The equity and efficiency effects of a relative GPA reward in college admissions

Tatiana Reyes*

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Abstract

Many college admissions systems use a combination of GPA and standardized test scores to determine access to more selective programs. In this paper, I study the impacts of a 2013 reform of the Chilean selective college admission system that introduced a third component, based on a student's relative GPA, designed to improve equity in the system. Simulating the admission mechanism with and without the relative GPA boost, I classify 2013 applicants into three groups: (i) those who gained access to more selective programs (pulled-up), (ii) those who lost access to more selective programs (pushed-down), and (iii) those whose admission was unaffected. Simulating the admission mechanism in earlier years with and without the same boost, I identify the groups who would have been pulled-up and pushed-down in those years, facilitating a difference-in-differences design to estimate the impacts of the relative GPA boost on enrollment, graduation, and earnings of winners and losers from the reform. As a result of the reform, pulled-up students shifted to programs with higher-performing peers and higher graduation rates, and experienced a large increase in the probability of graduating from a selective college program over the next 8 years, though no gain in BA completion. Pushed-down students, who tended to come from better-educated and higher-income families, experienced comparable-sized reductions in the probability of graduating from selective programs, offset by gains in graduation from less selective programs. My findings suggest that a targeted boost in admissions rankings based on relative GPAs can enhance the equity of a selective admission system without incurring large efficiency penalties.

*University of California, Berkeley. Email:tatiana_reyes@berkeley.edu. I am thankful to my advisors David Card, Jesse Rothstein and Christopher Walters for their support and guidance. I would also like to give special thanks to Hadar Avivi, Livia Alfonsi, Monica Saucedo, Damián Vergara, Pablo Muñoz, Harrison Wheeler, and Marina Dias for their thoughtful discussion of this paper at its earliest stage. This paper also benefited from discussion and suggestions from Sebastián Otero, Zach Bleemer, and the participants from the UC Berkeley Labor Seminar.

Introduction

The notion of higher education, especially at selective colleges, as a vehicle for upward social mobility makes the issue of access to these programs policy relevant (Autor, 2014; Chetty et al., 2017; Turner, 2020). Admissions policies at selective colleges tend to rely on a combination of standardized tests and high school grades, but the consistent evidence of disparities in test scores between students from different backgrounds has sparked a policy discussion regarding equity issues (J. M. Rothstein, 2004; Card & Rothstein, 2007; Zwick & Greif Green, 2007). Interventions in the admissions process, such as affirmative action and top-percent policies in the United States, are examples of the efforts to narrow admissions gaps between students from different backgrounds.[1] However, the potential effects of admission interventions on drop out, graduation, and post-graduation outcomes has raised concerns about a conflicts between equity and efficiency in the higher education system.[2]

An evaluation of the causal effects of changes in admission policies is particularly difficult in the case of highly selective programs with capacity constraints since any policy that raises the admission chances for one group necessarily lowers the chances for some other groups. The identification of the latter group requires a detailed model of the admissions system capable of precisely ranking students, and finding those that are pushed out when other students are pulled in to a program. Moreover, any evaluation requires a credible counterfactual for the outcomes of the winners and losers from an admission reform in the absence of the intervention.

This paper studies the direct and indirect effects of access-oriented admission interventions in higher education, extending the knowledge around admission to selective colleges. Specifically, I exploit a unique and comprehensive equity reform in the Chilean

[1]For California's "Eligibility in the Local Context" see Bleemer (2021), and S. E. Black et al. (2020) for Texas Top Percent Policy. For Brazil's affirmative action Otero et al. (2021) and Mello (2022) and Bagde et al. (2016) study an affirmative action policy in India.

[2]Dillon & Smith (2020) highlight this potential trade-off between equity and efficiency. In the case of California, Arcidiacono & Lovenheim (2016) find mixed evidence on the benefit of admission trough affirmative action. On the other hand Bleemer (2021) presents evidences that support that the benefit of more selective university enrollment is greater for affirmative actions under represented minorities enrollees

college admission system that incorporates a relative GPA measure (GPA$^+$) into the application process. The purpose of the reform was to augment the admission criteria with a performance measure that takes account of between-school differences and boosts the admission chances for good students from schools with relatively low standardized test scores. GPA$^+$ is based on the grades of a student relative to the historical distribution of GPAs at his or her high school and adds a positive boost to the GPA of students who score above the historical mean, with a maximum boost for those who score above the maximum past score at their school. The introduction of the this new measure into the admissions formulas used by different programs (which previously depended only on standardized test scores and GPA) implicitly created three groups of students: (1) those who were admitted to a higher-ranked program under the new formula (a group I call the "pulled-up"), (2) those who lost access to the program they would have been admitted to in the absence of the reform, and were instead admitted to a lower-ranked program (a group I call the "pushed-down"), and (3) those whose admissions outcomes were unaffected. Using the precise details of the centralized college admission system in Chile I am able to identify all three groups in the first year of the new system (2013). I am also able to identify the same three groups who would have been present if the reform had been adopted in 2012. I then conduct a simple difference-in-differences analysis of enrollment, persistence, graduation, and post-graduation outcomes, treating the pulled-up and pushed-down students as two treated groups and the unaffected students as a control group.

I find that the reform gave access to more selective college options to high performing students from lower income and less educated families. Getting access to these programs made pulled-up students more likely to enroll in a selective college, and made them more likely to attend programs where their classmates had higher test scores and higher high school GPA, as well has higher probability of graduating on time. Contrary to the mismatch hypothesis (Sowell, 1972), that states that low-test students targeted by access-oriented admission programs, like affirmative action, would be better off by attending programs where they match their peer characteristics, I find that the probability of graduation from the admission to a more selective program increases by 8 p.p. (36% increase in graduation) and preliminary results suggest that earnings also increase. For pushed-down students, the probability of immediate enrollment in selective college options decreases, but there is no change in the probability of college attendance. These students either enroll in the non-selective system or they have a late entry in the selective system (i.e, after the first year). Overall, the evidence confirms that the inclusion of the relative GPA measure to the college admission process increases admission equity without an efficiency loss.

Specifically, I exploit the variation generated by the relative GPA reform to identify the effects of access to better college options for students who didn't have those options in the past. Contrary to most papers that look at only one margin of selectivity, I look at the effects in the entire system, where a more selective program means that students are required better academic performance to be admitted than in a less selective program.[3] Additionally, I assess the indirect effect of losing access to better college options by estimating the effect on students who, as a consequence of the admission reform, were displaced from the selective system. Finally, the data allows me to track students through the entire college system (including the non-selective universities) to understand the ways that students respond to admission changes.

I use the rich information on preferences embedded in students' rank order lists and the transparency of the college admission rules to simulate students' assignment under the two admission regimes: pre-reform or status quo (SQ), and post-reform or GPA^+. The Chilean college admission system is a centralized system that used a Deferred Acceptance (DA) algorithm to match students to programs.[4] By replicating the mechanism, I simulate the counterfactual admission assignment that students would have gotten under the two regimes. This allows me to identify the set of students that could gain access to

[3] S. E. Black et al. (2020) look at enrollment in UT Austin with the implementation of Texas Ten Percent policy and Bleemer (2021) analyse the University of California system with the implementation of Eligibility in the Local Context.

[4] Admission is offered only to the most preferred program reported by the student for which they are eligible for based on their admission test scores and GPA. In each admission process enrollment can only occur at the admission program, even if the student is eligible for other less selective options listed as less preferred. Other centralized college admission system that use a similar system to match students to programs are Norway, Denmark and Turkey.

a better program with the reform - pulled-up - and those who could lose access to the programs that they used to access - pushed-down.

To estimate the causal effect of the reform on pulled-up and pushed-down students, I use a difference-in-differences design in which the dependent variables capture human capital acquisition and labor market outcomes. Simulating the admission mechanisms in earlier years, before the implementation of the reform, I can identify the students who would have been pulled-up or pushed-down in those years. Then, a difference-in-difference strategy allows me to compare students in the same pulled-up or pushed-down group, before and after the reform was implemented. In order to control for time changes I also use the information from students unaffected by the reform. The variation induced in the outcomes of interest by the implementation of the reform is unrelated to unobservable characteristics that also determine the outcomes, allowing for a causal interpretation of the effect of giving access to more selective programs.

The primary identification assumption is that, in the absence of the reform, the trend in the outcomes of interest for pulled-up, pushed-down, and unaffected groups would have evolved similarly - also known as parallel trends. In order to check this assumption, I conduct a placebo exercise; following the same strategy to classify students into the three relevant groups I use the same difference-in-differences specification for cohorts for which no reform was implemented (2011 and 2012). I find no significant difference between them when no reform is implemented.

For the identification of the treatment groups the main assumption is that the rank order list reported by students doesn't change with the incorporation of the relative GPA measure in the application score. Without restrictions in the report of preferences, the dominant strategy for the DA algorithm is to report preferences truthfully (Gale & Shapley, 1962; Roth, 1982). I assume that student preferences over programs don't depend on the inputs used by the assignment mechanism (test scores, GPA and GPA$^+$), and I use the fact that most students' list fewer than the maximum number of choices (Haeringer & Klijn, 2009; Pathak & Sönmez, 2013) in order to support the use of the reported rank order list to estimate the counterfactual assignment. However, a more recent literature has suggested a reporting behavior dependant on the feasibility of the options (Fack et al., 2019; Larroucau & Rios, 2018). Because the introduction of the boost could have changed those options for student with a high boost I test the sensibility of my results to samples without extreme boost values. Additionally, I estimate my results under alternative assumptions for inference, i.e., clustering at the school-year level. In both exercises - change of sample or assumptions - I find the same qualitative and quantitative results.

The distinctive setup for my difference-in-differences (not a panel) and the lack of further comparable periods pre-reform make it challenging to test the identification assumptions for this empirical strategy. Using an alternative research design that allows for the direct test of the identification assumptions like the regression discontinuity (RD) design, I examine the impact of getting access to the most desired program in a cross-sectional setting before the implementation of the reform. Even though the RD design identifies the effect of gaining access to the most preferred program for the marginal student, which mixes high and low GPA scoring, income, and test scoring students, the values are comparable in sign and magnitude to my diff-in-diff estimates, when the margins of treatment (fall back options of the students) are considered. Students who gain access to their most preferred option are 17.2 p.p. more likely to enroll and 8.2 p.p. more likely to graduate from their admission program.

My results also align with the results from other equity admission interventions that find that access-oriented admission policies at selective universities can promote economic mobility without efficiency losses (Otero et al., 2021; Bleemer, 2021; S. E. Black et al., 2020). Consistently with the results reported in S. E. Black et al. (2020) for the Texas Top Percent policy, I find similar graduation rates (inferred) for pulled-up students than for the average students pre-reform, suggesting that pulled-up students didn't struggle more.

This paper contributes to the understanding of equity admission interventions and the effect of admission to more selective universities for students who would not normally have access to them (S. E. Black et al., 2020; Bleemer, 2021, 2022; Arcidiacono & Lovenheim, 2016; Arcidiacono et al., 2016; Otero et al., 2021; Mello, 2022; Bagde et al., 2016). I build on prior empirical research employing a difference-in-differences approach, and take advantage of the transparency of the admission criteria in order to precisely identify the treatment groups resulting from the admission reform. Unlike earlier studies, this admissions change affected the full spectrum of selective colleges, not just the access to a single institution. Thus, I study the effect in the entire population of applicants and on the entire college system (selective and non-selective institutions). This paper also contributes to the literature on the mismatch hypothesis by providing evidence against it in an environment that stands out as suited for evaluating it.[5] This paper also contribute to the early, but growing, literature that evaluate changes in the assignment mechanism, in this case which inputs are used, on the basis of students outcomes (Agarwal et al., 2020; Otero et al., 2021; Larroucau & Rios, 2020).

The rest of the paper is organized as follows. In Section 1 I discuss the literature closely related to this paper; Section 2 outline the features of the Chilean setting, the policy and the data sources; Section 3 present the empirical strategy, which is divided into two subsections: (1) the identification of the treatment groups, and then, (2) the discussion of the difference-in-differences design to estimate the treatment effect of the reform on those groups. Section 4 present the results for enrollment, graduation and earnings; the effects of the reform on STEM applicants and the evaluation of the mismatch hypothesis are also discussed in this section, as well as a series of robustness checks. Section 5 presents an alternative design to validate the results presented in Section 4, and Section 6 concludes.

1 Related literature

There is a significant body of literature devoted to studying the returns to college. In particular, Dale & Krueger (2002); D. A. Black & Smith (2004); Lindahl & Regnér (2005);

[5]This literature focus on the potential negative effects of college selectivity on students admitted trough alternative mechanisms to academic performance. Several papers study the mismatch hypothesis with varying results, see for example Sander & Taylor (2012); Arcidiacono & Lovenheim (2016); J. Rothstein & Yoon (2008); Bleemer (2022); Arcidiacono et al. (2011)

Dale & Krueger (2014) highlight the difficulties of deriving causal estimates about the returns to college from observational data based on varying levels of quality or selectivity. Recently, numerous studies have used a regression discontinuity strategy to adjust for selection bias and have shown that applicants at admissions thresholds gain from admission (e.g., Hoekstra (2009); Zimmerman (2014); Anelli (2020)).[6] In addition, Cohodes & Goodman (2014); Goodman et al. (2015); Zimmerman (2014) present evidence from the United States that attending a selective university tends to increase graduation rates. In conclusion, the majority of evidence suggests that college quality has a beneficial impact on student performance, although this result is not universal. However, these methods may be inadequate for evaluating the effectiveness of access-oriented policies. Students at the margin may differ from those who are targeted. Dale & Krueger (2014) provides evidence of heterogeneous returns to selective degrees in the United States - positive for underrepresented groups but zero on average - by analyzing the differences in outcomes for students with similar sets of admission offers but different enrolment decisions. Zimmerman (2019) and J. Hastings et al. (2009) document heterogeneous effect for the case of Chile in terms of field of study and family income.[7] Additionally, treatment effects for those outside of the discontinuity may vary.[8]

The relationship between selectivity and outcomes for the two affected categories of students, those pushed into more elite programs and those displaced from more selective schools, is particularly relevant to the question of access-oriented policies. It will be beneficial if institutions with a greater level of selectivity have more and better learning materials. In contrast, it can be negative if students increase their likelihood of poor performance and school withdrawal. In the situation of differential impacts, student-resorting policies have the potential to generate both efficient and equitable benefits or

[6] Another body of research focuses on the differential returns to fields of study; see for example Kirkeboen et al. (2016) and J. Hastings et al. (2009)

[7] Zimmerman (2019) argues that the greatest returns to top business program attendance in Chile apply only to students from high-income families. Compared to J. Hastings et al. (2009), my regression discontinuity analysis provides larger results. This discrepancy is expected since I only examine threshold crossing for the first choice, which results in greater effects than other threshold crossings. Prior studies averaged across all thresholds.

[8] This justifies the choice of my main difference-in-differences specification. Otero et al. (2021) overcomes this challenge with a combination of admission thresholds and an exogenous score shifter.

costs.

My work contributes to the literature on the effects of access-oriented policies on selective colleges by looking at admission and enrollment, as well as medium-term outcomes such as dropout, college graduation, and earnings. I expand upon the research that employs differences-in-differences to examine the consequences beyond the admissions threshold. My paper is most closely connected to S. E. Black et al. (2020), however, I take advantage of my setting to construct the treatment groups intuitively and transparently. In addition, my study analyzes the entire college system.

The research on access-oriented policies focuses mostly on affirmative action and Top N percent programs (Arcidiacono et al., 2011; Arcidiacono & Lovenheim, 2016; J. Rothstein & Yoon, 2008; Bleemer, 2022; Otero et al., 2021; Mello, 2022; Bagde et al., 2016; S. E. Black et al., 2020; Bleemer, 2021; Kapor et al., 2020). The "percent plans" implemented in Florida, California, and Texas ensured admission to the public university systems for students with high grades compared to their high school peers, independent of their standardized test scores. The Chilean reform is similar to these policies in that it increases the likelihood of admission for students with strong grades and is demographically blind. However, there are numerous significant distinctions. The Chilean reform did not ensure access but rather increased the likelihood. Related to this, another distinction is that the Chilean reform compares current students to prior students from the same school, whereas the percent plans compared students from the same cohort. A further advantage of the Chilean context is the transparency of admission rules. The majority of the college admission systems in which access-oriented policies have been studied have some arbitrary component or they are structured in such a way that students could behave strategically to take advantage of changes in the admission policies. Cullen et al. (2013); Estevan et al. (2017); Mello (2021) analyze the school switching behavior for the US and Brazil.Concha-Arriagada (2022) shows that this occurred in Chile during the second and third years following the implementation of the reform but the problem was quickly resolved in 2016. My analysis can be extended to years after this issue was rectified; however, there is insufficient data on outcomes to add to the analysis at this time.

Much of the research on affirmative action has centered on measuring academic mismatch. The mismatch hypothesis posits that graduation rates for minority students who attended selective post-secondary institutions would be lower than for those who attended colleges and universities where their academic credentials are better matched to the institutional average. However, results have not being conclusive (Loury & Garman, 1993; J. Rothstein & Yoon, 2008; Sander & Taylor, 2012; Dillon & Smith, 2017, 2020; Arcidiacono et al., 2011, 2014; Bleemer, 2022, 2021). Similar to Bleemer (2021) for the case of California, I find that the benefits of more-selective enrollment are at least as large for high-GPA students whose low standardized test scores would have normally disqualified them from selective universities as they are for the higher-standardized test students admitted to those universities and that the graduation rate for the pulled-up students was roughly equivalent to the average for the non-affected students.

A closely connected literature evaluates the mismatch in the particular subgroup of STEM programs Arcidiacono et al. (2016); Bleemer (2021). The STEM mismatch hypothesis holds that students admitted through access-oriented policies are less persistent in STEM fields than they would be at universities with fewer admission requirements. Contrary to what previous studies show (Arcidiacono & Lovenheim, 2016; Bleemer, 2022; Mountjoy & Hickman, 2021), the evidence for the Chilean case suggests that students pushed into more selective STEM programs by the reform have a higher probability of graduation.

Lastly, this paper also relates to other studies interested in the same admission reform to answer different questions. The most related paper, Larroucau et al. (2015) evaluates the compositions of the students affected by the reform using the same simulation approach as this paper. Concha-Arriagada (2022) also relies on similar simulations to study the strategic behavior of students in 2015, after students learn about the construction of the relative GPA boost and before the policy was fixed to address the strategic behavior. In a similar spirit, Fajnzylber et al. (2019) evaluate the effects of the reform in terms of the GPA inflation and learning effort. Finally, Larroucau & Rios (2018) use the variation from 2013 to 2014 in the weights associated with the relative GPA component to estimate models of preferences for program choices.

2 Context

The Chilean test-based meritocratic college admission system is an ideal setting to evaluate the effects of an access-oriented admission intervention like the 2013 reform. It introduced a new component (the relative GPA measure), based on the student's relative GPA, designed to improve equity in the system. The transparency of the system, together with the availability of rich administrative data allows for the simulation of admission offers with and without the new GPA$^+$ component even in years before the reform was implemented, facilitating the construction of meaningful counterfactuals for winners and losers of the reform.

2.1 Chilean College Admission System

The admission process to selective universities in Chile is a centralized score-based meritocracy, based solely on standardized admission test scores and the high school GPA score of the students. The assignment mechanism - that uses a deferred acceptance (DA) algorithm- generates a seemingly strategy-proof environment and can be replicated when admission preferences, program vacancies and applications scores are available. I discuss with detail this two key characteristics to the implementation of my empirical strategy, particularly to the identification of the two treatment groups.

The college system and application procedure The Chilean college system has selective (public and private) and non-selective (private) colleges.[9] To enroll in a selective university students have to (i) graduate from high school, (ii) take the standardized admission test at the end of the academic year, and (iii) submit a rank ordered list

[9] In 2012 and 2013 the selective system was composed by 33 universities, which represented around 60% of college students.

of their preferences to the centralized admission system after learning about their test results. This process happens once-a-year and students can enroll only if they get an admission offer. To enroll in a non-selective college, students have to apply directly and follow the requirements of each institution.[10]

An important difference from other college systems is that it is organized around programs, instead of majors and universities. Programs have a highly fixed curriculum (which makes switching programs without going again through the application process hard and not common) with expected times for graduation between 4 to 7 years (5 being the mode). In most programs, students earn an academic degree after 4 years but they are required to attend a 5th year and pass a licensing exam to earn their professional degree and complete graduation. Programs provide the complete certification for most occupations, such as architecture, law, or medicine. This characteristic of the Chilean college system makes the relationship between college and labor market outcomes tighter compared to other settings.

The centralized admission process was established in the late 1960s in combination with a new admission test (in the same spirit as the SAT) and a single-offer assignment mechanism based on a student-proposing deferred acceptance (DA) algorithm (Gale & Shapley, 1962; Abdulkadiroğlu, Pathak, & Roth, 2005, 2009). Its development and implementation in the country was led by Erika Grassau.[11] The admission tests were redesigned at the beginning of 2000s and consist of a mandatory math and verbal exam, and one additional exam that could be science or history. Tests are taken simultaneously at a national level by the end of the academic year.[12] After scores are published (tests and GPA scores), students can start their application - exclusively online through the Department of Evaluation, Measurement and Educational Registration (DEMRE for its acronym in Spanish) website and without any monetary cost - by submitting a list with no more than

[10]In most of the cases colleges require the admission test score but don't set minimums for admission. Therefore, the restriction is a budgetary constrain.

[11]It is surprising the lack of recognition given to Erika Grassau and her team in charge of implementing that reform, considering how ahead of time it was when compared with the boom of the implementation of DA mechanisms in the last decade.

[12]The Chilean academic year normally goes from March to December, but it is shortened to November in the last high school year

ten programs, ranked in strict order of preference (their Rank Order List - ROL).[13] Once the application period is finished, the mechanism assigns students to schools using the deferred acceptance (DA) algorithm (Gale & Shapley, 1962; Abdulkadiroğlu, Pathak, & Roth, 2005).

Participation in the admission process is the only channel for students to enroll in any selective program.[14] Because students with higher application scores are more likely to be offered admission to a program than a student with a lower application score, and selection can only be based on that, it is considered a score-based meritocratic system. A program is considered more selective than others if the application score of the last student admitted - the program cutoff score - is higher. The application score is a weighted average of students' high school GPA and standardized test scores.

Deferred acceptance algorithm The Deferred Acceptance (DA) algorithm is the assignment procedure used to match students to programs, taking into consideration their preferences and the program vacancies.[15] The algorithm can be described as follows: In the initial step, each student proposes to their most preferred program listed in their ROL. Programs provisionally accept students based on their application scores until they fill their total number of seats, rejecting the rest. In subsequent cycles, rejected students propose to their most-preferred program among those that have not previously rejected them, and programs reject provisionally accepted applicants with lower application scores. This process iterates until all students are assigned to a single program or all unassigned students have been rejected by every program they have ranked. See Rios et al. (2021) for a thorough description.

A studied theoretical characteristic of the DA mechanism is that it is strategy-proof,

[13] To help applicants in their decision-making, DEMRE distributes a directory that provides an overview of the university admission process, key dates, information about vacancies, extra requirements, and the application score formula for each program for each university. While waiting for their results students can access a simulation mode site with a help video that explicitly states "when selected in one of the preferences all the following ones are eliminated, therefore it is very important the strict order of preferences from higher to lower personal interest."

[14] There are some special admission channels like switching students or students with disabilities but among those quotas admission score is always the selection criteria. This paper focuses on the regular admission channel.

[15] The variant of the student-proposing DA algorithm used by DEMRE establishes that all tied students for the last seat of a program must be admitted.

which makes reference to the fact that listing programs in order of true preferences is a weakly dominant strategy when students are allowed to rank every program, i.e. it cannot be manipulated by misrepresenting preferences (Dubins & Freedman, 1981; Roth, 1982). In the Chilean case, students are constrained to list only 10 choices, with extra conditions for some universities.[16] Table 1 shows that 90% of applicants rank less than 10 programs with a mode of 3, in which case truthful reporting is a dominant strategy (Haeringer & Klijn, 2009; Pathak & Sönmez, 2013). Assumptions over the rank order list and details about the assignment mechanisms are used to simulate admissions with and without the relative GPA measure. Section 3.1 discuss this procedure.

2.2 Relative GPA Reform

The relative GPA reform provides the variation needed to study the effect of giving access to more selective programs to students who normally would not have access to them. The reform created a grade-based measure in the context of a meritocratic admission system, therefore a demographically neutral access-oriented intervention, which could be desirable in contexts where race or gender policies are restricted. The relative GPA measure (GPA^+) increases the application score through a boost, and makes students with good performance at their high schools more competitive ($GPA^+ = GPA +$ relative boost). The way that the boost is constructed ensures that all well-performing students get a boost, but students from more disadvantaged schools -lower average GPAs- have a higher boost.

Equity concerns around college admission in the 1960s are what motivated the current admission system (meritocratic and transparent). Around the 2000s the admission test was changed in order to address socioeconomic differences in college admission but the socioeconomic gap in test scores persisted, even after controlling for income and parents' education. This evidence fueled a public debate that highlighted the need for a system able to identify high-ability students even when education conditions for them were not

[16]Universidad de Chile and Pontificia Universidad Catolica de Chile limit the applications to their programs, in order to be valid, to the first 4 preferences. For details analysis on how this could affect the report of preferences see Lafortune et al. (2016)

optimal to perform well in standardized test scores.

In the second half of 2012 academic year, the organization in charge of coordinating selective universities (CRUCH for its acronym in Spanish) informed the incorporation of a third element to calculate students' application scores in the 2013 admission process. The timing was such that students and programs had no scope for strategic responses, as students already have their GPA scores determined and universities have already made their capacity decisions.[17] Before the reform, application score (s_{ij}) for a student i to a program j was calculated as:

$$s_{ij} = \alpha_j \text{Tests Scores}_i + \beta_j \text{GPA}_i$$

The weights α_j and β_j were chosen by the programs under some minimum restrictions defined by the DEMRE such that $\alpha_j + \beta_j = 1$.[18] After the reform was implemented, the GPA^+ measure was included in the formula

$$s'_{ij} = \alpha'_j \text{Tests Scores}_i + \beta'_j \text{GPA}_i + \gamma'_j \text{GPA}^+_i$$

with $\alpha'_j + \beta'_j + \gamma'_j = 1$. For its first year, γ'_j was fixed at a mandatory 10% for all the programs. From Figure 1 we can see that most of the programs opted for reducing the weight on β_j to allocate the 10% for the GPA^+ measure, therefore most of the variation observed in allocations comes from the introduction of the relative boost.

The proposed new component was designed to make more competitive the application of students that performed well at their high school by awarding them a boost to their GPA score if they perform above their school average ($\text{GPA}^+ = \text{GPA} +$ relative boost). In Chile, grades are not fully curbed and they have an implicit reference to the minimum content expected by the national curriculum on each subject by year. Due to this, even the best student from a disadvantaged school that struggles to cover the minimum contents can have a very low GPA score. The GPA^+ component was designed

[17] The literal translation of the reform's name is "Ranking", which is misleading. Given that the score is assigned in relationship with the student's educational context rather than their class ranking, I will refer to it as relative GPA reform rather than Ranking reform.

[18] With a minimum 10% in each of the component.

such that with the boost, students that perform at the top of their school GPA distribution have a GPA$^+$ score that corresponds to that. By making the application score of good-performance students higher, the reform helped them access programs that would have rejected them when their application score was lower.

Relative GPA measure in detail The relative GPA (GPA$^+$) measure is based on the GPA score of the student, but it is adjusted with a boost that depends on the historical average ($\overline{GPA}$) and the historical maximum high school GPA of their high school ($\max GPA$). The historical average and the historical maximum are constructed based on the high school GPAs of the students from the previous 3 cohorts at that school. It was chosen as a reference for the within-school measure to avoid within-classmates' competition. The formula to calculate the (GPA$^+$) score is the following

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

Students with a GPA equal to or lower than the historical average at their schools have a relative GPA score equal to their GPA score. Students with a GPA bigger than the historical average but smaller than the historical maximum get their GPA score plus a boost that is determined by the slope of the line that connects the historical average GPA score with the historical maximum, which is for all schools the maximum possible score, 850.[19] This implies that students in this range, from a school with a more spread out high school GPA distribution will have a smaller boost in terms of score points for each extra point in their GPA. Finally, students that perform above the historical maximum at their high school get the maximum possible score (850), even if the GPA is, measured in application points, very low.

[19]Figure 2 correspond to an example to represent the relationship between GPA, GPA$^+$ and the boost.

In order to simulate the admission assignment under the the new mechanisms defined by the inclusion of the GPA$^+$ for cohorts previous to the implementation of the reform I construct the GPA$^+$ measure for the cohorts 2009 to 2012. According to the reform, students who graduate from cohorts before 2009 or students who didn't attend a school had the relative GPA score equal to their GPA score.

2.3 Data

I focus my analysis on the entire universe of applicants to selective universities during the years 2012 (pre-reform) and 2013 (post-reform). For the first part of the empirical analysis, I construct a unique dataset that replicates college admission offers with and without the inclusion of the relative GPA measure in the admission process. This allows me to classify students into one of the three possible groups of analysis: pulled-up, pushed-down, or unaffected. To assess human capital acquisition, I supplement these statistics with annual enrollment and graduation rates from selected and non-selective colleges for all the applicants to the 2012 and 2013 process. Finally, I add to the analysis information on employment and earnings on the private labor market up to 10 years following their application.

Admission process The relative GPA reform was implemented in the admission process of 2013. For that reason, my analysis focuses on the short and medium-long-term outcomes of all the students that participated in the admission process that year and the year before (2012). I use information from students in the 2011 cohort to validate my research design strategy.[20] The first part of the empirical analysis leads to the classifications of students in each cohort in one of the three groups of my analysis: pulled-in, pushed-out, and unaffected. Data on application preferences and application scores under status quo (pre-reform) and under the relative GPA (post-reform) regime are the main

[20] Even though information for later cohorts is available I don't consider it in my analysis because my empirical strategy is sensitive to the strategic behavior observed during those years. After 2013, some students switched schools in their last year of high school to improve their GPA$^+$ measurement. This potential for policy manipulation was fixed in the 2015 process.

requirements for this procedure.

Administrative data at the student level from the admission process was shared upon request by DEMRE. It consists of socioeconomic and demographic information of applicants (gender, date of birth, self-reported family income, and parents' education), applications scores (tests scores, GPA, and relative GPA score), high school characteristics, application information (rank order list of program preferences listed in the application with their final status: valid/invalid, offer/no offer and waitlist), and enrollment information (program, application score, and ranking of preference). This information is mainly used to simulate students' admission under a mechanism that uses two (test scores and GPA) or three (test scores, GPA and GPA^+) inputs to calculate the application score.

The "new" mechanism incorporates the relative GPA measure (GPA^+) into the application score formula. To compute the relative GPA measure for cohorts before the reform I use information from the national school records on high school performance for the entire population of high schoolers between 2002 and 2011 which is available online at the data platform of the Department of Education.[21] I compute the historical average and the historical maximum GPA at each school for each graduation cohort, and then the relative GPA score for students who graduated between 2008 to 2012 in the 2011 and 2012 admission process.[22] Figure 3 shows a binscatter graph with the boost score - i.e. the extra score relative to GPA- of the relative GPA score for students in application cohorts 2011 to 2013. The x-axis is the GPA score of the student minus the historical average high school GPA at the school of the student, therefore on the positive numbers we see the boost score in application points. Note that 2013 data is directly reported by DEMRE and 2011 and 2012 was calculated using the relative GPA score formula.

I also constructed a dataset with program characteristics like application score weights, application score restrictions, and the total number of seats from the public newsletter with the official information. Application score weights are required to calculate the application score under the two regimes. For each program, application scores under the

[21]`https://datosabiertos.mineduc.cl`

[22]Students can participate in the admission process as many times as they want. The proportion of freshmen and older applicants is around 60% to 40% in each cohort.

status quo regime (s_{ij}) are calculated using weights from the 2012 process, and application scores under the GPA$^+$ regime (s'_{ij}) are calculated with 2013 weights.[23]

Enrollment and graduation outcomes To measure the effect of the reform on educational outcomes I track all the students that participate in the application processes of 2012 and 2013 using yearly information on enrollment and graduation provided publicly by the Department of Education. From the admission data I can observe who got an admission offer and to which program. I create variables to indicate if a student enrolls in their admission offer or if they enroll in a non-selective college instead. By using the enrollment file in the second year ($t = 2$) I can check if the student persisted at their admission offer, if they re-apply or switched to a different selective program, if they switched or persisted in a non-selective college, or if they dropped out of college.

Additionally, for each application cohort, I track graduation by 6th, 7th, and 8th years after application because yearly graduation files were available only up to 2020. I construct 3 graduation measures: (1) program graduation or graduation from the admission offer in 2012 or 2013, (2) graduation from some selective university to take into account that students that don't get their desired admission may switch or re-apply in the following years, and (3) graduation from a non-selective college which is always an alternative. Having access to data of the entire system allows me to measure the complete impact of the reform in the selective system - the one that DEMRE attempt to coordinate-, as well as the impact on the entire college system.

Labor market outcomes To study the effect on earnings of giving access to better programs to students that normally couldn't access them I use information from the Unemployment Insurance (UI) data. The UI data has information on all the dependent workers over 18 years old that participate in the private sector.[24] All the information

[23]Music, arts, and acting programs require an additional aptitude test, which score is not reported separately in the data. For those cases, the application score used for the alternative regime was the same as the one reported originally.

[24]Data excludes: (i) workers subject to an apprenticeship contract; (ii) workers under 18 years of age; (iii) private home workers (until October 2020); (iv) pensioners; (v) independent or self-employed workers; and (vi) public sector workers. In a future version of the research, I will be able to include information on public sector workers and person-level data.

is aggregated at the treatment group level. For pulled-up, pushed-down, and unaffected students I observe the fraction that was present in the labor market (participation) and bins for their monthly taxable income from 8 to 10 years after the admission process.

3 Empirical Strategy

The empirical strategy is divided in two parts. First, I simulate the admission mechanisms with and without the relative GPA measure. I classify students into 3 groups based on the admissions simulations: (i) pulled-up, students who gain access to more selective admissions when the third component is considered in the assignment mechanism, (ii) pushed-down, students who loss access to more selective programs with the new mechanism, and (iii) unaffected, students whose admission options are unaffected by the change in the mechanism. By simulating the admissions under the two mechanisms in earlier years, before the reform was implemented, I can identify the groups who would have been pulled-up and pushed-down in those years. This facilitate a difference-in-differences design to estimate the impact of the inclusion of the relative GPA on enrollment, graduation and earnings for the students affected by the reform.

3.1 Identification of treatment groups: pulled-up, pushed-down and unaffected

The inclusion of the relative GPA measure into the admission process enhanced the equity of the college admission system. Students with relatively low test scores but high GPA from low-educated and low-income families got admissions into more selective program when the third component (GPA$^+$) was considered. There is also a higher representation of females in the pulled-up group of students. Pushed-down students tend to be in higher proportions from private schools, males, and from highly educated and high income families. In terms of the characteristics of the changes in the admission offers induced by the reform, most students affected had an admission one preference up or down with respect to the status-quo regime and most students get a new admission in the same field.

Simulation of the admission mechanism The relative GPA reform impacted the way that students were matched to the programs that they apply. Before its implementation the application score for a student i, applying to a program j was calculated using only 2 inputs: admission test scores e_i and GPA score g_i. With the implementation of the reform the new application score was calculated based on $s'_{ij}(e_i, g_i, c_i)$. Denote $\mu(\cdot)$ as the matching function defined by the mechanism that uses a Deferred Acceptance algorithm, the information from the pool of applicants, the application scores defined by the programs and the capacity restrictions of the program. The change in the inputs used by programs to evaluate students defines a new mechanism $\mu'(\cdot)$.

A student i can be characterized by $\theta_i(\succ_i, e_i, g_i, c_i)$ composed of their rank order list ($\succ_i$) and their scores. In each application year, for some students the admission assignment under both mechanisms will differ, $\mu(\theta_i) \neq \mu'(\theta_i)$, and for others it won't $\mu(\theta_i) = \mu'(\theta_i)$. I classify the pool of applicants into 3 mutually exclusive groups:

- Pulled-Up: $PU_i = 1\{\mu(\theta_i) \prec \mu'(\theta_i)\}$ students who get access to a program ranked higher in their list with the new mechanism μ' than with the old mechanism μ.
- Pushed-Down: $PD_i = 1\{\mu(\theta_i) \succ \mu'(\theta_i)\}$ students who get access to a program ranked lower in their list with the new μ' than with the old mechanism μ.
- Unaffected: $C_i = 1\{\mu(\theta_i) = \mu'(\theta_i)\}$ corresponding to students with access to the same programs with and without the inclusion of the GPA$^+$ measure.

Implementation of admission simulations For each student, in each application process, I start by computing their alternative application score. For students pre-reform this also includes computing the GPA$^+$ measure. For each program that the student listed, I use the weights from 2012 and 2013 to calculate the alternative application score (for students in the 2012 cohort I calculate s'_{ij} and for students in 2013 I compute s_{ij}).

I replicate the DA algorithm to simulate the admission assignment of students with the GPA$^+$ measure for pre-reform students ($\hat{\mu'}(\theta_i)$), and without it for post-reform students ($\hat{\mu}(\theta_i)$). In order to test the quality of the replication I simulated the admission assignments using s'_{ij} for cohort 2013; I replicate 99.9% of the real assignment offers.

For each student in application cohort 2012 or 2013, I compare the simulated with the real assignment offer and I classify them into the pulled-up (pushed-down) group if the admission assignment with the GPA^+ measure was higher (lower) in the list than the assignment without it. Students are classified as unaffected if the admission program under both regimes is the same.

Simulation assumptions There are three main assumptions needed for the simulation to be valid as a counterfactual under the alternative mechanism.

Assumption 1 *The rank order list of preferences that the students submit would have been the same with and without the reform*

Assumption 1 has two components, one that refers to the stability of preference and one that refers to the reporting behavior. I assume that preferences are stable with respect to the reform, which means that the indirect utility associated with each program does not depend on the components and weights used by the programs to evaluate applicants.

In terms of reporting behavior, I use the traditional approach taken by the literature that establish that without restrictions on the number of applications, the dominant strategy with a Deferred Acceptance (DA) algorithm is truthful reporting (Gale & Shapley, 1962; Dubins & Freedman, 1981; Roth, 1982). As most of centralized admission system, the Chilean application system restrict the application list (up to 10 options), however, because more than 90% of the students list fewer than 10 options, the restrictions can be interpreted as not binding (Haeringer & Klijn, 2009; Abdulkadiroğlu & Sönmez, 2003; Abdulkadiroğlu, Pathak, Schellenberg, & Walters, 2020).

One possible concern with respect to the reporting behavior arise from the most recent literature on mechanisms design and their interest on properly using the information from the centralized admission systems to estimate school choice demands (Agarwal et al., 2020; Fack et al., 2019; Larroucau & Rios, 2018). One way of rationalizing the fact that students don't fill up their application options relates to the idea that reporting behavior is based on students' feasible options. This behavior may violate assumption 1 if students that observe the boost (that potentially could increase their feasible options) reacted by adding more selective programs to the top of their list. This would create a problem in the identification of the treatment group if students get admitted to this added programs but similar students that didn't observed the boost (cohort of 2012) didn't get admitted under the simulation (because they didn't list the new options).

To assess this potential threat I first compare the number of admission options listed in 2012 and 2013 by students with a boost (by adding a program to the top of the list, the total number could increase). Students that observe the boost in 2013 are not more likely to have longer application lists than students with the same calculated boost but who didn't observed it (cohort of 2012). Additionally, I check the selectivity of the most preferred program or top ranked program of students with a boost, in 2012 and 2013. Figure 4 show that the selectivity of the first option increase in 2013 only in the highest values of boost score. In order to check for the sensitivity of the results I estimate the results without students with more than 150 points in their boost score (2% of the total sample). As discussed in Section 4.4, results don't change qualitatively or quantitatively with this sample restriction.

Assumption 2 *The number of available seats per program each year would have been the same with or without the reform*

Assumption 3 *Test scores and GPA scores would have been the same with and without the reform*

Assumption 2 and 3 are justified by the fact that the reform was announced in the last half of the academic year. At that point, universities have already made their capacity decisions and students' average GPA from the 4 year of high school was already determined, therefore there was no scope for strategic responses.[25]

[25] After the first year, there is some evidence, at least anecdotal, about students switching schools in their last year in order to graduate from schools with very low maximum historical GPA in order to gain the maximum score from the GPA$^+$ component. In 2015 this problem was addressed with a change in the policy, which established that the score was calculated relative to the GPA of the student and the school that they attended each year.

Characterization of treatment groups Table 2 shows the characteristics of the group of students identified as pulled-up, pushed-down and unaffected for cohorts of applicants in 2012 and 2013. Each year, pulled-up and pushed-down applicants account for approximately 4% of the applicant pool. From Table 2 we can see that the reform was able to impact the students that were targeted by it. Students in the pulled-up group have better GPA than those in the unaffected and pushed-down groups; yet, their exam scores are comparable to those in the unaffected group. Looking at pushed-down students, they have low GPA and high test scores. Moreover, pulled-up students are 3 times less likely to attend a private high school than a pushed-down student and looking at family characteristics, pulled-up students come from families with average income 30% lower than pulled-up students, and their parents are less educated.

Figure 5 presents the distribution of pulled-up and pushed-down students based on the number of positions moved in their rankings between the admission assignment with and without GPA$^+$. If the most preferred program that the student could reach without the GPA$^+$ measure was choice 3, but with the inclusion of the boost the student could get into their most preferred option (pulled-up students), then the student was moved 2 positions due to the reform. From Figure 5 we can see that the change in terms of preferences is similar for pulled-up and pushed-down groups.

A more detailed analysis of the distribution of rankings for admission is presented in Table 3. Each raw presents the number of students with admission assignments in that ranking when the relative GPA measure is considered. Each column presents that total number of students with admission assignment in that preference choice when the GPA$^+$ measure is not considered. Students assigned to the same program in both regimes are classified as unaffected and are presented in the table without background color (table diagonal). The percentage value in each cell correspond to the proportion of students in that group in that specific ranking combination. The main margins of treatment of the reform corresponds to movements along preferences 1 and 2, preferences 2 and 3, preferences 1 and 3, and between preference 1 and no admission offer. The high percentage of students moved along this last margin is not explained by a higher proportion of students with shorted rank order list but rather due to the a bigger proportion of students at the margin of the minimum requirements of not very demanded programs. More specifically, certain program establish complementary restrictions to admission, as minimum application scores (taking all the components into consideration) or minimum test score averages. Students in this margin have twice higher proportion of their total rank order list as invalid.

Finally, Tables 5 and 4 present the number of pulled-up and pushed-down student in each field with and without the inclusion of the GPA$^+$ component, based on the fields of the admission and simulated admission. In both cases, in most of the cases, students move along their ranking but they stay in the same field (diagonal of the table).

3.2 Difference-in-differences design

I estimate the effect of the reform on human capital acquisition and productivity, on the group of pulled-up and pushed-down students. My difference-in-differences design compares the outcomes of students who were affected by the reform versus those who weren't, before and after the inclusion of the relative GPA measure. Comparing the change in outcomes for these two groups across the two periods allows me to control for transitory variation in outcomes that happen due to factors that are unrelated to the reform. With the estimation of the effect of the reform on pulled-up and pushed-down students, I analyze the (outcome) efficiency impact of the reform on the system.

The parameters of interest to evaluate the effect of the inclusion of the relative GPA measure in the admission process can be expressed as the conditional average treatment effect for the group of students pulled-up and pushed-down.

$$\tau(PU) = \mathbb{E}[Y_i(\mu') - Y_i(\mu)|PU_i = 1]$$
$$\tau(PD) = \mathbb{E}[Y_i(\mu') - Y_i(\mu)|PD_i = 1]$$

In the potential outcome framework $Y_i = D_i Y_i(1) + (1 - D_i) \cdot Y_i(0)$ is the outcome of a student i, and $D_i = 1\{\text{when the relative GPA is used for admission assignment}\}$.

The observed outcomes is represented by $Y_i = 1\{t(i) = 2012\} \cdot Y_i(0) + 1\{t(i) = 2013\} \cdot Y_i(1)$. Assuming additive separability to capture any changes in time uncorrelated to the determinants of the outcomes with and without the inclusion of the GPA$^+$ measure, I estimate models of the form:

$$Y_i = \beta_1 PU_i + \beta_2 PD_i + \beta_3(PU_i \cdot Post_i) + \beta_4(PD_i \cdot Post_i) + \beta_5 Post_i + X_i'\Gamma + \varepsilon_i$$

where Y_i is the outcome variable of interest to evaluate the reform: enrollment, graduation and earnings. PU_i indicates if the student belong to the pulled-up group, PD_i indicates if the student belong to the pushed-down group, $Post_i$ is an indicator that takes the value of 1 if the students apply post reform. The omitted group are students that get access to the same programs under both regimes. X_i' is a vector of individual characteristics such as gender, family income, type of school, GPA and standardized test scores to control for possible changes in the composition characteristics of pulled-up and pushed-down students between 2012 and 2013. [26]

Here β_3 and β_4 are the estimates of the parameter of interest to evaluate the reform. β_3 capture the effect on outcome Y_i of gaining access to the a more preferred, but also more selective program due to the inclusion of the GPA$^+$ measure in the admission process. Likewise, β_4 capture the effect of losing access to more selective programs with the reform. [27]

Identification assumption The key identification assumption is that the outcomes for these three groups of students would have evolved similarly for the cohorts 2012 and 2013 if the reform would have not been implemented. I cannot directly test that, however, I conduct a placebo exercise with data from the 2011 application cohort that present suggestive evidence in support of it.

Following the same procedure used for cohort 2012, I start by computing the boost

[26] Results are presented with and without controls. Most of the results are quantitatively and statistically unchanged.

[27] The new admission program is more preferred by definition of the treatment group, but it has to be more selective because if it wasn't the case, that program would have been reached in the status quo scenario.

score for each student in 2011, and application scores for each program in their rank order list. With that, and keeping constant the vacancies observed that year I re-run the DA algorithm using the three components application score. Using the simulated admission assignment I classify 2011 students into pulled-up, pushed-down and unaffected. Finally, I estimate the diff-in-diff specification but with the variable $Post_i$ indicating if the student was observed in the 2012 admission process.

Table 6 shows the estimates for this placebo exercise, which can be interpreted as the effect in enrollment and graduation for pulled-up and pushed-down students when no reform is implemented. As expected, there is no significant effect, suggesting that when no reform is implemented these groups follow a similar trend. The estimates would be biased if the coefficients of interest reflect sample selection resulting from the impact of the reform on the composition of applicants. However, there is no change in the trend of total applicants, and no change in the probability of pulled-up students to reapply compared with the 2011 cohort. There also would be bias in the estimates if there were unexpected changes in 2013 in other determinants of outcomes that differentially affected the three groups. I am aware of no such change.

Notably, the intervention considered for this diff-in-diff evaluation occurred just once, so considerations regarding the calendar time of the comparison group observations, such as those stated by Goodman-Bacon (2021); Baker et al. (2022); De Chaisemartin & d'Haultfoeuille (2020), do not apply in this context.

4 Results

I find that the introduction of the relative GPA measure into admission scores improved the system's equity without sacrificing its efficiency. The increase in equity comes from the finding that pulled-up students have higher probability of enrollment and graduation from more selective programs. This access to more selective programs translates into higher earnings. I infer that there is no efficiency loss associated with the reform from the fact that, even though pushed-down students are less likely to enroll in their admission offer, they have higher probability of enrollment later in the selective system, no change in college completion rates, and there is no negative effect on earnings.

4.1 Enrollment

The change in the admission mechanism due to the inclusion of the relative GPA measure had a large impact on enrollment for pulled-up and pushed-down students. However, the changes are small or non-significant when considering the effects on enrollment in the entire college system and the time of enrollment (allowing for reapplication).

The difference-in-differences estimates in Table 8 show that, for pulled-up students there is a large effect in the probability of students choosing to enroll in their admission offer. After the reform, pulled-up students are 22 p.p. more likely to enroll in the selective program that they were admitted. This is a 40% effect on enrollment.[28] For pushed-down students the probability of enrollment decreases by 16.7 p.p. The difference (in absolute value) between the effect on enrollment for pulled-up and pushed-down students is significant, indicating that the inclusion of the GPA$^+$ measure improved the system in terms of identifying successful applicants, i.e. there is an increase in the total number of students that decide to enroll once admission is offered.

The total effect on enrollment uncover changes at two margins: the extensive margin - students that gain or lose the possibility of admission in the selective system - and the intensive margin - students that improve (worsen) their admission in the selective system, but that with or without the reform had some admission on the system. On the extensive margin, the reform changed the probability of a student of getting access to some selective program in pulled-up and pushed-down students by approximately 20%.

The total effect on enrollment is not fully driven by students at the extensive margin. To study the intensive margin, I restrict the sample to students that would have got some admission under the two regimes. Observing the admission offers under the two regimes allows me to correct for the potential selection bias of only observing enrollment if a student actually gets an offer. [29] Table 9 shows the results from my main specification, but

[28]Table 7 presents the average enrollment rates in the selective system for the 3 groups.

[29]All students in the pulled-up group got an admission offer in 2013 (if not they could not be better

restricted to the group of student at the intensive margin. The estimates on enrollment for pulled-up students after the reform is smaller (17 p.p.) but still large. Compared with the pushed-down students (11 p.p.), I find evidence of higher intensity of preferences for pulled-up students, i.e. that the reaction, in terms of enrollment decision, from getting access to a program higher in the rank order list is stronger than the reaction from losing access to it, for the pushed-down group.

I summarize the changes in the programs that students attend using traditional measures of quality like selectivity and graduation rate. Table 10 shows how the characteristics of the peers and programs that students attend before and after the reform changed. Columns 1 and 2 show the differences-in-differences estimates of a regression in which the dependent variable in one of these average program characteristics before the reform. The first 3 rows show that pulled-up students attend more selective programs after the reform, in the sense that the average student at the program that they enrolled had higher test scores and GPAs than the average student at the programs they enrolled before the reform was implemented. Graduation on time is an indicator of the probability that a student graduates in the number of years set by the program; after the reform pulled-up students enroll in programs where the average student is more likely to graduate on time. The results are symmetrical for pushed-down students. [30]

Enrollment by second year after application to the admission process Enrollment at selective universities can only occur at the program to which students are admitted based on the deferred acceptance algorithm. Thus, they can enroll in that most preferred program among the ones they are eligible for based on their application scores, or if they choose not to enroll they can (i) enroll in a non-selective college, (ii) re-apply the following year (normally after taking extra test preparation courses), or (iii) decline to attend college.

Given that not all the students enroll at the program that they get admitted in the

than without the GPA$^+$ measure), but not all pulled-up students got an admission offer in 2012 because the reform was still not implemented.

[30] The expected graduation time of the programs that pushed-down students enroll after the reform are on average 0.07 years shorter.

selective system, it is interesting to study how the alternative options changed with the implementation of the reform. Columns 3 and 4 in Table 8 show that in the first year, pushed-down students compensate for the decrease in the probability of enrolling in a selective program by enrolling in the non-selective system. However, the 3.9 p.p. increase in the probability of enrollment in the non-selective system does not offset completely the decrease in the probability of enrollment in the selective system. This means that the reform leads to some pushed-down students not enrolling in any university in the first year after high school.

By analysing enrollment one year after the admission process application I see that pushed-down students have the same probability of being enroll in college than before the reform but are more likely to be moving through college with at least one year of delay. Table 11 examines enrollment one year after the admission process into any program (selective or non-selective) and into a selective program. Columns 1 and 2 provide the evidence to discard a negative effect on the attempt to acquire college for pushed-down students; one year after their application process they have the same probability of being enroll in some program before and after the reform. Columns 3 and 4 in Table 11 shows that pushed-down students are less likely to be enroll in a selective program by the second year after their application. This effect is smaller (5.2 p.p.) than the effect on enrollment in the selective programs of the first year (16.7 p.p.) suggesting that students in the pushed-down group are more likely to re-apply to and enroll in selective programs (probably to try to reach those more desired choices) after the implementation of the reform. Table 12 corroborates this point, showing that pushed-down students are 7 p.p. more likely to reapply to the selective system after the reform was implemented.

4.2 Graduation

I find that pulled-up students are 8.4 p.p. more likely to complete their admission program; pushed-down students have a comparable opposite effect (-8.2 p.p.). An alternate exercise designed to test for mismatch hypothesis confirms this preliminary evidence against it. Pulled-up and pushed-down students have no effect on the probability of college graduation when considering graduation from any program (and not just from the new programs granted admission as a result of the reform) and the probability for them to remain enrolled due to the delay enrollment.

Admission program completion There is positive effect in the probability of complete their admission program for pulled-up students, with a comparable opposite effect for pushed-down students. Columns 1-3 of Table 13 present the results for graduation from the admission program at different points in time. Consistently, there is a large positive effect (8.4 p.p. increase by 8 years after application) of 36% on the likelihood of graduation from the admission program for pulled-up students. Column 4 also shows that pulled-up students are more likely to graduate on time after the implementation of the reform. For pushed-down students the effects on graduation are similar in magnitude but with the opposite sign.

The effects on graduation are equivalent when the sample is adjusted only to students affected at the intensive margin (students that got access to better or worse option with the reform, but who under both scenarios had access to some admission in the selective system). Table 14 shows the same results in for this restricted sample of students at the intensive margin of treatment. The results show that the previous effects are not driven by the fact that after the reform students are more or less likely to get admission offers into some selective program. The probability of graduation from the program of admission for pulled-up students increases by 7.8 p.p with the reform. Table 15 presents the results for graduation in the sample of enrolled students. However, because I don't have a model to control for selection into enrollment these results are biased. It is expected that because pulled-up students in 2013 are more likely to enroll, even comparing students with admission offers under both regimes, the sample is negatively selected. The zero effect on graduation for pulled-up students suggests that this is the case.

In essence, the reform enabled pulled-up students access to more selective programs which increased their likelihood of enrolling in and graduating from those programs. Putting the graduation effect for pulled-up students into perspective, the implied graduation rate for the marginal student admitted by the relative GPA is 38% (8.4/21.9). This does not differ much from the average graduation rate of unaffected students post-reform (40%) or from the pre-reform level of 39% percent. In addition, the impacts are qualitatively comparable to the findings of other equitable college admission programs, such as S. E. Black et al. (2020) and Bleemer (2021).

Mismatch hypothesis The mismatch hypothesis establishes that applicants with lower test scores targeted by equitable admission policies would benefit from enrolling in less selective universities, where their academic qualifications more closely "match" those of their peers (Sowell, 1972). This hypothesis found empirical support on some of the mixed results from the research around affirmative action policies like (Arcidiacono & Lovenheim, 2016). However, the evidence presented so far for the relative GPA reform contradict this hypothesis; I interpret the fact that students in the pulled-up group enroll in more selective programs after the reform and increase their probability of graduation from those programs as evidence against the mismatch hypothesis.

Because the main specification doesn't control for the tuple (specific pair of admission programs with and without the GPA^{+} measure) of admission programs, one possible concern refers to the potential imbalances on the programs that student get admitted with and without the reform, between 2012 and 2013.[31] In order to control for that, I estimate an alternative specification that includes as a control the admission assignment without the reform. This way, I can ensure that all the variation captured by the diff-in-diff comes from pulled-up students with the same admission assignment without the reform and with admission to more selective programs after the reform.[32] Table 16 shows the result from this exercise. Contrary to the mismatch hypothesis, more selective admission increased the graduation probability for pulled-up students, with similar effect than the estimated before (9 p.p.).

[31]Students in the pulled-up group are by definition admitted to more selective programs post-reform, but this is relative to their own assignment.

[32]Remember that the definition on pulled-up group is based on the ranking of the preference, but if something was ranked higher and was less selective than the admission assignment without the GPA^{+} measure, then the algorithm would have assigned the student to that program pre-reform.

STEM In recent year there has been an special interest around STEM degrees, and the focus around this topic for access-oriented policies has not been the exception (Loury & Garman, 1993; Holzer & Neumark, 2000; Arcidiacono et al., 2016). Arcidiacono et al. (2016) study major degrees for the case of California campuses when affirmative action policies were in place; the research states that a better matching of science students to universities by preparation level could increase minority science graduation.

I find that the effect on degree completion in STEM for STEM applicants is positive and significant (6 p.p.). Column 1 in Table 17 shows that the relative GPA reform increase the probability for pulled-up students to get admitted in a STEM program. Column 2 presents the effect for enrollment in a STEM program, conditional on student listing some STEM program in their application and column 3 also presents enrollment results but focusing on students at the intensive margin of treatment. The effect on enrollment (16.9 p.p.) compared to the effect on graduation in the same sample (6.1 p.p.) suggest that the implied graduation rate for the marginal student admitted by the reform is higher than the graduation rate in STEM degrees for the unaffected students in the entire system (36% vs 24%).

Other results I examine the effect of the reform dividing the group of pulled-up and pushed-down students into two groups based on the changes of selectivity (measured as average of test scores) between the admission program with GPA^+ and without GPA^+. Table 18 shows in column 1 that the effects on enrollment are positive and larger for pulled-up students with smaller changes in selectivity relative to the group with larger changes in selectivity. Column 2 presents the effect on graduation from the admission; the effects are positive for pulled-up students and larger for students with a bigger change in selectivity. For graduation from any program, small increases in selectivity have a detrimental effect on students, but this effect appears to be driven by students taking longer than eight years from their participation in the admissions process to graduate. Results for pushed-down students follow a similar pattern across all the outcomes, students with bigger reduction in selectivity are less likely to enroll, graduate from the admission program and graduate 8 years from their participation in the admission process from any program; however, the effects are non-significant when the outcome of graduation or still enrolled is considered.

Alternatively, Table 19 presents in columns 1 and 2 the main results for the sample of students moved only 1 position in their preference ranking for programs. On this sample the effects are smaller than in the entire population. Columns 3 and 5 present the same results but for the remaining sample of students moved more than 1 position in their ranking as well as students affected in the extensive margin of treatment. Columns 4 and 6 show the results only for students at the intensive margin of treatment but with big changes in terms of the ranking of the program accessed with and without the inclusion of the relative GPA measure. These effects are larger, 23 p.p for enrollment and 10.5 p.p for admission program completion, but in the same order of magnitude than the effect for the entire group.

Appendix A examines differential effects by gender, income and boost score of student on the main outcomes of enrollment, graduation from admission, college completion from any program, and graduation from a selective program. Table 25 shows differential effect of enrollment (15 p.p.) only for students with higher boost. In terms of graduation from admission Table 26 suggest that the main effect for pulled-up students is driven by the effect on females and students with high boost. There are no differential effect on college completion when any program is considered. I find some indications of variation of impacts across gender, family income, and boost score, but the overall picture is pretty consistent.

College completion There is no effect of the reform (pulled-up or pushed-down group) on human capital acquisition when it is measured as college completion and when the possibility for students to be still enrolled 8 years after the application process is considered. However, pulled-up students are more likely to earn degrees from selective programs after the reform.

Table 20 shows the average graduation from any program by 6, 7, and 8 years after the application process by treatment groups, before and after the implementation of the relative GPA reform. It is especially important to notice that graduation from any program captures some of the indirect effects of the reform in reapplications (therefore late enrollment in the selective system) and enrollment in the non-selective system. This could be one of the reasons why, even 8 years after the application process, there are still important changes in the graduation rates compared with the previous year, suggesting that the lack of more graduation data limits the full analysis of the reform.

The difference-in-differences estimates for the effect of graduation from any program are presented in Table 21. There is no change in college completion for pulled-up and pushed-up students by 7 year after application due to the reform. However, there is a negative effect on graduation by 8 years for pushed-down students, i.e. after the reform they are less likely to have completed some program. Because this result could be reflecting the fact that pushed-down students are more likely to graduate late (due to late enrollment after the reform) or the fact that pushed-down are acquiring less human capital after the reform, column 4 of Table 21 presents the result when the dependant variable indicates if the student graduate or is still enrolled 8 years after application. The null effect implies that pushed-down students are not acquiring less human capital after the reform.

Table 22 present the results divided by graduation from any selective program and Table 23 from any non-selective program. These results also suggest that changes in graduation at 8 years after application for pushed-down students are driven mostly by changes from selective enrollment, which requires a late enrollment if the student wants to enroll in a different program than the admission offered by the new mechanism after the inclusion of the relative GPA measure.

In summary, the reform made pulled-up students more likely to graduate from more selective programs, with no impact in college completion. For pushed-down students, the inclusion of the GPA^{+} made them less likely to graduate 8 years after application, however, this is not due to a decrease on the probability of college completion but due to a delayed enrollment in selective programs, for some of the students that didn't enroll or didn't stay in the program admitted after the reform.

4.3 Labor Market Outcomes

Finally, I study the labor market effects of the reform.[33] An important challenge refers to the long graduation times observed in the previous section. Given the extension of the programs and the reapplication dynamics, by positioning earnings ten years after application participation, a significant amount of student may not have made the transition to work. This fact limits the earnings analysis. Additionally, aggregated data - earnings with an indicator of group of treatment but without individual characteristics-only allows for very preliminary evidence at group level.

Figure 6 present earnings histograms for pulled-up and pushed-down groups of students pre and post implementation of the relative GPA reform. In each case histograms are presented relative to the unaffected group. Even though at the moment I cannot calculate the difference-in-differences estimates, a preliminary review of the aggregated data confirms that pulled-up and pushed-down students do not do worse than before the implementation of the reform. Overall, in terms of outcome efficiency - graduation and earning-, the evidence confirms that the new assignment mechanism didn't make the system less efficient.

4.4 Robustness checks

I conduct a number of checks to verify the robustness of my conclusions. I check different samples (removing students with boost higher than 150 points, only freshman applicants, or students attending programs over 6 years) and estimating my results clustering at the school-year level, and all of them support my main findings.

Changes in ROL due to the reform The key assumption for the identification of pulled-up and pushed-down groups is that the rank order list (ROL) of the application submitted by the applicants in each process would not change under a different assignment mechanism. Recent literature present evidence raising concerns over the inclusion of a more selective programs when the boost score is observed. By checking the selectivity of

[33] Up to this date, access to individual level data required to estimate the difference-in-differences specification used in the previous sections is under approval.

the first preference listed by students in 2012 and 2013 (measure as the cutoff score of that program) for students with the same boost we see some increase in the selectivity when boost is larger than 150.

As a robustness check I estimate the main results presented above but removing students with boost score higher than 150. The tables with the results for this case are presented in Appendix B. Results are not only qualitative but also quantitative similar for all the outcomes.

Sensitivity of the results to long programs Given the instability of graduation results even after 8 year of participation on the admission process, I restrict the analysis only to programs with expected graduation time of less than 6 year in Appendix D and to less than 7 years in Appendix C. Both sets of results present similar results in terms of magnitude and significance than the ones discussed previously.

Inference The previous results have been estimated using robust standard errors. Alternately, in Appendix E I present the main results allowing clustering at the school-year level. Nonetheless, any of the results take into consideration the potential error associated with the estimation of the pulled-up and pushed-down groups. Results presented in Appendix E are virtually equivalent to the results presented above.

5 Alternative approach

I validate my results with an alternative empirical strategy. The unique configuration for my difference-in-differences (which is not a panel) plus the lack of additional comparable pre-reform periods make it difficult to evaluate the identification assumptions in my main empirical approach. Using a regression discontinuity (RD) design that permits a direct test of the identification assumptions, I evaluate the impact of getting access to the most desired program in a cross-sectional setting before the implementation of the reform (2012).

I use a regression discontinuity design to estimate the effect on enrollment and graduation of threshold crossing the 1st preference's cutoff for all applicants prior to the reform's implementation because, as shown in Table 3, the main margin of treatment of the reform is with respect to individuals moved to and from their first preference.

I estimate the effects of crossing the admission cutoff for the most preferred program (δ) on admission in other choices (1st stages), enrollment, selectivity of the enrolled program, graduation from the admission program, and any graduation after 8 years using a standard regression discontinuity specifications of the form

$$Y_i = f(r_i) + \delta C_i + \eta_i$$

where Y_i is one of the outcomes listed above for individual i; r_i is the difference between the admissions score assigned to i's most preferred program and the admission cutoff score to that program or running variable; $f(r_i)$ is a smooth function (results presented in Appendix F for polynomials of degree 1 to 5) of the running variable (which can change on either side of the cutoff); C_i indicates if i's application score is greater than the cutoff score (so i is admitted to the most preferred choice), and η_i is an error term. I estimate this equation using data from all the programs with excess of demand (for which the cutoff is meaningful) on the whole range, with the exception of the linear specification, for which I limit the data to a small score window close to the cutoff.

Tables 44, 45 and 46 present the estimates for admission in choice 2, 3 and no-admission around the threshold for crossing the admission threshold for the 1st choice. From these first-stage estimates I can calculate the proportion of compliers for whom the relevant choice margin is choice 1 versus next-best option. According to these estimations, the effect on first preference admission is driven by 65% of students at the edge of 1st and 2nd preference, 20% of students at the margin of 3rd to 1st preference, and 10% of students at the margin of no-admission to 1st preference.

Table 24 provides a summary of the principal results from the difference-in-differences estimates at the margins of treatment relevant for the comparison with the RD estimates, the RD estimator employing a polynomial of order 3, and a weighted average of the diff-in-diff estimates based on the 1st stage results. The RD estimates are consistently smaller than the diff-in-diff estimates, with the same sign and order of magnitude. However, there are reasons to anticipate this pattern. First, the RD design identifies the effect of gaining access to the most preferred program for marginal students , i.e., students who could resemble pulled-up and pushed-down students. The diff-in-diff results at the relevant margins (given in Appendix F) demonstrate a non-symmetrical effect for pulled-up students as compared to pushed-down students, with the latter being smaller in all instances. If compliers of the RD are a blend of these two groups, one would anticipate a smaller estimate for the RD.

In addition, I estimate the same RD model while limiting the sample to students with a boost score greater than 5 (the average boost score for pushed-down students at the margin of first and no admission) in an effort to recover the effects from a population that is more comparable to the pulled-up group of students. Table 52 shows the enrollment results, while Table 52 displays the graduation estimates. In both instances, the outcomes are greater than the general RD and comparable to the weighted diff-in-diff outcomes (19.5 p.p. for enrollment and 9.9 p.p for graduation).

The tables 58 and 58 provide the findings of an alternative exercise designed to quantify the effects on a subset of pulled-up students. This experiment focuses on the 2013 application pool (after the implementation of the reform). Only students with admission at their 1st or 2nd preference (treatment margins 1-2) and with simulated admission at their 2nd preference without the GPA$^+$ measure are included (therefore threshold crossing due to the boost). By comparing pulled-up students with very comparable unaffected students, the sample restriction aims to determine the effect of threshold crossing for pulled-up students (non-crossing but similar - close to the margin). The small sample size resulting from the requisite limits makes the results unstable and imprecise; still, the sign and magnitude of the values for graduation from the admission offer fluctuate around the estimate for the margin between first and second preference (7.7 p.p).

Furthermore, I examine the threshold crossing impacts in 2013 to assess the possibility of a General Equilibrium (GE) effect over the entire system. Although this is improbable due to the small amount of students affected by the reform (about 4% of pulled-up and 4%

of pushed-down), enrollment, selectivity, and graduation rates are comparable to 2012, which is encouraging.

A potential threat to the regression discontinuity (RD) design is that people might try to sort themselves above the cutoff in order to receive an offer from their preferred program. Figures in Appendix F show that there are no discontinuities around the cutoffs in the density of applicants and in the observed characteristics support assumption against that type of sorting.In addition, the McCrary (2008) test is negligible and fails to reject the null hypothesis of no sorting.

6 Conclusion

This paper studies the impact of providing students with access to more selective college alternatives. I use the variation on admission generated by the inclusion of a relative GPA measure motivated by equity concerns. I explore the effects of the reform on enrollment, graduation and earning for the two groups directly and indirectly affected by this change: (i) students who gain access to more selective programs (pulled-up) and (ii) students who lose access to more selective programs (pushed-down).

The transparency of the college admission process combined with the properties of the assignment mechanism and the richness of the data available allow me to cleanly identify the groups of students affected by the reform, one of the big challenges in the evaluation of admission reforms. By simulation of the admission offers with and without the inclusion of the relative GPA measure I identify the group of affected students. The replication of the admissions with the GPA^{+} in the years before the reform helps me to identify the group of student that would have been affected. This simulation facilitate the implementation of a difference-in-difference design.

This empirical strategy compares the outcomes of students in the pulled-up and pushed-down groups before and after the implementation of the reform, therefore, before and after they get access to these more selective programs. The transitory variation on outcomes is controlled by the second difference with respect to the group of unaffected students.

I find that the incorporation of the relative GPA measure into the college admissions application score formula expanded the options available for students with significant less resources. As a result of the reform, pulled-up students became more likely to enroll in a selective program, and they chose to enroll in programs where their peers have higher test scores, GPA scores, and graduation rates. Contrary to the prediction of the mismatch hypothesis, reform-targeted applicants with lower test scores gained from enrolling in more selective options, boosting their likelihood of graduation by 8.4 percentage points.

For pushed-down students, I find that their likelihood of graduating from the admission program assigned by the new mechanism decreases by 8.2 p.p., but they are not less likely to receive a bachelor's degree. There is however an impact in the timing of their enrollment that would be interesting to study with more details once more data on graduation and earning becomes available. Nevertheless, preliminary evidence confirms that there is no negative impact on earning for pushed-down students.

Collectively, the evidence presented above indicate that test-based meritocratic admission system can be improved by the inclusion of in-school performance metric, increasing admission equity without incurring an efficiency penalty.

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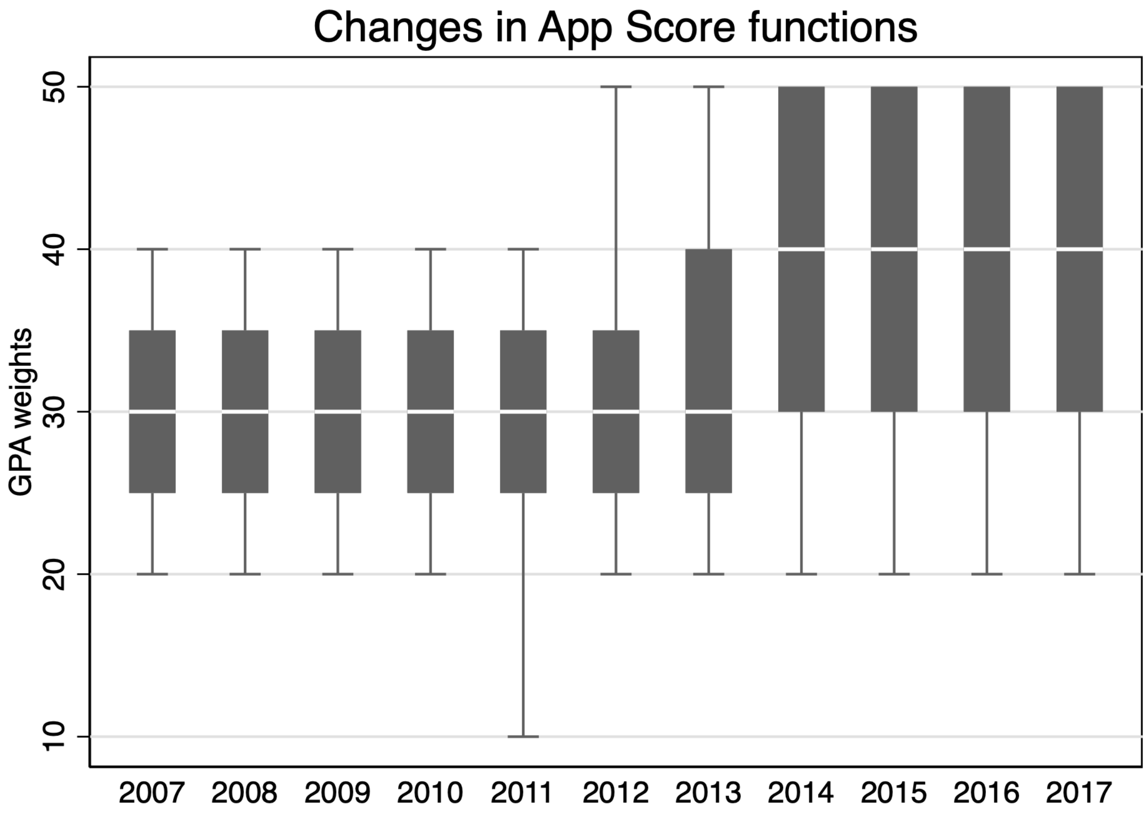
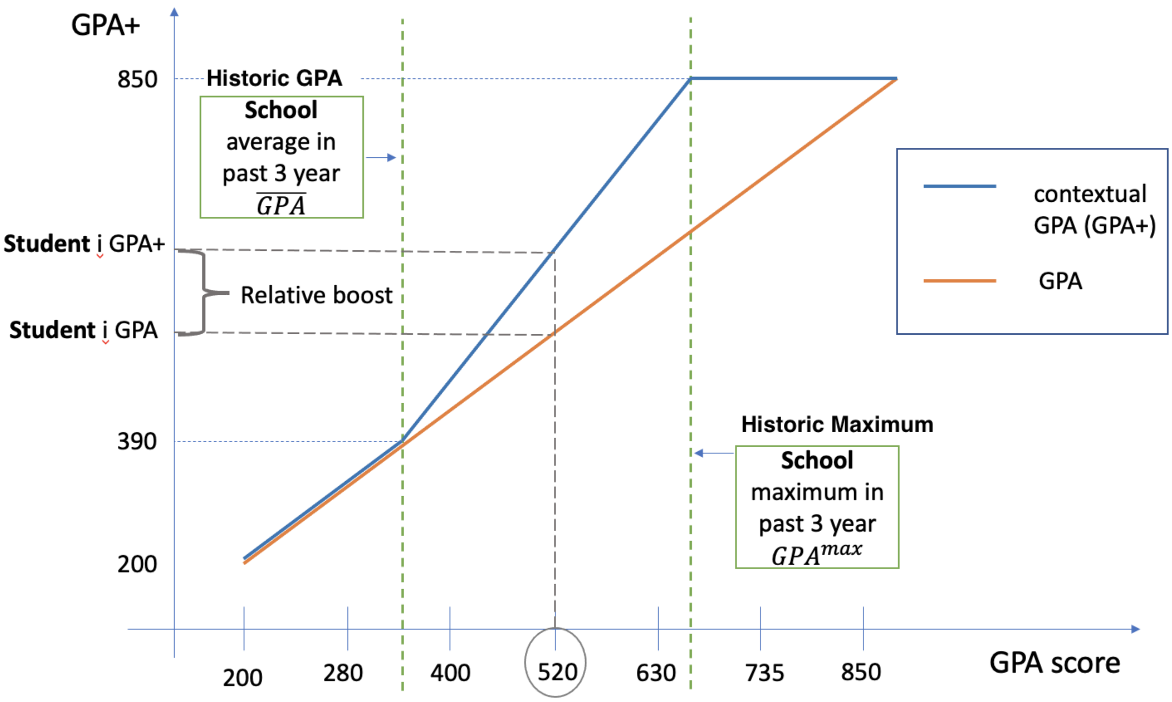
Figures and Tables

Figure 1: Weights of GPA components (GPA + GPA^{+}) in application scores by year



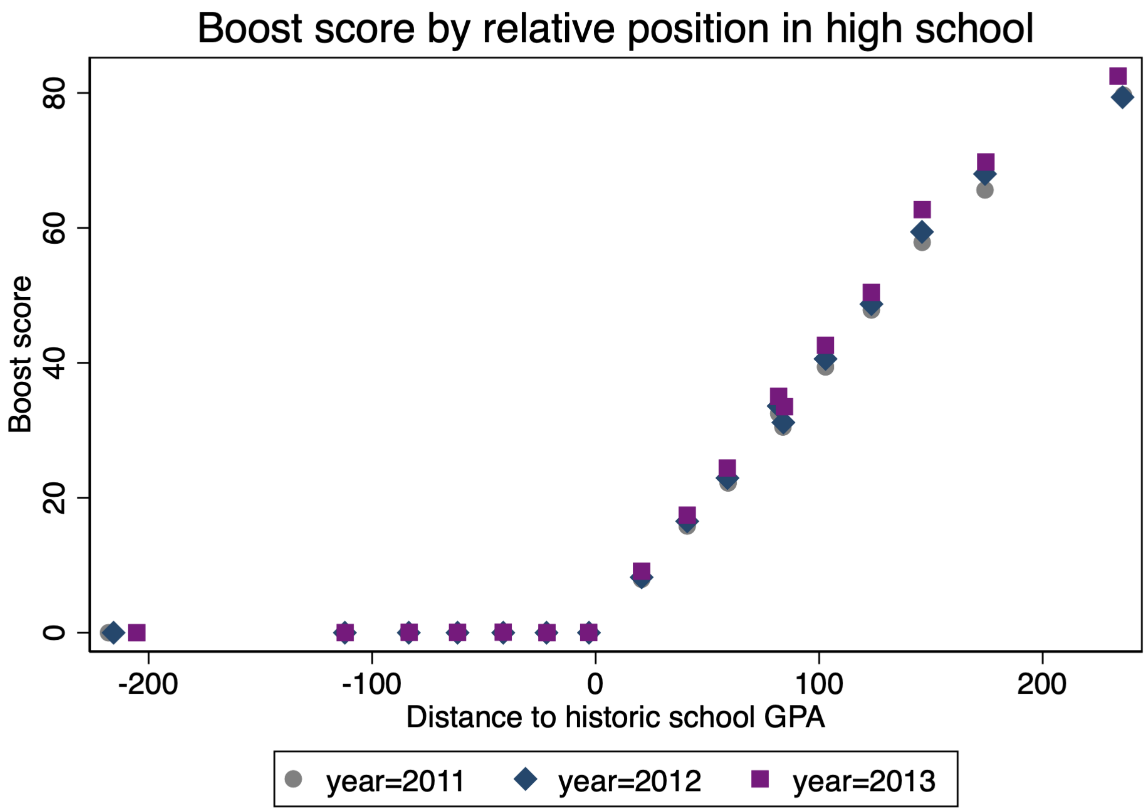
Notes: This figure shows the whisker plots for the distribution of the weights of the GPA components assigned by programs in the application score formula. The middle box represents 50% of the data, the white line corresponds to the median weight and the maximum and minimum values are displayed with vertical lines ("whiskers") .

Figure 2



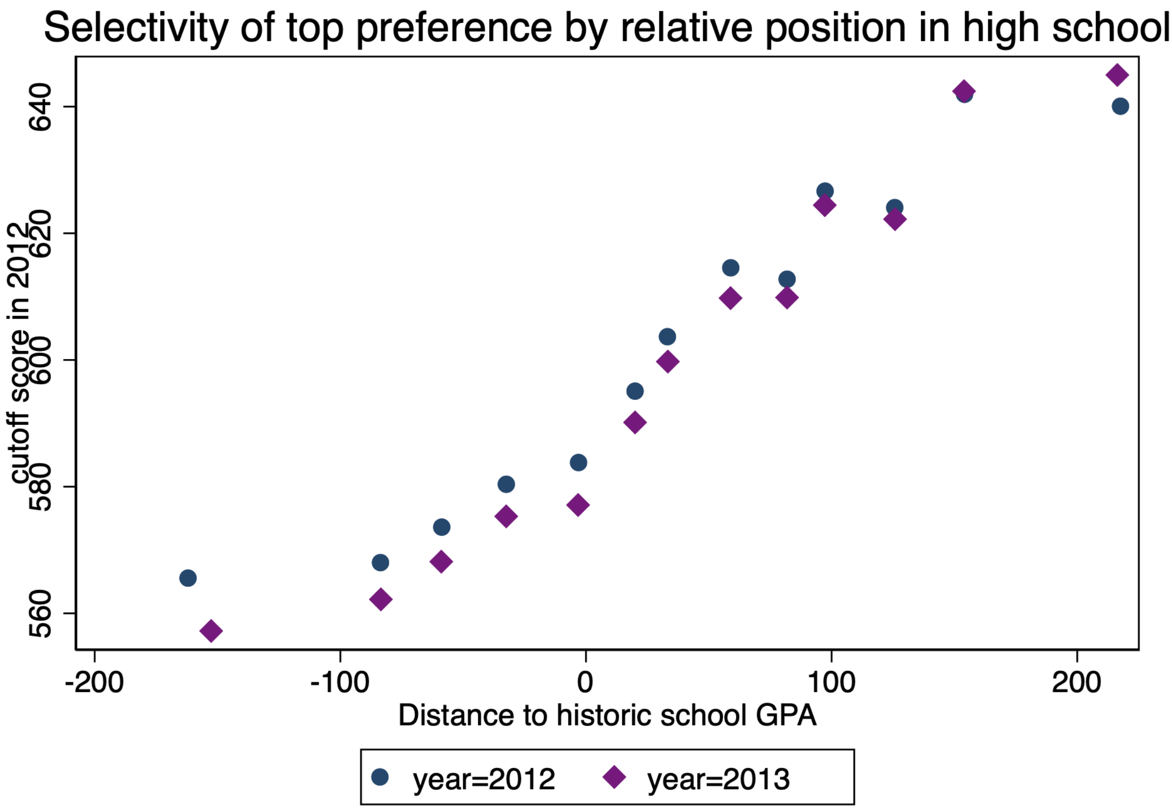
Notes: exemplary figure to show how GPA$^+$ depends on school averages and how it relates to the GPA score. Boost is obtained from the difference between GPA$^+$ score and GPA.

Figure 3



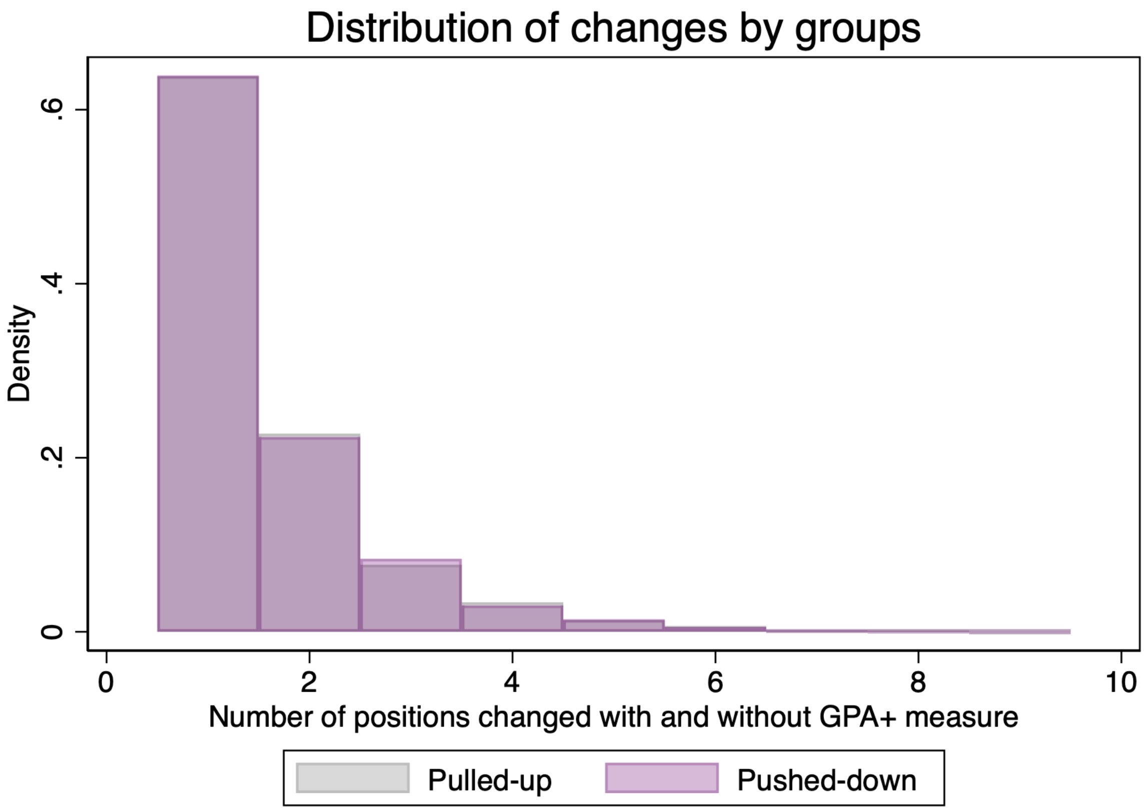
Notes: boost score for cohort 2011, 2012 and 2013. For 2013 GPA$^+$ (and the inferred boost) was provided on the application data. For 2011 and 2012 boost was calculated according to the GPA$^+$ formula using education records of the universe of high school students graduated between 2008 and 2012.

Figure 4



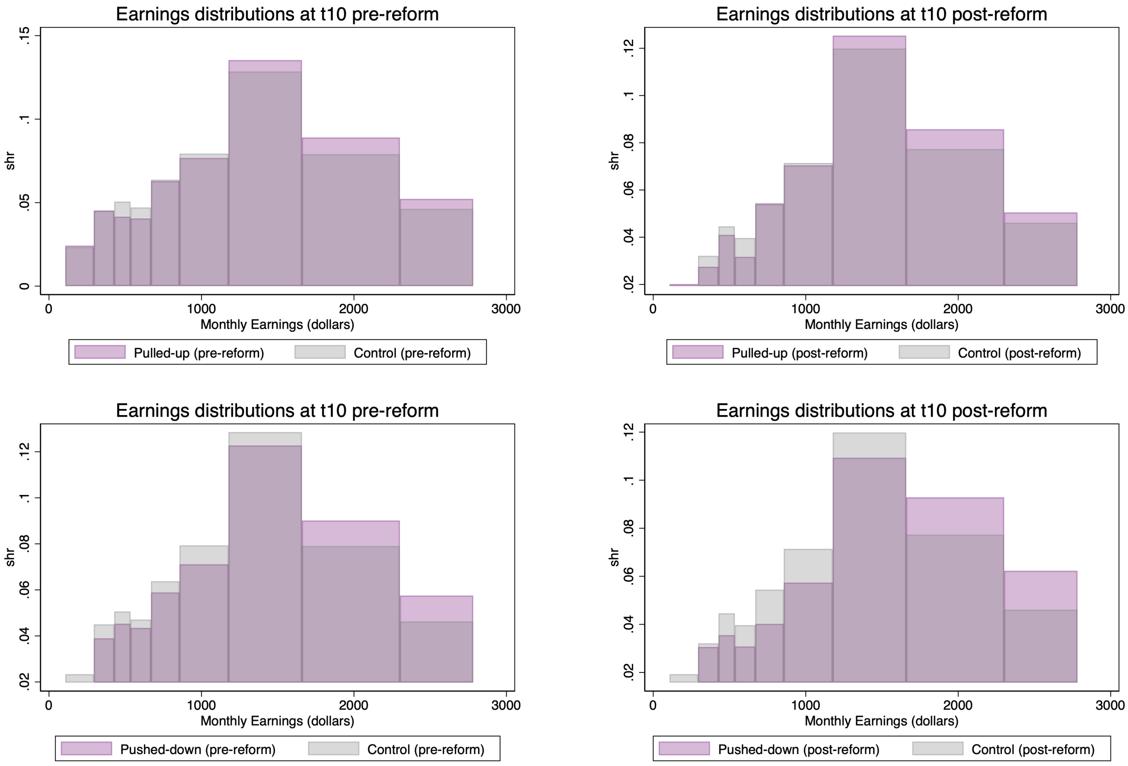
Notes: binscatter of the selectivity of the 1st preference by boost. Selectivity measure as the cutoff (application score of the last person admitted in the programs, measured pre-reform) of the program listed 1st. The x-axis have the GPA$^+$ measure, but centered around the average score of the school. By centered at the school average we have that positive values correspond to the boost score.

Figure 5



Notes: distribution of pulled-up and pushed-down students based on the number of positions moved in their ranking between admission with and without GPA$^+$.

Figure 6: Earnings distribution



Notes: Earnings distribution for pulled-up and pushed-down groups, relative to unaffected, 10 years after application. Figures on the left show earnings distribution for students in cohort 2012 (pre-reform) and figures on the right show earnings distribution for students in cohort 2013 (post-reform).

Table 1: Distribution of students reporting rankings by year

Ranking		2012	2013
1	Total N	116,336	118,208
	Only 1 (%)	0.07	0.07
	Up to 1 (%)	0.07	0.07
2	Total N	108,715	110,264
	Only 2 (%)	0.09	0.10
	Up to 2 (%)	0.16	0.17
3	Total N	98,166	98,245
	Only 3 (%)	0.17	0.20
	Up to 3 (%)	0.32	0.37
4	Total N	78,828	74,152
	Only 4 (%)	0.16	0.17
	Up to 4 (%)	0.48	0.55
5	Total N	60,420	53,693
	Only 5 (%)	0.14	0.14
	Up to 5 (%)	0.62	0.68
6	Total N	44,322	37,403
	Only 6 (%)	0.10	0.09
	Up to 6 (%)	0.72	0.78
7	Total N	32,720	26,182
	Only 7 (%)	0.07	0.07
	Up to 7 (%)	0.79	0.84
8	Total N	24,208	18,477
	Only 8 (%)	0.06	0.05
	Up to 8 (%)	0.85	0.89
9	Total N	17,041	12,572
	Only 9 (%)	0.04	0.03
	Up to 1 (%)	0.89	0.92
10	Total N	12,582	9,167
	Only 10 (%)	0.11	0.08
	Up to 10 (%)	1.00	1.00

Notes: The table shows the total number of students reporting each ranking, the percentage of students reporting a total of each ranking, and the percentage of students reporting each ranking or less options. In 2011 the maximum number of choices was increase and students were nudge to take advantage of that and list 10 options.

Table 2: Summary Statistics for Groups of Interest

	Unaffected		Pulled-up		Pushed-down	
	2012	2013	2012	2013	2012	2013
N	108,167	109,440	3,753	4,515	4,416	4,253
Female (%)	53	52	62	60	41	40
Public School (%)	28	27	29	29	26	25
Voucher School (%)	53	54	60	60	47	47
Private School (%)	19	18	10	11	27	28
Family Inc ($/mo)	689	714	573	594	809	869
Father with HS (%)	67	67	64	61	74	75
Mother with HS (%)	73	73	69	70	78	79
Father with College (%)	26	26	20	19	34	35
Mother with College (%)	21	21	16	16	27	29
Capital City (%)	39	39	46	46	54	53
Std Math	0.68	0.65	0.74	0.65	1.05	1.13
Std Verbal	0.66	0.65	0.70	0.62	1.01	1.04
Std GPA	0.75	0.73	1.40	1.28	0.42	0.58
Boost score	21	22	60	57	6	8

Notes: This table shows the summary statistics for the groups of interest, the year before and after the reform.

Table 3: Distribution of students by ranking with and without GPA$^+$

With GPA$^+$	Without GPA$^+$										
Ranking	1	2	3	4	5	6	7	8	9	10	NA
1	48,434	1,166	415	139	59	31	16	2	7	0	397
%	0.44	0.26	0.09	0.03	0.01	0.01	0.00	0.00	0.00	0.00	0.09
2	1,149	19,404	530	187	72	36	12	5	2	3	262
%	0.27	0.18	0.12	0.04	0.02	0.01	0.00	0.00	0.00	0.00	0.06
3	347	590	10,500	240	113	23	11	5	3	2	202
%	0.08	0.14	0.10	0.05	0.03	0.01	0.00	0.00	0.00	0.00	0.04
4	124	184	279	4,303	96	50	18	6	0	1	92
%	0.03	0.04	0.07	0.04	0.02	0.01	0.00	0.00	0.00	0.00	0.02
5	37	72	124	101	2,288	47	22	5	5	1	58
%	0.01	0.02	0.03	0.02	0.02	0.01	0.00	0.00	0.00	0.00	0.01
6	397	28	36	46	59	1,158	21	13	5	1	45
%	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.00	0.00	0.00	0.01
7	9	12	12	16	22	21	651	12	8	3	26
%	0.00	0.00	0.00	0.00	0.01	0.00	0.01	0.00	0.00	0.00	0.01
8	2	3	10	5	8	7	14	338	5	4	11
%	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
9	2	1	3	4	3	3	5	14	193	8	7
%	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
10	1	1	0	5	0	1	3		2	123	5
%	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
NA	269	220	141	94	62	29	13	15	8	9	22,048
%	0.06	0.05	0.03	0.02	0.01	0.01	0.00	0.00	0.00	0.00	0.20

Notes: this table presents the number of students in 2013 with admission at different ranking of their rank order list with and without the GPA$^+$. Values on green correspond to pushed-down cases and values on blue correspond to pulled-up students. Column 1 - row 1 (and all the diagonal) shows the number of students that with admission in their top choice under both regimes, therefore, they are classify as unaffected. The percentage value under the total number of students represent the proportion of students in that treatment group that have that combination of rankings.

Table 4: Distribution of pulled-up students by fields with and without GPA$^+$

With GPA$^+$	Without GPA$^+$									
Ranking	MedOdon	Health	Sci	Engi	Tech	Business	Art	SocSci	Law	Educ
MedOdon	126	54	7	10	2	2	0	3	4	1
Health	3	425	51	21	34	21	2	32	2	44
Sci	0	19	57	23	25	3	0	5	0	10
Engi	1	7	23	359	111	51	2	5	2	2
Tech	0	9	19	79	266	20	20	7	1	10
Business	0	1	12	19	29	224	3	8	3	6
Art	0	0	1	0	7	0	18	7	0	4
SocSci	0	8	8	3	17	32	15	200	18	47
Law	0	0	0	1	4	11	0	29	63	9
Educ	0	4	4	6	13	9	6	29	4	194

Notes: Total number of pulled-up student in 2013 in each field combination based on the field of the program that they get admitted with the GPA$^+$ and the field of the program that they get admitted without the GPA$^+$.

Table 5: Distribution of pushed-down students by fields with and without GPA$^+$

With GPA$^+$ Ranking	Without GPA$^+$ MedOdon	Health	Sci	Engi	Tech	Business	Art	SocSci	Law	Educ
MedOdon	126	54	7	10	2	2	0	3	4	1
Health	3	425	51	21	34	21	2	32	2	44
Sci	0	19	57	23	25	3	0	5	0	10
Engi	1	7	23	359	111	51	2	5	2	2
Tech	0	9	19	79	266	20	20	7	1	10
Business	0	1	12	19	29	224	3	8	3	6
Art	0	0	1	0	7	0	18	7	0	4
SocSci	0	8	8	3	17	32	15	200	18	47
Law	0	0	0	1	4	11	0	29	63	9
Educ	0	4	4	6	13	9	6	29	4	194

Notes: Total number of pushed-down student in each field combination based on the field of the program that they get admitted with the GPA$^+$ and the field of the program that they get admitted without the GPA$^+$.

Table 6: Difference-in-differences estimates for 2012 and 2011

	(1) Enroll	(2) Enroll	(3) Grad by 8yr	(4) Grad by 8yr
Pulled-Up	0.001	-0.015	0.006	-0.009
	(0.011)	(0.010)	(0.011)	(0.011)
Pushed-Down	-0.010	-0.002	0.007	0.005
	(0.009)	(0.009)	(0.011)	(0.010)
Observations	211,872	211,872	211,872	211,872
Controls		✓		✓

Robust standard errors in parentheses

Notes: columns 1 and 3 have the estimates from the difference-in-difference without controls and columns 2 and 4 have the estimates for the same outcomes but controlling by individual characteristics.

Table 7: Enrollment rates at admission program by groups, before and after the reform

Total	**Unaffected**	**Pulled-Up**	**Pushed-Down**
Enrollment Pre-Reform (2012)	0.80	0.83	0.91
Enrollment Reform (2013)	0.79	0.87	0.85
Difference	-0.01	0.04	-0.06

Program	**Unaffected**	**Pulled-Up**	**Pushed-Down**
Enrollment Pre-Reform (2012)	0.60	0.53	0.78
Enrollment Reform (2013)	0.62	0.75	0.66
Difference	0.02	0.22	-0.12

Non-selective	**Unaffected**	**Pulled-Up**	**Pushed-Down**
Enrollment Pre-Reform (2012)	0.13	0.12	0.08
Enrollment Reform (2013)	0.10	0.05	0.08
Difference	-0.03	-0.07	0.00

Notes: averages for a variable that indicates if the student choose to enroll in the admission assignment. The difference by group, between after and before the reform is shown in the 3rd row.

Table 8: Diff-in-diff estimates for enrollment

	(1) Enrollment	(2) Enrollment	(3) Non-Select	(4) Non-Select
Pulled-Up x after	0.199***	0.219***	-0.049***	-0.057***
	(0.011)	(0.010)	(0.007)	(0.007)
Pushed-Down x after	-0.136***	-0.167***	0.023***	0.039***
	(0.010)	(0.010)	(0.007)	(0.006)
Obs.	234,544	234,544	234,544	234,544
Controls		✓		✓
Test	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$
p-value	0.000	0.000	0.008	0.048

Robust standard errors in parentheses

Notes: columns 1 and 2 have estimates when the outcome is enrollment at the admission program. Column 3 and 4 have estimates for an indicator if the student enroll in a non-selective program. Columns 2 and 4 control for standardized test scores, GPA, family income, region, type of high school and gender.

Table 9: Diff-in-diff estimates for enrollment: sample with some admission offer under both regimes

	(1) Enrollment	(2) Enrollment
P-Up x after	0.165***	0.175***
	(0.0113)	(0.0111)
P-Down x after	-0.0947***	-0.110***
	(0.00987)	(0.00965)
Obs.	186,734	186,734
Controls		✓
Test	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$
p-value	0.000	0.000

Robust standard errors in parentheses

Notes: diff-in diff estimates with same specification than Table 8, but restricted to students with some admission offer under both regimes to capture enrollment effect on students at the intensive margin.

Table 10: Changes in peer characteristics at chosen programs

Program	Diff-in-Diff		Pre-Reform ($\bar{x}$)		
Charact.	Pulled-up	Pushed-down	Control	Pulled-up	Pushed-down
Math (std)	0.264***	-0.212***	1.104	1.262	1.272
	(0.008)	(0.007)	[0.610]	[0.603]	[0.611]
Verbal (std)	0.235***	-0.230***	1.114	1.240	1.261
	(0.008)	(0.008)	[0.567]	[0.537]	[0.530]
GPA (std)	0.280***	-0.288***	1.165	1.317	1.276
	(0.009)	(0.008)	[0.575]	[0.519]	[0.544]
Grad on time	0.044***	-0.040***	0.389	0.384	0.374
	(0.007)	(0.006)	[0.263]	[0.271]	[0.267]
E(grad time)	0.026	-0.065**	5.110	5.163	5.161
	(0.027)	(0.027)	[0.725]	[0.779]	[0.820]

Robust standard errors in parentheses. Standard deviation in square brackets.

Notes: Columns 1 and 2 show the results for the main diff-in-diff specification for the outcome 5 different outcomes: (i) average math score of students enrolled at the chosen program pre-reform, (ii) average verbal score of students enrolled at the chosen program pre-reform, (iii) average GPA score of the students enrolled at the chosen program pre-reform, (iv)probability of graduation on time by the students enrolled at the chosen program pre-reform, (v) expected graduation time based on the class structure at the chosen program. Columns 3-5 show the averages and standard deviation of these variables for the 3 groups of interest, pre-reform.

Table 11: Effect on enrollment 2 years after the application process

	(1) Any	(2) Any	(3) Selective	(4) Selective
P-Up x after	0.0033	0.0181**	0.0410***	0.0643***
	(0.0079)	(0.0076)	(0.0098)	(0.0090)
P-Down x after	0.0128*	-0.0075	-0.0159*	-0.0521***
	(0.0071)	(0.0070)	(0.0091)	(0.0085)
Obs.	234,544	234,544	234,544	234,544
Controls		✓		✓
Test	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$
p-value	0.141	0.315	0.066	0.338

Robust standard errors in parentheses

Notes: Columns 1 and 2 show the results for the main diff-in-diff specification using an indicator if the student is enroll at some program by second year. Columns 3 and 4 show the estimates for an indicator of enrollment in a selective program by second year.

Table 12: Effect on re-application by second year

	(1) Reapplication	(2) Reapplication
P-Up x after	-0.0387***	-0.0374***
	(0.0091)	(0.0091)
P-Down x after	0.0739***	0.0717***
	(0.0086)	(0.0086)
Obs.	234,544	234,544
Controls		✓

Robust standard errors in parentheses

Notes: diff-in-diff estimates using an indicator if the student participate on the application process on the second year.

Table 13: Effect on graduation from admission program

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad on time
P-Up x after	0.042***	0.072***	0.084***	0.043***
	(0.008)	(0.009)	(0.010)	(0.010)
P-Down x after	-0.033***	-0.060***	-0.082***	-0.039***
	(0.007)	(0.009)	(0.009)	(0.010)
Obs.	234,544	234,544	234,544	234,544
Controls	✓	✓	✓	✓
Test	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$
p-value	0.397	0.366	0.917	0.742

Robust standard errors in parentheses

Notes: Columns 1-3 show diff-in-diff estimates for an indicator if the student graduate from the admission program by 6, 7 and 8 years after application. Column 4 show the results for the outcome of graduation on time.

Table 14: Effect on graduation from admission program conditional on some admission offer with both mechanism

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad on time
P-Up x after	0.037***	0.065***	0.078***	0.034***
	(0.009)	(0.011)	(0.011)	(0.012)
P-Down x after	-0.020**	-0.040***	-0.062***	-0.017
	(0.009)	(0.010)	(0.011)	(0.011)
Obs.	186,734	186,734	186,734	186,734
Controls	✓	✓	✓	✓
Test	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$
p-value	0.195	0.100	0.305	0.295

Robust standard errors in parentheses

Notes: Diff-in-diff results for the sample of students with some admission with and without the inclusion of the GPA$^+$ measure. Columns 1-3 show diff-in-diff estimates for an indicator if the student graduate from the admitted program by 6, 7 and 8 years after application. Column 4 show the results for the outcome of graduation on time.

Table 15: Effect on graduation from admission program conditional on enrollment

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad on time
P-Up x after	-0.012	-0.009	-0.010	-0.000
	(0.012)	(0.013)	(0.014)	(0.014)
P-Down x after	-0.005	-0.018	-0.034***	0.001
	(0.010)	(0.012)	(0.012)	(0.012)
Obs.	144,540	144,540	144,540	144,540
Controls	✓	✓	✓	✓
Test	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$
p-value	0.302	0.126	0.020	0.989

Robust standard errors in parentheses

Notes: Diff-in-diff results for the sample of students that enroll in 1st year. Columns 1-3 show estimates for an indicator if the student graduate from the admission program by 6, 7 and 8 years after application. Column 4 show the results for the outcome of graduation on time.

Table 16: Mismatch effect exercise

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad on time
P-Up x after	0.047***	0.078***	0.091***	0.049***
	(0.007)	(0.009)	(0.010)	(0.010)
Obs.	234,529	234,529	234,529	234,529
Controls	✓	✓	✓	✓

Robust standard errors in parentheses

Notes: This table shows the effect on graduation from admission into a more selective program. The diff-in-diff specification controls by the admission program without the relative GPA reform in order to ensure that estimation uses only variation from students with admission to more selective programs after the reform, and not from potential changes in the compositions of admission programs between 2012 and 2013.

Table 17: Effect on STEM applicants

	(1) Admission	(2) Enrollment	(3) Enrollment	(4) Grad by 8yr	(5) Grad or enroll by 8 yr
P-Up x after	0.061***	0.216***	0.169***	0.061***	0.052***
	(0.010)	(0.014)	(0.015)	(0.014)	(0.016)
P-Down x after	-0.030***	-0.166***	-0.120***	-0.047***	-0.035**
	(0.010)	(0.013)	(0.012)	(0.014)	(0.016)
Obs.	234,544	110,791	97,350	97,350	97,350
Controls	✓	✓	✓	✓	✓

Robust standard errors in parentheses

Notes: Column 1 shows the coefficient for the indicator of admission offer in STEM using the main diff-in-diff specification. Column 2 shows the effect on enrollment for STEM applicants. Column 3 restrict the sample of column 2 only to students that have some admission offer with and without GPA. Column 4 present the effects on graduation for the same sample than column 3. Finally, column 5 present the results conditional on enrollment.

Table 18: Differential effect for students with big and small changes in selectivity

	(1) Enrollment	(2) Grad by 8yr	(3) Grad by 8yr from any	(4) Grad or enroll by 8 yr
Small Pulled-Up x after	0.195***	0.069***	-0.034**	0.005
	(0.015)	(0.016)	(0.017)	(0.015)
Big Pulled-Up x after	0.151***	0.080***	0.007	-0.003
	(0.016)	(0.016)	(0.017)	(0.015)
Small Pushed-Down x after	-0.108***	-0.040**	0.000	0.007
	(0.013)	(0.016)	(0.017)	(0.015)
Big Pushed-Down x after	-0.112***	-0.085***	-0.036**	-0.014
	(0.014)	(0.016)	(0.017)	(0.015)
Obs.	186,734	186,734	186,734	186,734
Controls	✓	✓	✓	✓

Robust standard errors in parentheses

Notes: Based on how much the average test score of the peers (selectivity of the programs) changed between the simulated program and the admission program, the pulled-up and pushed-down groups are split into big and small changes in selectivity. The sample contains only students who have some admission offer in both regimes. Column 1 shows how the reform affected enrollment for the four subgroups. Column 2 shows the change in the probability of program completion. Column 3 shows the effects of graduating from any program 8 years after admission. Column 4 shows the effects of graduating or still being in school 8 years after application, which takes into account the fact that students in the selective system may switch programs, which will cause them to graduate from college later.

Table 19: Effects on sample of students moved only one or more positions in their ranking with and without GPA$^+$

	(1) Enrollment	(2) Grad by 8yr	(3) Enrollment	(4) Enrollment	(5) Grad by 8yr	(6) Grad by 8yr
P-Up x after	0.130***	0.050***	0.283***	0.231***	0.101***	0.105***
	(0.014)	(0.014)	(0.015)	(0.019)	(0.013)	(0.018)
P-Down x after	-0.084***	-0.043***	-0.211***	-0.114***	-0.088***	-0.051***
	(0.012)	(0.014)	(0.015)	(0.017)	(0.013)	(0.018)
Obs.	181,950	181,950	226,088	178,278	226,088	178,278
Controls	✓	✓	✓	✓	✓	✓
xmark	Moved 1	Moved 1	Moved more	Moved more + offer	Moved more	Moved more + offer

Robust standard errors in parentheses

Notes: Columns 1 and 2 show the effect on enrollment and graduation from the admission program when students are moved one position in their ranking between using GPA$^+$ and not using it in the assignment process. Columns 3 and 4 show the enrollment effect on the sample of students whose admission was moved more than 1 position in their ranking; column 4 restrict the sample only to student that had some admission offer under both regimes. Columns 5 and 6 show the effect on program completion for the same samples of column 3 and 4.

Table 20: Graduation averages from any program by groups, before and after the reform

	Unaffected	**Pulled-Up**	**Pushed-Down**
Grad by 6yr Pre-Reform (2012)	0.22	0.24	0.21
Grad by 6yr Reform (2013)	0.21	0.22	0.19
Difference	-0.01	-0.02	-0.02

	Unaffected	**Pulled-Up**	**Pushed-Down**
Grad by 7yr Pre-Reform (2012)	0.36	0.40	0.35
Grad by 7yr Reform (2013)	0.34	0.37	0.34
Difference	-0.02	-0.03	-0.01

	Unaffected	**Pulled-Up**	**Pushed-Down**
Grad by 8yr Pre-Reform (2012)	0.46	0.51	0.47
Grad by 8yr Reform (2013)	0.42	0.45	0.42
Difference	-0.04	-0.06	-0.05

Notes: averages for a variable that indicates if the student graduates from some program (selective or non-selective). The difference by group, between after and before the reform is shown in the 3rd row.

Table 21: Effects on graduation from any program

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad or enroll by 8 yr
P-Up x after	-0.003	0.005	-0.008	0.006
	(0.009)	(0.011)	(0.011)	(0.010)
P-Down x after	-0.012	-0.015	-0.032***	-0.006
	(0.009)	(0.010)	(0.011)	(0.010)
Obs.	234,544	234,544	234,544	234,544
Controls	✓	✓	✓	✓

Robust standard errors in parentheses

Notes: diff-in-diff estimates for the indicator if the student graduate from some program by 6, 7 or 8 years. Column 4 show the results when the dependent variable takes the value of 1 if the student graduate or if the student is enrolled in some program 8 years after application.

Table 22: Effect on graduation from a selective program

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad or enroll by 8yr
P-Up x after	0.019**	0.030***	0.019*	0.024**
	(0.009)	(0.010)	(0.011)	(0.010)
P-Down x after	-0.022***	-0.028***	-0.046***	-0.030***
	(0.008)	(0.010)	(0.010)	(0.010)
Obs.	234,544	234,544	234,544	234,544
Controls	✓	✓	✓	✓
	Selective	Selective	Selective	Selective

Robust standard errors in parentheses

Notes: Columns 1-3 show the results for graduation from a selective program by 6, 7 or 8 years after application. Columns 4-5 show the same results for non-selective programs.

Table 23: Effects on graduation from a non-selective program

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad or enroll by 8yr
P-Up x after	-0.022***	-0.025***	-0.026***	-0.019***
	(0.004)	(0.005)	(0.005)	(0.006)
P-Down x after	0.009***	0.013***	0.014***	0.022***
	(0.004)	(0.004)	(0.004)	(0.005)
Obs.	234,544	234,544	234,544	234,544
Controls	✓		✓	✓
	Non-Selective	Non-Selective	Non-Selective	Non-Selective

Robust standard errors in parentheses

Notes: Columns 1-3 show the results for graduation from a selective program by 6, 7 or 8 years after application. Columns 4-5 show the same results for non-selective programs.

Table 24: Difference-in-differences and RD comparison

	(1) DD	(2) DD	(3) DD	(4) DD	(5) RD	(6) wRD
Enroll	0.189***	0.094***	0.276***	0.542***	0.172***	0.259
	(0.017)	(0.028)	(0.030)	(0.036)	(0.007)	
Selectivity	0.250***	0.231***	0.254***	0.443***	0.220***	0.280
	(0.009)	(0.013)	(0.015)	(0.011)	(0.004)	
Grad Admission	0.077***	0.059**	0.160***	0.181***	0.082***	0.109
	(0.019)	(0.029)	(0.033)	(0.029)	(0.007)	
Grad by 8yr	-0.028	0.028	0.033	0.010	-0.003	-0.010
	(0.020)	(0.031)	(0.036)	(0.038)	(0.008)	
Obs.	137,400	61,188	116,834	140,033	89,970	
	Moved 2→1	Moved 3→2	Moved 3→1	Moved NA→1	RD	wRD

Notes: Column 1 - 4 present the diff-in-diff results for the pulled-up students at the 4 main margins for the main diff-in-diff specification. Column 5 presents the RD estimates for crossing threshold of the 1st preference using a 3rd order polynomial and column 6 presents a weighted average of the diff-in-diff results using the 1st stage coefficients to determine the right proportion at each margin of treatment.

A Heterogeneity Analysis

Table 25: Heterogeneity: Effects on enrollment by gender, family income and boost

	(1) Enrollment	(2) Enrollment	(3) Enrollment
P-Up x after	0.193***	0.192***	0.080***
	(0.017)	(0.014)	(0.029)
P-Down x after	-0.148***	-0.145***	-0.148***
	(0.013)	(0.011)	(0.012)
P-Up x after x Characteristic	0.009	0.020	0.155***
	(0.022)	(0.021)	(0.031)
P-Down x after x Characteristic	0.027	0.033	0.012
	(0.020)	(0.022)	(0.020)
After x Characteristic	-0.005	-0.002	-0.023***
	(0.004)	(0.004)	(0.004)
Obs.	234,544	234,544	234,544
Controls	✓	✓	✓
Characteristic	Female	Low Income	Boost
PU Fraction	61%	45%	85%
PD Fraction	41%	30%	32%

Robust standard errors in parentheses

Notes: main diff-in-diff specification for enrollment fully interacted with (i) female indicator, (ii) low income indicator, and (iii) boost indicator.

Table 26: Heterogeneity: Effects on graduation from same program by gender, family income and boost

	(1) Grad by 8yr	(2) Grad by 8yr	(3) Grad by 8yr
P-Up x after	0.042***	0.082***	0.025
	(0.014)	(0.013)	(0.022)
P-Down x after	-0.051***	-0.058***	-0.055***
	(0.012)	(0.012)	(0.011)
P-Up x after x Characteristic	0.047**	-0.025	0.065***
	(0.019)	(0.020)	(0.025)
P-Down x after x Characteristic	-0.020	-0.013	-0.049**
	(0.020)	(0.021)	(0.021)
After x Characteristic	0.010***	0.006*	-0.015***
	(0.004)	(0.004)	(0.004)
Obs.	234,544	234,544	234,544
Controls	✓	✓	✓
Characteristic	Female	Low Income	Boost
PU Fraction	61%	45%	85%
PD Fraction	41%	30%	32%

Robust standard errors in parentheses

Notes: main diff-in-diff specification for graduation from assigned program fully interacted with (i) female indicator, (ii) low income indicator, and (iii) boost indicator.

Table 27: Heterogeneity: Effects on graduation from any program by gender, family income and boost

	(1) Grad by 8yr	(2) Grad by 8yr	(3) Grad by 8yr
P-Up x after	-0.023	-0.008	0.006
	(0.017)	(0.015)	(0.028)
P-Down x after	-0.004	-0.002	-0.008
	(0.014)	(0.013)	(0.013)
P-Up x after x Characteristic	0.003	-0.030	-0.018
	(0.023)	(0.023)	(0.031)
P-Down x after x Characteristic	-0.008	-0.026	-0.042*
	(0.022)	(0.024)	(0.023)
After x Characteristic	0.003	0.011***	-0.015***
	(0.004)	(0.004)	(0.004)
Obs.	234,544	234,544	234,544
Controls	✓	✓	✓
Characteristic	Female	Low Income	Boost
PU Fraction	61%	45%	85%
PD Fraction	41%	30%	32%

Robust standard errors in parentheses

Notes: main diff-in-diff specification for graduation from any program fully interacted with (i) female indicator, (ii) low income indicator, and (iii) boost indicator.

Table 28: Heterogeneity: Effects on graduation from a selective program by gender, family income and boost

	(1) Grad by 8yr	(2) Grad by 8yr	(3) Grad by 8yr
P-Up x after	-0.015	0.006	0.038
	(0.017)	(0.015)	(0.026)
P-Down x after	-0.006	-0.007	-0.022*
	(0.014)	(0.013)	(0.013)
P-Up x after x Characteristic	0.026	-0.010	-0.025
	(0.022)	(0.022)	(0.029)
P-Down x after x Characteristic	-0.022	-0.035	-0.031
	(0.022)	(0.023)	(0.023)
After x Characteristic	0.018***	0.021***	-0.021***
	(0.004)	(0.004)	(0.004)
Obs.	234,544	234,544	234,544
Controls	✓	✓	✓
Characteristic	Female	Low Income	Boost
PU Fraction	61%	45%	85%
PD Fraction	41%	30%	32%

Robust standard errors in parentheses

Notes: main diff-in-diff specification for graduation from a selective program fully interacted with (i) female indicator, (ii) low income indicator, and (iii) boost indicator.

Table 29: Heterogeneity: Effects on graduation or enroll after 8 years from a selective program by gender, family income and boost

	(1) Grad or enroll by 8 yr	(2) Grad or enroll by 8 yr	(3) Grad or enroll by 8 yr
P-Up x after	-0.015	-0.001	-0.030
	(0.017)	(0.013)	(0.029)
P-Down x after	0.028**	0.014	0.013
	(0.013)	(0.012)	(0.013)
P-Up x after x Characteristic	0.005	-0.021	0.037
	(0.021)	(0.021)	(0.031)
P-Down x after x Characteristic	-0.014	0.020	-0.013
	(0.020)	(0.022)	(0.020)
After x Characteristic	0.010**	0.013***	-0.007*
	(0.004)	(0.004)	(0.004)
Obs.	234,544	234,544	234,544
Controls	✓	✓	✓
Characteristic	Female	Low Income	Boost
PU Fraction	61%	45%	85%
PD Fraction	41%	30%	32%

Robust standard errors in parentheses

Notes: main diff-in-diff specification for graduation or enrollment after 8 year from a selective program fully interacted with (i) female indicator, (ii) low income indicator, and (iii) boost indicator.

B Main results from Section 4.4, boost sensitivity

Table 30: Diff-in-diff estimates for enrollment

	(1) Enrollment	(2) Enrollment	(3) Non-Select	(4) Non-Select
P-Up x after	0.197***	0.219***	-0.047***	-0.056***
	(0.011)	(0.011)	(0.007)	(0.007)
P-Down x after	-0.136***	-0.167***	0.023***	0.039***
	(0.010)	(0.010)	(0.006)	(0.006)
Obs.	233,789	233,789	233,789	233,789
Controls		✓		✓
Test	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$
p-value	0.000	0.000	0.014	0.064

Robust standard errors in parentheses

Notes: columns 1 and 2 have estimates when the outcome is enrollment at the admission program. Column 3 and 4 have estimates for an indicator if the student enroll in a non-selective program. Columns 2 and 4 control for standardized test scores, GPA, family income, region, type of high school and gender.

Table 31: Diff-in-diff estimates for graduation from GPA$^+$ program on sample without boost > 150

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad on time
P-Up x after	0.041***	0.070***	0.083***	0.041***
	(0.008)	(0.009)	(0.010)	(0.010)
P-Down x after	-0.033***	-0.060***	-0.082***	-0.038***
	(0.007)	(0.009)	(0.009)	(0.010)
Obs.	233,789	233,789	233,789	233,789
Controls	✓	✓	✓	✓
Test	0	0	0	0
p-value	0.483	0.418	0.968	0.852

Robust standard errors in parentheses

Notes: Columns 1-3 show diff-in-diff estimates for an indicator if the student graduate from the admission program by 6, 7 and 8 years after application. Column 4 show the results for the outcome of graduation on time.

Table 32: Diff-in-diff estimates for any graduation on sample without boost > 150

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad or enroll by 8 yr
P-Up x after	-0.005	0.005	-0.007	0.007
	(0.010)	(0.011)	(0.011)	(0.010)
P-Down x after	-0.012	-0.014	-0.032***	-0.006
	(0.009)	(0.010)	(0.011)	(0.010)
Obs.	233,789	233,789	233,789	233,789
Controls	✓	✓	✓	✓

Robust standard errors in parentheses

Notes: Columns 1-3 show diff-in-diff estimates for an indicator if the student graduate from the admission program by 6, 7 and 8 years after application. Column 4 show the results for the outcome of graduation on time.

C Main results from Section 4.4, long programs

Table 33: Diff-in-diff estimates for enrollment on sample without long programs

	(1) Enrollment	(2) Enrollment	(3) Non-Select	(4) Non-Select
P-Up x after	0.210***	0.238***	-0.067***	-0.078***
	(0.014)	(0.013)	(0.010)	(0.009)
P-Down x after	-0.141***	-0.177***	0.025***	0.043***
	(0.013)	(0.013)	(0.009)	(0.009)
Obs.	178,760	178,760	178,760	178,760
Controls		✓		✓
Test	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$
p-value	0.000	0.001	0.002	0.009

Robust standard errors in parentheses

Notes: results for sample without students in programs with 6 or 7 expected year. Columns 1 and 2 have estimates when the outcome is enrollment at the admission program. Column 3 and 4 have estimates for an indicator if the student enroll in a non-selective program. Columns 2 and 4 control for standardized test scores, GPA, family income, region, type of high school and gender.

Table 34: Diff-in-diff estimates for graduation from GPA$^+$ program on sample without long programs

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad on time
P-Up x after	0.065***	0.084***	0.098***	0.061***
	(0.011)	(0.012)	(0.012)	(0.013)
P-Down x after	-0.051***	-0.077***	-0.087***	-0.061***
	(0.011)	(0.012)	(0.012)	(0.013)
Obs.	178,760	178,760	178,760	178,760
Controls	✓	✓	✓	✓
Test	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$
p-value	0.367	0.667	0.552	0.981

Robust standard errors in parentheses

Notes: Columns 1-3 show diff-in-diff estimates for an indicator if the student graduate from the admission program by 6, 7 and 8 years after application. Column 4 show the results for the outcome of graduation on time.

Table 35: Diff-in-diff estimates for any graduation on sample without long programs

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad or enroll by 8 yr
P-Up x after	0.008	0.007	0.008	0.009
	(0.013)	(0.014)	(0.014)	(0.013)
P-Down x after	-0.030**	-0.034**	-0.040***	-0.008
	(0.012)	(0.013)	(0.014)	(0.013)
Obs.	178,760	178,760	178,760	178,760
Controls	✓	✓	✓	✓

Robust standard errors in parentheses

Notes: Columns 1-3 show diff-in-diff estimates for an indicator if the student graduate from the admission program by 6, 7 and 8 years after application. Column 4 show the results for the outcome of graduation on time.

D Main results from Section 4.4, extra long programs

Table 36: Diff-in-diff estimates for enrollment on sample without extra long programs

	(1) Enrollment	(2) Enrollment	(3) Non-Select	(4) Non-Select
P-Up x after	0.195***	0.216***	-0.052***	-0.060***
	(0.011)	(0.011)	(0.007)	(0.007)
P-Down x after	-0.136***	-0.167***	0.023***	0.038***
	(0.010)	(0.010)	(0.007)	(0.006)
Obs.	228,741	228,741	228,741	228,741
Controls		✓		✓
Test	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$
p-value	0.000	0.001	0.004	0.029

Robust standard errors in parentheses

Notes: results for sample without students in programs with 6 or 7 expected year. Columns 1 and 2 show estimates when the outcome is enrollment at the admission program. Column 3 and 4 have estimates for an indicator if the student enroll in a non-selective program. Columns 2 and 4 control for standardized test scores, GPA, family income, region, type of high school and gender.

Table 37: Diff-in-diff estimates for graduation from GPA$^+$ program on sample without extra long programs

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad on time
P-Up x after	0.047***	0.066***	0.080***	0.042***
	(0.008)	(0.009)	(0.010)	(0.010)
P-Down x after	-0.034***	-0.055***	-0.072***	-0.035***
	(0.008)	(0.009)	(0.010)	(0.010)
Obs.	228,741	228,741	228,741	228,741
Controls	✓	✓	✓	✓
Test	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$
p-value	0.272	0.411	0.547	0.623

Robust standard errors in parentheses

Notes: Columns 1-3 show diff-in-diff estimates for an indicator if the student graduate from the admission program by 6, 7 and 8 years after application. Column 4 show the results for the outcome of graduation on time.

Table 38: Diff-in-diff estimates for any graduation on sample without extra long programs

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad or enroll by 8 yr
P-Up x after	0.002	0.001	-0.003	0.005
	(0.010)	(0.011)	(0.011)	(0.010)
P-Down x after	-0.014	-0.011	-0.023**	-0.005
	(0.009)	(0.010)	(0.011)	(0.010)
Obs.	228,741	228,741	228,741	228,741
Controls	✓	✓	✓	✓

Robust standard errors in parentheses

Notes: Columns 1-3 show diff-in-diff estimates for an indicator if the student graduate from the admission program by 6, 7 and 8 years after application. Column 4 show the results for the outcome of graduation on time.

E Main results from Section 4.4, Inference

Clustered standard errors at school-year level

Table 39: Diff-in-diff estimates for enrollment at admission program with school-year cluster standard errors

	(1) Enrollment	(2) Enrollment	(3) Non-Select	(4) Non-Select
P-Up x after	0.199***	0.219***	-0.049***	-0.057***
	(0.012)	(0.011)	(0.007)	(0.007)
P-Down x after	-0.136***	-0.167***	0.023***	0.039***
	(0.011)	(0.011)	(0.007)	(0.007)
Obs.	234,544	234,544	234,544	234,544
Controls		✓		✓
Test	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$
p-value	0.000	0.001	0.014	0.063

Robust standard errors in parentheses

Notes: Columns 1 and 2 show estimates when the outcome is enrollment at the admission program. Column 3 and 4 have estimates for an indicator if the student enroll in a non-selective program. Columns 2 and 4 control for standardized test scores, GPA, family income, region, type of high school and gender.

Table 40: Diff-in-diff estimates for graduation at admission program with school-year cluster standard errors

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad on time
P-Up x after	0.042***	0.072***	0.084***	0.043***
	(0.008)	(0.009)	(0.010)	(0.011)
P-Down x after	-0.033***	-0.060***	-0.082***	-0.039***
	(0.008)	(0.009)	(0.010)	(0.010)
Obs.	234,544	234,544	234,544	234,544
Controls	✓	✓	✓	✓
Test	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$	$\beta_3 = -\beta_4$
p-value	0.412	0.376	0.918	0.751

Robust standard errors in parentheses

Notes: Columns 1-3 show diff-in-diff estimates for an indicator if the student graduate from the admission program by 6, 7 and 8 years after application. Column 4 show the results for the outcome of graduation on time.

Table 41: Diff-in-diff estimates for college graduation (any program) with school-year cluster standard errors

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad or enroll by 8 yr
P-Up x after	-0.003	0.005	-0.008	0.006
	(0.010)	(0.011)	(0.011)	(0.010)
P-Down x after	-0.012	-0.015	-0.032***	-0.006
	(0.009)	(0.010)	(0.011)	(0.010)
Obs.	234,544	234,544	234,544	234,544
Controls	✓	✓	✓	✓

Robust standard errors in parentheses

Notes: Columns 1-3 show diff-in-diff estimates for an indicator if the student graduate from the admission program by 6, 7 and 8 years after application. Column 4 show the results for the outcome of graduation on time.

Table 42: Diff-in-diff estimates for graduation with school-year cluster standard errors

	(1) Grad by 6yr	(2) Grad by 7yr	(3) Grad by 8yr	(4) Grad by 6yr	(5) Grad by 7yr	(6) Grad by 8yr
P-Up x after	0.019**	0.030***	0.019*	-0.022***	-0.025***	-0.026***
	(0.009)	(0.011)	(0.011)	(0.004)	(0.005)	(0.005)
P-Down x after	-0.022***	-0.028***	-0.046***	0.009**	0.013***	0.014***
	(0.008)	(0.010)	(0.010)	(0.004)	(0.004)	(0.005)
Obs.	234,544	234,544	234,544	234,544	234,544	234,544
Controls	✓	✓	✓	✓		✓
	Selective	Selective	Selective	Non-Selective	Non-Selective	Non-Selective

Robust standard errors in parentheses

Notes: Columns 1-3 show diff-in-diff estimates for an indicator if the student graduate from the admission program by 6, 7 and 8 years after application. Column 4 show the results for the outcome of graduation on time.

F Regression Discontinuity Results

Table 43: RD on admission in 1st pref for crossing threshold for 1st preference

	(1) prob adm 1st	(2) prob adm 1st	(3) prob adm 1st	(4) prob adm 1st	(5) prob adm 1st
RD estimator	0.747***	0.776***	0.807***	0.841***	0.707***
	(0.006)	(0.005)	(0.004)	(0.003)	(0.007)
Obs.	104,275	104,275	104,275	104,275	30,363
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: The expected effect would be zero, however, the effect is smaller in part due to additional requirements that the program may impose (like minimum averages on test scores) or admission through the waiting list after the first round of enrollment creating some measurement error.

Table 44: RD on admission in 2nd pref for crossing threshold for 1st preference

	(1) prob adm 2nd	(2) prob adm 2nd	(3) prob adm 2nd	(4) prob adm 2nd	(5) prob adm 2nd
RD estimator	-0.462***	-0.489***	-0.508***	-0.501***	-0.419***
	(0.007)	(0.006)	(0.005)	(0.004)	(0.009)
Obs.	104,275	104,275	104,275	104,275	30,363
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: The outcome variable is an indicator of admission in student's 2nd choice. The running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20). Specification allow for different slope in each side of the cutoff.

Table 45: RD on admission in 3rd pref for crossing threshold for 1st preference

	(1) prob adm 3rd	(2) prob adm 3rd	(3) prob adm 3rd	(4) prob adm 3rd	(5) prob adm 3rd
RD estimator	-0.117***	-0.129***	-0.153***	-0.181***	-0.121***
	(0.005)	(0.005)	(0.004)	(0.003)	(0.006)
Obs.	104,275	104,275	104,275	104,275	30,363
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: The outcome variable is an indicator of admission in student's 3rd choice. The running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20). Specification allow for different slope in each side of the cutoff.

Table 46: RD on no-admission for crossing threshold for 1st preference

	(1) prob no adm	(2) prob no adm	(3) prob no adm	(4) prob no adm	(5) prob no adm
RD estimator	-0.116***	-0.116***	-0.103***	-0.084***	-0.112***
	(0.005)	(0.005)	(0.004)	(0.003)	(0.006)
Obs.	104,275	104,275	104,275	104,275	30,363
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: The outcome variable is an indicator of no admission at any option. The running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20). Specification allow for different slope in each side of the cutoff.

Table 47: RD estimates on enrollment in admission offer for crossing threshold for 1st preference

	(1) Enrollment	(2) Enrollment	(3) Enrollment	(4) Enrollment	(5) Enrollment
RD estimator	0.178***	0.178***	0.172***	0.162***	0.173***
	(0.009)	(0.008)	(0.007)	(0.005)	(0.010)
Obs.	89,970	89,970	89,970	89,970	28,380
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: 2012 sample of students at program with excess of demand. The outcome variable indicate if the student enroll in the admission program. The running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20).

Table 48: RD estimates on selectivity for crossing threshold for 1st preference

	(1) Selectivity	(2) Selectivity	(3) Selectivity	(4) Selectivity	(5) Selectivity
RD estimator	0.202***	0.213***	0.220***	0.207***	0.189***
	(0.005)	(0.004)	(0.004)	(0.003)	(0.005)
Obs.	92,637	92,637	92,637	92,637	28,488
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: 2012 sample of students at program with excess of demand. Selectivity is measured as the average test score of the students at the admission program. The running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20).

Table 49: RD estimates on graduation from admission offer for crossing threshold for 1st preference

	(1) Grad by 8yr	(2) Grad by 8yr	(3) Grad by 8yr	(4) Grad by 8yr	(5) Grad by 8yr
RD estimator	0.075***	0.077***	0.082***	0.072***	0.072***
	(0.010)	(0.008)	(0.007)	(0.006)	(0.010)
Obs.	89,970	89,970	89,970	89,970	28,380
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: 2012 sample of students at program with excess of demand. The outcome variable indicates if the students graduate from the admission program. The running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20).

Table 50: RD estimates on college graduation for crossing threshold for 1st preference

	(1) Grad (any) by 8yr	(2) Grad (any) by 8yr	(3) Grad (any) by 8yr	(4) Grad (any) by 8yr	(5) Grad (any) by 8yr
RD estimator	0.000	-0.004	-0.003	-0.015**	-0.002
	(0.010)	(0.009)	(0.008)	(0.006)	(0.011)
Obs.	89,970	89,970	89,970	89,970	28,380
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: 2012 sample of students at program with excess of demand. The outcome variable indicates if the students graduate from any program 8 years after the admission process. The running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20).

Table 51: RD estimate on enrollment for crossing threshold for 1st preference for boost students

	(1) enroll	(2) enroll	(3) enroll	(4) enroll	(5) enroll
RD estimator	0.199***	0.199***	0.195***	0.182***	0.193***
	(0.012)	(0.011)	(0.009)	(0.007)	(0.013)
Obs.	47,584	47,584	47,584	47,584	15,490
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: 2012 sample of students at program with excess of demand and a boost score larger than 5. The outcome variable indicates if the students graduate from any program 8 years after the admission process. The running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20).

Table 52: RD estimate on graduation from the admission offer for crossing threshold for 1st preference for boost students

	(1) Grad by 8yr	(2) Grad by 8yr	(3) Grad by 8yr	(4) Grad by 8yr	(5) Grad by 8yr
RD estimator	0.096***	0.095***	0.099***	0.082***	0.095***
	(0.014)	(0.012)	(0.010)	(0.008)	(0.015)
Obs.	47,584	47,584	47,584	47,584	15,490
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: 2012 sample of students at program with excess of demand and a boost score larger than 5. The outcome variable indicates if the students graduate from any program 8 years after the admission process. The running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20).

Table 53: RD estimate on college graduation for crossing threshold for 1st preference for boost students

	(1) Grad in 8yr	(2) Grad in 8yr	(3) Grad in 8yr	(4) Grad in 8yr	(5) Grad in 8yr
RD estimator	0.001	-0.011	-0.011	-0.027***	-0.001
	(0.014)	(0.013)	(0.011)	(0.009)	(0.015)
Obs.	47,584	47,584	47,584	47,584	15,490
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: 2012 sample of students at program with excess of demand and a boost score larger than 5. The outcome variable indicates if the students graduate from any program 8 years after the admission process. The running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20).

Table 54: Diff-in-diff estimates on enrollment at admission offer

	(1) Enroll	(2) Enroll	(3) Enroll	(4) Enroll
P-Up x after	0.189***	0.094***	0.276***	0.542***
	(0.017)	(0.028)	(0.030)	(0.036)
P-Down x after	-0.113***	-0.088***	-0.117***	-0.498***
	(0.015)	(0.023)	(0.026)	(0.039)
Obs.	137,400	61,188	116,834	140,033
Controls	✓	✓	✓	✓
	Moved 1-2	Moved 2-3	Moved 1-3	Moved 1-0

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: diff-in-diff estimates using the sample of students for whom the admissions with and without the inclusion of the GPA^{+} measurement moves them between the respective margins.

Table 55: Diff-in-diff estimates of peers performance at admission offer

	(1) Selectivity	(2) Selectivity	(3) Selectivity	(4) Selectivity
P-Up x after	0.250***	0.231***	0.254***	0.443***
	(0.009)	(0.013)	(0.015)	(0.011)
P-Down x after	-0.256***	-0.221***	-0.216***	-0.128***
	(0.010)	(0.012)	(0.016)	(0.013)
Obs.	137,341	61,163	116,783	139,990
Controls	✓	✓	✓	✓
	Moved 1-2	Moved 2-3	Moved 1-3	Moved 1-0

Robust standard errors in parentheses

Notes: diff-in-diff estimates using the sample of students for whom the admissions with and without the inclusion of the GPA^{+} measurement moves them between the respective margins. Selectivity is measured as the average test score of the students at the admission program; for students without any admission selectivity is measure as the average test score of students without any admission.

Table 56: Diff-in-diff estimates for graduation from admission offer

	(1) Grad by 8yr	(2) Grad by 8yr	(3) Grad by 8yr	(4) Grad by 8yr
P-Up x after	0.077***	0.059**	0.160***	0.181***
	(0.019)	(0.029)	(0.033)	(0.029)
P-Down x after	-0.073***	-0.042	-0.089**	-0.240***
	(0.020)	(0.027)	(0.035)	(0.030)
Obs.	137,400	61,188	116,834	140,033
Controls	✓	✓	✓	✓
	Moved 1-2	Moved 2-3	Moved 1-3	Moved 1-0

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: diff-in-diff estimates using the sample of students for whom the admissions with and without the inclusion of the GPA^+ measurement moves them between the respective margins.

Table 57: Diff-in-diff estimates for college graduation

	(1) Grad by 8yr	(2) Grad by 8yr	(3) Grad by 8yr	(4) Grad by 8yr
Pulled-Up x after	-0.028	0.028	0.033	0.010
	(0.020)	(0.031)	(0.036)	(0.038)
Pushed-Down x after	-0.019	-0.005	-0.024	-0.111***
	(0.021)	(0.029)	(0.037)	(0.037)
Obs.	137,400	61,188	116,834	140,033
Controls	✓	✓	✓	✓
	Moved 1-2	Moved 2-3	Moved 1-3	Moved 1-0

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: diff-in-diff estimates using the sample of students for whom the admissions with and without the inclusion of the GPA^+ measurement moves them between the respective margins.

Table 58: RD estimates on enrollment for crossing threshold for 1st preference for pulled-up students

	(1) Enroll	(2) Enroll	(3) Enroll	(4) Enroll	(5) Enroll
RD estimator	0.137***	0.141***	0.122***	0.117***	0.156***
	(0.027)	(0.023)	(0.020)	(0.017)	(0.024)
Obs.	18,375	18,375	18,375	18,375	4,588
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: Sample of analysis only consider students with admission at their 1st or 2nd preference (margin of treatment 1-2) and with the admission simulated without the reform at 2nd preference (therefore threshold crossing mostly due to the boost)in 2013. The sample restriction attempt to capture the effect of threshold crossing for pulled-up students by comparing them with very similar controls (non-crossing but similar score). Running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20). Specification allow for different slope in each side of the cutoff.

Table 59: RD estimates on enrollment for crossing threshold for 1st preference for pulled-up students

	(1) Grad in 8yr	(2) Grad in 8yr	(3) Grad in 8yr	(4) Grad in 8yr	(5) Grad in 8yr
RD estimator	0.097***	0.079***	0.043*	0.043**	0.104***
	(0.032)	(0.028)	(0.024)	(0.021)	(0.029)
Obs.	18,375	18,375	18,375	18,375	4,588
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: Sample of analysis only consider students with admission at their 1st or 2nd preference (margin of treatment 1-2) and with the admission simulated without the reform at 2nd preference (therefore threshold crossing mostly due to the boost)in 2013. The sample restriction attempt to capture the effect of threshold crossing for pulled-up students by comparing them with very similar controls (non-crossing but similar score). Running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20). Specification allow for different slope in each side of the cutoff.

Table 60: RD estimates on enrollment for crossing threshold for 1st preference (2013)

	(1) Enroll	(2) Enroll	(3) Enroll	(4) Enroll	(5) Enroll
RD estimator	0.170***	0.185***	0.184***	0.178***	0.136***
	(0.009)	(0.008)	(0.007)	(0.005)	(0.012)
Obs.	90,205	90,205	90,205	90,205	15,623
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: 2013 sample of students at program with excess of demand. The outcome variable indicates if the students enrolled at the admission program. The running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20).

Table 61: RD estimates on peers' selectivity for crossing threshold for 1st preference (2013)

	(1) Selectivity	(2) Selectivity	(3) Selectivity	(4) Selectivity	(5) Selectivity
RD estimator	0.197***	0.211***	0.217***	0.217***	0.166***
	(0.005)	(0.005)	(0.004)	(0.003)	(0.007)
Obs.	84,770	84,770	84,770	84,770	15,011
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: 2013 sample of students at program with excess of demand. The outcome variable correspond to the average test score of the students enrolled at the same program. The running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20).

Table 62: RD estimates on enrollment for crossing threshold for 1st preference (2013)

	(1) Grad by 8yr	(2) Grad by 8yr	(3) Grad by 8yr	(4) Grad by 8yr	(5) Grad by 8yr
RD estimator	0.065***	0.068***	0.067***	0.063***	0.071***
	(0.009)	(0.008)	(0.007)	(0.006)	(0.014)
Obs.	90,205	90,205	90,205	90,205	15,623
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: 2013 sample of students at program with excess of demand. The outcome variable indicates if the students graduate from the admission program 8 years after the admission process. The running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20).

Table 63: RD estimates on enrollment for crossing threshold for 1st preference (2013)

	(1) Grad by 8yr	(2) Grad by 8yr	(3) Grad by 8yr	(4) Grad by 8yr	(5) Grad by 8yr
RD estimator	-0.006	-0.006	-0.012	-0.021***	0.002
	(0.010)	(0.009)	(0.008)	(0.006)	(0.015)
Obs.	90,205	90,205	90,205	90,205	15,623
Controls	✓	✓	✓	✓	✓
Poly	5	4	3	2	1

Robust standard errors in parentheses

Notes: 2013 sample of students at program with excess of demand. The outcome variable indicates if the students graduate from any program 8 years after the admission process. The running variable is the application score in the most preferred choice minus the cutoff score (ex-post defined by the application of the last person admitted at that program based on the vacancies) at that program. Columns 1-4 use the entire sample. Column 5 estimate the effect with a bandwidth of 20 point (optimal bandwidth range between 10 and 20).