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Essays on the Economics of Education

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Abstract

My dissertation consists of three chapters studying centralized education markets.

In the first chapter, we study the effects of changing the priority structure in the centralized high school admission system in Mexico City. Academically elite schools experience excess demand, while admission priorities are based on a standardized admission exam. The system ignores other skill measures such as grade point average (GPA), which may better capture non-cognitive skills that are important for later education and life-cycle outcomes. Using a Regression Discontinuity Design (RDD), we first show that marginal admission to an elite school decreases the graduation probability for students with below-median GPA and increases it for students with above-median GPA. Guided by this evidence, we then study the effects of a counterfactual admission policy wherein elite schools use a priority index that flexibly combines information on both the admission exam and the middle school overall GPA. Our counterfactual results show that more females and lower-income students would be admitted to elite schools, and the graduation rate at elite schools would increase by six percentage points. Overall, our findings show that including the information contained in GPA to define a priority structure improves equity of access, decreases mismatch, and increases graduation.

The second chapter uses five years (2005-2009) of administrative data on the centralized high school admission system in Mexico City to study whether the academic effects of being marginally admitted to an elite science school depend on the year of admission. I show that the effect on mathematics test scores at the end of high school decreases each year, starting positive and statistically significant in 2005 and ending close to zero and not significant by 2009. I propose two mechanisms to explain this trend. The first is related to changes over time in the composition of marginally admitted and rejected students combined with heterogeneity in the effect of marginal admission. The second considers changes over time in the production functions of elite and non-elite schools. Together, these results highlight the limited external validity of estimates obtained at a single point in time as they may be systematically influenced by time-varying changes in the educational context.

The third chapter studies students' choice between academic and non-academic schools when they are uncertain about their academic skills. We implement a Randomized Control Trial (RCT) and find that providing students in Mexico City with more accurate information about their academic skills creates a better alignment between students' skills and the type of schools they attend. This better alignment increases on-time graduation.

Keywords: School Choice, Mismatch, Selective Education, Educational Attainment

Summary for Lay Audience

The first chapter of my dissertation is motivated by the treatment of many centralized education markets as one-sided matching problems. In these markets, schools are seen as objects to be consumed by students, and the central planner creates a priority structure that schools should follow in admitting students. Since the priority structure is ultimately a planner's choice, current systems can improve it, especially if the one in place increases inequality and affects educational outcomes.

The motivation for the second chapter of my dissertation comes from the observation that education markets are not static. Schools and students within a centralized market may change over time. In this context, estimates of the effect of elite schools on test scores for a given period may depend on the aggregate state of the education market. If this is the case, we would expect to observe time-varying effects.

The third chapter of my dissertation focuses on school choice under uncertainty. A particular type of uncertainty students face when choosing schools is that they may not know their academic ability. Suppose students over or underestimate their academic ability when selecting schools, and there are match effects between schools and students. In that case, information could improve students' choices and help create better matches.

Co-Authorship Statement

Chapter 1 of this thesis is co-authored with Maria Elena Ortega-Hesles. Maria Elena allowed me access to the administrative level data for the analysis and helped me better understand the Mexican context.

Chapter 3 of this thesis is co-authored with Matteo Bobba and Veronica Frisancho. All the authors are equally responsible for the work that appears in Chapter 3 of this thesis.

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Matteo Bobba has been guiding me since 2012 and was always willing to help me navigate graduate school. His excitement for research has always inspired me. His continued belief in my academic potential was instrumental in me pursuing a Ph.D. and enduring this educational adventure.

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Of course, none of this would have been possible without the unconditional encouragement, support, and love from my partner, Sylvia Blom.

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Chapter 1

School Choice, Mismatch, and Graduation

1.1 Introduction

The use of centralized education systems that assign students to public schools is expanding worldwide [Neilson, 2019].[1] Because schools have limited seats and some schools experience higher demand than offered seats, system designers need a way to ration the available seats [Shi, 2022]. Since using prices as a rationing mechanism is not feasible in public education, and schools are not allowed to have preferences over students, policymakers define priority structures that assign a priority index to each student. Priorities solve the excess demand problem by providing an ordering in which students gain admission to schools. Typical components of priority indices are siblings, residential zones, lotteries, a standardized exam, and GPA.

In practice, there is substantial heterogeneity in how current systems define priorities. Understanding the consequences of implementing a given priority structure is essential for several reasons. First, the inputs used to create a priority index within a system could affect the equity of access. For example, consider a scenario where males score higher on standardized exams while females have higher GPAs. If the system only uses a standardized exam to prioritize students, males will have more access than females to highly demanded schools. Second, a particular way to rank students could affect the graduation rate if it creates a mismatch between students' skills and schools' academic requirements. For example, giving higher admission priorities for the most academically demanding schools to students without the skills needed to graduate from them may potentially result in low graduation rates.

In this paper, we explore the issues mentioned above by exploiting the case of the centralized high school admission system in Mexico City. In this system, students' priority index is solely based on their scores in a system-wide admission exam. The system has different types of high schools. Elite high schools are more academically demanding and experience much higher demand than available seats. Both elite and non-elite schools use the same priority index.[2] We focus on the following question: Is the system ignoring valuable information that could be used to create better matches? Specifically, the system could benefit from broadening the priority index by also considering the information contained in GPA. We focus on GPA

[1] See Appendix A.1 for examples of centralized education markets and their common structure.

[2] Elite schools also have a minimum GPA requirement of 7/10, but most of the students meet this requirement (more than 90%). The minimum GPA to graduate from middle school is 6/10.

as a potential channel to improve student-school matches to the extent that previous literature shows that grades measure non-cognitive skills (e.g., effort and self-control) to a higher degree than achievement tests and that non-cognitive skills are important determinants of desirable educational outcomes such as graduation [Stinebrickner and Stinebrickner, 2006; Duckworth et al., 2012; Borghans et al., 2016; Jackson, 2018].

We use the administrative records of all the participants in the centralized high school admission process in Mexico City. We complement the admission data by collecting official high school graduation records for all the students assigned to schools through the centralized admission process. This unique dataset features three advantages for the analysis. First, we have information on the application and high-school graduation rates for more than 200,000 students, allowing us to explore rich heterogeneity without running into statistical precision problems. Second, we observe strategy-proof measures of students' ranking of schools (i.e., students' preferences).[3] Third, our dataset includes all the information necessary to replicate the realized student-school matches and additional student characteristics (such as the middle school GPA) that was not used to define priorities in the system.

The first part of the analysis sheds light on the importance of the skills captured by GPA and their influence on students' probability of graduation from the most over-subscribed schools in the system (i.e., elite schools). We do so by estimating the effect of being marginally admitted to an elite school on the probability of graduation. The assignment mechanism creates exogenously determined admission cut-offs for elite school admission. Using an RDD, we find that the effect of marginal admission to an elite school on the probability of graduation is close to zero and not statistically significant. However, students at the margin of admission to an elite school are very heterogeneous in terms of their middle-school GPAs. To study heterogeneity in the effect of interest, we estimate an RDD separately for students with above and below- median GPAs. We find that admission to an elite school *decreases* the probability of graduation by eight percentage points for students with below-median GPA. For students with above-median GPA, elite-school admission is associated with an *increase* in their probability of graduation of seven percentage points. The lack of an overall effect is explained by these two effects canceling each other out. Our results indicate that to benefit from a higher graduation probability when gaining marginal admission to an elite school, a student requires enough of the skills that GPA better captures.

We also implement RDDs separately for males and females and find heterogeneous effects by gender. We find that the effect for males is similar to the one for students with low GPAs, and the effect for females is similar to that for students with high GPAs. Males experience a decrease in their graduation probability, while females experience an increase in their graduation probability. A potential explanation behind these results is that in our data, even though males have higher admission exam scores than females, females have higher GPAs than males.

In terms of our research question, our first set of results imply that, even for students at the margin of admission to an elite school, an assignment mechanism that relies on a scalar measure of skills may create mismatch by missing out on important information about students' academic potential.

[3]The matching algorithm is the Serial Dictatorship which is strategy-proof [Svensson, 1999]. In addition, in Mexico City, students submit their ranking of schools before they know their priority index. Uncertainty in the priority index incentivizes truthful revelation of preferences.

In the second part of the analysis, we study the effects of a counterfactual admission policy that could better match students to schools. Our approach combines the reassignment of students to schools prompted by a change in the priority structure with a flexible discrete choice graduation model. We model the youth's decision to complete high school because our counterfactual admission policy affects students beyond the margin of admission to elite schools for whom our RDD estimates may not be informative.[4] We follow Dale and Krueger [2014] in that our graduation model includes controls for the characteristics of students' application lists to deal with commonly unobserved students' characteristics that could affect graduation. We validate the model predictions by showing that they reproduce the main patterns we previously obtained in our RDD analysis even though we did not target them when estimating the model.

Our counterfactual admission policy changes elite schools' priority index to equally weight GPA and the admission exam score, while non-elite schools still follow the status-quo priority rule.[5] Because of potential concerns regarding differential grading standards between middle schools, we also consider a case where instead of GPA, elite schools' priority index includes within middle school percentile ranking by GPA. Our results are not sensitive to this change. We only change the priorities of elite schools because those are the schools for which we showed heterogeneous results in the RDD analysis. In addition, we show that GPA influences elite school graduation more than it does non-elite school graduation.

Under the new priority structure, we run a more general version of the same assignment algorithm that allows schools to have differentiated priorities. The new algorithm preserves the theoretical properties of the one that is currently in place (e.g., strategy-proof).[6] We assume that students' ranking of schools does not change in the counterfactual because students' preferences do not depend on the system priority structure under a strategy-proof assignment algorithm. However, the change in priorities does affect the assignment of students to elite and non-elite schools.

Our counterfactual generates important changes in the composition of students assigned to elite schools. First, it increases the share of females assigned to them by nine percentage points. Females gain more access to elite schools thanks to receiving a higher priority index for their relatively high GPAs. Second, the share of low-income students at elite schools increases. Low-income students gain more access to elite schools because GPA is less stratified by income than the admission exam score.

Our graduation model gives us a mapping from students' characteristics to their probability of graduation from a particular type of school. We combine the estimated parameters from our model and the new students' characteristics allocated to each school to predict graduation rates in the counterfactual. We find that the graduation rate from elite schools increases six percentage points. The graduation rate increases because the counterfactual matches elite schools with higher GPA students who have more of the skills necessary to graduate.

Lastly, since our counterfactual assigns students to higher or lower-ranked schools in their application lists, we quantify its effects on students' ex-ante welfare. Students' rankings of

[4]For instance, under treatment effect heterogeneity by the running variable, RDD estimates are only informative for students at the margin [Rokkanen, 2015].

[5]As a robustness check, we do a counterfactual where all schools in the system use the information contained in GPA. Our results are not affected by this change.

[6]We also maintain the timing of the process. Students do not know their priority index before they submit their ranking of schools which incentivizes truthful revelation of preferences.

schools give us ordinal information about their preferences. However, students could have different valuations for schools and, depending on the intensity of their preferences, be affected differently by the change in admission policy. To facilitate comparisons of gains and losses on the same scale, we use students' ranking of schools and estimate students' indirect utilities in willingness-to-travel space to measure utility in miles. We find that in our counterfactual, females' welfare increases, and males' welfare decreases by approximately the same amount. In addition, low-income students' welfare increases while high-income students' welfare decreases. Behind our effects on students' welfare is that females and low-income students gain access to their top choices, while males and high-income students see less access to their top choices.

Our paper contributes to three strands of the literature. First, it contributes to the literature on centralized education systems. Most of the previous literature considers school priorities as given and studies the consequences of using different matching mechanisms to allocate students to schools [Pathak, 2011; Agarwal and Somaini, 2020]. Yet, defining a priority structure is an integral part of the design in a centralized system. Neilson [2019] review centralized education systems worldwide and highlight that the consequences of implementing different priority structures are currently understudied. Shi [2022] and Abdulkadiroğlu et al. [2021] are the more related papers to ours. Their focus is on finding optimal priority structures in centralized education systems. However, they do not look at students' downstream outcomes, such as graduation rates, that are crucial to assess mismatch within the assignment system.[7]

Second, we also contribute to the literature on using achievement tests and grades in education policy. The informational content of grades grows in importance when considering non-cognitive skills. Stinebrickner and Stinebrickner [2006] find that high school GPA is a strong predictor of study effort during college, while the ACT score is not. Duckworth et al. [2012] show that grades measure students' self-control more than achievement tests. Borghans et al. [2016] show that grades measure personality more than achievement tests and that personality is an important determinant of many relevant life outcomes. The informational content of grades calls into question the prominent role of achievement tests in educational policy. For example, Heckman et al. [2014] study a policy that treats the GED as equivalent to a high school diploma, while Duckworth et al. [2012] consider a policy that conditions school funding on the use of standardized tests. Our paper complements this previous literature by focusing on the consequences of a policy that ignores the informational content of grades when prioritizing students in a centralized education market.

Third, we contribute to the literature on heterogeneous treatment effects in an RDD by focusing on a case where a null average treatment effect occurs because positive and negative effects cancel each other out. Hsu and Shen [2019] design a test for heterogeneous treatment effects in RDDs and find that the effect of attending a better high school on the take-up rate of an exit exam is heterogeneous. They argue that heterogeneous treatment effects could explain previous findings showing a null average effect. Becker et al. [2013] implement an RDD and find that the effect of regional transfers in the European Union depends on regions having enough absorptive capacity to take advantage of them. Our results parallel theirs in that the

[7] As Agarwal et al. [2020] and Larroucau and Rios [2020] highlight, it is essential to understand how assignment mechanisms perform when evaluated on outcomes of policymakers' concern. Our focus on graduation rates gains relevance because students do not necessarily choose schools based on their match quality [Abdulkadiroğlu et al., 2020], yet policymakers care about graduation rates.

effect of marginal admission to an elite school depends on a student having enough of the skills required to take advantage of what elite schools offer.

The remainder of the paper proceeds as follows. Section 2 describes the education system in Mexico City. Section 3 provides details about the data we use for the analysis. Section 4 contains the first part of our analysis describing the implementation and results of our RDDs. Section 5 includes the definition of our counterfactual admission policy and its effects on assignment, graduation, and students' ex-ante welfare. Section 6 contains our conclusions.

1.2 Education in Mexico City

The schooling system in Mexico has three levels: elementary school, middle school, and high school. Elementary school is six years in length, middle school and high school are both three years. The centralized high school education system in Mexico City encompasses all the Federal District and 22 nearby urban municipalities in the State of Mexico. Most of the high school admission process participants are middle school students who reside in Mexico City and are in their last semester of middle school. Additional participants (less than 25%) attend middle schools outside Mexico City, already have a middle school certificate, or are enrolled in adult education.

Public high schools in Mexico City belong to one of nine sub-systems (Table 1.1). Each sub-system manages a different number of schools and offers its own curriculum. Two sub-systems, SUB 6 and SUB 7 in Table 1.1, are affiliated with the two most prestigious public universities in Mexico City and offer a more advanced curriculum. For the rest of the paper, we refer to the schools belonging to these sub-systems as elite schools.

Table 1.1: Sub-systems in 2007

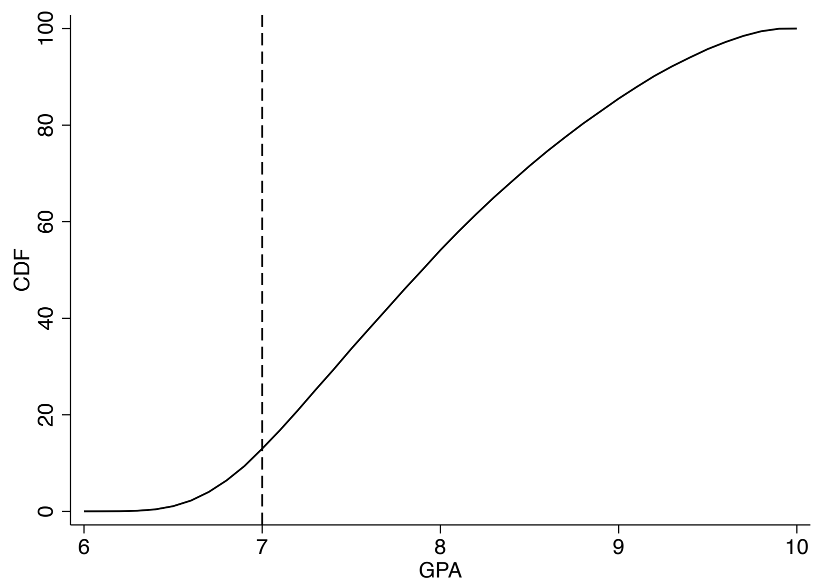
	Number of schools	Seats	First option	Admission cut-off
SUB 1	40	16.9%	6.1%	49.2
SUB 2	179	18.4%	5.8%	35.8
SUB 3	2	0.9%	0.5%	60.5
SUB 4	5	0.3%	0.2%	32.4
SUB 5	186	17.6%	7.7%	44.5
SUB 6	16	8.7%	14.5%	79.6
SUB 7	14	14.1%	48.5%	86.3
SUB 8	215	22.8%	16.1%	47.0
SUB 9	1	0.4%	0.7%	74.0
Total	658	100.0%	100.0%	45.0

The first column of Table 1.1 shows the number of schools affiliated with each sub-system. The second column indicates that elite schools offer only 23% of the total number of seats in the system. The third column shows a high demand for elite schools, 63% of students include an elite school as their first option. Since elite schools are heavily over-subscribed, admission to elite schools is very competitive, which leads to these schools having high admission cut-offs. We define an admission cut-off as the minimum score obtained by a student assigned to a given school in the 2006 admission cycle. The scores in the admission exam are between 31 and 128 points. The fourth column of Table 1.1 shows that elite schools' average admission cut-offs are the highest.

Every year around 300,000 students participate in the centralized high school admission process. In February, students receive an information booklet describing the steps they need to follow. The information booklet also lists all the available schools, their specializations, addresses, and previous years' admission cut-offs. The government also provides a website where students can download additional information about each school and use a mapping tool to see each school's exact location. In March, students submit a Rank Order List (ROL) listing up to 20 schools. In June, all students take a system-wide admission exam. We include a more detailed description of the admission exam in Appendix A.3.

All schools prioritize students based on the admission exam score. Elite schools exclude from consideration students with a middle school GPA lower than 7 out of 10. However, most of the students meet this requirement. To obtain a middle school certificate, students must have a GPA of at least 6 out of 10. In 2007, 90.62 percent of students met the requirement (Figure 1.1).

Figure 1.1: Elite schools minimum GPA requirement



Before implementing the matching algorithm, the schools decide the number of seats to offer. During the matching process, some students may have the same admission exam score and compete for the last available seats at a given school. In this case, schools decide to admit or reject all tied students. For example, if a school has ten remaining seats during the matching process, but 20 tied students compete for them, the school must decide between admitting all 20 or rejecting them all.

The matching algorithm is the Student Proposing Deferred Acceptance (SPDA).[8] Since all schools use the same priorities, the algorithm is equivalent to the Serial Dictatorship. The Serial Dictatorship algorithm ranks students by the admission exam score and, proceeding in order,

[8]We include a description of this algorithm in Appendix A.4.

matches each applicant to her most preferred school among the schools with available seats. We provide a more detailed explanation of the the Serial Dictatorship algorithm in Appendix A.5.

After implementing the matching algorithm, a student can be matched or unmatched. There are two reasons why some students are unmatched. First, some students do not clear the cut-off for any of the schools they list in their ROLs. Second, some students only apply to elite schools and do not meet the minimum GPA requirement. Unassigned students get the chance to register into schools that still have available seats after the matching process is over.

1.3 Administrative data

We use individual-level administrative data from the 2007 high school admission process in Mexico City for the analysis. In that year, there were a total of 296,778 students applying to 658 high schools. We observe students' admission exam score, ROL, GPA, assigned school, and socio-demographic characteristics such as gender and parental income.

On the high school side, we have information on the number of seats offered by each school, the sub-system to which each school belongs, and previous years' admission cut-offs for each school. With this information, we use the Serial Dictatorship algorithm and fully replicate the assignments we observe in the data (Table 1.2). Being able to replicate the student-school matches observed in the data gives us confidence in the transparency of the admission system.

Table 1.2: Matching outcome in 2007

		N	%
Matched		216,717	73.02
Unmatched		39,618	13.35
Subtotal		256,335	
Ineligible	< 31 in exam	5,841	1.97
	No exam	6,353	2.14
	No middle school	28,249	9.52
Total		296,778	100

We collected administrative graduation records from 2010-2012 (3-5 years after admission) to measure graduation. Because high school duration is three years, not graduating by 2012 is likely to measure drop-out. We obtained graduation records for all the students assigned to eight out of the nine sub-systems (80% of all the assigned students), including the two elite sub-systems. For the missing sub-system, we proxy for graduation using students' participation in a standardized exam they take during the last semester of high school (Dustan et al., 2016). Not all the schools participate in this exam, but all the schools in our missing sub-system do so. To be consistent in our definition of graduation, we use exam participation in any year between 2010-2012. We employ students' national identification numbers to merge the admissions data with the graduation or exam records.

Our data collection efforts provide us with three major advantages. First, we observe application and graduation records for a large number of students, which allows us to study heterogeneity in an RDD (Section 4). Second, thanks to the properties of the matching algorithm in place and the timing of the admission process, we observe strategy-proof measures of students' ranking of schools (i.e., their preferences). Third, having information on GPA allows us to explore a counterfactual admission policy that uses the information contained in this measurement to define alternative priority structures (Section 5).

Before proceeding with the analysis, we highlight two pieces of descriptive evidence. First, the graduation rate from elite schools in Mexico City is low (65%). Second, GPA matters for elite school graduation more than it does for non-elite school graduation. We show evidence on this second point in Appendix A.6 where include the results of estimating simple linear probability models of high school graduation for elite and non-elite schools.

1.4 Regression Discontinuity Evidence

1.4.1 Setup

All elite schools are over-subscribed, and admission to them requires clearing their admission cut-offs. We exploit these cut-offs to identify the effect of marginal admission to an elite school on the probability of graduation. We treat admission as equal to enrolment because enrolment at elite schools is almost universal. In our data, the enrolment rate for students admitted to an elite school is 97.42%.

We follow Dustan et al. [2017] and construct a sample of students assigned to an elite school with a score above or equal to a cut-off and assigned to a non-elite school otherwise. We impose three sample restrictions. First, we exclude all students that are ineligible for admission to an elite school. To be eligible for admission to an elite school, students must have a GPA higher than 7/10 during middle school. Second, we only include students that have applied to at least one elite school and one non-elite school. Third, we only include students that rank elite schools higher than non-elite schools. The purpose of the last restriction is to select students with similar preferences in that they prefer elite schools to non-elite schools.

The design follows the same intuition of Kirkeboen et al. [2016] strategy to estimate the effect of admission to a particular institution. In our case, we consider only two institutions, elite and non-elite. In the estimation sample, we have students whose first best is an elite school and their second-best a non-elite school in the local institution ranking (i.e., same preferences around their admission score). However, in addition to students having the same prefereces in the local institution ranking, we are only considering students that prefer elite to non-elite schools in the full ranking. We can impose this last restriction because most of the students who apply to both types of schools rank elite schools higher than non-elite schools. The previous restriction excludes 815 (0.76%) students, and our results are not sensitive to including or excluding these students.

In our estimation sample, each student has a minimum cut-off for elite admission c_k that depends on her preferences. For example, if a student applied to multiple elite schools, her admission cut-off would be the minimum cut-off among the elite schools she included in her application. Specifically, we define $k = 30$ groups of students that share a c_k. Within each group k, the following condition is satisfied:

$$\begin{cases} S_i \geqslant c_k & \text{admitted to some elite school} \\ S_i < c_k & \text{admitted to some non-elite school.} \end{cases}$$

To estimate our effect of interest, we pool our previously defined k groups and use a local linear regression with a triangular kernel. We obtain an optimal bandwidth following Calonico et al. [2014]. Our empirical specification is the following:

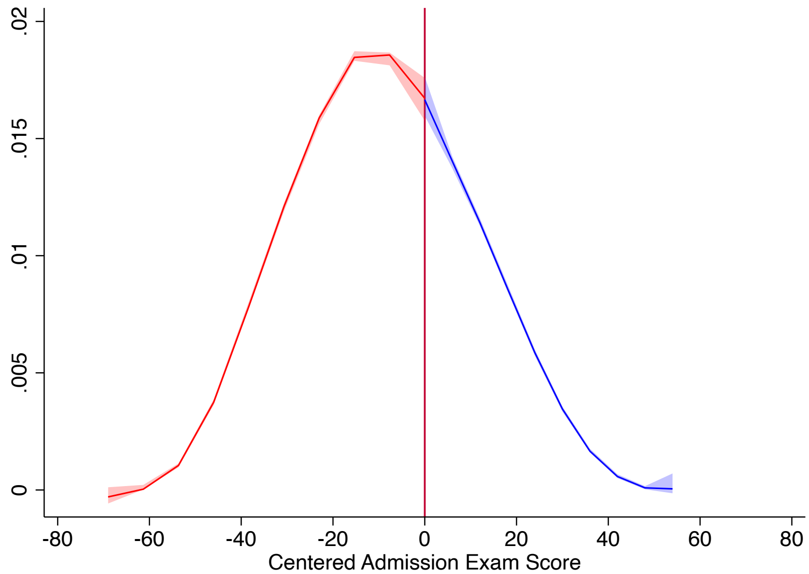
$$Y_{ik} = \mu + \gamma admit_i + \delta(S_i - c_k) + \tau(S_i - c_k) \times admit_i + \epsilon_{ik}. \tag{1.1}$$

In Equation 1, Y_{ik} is a dummy variable that denotes graduation for student i in group k. S_i is our running variable and denotes the score in the admission exam. We center the running variable by the group-specific admission cut-off c_k such that a positive value of $S_i - c_k$ indicates admission to an elite school. The dummy variable $admit_i$ takes a value of one when a student is admitted to an elite school and zero otherwise. We estimate this specification and one that includes cut-off school fixed effects; our results do not change.[9]

Regarding the validity of the design [Imbens and Lemieux, 2008], we show that there is no evidence of manipulation of the running variable around the admission cut-offs. If students could manipulate the running variable, they could sort themselves to be above an elite school admission cut-off. This type sorting is unlikely in our context for two reasons. First, admission cut-offs are determined in equilibrium after students submit their applications and take the admission exam. Second, students do not know their score in the admission exam until the end of the process. If there were manipulation, we would expect to observe bunching on the density of the running variable just above the admission cut-offs. Figure 1.2 shows the density of the running variable. The density does not show any bunching, and we do not reject its continuity at the admission cut-offs (T=-1.2). Our findings are consistent with the absence of manipulation.

[9] In addition, in Appendix A.9 we show that the results are not sensitive to restricting the sample to groups k with low or high cut-offs c_k.

Figure 1.2: Continuity test



As additional evidence supporting the validity of the design, Figure 1.3 shows that other predetermined covariates such as gender and GPA do not vary discontinuously at the cut-offs. We include estimates and standard errors in Appendix A.7.

Figure 1.3: Predetermined covariates

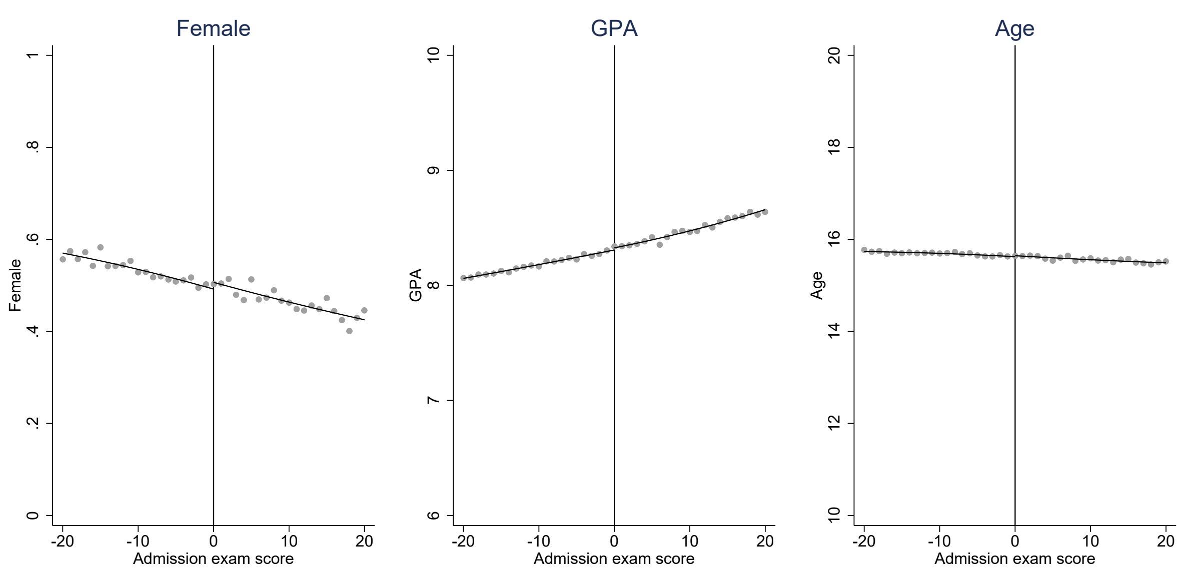
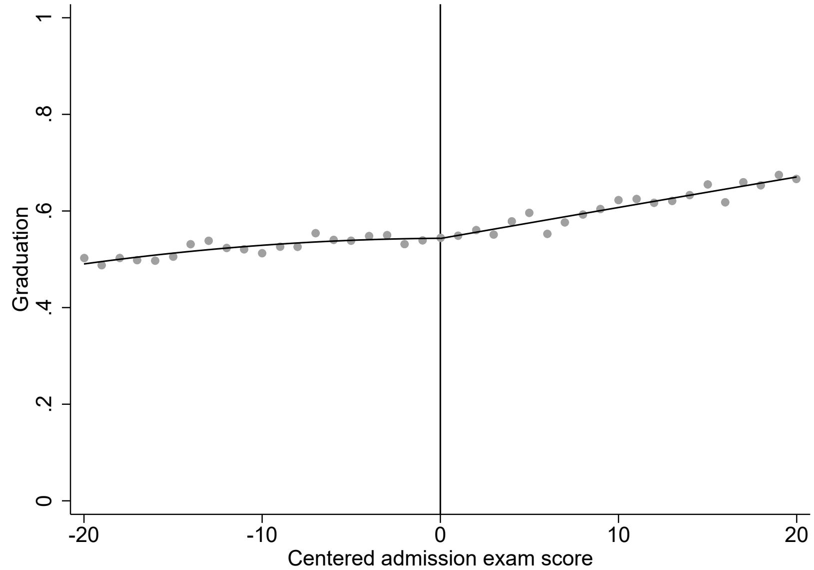


Figure 1.4 shows a graphical representation of the effect of marginal admission to an elite school on gradutaion without considering heterogeneity. Elite schools do not affect graduation for students marginally admitted to them. We show the estimated parameter $\hat{\gamma}$ and its standard error in Appendix A.8. The parameter is close to zero and is not statistically significant.

Figure 1.4: Elite schools effect on graduation



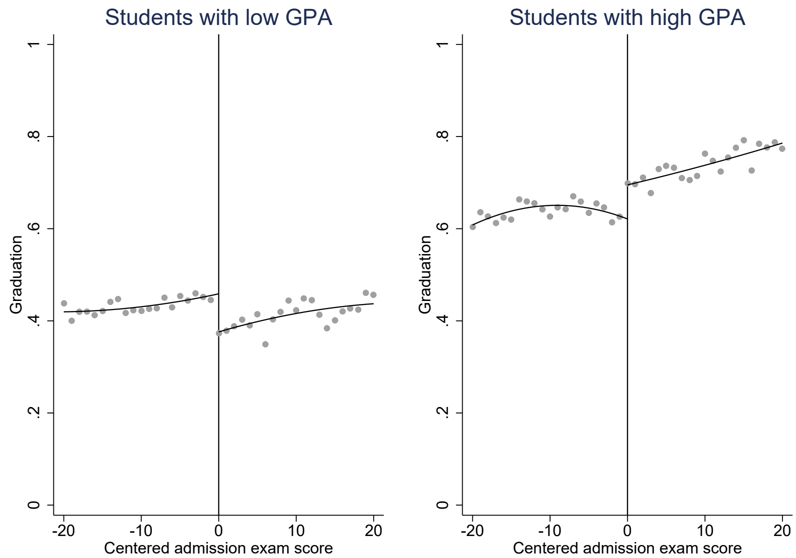
1.4.2 Heterogeneity by GPA

Students at the elite schools' admission cut-offs can be heterogeneous in other characteristics that affect graduation. For example, they can have high or low GPAs. Borghans et al. [2016] show that grades and achievement tests capture IQ and personality traits, but grades weigh personality traits more heavily. Since personality traits such as self-control or conscientiousness could matter for elite school graduation, we next explore if the effect is different for students with above and below-median GPAs.

In an extreme example, consider the case where the admission test captures IQ while GPA captures effort. Then, exploring our heterogeneity of interest would be equivalent to differentiating the effect of elite schools between high-ability low-effort students and high-ability high-effort students. In this example, to gain admission to an elite school, a student needs to perform well in the admission exam (high-ability), but she could be hard working or not. To the extent that graduating from an elite school requires you not only to have high ability but also to be hardworking, we would expect differentiated effects.

Figure 1.5 shows that the effect of marginal admission to an elite school on graduation is heterogeneous by previous GPA. It is negative (8 percentage points) and significant for students with below-median GPA, and it is positive (7 percentage points) and significant for students with above-median GPA. We include point estimates and standard errors in Appendix A.8. When we group high and low GPA students, the two effects cancel out and we get the results in Figure 1.4. We take these results as evidence that elite schools require a combination of ability and other skills that GPA better measures for a student to benefit from them (in terms of a higher graduation probability).

Figure 1.5: Elite school admission and graduation by GPA



1.4.3 Heterogeneity by gender

Previous literature shows that females tend to perform worse in standardized tests than males [Niederle and Vesterlund, 2010]. This gap in performance does not mean that females have lower skills but that there are gender differences in performance under competitive pressure. In this context, assigning students to elite schools based only on performance in an admission exam could be limiting females' access to them. Further, if females do have the skills required to benefit from elite schools, such an admission rule could increase mismatch and affect the graduation rate.

Consistent with previous research, our data shows that males score higher in the admission exam score, while females have higher GPAs (Figure 1.6). In the last section, we show that the effect of elite schools on the graduation probability depends on previous GPA. Since females have higher GPAs and, arguably, more of the skills needed to graduate from an elite school, we would expect to also observe heterogeneous effects by gender.

Figure 1.6: Admission exam score and GPA by gender

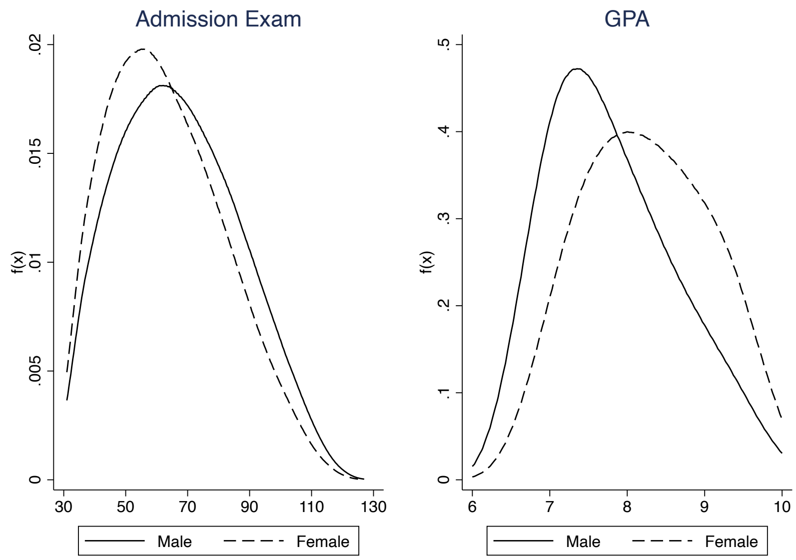
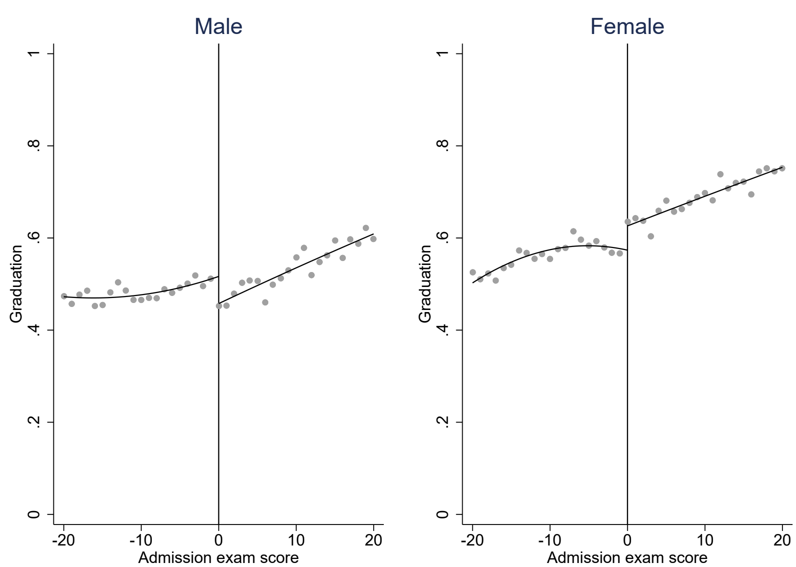


Figure 1.7 shows the results of implementing an RDD separately for males and females. The effect for males is almost identical (decrease of 6 percentage points) to the effect for students with below-median GPA, while the effect for females almost replicates (an increase of 6 percentage points) the effect for students with above-median GPA. We include point estimates and standard errors in Appendix A.8. Our results can partially be explained by differences in the skills that GPA measures between males and females.

Figure 1.7: Elite school admission and graduation by gender



Overall, the results of our RDD analysis tell us two facts. First, admission to elite schools increases the graduation probability for students with enough of the skills required to graduate from them, and GPA is better capturing these skills. Second, the current admission policy limits females' access to elite schools even though they could potentially benefit the most from them (in terms of higher graduation probabilities).

1.5 Counterfactual Admission Policy

Motivated by our RDD results, we examine the effects of a counterfactual admission policy wherein the central planner puts equal weight on the admission exam score and GPA when defining the priority index of elite schools. The priority index of non-elite schools does not change and remains using the admission exam score.[10] Because of concerns regarding differential grading standards between middle schools, we also study a counterfactual where instead of using GPA as an additional input, the central planner uses within middle school percentile rank by GPA in the priority index of elite schools. Our results are not sensitive to this change. In this section, we show the counterfactual using GPA, and we include the results using middle school percentile rank by GPA in Appendix A.11.

In terms of the matching algorithm, in the counterfactual, elite schools have a priority index different from non-elite schools. This change is equivalent to letting the centralized system use the more general SPDA algorithm that allows different schools to have different priority indexes. Thus, in our counterfactual, the matching algorithm is a more general case of the previously implemented, which does not affect its theoretical properties (e.g., strategy-proof).

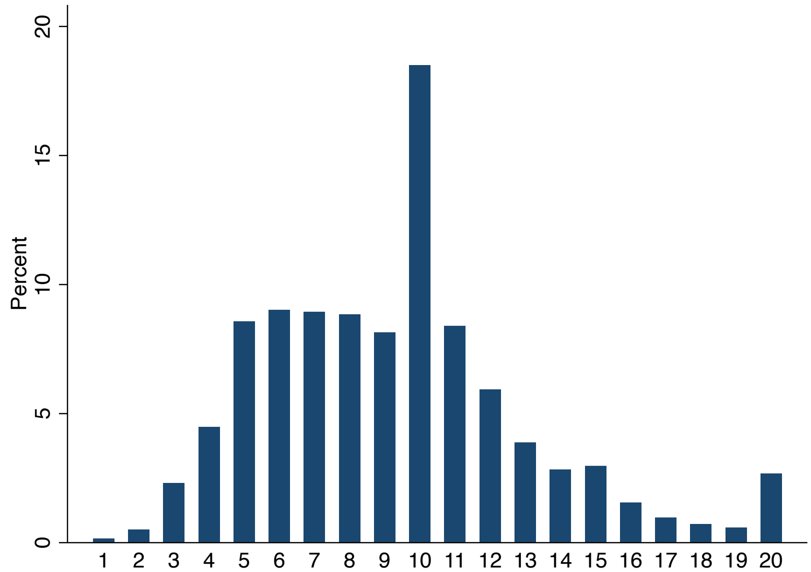
The primary assumption we make when analyzing the effect of our counterfactual policy is that students' ROLs do not change when priorities change. There are some cases when the change in priorities could affect the ROLs. One case considers that students could be strategic when choosing their ROLs. In this case, the change in priorities would change students' admission probabilities, and strategic students would consider the new admission probabilities and change their ROLs. We believe that, in our context, students are not strategic for two reasons.

First, the SD and SPDA algorithms are strategy-proof when the length of students' ROLs is unrestricted [Haeringer and Klijn, 2009]. Although the Mexican system constrains the length of the ROLs to 20, only 2.7% of students submit a ROL of the maximum length. In Figure 1.8, we show the distribution of ROLs lengths in our data. Since the constraint does not appear to be binding, the strategy-proof theoretical property likely holds in practice. That is, students truthfully report their preferences as their ROLs without considering admission probabilities.[11]

[10] Our results are not sensitive to the central planner putting equal weight on the exam score and GPA in the priority index of elite and non-elite schools. We include these results in Appendix A.10.

[11] Abdulkadiroğlu et al. [2017] impose a similar assumption when studying the centralized education system in New York City (NYC). The NYC system has around 400 high schools. Students can rank up to 12 schools. One of their arguments in favor of truthful revelation of preferences is that in practice, only 20% of students rank 12 schools.

Figure 1.8: ROLs length in 2007



Second, another case where truth-telling may break even under a strategy-proof algorithm is the strict priority setting. Fack et al. [2019] consider this case. In the strict priority setting, students know their priority indices (e.g., admission exam scores) before choosing their ROLs. Consequently, students face limited uncertainty about their admission outcomes and may choose to omit schools for which they have zero ex-ante probability of admission. Students can be more uncertain about their admission outcomes if schools use a priority index unknown to them when choosing their ROLS. This is the case in Mexico City. The uncertainty in the priority index leads to admission probabilities that are rarely zero ex-ante and incentivize truthful revelation of preferences.

Additionally, ROLs could change in the counterfactual if students' preferences depend on equilibrium outcomes. Consider the case where students' preferences for schools depend on the average skills of their future peers. Then, the change in priorities could affect the average skills of students assigned to different schools, changing students' preferences for schools and their ROLs. A common assumption in the school choice literature is that preferences do not depend on equilibrium outcomes. We also work under this assumption. Importantly, even though some students get placed and displaced from different schools in the counterfactual, the changes in average students' skills (admission exam score combined with GPA) at schools are small.

We estimate a flexible graduation model to obtain a relationship between students' characteristics and their graduation probability (Equation 1.2). We use a graduation model because our counterfactual admission policy affects students, for which our RDD estimates may not be informative. For instance, it affects students beyond the margin of admission to an elite school.

We do sub-system specific estimations to allow flexibility in how individual-level characteristics differentially affect graduation probabilities from different schools. The dependent variable Y is a binary variable that equals one if student i graduated from a high school in subsystem s, and zero if not. The independent variables (vector x) are the score in the admission exam, middle school GPA, gender, age, and a constant. Notice that the constants α_s in Equation 1.3 capture sub-system-specific effects on the graduation probability.

$$P_s(x) = P_s[Y = 1 \mid X = x] = E_s[Y \mid X = x] \text{ , where } j \in \{1, ..., 9\} \tag{1.2}$$

$$P_s(x) = G(\alpha_s + x'\beta_s). \tag{1.3}$$

Equation 1.3 provides a mapping between student characteristics and the probability of graduation from sub-system s. The mapping is defined by the parameters α_s, β_s and the link function G, which we assume to be the logistic distribution.

Besides this first specification, we include in vector x control variables for some commonly unobservable attributes that can affect the graduation probability and be correlated with the included regressors. This set of additional controls are motivated by Dale and Krueger [2014] empirical specification, which takes advantage of the information revealed in college application lists. To include measures of aspirations or motivation, we add controls for the number of elite schools in students' ROLs, the length of their ROLs, and the average quality of the schools in their ROLs.[12] This is the specification we use to predict graduation outcomes in the counterfactual.

Because our graduation model relies on a distributional assumption, we perform a model validation exercise. For our model validation, we use the graduation probabilities predicted by our model in the baseline and perform the RDD analysis from the previous section using these predictions as the outcome. We find that our model prediction reproduces the main patterns of our RDD results. First, there is no average effect of marginal admission to an elite school. Second, there is a negative effect for below-median GPA students, and a positive effect for above-median GPA students. Third, there is a negative effect for males and a positive effect for females. We include the validation figures and tables in Appendix A.12.

Our counterfactual assigns some students to different schools than their initial assignment. For example, consider a student assigned to a school in sub-system s in the data who is assigned to a school in sub-system s' in the counterfactual. To calculate her graduation probability at the new sub-system, we use the mapping from student characteristics to the graduation probability we previously obtained for sub-system s'. The counterfactual probability of graduation for this student follows Equation 1.4.

$$\hat{P}_{s'}(x) = G(\hat{\alpha}_{s'} + x'\hat{\beta}_{s'}) \tag{1.4}$$

Notice that an implicit assumption in this exercise is that the parameters α_s and β_s do not change in the counterfactual. Consider the case where these parameters capture fixed sub-system characteristics such as infrastructure or quality of teachers. Then, the counterfactual is changing the composition of students that interact with these attributes. A more complex case is when α_s and β_s also capture the effect of the average peer quality on a student graduation

[12] Our measures of quality are the schools' admission cutoffs in the previous year. The average quality of a ROL is the average of the previous year schools' cutoffs listed in the ROL.

probability. Even under this case, our counterfactual remains informative if the average peer quality at different sub-systems does not change much. If we measure peer quality by a combination of the admission exam score and GPA, the changes in average peer quality are small. For example, in the counterfactual, students' at the elite sub-systems have higher GPAs but lower admission exam scores.

1.5.1 Results

Our counterfactual exercise results in a different allocation of students across schools. In Table 1.3, we show the reallocations across elite and non-elite schools. In general, most of the students remain in their initial type of school. Importantly, our counterfactual exercise still considers students' choices and only moves students to other schools if they are part of their ROLs and are ranked in nearby positions as the schools of initial assignment.

Table 1.3: Initial and counterfactual assignment

	Counterfactual		
	Non-Elite	Elite	Total
Non-Elite	152,117	9,317	161,434
	94%	6%	
Elite	8,930	42,220	51,150
	17%	83%	

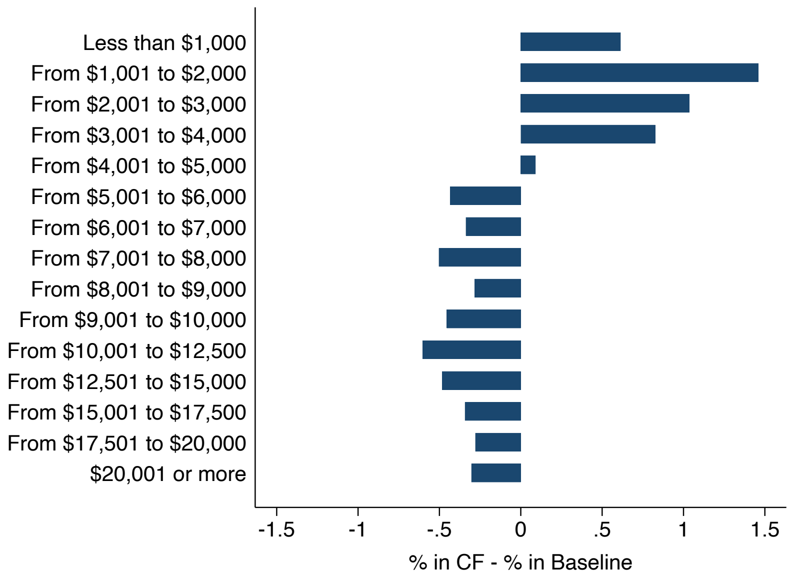
We next investigate if our counterfactual has some consequences in the gender composition of students assigned to elite schools. Table 1.4 shows that our policy increases the share of female students assigned to elite schools by nine percentage points. This change occurs because females demand elite schools in their ROLs, but the current admission policy limits their access. By adding weight to GPA, a measure in which females outperform males, more females gain access to elite schools. Table 1.4 also shows that our counterfactual increases elite schools' graduation rate by six percentage points. The graduation rate increases because the counterfactual assigns more high GPA students to elite schools, and these students have more of the skills needed to graduate from them.

Table 1.4: Changes in composition and graduation rates

	Initial	Counterfactual	Diff
Elite			
Female	45.12%	53.32%	8.21
Graduation	64.58%	70.71%	6.14
Non-Elite			
Female	50.88%	48.50%	-2.37
Graduation	45.88%	44.72%	-1.16

In addition, in Figure 1.9, we show that our counterfactual increases the share of low-income students assigned to elite schools. Income is highly correlated with the admission exam score but less correlated with GPA. The correlation between income and the admission exam score can be partially explained by high-income students having access to private exam-preparation institutions that are costly. Adding weight to GPA makes the admission exam score relatively less important and increases low-income students' access to elite schools. Both low and high-income students demand elite schools, but low-income students have less access to them in the current system.

Figure 1.9: Changes in the income composition of students at elite schools



1.5.2 Ex-ante Students' Welfare

To approximate effects on students' ex-ante welfare, we first use the position in the ROL a student is assigned. Since students choose schools based on their utilities, gaining admission to a highly ranked option provides them higher utility. In Table 1.5, we show that, on average, there are no changes in the position in their ROLs students are assigned. However, when separating males and females, we can see that the share of females assigned to their first option increases while the share of males assigned to their first option decreases. We interpret these results as a welfare trade-off between females and males.

Table 1.5: Position in ROL

	All		Female		Male	
	Baseline	CF	Baseline	CF	Baseline	CF
1	40.43%	40.46%	35.02%	38.58%	45.71%	42.35%
2	14.04%	14.03%	13.92%	13.89%	14.15%	14.16%
3	10.01%	10.09%	10.61%	10.42%	9.43%	9.77%
4	8.28%	8.26%	9.21%	8.54%	7.37%	7.98%
5	7.21%	7.09%	8.07%	7.31%	6.37%	6.87%

A limitation of our results in Table 1.5 is that we cannot quantify how much males' and females' welfare change in the counterfactual. For example, it could be the case that females' welfare increases slightly while males' welfare decreases by a lot, since these quantities depend on males and females' preferences. To complement our welfare analysis with a measure with cardinal value, we estimate students' preferences and scale the indirect utility by the distance coefficient. In this way, we can measure students' welfare in miles.

Define the indirect utility U_{ij} student i gets from school j as follows:

$$U_{ij} = \underbrace{x_j'\beta + \xi_j}_{\delta_j} + x_j'\Gamma z_i - D_{ij} + \epsilon_{ij} \tag{1.5}$$

$$U_{ij} = \underbrace{\delta_j + x_j'\Gamma z_i - D_{ij}}_{V_{ij}} + \epsilon_{ij} \tag{1.6}$$

In Equation 1.5, x_j is a vector of school characteristics that includes an elite school indicator and the previous year's admission cut-off. We also add an unobserved school characteristic denoted by ξ_j. We group individual invariant regressors in the coefficients δ_j that capture school fixed-effects. In vector z_i, we include individual-level characteristics: a standardized exam score (different from the admission exam), GPA, and gender. We do not include the admission exam score because students do not have this information when choosing their ROLs. We also include the distance from a student middle school to each of the available high schools (D_{ij}). We normalize the distance coefficient to one.

We follow Beggs et al. [1981] and estimate preferences using a Rank-Ordered Logit. Denote the length of a student ROL_i as K_i. Then, the probability that student i chooses ROL_i is:

$$P[ROL_i = (j_1, j_2, ..., j_{K_i}) \mid x_j, z_i, D_{ij}; \theta] = \frac{exp(V_{ij_1})}{\sum_{l \in J} exp(V_{il})} \times ... \times \frac{exp(V_{ij_K})}{\sum_{l \in J \setminus \{j_1, ..., j_{K_i - 1}\}} exp(V_{il})}. \tag{1.7}$$

The log-likelihood of the observed ROLs in the data is:

$$L(\theta) = \sum_i^N logP[ROL_i = (j_1, j_2, ..., j_{K_i}) \mid x_j, z_i, D_{ij}; \theta]. \tag{1.8}$$

Even after obtaining estimates of the preferences' parameters, we still do not observe individual-level indirect utilities. However, we can use our estimated choice model to calculate the expected indirect utility a student obtains from her assignment in the data and her assignment in the counterfactual. Notice that the observed ROLs impose restrictions in the space where ϵ_{ij} can be [Abdulkadiroğlu et al., 2017]. Denote $\mu_{data}(i)$ and $\mu_{CF}(i)$ as functions that indicate to which school j student i is assigned in the data and in the counterfactual, respectively. We calculate student welfare in the data as:

$$W(\mu_{data}(i)) = E\left[U_{i\mu_{data}(i)} \mid V_{ij}, ROL_i\right]. \tag{1.9}$$

To calculate welfare in the counterfactual we use the assignments we obtained after implementing the SPDA matching algorithm under the new priorities. Student welfare in the counterfactual is:

$$W(\mu_{CF}(i)) = E\left[U_{i\mu_{CF}(i)} \mid V_{ij}, ROL_i\right]. \tag{1.10}$$

Consistent with our results in Table 1.5, students' welfare distribution does not change in the counterfactual (Figure 1.10). But as we also know from Table 1.5, there is heterogeneity in this effect by gender. Figure 1.11 shows that the welfare distribution is shifted to the right for females while shifted to the left for males. Average female welfare increases 0.5 miles, and average male welfare decreases 0.4 miles. These results show that our counterfactual induces a welfare trade-off between males and females, and it does not disproportionately affect one group or benefit another.

Figure 1.10: Change in welfare

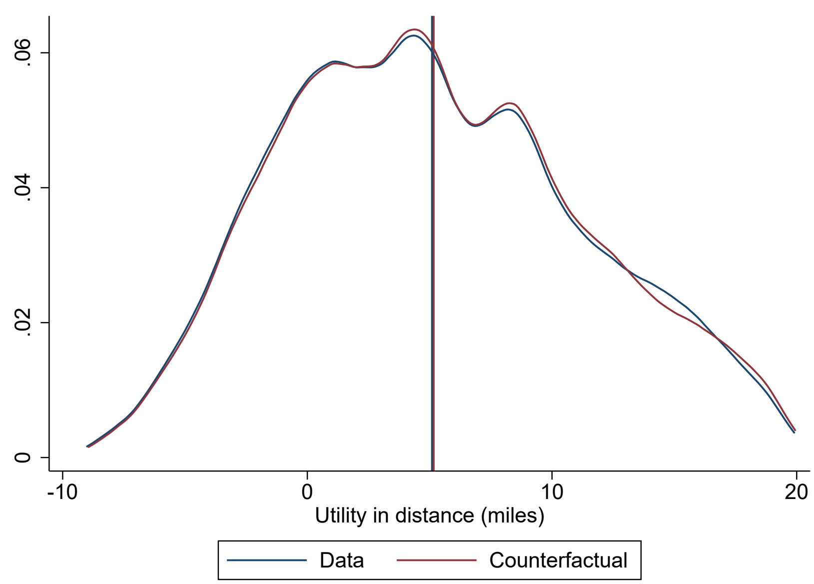
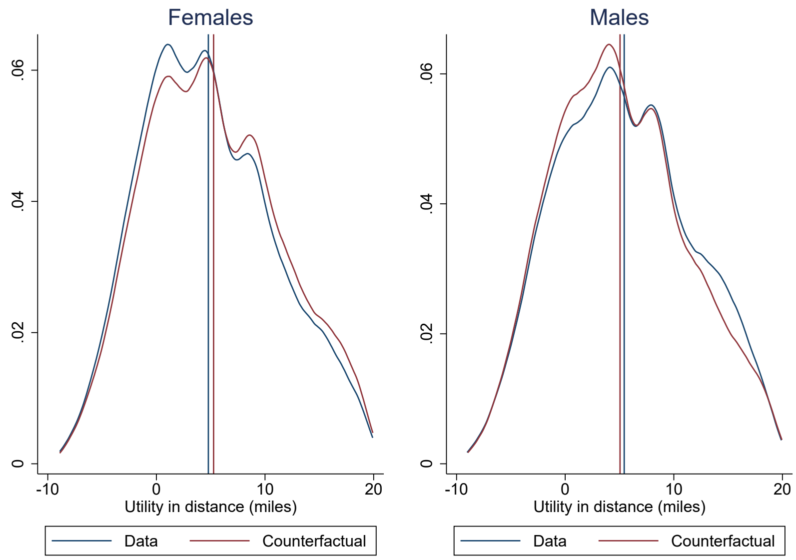
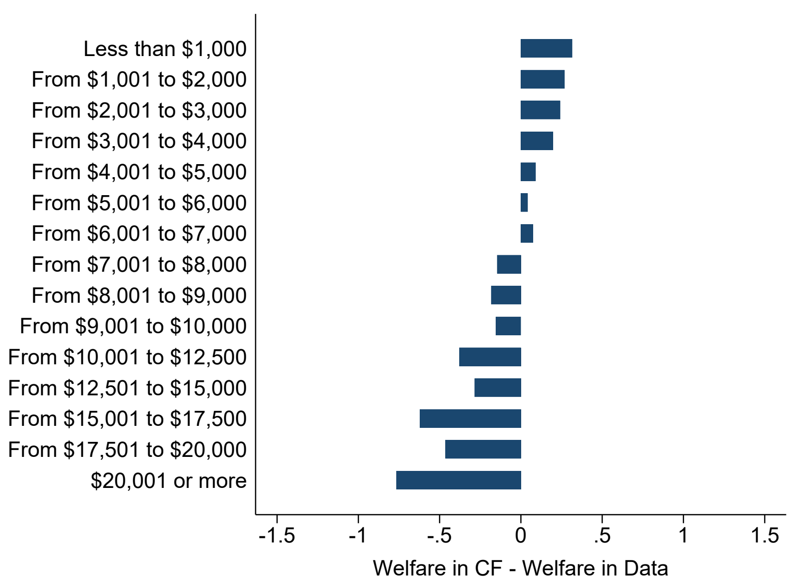


Figure 1.11: Change in welfare by gender



Lastly, as we show in Figure 1.9, our counterfactual increases low-income students' access to elite schools. If elite schools are valuable for these students, then we would expect an increase in their welfare. In Figure 1.12, we show that this is the case. The welfare of low-income students increases while the welfare of high-income students decreases. The direction of the effect changes at a family income of 7,000 pesos. Importantly, 79% of students come from households with family incomes of less than or equal 7,000 pesos.

Figure 1.12: Change in welfare by income



1.6 Conclusions

The way a priority structure is defined in a centralized education system can affect equity of access and graduation rates. The relevance of this choice is highlighted when priorities include skill measurements, and students have heterogeneous skills because a given priority structure could match students without the skills needed to graduate with the most academically demanding schools. School priorities also play an essential role when evaluations of centralized education systems go beyond efficiency measures based on revealed preferences and consider other policy-relevant outcomes such as equity of access and graduation rates.

We exploit the case of the centralized high school admission system in Mexico City, where priorities are based on a standardized admission exam, to study the effects of including the information contained in GPA as part of elite high schools' priority index. We focus on GPA because previous literature shows that grades measure non-cognitive skills to a higher degree than achievement tests and that non-cognitive skills are a strong predictor educational success. We first show that students marginally admitted to elite schools experience an increase in their graduation probability only when they have above-median middle school GPA. In addition, the effect is also positive only for females, partially because they have higher GPAs than males. Our first set of results motivate the importance of considering heterogeneity in skills when studying how elite schools affect the graduation probability even for students at the margin of admission.

Guided by our first set of results, we then study the effects of a counterfactual admission policy where the central planner puts equal weight on the admission exam and GPA in elite schools' priority index. We also consider a case where elite schools' priority index includes middle school percentile ranking by GPA instead of GPA. Our results are not sensitive to this change. Our counterfactual results are three. First, more females and low-income students gain access to elite schools. Second, the graduation rate from elite schools increases by six percentage points. Third, the system's average ex-ante students' welfare remains the same, but females' and low-income students' welfare increases while males' and high-income students' welfare decreases. Female welfare increases by approximately the same quantity as male welfare decreases, which indicates a welfare trade-off.

A limitation of our paper is that our counterfactual admission policy could induce additional behavioral responses that we are not currently considering. For example, it could change students' effort allocation between exam preparation and middle school coursework by increasing the effort allocated to coursework. In this paper, we assume that study effort does not change. However, if increased study effort in middle school coursework leads to higher study effort in high school coursework, then our effect on the elite schools' graduation rate would be a lower bound.[13] Another possibility is that middle school grades could increase not because of students' higher study effort but because of teachers' changing grading standards, leading to grade inflation. However, as we showed in the analysis, our results do not vary if, instead of GPA, the elite schools' priority index uses middle school percentile ranking by GPA, which is not affected by grade inflation. In general, depending on their primary concern, policymakers could flexibly choose how to incorporate the information contained in GPA instead of throwing

[13] Stinebrickner and Stinebrickner [2006] show that coursework study effort is strongly correlated across time between high school and college.

that information away.

From a policy perspective, our results indicate that using the information contained in GPA when defining admission priorities to elite over-subscribed schools can benefit the centralized system in Mexico City. More broadly, other centralized systems like the one studied here in that they rely on a unique standardized test to define school priorities could also benefit from adding some weight to the skills better measured by grades. Examples of such systems are the centralized education systems in Romania, Kenya, and the college admission system in China.

Chapter 2

Time Varying Effects of Elite Schools: Evidence from Mexico City

2.1 Introduction

In many centralized education systems a considerable proportion of students apply to attend academically elite schools (hereafter, referred to as elite schools). They apply hoping that the known attributes that these elite schools offer, such as better peers, better infrastructure and more qualified teachers, will translate into high academic returns for themselves. However, it remains unclear if the expected benefits of such attributes do materialize and whether they accrue to all students equally. Furthermore, the potential benefits could depend on the admission year and the characteristics of the educational system at that time. For example, if a centralized education system increases school competition, non-elite schools could seek to catch up with the academic quality of elite schools. In this case, the effect of admission to an elite school would depend on where in the catch up process the non-elite schools are relative to the elite ones.

Estimating the effects of elite schools on academic outcomes is challenging because applying and gaining admission to an elite school may be correlated with unobservable student characteristics, such as ability. Previous literature has addressed this problem by exploiting the way centralized educational systems operate using Regression Discontinuity Designs (RDDs) to estimate causal effect(s). The intuition behind this identification strategy is that oversubscribed schools in centralized systems generate admission cutoffs that allow for the comparison of academic outcomes between marginally admitted and marginally rejected students who are ex-ante equivalent other than their admitted or rejected status.

The results of the RDD studies that examine the effect of elite admission vary across settings. For example, Abdulkadiroğlu et al. [2014] found that admission to elite schools (exam schools) in Boston and New York City had no effect on test scores. Similarly, Dobbie and Fryer Jr [2014] found no effects of exam schools in New York City on future college enrolment or college quality. Contrasting the previous findings, Pop-Eleches and Urquiola [2013] found that gaining access to higher achieving schools had a positive effect on graduation test scores for students in Romania. Similarly, Kirabo Jackson [2010] found large positive effect of attending better secondary schools on exam scores in Trinidad and Tobago. In Mexico, Dustan et al. [2017]found a positive effect of admission to academically elite high schools on mathematics test scores at the end of high school. The heterogeneity of these results could be due to institutional differences across the countries being studied [Hanushek, 2021]. In addition, it is also possible that within a given country there are institutional differences over time and they systematically affect the results of particular years.

In this paper I study if the estimated effect of elite schools on academic outcomes change over time for a fixed set of elite schools. I focus on the case of Mexico City over a period of five years from 2005 to 2009 and work with yearly administrative data from its centralized high school admission system. These data present two advantages. First, the centralized admission process did not change its admission policies over this period and every year it only considered the results of a placement exam and the students' preferences for high schools. This allows me to implement RDDs and estimate the academic effects of being marginally admitted to an elite high school in each admission year. Second, having five years of administrative data allows me to study how time-varying factors in the educational system affect the effect of elite admissions over time. Throughout this paper, I consider as elite high schools a group of 16 schools that specialize in providing science education. All of them enjoy a high reputation and are perceived to be among the best high schools in Mexico City.

My main results indicate that the effect of being marginally admitted to an elite high school on end of high school math test scores depends on the admission year and decreases across time. In particular, the effect monotonically decreases every year and changes from being positive and statistically significant in 2005 to being insignificant by 2009. In addition, the effect on end of high school Spanish test scores go from being insignificant to negative and statistically significant. I also find that marginal admission to an elite school increases the probability of dropping out, but this effect is roughly constant over time. This alleviates concerns regarding time-changes in sample selection as an explanation for the time varying effects on test scores.

In order to explain the trend of the effects on test scores, I propose two mechanisms related to changes in the educational system over this period. First, between 2005 and 2009 there was an increase in the demand for elite high schools, while the number of spots they offered remained constant. This mechanically increased their selectivity level and affected the composition of marginally admitted and rejected students. If the effect of marginal admission is heterogeneous, this change in the composition of students at the margin could partially explain the decrease in the academic effect under certain assumptions on the education production functions of elite and non-elite high schools.

The second mechanism to explain my results explores changes over time in the academic quality of elite and non-elite high schools. To support this mechanism, I provide evidence of a decreasing trend in the academic quality of the majority of elite high schools and an increase in the academic quality of the high schools where most of the marginally rejected students ended up assigned by the centralized mechanism. My findings suggest that a combination of both mechanisms could explain why the effects on test scores changed so dramatically. I leave a quantitative analysis separating the relative contribution of each mechanism for future work.

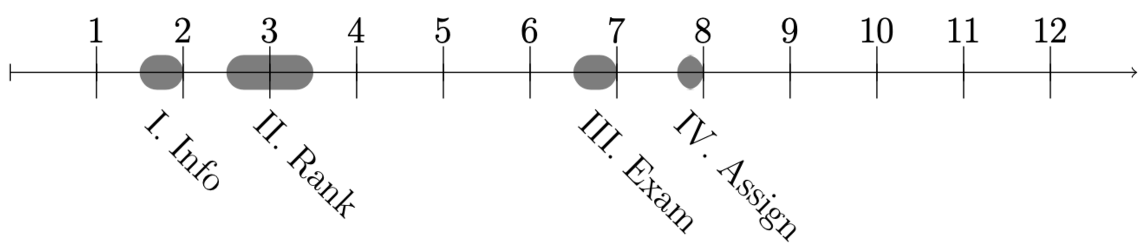
The remainder of the paper proceeds as follows. Section 2 describes the education system under study. Section 3 describes the data and sample used for the estimation. Section 4 describes the empirical strategy and offers evidence on the validity of the research design. Section 5 presents the main results of the paper. Section 6 explores different mechanisms that could explain the results. Section 7 concludes.

2.2 The centralized system

2.2.1 Students

In this paper, I study the school choice admission process in Mexico City that matches middle school students to public high schools. Figure 2.1 shows its monthly timeline. The admission process remained unchanged during the study period (2005-2009).

Figure 2.1: Timeline of the admission process



A booklet with information about available high schools is provided at the end of January to middle school students. Between late February and early March, students submit a list of up to twenty high schools ranked in order of preference. At the end of June, students take a standardized admission exam. Exam scores are released at the end of July, and students are assigned to high schools based on their exam scores and stated preferences.

2.2.2 Schools

There are approximately 600 high schools in Mexico City. Each school determines the number of available seats before the assignment of students, and this is not known by students when submitting applications.

Every high school in Mexico City belongs to one of nine subsystems. Two of the subsystems are considered elite: the IPN and the UNAM. High schools belonging to the IPN and UNAM subsystems face high student demand and are consistently oversubscribed.

In this paper, I will only study the effects of being admitted to the IPN subsystem because I do not have information on outcome variables for students admitted to UNAM. The IPN subsystem includes 16 high schools, all with science-focused curricula. I will refer to these 16 high schools as elite high schools.

2.2.3 Matching

The assignment process follows the serial dictatorship mechanism, a particular case of the student proposing deferred acceptance mechanism. The main steps are the following:

1. Students' preferences and exam scores are collected.
2. Students are ordered according to their scores.
3. Highest scoring students are assigned first. Students are assigned to their most preferred school that still has an available seat.

Since high schools define the number of seats before the matching, some high schools experience more demand than available seats. I will refer to these high schools as oversubscribed.

2.2.4 Academic outcomes

At the end of high school, all Mexican students take a standardized exam that evaluates them on mathematics and Spanish. The government mainly uses this test to assess high school-level performance. It has been previously shown that participation in this test is a good proxy for graduation from high school [Dustan et al., 2017] since students who participate must be registered in the last semester of classes.

By combining the admission and high school exit exam data, I generate my outcomes of interest: graduation/drop-out and end of high school test scores. A dropout is defined as a student assigned to a high school in the admission process that does not take the exit exam three or four years later. For the students who do not drop out, I consider their performance on the mathematics and Spanish tests to measure learning.

2.3 Sample

This section describes how I construct the analysis sample for a given year. I follow the same procedure for each year in the sample period (2005-2009). To use a sharp RDD, I need to have a sample of students assigned to an elite high school with a score above or equal to a predetermined cut-off and be assigned to a non-elite high school otherwise.

To define an admission cut-off, I use the fact that there is a last admitted student for every high school that is oversubscribed. This is the case for all elite high schools and many non-elite high schools. Thus, I define the admission cutoff for an oversubscribed high school as the score of its last admitted student. For high schools that are not oversubscribed, I define their admission cutoff as the minimum score students can obtain and still participate in the assignment process.

2.3.1 Sample restrictions

I impose several sample restrictions. First, I exclude all students that would never be assigned to an elite high school. To be eligible for admission to an elite high school, students must have a GPA higher than 7/10 during middle school. Therefore, I only include in my sample students that meet this requirement. Additionally, I only include students that have applied to at least one elite high school and one non-elite high school. Second, to minimize the risk of students switching to a private sector school if they don't get admitted to their preferred school, I only keep students that attended public middle schools (as opposed to private middle schools). Third, I only include students assigned in the first round of the matching algorithm. This is primarily because being assigned in the second round implies only being able to choose from a subset of schools that still have open seats, and the behavior of these students could differ greatly from the rest.

An additional sample restriction is related to students' type of preferences to satisfy the conditions for a sharp RDD. For this sample restriction, I follow Dustan et al. [2017], where the authors also studied the effects of marginal admission to the same set of elite high schools from 2005-to 2006. I only keep students that first rank elite high schools and then non-elite high schools in their preferences. This sample restriction allows me to satisfy the condition that a student is assigned to a high school in the elite group with a probability of one above a given admission cut-off. Examples of the type of preferences students have in the final sample are shown in Figure 2.2. For these students, I define the individual-specific admission cut-off score as the cut-off of her lowest-ranked elite school (shaded squares in Figure 2.2).

Figure 2.2: Preferences in sample

ID	OP1	OP2	OP3	OP4	OP5	OP6
1	ELITE-1	ELITE-2	ELITE-3	ELITE-4	NOELITE-1	NOELITE-2
2	ELITE-2	ELITE-3	NOELITE-2	NOELITE-3	NOELITE-4	NOELITE-5
3	ELITE-1	ELITE-2	NOELITE-2	NOELITE-3		
4	ELITE-4	NOELITE-3	NOELITE-4			

Finally, I drop from my sample any student that could be assigned to a UNAM high school instead of a non-elite high school when scoring below her elite school cutoff score. This is because I do not have data on outcomes for the other elite subsystem (UNAM), and I am studying the effects of gaining admission to an elite high school in comparison to a non-elite high school.

2.3.2 Sample characteristics

My final samples consist of approximately 20,000 students each year between 2005 and 2009. As an illustration, Table 2.1 shows some differences between the total number of students and the students included in the 2009 sample [1]. In the final sample, a higher proportion of students are male, have more educated fathers, come from higher-income families, and obtain higher average scores on the admission test.

Table 2.1: Sample 2009

	All	Sample
Male	0.51	0.62
Father has high school or more	0.35	0.37
Family income is more than 5k	0.34	0.36
Admission exam score	60.79	70.52
Observations	317,603	23,318

[1] The respective tables for the other years are included in Appendix B.1.

All of the differences shown are statistically significant. These differences can be explained by selecting the sample based on having a relatively high GPA and intentions to attend an elite high school. Thus, any results obtained are constrained to students with the particular characteristics of my final sample and cannot be generalized to the complete set of applicants.

2.4 Empirical strategy

2.4.1 Specification

To identify the effects of being admitted to an elite high school, I will use an RDD. The intuition behind the identification strategy is that marginally admitted and rejected students from elite high schools are similar in observable and unobservable characteristics. I follow Dustan et al. [2017] for my empirical specification but estimate a year-specific effect between 2005 and 2009.

$$Y_{ik} = \alpha_1 admit_i + \mu_k + \alpha_{3k}(S_i - \underline{s}_k) + \alpha_{4j}(S_i - \underline{s}_k)admit_i + \epsilon_{ik} \tag{2.1}$$

Where:

- Index i is for student and $k \in \{1, \cdots, 16\}$ for cutoff high school
- S_i is the admission exam score
- $\underline{s}_k$ is the admission cutoff of the cutoff high school
- $admit_i$ is a dummy variable such that $admit_i = 1$ when $S_i - \underline{s}_k \geq 0$
- μ_k is a cutoff high school fixed effect

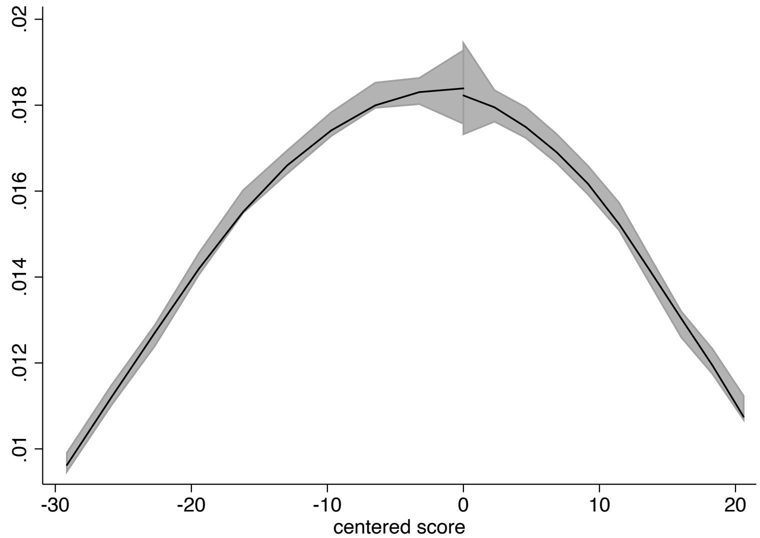
The coefficient of interest is α_1. It measures the intent-to-treat (ITT) effect of gaining marginal admission to an elite high school instead of a non-elite one. For the estimation, I use the optimal bandwidth obtained by following Calonico et al. [2014], which minimizes the mean square error. Within the optimal bandwidth, I estimate the parameters in Equation 2.1 by using a local linear regression with a triangular kernel and cluster the standard errors at the admitted high school level.

2.4.2 Validity

As per Imbens and Lemieux [2008], certain conditions need to be met to guarantee the validity of an RDD. Figure 2.3 shows the density of the centered admission score for the pooled sample for 2005-2009. There is no evidence that the density is discontinuous around the cutoff point, which indicates that manipulating the running variable is unlikely. A formal statistical test supports the visual evidence (T=1.3).

As further support for the validity of the design, Table 2.2 presents results from estimating Equation 2.1 over covariates that are expected to be continuous around the centered admission

Figure 2.3: Density test



cutoffs. For the pooled sample of years 2005-200, being admitted to an elite high school has no statistically significant effect on family income, gender, parents' education, or students' GPA in middle school.

Table 2.2: Covariates

	(1) Family income	(2) Male	(3) Father's schooling	(4) Mother's schooling	(5) GPA
admitted	-0.043	0.002	-0.026	0.039	-0.021
	(0.057)	(0.012)	(0.048)	(0.047)	(0.015)
Observations	51561	55742	40636	36980	55742

* significant at 10%; ** significant at 5%; *** significant at 1%

NOTES: Standard errors are in parentheses.

2.5 Results

2.5.1 Effect on test scores

To study if the effect of being marginally admitted to an elite high school on test scores changes over time, I estimate Equation 2.1 separately for each year between 2005 and 2009. Table 2.3 presents coefficients for mathematics performance. A clear pattern emerges: the effect goes from being positive and significant in 2005 but steadily decreases, becoming not significant in 2009. As previously found by Dustan et al. [2017][2], if I only focus on 2005 and 2006, the coefficients for these two years are positive and significant. However, this is not the case in later years.

[2]Appendix B.2 shows a replication of their results.

Table 2.3: Effects on math by year

	(1)	(2)	(3)	(4)	(5)
	2005	2006	2007	2008	2009
admitted	0.203***	0.177***	0.078	-0.048	-0.070
	(0.039)	(0.043)	(0.048)	(0.044)	(0.043)
Mean (C)	-0.219	-0.165	-0.119	-0.127	-0.113
Obs	4909	4471	4958	4561	6336

* significant at 10%; ** significant at 5%; *** significant at 1%

NOTES: Standard errors clustered at the admitted high school level are in parentheses. Mean (C) is the outcome average for the rejected students.

Table 2.4 shows that the coefficients for Spanish are not statistically significant for the years 2005 and 2006, the same as in Dustan et al. [2017]. However, from 2008 to 2009, the coefficients became negative, significant, and larger each year. The results on mathematics and Spanish performance indicate that the effect of being marginally admitted to an elite high school on academic performance has been decreasing over time.

Table 2.4: Effects on Spanish by year

	(1)	(2)	(3)	(4)	(5)
	2005	2006	2007	2008	2009
admitted	0.004	0.018	0.005	-0.124***	-0.135***
	(0.050)	(0.047)	(0.042)	(0.042)	(0.045)
Mean (C)	-0.200	-0.193	-0.159	-0.177	-0.168
Obs	4313	5696	4628	4908	4359

* significant at 10%; ** significant at 5%; *** significant at 1%

NOTES: Standard errors clustered at the admitted high school level are in parentheses. Mean (C) is the outcome average for the rejected students.

Overall, the results show the time specificity of the estimated effects on academic performance. The next step is to understand why the effect has changed over time and, in particular, why the positive effect on academic performance decreased so dramatically.

2.5.2 Effect on drop-out

Suppose marginal admission has a statistically significant effect on the probability of dropping out. In that case, estimates of the effect on other outcomes only observed for those who do not drop out could be biased. Because this is the case for test scores at the end of high school, I estimate the year-specific effect of marginal admission on the probability of dropping out following Equation 2.1.

The results in Table 2.5 indicate that the estimated coefficients are positive, between 9 to 16 percentage points, and always statistically significant. The more demanding curriculum could explain this in elite schools. Nevertheless, having statistically significant coefficients raises bias concerns in the previously shown year-specific effect on test scores.

Table 2.5: Effects on drop-out by year

	(1)	(2)	(3)	(4)	(5)
	2005	2006	2007	2008	2009
admitted	0.125***	0.104***	0.089***	0.101***	0.157***
	(0.024)	(0.022)	(0.021)	(0.023)	(0.023)
Mean (C)	0.489	0.493	0.499	0.505	0.486
Obs	7647	8656	10550	9046	10220

* significant at 10%; ** significant at 5%; *** significant at 1%

NOTES: Standard errors clustered at the admitted high school level are in parentheses. Mean (C) is the outcome average for the rejected students.

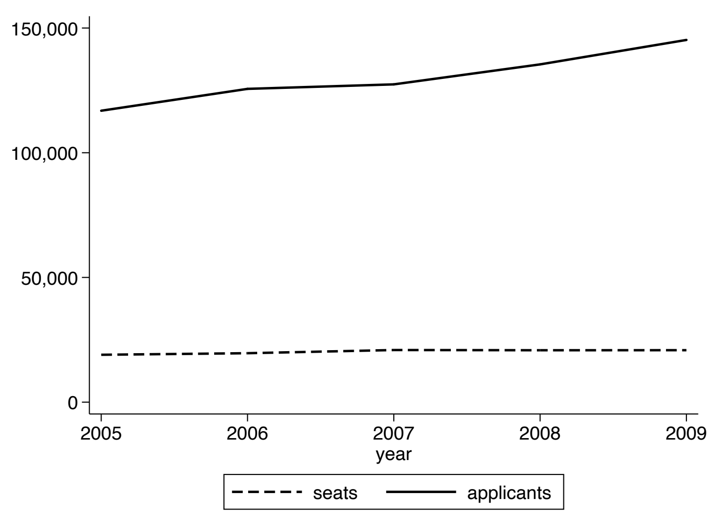
Previous literature has estimated upper and lower bounds of the effect of interest to deal with potential bias, as in Lee [2009]. However, in my case, Table 5 also shows no clear time trend on the effect on the probability of dropping out, alleviating concerns of differential selection over time. Under non-differential selection over time, current methodologies to correct for bias would shift the estimates on test scores in the same direction without affecting the time trend on the effect on test scores.

2.6 Mechanisms

2.6.1 Did students change?

Since I am using cross-sectional data for different years, the composition of students within the bandwidth for the RDD could have changed over time. In other words, the marginally accepted and rejected students could belong to different percentiles of the initial skill distribution each year. This could be the case if elite high schools' admission requirements became more or less stringent over time.

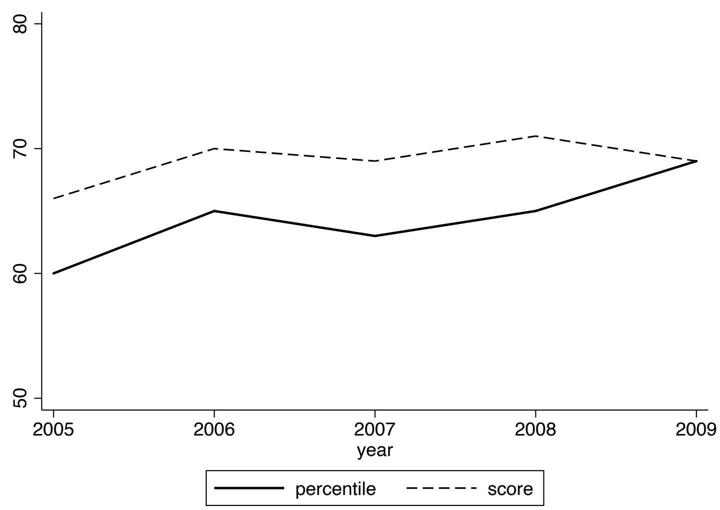
Figure 2.4: IPN seats and applicants



As a first step, I study changes over time in the supply and demand for elite high school seats by comparing the evolution of the number of applicants and available seats for 2005-2009. Figure 2.4 shows that while the number of available seats remained almost constant, the number of elite high school applicants consistently increased over time.

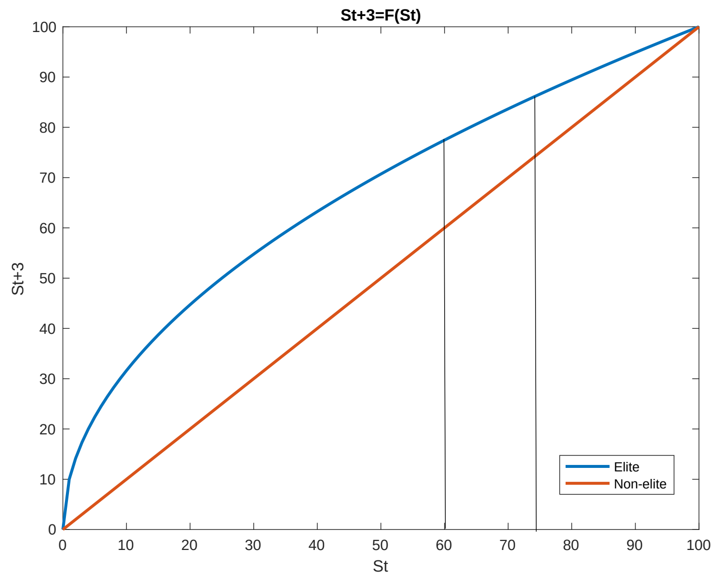
The higher demand for elite high schools made it more difficult to gain admission to them by increasing the cutoff scores for admission. More importantly, it also increased the percentile in the score distribution a student had to be during the year in which she applied to gain admission. These two patterns are illustrated in Figure 2.5.

Figure 2.5: Minimum IPN scores and percentiles



Consequently, the composition of students who were marginally accepted and rejected changed over time. More specifically, the academic performance of the marginally accepted and rejected students before admission improved over time. This fact could explain the negative time trend in academic effects if elite schools have an education production function with decreasing returns to initial skills, while non-elite schools experience constant returns. This is illustrated in Figure 2.6, where S_t indicates skill level at the beginning of high school and S_{t+3} skills at the end of high school. In this example, the treatment effect of marginal admission is represented by the difference between the blue and the red lines. For certain initial skill levels, the treatment effect is decreasing in initial skills. For instance, in the figure below, this is the case when the initial skills move from 60 to 75.

Figure 2.6: Decreasing effects of marginal admission



2.6.2 Did high schools change?

Another posibility is that the quality of elite and non-elite schools changed over time. To explore this mechanism, I estimate separate regressions for elite schools (IPN) and a set of schools where most of the marginally rejected students from elite schools attend (DGETI). In Equations 2.2 and 2.3, I regress end of high school math test scores (M_{ijt}) on year of admission (t), controlling for the variation in the composition of students admitted at a given school. School fixed effects are included through α_j and θ_j .

$$M_{ijt}^{IPN} = \alpha_j + \underbrace{t\lambda_j^{IPN}}_{\text{time trend}} + \underbrace{X'_{jt}\beta + \delta S_{ijt}}_{\text{composition}} + \epsilon_{ijt} \tag{2.2}$$

$$M_{ijt}^{DGETI} = \theta_j + \underbrace{t\lambda_j^{DGETI}}_{\text{time trend}} + \underbrace{X'_{jt}\phi + \tau S_{ijt}}_{\text{composition}} + \eta_{ijt} \tag{2.3}$$

- i individual, j high school, $t \in \{2008, \ldots, 2013\}$ year
- M_{ijt} end of high school math score
- X_{jt}: average of admission scores, standard deviation of admission scores
- S_{ijt}: admission score

My coefficients of interest are λ_j^{IPN} and λ_j^{DGETI}, as they capture the school specific time trend effect on the mathematics test score. Figure 2.7 plots the λ_j^{IPN} coefficients with their respective confidence intervals. The main result is that most of the time trends are negative and statistically significant, providing evidence that elite high schools became worse over time.

Figure 2.7: Time trends, elite

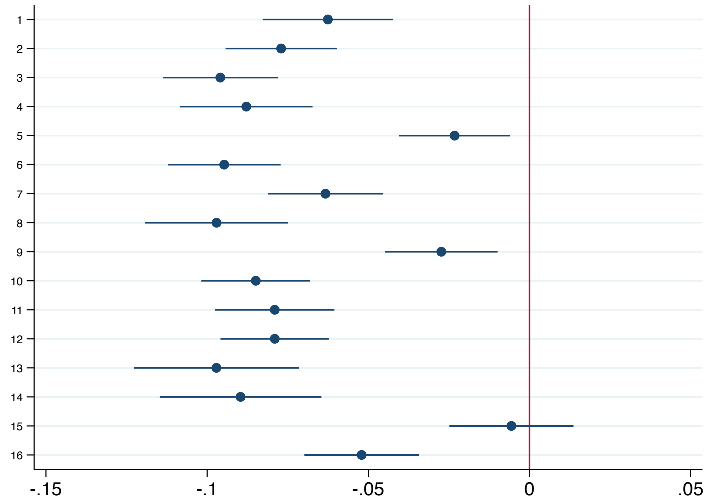
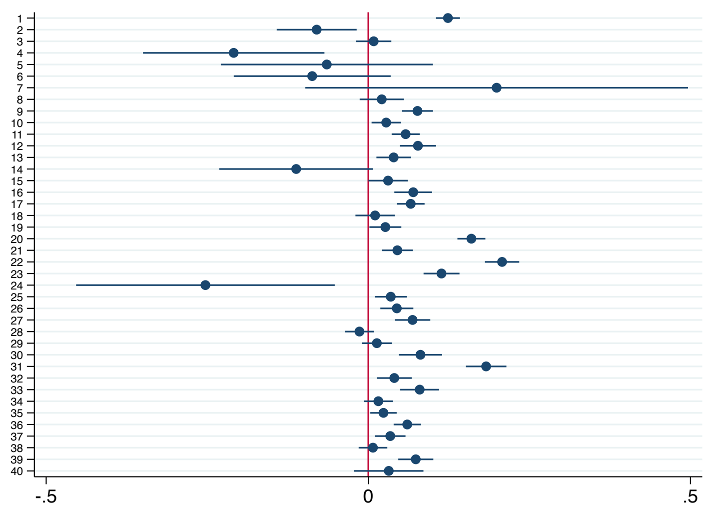


Figure 2.8 shows that of the DGETI high schools have positive time trends that are statistically significant while very few have negative trends. Overall, it seems that DGETI high schools improved over time.

Figure 2.8: Time trends, DGETI



Overall, these results indicate that in addition to a composition change in the students admitted and rejected from elite high schools, there seem to be other school level factors changing over time that could explain why the elite high schools' impact on test scores are consistently decreasing.

2.7 Conclusions

The results presented in this paper indicate that the effect of being marginally admitted to an elite high school is not constant across time and is related to aggregate time changes in the educational context. In the case of Mexico City, I find that over five years (2005-2009), the effect of elite schools on academic test scores monotonically decreased. Two channels reflecting aggregate changes in the education context help explain this result.

First, increased demand pressure made it more difficult to gain admission to an elite high school, forcing the marginally accepted and rejected students to have a higher initial level of skills. This, together with elite high schools having an educational production technology with diminishing returns to initial skills relative to comparison schools, can partially explain a decreasing effect of marginal admission. Second, elite and non-elite high schools also changed during the period under study. Elite schools showed a negative trend in their effect on the end of high school test scores. In contrast, the high schools where most of the marginally rejected students attended experienced quality improvements leading to higher positive effects on test scores.

My results highlight that it is important to understand how the context influences the effect of elite schools obtained for a given period in an evolving educational context. In this sense, caution when generalizing results from within-country analyses to other countries should also be extended to generalizing within-period studies to different periods.

Chapter 3

Perceived Ability and School Choices

3.1 Introduction

This paper studies the role of youth's subjective expectations about their own ability in shaping school choices in secondary education and how these choices affect subsequent schooling trajectories. We design and implement a field experiment that provides students with individualized feedback on their academic skills during the transition from middle to high school.

The context of the study is the centralized assignment mechanism that allocates students across high school programs in Mexico City according to applicants' school rankings and performance on an achievement test. Since students submit their school choices *before* taking the admission exam, they rely on perceptions about their own academic skills when making high-stakes decisions about future academic trajectories. We administer a mock version of the admission test, communicate individual scores to a randomly chosen subset of applicants, and elicit probabilistic statements about performance beliefs in the admission test using bean counts. In this setting, the score in the mock exam provides students with a signal about their own academic potential that is easy to interpret and contains relevant information on individual-specific returns across schooling careers.

Results from the experiment show that providing feedback on individual performance generates a steeper gradient of the relationship between the demand for academically-oriented schools and the score in the mock test, with better performing (lower performing) students increasing (decreasing) the share of academic options in their application lists. This choice response alters the realized skill composition across high-school tracks in our sample. Unique follow-up administrative data further enable us to track the medium-run consequences of the change in the sorting patterns by ability triggered by the intervention. Three years after school assignment, the probability of graduating from high school on time is on average 4 percentage points higher among students who received performance feedback.

3.2 Data

We use data from the 2014 cohort of participants in Mexico City's centralized education market. Over 238,000 students were placed in 628 public high schools. The Mexican system offers three educational tracks at the upper secondary level: General, Technical, and Vocational Education. Each school within the assignment system offers a unique track. The general track is academically oriented and includes traditional schools more focused on preparing students for tertiary education. Technical schools cover most of the curriculum of general education programs, but they also provide additional courses allowing students to become technicians upon completion of high school. The vocational track exclusively trains students to become professional technicians. A set of 16 technical schools within the assignment system are affiliated with a higher education institution (the National Polytechnic Institute, IPN by its Spanish acronym). These are highly selective options and graduating cohorts usually enroll in tertiary education programs sponsored by the IPN. In what follows, we group general track and IPN-sponsored schools into an "academic" track while all remaining technical and vocational schools are assigned to a "non-academic" track.

Admission records from the 2014 assignment process allow us to observe school preference rankings, admission exam scores, cumulative GPA in middle school, and placement outcomes. We link these records to data from the registration form, which includes additional socio-demographic variables such as gender, age, household assets, parental education and occupation, personality traits, and study habits, among others. We also collected and harmonized additional administrative records from each of the nine high-school institutions that cater to the centralized assignment system for the academic years 2014-15 and 2016-17 – i.e., the first and last statutory year of high school for the students who participate in the 2014 round of the school assignment mechanism. These data allow us to measure enrollment and graduation on time from the upper secondary level for the students in our sample.

We complement the administrative data with individual records from the application of a mock version of the admission exam. The mock exam was designed by the same institution that prepares the official admission exam in order to mirror the latter in terms of structure, content, level of difficulty, and duration (three hours). The test is comprised of 128 multiple-choice questions worth one point each, without negative marking. To reduce preparation biases due to unexpected testing while minimizing absenteeism, we informed students about the application of the mock exam a few days in advance but did not tell them the exact date of the event. In order to guarantee that the mock test was taken seriously, we also informed parents and school principals about the benefits of additional practice for the admission exam. We also made sure that the school principal sent the person in charge of the academic discipline and/or a teacher to proctor the exam along with the survey enumerators. Without negative marking, the expected value of guessing is always higher than leaving a question blank, which implies that students have no incentive to skip a question. Indeed, the average number of skipped questions in our mock exam was only 1.4 out of 128, and more than 80 percent of the students did not leave any question unanswered.

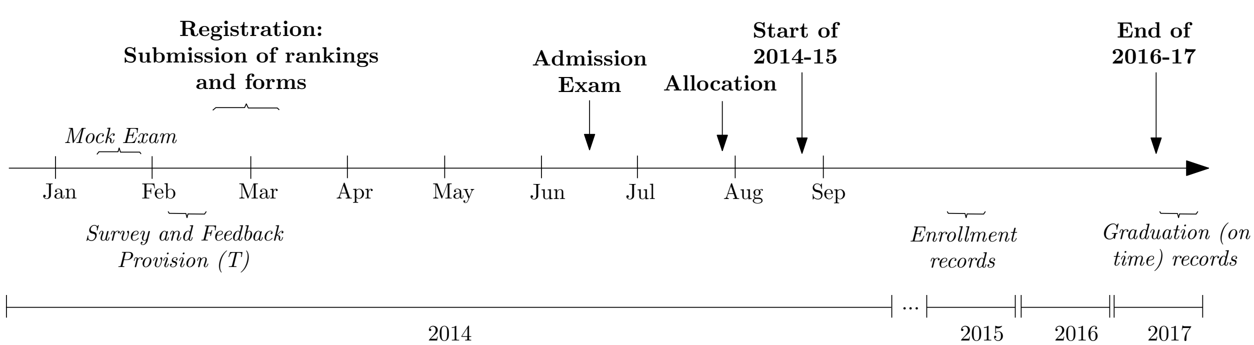
We argue that the score in the mock exam was easy to interpret for the students in our sample while providing additional and relevant information about their academic skills. The linear correlation in our sample between performance in the mock exam and the actual exam is 0.82. Moreover, this relationship does nor vary along the exam score distribution. Controlling for middle school GPA, the mock exam score also predicts success in high school: a one SD increase in the mock exam score is associated with a 2.6 percentage-point increase (std.err.=0.030) in the probability of graduating from high school on time.

We collect rich survey data with detailed information on the subjective distribution of beliefs about performance in the admission exam. In order to help students understand probabilistic concepts, the survey relied on visual aids [Delavande and Kohler, 2015]. We explicitly linked the number of beans placed in a cup to a probability measure, where zero beans means that the student assigns zero probability to a given event and 20 beans means that the student believes the event will occur with certainty. Students were provided with a card divided into six discrete intervals of the score. Surveyors then elicited students' expected performance in the test by asking them to allocate the 20 beans across the intervals so as to represent the chances of scoring in each bin. Assuming a uniform distribution within each interval of the score, mean beliefs are constructed as the summation over intervals of the product of the mid-point of the bin and the probability assigned by the student to that bin.

3.3 The field experiment

Figure 3.1 depicts the timing of the activities related to the intervention (in italics) as well as the important dates of the assignment process and of the school calendar year (in bold). Students took the mock exam early during the second half of the 2013-14 academic year. The survey was administered one or two weeks after the application of the mock test, right before the submission of the school rankings. Both the elicitation of beliefs about exam performance and the delivery of individual feedback on test performance occurred during the survey, in a setting secluded from other students or school staff in order to avoid the role of peer effects and/or social image concerns when reporting [Ewers and Zimmermann, 2015; Burks et al., 2013]. After a first elicitation of beliefs, surveyors showed each student a personalized graph with two pre-printed bars: the average score in the universe of applicants during the 2013 edition of the school assignment mechanism and the average mock exam score in her class. Both pre-printed bars served the purpose of providing the student with additional elements to better frame her own score, which is the main object of interest of the analysis. Surveyors plotted a third bar corresponding to the student's score in the mock exam and then elicit again the subjective distributions of performance in the exam.

Figure 3.1: Timeline of Events



To select the experimental sample, we focus on middle schools with a considerable mass of applicants in the 2012 placement round (more than 30) and that are located in neighborhoods with high or very high poverty levels (according to the National Population Council in 2010). The latter criterion responds to previous evidence that shows that less privileged students tend to be relatively more misinformed when making educational choices [Hastings and Weinstein, 2008; Avery and Hoxby, 2012]. In the year 2012, 44 percent of the applicants enrolled in schools from more affluent neighborhoods took preparatory courses before submitting their school rankings, but this figure drops to 12 percent among applicants from schools in high poverty areas. Among the applicants in our sample, 16 percent report previous exposure to a mock test of the admission exam with performance feedback, and this share is balanced across treatment arms. Despite our focus on less advantaged students our sample of ninth-grade students is largely comparable to the general population of applicants in terms of initial credentials such as GPA in middle school or admission exam score.

Schools that comply with the criteria imposed are grouped into four geographic regions and terciles of school average performance amongst ninth graders in a national standardized test aimed at measuring academic achievement (ENLACE, 2012). Treatment assignment is randomized within strata at the school level. As a result, 44 schools are assigned to a treatment group in which we administer the mock exam and provide face-to-face feedback on performance while 46 schools are assigned to the control group in which we only administer the mock exam. Beliefs are measured twice for students in the treatment group, both before and after the provision of feedback, and once for students in the control group. Within each school, we randomly pick one ninth grade classroom to participate in the experiment.

The mock exam was administered to 2,978 students in 90 schools, and a subset of 2,732 were also present in the follow-up survey. Since the delivery of feedback about test performance took place during the survey, it cannot induce differential attrition patterns. The match rate between the endline and the administrative records is 88 percent (2,828 students) and it is not differential by treatment arm. The discrepancy between the survey and the administrative data is driven by students who do not to participate in the assignment process. We focus on the 2,493 applicants who are assigned either in the first round of the matching algorithm or during the scramble round.[1] Appendix C.1 provides basic descriptive statistics and a balancing test of the randomization for the pre-determined covariates used in the empirical analysis. Very few and erratic significant differences are detected across treatment arms.

3.4 Results

Providing information about individual performance in the mock exam potentially allows students to revise their beliefs and make better informed choices. To measure the effect of the feedback delivery on beliefs, we define the perception gap as the absolute value of the difference between mean beliefs and actual performance in the mock exam. Table 5 shows that the delivery of the individual scores in the mock test shrinks the perception gap by 6.6 points on average.[2] The magnitude of this effect is quite large as it is equivalent to a third of the mean absolute gap in the control group. Moreover, the coefficient for the interaction term indicates that the correction of the bias induced by the treatment is decreasing in students' ability index.

[1] Exposure to the performance feedback therein does not systematically affect the fraction of applicants assigned in the first or second round of the assignment process.

[2] For more details on the updating process triggered by the treatment, see Bobba and Frisancho [2020].

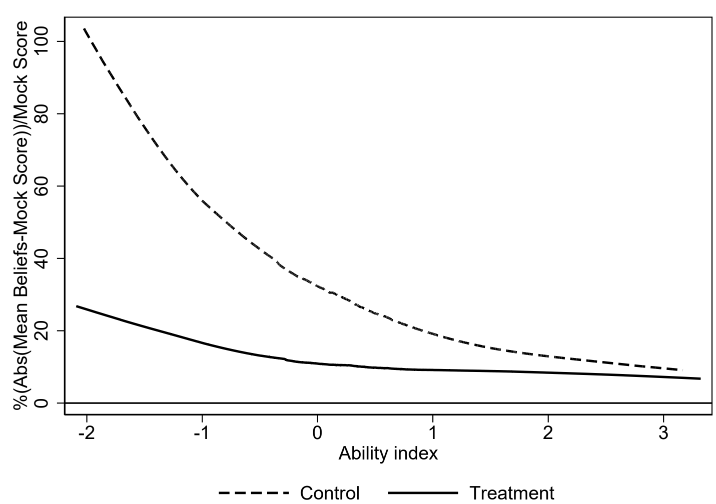
Table 3.1: Perception Gap about Performance

	Abs(Mean Beliefs-Mock Score)
	(1)
Treatment	-6.587***
	(0.636)
Ability index	-5.680***
	(0.476)
Treatment X Ability index	1.878***
	(0.507)
Mean Control	18.759
Number of Observations	2178
R-squared	.2334
Number of Clusters	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Standard errors clustered at the middle school level and they are reported in parenthesis. The ability index is a standardized weighted index of middle school GPA, mock exam score, and exam score, constructed following Anderson [2008].

Figure 3.2 presents non-parametric estimates of the relationship between the perception gap and the score in the mock exam, estimated separately for students in the treatment group and in the control group. In particular, the perception gap is measured as a percentage of the mock exam score. The evidence displayed shows that the update on mean beliefs in response to performance feedback occurs across the entire distribution of the mock test, with corresponding larger gap reductions among lower performing students as they start off with larger biases.

Figure 3.2: Gap between Expected and Mock Exam Score Along the Ability Distribution and by Treatment Arm



NOTE: The ability index is a standardized weighted index of middle school GPA, mock exam score, and exam score, constructed following Anderson [2008].

In our setting, high schools differ in terms of the curricular track, or modality, that they offer. Academically oriented schools tend to provide students with general skills and adequate training to pursue a college education. Non-academic schools, either technical or vocational, focus more on fostering specific skills that are geared to provide access to the labor market after completion of secondary schooling. Schools also vary greatly in terms of their selectivity and, in turn, the level of their academic requirements for graduation. The assignment mechanism described generates sorting across schools based on individual performance in the admission exam. In the context of the assignment mechanism under study, the admission cut-off score is a good proxy for peers' quality and the associated level and pace of instruction – e.g., median scores are almost perfectly correlated with cut-off scores. Since equilibrium cut-off scores in 2013 are observable by the applicants at the time they submit their school rankings for the 2014 placement round, we rely on them to measure school quality and construct an indicator based on whether the cut-off for any given school falls above or below the median across all schools (irrespective of the curricular track).

School placement under the assignment mechanism exclusively depends on two student-level observable factors: individual school rankings and the score in the admission exam. The intervention does not systematically alter exam scores (see Appendix C.2). Therefore, any treatment-control differences in students' final assignments across curricular tracks are mainly driven by the observed differential changes in their demand for academic programs.

Table 3.2 presents the treatment impacts of feedback provision on preferences and its subsequent effect on placement outcomes. Column 1 shows that the provision of performance feedback does not affect the demand for academic programs for students with test scores around the sample average. The positive and significant estimated coefficient on the interaction term between the feedback provision indicator and the ability index implies that a one-standard-deviation increase in students' scholastic success increases the share of academic schools requested by the applicants in the treatment group by 3.3 percentage points. This compositional change in the demand for academic schools significantly alters the assignment patterns realized under the mechanism, as shown in Column 2: on average, the treatment reduces the fraction of students admitted into an academic program by 4.8 percentage points. However, as shown by the coefficient in the interaction term, placement in academic track schools increases along the ability distribution, suggesting that the treatment leads to a better match between students' academic skills and high-school track.

In turn, the results reported in Columns 1 and 2 of Table 3.3 show that the provision of performance feedback in the mock test does not systematically alter preferences for or assignment into selective schools. Neither the average treatment impact nor the heterogeneous effects by ability are significantly different from zero. The lack of an impact on preferences for and placement in selective schools suggest that students are not behaving strategically after receiving the feedback; applicants in the treatment group do not change their ROLs to target schools with equilibrium cutoffs closer to their individual expected performance.

In principle, changes in observed demand for schools could be due to changes in the strategic behavior of students if they define their application lists considering their ex-ante admission probabilities. However, in our case, the matching algorithm is strategy-proof, and the timing of the admission process creates additional incentives for the truthful revelation of preferences. Uncertainty about the admission score at the time of application incentivizes students not to omit selective schools ex-ante. Consistent with the truthful revelation of preferences, Appendix

Table 3.2: Preferences and Placement

	(1) Share Academic	(2) Placed Academic
Treatment	-0.002	-0.048*
	(0.017)	(0.026)
Ability index	0.027**	0.092***
	(0.010)	(0.024)
Treatment X Ability index	0.033**	0.055**
	(0.013)	(0.028)
Mean Control	0.635	0.547
Number of Observations	2493	2493
R-squared	.1516	.1079
Number of Clusters	90	90

* significant at 10%; ** significant at 5%; *** significant at 1%
Standard errors clustered at the school level

C.2 shows that our information treatment does not affect the minimum or maximum level of selectivity of the schools that students include in their application lists.

Table 3.3: Selectivity

	(1) Share Selective	(2) Placed Selective
Treatment	-0.010	-0.024
	(0.016)	(0.022)
Ability index	0.044***	0.198***
	(0.006)	(0.017)
Treatment X Ability index	0.016	0.003
	(0.010)	(0.021)
Mean Control	0.791	0.671
Number of Observations	2493	2493
R-squared	.3902	.3117
Number of Clusters	90	90

* significant at 10%; ** significant at 5%; *** significant at 1%
Standard errors clustered at the school level

All in all, these results suggest that the provision of feedback has real consequences on sorting patterns across high-school tracks that seem to result in a better alignment between individual skills and education careers.

The centralized assignment mechanism seems to deliver school-placement outcomes that are satisfactory for the great majority of the applicants, at least in the short-run. About 80 percent of the students in the control group enroll in the school they were assigned through the placement process. However, among these students, only 52 percent graduate on time from high school – i.e. three years after enrollment in tenth grade. There is some heterogeneity by track, with timely graduation rates in the academic and non-academic tracks at 50 and 54 percent, respectively, which may be partly explained by selection issues across tracks. These figures clearly reflect inadequate academic progress through upper secondary education due to either school dropout or grade retention, which are both strong indicators of mismatch between schools and students.

As shown above, the provision of performance feedback improved the alignment between (measured) academic skills and track choices. The associated changes in school placement may thus result in a better match that can further foster individual performance along the education careers of the students in the treatment group. Estimates reported in column 1 of Table 3.4 show that, on average, there are no discernible differences in the high-school enrollment rates between students in the treatment and control groups. However, column 2 shows that, conditional on enrollment, the probability of graduation on time is almost 4 percentage points higher for students who receive performance feedback when compared to those who did not. The magnitude of this average effect is quite remarkable, as it corresponds to a 7 percent increase in high-school graduation rates when compared to the sample average in the control group.

Table 3.4: Enrollment and Graduation on Time

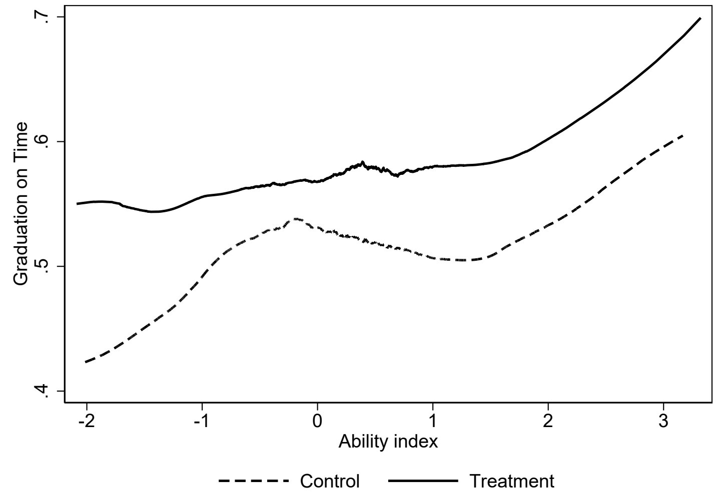
	(1) Enrollment	(2) Graduation on Time
Treatment	-0.005	0.038*
	(0.017)	(0.022)
Mean Control	0.813	0.515
Number of Observations	2493	2024
R-squared	.0656	.1997
Number of Clusters	439	377

* significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors clustered at the school level

The sustained impacts of the intervention on high school outcomes may well vary according to the students' ability level. Figure 3.3 displays non-parametric estimates of the relationship between the rates of graduation on time and the ability index by treatment arm. The plot clearly shows that the effects of performance feedback are present along the ability distribution, with larger effects around both tails of the score distribution. This pattern suggests that the treatment benefits both low and high ability students, without pervasive effects on pre-existing gaps. If anything, the slope reduction in the treatment group relative to the control, indicates that the feedback narrows initial differences in graduation rates by ability level.

Figure 3.3: Graduation on Time Along the Ability Distribution and by Treatment Arm



Note: The ability index is a standardized weighted index of middle school GPA, mock exam score, and exam score, constructed following Anderson [2008].

3.5 Conclusion

Individuals' lack of adequate and timely information about their own academic potential partly explains unfit educational choices that may eventually lead to mismatch and dropout later on. This paper represents one of the first attempts to understand the channels through which the provision of relevant and personalized information about students' own academic ability alter school choices in secondary education and subsequent academic trajectories. We do this in the context of a large- scale centralized school assignment mechanism in Mexico City means of a research design that provides randomized students with information about their performance in a standardized achievement test.

Our empirical findings show that students face important knowledge gaps related to their own academic potential and skills. Providing individualized feedback on academic performance substantially shifts the location of the individual belief distributions toward realized performance in the mock test. The treatment-induced changes in beliefs have real consequences on the sorting patterns across high-school tracks that seem to result in a better alignment between individual skills and education careers. Follow-up administrative data confirm that the information intervention effectively improves student outcomes at the end of high school, raising the probability of graduation on time by 4 percentage points.

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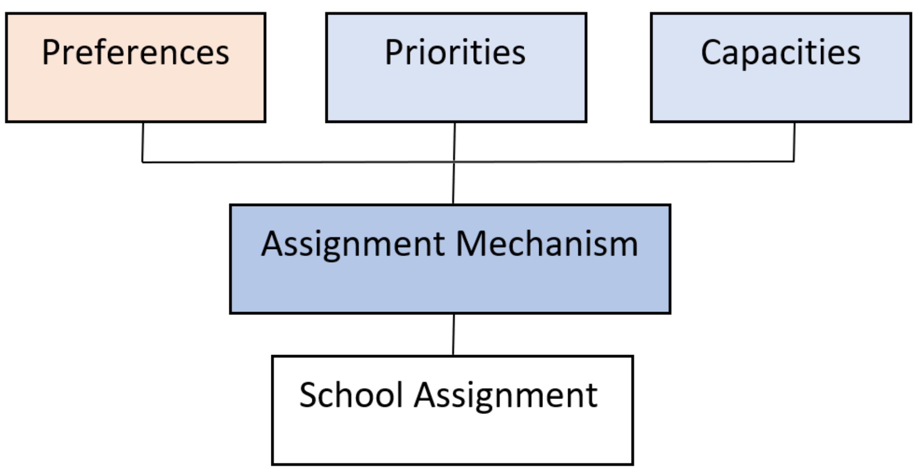
Appendix A

Chapter 1 Appendix

A.1 Centralized education markets

Figure A.1 shows the different components of a centralized education market. Examples of markets that follow this structure are: Boston, Cambridge, Chicago, Denver, Miami, New Orleans, NYC, San Francisco, Washington DC, Mexico City, Romania, Ghana, and Kenya.

Figure A.1: Structure of a centralized education market



A.2 The students

Table A.1 shows some descriptive statistics of the students that participate in the matching algorithm. We divide the participants into three groups, the ones assigned to elite schools, non-elite schools, and the unassigned.

Table A.1: Students' characteristics by assignment group

	All	Elite	Non-Elite	Unassigned
Exam Score	65.24	90.16	60.27	51.20
	(-19.21)	(-10.87)	(-14.88)	(-12.80)
GPA	8.03	8.56	7.88	7.89
	(-0.84)	(-0.81)	(-0.81)	(-0.73)
Female	0.51	0.45	0.51	0.61
Age	15.82	15.56	15.90	15.88
	(-1.60)	(-1.23)	(-1.72)	(-1.55)
Length of ROL	9.32	9.62	9.53	8.03
	(-3.75)	(-3.92)	(-3.71)	(-3.41)
Position assigned	2.81	1.94	3.79	-
	(-2.96)	(-1.72)	(-3.11)	-
	256,335	54,654	162,063	39,618

A.3 The admission exam

Table A.2: Exam sections

	Questions
Math	12
Physics	12
Chemistry	12
Biology	12
Spanish	12
History	12
Geography	12
Civics and Ethics	12
Verbal ability	16
Math ability	16
Total	128

The admission exam is a multiple-choice exam with 128 questions and five choices per question. Each correct answer is worth 1 point, and there are no negative points for wrong answers. Table A.2 shows the different sections of the admission exam. The total score is calculated by adding up all the correct answers. Students must obtain a score no lower than 31 points in the admission exam to participate in the assignment process.

A.4 SPDA mechanism

For the SPDA mechanism, schools can have different priorities over the students, and each student defines her ROL. The matching algorithm is as follows:

- Step 1: Schools receive applications from students who ranked them first in their ROL. Schools that received fewer applications than their capacity hold on to these applications. Each school j that received more applications than its capacity q_j temporarily holds on to the q_j applicants with the highest priority and rejects all others.
- Step (k+1): For any $k \geq 1$, students who received a rejection notification at step k send an application to the school ranked next on their ROL. Schools then consider their total pool of applications: those just received and those held on at step k. Schools that have fewer applications than their capacity hold on to these applications. Each school j with excess applications temporarily holds on to the q_j applicants with the highest priority and rejects all others.
- Stop: The algorithm stops after all students who received rejections have exhausted their list of acceptable schools. Schools formally admit applicants they hold on to at this stage.

When all the schools have the same priorities, the algorithm is equivalent to the Serial Dictatorship.

A.5 Serial Dictatorship mechanism

All schools have the same priorities, and each student defines her ROL. Then, the matching algorithm is as follows:

- Step 1: The first ranked student is assigned to the first school on her ROL.
- Step (k+1): For any $k \geq 1$, once the k^{th} student in the priority ranking has been assigned, the student ranked $(k+1)^{th}$ is assigned to the highest-ranked element of her ROL that still has a vacancy. If all of the schools in her ROL are full at that point, she is left unassigned, and the algorithm proceeds to the next student.
- Stop: The algorithm stops after all students have been processed.

This algorithm is equivalent to an SPDA algorithm in which all schools have the same priorities.

A.6 Descriptive evidence

Table A.3 shows the results of estimating a linear probability model (LPM) of graduation for elite and non-elite schools. Regressors include a dummy variable that indicates if a student has an above-median GPA and the admission exam score. We also include controls for gender, age, and student preferences. For students at non-elite schools, we restrict the sample to students with a GPA of at least 7/10 and an admission score of at least 69/128. The reason for the sample restriction is that all students at elite schools have a GPA of at least 7/10 and the lowest admission score of a student admitted to an elite school is 69/128.

Table A.3: Graduation

	(1) Elite	(2) Non-elite
Above-median GPA	0.277***	0.200***
	(0.005)	(0.005)
Admission exam	0.069***	0.065***
	(0.003)	(0.006)
Mean graduation	0.646	0.589
Obs	54,652	40,502

Standard errors in parenthesis

A.7 Predetermined covariates

Table A.4 shows the point estimates and standard errors of estimating the effect of marginal admission to an elite school on predetermined covariates using a local linear regression with a triangular kernel. The point estimates are close to zero and not statistically significant.

Table A.4: Predetermined covariates

	(1) Female	(2) Poor	(3) GPA
RD Estimate	0.011	-0.009	0.022
	(0.010)	(0.011)	(0.016)
N	49,784	37,664	43,238

Standard errors in parenthesis

A.8 Estimates

Table A.5 shows the point estimates and standard errors of estimating the effect of marginal admission to an elite school on graduation for different groups of students. For the estimation we use a local linear regression with a triangular kernel.

Table A.5: Graduation

	(1) All	(2) Low GPA	(3) High GPA	(4) Males	(5) Females
RD Estimate	-0.003	-0.081***	0.074***	-0.060***	0.064***
	(0.010)	(0.015)	(0.015)	(0.015)	(0.016)
N	49,784	19,489	18,797	21,495	18,371

Standard errors in parenthesis

A.9 Elite schools with high and low cut-offs

For the RDD analysis we pool *k* groups of students that share a common elite school cut-off c_k. In this appendix we show that the effects on graduation do not depend on elite schools having high or low cut-offs. Instead of pooling together our *k* groups, we separate this groups into low and high elite school cut-offs and repeat the analysis for each sub-sample.

Figure A.2: Elite schools with low cut-off

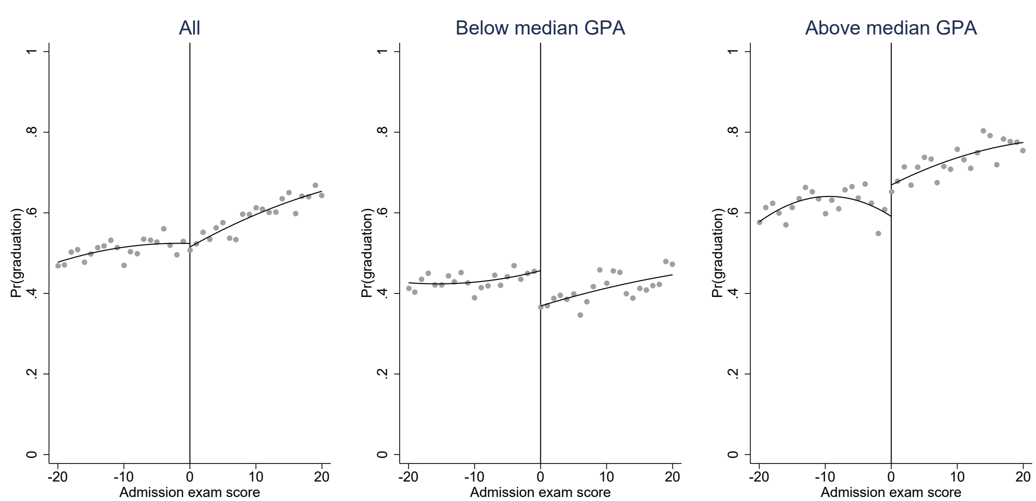


Figure A.3: Elite schools with high cut-off

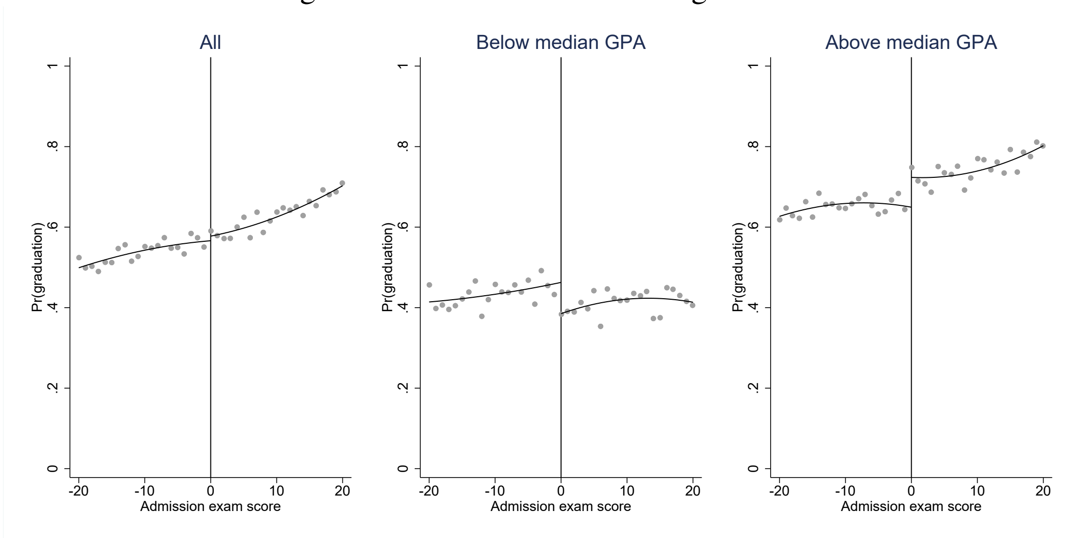


Figure A.2 and Figure A.3 show that our main results do not change when we only consider elite schools with high or low cut-offs. Marginal admission to an elite school does not affect graduation. But this effect depends on students middle school GPA, for students with below median GPA, the effect is negative, while for students with above median GPA the effect is positive.

A.10 All schools use GPA

Table A.6 shows the results of a counterfactual where the central planner puts equal weight on the admission exam score and GPA in the priority index of elite and non-elite schools.

Table A.6: Robustness, all schools add GPA

	Initial	Counterfactual	Diff
Elite			
Female	45.12%	53.32%	8.21
Graduation	64.58%	70.71%	6.14
Non-Elite			
Female	50.88%	48.51%	-2.37
Graduation	45.88%	44.71%	-1.17

A.11 Alternative priority structure

Table A.7 shows the results of a counterfactual where the central planner puts equal weight on the admission exam score and students' within middle school percentile ranking by GPA in the priority index of elite schools.

Table A.7: Robustness, middle school percentile ranking by GPA

	Initial	Counterfactual	Diff
Elite			
Female	45.12%	53.27%	8.16
Graduation	64.58%	70.02%	5.44
Non-Elite			
Female	50.88%	48.56%	-2.32
Graduation	45.88%	44.82%	-1.06

A.12 Validation

For our model validation exercise, we repeat our RDD analysis using as an outcome our model-predicted graduation probabilities ($\hat{Y}$). Figure A.4 shows that there is no effect of marginal admission to an elite school on $\hat{Y}$.

Figure A.4: Model validation

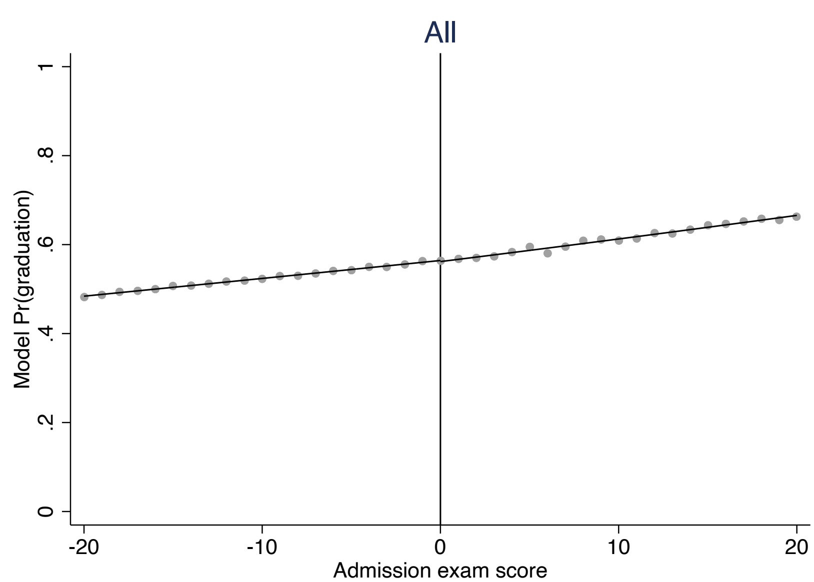


Figure A.5 and Figure A.6 show that our model also reproduces the heterogeneous effects by GPA and gender when we use $\hat{Y}$ as our outcome.

Figure A.5: Model validation, GPA

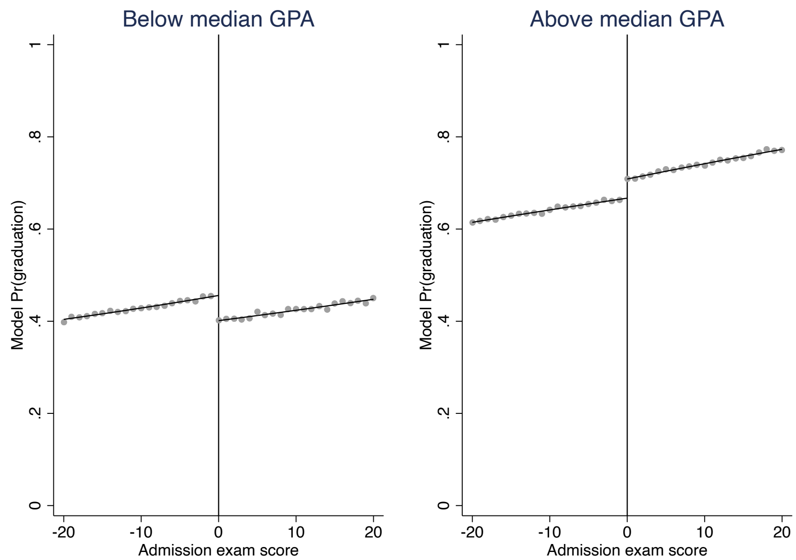
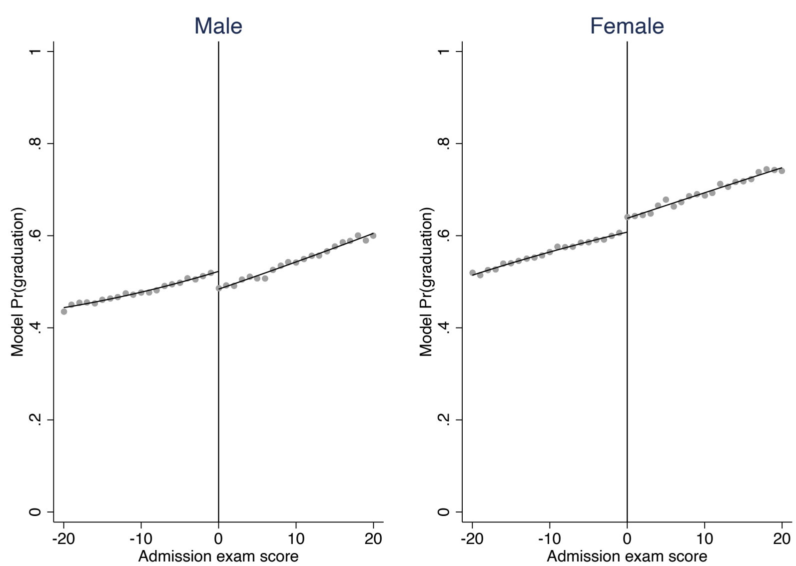


Figure A.6: Model validation, gender



Lastly, Table A.8 shows the point estimates and standard errors of our validation exercise. All the effects go in the same direction as the results using the data. However, the point estimates are smaller in magnitude.

Table A.8: Model Pr(graduation)

	(1) All	(2) Low GPA	(3) High GPA	(4) Males	(5) Females
RD Estimate	-0.003	-0.056***	0.042***	-0.038***	0.029***
	(0.003)	(0.003)	(0.002)	(0.005)	(0.004)
N	49,784	17,826	28,401	21,495	25,077

Standard errors in parenthesis

Appendix B

Chapter 2 Appendix

B.1 Sample Characteristics

Table B.1: Samples 2005-2009

	All	Sample
2005		
Male	0.50	0.62
Father has high school or more	0.33	0.34
Family income is more than 5k	0.29	0.28
Admission exam score	62.16	73.15
Observations	287886	17906
2006		
Male	0.50	0.63
Father has high school or more	0.33	0.34
Family income is more than 5k	0.31	0.32
Admission exam score	63.78	74.46
Observations	298291	19250
2007		
Male	0.50	0.61
Father has high school or more	0.34	0.36
Family income is more than 5k	0.34	0.35
Admission exam score	63.56	73.17
Observations	296778	21158
2008		
Male	0.50	0.61
Father has high school or more	0.35	0.37
Family income is more than 5k	0.34	0.35
Admission exam score	64.68	73.76
Observations	303224	21842
2009		
Male	0.51	0.62
Father has high school or more	0.35	0.37
Family income is more than 5k	0.34	0.36
Admission exam score	60.79	70.52
Observations	317603	23318

B.2 Replication

In order to have an initial reference for my estimates, I replicate the results obtained by Dustan et al. [2017] using COMIPEMS data for 2005-2006. Table B.2 presents the results of this exercise.

Table B.2: Effects of elite assigment, 2005-2006

	(1)	(2)	(3)
	dropout	math	spanish
admit	0.094***	0.197***	0.028
	(0.017)	(0.030)	(0.031)
N	17850	11959	11216

NOTES: Standard errors are in parentheses.

The effect of elite assignment on the probability of dropout is identical to the result they obtained, and it is also statistically significant at 99%.

The effect on the mathematics test score it is slightly smaller than their result (their point estimate is 0.246) but is also statistically significant at 99%. This difference comes from the way we merge the COMIPEMS and ENLACE datasets, since for some students that did not have the unique identifier (mostly elite students) they imposed the condition that they finished high school at the same high school were they ended up assigned. I do not impose this condition because I consider it creates selection problems when calculating the ITT effect. Instead, I solve the issue of missing identifiers by creating my own identifier for all the students (based on their names).

Lastly, the effect on the Spanish test score is similar in magnitude and it is also not statistically significant.

Appendix C

Chapter 3 Appendix

C.1 Balance

Table C.1: Summary Statistics and Randomization Check

	Control	Treated	T-C
	(1)	(2)	(3)
Mock exam score	60.540	62.366	1.496
	(15.416)	(16.290)	[1.065]
Exam score	65.541	65.248	–0.169
	(19.516)	(19.284)	[1.248]
GPA (middle school)	8.116	8.122	–0.013
	(0.846)	(0.846)	[0.047]
Does not skip classes	0.971	0.971	–0.001
	(0.169)	(0.169)	[0.010]
Plans to go to college	0.670	0.671	–0.003
	(0.470)	(0.470)	[0.022]
Male	0.444	0.461	0.016
	(0.497)	(0.499)	[0.020]
Does not give up	0.878	0.889	0.015
	(0.327)	(0.315)	[0.014]
Tries his best	0.735	0.722	–0.016
	(0.442)	(0.448)	[0.021]
Finishes what he starts	0.720	0.712	–0.015
	(0.449)	(0.453)	[0.020]
Works hard	0.725	0.739	0.010
	(0.447)	(0.439)	[0.022]
Parental background and supervision	0.032	0.058	0.011
	(0.786)	(0.760)	[0.035]
High SES (asset index)	0.463	0.485	0.019
	(0.499)	(0.500)	[0.025]
Previous mock exam with feedback	0.133	0.166	0.028
	(0.340)	(0.372)	[0.033]
N. Obs.	1290	1203	2493

NOTE: Columns 1 and 2 report means and standard deviations (in parenthesis).

C.2 Other outcomes

Table C.2: Average Treatment Effects on Application Outcomes

	Participates in COMIPEMS (1)	Exam Score (2)	Length of ROL (3)	Max cutoff in ROL (4)	Min cutoff in ROL (5)
Treatment	0.002	-0.314	0.126	1.732	-0.265
	(0.007)	(1.182)	(0.216)	(1.434)	(0.836)
Mean Control	0.881	65.541	9.465	90.491	35.022
Number of Observations	3215	2493	2493	2493	2493
R-squared	.6354	.1744	.03185	.2186	.1775
Number of Clusters	90	90	90	90	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%.

Curriculum Vitae

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Post-Secondary Education and Degrees:	Pennsylvania State University State College, PA 2014 - 2016 M.A.
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