by application year, stratifying by programs' 2014 GPA+ weight choice, for programs with enrollment in each year from 2012 to 2014.

Quantifying the Role of Match Effects We now turn to examining the role of match effects in determining student outcomes. Following our conceptual framework, Table 4 decomposes variation in predicted outcomes into ability, program, and match effects, both across students and program-level averages. The "ensemble" decomposition shows the share of the predicted variance explained by each component, while the full decomposition includes covariance terms that measure sorting across components.[34] We use the bootstrap to bias correct these variance components to account for the influence of sampling error.

Focusing on the student ensemble decomposition, the first takeaway is that idiosyncratic shocks explain most graduation outcomes. The R^2 of the model is 0.16. Individual and program components explain most of the variation in predicted graduation, with ability accounting for 49 percent of the variation and program average effects for another 30 percent. The remaining

[34]Specifically, for the share of $\hat{Y}$ that is explained by X, the ensemble decomposition reports $\text{Cov}(\hat{Y}, X)$ and the full decomposition reports $\text{Var}(X)$. Both are converted into shares by dividing by $\text{Var}(\hat{Y})$. Suppose that $\hat{Y} = X + Z$. This is a valid decomposition as $\text{Var}(\hat{Y}) = \text{Cov}(\hat{Y}, X + Z) = \text{Cov}(\hat{Y}, X) + \text{Cov}(\hat{Y}, Z) = \text{Var}(X) + \text{Var}(Z) + 2\,\text{Cov}(X, Z)$. This shows that the ensemble decomposition ignores sorting by dividing the covariance between components into each main component equally.

variation is explained by match effects, about 21 percent. Three-quarters of the match component is explained by observable characteristics, with unobserved components determining the rest. The full decomposition shows negative sorting between ability and program effects: higher ability students enroll at lower-ATE programs. A likely explanation for this pattern is differences in program difficulty.

Turning to the decomposition at the program-level, our model explains around 68 percent of raw differences across programs. This is not surprising, as averaging smooths out residual idiosyncratic shocks. Program average causal effects explain 45 percent of differences across programs, while student quality (average ability) accounts for 34 percent. Observable match accounts for 16 percent while unobserved match accounts for 5 percent. The full decomposition shows similar sorting patterns reflected in the covariance between these components — good students tend to go to programs that lower graduation rates on average, but also sort towards programs where they have higher match.

Table 4: Variance Decomposition of Graduation

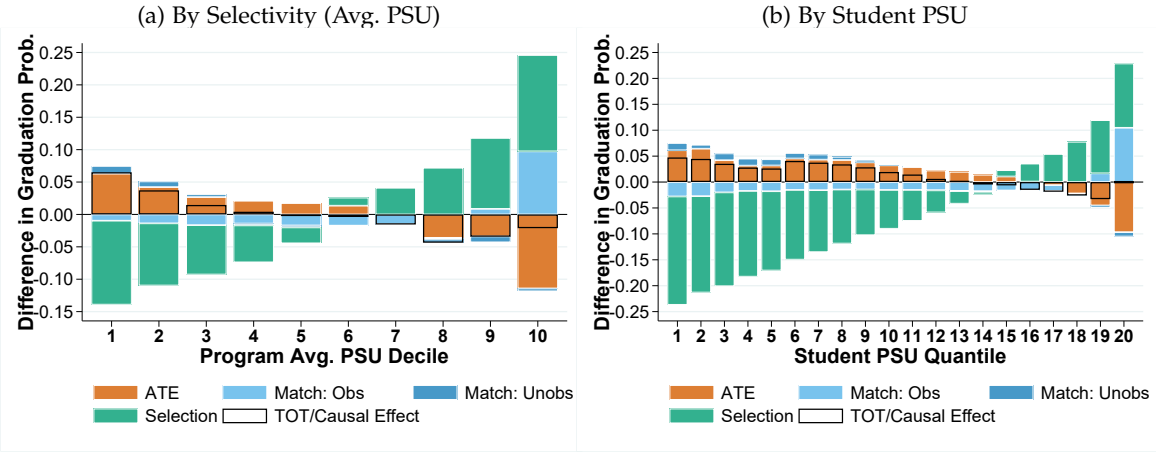
	A. Student				*B. Program*			
	Ensemble		*Full*		*Ensemble*		*Full*	
	Var	Share	Var	Share	Var	Share	Var	Share
Variance in Predicted Graduation	0.040		0.040		0.027		0.027	
	(0.000)		(0.000)		(0.000)		(0.000)	
Variance of Ability (Selection)	0.020	0.492	0.021	0.527	0.009	0.336	0.010	0.379
	(0.001)	(0.012)	(0.001)	(0.012)	(0.000)	(0.018)	(0.000)	(0.015)
Variance of ATE	0.012	0.302	0.015	0.381	0.012	0.452	0.015	0.570
	(0.000)	(0.011)	(0.001)	(0.018)	(0.000)	(0.016)	(0.001)	(0.027)
Variance of Observable Match	0.006	0.153	0.006	0.139	0.004	0.163	0.004	0.140
	(0.000)	(0.009)	(0.000)	(0.012)	(0.000)	(0.013)	(0.000)	(0.015)
Variance of Unobservable Match	0.002	0.053	0.002	0.060	0.001	0.050	0.001	0.040
	(0.000)	(0.010)	(0.000)	(0.008)	(0.000)	(0.015)	(0.000)	(0.007)
2Cov(Ability,ATE)			-0.005	-0.132			-0.005	-0.198
			(0.001)	(0.024)			(0.001)	(0.037)
2Cov(Ability,Observable Match)			0.004	0.099			0.004	0.137
			(0.001)	(0.015)			(0.001)	(0.024)
2Cov(Ability,Unobservable Match)			-0.001	-0.036			-0.001	-0.027
			(0.001)	(0.017)			(0.000)	(0.015)
2Cov(ATE,Observable Match)			-0.002	-0.059			-0.002	-0.088
			(0.001)	(0.018)			(0.001)	(0.027)
2Cov(ATE,Unobservable Match)			0.001	0.033			0.001	0.049
			(0.001)	(0.014)			(0.001)	(0.021)
2Cov(Observable Match,Unobservable Match)			-0.000	-0.011			-0.000	-0.003
			(0.000)	(0.006)			(0.000)	(0.009)
Variance in Data	0.249		0.249		0.040		0.040	

Notes: Decomposition of model-predicted graduation of enrollees into ability, program effect, and observable and unobservable match, at the student level (A) and averaged at the program level (B). "Ensemble" columns report each component's covariance with graduation and its share of the variance. "Full" columns report the component variances and cross-covariance terms (which capture sorting). Computations use posteriors inferred from conditioning on the ROL only. Components are bias corrected and standard errors are computed using 100 bootstrap draws of student model parameters.

Figure 13 shows these match components are at least partially related to selectivity, especially at the most elite programs. Figure 13(a) shows that selection by student ability explains much of the observed difference in graduation rates by program selectivity (program average PSU). Very

selective programs admit students who, on average, graduate more from *every* program. The model indicates that selective programs are also associated with declines in graduation rates for the average student — ATEs in the top decile programs lower graduation rates by a little more than 10 percentage points. While we cannot offer a conclusive answer, one hypothesis is that the model is capturing the difficulty of coursework at top programs. In addition, these programs are also characterized by sorting on match effects. This dimension of selection largely offsets the negative effects for the average admitted student, resulting in a treatment on the treated close to zero.[35] Similar patterns emerge from student-level test scores in Panel (b).

Figure 13: Program-Level Decomposition of Predicted Graduation into Components

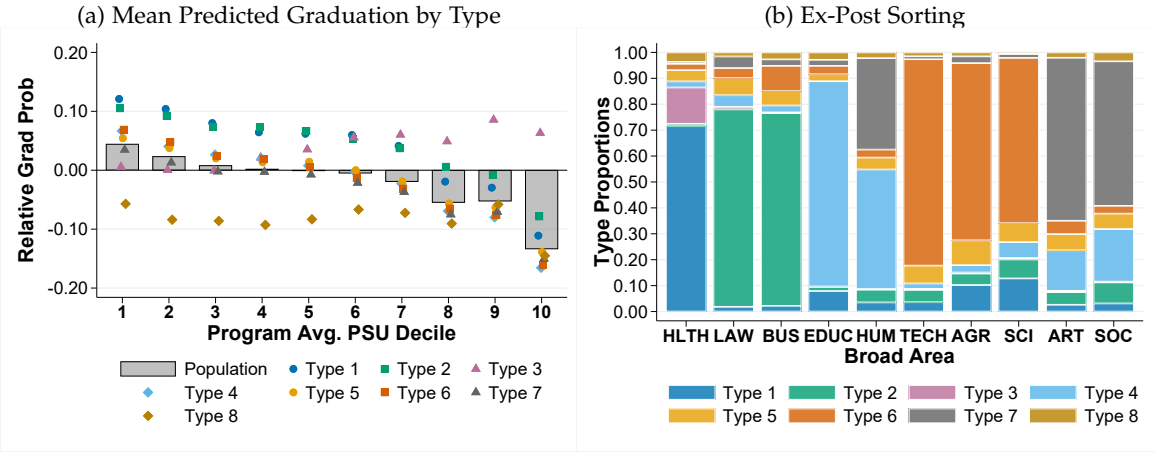


Notes: Each program's deviation of predicted graduation from the enrollment-weighted population mean, split into ATE, observable match, unobservable match, and selection bias (demeaned ability A). The outlined bar is the total causal effect on treated students (ATE plus average match). Panel (a) bins programs by enrollment-weighted average PSU decile. Panel (b) bins students by ventiles of the student-level PSU distribution among applicants.

Figure 14 shows that the unobserved types differ both in their average graduation rates and in the effect of selectivity. Figure 14(a) examines type-specific causal effects by program selectivity deciles (average PSU), normalized relative to average ability. Type labeling is sorted in order of declining ability. Most types, including the highest ability, see lower graduation probabilities and vary in their relative ranking by program selectivity. Mirroring the previous results, selective programs have negative average effects — the typical student would see reduced graduation rates compared to the average program. However, types differ as we go to higher selectivity deciles; in particular, types 3 and 8 are more resilient to the decline associated with more selective programs. Figure 14(b) shows that the latent types strongly capture sorting across fields. For example, Type 1, the highest average ability, tends to sort strongly into health programs.

[35] This finding mirrors the results in Zimmerman (2019), who shows that only higher-SES students benefit from elite degrees in terms of earnings gains.

Figure 14: Predicted Graduation by Latent Student Type: Selectivity Decile



Notes: Panel (a) shows type-specific average treatment effects on graduation, averaged across the programs in each selectivity decile weighting by observed enrollment and expressed relative to the population mean. The gray bars display the population average effect. Panel (b) shows the composition of enrollees in each broad area of study implied by the posteriors (conditioning only on ROLs).

Connecting the Structural and Reduced-Form Frameworks Table 5 addresses the relationship between these model components and the research designs from Section 3 by using the model components as outcomes in our program-level difference-in-differences (DiD) design. Conditioning on enrollment, the model predicts a zero effect on graduation, not statistically different from the DiD estimate (−2.5pp; SE=1.8). However, the effect is due to composition — adopting programs pull in higher-ability students with slightly worse match effects at these programs. At high GPA+ programs, the model predicts a decline in graduation (−1.7pp), again not statistically different from the DiD estimate (−5.5pp; SE=2.2). This is driven by bringing in students with a smaller ability gain, but with the same decline in match. This result is consistent with the adverse effects of match being small for the marginally affected students, but increasing in relevance as programs reach deeper into the distribution of student preparation, mirroring findings in Carlana, Miglino, and Tincani (2024).

The role for this to generate "mismatch" depends on a student's next option. Figure 15 shows that students sort on match effects, and the degree of match is correlated within application portfolios. Panel (a) shows that students apply to programs where they see idiosyncratically higher graduation rates by about 6 percentage points. In addition, almost the entire distribution of match is positive, with the 25th percentile of match at each rank through 8 being positive. In contrast, there is no positive relationship between application rank and program effect, the latter being slightly negative at more preferred programs. Panel (b) shows that both match and ATE are strongly correlated within application portfolios. Finally, Appendix Figure A6 relates this to

Table 5: Program-Level Difference-in-Differences Analysis: Model Components

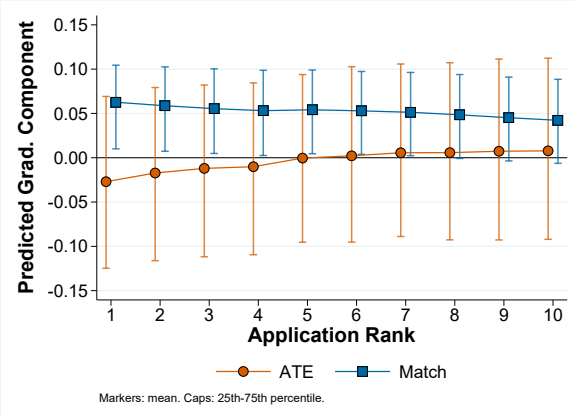
	All				High GPA+			
	(1) Data: Grad	(2) PrGrad	(3) A	(4) M	(5) Data: Grad	(6) PrGrad	(7) A	(8) M
Conditional on Enrollment								
Exposed	-0.022*	-0.039***	-0.054***	0.015***	-0.013	-0.034***	-0.048***	0.014***
	(0.012)	(0.004)	(0.004)	(0.002)	(0.014)	(0.004)	(0.004)	(0.002)
Exposed × Post	-0.025	0.000	0.026***	-0.026***	-0.055**	-0.017**	0.010*	-0.026***
	(0.018)	(0.006)	(0.006)	(0.003)	(0.022)	(0.007)	(0.006)	(0.004)
Outcome Mean	0.541	0.548	0.521	0.078	0.566	0.560	0.518	0.083
N (Students)	13,811	13,811	13,811	13,811	8,616	8,616	8,616	8,616
N (Programs)	482	482	482	482	216	216	216	216

Notes: Same sample and specification as the program-level analysis, replacing the outcome with model-predicted outcome quantities, evaluated at each student's posterior type given the ROL. Columns (1) and (5) display realized graduation in the data. SEs do not account for first-stage estimation error and are clustered at the program level.

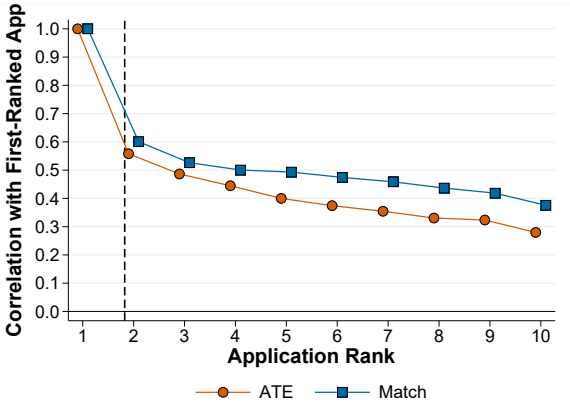
program-year effects and shows that treatment on the treated lies above average causal effects, showing that some sorting on gains matters for differences across programs.

Figure 15: Application Sorting on Outcome Components

(a) Mean Components



(b) Correlation with First Rank



Notes: This figure characterizes the mean and distribution of model-predicted graduation components across applications. Panel (a) shows the average component at each rank, for students submitting at least as many applications as are displayed on the x-axis (sample changes across ranks). Range bars show the 25th and 75th percentile of each component by rank. Panel (b) shows the correlation of these components with the first ranked application's components. The dashed black line shows the average rank of assignment.

5 Discretion and College Objectives

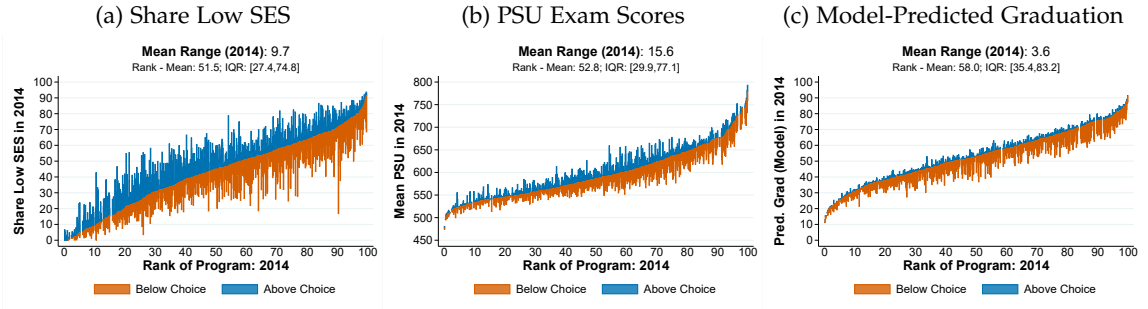
Characterizing Programs' Choice Sets We begin our investigation of programs' objectives by non-parametrically displaying choice sets over admitted students in the post-discretion 2014 equilibrium. For every program j and possible set of weights, which we call rules r, we simulate DA given observed applications and competitor choices, resulting in a set of admitted students

$A_j(r|r_{-j})$. Averaging characteristics of admitted students results in the student composition associated with program j's rule choice r, x_{jr}. This procedure yields a range of *possibilities*, or choices a program could have made regarding the composition of their students across all rules. Differences in choice sets reflect constraints imposed by students' applications and competitors' choices.

Panel (a) of Figure 16 shows programs' choice sets over low-SES shares. On average, programs could change their low-SES share by 10pp. However, there are substantial differences across programs. We observe large heterogeneity in choices subject to these constraints. Across the distribution, a majority of programs choose weights that place them in the middle of their choice set. Panel (b) in Figure 16 shows the possibilities for test scores, which tell a similar story.

In contrast, Panel (c) shows programs consistently maximize near the frontier of graduation probabilities of their student body, even as they are constrained by application pools and the design of admission rules. Despite not being able to target graduation measures directly, programs adapt their weights to maximize student outcomes.[36] We interpret this pattern as strong validation of our outcome model. This finding further reflects the importance of college discretion in mediating the trade-offs seen in the difference-in-differences designs — the lack of observed impacts on graduation is partially due to *programs' choices*.

Figure 16: Possibility Frontiers of Admitted Student Characteristics in 2014



Notes: Shows the range of an average student characteristic of the admitted pool attainable at each program's 2014 set of possible admission weights. Programs are ordered by the value under their chosen rule. The blue section of the range denotes values above those observed in the data; the orange section shows values below. The mean range across programs is reported in each panel title. Panel (a) shows the range of predicted low-SES share, panel (b) shows the range of PSU exam scores (average of Math and Verbal scores), and panel (c) the range of model-predicted graduation.

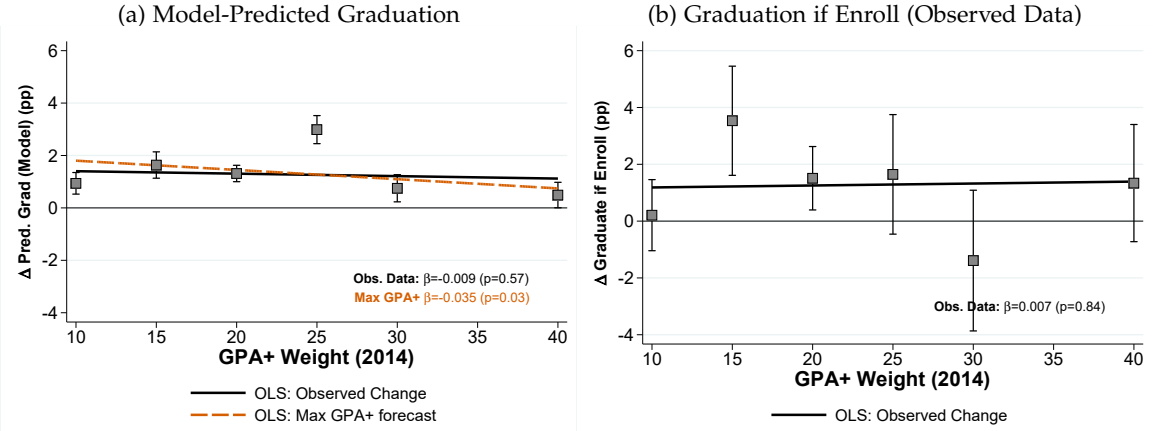
To further understand the college's tradeoffs, we revisit the empirical facts documented in Section 3 using our graduation model. We showed in Figure 7 that those choosing the minimum GPA+ could have increased their low-SES share but chose not to. Does this reflect a tradeoff with student achievement? The difference-in-differences analysis in Section 3 cannot answer this, as those causal estimates are only identified based on the program weight adoption we see in the

[36] Appendix Figure A7 shows how these figures change between 2013 and 2014, where discretion expanded the feasible sets. Consistent with Figures 6 and 7, the 2014 discretion led programs to move away from test scores and increase the share of low-SES students. Panels (e) and (f) show programs consistently rank near the top of the expected graduation frontier in both years, even as they alter characteristics of the overall admitted pool.

data.

Figure 17 repeats the exercise of Figure 7, focusing on changes in model-predicted graduation probabilities. Lower adopting programs would not have harmed their graduation had they chosen the maximum GPA+ weight. If anything, students that would have been pulled in by choosing the maximum GPA+ weight are predicted to graduate at slightly higher rates. This pattern suggests that the choice to not adopt a higher GPA+ weight at these programs is not due to concerns related to student performance, but likely reflects differences in preferences for student composition.

Figure 17: 2014 *GPA+* Weight and Changes in Graduation of Admitted Students



Notes: Each square shows the mean change in the indicated admissions characteristic by 2014 GPA+ weight, weighting programs by number of enrollments. Panel (a) shows changes in model predicted graduation conditional on enrollment. Panel (b) shows the observed change in graduation rates among those enrolling in each program. The orange/dashed line shows predicted changes from simulations that force programs to choose the maximum GPA+ weight, unilaterally deviating from their observed choice to the maximum weight in the 2014 equilibrium using 2014 applications. Standard errors for OLS relationships are clustered at the university level. Error bars report 95 percent confidence intervals and are not adjusted for clustering. See text for more details.

Modeling Program Payoffs We quantify the tradeoffs summarized in the previous figures using a random coefficients logit model. Our focus is on a rich characterization of how programs use admission rules to trade off student composition against graduation and its model-predicted components. The literature suggests other microfoundations of the college objective function we do not consider here, for example, trading off revenue (Epple, Romano, and Sieg 2006; Fu 2014) or long-run reputation maximization (MacLeod and Urquiola 2015). We view our program preference estimates as reduced-form approximations of these richer models, limiting our counterfactuals to those shifting assignment, and assuming preferences for student composition are stable in the counterfactuals.[37]

[37] Given that Chile already operates on centralized assignment, we think it is unlikely that shifts in the admissions index would fundamentally change considerations related to revenue or prestige maximization. However, it is possible these considerations could become important in counterfactuals related to a shift towards full discretion.

We model GPA+ as shifting the set of rules available, $\mathcal{R}_t$, where $t \in \{2013, 2014\}$. GPA+ weights above 10 percent can only be chosen in 2014. We model program j's payoff in year t from choosing rule $r \in \mathcal{R}_t$ as

$$v_{jtr} = x'_{jtr}\beta_{s(j)} + w'_{jtr}\eta_j + \epsilon_{jtr}. \tag{5.1}$$

x_{jtr} are characteristics of the admitted student pool over which college preferences are defined. These covariates are computed by simulating counterfactual admit pools at program j under rule r in year t, holding competitors' choices and applications fixed. The parameters $\beta_{s(j)}$ are mean coefficients that differ between public and private programs, classified by $s(j)$. Finally, $w_{jtr} \subseteq x_{jtr}$ collects the covariates with random coefficients. The unobserved taste parameters $\eta_j \sim N(0, \Sigma)$ are held constant across years. The error ϵ_{jtr} is distributed type I extreme value with unit scale. We interpret this term as an optimization error, and compute programs' payoffs net of it in counterfactuals. We defer details on the likelihood and estimation to Appendix B.3.

Programs know students' observables and have an expectation of their latent type based on the posterior of their submitted ROL. We treat this posterior as stable in counterfactuals, abstracting from a complex application-admissions game (e.g. Friedrich et al. 2025). This is consistent with programs having a "sense" of student unobservables.

Estimates of College Payoffs Table 6 reports our program estimates for student composition across different models. Columns (1)–(2) show that we explain 6.5 percent of the variation in choices using only linear effects in test scores and composition. Columns (3)–(4) show that adding model-predicted graduation slightly increases fit and, importantly, that graduation is valued over and above the main controls for test scores, mirroring Figure 16. This finding is important, as predicted graduation cannot be directly targeted by the admissions process. Columns (5)–(6) add heterogeneity through random coefficients. There is wide variation in preferences for graduation and for the share of students who are low SES. Tastes for both characteristics are positively correlated.

Columns (7)–(8) of Table 6 show our preferred specification of program payoffs, breaking graduation into student quality (ability) and student fit (match).[38] Adding both match and heterogeneity substantially improves the fit of the model, explaining 10.9 percent of the variation in choices. This measure of fit is specific to precise rule choices, which likely understates the true fit of the model given our discretization of the rule choices into a multinomial problem. Appendix Figure A8 shows that the model broadly reproduces descriptive patterns in the take-up of GPA+

[38] ATE is a program-year specific variable and cannot be included in the random utility model, as it is constant across rule choices within a program.

in 2014, stratified by 2013 weight on test scores, as well as the resulting characteristics of the admit pools.

There are four main takeaways from Columns (7)–(8) of Table 6. First, the preference for graduation measured in Columns (5)–(6) operates primarily through match, with the mean coefficient on student ability/quality being close to zero across both sectors.[39] At least some programs know which students are good fits for them. Second, there is a weak but significant preference for low-SES students across both public and private colleges. Third, there is a consistent disutility estimate for women across sectors (and specifications). Given the large main effects on test scores and that women tend to have higher graduation rates, we believe this likely reflects biased beliefs about the importance of test scores versus grades for student performance. Fourth, the preferences for average ability, match, and low SES are heterogeneous across programs. Programs have different goals.

Figure 18 visualizes heterogeneity in this relative preference for graduation components and low-SES enrollment by plotting the distribution of program-level ratios between tastes for student graduation components and the taste for low-SES students. These quantities reflect the relative importance assigned to different components: what increase in the low-SES share would be equivalent to a 1 percentage point increase in graduation from the specific component. Panel (a) shows that on average a 1pp increase in student quality is equivalent to a 5.4pp increase in the low-SES share. In other words, a percentage point increase in ability is worth more to the college than a percentage point increase in the low-SES share. Panel (b) shows a similar exercise for match effects. A 1pp increase in student match is equivalent to a 9.1pp increase in the low-SES share: match is preferred to quality on average.

However, this tradeoff is heterogeneous, as both distributions have large standard deviations. This variation results in many programs having a negative ratio: for these, an increase in ability/match is equivalent to a *decline* in the share of low-SES students. Programs therefore disagree on the importance of socioeconomic diversity, which is reflected in the differential take-up of GPA+ we saw in Section 3.

6 Counterfactual Admissions Policies

We now turn to implications of these estimates for counterfactual approaches to assigning seats. We are interested in exploring the effects of both restricting and increasing discretion in college admissions, and how these changes impact student access, segregation, and outcomes.

[39] This result does not imply colleges do not value student ability, as the positive coefficient on test scores is likely capturing part of that channel.

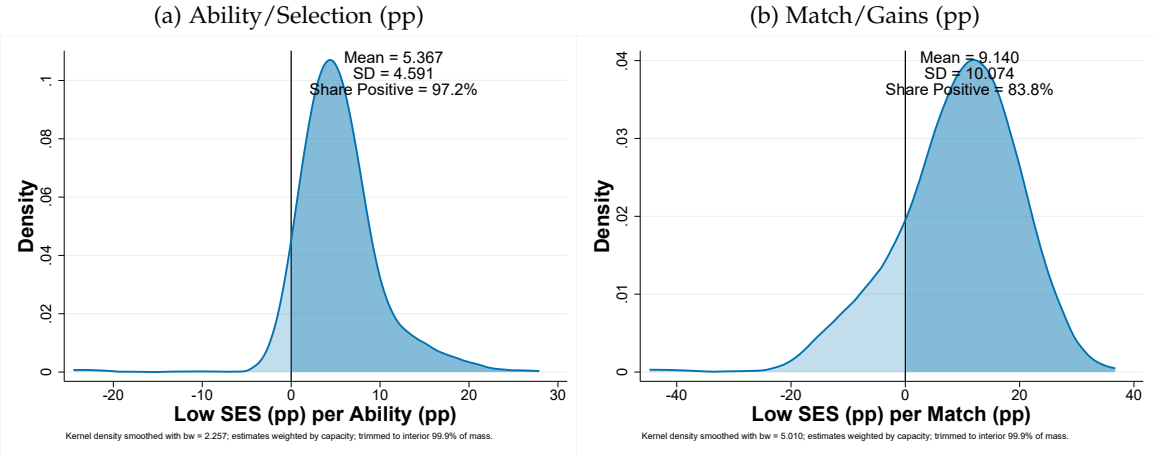
Table 6: Estimates of Program Objectives

	Basic Covariates		Add Graduation		Add RCs		Add Match	
	(1) Public	(2) Private	(3) Public	(4) Private	(5) Public	(6) Private	(7) Public	(8) Private
Panel A: Program preference coefficients								
Math PSU /10	0.60***	1.82***	-0.11	1.05***	0.13	1.65***	0.83***	3.04***
	(0.16)	(0.17)	(0.20)	(0.18)	(0.21)	(0.22)	(0.27)	(0.34)
Language PSU /10	0.13	0.80***	0.19*	0.89***	0.46***	1.34***	0.34**	1.26***
	(0.12)	(0.11)	(0.11)	(0.11)	(0.14)	(0.15)	(0.15)	(0.17)
Low SES (pp)	0.04**	0.08***	0.07***	0.14***	0.13***	0.14***	0.08***	0.09***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Female (pp)	-0.16***	-0.15***	-0.20***	-0.22***	-0.23***	-0.23***	-0.14***	-0.11***
	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
GPA	27.05***	26.14***	14.77***	10.18***	20.80***	17.98***	42.03***	50.36***
	(2.28)	(2.41)	(4.35)	(3.05)	(4.91)	(3.99)	(5.52)	(5.50)
Grad Prob (pp)			0.56***	0.85***	1.05***	1.11***		
			(0.16)	(0.14)	(0.16)	(0.16)		
Ability (pp)							0.08	-0.13
							(0.20)	(0.23)
Match (pp)							2.08***	2.30***
							(0.40)	(0.28)
Panel B: RC parameters								
Standard deviations								
Grad Prob					1.04***			
					(0.09)			
Ability							1.18***	
							(0.09)	
Match							1.58***	
							(0.24)	
Low SES					0.22***		0.17***	
					(0.02)		(0.02)	
Correlations								
Corr(Grad Prob, Low SES)					0.53***			
					(0.10)			
Corr(Ability, Match)							0.22	
							(0.15)	
Corr(Ability, Low SES)							0.65***	
							(0.14)	
Corr(Match, Low SES)							0.00	
							(0.26)	
Panel C: Fit								
Pseudo-R^2	0.065		0.073		0.090		0.109	
Avg. neg. log-likelihood	11.12		11.04		10.83		10.60	
BIC	22.34		22.19		21.81		21.39	
N(Parameters)	10		12		15		20	
N(Program clusters)	716		716		716		716	

Notes: Estimates of the program choice model inferred from observed admission rules over the characteristics of the admitted pool implied by each feasible rule, separated by public and private sector. Columns add the following covariates: (3)–(4) mean model-predicted graduation, (5)–(6) program random coefficients, and (7)–(8) the ability and match components. Panel B reports the random-coefficient standard deviations and correlations. Panel C reports model fit. PSU scores are in tens of points and shares in percentage points. Robust standard errors are clustered by program.

We consider two versions of uniform rules restricting discretion. First, we consider a test-only admissions policy. We think of this as a fully "meritocratic" approach. Second, we consider a fully "egalitarian" approach using lottery admissions, replicating a common approach to assignment in K-12 education. This policy extends the conclusion of "equity improvements without efficiency losses" to its limit (Black, Denning, and Rothstein 2023; Bleemer 2024). We compare these uniform

Figure 18: Preference Heterogeneity: Low SES Percentage Point Increase Equivalent



Notes: Distribution across programs of the relative preference for (a) ability/selection and (b) match, expressed as low SES percentage point equivalents. Densities are weighted by seats and trimmed to the interior 99.9 percent of mass.

rules to a full discretion counterfactual, where each program submits its truthful rank over applicants to the assignment mechanism, mirroring the approach undertaken in medical assignment (Agarwal 2015).[40] Finally, we consider an assignment where each program's priority index is the student's predicted graduation at the program, a likely infeasible benchmark.

To conduct these counterfactuals, we re-draw all unobservables from their posteriors given observed choices and outcomes in the data, as well as lottery priorities. We then re-simulate assignments for the 2013 and 2014 cycles through deferred acceptance using the full list of truthful preferences generated by the draws. To minimize simulation error, we repeat this procedure for 20 simulation draws. See Appendix Section B.4 for details. In our evaluations, we focus on student utility, graduation, access, and college payoffs, and on how these differ across groups of interest. These simulations include all exam takers in each cycle, including eligible non-applicants. The results are shown in Table 7.

In terms of uniform rules, test-only admission (column 2 of Table 7) has a small negative effect on graduation (-0.25pp), while increasing SES segregation across programs (+3.2 percent) and reducing access, especially for disadvantaged groups (low-SES and female students are 1.27pp and 0.96pp more likely to be unassigned). This result is driven by the fact that non-test score criteria are an important driver of graduation, while low-SES and female students tend to have lower test scores (see Section 2). While average student payoffs increase in aggregate (+1.02), the

[40] A concern here is whether this extrapolation is too far out of sample. As stated above, it may be the case that program preferences are not stable in this counterfactual if, in the long run, it opens up considerations related to prestige or endowment maximization that are not present in Chile. It may also be the case that programs undertake procedures to better assess program fit, for example, by conducting essays or interviews. A final concern relates to strategic considerations. We are implicitly assuming that each student's contribution to program payoffs is small, so that truthful reporting is approximately optimal for programs.

gain accrues to students who are neither low-SES nor female (Panels B and C), and the negative effects on graduation and access mean that this regime would only be preferred by a social planner who cares solely about aggregate student payoffs.

Moving to lotteries (column 3 of Table 7) worsens outcomes on most dimensions, as it removes the role of college discretion in driving the final allocation but preserves the role of student preferences. Graduation declines markedly (-1.94pp), including conditional on assignment (-3.06pp). Importantly, this is realized across all groups. Both student and college payoffs see strong declines. However, SES access increases slightly (unassigned -2.08pp) and segregation decreases markedly (by 7.3 percent). This result is driven by the importance of college selection on match in driving outcomes — removing discretion without consideration of academic factors causes graduation declines. Only a social planner whose sole goal is equity would prefer this alternative.

Moving to full discretion has no impact on graduation (column 4 of Table 7), reflecting the fact that many colleges are already near their frontier (Figure 16). Full discretion markedly increases segregation across programs, increasing the standard deviation in shares across programs by about 7 percent for low-SES students and 11 percent for female students. Access increases slightly for low-SES students (-0.84pp in the unassigned share), but declines markedly for women (+1.68pp in the unassigned share). As can be expected, this increases college payoffs while decreasing student payoffs. Taken together, full discretion would only be preferred by a social planner who is particularly interested in maximizing programs' payoffs.

Column 5 of Table 7 shows that allocating seats based on expected graduation, shutting down the preference for non-academic factors, would only slightly increase graduation while also increasing segregation across programs. In this scenario, graduation only increases by 0.50pp and conditional on assignment only 0.72pp, reflecting the fact that programs select near their graduation frontier. It also does not improve student payoffs. It does, however, result in strong benefits for women, reflecting that women have higher graduation rates and experience higher priorities in this counterfactual.

Table 7: Counterfactual Admissions Systems

	(1) Baseline	(2) Δ Test Only	(3) Δ Lottery	(4) Δ Full Discretion	(5) Δ Predicted Graduation
Panel A: All Students					
Unassigned (%)	43.51	+0.10	+0.44	+0.02	-0.18
Student Payoff	153.24	+1.02	-9.28	-1.30	+0.01
Graduation (%)	28.96	-0.25	-1.94	+0.00	+0.50
...if Assigned (%)	51.26	-0.35	-3.06	+0.02	+0.72
Grad. Weighted PSU	175.71	-1.67	-12.35	-0.09	+3.38
...if Assigned	311.04	-2.41	-19.57	-0.06	+4.95
Panel B: Low-SES Students					
Unassigned (%)	52.08	+1.27	-2.08	-0.84	+0.26
Student Payoff	131.19	-2.82	-2.23	+1.82	-1.66
Graduation (%)	22.69	-0.75	-0.31	+0.40	+0.27
...if Assigned (%)	47.35	-0.32	-2.58	-0.01	+0.83
Panel C: Female Students					
Unassigned (%)	44.55	+0.96	+0.16	+1.68	-2.52
Student Payoff	148.47	-1.57	-8.89	-7.00	+7.77
Graduation (%)	32.31	-0.59	-1.83	-0.60	+1.56
...if Assigned (%)	58.26	-0.06	-3.13	+0.70	+0.17
Panel D: Segregation					
SD Share Low SES	20.29	+0.65	-1.48	+1.36	+1.04
SD Share Female	19.22	-0.06	+0.04	+2.18	+2.49
Panel E: College Outcomes					
College Payoff	274.19	-0.97	-4.58	+2.81	-0.34
Share seats filled	84.58	-0.15	-0.66	-0.03	+0.28
Panel F: Other Statistics					
N(Assigned)	188,550	188,214	187,071	188,492	189,166
N(Total Students)	333,759	333,759	333,759	333,759	333,759

Notes: Column (1) ("Baseline") is the observed partial discretion DA regime simulated from the estimated student and college models. The other columns report changes when programs instead rank applicants by (2) PSU only, (3) by lottery, (4) by their own estimated marginal payoff (Full Discretion), or (5) by model-predicted graduation. Each column averages over 20 simulation draws, where each simulation draws latent type (conditioning on the posterior), student preference orderings, and college tastes and reruns DA. "Graduation" is set to zero for those unassigned. "Grad. Weighted PSU" shows the average program PSU measure in 2010-2011 multiplied by the graduation probability, which allows a sense of the selectivity of assigned programs.

7 Conclusion

This paper studies the aggregate and distributional consequences of allowing colleges discretion over whom to admit, exploiting two features of Chile's centralized college admissions system: programs' admission rules are publicly observed, and a 2014 reform expanded programs' discretion over those rules. We combine policy variation from the reform with a structural model of student preferences and graduation to understand the determinants of student outcomes across programs. We then use the model's forecasts to explore how programs trade off academic and non-academic characteristics, treating observed rules as revealed-preference expressions of programs' objectives. Finally, we build on these estimates of program payoffs to analyze the impact on graduation, access, and segregation of alternative admissions regimes that expand or limit discretion.

We find that match effects are an important component of student graduation. Two sources of sorting mute their effects at the margin: students select on fit through their applications, and colleges select on fit through their admission rules. This helps reconcile the small displacement effects found in reduced-form evaluations of admissions policies with the quantitative importance of match we estimate, and suggests that evaluating admissions reforms requires looking beyond the marginal student and any single policy. The more a policy restricts the choices through which students and colleges sort, and the more substantially it reallocates students, the more important the role of match becomes.

Our findings also illustrate an important role for discretion in the design of college admissions. Discretion gives programs a lever to select students based on fit. Programs use it, raising aggregate graduation. However, a little discretion goes a long way. Under full discretion, segregation increases with no gain in graduation, consistent with programs' heterogeneous preferences over low-SES students. This tension is why Chile's partial discretion performs well: it gives programs enough levers to select on fit while keeping their choices within bounds that limit adverse impacts on access and graduation. The results support delegating some admissions authority to colleges, but with guardrails that keep their choices aligned with a policymaker's desired balance of equity and efficiency.

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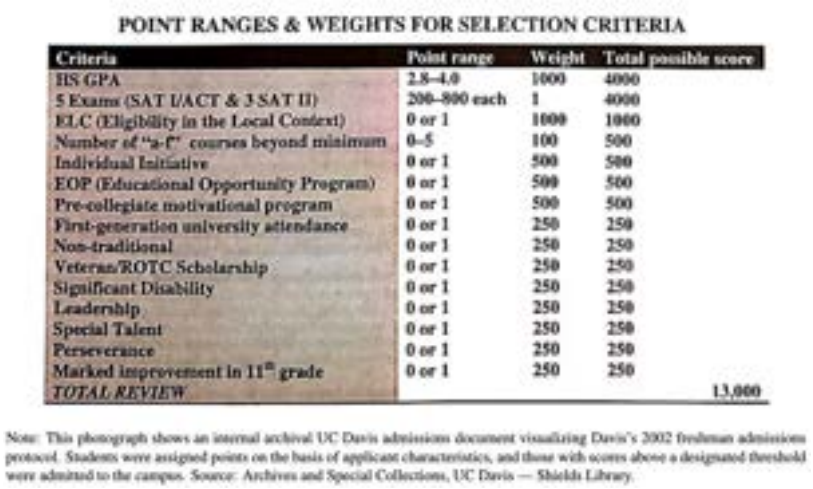
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Appendix A: Additional Figures and Tables

Figure A1: UC Davis Admission Requirements and Weights, 2002



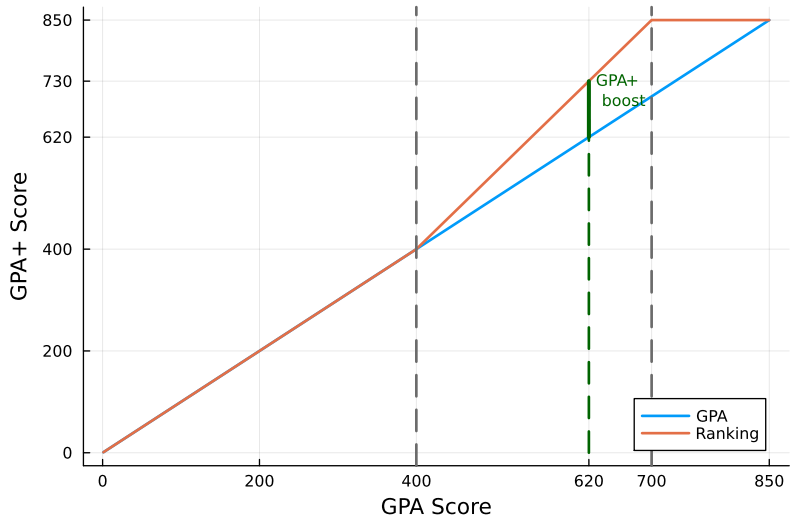
POINT RANGES & WEIGHTS FOR SELECTION CRITERIA

Criteria	Point range	Weight	Total possible score
HS GPA	2.8–4.0	1000	4000
5 Exams (SAT I/ACT & 3 SAT II)	200–800 each	1	4000
ELC (Eligibility in the Local Context)	0 or 1	1000	1000
Number of "a-f" courses beyond minimum	0–5	100	500
Individual Initiative	0 or 1	500	500
EOP (Educational Opportunity Program)	0 or 1	500	500
Pre-collegiate motivational program	0 or 1	500	500
First-generation university attendance	0 or 1	250	250
Non-traditional	0 or 1	250	250
Veteran/ROTC Scholarship	0 or 1	250	250
Significant Disability	0 or 1	250	250
Leadership	0 or 1	250	250
Special Talent	0 or 1	250	250
Perseverance	0 or 1	250	250
Marked improvement in 11th grade	0 or 1	250	250
TOTAL REVIEW			13,000

Note: This photograph shows an internal archival UC Davis admissions document visualizing Davis's 2002 freshman admissions protocol. Students were assigned points on the basis of applicant characteristics, and those with scores above a designated threshold were admitted to the campus. Source: Archives and Special Collections, UC Davis — Shields Library.

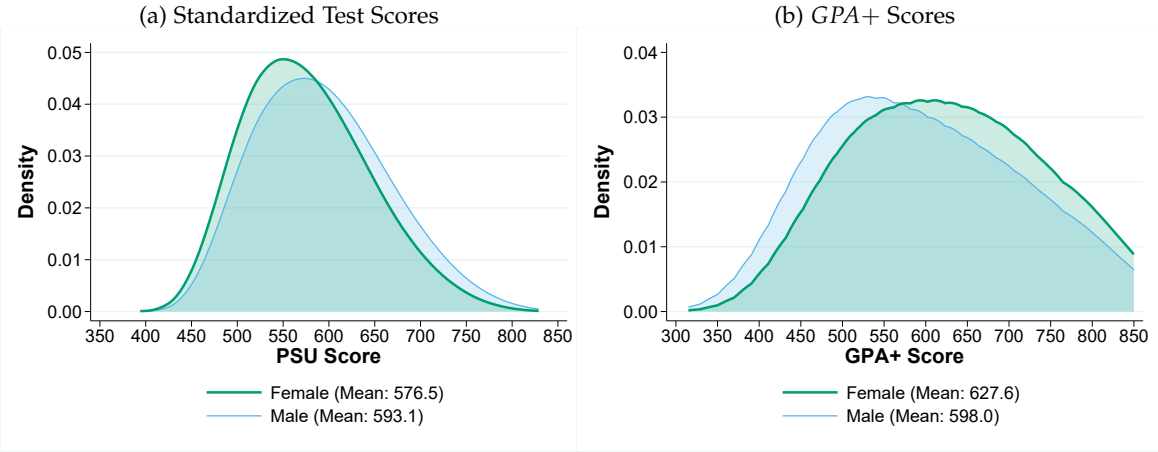
Source: Bleemer (2024).

Figure A2: GPA+ Score



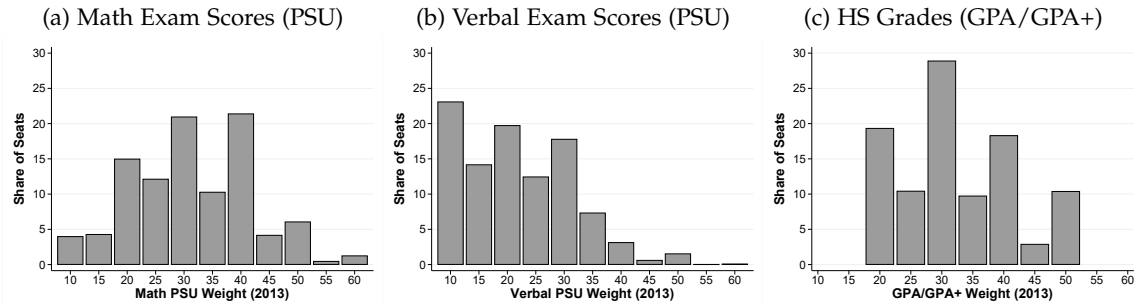
Notes: This figure shows the calculation of the GPA+ score for a hypothetical high school. Across the last three cohorts (not including the one for which the score is being calculated), both the average and maximum GPA score are computed. The GPA+ is, by construction, always equal to or higher than the GPA score. Students at or below the historical average have a GPA+ equal to their GPA score. The maximum GPA+ score (850) is achieved if the student's GPA is equal to the maximum GPA. For all other students, the GPA+ score is calculated as a linear interpolation between the average and maximum scores. In this example, the historical average GPA score is 400, and the historical maximum GPA is 700 (dashed gray lines). A hypothetical student with a GPA score of 620 has a GPA+ score of 730, representing a 110-point (i.e., 1 standard deviation) "boost" in their score (green line).

Figure A3: Distribution of Admission Requirements by Gender



Notes: See notes to Figure 2.

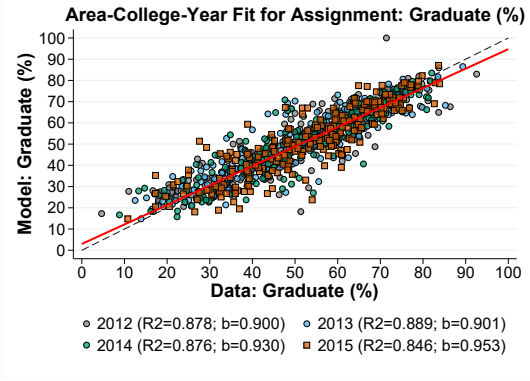
Figure A4: Distribution of 2013 Admission Weights



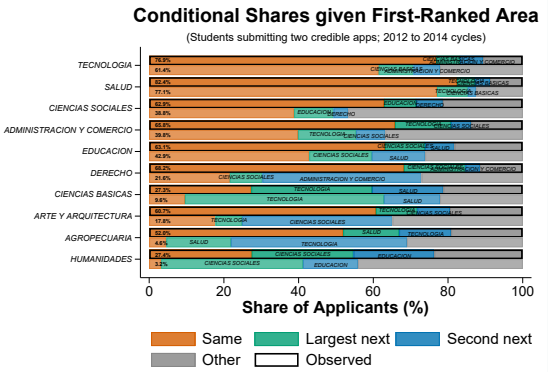
Notes: Seat-weighted distribution of 2013 weights across the 716 programs selective in both 2013 and 2014. Panel (c) combines the boosted (GPA+) and raw (GPA) high school grade measure. In 2013 the GPA+ component was fixed at 10, so panel (c) varies only through the raw GPA weight.

Figure A5: Detailed Student Choice and Outcome Model Fit

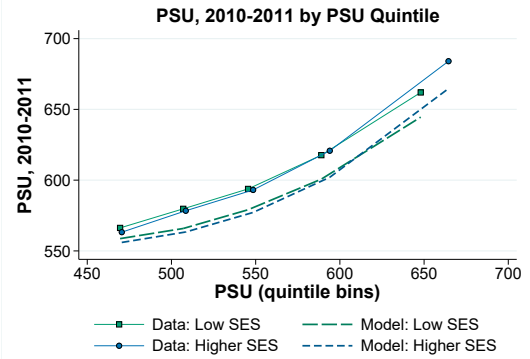
(a) College-Area-Year Graduation Rates



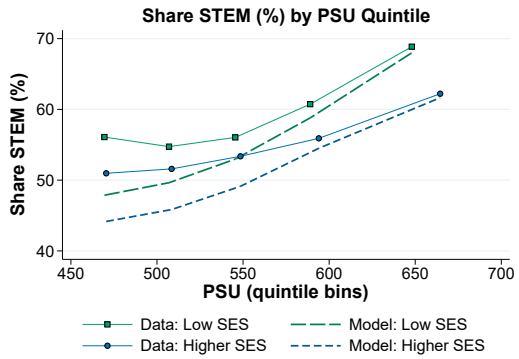
(b) Second Choice Area Fit



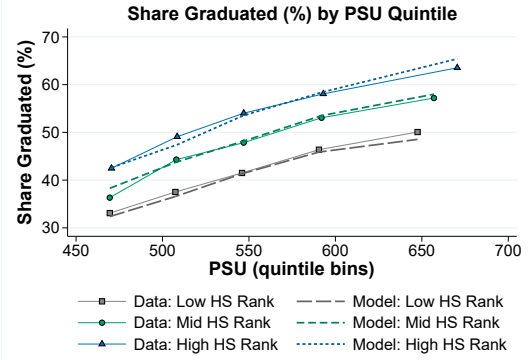
(c) Selectivity Sorting: By PSU and Low SES



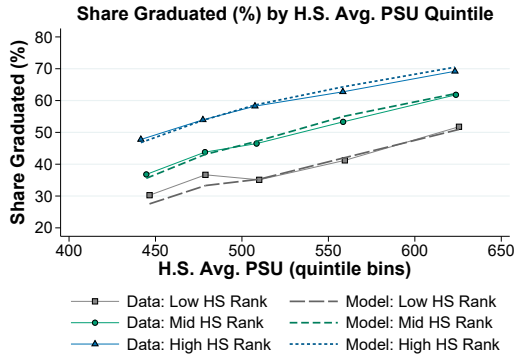
(d) STEM Selectivity Sorting: By PSU and Low SES



(e) Graduation: Student PSU and Within-HS Rank



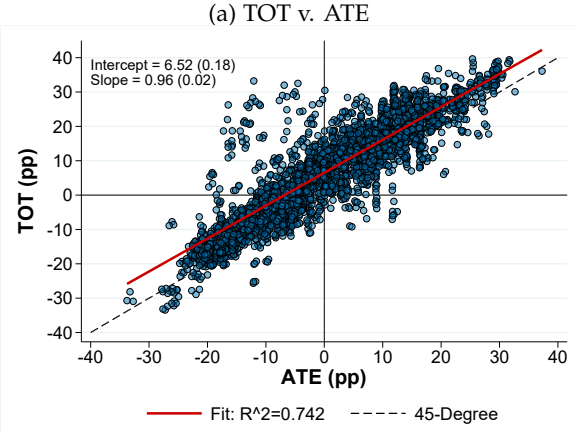
(f) Graduation: High School PSU and Within-HS Rank



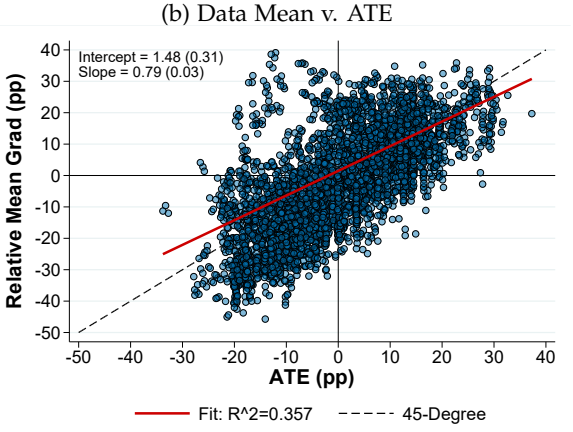
Notes: This figure shows additional fit of the student model. The simulation draws each applicant's type from her posterior given characteristics and application (not graduation) and new preference shocks, updating admission cutoffs. Panel (a) shows graduation rates by college-area-year among students who enroll in their assigned program, for cells with more than 50 assigned students in the data. The black dashed line is the 45-degree line and the solid red line is the line of best fit, weighted by the number of students. Orange squares mark 2015, which is not used in estimation. Panel (b) shows the broad area of the second-ranked credible application by the area of the first, for students with at least two credible applications, ordered by first-choice shares (smaller areas are displayed lower). The two largest other areas in the data are labeled, and in each pair the upper, outlined bar is the data. Panel (c) shows the average baseline selectivity (2010–2011 average enrollee PSU) of first-ranked programs by student PSU quintile and low-SES status, and panel (d) the share of first-ranked programs in STEM. Panel (e) shows graduation rates among enrollees by student PSU quintile and within-high-school GPA rank tercile, and panel (f) by *high school* average PSU quintile. Panels (b) to (f) use 2012–2014 applicants; in panels (c) to (f), solid lines are the data and dashed lines the model.

Figure A6: Relationship between Program-Year-Level Treatment Effects

(a) TOT v. ATE



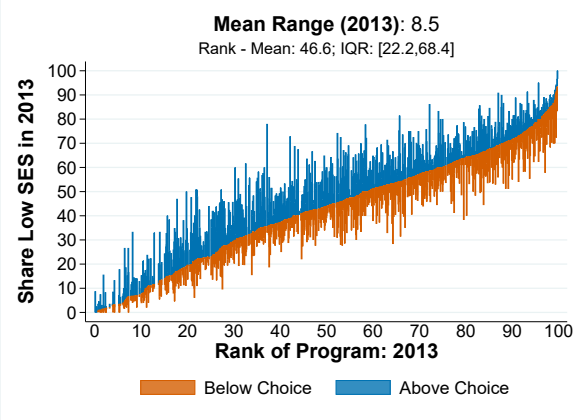
(b) Data Mean v. ATE



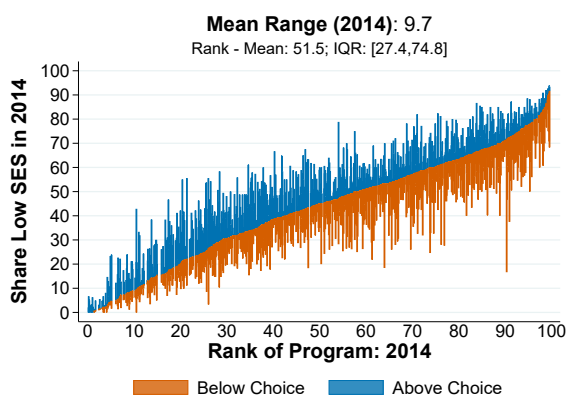
Notes: Shows the relationship between treatment effect quantities and mean graduation as predicted within the estimated student model, at observed enrollments. Each dot is a program-year. Panel (a) shows the relationship between treatment on the treated on the y-axis (ATE_j plus the average M_{ij} for enrollees) against ATE_j. Panel (b) shows the relationship between predicted graduation and ATE_j. The black dashed line is the 45-degree line. The red solid line is a weighted linear fit.

Figure A7: Possibility Frontiers of Admitted Student Characteristics: 2013 versus 2014

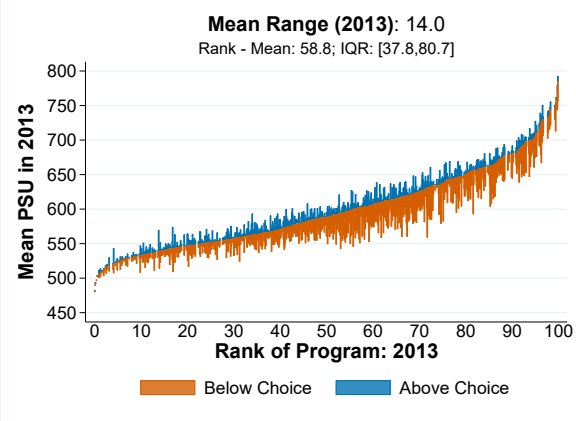
(a) Share Low SES: 2013



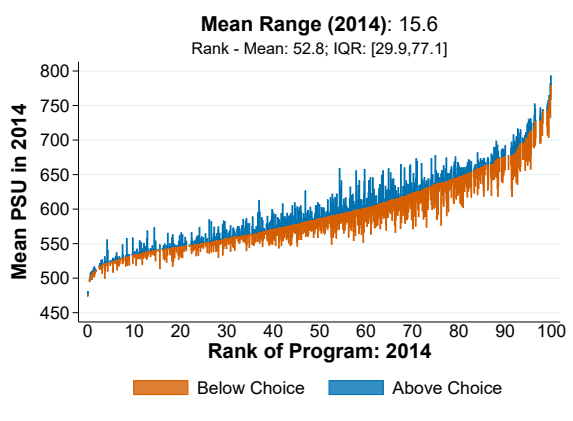
(b) Share Low SES: 2014



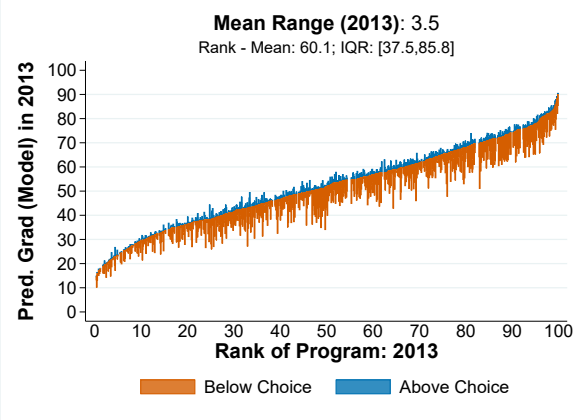
(c) PSU: 2013



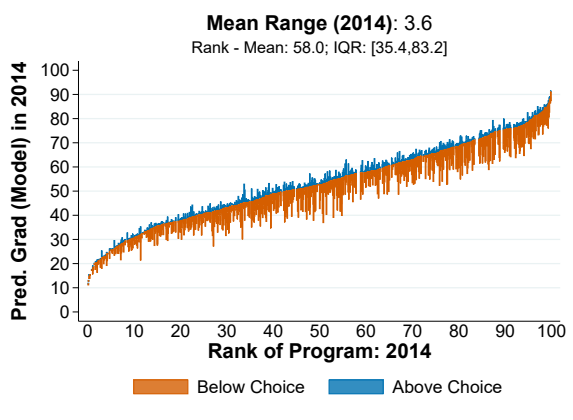
(d) PSU: 2014



(e) Model-Predicted Graduation: 2013



(f) Model-Predicted Graduation: 2014

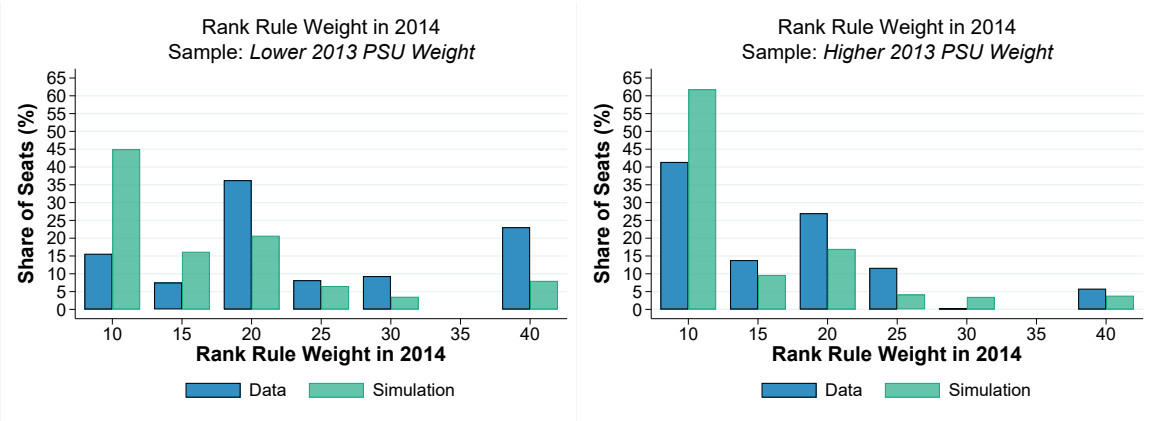


Notes: Shows the range of a characteristic of the admitted pool attainable within each program's set of possible admission weights, comparing 2013 rules with the expanded set of 2014 rules. Programs are ordered by the value under their chosen rule. The mean range across programs is reported in each panel title.

Figure A8: Fit of the Program Model

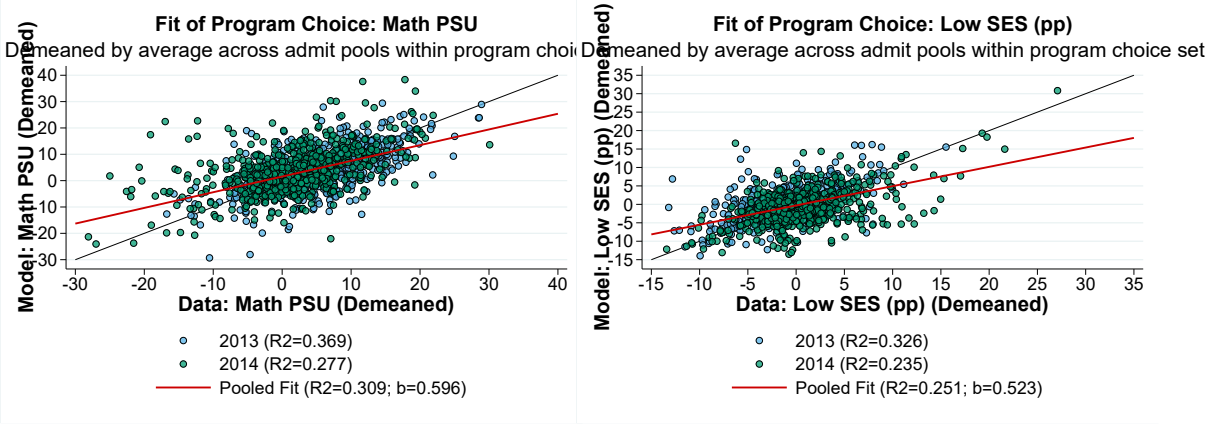
(a) Low 2013 PSU Weight

(b) High 2013 PSU Weight



(c) Admitted Students: Math PSU

(d) Admitted Students: Low-SES Share



Notes: Panels (a) and (b) compare distributions of GPA+ weight choices in 2014 in the data with those simulated in the estimated program model, stratified by whether the programs placed a high weight on test scores in the 2013 data (composite PSU weight above 50 percent). Panels (c) and (d) show characteristics of the chosen pools of admitted students, for math PSU scores and the share of students who are low SES. The black line is the 45-degree line. The red lines are seat-weighted linear fits of model quantities on data quantities.

Table A1: Summary of 2013 Program and Enrollee Characteristics by 2014 GPA+ Weight

	Mean and Standard Deviation				*Difference: High v. Min*	
	(1) All	(2) Min: 10	(3) Low: (10,20]	(4) High: (20,40]	(5) Raw	(6) Controlled
Mean PSU	614.02	603.85	628.55	601.48	-2.37	12.39
	(53.42)	(43.31)	(58.76)	(47.74)		
Mean GPA	621.89	583.08	638.90	629.94	46.87***	45.77***
	(62.19)	(49.26)	(63.92)	(55.11)		
Mean GPA+ (Rel. GPA)	650.45	602.11	670.62	661.97	59.85***	52.61***
	(73.61)	(56.22)	(74.69)	(66.53)		
Mean HS GPA Rank	66.57	54.80	70.14	71.33	16.53***	11.54***
	(13.96)	(12.52)	(12.35)	(11.46)		
Low SES (%)	38.30	27.99	34.90	51.96	23.96***	10.43*
	(22.27)	(21.17)	(21.14)	(17.83)		
SEP (%)	20.98	14.28	19.01	29.52	15.23***	10.02***
	(12.51)	(9.90)	(11.28)	(11.53)		
Elite Public HS (%)	14.35	12.83	15.14	14.49	1.66	0.89
	(7.95)	(6.44)	(8.20)	(8.57)		
Private (%)	26.31	38.03	29.67	11.48	-26.55***	-11.03
	(25.60)	(31.14)	(24.41)	(11.34)		
Female (%)	48.67	47.66	48.30	50.05	2.39	0.98
	(23.43)	(24.06)	(22.89)	(23.70)		
Metro (%)	67.26	79.24	58.85	69.37	-9.87	-3.67
	(31.58)	(19.27)	(34.87)	(31.40)		
Graduate if Enroll (%)	53.97	54.87	51.90	56.24	1.37	9.47**
	(19.55)	(19.21)	(19.42)	(19.80)		
Seats	140.88	157.76	159.79	99.04	-58.72**	-45.90***
	(131.40)	(115.67)	(167.55)	(49.21)		
Apps Admitted (%)	47.21	53.44	47.14	42.06	-11.38***	-13.05***
	(21.96)	(22.89)	(20.78)	(21.59)		
Apps Ranked First (%)	24.61	20.83	28.49	22.16	1.33	0.53
	(11.06)	(7.44)	(12.84)	(8.81)		
Santiago Region (%)	49.75	71.87	47.94	33.68	-38.18*	-51.11***
	(50.03)	(45.10)	(50.04)	(47.36)		
Private Univ (%)	62.31	81.55	61.68	46.96	-34.59	-7.19
	(48.49)	(38.91)	(48.69)	(50.01)		
G25 Univ (%)	74.45	29.66	89.91	89.83	60.18***	45.37***
	(43.64)	(45.81)	(30.17)	(30.29)		
STEM Program (%)	59.71	54.25	58.53	66.05	11.80**	13.93
	(49.08)	(49.97)	(49.35)	(47.45)		
High 2013 PSU Weight (%)	50.31	78.30	47.95	30.07	-48.23**	4.30
	(50.03)	(41.34)	(50.04)	(45.96)		
N(Programs)	716	164	316	236		
N(Colleges)	33	15	21	16		

Notes: Columns (1) to (4) show the mean and standard deviation of variables for programs that are selective in both 2013 and 2014, broken out by 2014 GPA+ weight choice. Columns (5) and (6) show differences in means, where standard errors and p-values are computed from a regression, among programs at the minimum or with a high weight, of the variable on an indicator for having a high GPA+ weight and clustering by university. Column (6) augments this comparison by controlling for indicators for baseline weight choice. These admission requirement cells are constructed as possible combinations of total PSU weight (the sum of math and verbal components) and the weight on raw GPA, first coarsened by rounding each down to the nearest 10. These admission weight cells thus ignore optional exam components and the differences between math and verbal. All variables are computed among enrollees unless otherwise stated. All statistics and regressions are weighted by the number of enrollees in 2013. Standard deviations are shown in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A2: 2014 GPA+ Weight and 2013 Baseline Characteristics

	Summary	*Outcome: 2014 GPA+ Weight*				*Outcome: High Weight: (20,40]*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mean/SD	Univariate	Joint	Add 2013 Weight Controls	Add College FE	Univariate	Joint	Add 2013 Weight Controls	Add College FE
Mean PSU	614.023	-0.015	-0.118***	-0.023	-0.017	-0.001*	-0.004	-0.002	-0.001
	(53.421)	(0.017)	(0.043)	(0.029)	(0.019)	(0.001)	(0.003)	(0.001)	(0.001)
Mean GPA	621.890	0.029**	0.138***	0.045*	0.021	0.001	0.005**	0.002**	0.001
	(62.195)	(0.014)	(0.038)	(0.023)	(0.021)	(0.001)	(0.002)	(0.001)	(0.001)
Low SES (%)	38.295	0.178***	0.146***	0.101**	-0.044*	0.008***	0.008***	0.005**	-0.001
	(22.270)	(0.053)	(0.033)	(0.041)	(0.026)	(0.002)	(0.003)	(0.002)	(0.001)
Elite Public HS (%)	14.349	0.053	0.144*	0.081	-0.009	0.001	0.010**	0.004	0.002
	(7.953)	(0.124)	(0.076)	(0.051)	(0.031)	(0.005)	(0.004)	(0.003)	(0.001)
Female (%)	48.667	0.005	-0.059*	-0.018	-0.014	0.001	-0.002	-0.002	-0.001
	(23.425)	(0.023)	(0.032)	(0.025)	(0.018)	(0.001)	(0.002)	(0.001)	(0.001)
Metro (%)	67.260	-0.008	0.078***	0.082***	-0.001	0.001	0.004	0.005**	-0.001
	(31.577)	(0.041)	(0.028)	(0.030)	(0.018)	(0.002)	(0.003)	(0.002)	(0.001)
Rural (%)	7.701	0.073	0.106	0.117*	0.110**	0.001	0.002	0.003	0.004*
	(17.604)	(0.066)	(0.076)	(0.065)	(0.044)	(0.003)	(0.004)	(0.003)	(0.002)
Graduate if Enroll (%)	53.973	0.015	0.013	0.043*	0.034**	0.002	0.002	0.003	0.002
	(19.551)	(0.030)	(0.027)	(0.023)	(0.015)	(0.002)	(0.002)	(0.002)	(0.002)
D(Santiago Region)	0.498	-2.911	1.714	-1.484	-1.904*	-0.195	-0.104	-0.185*	-0.025
	(0.500)	(3.451)	(3.484)	(2.100)	(1.053)	(0.171)	(0.187)	(0.104)	(0.031)
D(Private Univ)	0.623	-6.945*	-2.844	-3.591	0.000	-0.198	-0.067	-0.101	0.000
	(0.485)	(3.583)	(2.287)	(2.220)	(0.000)	(0.187)	(0.167)	(0.138)	(0.000)
D(G25 Univ)	0.745	8.279**	0.479	-0.844	0.000	0.245*	-0.093	-0.088	0.000
	(0.436)	(3.070)	(3.306)	(2.529)	(0.000)	(0.141)	(0.154)	(0.124)	(0.000)
D(STEM Program)	0.597	2.377**	-1.225*	-0.019	0.179	0.080	-0.075*	-0.016	0.025
	(0.491)	(0.957)	(0.707)	(0.567)	(0.490)	(0.048)	(0.041)	(0.034)	(0.025)
Apps Ranked First (%)	24.611	-0.033	-0.085	-0.099**	-0.032	-0.006*	-0.006**	-0.006**	-0.001
	(11.058)	(0.089)	(0.063)	(0.046)	(0.045)	(0.003)	(0.003)	(0.002)	(0.002)
Apps Admitted (%)	47.214	-0.068***	0.045*	0.018	0.007	-0.003***	0.001	0.000	0.001
	(21.965)	(0.017)	(0.026)	(0.020)	(0.010)	(0.001)	(0.001)	(0.001)	(0.000)
log(Seats)	4.682	-2.332*	0.132	0.650	-0.501	-0.131**	0.004	0.031	-0.045
	(0.670)	(1.211)	(0.818)	(0.614)	(0.609)	(0.049)	(0.039)	(0.030)	(0.029)
R2			0.410	0.529	0.807		0.337	0.490	0.839
N(Programs)	716	716	716	716	716	716	716	716	716
N(Colleges)	33	33	33	33	33	33	33	33	33

Notes: This table reports relationships between 2014 GPA+ admission weight and 2013 baseline characteristics for programs that are selective in both 2013 and 2014. Column (1) reports raw means and standard deviations. Columns (2) to (5) use continuous GPA+ weights as the outcome while columns (6) to (9) use an indicator for having a high GPA+ weight (20,40] as the outcome. Columns (2) and (6) report univariate regression results in each row. Standard errors are clustered at the university level. * p<0.10, ** p<0.05, *** p<0.01

Table A3: Decomposition of Changes in Admitted Student Characteristics

	A. *OLS on GPA+ Weight*			B. *Decomposition of Change*		
	All	Public	Private	All	Public	Private
I. Δ Mean PSU						
Coef/SD	-0.372	-0.302	-0.410	10.543	10.840	10.168
SE	(0.067)	(0.097)	(0.117)			
Share: Apps (%)	20.1%	3.3%	28.0%	64.8%	64.6%	66.3%
Share: Partial EQ (%)	108.6%	121.8%	103.1%	25.2%	28.1%	20.4%
Share: EQ Adjustment (%)	-28.0%	-23.9%	-30.5%	9.9%	7.3%	13.3%
Share: Sim Error (%)	-0.8%	-1.2%	-0.6%	0.1%	0.0%	0.1%
II. Δ Mean GPA						
Coef/SD	0.347	0.277	0.483	17.192	17.387	17.087
SE	(0.111)	(0.105)	(0.210)			
Share: Apps (%)	10.5%	79.7%	-6.4%	66.2%	70.9%	63.5%
Share: Partial EQ (%)	133.1%	105.0%	132.3%	18.8%	19.6%	18.1%
Share: EQ Adjustment (%)	-43.6%	-84.5%	-26.1%	15.0%	9.7%	18.2%
Share: Sim Error (%)	-0.0%	-0.3%	0.3%	-0.0%	-0.3%	0.2%
III. Δ Share Low SES						
Coef/SD	0.191	0.170	0.229	7.565	8.028	7.260
SE	(0.034)	(0.050)	(0.048)			
Share: Apps (%)	15.0%	26.8%	12.3%	78.4%	77.6%	79.6%
Share: Partial EQ (%)	123.7%	120.2%	119.5%	20.1%	19.9%	19.5%
Share: EQ Adjustment (%)	-39.2%	-47.5%	-32.3%	1.3%	2.2%	0.9%
Share: Sim Error (%)	0.4%	0.5%	0.5%	0.2%	0.4%	0.0%
IV. Δ Mean HS Rank						
Coef/SD	0.155	0.125	0.192	5.004	4.861	5.064
SE	(0.028)	(0.032)	(0.051)			
Share: Apps (%)	12.8%	43.5%	3.3%	66.6%	69.7%	66.2%
Share: Partial EQ (%)	117.5%	112.6%	118.0%	24.0%	26.9%	21.2%
Share: EQ Adjustment (%)	-30.4%	-56.4%	-21.7%	9.1%	3.3%	12.3%
Share: Sim Error (%)	0.1%	0.2%	0.3%	0.2%	0.0%	0.3%
V. Δ Share Female						
Coef/SD	0.075	0.019	0.123	6.937	7.320	6.691
SE	(0.035)	(0.043)	(0.060)			
Share: Apps (%)	-16.3%	-155.9%	12.2%	87.6%	89.1%	86.9%
Share: Partial EQ (%)	152.3%	398.8%	113.1%	9.2%	10.6%	7.7%
Share: EQ Adjustment (%)	-34.9%	-129.5%	-26.8%	2.6%	-0.7%	5.0%
Share: Sim Error (%)	-1.0%	-13.4%	1.5%	0.6%	1.0%	0.4%
N(Programs)	716	291	425	716	291	425
N(Colleges)	33	16	17	33	16	17

Notes: This table decomposes changes in admitted student characteristics between 2013 and 2014 into components due to application shifts, equilibrium adjustments, partial equilibrium (i.e., program discretion), and residual effects of rule changes. Panel A decomposes the OLS coefficient relating the change in a student observable to the 2014 GPA+ weight. Panel B decomposes the cross-program variance of changes in each characteristic (via covariance shares). Programs are weighted by 2013 enrollment. The partial equilibrium component is computed by simulating admissions using the 2013 applicant pool while changing the target program's rule from its 2013 to its 2014 weights and holding all competing programs at their 2013 weights. The equilibrium adjustment component is computed by simulating admissions using the 2013 applicant pool while holding the target program fixed at its 2014 weights and shifting competing programs from their 2013 to their 2014 weights. The application component is computed by shifting the applicant pool from 2013 to 2014 while holding all programs' weights fixed at their 2014 admission weights. The simulation error share is the residual not accounted for by these components. See text for more details. Standard errors are clustered at the university level.

Table A4: GPA+ and Changes in Admitted Student Characteristics

	(1)	(2)	(3)
Δ Mean PSU	-0.372***	-0.392***	-0.357***
	(0.067)	(0.064)	(0.071)
Δ Mean GPA	0.347***	0.661***	0.705***
	(0.111)	(0.121)	(0.101)
Δ Low SES (%)	0.191***	0.280***	0.085**
	(0.034)	(0.043)	(0.038)
Δ Elite Public HS (%)	-0.130***	-0.107***	0.011
	(0.030)	(0.020)	(0.020)
Δ Female (%)	0.075**	0.059	-0.052
	(0.035)	(0.042)	(0.051)
Δ SEP (%)	0.104***	0.114***	-0.013
	(0.026)	(0.032)	(0.028)
Δ Mean HS GPA Rank	0.155***	0.229***	0.031**
	(0.028)	(0.028)	(0.014)
Δ First Rank (%)	0.029	0.119**	0.030
	(0.045)	(0.055)	(0.062)
Δ Metro (%)	-0.019	-0.009	-0.002
	(0.029)	(0.032)	(0.034)
Controls			
Baseline Controls		X	X
Baseline Weight FE		X	X
Correlated Changes			X
N (Programs)	716	716	716

Notes: This table reports relationships between changes in admitted student characteristics between 2013 and 2014 with GPA+ weight. Each row corresponds to a change in a key characteristic and displays coefficients for separate regressions. Column (1) reports raw OLS relationships. Column (2) adds controls for baseline weight choices and characteristics (mean PSU and GPA, shares low SES, female, metro, rural, and graduating among enrollees, share admitted, share ranked first, log seats, and indicators for Santiago, private university, G25, and STEM). Column (3) adds the contemporaneous changes in the other characteristics listed in the table. Observations are restricted to selective programs in both 2013 and 2014 and weighted by 2013 enrollment. Standard errors are clustered at the university level and reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix B: Estimation and Computation Details

B.1 Estimation of the Student Model

This section provides details on the estimation of the student model in Section 4. Let $\theta = \{\theta_\pi, \theta_U, \theta_G\}$ denote the parameters governing the type priors, student utility, and graduation outcomes. With $Q = 8$ latent types there are 1,026 parameters: 70 in the type priors, 479 in utility, and 477 in graduation outcomes. We estimate the model on $N = 498,977$ student-years from the 2012 to 2014 admission cycles. This sample includes 180,835 non-applicants who took the PSU exams but did not apply even though they had feasible DEMRE options. Non-applicants enter the type priors and the utility likelihood (and are treated as choosing the outside option), but not the graduation likelihood.

Feasible Choice Sets Although truthfully reporting preferences is a dominant strategy in DA when lists are unrestricted, students may deviate from truth-telling in practice, for example because lists are limited to ten programs or because they leave out programs where they have little chance of admission (Larroucau and Rios 2020). We follow Kapor, Karnani, and Neilson (2024) in assuming students truthfully rank options in a local bandwidth of feasible options. This allows some strategic behavior as students may "skip the impossible" but sidesteps having to model a computationally demanding portfolio choice problem. In counterfactuals, we adjust assignments by simulating DA under students' true preference ranking. We validate our approach by showing we are able to recover allocations observed in the data as well as out-of-sample allocations in 2015, a year not used in estimation.

Let $Index_{ij}$ be student i's application score at program j and $Cutoff_j$ the lowest score among students admitted to j in the data. We set $Cutoff_j$ to zero for programs that did not fill their seats, since any applicant would be admitted. We classify an application as a "reach" if $Index_{ij}$ is more than 25 points below $Cutoff_j$ and as a "safety" if it is more than 50 points above. We drop reach applications as well as all applications listed after a student's first safety, and treat the trimmed list as a truthful ranking of utilities within the programs in the student's admission cycle that are not clearly out of reach:

$$\Omega_i = \{\, j : Index_{ij} + 25 \geq Cutoff_j \,\} \tag{B.1}$$

We infer that a student *ranks the outside option* if (1) their original list is a short list, with no safety and fewer than ten programs and (2) there are programs left in Ω_i they did not rank. If these conditions are satisfied, we add the outside option as the last entry of their trimmed ROL,

inferring it as revealed preferred to all unranked feasible programs.

The above approach serves as an ad hoc middle ground between assuming full truth-telling and alternative approaches that only assume the final assignment is stable, which restrict attention to the ex-post feasible set $\{j : Index_{ij} \geq Cutoff_j\}$ (Agarwal 2015; Fack, Grenet, and He 2019). We are able to use this to recover students' true preferences, and the longer list helps discipline unobserved heterogeneity. However, in counterfactuals, our assumptions are similar to Fack, Grenet, and He (2019)'s approach: the resulting assignment is stable with respect to true student preferences and admissions priorities.

Student Likelihood The full log-likelihood can be written as:

$$\mathcal{L}(\theta) = \sum_i \log \left(\sum_{q=1}^{Q} \underbrace{\Pi_q(X_i;\theta_\pi)\, P_{i|q}(ROL_i;\theta_U)\, P_{i|q}(Grad_i;\theta_G)}_{P_{i|q}(\theta)} \right) \tag{B.2}$$

where each individual's contribution integrates over the distribution of unobserved types. Admissions outcomes do not enter the likelihood, as they are deterministic given weights, applications, and capacities. For simplicity, we abstract from modeling the enrollment decision given assignment.

The prior contribution depends on student observables X_i alone:

$$\Pi_q(X_i;\theta_\pi) = \frac{\exp\{\pi_0^q + X_i'\pi_X^q\}}{1 + \sum_{r=2}^{Q} \exp\{\pi_0^r + X_i'\pi_X^r\}}. \tag{B.3}$$

Let $\bar{U}_{ijq}$ be the utility of program j net of the preference shock ϵ_{ijq} for a student i of type q: $\bar{U}_{ijq} = \delta_{g(j)} + \mu_{iq} + W_j'\gamma_{iq}^W - \lambda(loc_i, loc_j)$. The probability of a student's trimmed rank-ordered list $ROL_i = \{j_i^1, \ldots, j_i^{|ROL_i|}\}$ conditional on type is an exploded logit:

$$P_{i|q}(ROL_i;\theta_U) = \prod_{r=1}^{|ROL_i|} P_{i|q}(j_i^r|\Omega_{i|r};\theta_U) \tag{B.4}$$

$$P_{i|q}(j_i^r|\Omega_{i|r};\theta_U) = \frac{\exp\{\bar{U}_{ij_i^r q}\}}{1 + \sum_{k\in\Omega_{i|r}} \exp\{\bar{U}_{ikq}\}} \tag{B.5}$$

where $\Omega_{i|r} = \Omega_i \setminus \{j_i^1, \ldots, j_i^{r-1}\}$ removes the options ranked above r from the choice set. The utility of the outside option is normalized to zero.

Let $PrGrad_{ijq}$ be the corresponding graduation probability conditional on being type q:

$$PrGrad_{ijq} = \Lambda\left(\alpha_{g(j)} + \varphi_{iq} + W_j'\tau_{iq}^W\right) \tag{B.6}$$

Then the likelihood has the form:

$$P_{i|q}(Grad_i;\theta_G) = \left(PrGrad_{i,j_i^{Enroll}q}\right)^{Grad_i} \cdot \left(1 - PrGrad_{ij_i^{Enroll}q}\right)^{1-Grad_i} \tag{B.7}$$

We only observe graduation at the program j_i^{Enroll} where a student enrolls. Students who do not enroll have $P_{i|q}(Grad_i;\theta_G) = 1$ and are dropped from the likelihood.

Given the above likelihood structure and parameters θ, the posterior probability that an individual is of type q is:

$$\widetilde{P}_{q|i}(\theta) = \frac{\Pi_q(X_i;\theta_\pi)\, P_{i|q}(ROL_i;\theta_U)\, P_{i|q}(Grad_i;\theta_G)}{\sum_{r=1}^{Q}\Pi_r(X_i;\theta_\pi)\, P_{i|r}(ROL_i;\theta_U)\, P_{i|r}(Grad_i;\theta_G)} \tag{B.8}$$

EM Algorithm Maximizing this likelihood directly is computationally difficult. Arcidiacono and Jones (2003) show that maximizing this likelihood is equivalent to iteratively maximizing a *surrogate likelihood* that conditions on the posterior of unobserved types and subsequently updating the posterior. Given current parameters θ^{t-1}, this objective can be written as:

$$\mathcal{S}(\theta|\theta^{t-1}) = \sum_i \sum_{q=1}^{Q} \widetilde{P}_{q|i}(\theta^{t-1}) \cdot \log\left(\Pi_q(X_i;\theta_\pi)\, P_{i|q}(ROL_i;\theta_U)\, P_{i|q}(Grad_i;\theta_G)\right) \tag{B.9}$$

$$= \underbrace{\sum_{i,q}\widetilde{P}_{q|i}(\theta^{t-1})\log\Pi_q(X_i;\theta_\pi)}_{\mathcal{S}^\pi(\theta_\pi)} + \underbrace{\sum_{i,q}\widetilde{P}_{q|i}(\theta^{t-1})\log P_{i|q}(ROL_i;\theta_U)}_{\mathcal{S}^U(\theta_U)} + \underbrace{\sum_{i,q}\widetilde{P}_{q|i}(\theta^{t-1})\log P_{i|q}(Grad_i;\theta_G)}_{\mathcal{S}^G(\theta_G)} \tag{B.10}$$

This objective is additively separable in logs once conditioning on the posterior (all dependence between steps of the model is captured by types), which enables a sequential optimization of each step of the likelihood using the posterior from the previous step as weights. Specifically, at iteration t with current parameter guess θ^{t-1}:

1. *[E] Posterior*: At the current parameter θ^{t-1}, compute the posterior probability that student i is of type q given their applications and graduation outcome using Bayes' rule, $\widetilde{P}_{q|i}(\theta^{t-1})$.
2. *[M1] Update Type Priors*: Update θ_π using $\mathcal{S}^\pi$. This is a multinomial logit of types on covariates X_i where the posterior plays the role of the outcome.
3. *[M2] Utility*: Update θ_U using $\mathcal{S}^U$. This is an exploded logit where each student enters once

for every type q with weight $\widetilde{P}_{q|i}(\theta^{t-1})$.

4. *[M3] Graduation*: Update θ_G using $\mathcal{S}^G$. This is a simple weighted binary logit among enrollees, weighting by $\widetilde{P}_{q|i}(\theta^{t-1})$.

The posterior conditions on both applications and graduation, so the algorithm uses choices and outcomes jointly to "cluster" students into types. We use joint estimation for statistical efficiency, but do not condition on the observed graduation outcome when computing predicted graduation, including in the estimation of program preferences. In counterfactuals, we do condition on graduation when drawing each individual's simulated type, so that the simulated distribution of types matches the distribution in the data as closely as possible.

To speed up convergence, each of [M1] to [M3] is a single Newton iteration. We derive the analytic scores for the above surrogate objectives and use a Berndt-Hall-Hall-Hausman (BHHH) approximation of the Hessian based on the posterior-weighted outer product of the scores.

Starting Values and Convergence As is standard in the EM literature, we use multiple starting points and random initial posteriors to ensure convergence to a global maximum. We proceed in three steps. First, we estimate the model with no unobservables ($Q = 1$). In this case the likelihood separates into an exploded logit and a binary logit, both of which are globally concave and can be estimated separately. Second, we center the $Q = 8$ model around these estimates, adding a small amount of noise to starting parameters and start the algorithm from random posteriors. We run the EM algorithm from 20 of these random starting points. Third, as we found the best EM chains to reveal themselves quickly, we run these 20 starting points for 200 iterations and then refine the best 5 starting points until convergence, selecting the highest likelihood as our preferred estimate. We stop when an iteration improves the log-likelihood per student by less than 10^{-6}. We validate local identification by checking that the Hessian of the full likelihood is negative definite at the maximum.[41]

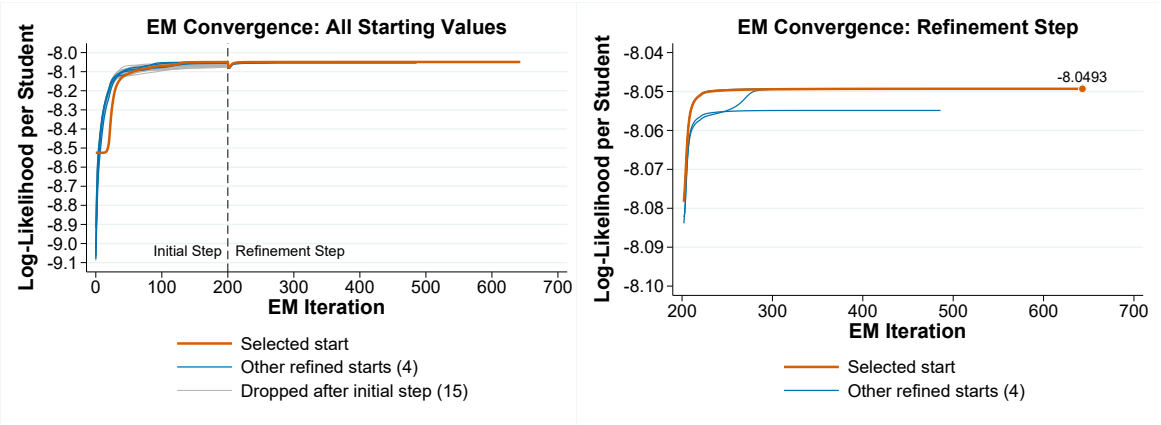
Figure B1 shows the log-likelihood per student along each EM chain. The likelihood barely moves over the first dozen iterations but then improves rapidly as the types separate. The selected start converges after 537 total iterations at -8.049, compared to -8.525 with no unobservables. Four of the five refined starts reach the same optimum.

Choosing the Number of Types We decided on 8 types based on runs of earlier versions of the model, which showed that BIC with an entropy penalty in the posterior type probabilities flattened out between 8 and 10 types. We set $Q = 8$ to balance flexibility in unobserved heterogeneity against computational cost and the precision of type-specific coefficients. Each additional type

[41] As the labeling of types is arbitrary, we order types from highest to lowest average ability A_i after estimation.

Figure B1: Convergence of the EM Algorithm across Starting Values

(a) All starting values (b) Refinement step



Notes: Log-likelihood per student at each EM iteration of the $Q = 8$ model. Panel (a) shows all 20 starting values. The dashed line marks the end of the initial step of 200 iterations, after which only the five starts with the highest likelihood are continued in the refinement step. Panel (b) shows these five starts during the refinement step. The brief decline at the start of the refinement step arises because posteriors are re-initialized and dampened for 20 iterations.

adds 32 parameters: 10 in the type priors, 11 in utility, and 11 in graduation outcomes. Moving from the model with no unobservables to eight types adds 224 parameters and lowers the BIC per student from 17.071 to 16.126.

Standard Errors We use the sandwich formula $\hat{V}_\theta = \hat{B}^{-1}\hat{M}\hat{B}^{-1}/N$, where $\hat{M}$ is the average outer product of individual scores and $\hat{B}$ is the Hessian.

B.2 Computational Details on the Graduation Decomposition

The decomposition in Section 4 requires each student's predicted graduation at every program in their admission cycle J_t, not only at the programs in their feasible set. We integrate over each student's unobserved type using the posterior implied by their characteristics and applications, *but not their graduation outcome*:

$$PrGrad_{ij} = \sum_{q=1}^{Q} \widetilde{P}^{ROL}_{q|i}\, PrGrad_{ij|q}, \qquad \widetilde{P}^{ROL}_{q|i} = \frac{\Pi_q(X_i;\theta_\pi)\, P_{i|q}(ROL_i;\theta_U)}{\sum_{r=1}^{Q} \Pi_r(X_i;\theta_\pi)\, P_{i|r}(ROL_i;\theta_U)} \tag{B.11}$$

Excluding the outcome keeps a student's realized graduation out of their own predicted outcomes, so the components only use information available at the time of application, which is also what programs observe when choosing their rules. Ability is then $A_i = \sum_q \widetilde{P}^{ROL}_{q|i} A_{i|q}$ with $A_{i|q} = \frac{1}{|J_t|}\sum_{j\in J_t} PrGrad_{ij|q}$, and ATE_j averages $(PrGrad_{ij} - A_i)$ over all N_t applicants in the admission cycle. By construction, ATE_j averages to zero across programs within a cycle.

We further split the match effect into observable and unobservable components, defining the

unobservable component as the additional information gained from self-revelation via the student's ROL. Since the posterior $\widetilde{P}^{ROL}_{q|i}$ incorporates information about outcomes observed from the ROL, both A_i and M_{ij} as defined above incorporate selection on unobservables. To separate observables from unobservables, we repeat the previous computations only using the student's observed covariates, X_i. In other words, we use the type prior. Let $M_{ij|q} = (PrGrad_{ij|q} - A_{i|q} - ATE_j)$ be student i's match effect at j *if they were of type* q. The observable match effect weights these by the prior which depends only on the observables X_i. The unobservable match effect is the remainder that arises solely through a student's self-revelation of their type through their rank-ordered list:

$$M_{ij} = \underbrace{\sum_q \Pi_q(X_i;\theta_\pi)\, M_{ij|q}}_{M^{Obs}_{ij}} + \underbrace{\sum_q \left(\widetilde{P}^{ROL}_{q|i} - \Pi_q(X_i;\theta_\pi)\right) M_{ij|q}}_{M^{Unobs}_{ij}} \tag{B.12}$$

For a student whose list is equally likely under every type, the posterior equals the prior and $M^{Unobs}_{ij} = 0$.

The variance decompositions in Table 4 evaluate these components at each student's program of enrollment. To account for uncertainty over types, we treat each student-type pair as an observation with weight $\widetilde{P}^{ROL}_{q|i}$, using the type-specific components $A_{i|q}$, ATE_j, M^{Obs}_{ij}, and $M_{ij|q} - M^{Obs}_{ij}$, which sum to $PrGrad_{ij|q}$ for every pair. This leaves means unchanged, while variances include the variation across types within a student. Program-level decompositions use enrollment-weighted variances of program averages of the same components.

Finally, we compute type-specific average treatment effects $ATE_{j|q}$, displayed in Figure 14. These quantities show what would be the causal impact of program j for someone drawn at random *from the pool of students of type* q. The standard ATE_j instead averages over the population. To compute this, we first compute the average outcome for students of type q at program j using the posterior as weights $\widetilde{P}^{ROL}_{q|i}$: $PrGrad_{j|q} = \frac{\sum_i \widetilde{P}^{ROL}_{q|i} PrGrad_{ij|q}}{\sum_i \widetilde{P}^{ROL}_{q|i}}$. The type-specific $ATE_{j|q}$ subtracts the average of this over all programs:

$$ATE_{j|q} = PrGrad_{j|q} - \bar{A}_q, \qquad \bar{A}_q = \frac{1}{|J_t|}\sum_{k\in J_t} PrGrad_{k|q} \tag{B.13}$$

B.3 Estimation of the College Model

This section provides details on the estimation of the model of programs' admission rules in Section 5. We estimate the model on programs' rule choices in 2013 and 2014, taking the estimates of the student model as given. The preferred specification in columns (7)–(8) of Table 6 has 20 parameters: 7 mean preferences for each of the public and private sectors (14 parameters) and 6

parameters in the Cholesky factor of the covariance of three random coefficients.

Rule and Admit Pools Programs choose among a discrete menu of admission rules available in each year, $r \in \mathcal{R}_t$. The set of possible rules consists of the 536 distinct combinations of weights on high school GPA, GPA+, and the PSU tests chosen by at least one program in 2013 or 2014. In 2013, every program was required to place a 10 percent weight on GPA+, restricting the menu of rules to 275 possible rules. Since competitors' rules are held at their observed values, we interpret each program's choice as a best response to the equilibrium in the data.

For each program j, year t, and rule r, we simulate DA, holding submitted applications and competitor rule choices fixed, and record the set of admitted students $\mathcal{A}_{jt}(r|r_{-j})$. The included covariates x_{jtr} are averaged over students in $\mathcal{A}_{jt}(r|r_{-j})$. Since programs do not observe graduation when they choose their rule, A_i and M_{ij} use the posterior in Equation B.11, which conditions on each student's characteristics and applications but not their graduation outcome.

Program Likelihood Program j of group $s(j)$ in year t chooses the rule $r \in \mathcal{R}_t$ that maximizes:

$$v_{jtr} = x'_{jtr}\beta_{s(j)} + w'_{jtr}\eta_j + \epsilon_{jtr} \tag{B.14}$$

where $\beta_{s(j)}$ are mean coefficients that differ between public and private programs, and $w_{jtr} \subseteq x_{jtr}$ are the covariates with random coefficients. The unobserved taste parameters $\eta_j \sim N(0, \Sigma)$ are constant across years. The error ϵ_{jtr} is distributed type I extreme value with unit scale, which we interpret as an optimization error. As a result, we do not add it to programs' payoffs in counterfactuals.

Since η_j is shared across years, the likelihood of the observed choices r^*_{jt} integrates over η jointly for 2013 and 2014. We compute this integral using Gaussian quadrature with 5 grid points in each dimension (125 total simulation points), with each simulation point $\hat{\eta}$ getting simulation weight $\omega(\hat{\eta})$. To operationalize this, we treat the elements of the triangular Cholesky factor L_η as unrestricted parameters and compute the variance-covariance of the random coefficients at evaluation, $\Sigma_\eta = L_\eta L'_\eta$. In addition, we weight each program's contribution by its total seats across both 2013 and 2014, w_j. The program likelihood is:

$$\mathcal{L}(\theta) = \sum_j w_j \cdot \log\left(\sum_{\hat{\eta}} \prod_t P_{jt}(r^*_{jt}|\hat{\eta};\theta) \cdot \omega(\hat{\eta})\right) \tag{B.15}$$

where the probability of program j in year t choosing rule r^*_{jt} given unobserved tastes η is the

standard multinomial logit probability:

$$P_{jt}(r_{jt}^* = r|\eta;\theta) = \frac{\exp\{x'_{jtr}\beta_{s(j)} + w'_{jtr}\eta\}}{\sum_{r'\in\mathcal{R}_t}\exp\{x'_{jt,r'}\beta_{s(j)} + w'_{jt,r'}\eta\}} \tag{B.16}$$

Note that we fix the scale of ϵ_{jtr} to one but do not need a location normalization as we do not include any alternative-specific covariates.

Starting Values and Convergence The random coefficients make the likelihood non-concave, so we again use 20 random starting values, taking our estimates from the starting point with the highest likelihood. We use Newton's method with analytic gradients and a BHHH approximation of the Hessian. As a check against local optima, after estimation we conduct a derivative-free one-dimensional search. At our final estimates the Hessian is negative definite, indicating local identification.

Standard Errors We again use the standard sandwich formula: $\hat{V} = \hat{B}^{-1}\hat{M}\hat{B}^{-1}$. Standard errors of the standard deviations and correlations of the random taste parameters follow from the delta method. These standard errors take the student model estimates as given, and so do not account for estimation error in x_{jtr}.

B.4 Counterfactual Simulations

The counterfactuals in Section 6 simulate assignments for the 2013 and 2014 admission cycles under alternate priority rules, holding each program's capacity fixed. The sample is every exam taker in 2013 and 2014, including those who do not apply but are eligible. This allows some extensive margin application response, but holds exam-taking behavior fixed. We report values averaged over 20 simulations.

We do not run any counterfactuals where programs need to choose alternate rules. As we estimate the model assuming programs are choosing their best admissions weights conditioning on competitors' choices, allowing a partial discretion equilibrium would require re-solving the equilibrium through iterated best response. As this is computationally demanding, we defer this for future work, and focus on counterfactuals that do not involve strategic interactions. To leverage the college preference estimates, we do run a full-discretion counterfactual, which assumes programs truthfully report their priorities. This is appropriate as there are no interactions between students, each contributing linearly to overall program payoffs.

Each simulation proceeds in three steps:

1. *Draw Persistent Unobservables*: First, draw each student's type q from their posterior $\widetilde{P}_{q|i}$ computed as in Equation B.8, which conditions on both their applications and graduation outcome in order to most closely mirror the distribution of types in the data. Second, draw each program's unobserved tastes η_j from its posterior over the quadrature nodes. Again, we use the posterior to most closely mirror the unobservables in the data, which can be computed using Bayes' rule: $\widetilde{P}_j(\eta) = \frac{\prod_t P_{jt}(r^*_{jt}|\eta;\theta)\cdot\omega(\eta)}{\sum_{\hat{\eta}}\prod_t P_{jt}(r^*_{jt}|\hat{\eta};\theta)\cdot\omega(\hat{\eta})}$.
2. *Simulate Applications*: Draw new preference shocks ϵ_{ijq} for every program in the student's admission cycle $J_{t(i)}$ and for the outside option. As we change admissions priorities, it is important to not restrict to feasible options and to not cap list length. Each student ranks every program they prefer to the outside option in order of utility.
3. *Run DA*: Assign students with the DA algorithm from our replication of the assignment mechanism, separately by year, under each priority rule:
   (a) *Baseline*, the observed admission index in the data.
   (b) *Gaokao/Test Only*, the average of math and language PSU, common to all programs.
   (c) *Lottery*, a uniform random number drawn independently for each student and program. Lottery numbers are redrawn on every simulation.
   (d) *Full Discretion*, the student's contribution to the program's payoff, $x'_{ij}b_j$, where b_j are observed and unobserved tastes.
   (e) *Predicted Graduation*, $PrGrad_{ij}$, which uses model-predicted graduation as a program's priorities. When computing this priority, we use the type-posterior conditioning on only observables and the ROL, $\widetilde{P}^{ROL}_{q|i}$, which is what programs and the social planner can observe at application. Note that this is not the graduation-maximizing allocation, but should come close.