Just Making the Admission Cut-off: The Impact of the Offer of Admission to, and Enrollment in, a More Competitive School on the Graduation Outcomes of Upper-Secondary Applicants in Mexico City

María Elena Ortega Hesles

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Abstract

Many education systems around the world use a centralized admission process to assign students to schools (Pathak & Sethuraman, 2011; Lucas & Mbiti, 2012). However, applicants to oversubscribed schools are not always assigned to their most-preferred school. Thus, one naturally asks whether it makes a difference to applicants' subsequent educational outcomes if they are offered a place in, and if they enroll in, a more competitive school. In Mexico City, each year about 300,000 teenagers apply for a seat at one of the nearly 650 public upper-secondary school options. The centralized admission process in Mexico City, where applicants are assigned based on the interplay between their score on the admission examination and their ranked list of school choices, provides a natural experiment that permits these questions to be addressed in a case study of one school system. In this paper, I capitalize on the natural experiment created at each oversubscribed public upper-secondary school in Mexico City, by the imposition of exogenous admission cut-off scores. Using rich administrative data provided by the school system for the 2005-2009 application cohorts, I estimate that on average applicants who are barely offered admission to a more competitive school option, and therefore lie at the bottom of that school's ability distribution, have on average a 3.1 percentage points lower probability of graduating on-time, and 1.1 percentage points lower probability of graduating from public upper-secondary education relative to applicants who scored just below the admission cut-off. Given the high take-up rates of the offers of admission, I find that the effects of enrollment in a more competitive school are only slightly larger than they are in their analogous reduced-form estimates. In addition, I show that effects differ across the distribution of admission cut-off scores and for applicants with selected socio-demographic characteristics who scored just above the admission threshold. My results are robust to sensitivity analyses.

1. Introduction

In education markets with oversubscribed schools, not all the applicants can be assigned to their most-preferred school. Therefore, important policy questions concern whether it matters to applicants' subsequent educational outcomes which school they are offered admission to, and enroll in. The empirical difficulties in identifying the causal effect of admission to, and enrollment in, a more competitive[1] school lie in the fact that students may self-select schools based on the latter's observed and unobserved characteristics. Nevertheless, using quasi-experimental designs that capitalize on plausibly exogenous variation in school admission among students, some researchers have provided credible estimates of the effect on subsequent educational outcomes of having access to a better quality, or more selective, secondary school.[2]

In this paper, using such a quasi-experimental discontinuity design, I estimated the causal impact on subsequent upper-secondary graduation outcomes, of an offer of admission to, and enrollment in, a more competitive school for an applicant at the margins of being accepted into that school, using data from Mexico City. In particular, I explore the effect on 3-year on-time graduation and graduation within 5 years. These outcomes are relevant because they are closely related to other education outcomes and subsequent labor market outcomes, such as earnings.[3] I am able to use this methodology because, since 1996, the Metropolitan Area of Mexico City has been implementing a centralized merit-based admission mechanism in its upper-secondary public school system. As I explain in more detail later, applicants are offered admission to their preferred school option subject to the school's capacity constraints, with priority given to students with higher scores on a standardized entrance examination.[4] The data available include examination scores, the students' school preferences, and the school assignment resulting from the application of this merit-based admission rule.

My identification strategy exploits arguably exogenous variation in the placement offer, at each oversubscribed school, among applicants of equivalent skills and preferences at the margins of the cut-off for admission. In my research, I adopt a regression-discontinuity design –in combination with instrumental-variables estimation– to estimate the causal impact of both the *offer of admission* to, and *enrollment* in, a more competitive school (i.e. higher cut-off score) for applicants just below, and just above, a school's admission threshold. Thus, I compare the graduation outcomes of students closely on either side of an admission threshold, as a measure of the impact of gaining admission to a school with a higher cut-off score and thus, being immersed

[1] Throughout the paper I use the term "more competitive" to define a school with a higher entrance cut-off score than the comparison. By definition, it is always the case here that a school with a higher cut-off score is more-preferred and has higher-achieving peers than a school with a lower cut-off score, see Figure 1.

[2] Peer ability is often used as a proxy of school quality.

[3] Aguilar and Ortega (2006) estimated that the marginal return of completing upper-secondary education on labor income was about 27% in 2002.

[4] The school-assignment mechanism is a serial dictatorship with applicants ordered by admission score. It gives the incentives to reveal their true preference ranking.

in a school environment with higher-achieving peers (see Figure 1) and often higher expenditure per student.

I found that, on average, applicants who scored just above the admission threshold, and thereby effectively received an offer to attend a more competitive school, had a 3.1 percentage points lower probability of graduating within 3 years (on time) and a 1.1 percentage points lower probability of graduating within 5 years, relative to applicants who scored just below the admission threshold, and did not receive the offer of admission to the more competitive school.[5] These effects are therefore estimates of an intent-to-treat effect local to the population at the cut-off. In addition, I use the offer of admission as an instrument for enrollment in the more competitive school, and estimate local treatment-on-the-treated (LATE) effects that are very similar (i.e. -3.2 and -1.2 percentage points, respectively) to their analogous reduced-form estimates. My results are robust to several sensitivity analyses that I describe. Overall, in this particular setting, it seems that it is better in terms of graduation outcomes to be an average-ranked student in a school with lower- or average-achieving peers, than the lowest-ranked student in a more competitive school with higher-achieving peers. However, I acknowledge that in some selective schools the costs in terms of graduation outcomes for the marginal student should be weighed against the benefits in terms of academic preparation for those who eventually graduate.

My main findings only pertain to the marginal applicant offered admission to a more competitive school. It is possible that the effects are different for specific demographic groups, and at different parts of the cut-off score distribution. Thus, in an extension to the baseline analysis, I show that the negative impact of the offer of admission to a more competitive school for the marginal applicant is larger for males, applicants from low socio-economic status families, and in public lower-secondary schools than for females and those from higher-SES families and those who attended private lower-secondary schools. I also find that the magnitude of the effects I detected differ according to the location of a school's cut-off score in the overall admission cut-off distribution. In particular, the negative causal effects of the offer of admission tend to disappear as one moves to the admissions decisions of the less selective schools. Finally, I show that the magnitude of the estimated effects on graduation outcomes are larger when focusing only on the offer of admission to, and enrollment in, a first-choice school than when considering all the choices.

There are two important caveats to my analysis. First, I cannot generalize my findings to applicants who score far from the admission cut-off score, to not-oversubscribed public schools, or to private schools. Second, given the available data, I cannot effectively disentangle the particular channels that are driving my findings. Peers may not be the only thing that changes with admission to a more competitive school. Applicants who score above the cut-off for a particular

[5] These effects represent a 6% and 2% decrease, respectively, among applicants near the eligibility cut-off. The average probability of graduating on time is 47%, while probability of graduating within 5 years is 57%.

school might be exposed to different schooling environments (i.e. peers, teachers, resources, etc.) than those who scored just below.

I have organized the remainder of this paper as follows. In the next section, I begin with a review of the existing literature on the impact of the offer of admission to, and enrollment in, a better quality –or more selective– school on the students' subsequent educational outcomes. I also describe the student-allocation mechanism implemented in the admissions process in Mexico City. I end this section by posing my specific research questions. In the following section, I then detail my research design, describing my dataset and identification strategy. I next present my results and describe the potential threats to validity of my inferences. I conclude with a discussion of the findings and opportunities for further research.

2. Background and Context

In many contexts, it is not always possible to assign students to their most-preferred school because of capacity constraints. So different student-allocation mechanisms have been devised and implemented to complete the assignment process. The design of the student-assignment mechanism thus becomes fundamental in settings where demand for schools exceeds supply (Abdulkadiroğlu & Sönmez, 2003). Commonly, centralized admission processes allocate students to schools based on their revealed preferences over schools and, in some cases, schools' priority ordering[6] and maximum capacity (Abdulkadiroğlu & Sönmez, 2003; Klijny & Haeringerz, 2007).

School choice has become a topic discussed broadly in education (Abdulkadiroğlu & Sönmez, 2003).[7] The underlying rationale for allowing students to choose their schools is that it is hypothesized that they will benefit from being able to enroll in a school that best matches their interests and abilities. The offer of admission to one's preferred school may then play a role in the student's engagement in academic work, which in turn is associated with the decision to persist, or drop out of, school (Rumberger, 2011). For instance, Hastings et al. (2012) suggest that the opportunity to attend a first-choice school improves intrinsic motivation for students, even before they arrive at their new school, and has subsequent positive effects on test scores.

Researchers have linked differences in school quality and student achievement to ultimately differences in labor market outcomes (Card & Krueger, 1992; Grogger, 1996; Murnane et al., 2000), suggesting that attending a better quality school may have important long-term effects. The empirical difficulties in identifying the causal effect of admission to, and enrollment in, a 'better' school lie in the fact that students may self-select schools based on unobserved characteristics;

[6] Some school systems give priority based on: attendance to feeder schools, residence in the school's walk zone, and sibling attendance at the school (Abdulkadiroğlu et al., 2006).

[7] The school choice problem is closely related to the college admissions problem introduced by Gale and Shapley (1962). In the college admissions problem students have preferences over schools and vice versa, while in the school choice problem schools are simply objects to be consumed by students (Abdulkadiroglu & Sonmez, 2003).

some of which may also affect their educational outcomes directly (Manning & Pischke, 2006; Imbens & Wooldridge, 2009). For instance, children may attend different schools because they have different preferences or levels of motivation (Jackson, 2010). Despite these selection biases, some researchers have estimated the impact of the offer of admission to, and enrollment in, a better quality or more selective secondary school by relying on plausibly exogenous variation in school admission created by the action of external student-allocation mechanisms.

Prior studies have applied a regression-discontinuity design (RDD) to compare the average outcomes of students just below, and just above, an exogenously-set admissions cut-off, arguing that such students are equivalent in all respects other than falling on one side, or the other, of the cut-off. In this way, Pop-Eleches and Urquiola (2013) exploited the admission cut-offs of almost 2,000 secondary schools in Romania and found that, on average, students just above the cut-off for a school or track with higher-achieving peers scored 0.02 to 0.10 standard deviations higher on the graduation test than those who just missed the cut-off. Using survey data, they found evidence that students who barely made it into more selective schools perceived themselves as weaker than their classmates and felt marginalized. De Hoop (2011) estimated the impact of admission to competitive elite public-secondary schools in Malawi and found that marginal students, who were not assigned to a public school, were more likely to retake the primary-school examination. He also found that marginal students selected into 'high-quality' public secondary schools, defined in terms of peer ability and school characteristics, were less likely to drop out or switch to another school than those assigned to lower quality schools.

In contrast, for the case of Mexico City, Dustan (2010) estimated that the offer of admission to a set of 'elite' upper-secondary schools had a 0.19 standard deviations positive impact on the 12th grade examination for those at the margin of admission. Analyzing the same subsample for Mexico City, de Janvry et al. (2012) found that the marginal applicant admitted to an IPN-affiliated school had on average a 7.7 percentage points lower probability of taking the 12th grade examination.[8] They also found that, students who just passed the IPN's school admission threshold and took the 12th grade examination had, on average, 0.12 standard deviations higher scores than those who did not pass the threshold to enter an elite school.

Although more complex analytically, other studies have sought to estimate the causal effect of enrollment in a first-choice or selective secondary school on student outcomes, rather than focusing simply on the impact of the offer of admission. For instance, using lottery assignment to oversubscribed schools and instrumental-variables estimation, Cullen et al. (2006) found no impact of attending a first-choice public school in Chicago on a student's subsequent test scores.

[8] IPN stands for National Polytechnic Institute. Although authors interpret "not taking" the national standardized examination as dropout, this is a low-stakes test and there is anecdotal evidence of self-selection of students in certain schools into taking the test. In addition, students attending UNAM-affiliated schools do not take the standardized 12th grade test. UNAM stands for Universidad Nacional Autonoma de Mexico and it is the largest public university in the country.

Similarly, Hastings et al. (2006) showed that, in a school district of North Carolina, winning a lottery to attend a first-choice school led to no gain in test scores, on average. However, in contrast to this, in a follow-up study, Hastings et al. (2012) detected gains on average test scores –across all subjects– among lottery winners who attended their first-choice school, compared to those who lost the lottery. Capitalizing on a regression-discontinuity design (RDD), Dobbie and Fryer (2011) found that attending an examination school (i.e. elite public high schools) in New York City had little impact on SAT scores and college enrollment, but increased the rigor of the high-school courses taken. Dobbie and Fryer (2014) found that exposure to higher-achieving and more homogeneous peers has little impact on college enrollment, college graduation, or college quality.

Outside the U.S., Clark (2007) found that, using a RDD, attendance at selective secondary schools in the United Kingdom did not improve test scores four years later for those at the margin of admission, but had positive effects on course-taking and university enrollment. Relying also on a RDD, Jackson (2010) studied the student-allocation mechanism in Trinidad and Tobago based on an algorithm using student preferences and standardized test scores. Jackson found that students attending schools with higher-achieving peers had positive effects on performance in the end-of-secondary education examination, and on earning the prerequisites for admission to tertiary education. Using a similar methodology, Lucas and Mbiti (2014) studied the causal effect of attending an elite government school on student progression and test scores in secondary school. They showed that attending a national school resulted in exposure to a higher quality and more diverse peer group in a better resourced school. However, they found little evidence of positive impacts on test scores and no impact on the probability of timely progression through secondary school for the marginally admitted student. In this tradition, in my own research, I capitalize on the exogenous variation in school placement created by the student-assignment mechanism in Mexico City to estimate the causal effect of both the offer of admission to, and enrollment in, a more competitive school, on the probability of graduating and graduating on time, among those applicants at the admission cut-off.

Previous research has also found evidence that the impacts of the offer of admission to, and attendance at, a selective or better school differ among demographic subgroups.[9] For instance, Hastings et al. (2006) detected positive effects of attendance at a first-choice school among white and high-income students who won the lottery, but no effects for African-Americans and low-income students. Jackson (2010) reported that, at the admission cut-off, the effect for girls of attending a school with higher-achieving peers on examination performance was twice the effect for boys. Clearly, such differences by group may not be universal, but are perhaps unique to the specific settings, systems and students. In this paper, I explore possible heterogeneity in the effects

[9] A "selective school" often refers to an elite school or a school with very high average peer scores. A "better school" is often defined as a school with higher average peers scores and/or higher average resources.

of an offer of admission into, and enrollment in, a more competitive school, by selected demographic characteristics.

A shortcoming of earlier research is that it has focused mostly on the impact of the offer to, or attendance at, a selective school on student test performance but few have investigated the impact on student graduation outcomes for schools of different selectivity levels. Graduation outcomes are relevant because they are closely related to other education outcomes and subsequent labor market outcomes, such as earnings. Although researchers recognize that the effects of school quality can be driven by different factors (such as applicant's motivation and interest, peer composition, teacher attention, school resources, match quality), most studies have focused on peer quality as the main mechanism through which admission to a selective school ultimately impacts students' outcomes (Abdulkadiroğlu et al., 2014). Overall there is no consensus in the literature about the impact on subsequent educational outcomes of attending selective or better schools for the marginal applicant. On the one hand, students could benefit from the interaction with higher-achieving peers (Duflo et al., 2010). On the other hand, the effect of the interacting environment with higher-achieving peers might depend on where he or she falls in the ability distribution and on teacher attention to low-performing students (Cicala et al., 2011). The student-assignment mechanism implemented in Mexico City gives me the opportunity to estimate the impact of the offer of admission to, and enrollment in, a more competitive school on subsequent graduation outcomes of the student, which I argue may be more closely related to labor market outcomes than test scores. This particular setting allows me to estimate the net impact of attending a more competitive school relative to a school with a lower cut-off score. However, given the available data, in this paper, I cannot effectively separate the particular channels that are driving my findings.

2.1. **The Upper-Secondary Admission Process in Mexico City**

The Metropolitan Area of Mexico City[10] has the largest education market in the country and has been implementing a merit-based student-assignment mechanism in the upper-secondary public school system for more than a decade (COMIPEMS, 2008). Before that, teenagers who wanted to attend this education level had to apply to several schools simultaneously and then withdraw from all but the most-preferred school that had accepted them. This meant that often certain schools were left with vacant seats while many applicants were not admitted to any of the schools to which they applied. In this inefficient context, applicants from lower-income families were at greater disadvantage because they had fewer financial resources to cover the costs of applying to many different schools (COMIPEMS, 2008).

[10] The Metropolitan Area includes the Federal District and 22 municipalities of the State of Mexico.

In 1996, nine upper-secondary school subsystems from the Federal District and the State of Mexico created a commission, known as COMIPEMS,[11] to coordinate institutional efforts and respond to the increasing demand for upper-secondary education (COMIPEMS, 2012). The main task of this commission was to create a centralized competitive admissions process, currently known as the CIEMS.[12] Through the CIEMS process, applicants are assigned to public schools based on a designed and explicit interplay between their score on an entrance examination, a ranked list of up to 20 preferred school options,[13] and the pre-determined capacity constraints of schools.

The student-assignment process proceeds as follows.[14] In any given academic year, the process starts in late January with a public announcement of the CIEMS on the Internet and local newspapers (COMIPEMS, 2012). Interested candidates access the registration materials through their current lower-secondary school, local information centers, or the COMIPEMS website.[15] By the beginning of March, applicants must complete all the required documents, including the ranked list of up to 20 school options. Registration has a fixed cost of about $25 USD regardless of the number of schools listed. At the registration center, applicants receive a study guide for the test and an identification voucher necessary for taking the test. The entrance examination takes place at the end of June.

Once the entrance tests have been centrally graded by a computer, COMIPEMS's board meets for the assignment process. Applicants who scored less than 31 points, or have not graduated from lower-secondary education, are excluded from further consideration.[16] During the first round, all qualified applicants are ranked by their examination score, from highest to lowest. Then, a computer algorithm is used to assign ranked applicants to their most-preferred school with open seats, once their turn arrives. That is, seats are allocated down the student ranking: the top scorer is assigned to his first choice, the second-highest scorer then gets his most-preferred choice among schools with open seats, and so on. If several applicants with the same score compete for the last seats at a specific school, a representative from that institution must decide immediately whether he opens new seats to accept all tied applicants, or rejects all of them.[17] The process continues until

[11] COMIPEMS are the initials in Spanish for the Metropolitan Commission of Upper-Secondary Public Institutions. In Appendix A I include a list of the subsystems that integrate COMIPEMS.

[12] CIEMS is the acronym for Concurso de Ingreso a la Educación Media Superior, which translates to Upper-Secondary Education Entrance Competition.

[13] Applicants have to rank schools or school/track combinations. In the case of technical and some technological schools applicants must indicate their desired track or area of specialization (for example, accounting). Therefore, an applicant can list multiple tracks from the same school but each track will have a different cut-off score. For simplicity, I use the term "school" to refer to school/track combinations.

[14] See Appendix B.1 for the timeline of the process.

[15] Materials include registration forms, a calendar, and a manual with the list of schools, their location and specialization fields, if applicable. A complete list of the registration materials is presented in Appendix B.2.

[16] The minimum score restriction was eliminated in 2013. Schools affiliated to UNAM and IPN also require a minimum GPA of 7 out of 10 in lower-secondary education.

[17] The representative only has information regarding the number of tied applicants and their score but not about their personal characteristics.

all applicants are assigned, except for those who only request options with cut-off scores above their own score. Thus, the cut-off score for each school option, each year, is set equal to the entrance examination score of the applicant who fills the last seat. Final school-assignment decisions are published by the end of July.

During the second round, applicants who did not get a seat at one of their listed choices, but meet the requirements, have the opportunity to select a school that did not fill their seats. Finally, applicants must complete the paperwork at their assigned school in order to register. Applicants are only allowed to register at their assigned school.[18]

The merit-based admission system implemented in Mexico City provides exogenous variation in the offer of admission, and ultimately enrollment. Two applicants with the same preferences over schools could be assigned to different schools for having a one-point difference on the entrance examination. This allows the estimation of the causal impact on subsequent graduation outcomes, among applicants immediately on either side of the cut-off score for a specific oversubscribed school. It is such comparisons that are the topic of this paper.

2.2. **Specific Research Questions**

Like Dustan (2010) and de Janvry et al. (2013), I use data from the upper-secondary student-assignment process in Mexico City.[19] While they analyze the impact of the offer of admission to a limited sample of 'elite' schools on the 12th grade test scores and the probability of taking such tests,[20] I focus on the impact on ultimate high-school graduation of both the offer of admission to, and enrollment in, a more competitive school using a large sample of oversubscribed schools, both elite and non-elite. I exploit the exogenous variation generated by the student-assignment mechanism implemented in Mexico City to estimate the causal effect on the probability of graduating and graduating on time, at the threshold of admission, of the *offer of admission* to and *enrollment* in a school with a higher cut-off score and hence, higher-achieving peers. Specifically, I address the following research questions:

(1) *Does the offer of admission to a more competitive upper-secondary public school increase the probability that an applicant at the margin of being admitted will graduate within three years (on time) and within five years?*

[18] If applicants want to switch to another school they would have to take the exam again the following year.

[19] Estrada and Gignoux (2014) also use COMIPEMS data combined with a 12th grade survey in a subsample of schools to estimate the effect of the offer of admission to an IPN-affiliated (elite) upper secondary school on expected earnings at the end of secondary and higher education.

[20] They consider taking the ENLACE 12th grade test as a proxy for graduating, not necessarily on time. Some bias can arise from students who do not take the exam if they are systematically different from those that do take the low-stakes ENLACE test. Also, UNAM-affiliated schools, listed as the top-choice by about 30 percent of the applicants, do not take the ENLACE test so they are excluded from their sample.

(2) *Does* <u>*enrollment*</u> *in a more competitive upper-secondary public school increase the probability that the individual at the margin of being admitted will graduate from upper-secondary education within three years (on time) and within five years?*

3. Research Design

Due to the sorting of students into schools it is often challenging to estimate the impact of an offer of admission to, or enrollment in, a chosen upper-secondary school. In this paper, I deal with this self-selection problem by exploiting the way in which individuals are offered admission to public upper-secondary schools in Mexico City; according to the value of an observable "forcing" variable, relative to an exogenously defined cut-off point (Shadish et al., 2002). The source of variation comes from comparing the outcomes of applicants, who have similar characteristics and preferences, but are offered admission to different schools because they scored just below, or just above, the cut-off for admission to a specific school. To address my two research questions concerning the impact of the offer of admission to, and ultimately enrollment in, the chosen school, I use both a "sharp" and a "fuzzy" regression-discontinuity design (RDD), respectively.[21]

The idea behind the RDD is that the probability of being offered admission to a more competitive school, and hence attending a school with higher achieving peers, changes discontinuously at the cut-off score of that particular school. The key identifying mechanism underlying the RDD, which ensures estimates of treatment effect are unbiased and can be interpreted causally, is that the cut-off on the admissions forcing variable must be assigned exogenously and cannot be subject to strategic responses or manipulation by participants (Shadish et al., 2002). Under this continuity assumption, individuals close to the admission threshold, and on either side of it, will be equal in expectation prior to treatment, so that differences in their average outcomes post-treatment can be interpreted causally (Murnane & Willett, 2011). I argue that these conditions prevail in the Mexico City data because applicants do not know their examination score nor the schools' exact cut-offs when submitting their ranked list of preferences five months before the examination. Therefore, I can attribute any discontinuous jumps in the outcomes, on average, at the threshold, to the increase in the probability of being offered admission to a more competitive school. The main caveat of this approach is the localness of the treatment that only yields internally valid estimates for the marginally admitted applicant.

Using data from multiple cohorts and oversubscribed schools, each of which establishes its own cut-off score on the admissions examination yearly, I constructed 'sharp samples' for

[21] While in the "sharp" RDD the forcing variable perfectly predicts the treatment status (i.e. probability of treatment jumps from 0 to 1), in the "fuzzy" RDD the treatment status is only partially determined by the forcing variable (i.e. the probability of treatment changes by less than 1) (Jacob et al., 2012).

oversubscribed schools following Abdulkadiroglu et al. (2014).[22] I re-center the forcing variable for each applicant relative to the cut-off score of his respective sharp-sample school, such that the new variable will express the difference between the applicant's examination score and the minimum score required to be offered admission at that specific school. As I mentioned, my data contains many cut-offs, one for each oversubscribed school. Therefore, I follow the approach used in Pop-Eleches and Urquiola (2013) to combine all these discontinuities into a single analysis, pooled across applicants and oversubscribed schools, where the capacity constraint is binding, into one discontinuity with suitable fixed effects for the combination of cohort and application risk sets (i.e. School-School-Cohort).[23] I describe this approach in more detail later.

3.1. **Data**

To address my research question, I draw on a linked dataset that allows me to track the graduation of students who participated in the public upper-secondary assignment process in Mexico City between 2005 and 2009. My analysis incorporates two sources of data.

I use primary data from the set of applicants who registered for the COMIPEMS admission processes between 2005 and 2009. These five cohorts include over 1.5 million applicants and about 2,200 oversubscribed school/track options. I chose to focus on these particular cohorts because they include the students for whom I am able to measure on-time graduation (i.e. graduating within three years). I can also observe graduation within five years for the 2005 to 2007 cohorts. The dataset contains rich information on applicants' background, entrance-test score, ranking of up to 20 school options, assigned school, and responses to a context survey. Background information includes participants' gender, age, zip code, lower-secondary school attended and GPA. In addition, the context survey gathers information about the students' family structure, parental education, durable goods, family income, financial aid, and other related socio-economic and family questions. The data were made available to me through the COMIPEMS technical committee.

I augment the COMIPEMS database with student administrative records from different upper-secondary subsystems, again for the 2005-2009 applicant cohorts. Each upper-secondary subsystem keeps administrative records of students' enrollment and graduation from its schools, thereby providing information on my outcomes of interest, on-time upper-secondary graduation

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[22] The "sharp sample" for school *S* is the sample for applicants for whom offers of admission are sharp in the sense of being deterministically linked with *S*'s forcing variable. See Appendix C for a more detailed explanation of the construction of the sharp samples and the structure of the data.

[23] That is, dummies for interaction of cohort and applicant preferences ordering over schools (i.e. school pair combinations: School-School-Cohort). For example, when estimating the effect of enrolling in school A over school B, I pooled applicants whose ranking are A, B, C, with applicants whose ranking is A. B, D but some scored just above the cut-off for A while others scored just below the cut-off for A and got assigned to B. For more detail, see Appendix C.

and graduation.[24] I merged the existing databases using the COMIPEMS registration number and/or the national identification number (known as CURP).

3.2. **Analytic Sample**

I include, in my analytic sample, applicants who participated in the 2005 to 2009 upper-secondary admission's processes in Mexico City and who, if they had graduated on time would have done so between 2008 and 2012. I limit my sample to applicants who were assigned during the first round of the process to oversubscribed schools that did not require specifying a specialization field.[25] I further limit my sample to individuals that were taking the examination for the first time (i.e. non-repeaters) and who were age 25 or younger when taking the entrance exam.[26]

As I mentioned before, I constructed a 'sharp sample' for each pair of oversubscribed schools in the analytic sample. Each applicant serves as an observation at least for the sharp sample of its assigned school (see Appendix C).[27] Given that each applicant can be in more than one sharp sample, in the analysis I adjusted the standard errors of my parameter estimates to account for the clustering of observations within student.

My analytic sample includes students applying to 305 oversubscribed schools that did not require the explicit selection of a specialization field. In the pooled sharp samples, I have approximately 505,000 observations on 336,000 applicants who were assigned, between 2005 and 2009, during the first round to those oversubscribed schools. For comparability reasons across statistical specifications, I drop applicants with missing baseline covariate information and use the remaining sample of students in my discontinuity analysis.[28] Thus, my discontinuity sample consists of about 272,000 observations assumed to fall just below, or just above, their respective admissions threshold.[29]

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[24] One limitation of the data is that I do not observe graduation outcomes for applicants assigned to the State of Mexico's subsystem. This subsystem represents about 20% of the total universe of applicants assigned during the first round.

[25] This subsample includes schools from 5 subsystems (i.e. IPN, UNAM, UAEM, DGB, COLBACH), excluding the SE subsystem for which students' graduation records are not available. To assess the potential bias of omitting applicants assigned to the SE subsystem, I used the 12^{th} grade standardized test as a proxy of graduation. Results are not very different from the ones presented here. Also, results for other estimations, including those that consider the rest of the subsystems and each school/track combination as a separate school have a slightly smaller magnitude, but same direction and statistical significance, than the ones for the analytical sample.

[26] I limit my sample to first-time applicants because applicants who decide to retake the test in the following year may have different unobserved characteristics, like motivation, than applicants who did not retake the entrance examination.

[27] For instance, an applicant who ranked school A first but did not qualify there, is also in the sharp sample for school B if B is his second choice and he is assigned there.

[28] I acknowledge that this can pose a threat to internal validity if those who did not answer certain questions were systematically different from those that did. A comparison between the unrestricted and the restricted samples showed very little differences in the average baseline characteristics, which leads me to believe that missing values are random. I discuss this further in the Threats to Validity section.

[29] Considering a bandwidth of 9 points obtained through the cross-validation (CV) method.

3.3. **Descriptive Statistics**

In Table 1, I present a set of summary statistics for the four different samples. In column 1, I report the average values of the included variables for all the applicants. About 70 percent of them were assigned during the first round to both oversubscribed and non-oversubscribed schools. In column 2, I display the average values of the same variables for the same applicants in a sample that I have restricted to first-time applicants under the age of 25 who were assigned during the first round of the process and who listed oversubscribed schools. In column 3, I report mean values on the same variables for my analytic sample comprised of applicants with non-missing values for the selected covariates. Finally, in column 4, I present the average values for those same variables for applicants assumed to fall within my ultimate preferred bandwidth of 9 points (that is, those that fall within 9 points either side of their respective cut-off points on the admissions-score forcing variable). I report standard deviations of all variables in parentheses. Note that there are explicit differences in averages between the full sample of upper-secondary applicants and the other samples. That is expected given that not all applicants are assigned to an oversubscribed school during the first round.

Those that are not offered admission to an oversubscribed school tend to have lower scores and come from more vulnerable families. For instance, the full sample has a greater percentage of low-SES applicants and household heads with fewer years of schooling than the other subsamples. Despite the expected differences with the full sample, the rest of my samples (columns 2 to 4) considering only oversubscribed schools are comparable, in average value, on most of the presented variables.

In my analytic sample, in column 3, I observe that 48 percent of applicants are males, the average age is 15.2 years, only 2 percent are indigenous, the average GPA in lower-secondary education is 8.4 (out of 10), 12 percent attended a private lower-secondary school, and 92 percent of them live in the Metropolitan Area of Mexico City. In addition, I notice that the household head has on average 11 years of schooling (i.e. incomplete upper-secondary), 42 percent come from families below the median income but only 3.3 percent of them work and 1.6 percent receive a conditional cash transfer (i.e. Oportunidades-Progresa). Regarding their school preferences (Panel B), applicants in the analytic sample listed, on average, 9 school options and were assigned to their third option. The average score in the entrance test was 78 points (out of 128). Finally, in Panel C of Table 1, I show the average values for the two outcomes of interest: about 46.7 percent of the applicants graduate on time ultimately, while 57.0 percent of them graduate within five years of starting upper-secondary education.

3.4. **Measures**

For each individual in my analytic sample, I create the *outcome* variable, GRADONTIME, a dichotomous indicator of whether the applicant graduated from the assigned school within three years (=1; 0 otherwise).[30] I also created the outcome variable, GRAD, a dichotomous indicator of whether the applicants from cohort 2005 to 2007 graduated from the assigned school within five years (=1; 0 otherwise). I will refer to these outcomes as Y.[31] These outcomes have clear policy relevance because they are closely related to other education outcomes and subsequent labor market outcomes. In my regression-discontinuity analyses, my *forcing variable* (Murnane & Willett, 2011) is the upper-secondary admission-examination score (out of 128), *SCORE,* that was used to determine assignment each year. I center the forcing variable, SCOREc, with respect to the admissions cut-off of the relevant school in a specific year. That is, applicants' scores on the forcing variable are expressed in terms of their distance from the respective sharp sample cut-off (j) such that the marginal admitted student has a SCOREc of zero.

My *question predictors* include the dichotomous variables OFFER and ENROLL, for my two research questions respectively. OFFER is a dichotomous variable that records whether an applicant scored above the relevant cut-off score and, therefore, received an admission offer (=1; 0 otherwise). ENROLL is a dichotomous indicator recording whether the applicant enrolled subsequently in his or her assigned school (=1; 0 otherwise).

To increase the precision of my estimates, I include selected background covariates that I hypothesize to be related to my outcome (Bloom, 20012; Jacob et al., 2012).[32] They include indicators of gender, age, indigenous language, lower-secondary GPA and working status, as well as indicators of parental education, socio-economic status,[33] living outside the metropolitan area, family is recipient of conditional cash transfer (Progresa-Oportunidades), and version of the examination[34] taken. In my statistical models, I refer to the complete covariates as the vector $\boldsymbol{X}$. Additionally, as I mentioned before, I include dummies for the combination of cohort and application risk sets; that is, school-school-cohort fixed effects, as described in Appendix C.

3.5. **Empirical Strategy**

To address my research questions, I use a standard regression-discontinuity design (RDD) approach, based on the assumption that the relationships between the probability of upper-secondary school graduation, and on-time graduation, with my centered forcing variable are

[30] Applicants can only register at their assigned school; otherwise they would have to participate again in the CIEMS the following year.

[31] There is a positive relationship between the cut-off score for a school and the probability of graduation or on-time graduation. The best fit for this relationship is a quadratic polynomial.

[32] I list and define these covariates in Appendix D.

[33] I created a principal components index of access to durable goods (like car, computer, DVD) and services (like sewerage, telephone, Internet) at home. This shows positive correlation with reported family income. Family income is not used here because not everyone reported it.

[34] Applicants who list a UNAM-affiliated school as their first choice take UNAM's version of the examination, while the rest take CENEVAL's. These examinations are supposed to be "technically equivalent" in content and difficulty, and to have a high degree of reliability, validity, lack of bias, and equity (COMIPEMS, 2012). Empirical analysis, not shown here, suggests that the general results are not sensitive to pooling across examination versions within the same cohort. In the analyses, I include a dummy of the UNAM version of the exam.

"locally" linear within a bandwidth of the respective cut-off point. I implement a RDD using local linear regression (LLR),[35] in combination with a cross-validation bandwidth determined by the methods of Imbens and Lemieux (2008).[36] As a robustness test, I check the sensitivity of my results to different bandwidth choices.

Under my design, applicants may have received an offer of admission depending on their placement relative to the exogenous admission cut-off at a specific school. Applicants, who scored just below the cut-off for a particular school and have the same preference ordering,[37] provide an adequate counterfactual for those who scored just above. Any difference in their outcomes can be attributed to the fact they have access to different schools.

I first address the question of the impact of the offer of admission to a more competitive school (or *intent-to-treat*) on the probability of graduating on time, or graduating, by fitting the following "reduced-form" regression:

$$P(Y_{ij}=1)=\pi_o+\pi_1 OFFER_{ij}+\pi_2 SCOREc_{ij}+\pi_3 SCOREc\times OFFER_{ij} + X_i^{'}\delta+\varphi_j+\varepsilon_{ij} \quad [1]$$

for the i[th] student in the sharp sample *j*, where φ_j is the vector of school-school-cohort fixed effects and ε_{ij} is the residual. As I mentioned before, I adjust the standard errors for the clustering of observations within individual as a student may appear in more than one sharp sample. I include the interaction between the treatment and the forcing variable to allow the slope to differ at the two sides of the cut-off. In the model, all parameters have their usual interpretations. Note that I have re-centered the forcing variable to express the difference between each applicant's examination score and the minimum score required to be offered admission at a specific school and in a specific year. Because of that, the critical parameter is the slope associated with question predictor OFFER (π_1), referred to as the local intent-to-treat (ITT) effect (Bloom, 2012). This parameter expresses the causal effect of being offered admission to a more competitive school (i.e. a school with higher cut-off score and higher-achieving peers).

Not all the applicants who score above the cut-off enrolled in their assigned school; some decided to re-take the exam the following year. Given the design of the allocation mechanism under study, it is not possible for an applicant to enroll in a school to which he or she was not

[35] In this case, a probit or logit specification yields marginal effects similar to those obtained under a linear-probability model.

[36] With a discrete forcing variable it is not possible to compare outcomes in very narrow bins, like the optimal bandwidth IK by Imbens & Kalyanaraman (2012), just to the right and the left of the threshold (Lee & Card, 2008). Based on the cross-validation criterion, I chose a bandwidth of 9. I use the term bandwidth to indicate the width of the analytic window used for estimation.

[37] I define "same preference ordering" as having the same ranking as pertaining to the two choices at issue. For example, in comparing the impact of enrolling in school A or B, I pooled together applicants whose rankings are A,B, C, with applicants whose ranking is A, B, D. For more detail see Appendix C.

offered admission. Consequently, the magnitude of the discontinuity at the threshold may be reduced because a proportion of the group offered admission did not register (Bloom, 2012). Thus, to estimate the causal impact of enrollment (i.e. *take-up* of the admission offer), I use a standard instrumental-variable (IV) approach to model the relationship between on-time graduation (or graduation) and enrollment following a "fuzzy" regression-discontinuity design (Murnane & Willett, 2011; Jacob et al., 2012). In the first stage of the IV analysis, I predict the probability of attending a more competitive school using a student's position relative to his admission cut-off as an instrument in a local-linear probability model, as follows:

$$P(ENROLL_{ij}=1)=\alpha_o+\alpha_1 OFFER_{ij}+\alpha_2 SCOREc_{ij}+\alpha_3 SCOREc\times OFFER_{ij} + X_i^{'}\theta_1+\varphi_{1j}+\delta_{ij} \qquad [2a]$$

for the i^th^ student in the sharp sample j. Where, δ_{ij} is the first-stage residual and φ_j is the vector of school-school-cohort fixed effects. In the second stage of the IV approach, I fit the following regression model:

$$P(Y_{ij}=1)=\beta_o+\beta_1 EN\hat{ROLL}_{ij}+\beta_2 SCOREc_{ij}+\beta_3 SCOREc\times OFFER_{ij} + X_i^{'}\theta_2+\varphi_{2j}+\varepsilon_{ij} \qquad [2b]$$

where $EN\hat{ROLL}$ is the predicted value (i.e. the exogenous part) of the first-stage outcome for each applicant. Because of my inclusion of these predicted values in place of the nominal (and potentially endogenous) question predictor, ENROLL, I adjust the second-stage standard errors of the parameter estimates using standard methods (Murnane & Willett, 2011; Wooldridge, 2002).[38] In the model, the parameter of interest is β_1, referred to as the local treatment-on-the-treated (TOT). It represents the causal effect of enrolling in a more competitive school on the probability of graduating, or graduating on time, from upper-secondary education for applicants at the margin of admission (Bloom, 2012).

The selection of bandwidth, or the window around the cut-off, is an important decision in the application of the RDD approach. The closer you get to the cut-off, the more similar the individuals are, but the less power you have; the further away you move from the cut-off point, you gain in power, but you lose on similarity of groups. Nevertheless, a wider bandwidth makes it more difficult to model the functional form of the relationship between the forcing variable and the outcome, as one must consider non-linear functional forms. Although I chose the cross-validation bandwidth of 9 points as my preferred bandwidth, as a robustness test I check the sensitivity of my results to different bandwidth choices.

[38] In practice I use the ivregress 2sls and xtivreg commands available in the Stata software, which yields the adjusted second-stage standard errors.

4. Results

In this section I present my empirical results. I begin by examining the soundness of the proposed identification strategy. First, I show that the discontinuity in the offer of admission is not associated with discontinuous changes in baseline covariates at the threshold for admission. I also show that within defined choice sets applicants cannot manipulate their position with respect to the cut-off score. Then, I turn to the main results. I present the results from the "sharp" and "fuzzy" regression-discontinuity designs, pooling together the cut-offs of oversubscribed schools under analysis. Finally, I re-estimate the effects of the offer of admission for selected demographic groups and at different parts of the cut-off score distribution.

4.1. **Internal Validity**

The rationale behind the regression-discontinuity design is that, if the offer of admission to a more competitive school changes discontinuously at the threshold, then the causal effect of the offer of admission can be identified at the cut-off even when applicants' scores are related systematically to factors that affect graduation and on-time graduation. The main identification assumption of this methodology is that individuals cannot manipulate their position with respect to the cut-off point, such that the cut-off point should work as a lottery for those close to it. Individuals immediately on either side of the cut-off are, on average, equivalent on observed and unobserved characteristics and differ only in position relative to the threshold. To confirm that the regression-discontinuity design provides internally valid estimates, I verify that two key conditions are met. First, a valid regression-discontinuity design requires that the determination of school-specific thresholds are made independently of the examination scores obtained by applicants (Bloom, 2012). In this sense, a potential threat arises when applicants are able to manipulate their position relative to the cut-off score or when schools choose cut-offs so they can admit specific applicants. Second, the RDD methodology requires that the distribution of baseline characteristics is the same for applicants whose scores place them just on one, or the other, side of the cut-off (Bloom, 2012; Murnane & Willett, 2011). Thus, an important potential threat to validity may be that there are statistically significant differences in baseline characteristics between applicants just below, and just above, the cut-off score.

First, I argue that the cut-off score for each school is unknown a priori by both applicants and schools, leaving little room for manipulation. Applicants do not know their examination score when ranking their options, while the cut-off scores are not determined until the allocation algorithm is run and ties are broken.[39] Examinations include only multiple-choice questions that are marked centrally by a computer, limiting the possibility of manipulating the scores. I inspect

[39] Decisions regarding tied applicants are based solely on scores and the number of additional seats each school could open. The school administrators have no information on the characteristics of tied applicants and have to decide whether they accept or reject all tied applicants.

the density of the forcing variable graphically in order to rule out a discontinuity in the number of observations around the admission cut-off.[40] In Figure 1, I display a histogram of the applicants at each admission score on the re-centered forcing variable, for all schools in my sample. The density is relatively smooth in the neighborhood of the cut-off. In the presence of manipulation, I would have expected to see a big lump just above the cut-off. Therefore, endogenous sorting, or manipulation, does not seem to be a major concern.

As an additional test, I look for potential discontinuities in several of the background characteristics. I examine the distribution of selected covariates to reveal that no major discontinuities existed at the cut-off that might contribute to my results. I re-fit the hypothesized RDD model [1] for alternative bandwidths and without additional controls, while treating each of the baseline characteristics as a dependent variable. In Table 2, I present the different estimates for the slope parameter associated with the offer of admission for each covariate and for different choices of bandwidth. Each row in Table 2 represents the estimate of a different baseline covariate regressed on the offer of admission for different choices of bandwidth. In these analyses, the parameter of interest is the estimate associated with the offer of admission. In most of the cases I fail to reject the null hypothesis that the parameter associated with the offer of admission is equal to zero, in the population, for the selected covariates, except for schooling of the household head.[41] To further test the assumption of baseline equivalence, I regress the offer of admission on a set of applicants' characteristics. Across all window widths, I fail to reject null hypothesis that the covariates jointly explain the variation in the offer of admission. Based on these results, I claim that applicants around the threshold are, on average, equal in expectation such that any differences in the outcomes of interest result from discontinuities in the offer of admission.[42]

4.2. **Causal Effect of the Offer of Admission on Graduation Outcomes**

In Figure 2, I provide a graphical representation of the bivariate relationship between the forcing variable, SCOREc, and my outcomes of interest. In this graph, I plot the sample mean of outcome against the re-centered forcing variable. By visual inspection, it appears that the offer of admission to a more competitive school has led to a discrete decrease on the probability of graduating and graduating on time. On average, applicants just above the cut-off (i.e. offered admission) appear to have lower probability of timely graduation than applicants just below the cut-off (Figure 2a). The effect on graduation within 5 years is also negative but slightly smaller (Figure 2b). The difference between these plots suggest that some students are held back 1 or 2 grades but eventually graduate.

[40] McCrary (2008) proposed to test directly for any manipulation of the forcing variable. This works well when the running variable is continuously distributed, but is inconsistent when the running variable is discrete (Lee & Card, 2007; Frandsen, 2014).

[41] I attempt to address this issue by controlling for observable characteristics of the applicants.

[42] The lack of a statistically significant break for the covariates, around the admission threshold, is similar to the finding of no difference in mean values in a randomized control trial (Lee and Lemieux, 2010).

I confirm the previous trend by fitting a local-linear probability model [1]. In Table 3, I present the intent-to-treat (or reduced-form) estimates of the parameter of interest, under alternative model specifications. As a robustness test, I check the sensitivity of results to a variety of window widths, in addition to the CV bandwidth (9 points), with each of the columns containing the results of fitting the same model in a slightly different window width. Going from left to right, the columns present models fitted in progressively wider windows, starting with 3 points in column 1 and ending with 15 points in column 5. I observe little fluctuation in the impact of OFFER. This suggests that my results are, in fact, robust to the choice of bandwidth.

The estimates in Panels A and B of Table 3 represent the causal effect, for applicants at the margin of admission (within the bandwidth noted in the column heading), of receiving an offer of admission to a more competitive school on the subsequent probability of graduating on time from upper-secondary education, controlling for baseline characteristics and school-school-cohort fixed effects. Similarly, estimates in Panel C and D correspond to the effect, for the marginal applicant, of the offer of admission to a more competitive school on upper-secondary graduation within 5 years.

My preferred specification uses a CV bandwidth of 9 points (column 3 in Table 3), controls for baseline characteristics and school-school-cohort fixed effects (Panels B and D). It also includes the two-way interaction between the treatment and the forcing variable (OFFER×SCOREc). I interpret the parameter estimates as indicating that, on average, applicants at the margin of admission to a more competitive school are 3.1 percentage points less likely to graduate on time and 1.1 percentage points less likely to graduate within five years, relative to similar applicants who just did not receive an offer to that particular school in the same year. For the analytic sample, these represent a 6.5 percent decrease below the mean on-time graduation rate of 46 percent, as well as a 2 percent of the mean on-time graduation rate of 57 percent. The estimates are statistically significant at conventional levels (p-value<0.01) and are relatively stable across different bandwidths.

In the previous analyses, I assumed a linear relationship between my outcomes of interest and my forcing variable. However, given the sensitivity of RD estimates to non-linear specifications, Imbens & Lemieux (2008) suggest choosing a bandwidth within which the relationship between the outcome and the forcing variable is locally linear. As I illustrate in Figure 3, the relationship between the outcomes and the forcing variable seems linear within the selected bandwidth. To assess the sensitivity of my findings to the linearity assumption, however, in my reduced-form regression I include polynomial specifications of SCOREc and two-way interactions between each polynomial term and OFFER. In Table 4, I report the point estimates for the different non-linear specifications (up to quartic) within my selected 9 points bandwidth, controlling for baseline covariates and school-school-cohort fixed effects. Although my estimates fluctuate a little, I find that the direction and statistical significance of my findings are preserved across the different specifications. In addition, the terms that describe the polynomial specifications of the relationship are not statistically significant suggesting that within the selected bandwidth the relationship is locally linear.

4.3. **Causal Effect of Enrollment on Graduation Outcomes**

Since not all applicants enroll in their assigned school, the jump in the probability of enrollment around the cut-off is smaller than unity. To account for this, I follow an instrumental-variables approach and use the offer of admission to instrument for actual enrollment in a more competitive school. I argue that this variable meets the two conditions needed for valid instrument: exogeneity and relevance.

First, although I cannot formally test the exogeneity condition in my model,[43] throughout this paper I have mentioned that this particular student-allocation mechanism creates an exogenous variation in the placement offer at an oversubscribed school among applicants of equivalent skills at the cut-off for admission. That is, the variation in enrollment captured by the offer of admission at the threshold is exogenous.

Second, I argue that the offer of admission is related to the variation in enrollment in a more competitive school. In Figure 4, I illustrate this discrete discontinuity on the probability of enrollment at the cut-off as a function of the applicant's admission score. I indicate the cut-off score for receiving the offer of admission (SCOREc$\geq$0) with a dashed vertical line at SCOREc=0. Given the design and enforcement of the student-allocation mechanism under analysis, it is not surprising that enrollment to the left of the admission cut-off score is 0. Although there is a substantial jump to the right of the cut-off score, the proportion of the applicants who enroll at their assigned school does not rise to one. On average, 95% of the applicants offered a seat take the offer, which indicates a very high compliance rate.[44] This is why in this particular case the intent-to-treat (ITT) are so close to the treatment-on-the-treated (TOT) estimates.

My first-stage estimates confirm that receiving an offer of admission results in a substantial jump in the probability of enrollment at the cut-off for admission. In Table 5, I report the first-stage estimates for the difference in the average probability of enrollment in a more competitive school between those who scored just below and just above the admission threshold, for alternative choices of bandwidth. Going from left to right, the columns present models in progressively wider bandwidths, starting with 3 points in column 1 and ending with 15 points in column 5. The coefficient on the parameter of interest, OFFER, is statistically significant at conventional levels for the different choices of bandwidth. In addition, the large F-statistic (>50) associated with this parameter suggests that the offer of admission is a strong instrument for predicting the probability

[43] To test this condition, it is necessary to have an over-identified model. That is, a specification with a larger number of instruments than endogenous regressors.

[44] Take-up rates vary by school subsystem. The opportunity cost of not registering is high because applicants who do not enroll in their assigned school cannot register to attend another public school and need to re-take the entrance test the following year.

of enrollment of an applicant even within the narrowest bandwidth. The magnitude and direction of my estimates at the cut-off are comparable across the different choices of bandwidth.

My instrumental-variable estimates of the causal impact of enrolling in a more competitive school also indicate a negative effect on graduation and on-time graduation for applicants that were just offered admission relative to those who just missed the admission cut-off. In Table 6, I present the local treatment-on-the-treated (TOT) estimates for different choices of bandwidth. The parameters of interest in my preferred specification, with bandwidth of 9, covariates, and school-school-cohort fixed effects (Panels B and D), have negative and statistically significant coefficients for on-time graduation and graduation. They indicate that, on average, an applicant who enrolled in a more competitive school is 3.2 percentage points less likely to graduate on time, and 1.2 percentage points less likely to graduate from upper-secondary education than a similar applicant student who scored just below the admission threshold. Given the high compliance rates, IV estimates are only slightly larger than their analogous reduced-form estimates.

4.4. **Heterogeneous Effects**

As discussed in the literature review, some previous studies have found evidence of differential effects of admission to a selective school, by students' characteristics such as gender, race and income level. In this section, I explore possible interactions between the parameter of interest and selected demographic characteristics. I also examine whether the effects differ according to where the schools' cut-offs are located in the admission cut-off score distribution. Finally, I investigate whether the effects of the offer of admission to, and enrolment in, a more competitive school are different for those at the margin of admission to their top-choice school (i.e. their most-preferred option).

In Table 7, I explore the effects of possible interactions between the predictor of interest and selected characteristics of applicants (CHARACT), such as: gender, socio-economic status, working status, type of lower-secondary school attended and lower-secondary GPA.[45] For the sake of space and simplicity, in this table, I only present the reduced-form estimates for observations within my preferred bandwidth (i.e. 9 points). I fitted the following general "reduced-form" regression:

$$P(Y_{ij}=1)=\pi_o+\pi_1 OFFER_{ij}+\pi_2 SCOREc_{ij}+\pi_3 SCOREc\times OFFER_{ij} + \pi_4 CHARACT\times OFFER_{ij}+X_i^{'}\delta+\varphi_j+\varepsilon_{ij} \qquad [3]$$

In column 1, I interact OFFER with MALE and find a negative relationship. On average, males that scored just above the cut-off to a specific school have an estimated probability of graduating on time that is 1.8 percentage points less, and have an estimated probability of

[45] I do not find a statistically significant interaction with other covariates such as indigenous, working status, and receiving Oportunidades.

graduating within five years that is 1.6 percentage points less, than similar females that just cleared the threshold for admission. That is, while the effect for both males and females is different from zero, I find larger negative effects for males than for females of being a low-scoring student in a more competitive school. In column 2, I examine whether the effect differs for applicants who work for a wage compared to those that do not work. Though I estimate a negative relationship, it is not statistically significant for either of the graduation outcomes. In column 3, I interact OFFER with a binary indicator of low socio-economic status and find a negative relationship. Thus, I estimate that marginal applicants who come from low-SES households are at greater risk of falling behind and not graduating than similar applicants who scored just above the cut-off but come from high-SES families.

Regarding the academic characteristics of applicants, I find that marginal applicants, who attended a private lower-secondary school, have a higher estimated probability of graduating and graduating on time (column 4). Finally, in column 5, I display the results of interacting OFFER with the GPA. I find that for those at the margin of admission, a one-point increase in GPA (out of 10) is associated with an increase of 1 percentage points in the estimated probability of graduating on time and 0.9 percentage points in the probability of graduating within 5 years. This indicates that high- and low-ability students close to the threshold of a specific school are affected differently. All these interactions, except for the one with working status, were statistically significant at conventional levels (p-value<0.05). They confirm that, at the margin, the effect of the offer of admission on timely graduation, and graduation, differs according to certain demographic and academic characteristics of applicants. Taken together, these results suggest that the negative effect of scoring just above the threshold, and receiving an offer of admission to a more competitive school, is larger for the marginal applicants who come from more vulnerable contexts (i.e. low-SES, public lower-secondary school, lower GPA).

The results that I have present so far pool together students applying for admission to all oversubscribed schools. Given that the effects may differ depending on the selectivity of the school, I next examine how the impact of the offer of admission on timely graduation differs according to where the school cut-offs are located in the admission cut-off distribution. I subdivide the sharp samples according to their quartile in the admission cut-off distribution, with the most selective school being included in the top interquartile.[46] Given this structure, the higher the admission cut-off score, the more selective a school is (and the higher the average peer achievement is).

In Table 8, I present the results for school choices in each of the interquartiles. I showed that the magnitude and statistical significance of the effect of the offer of admission on timely graduation and graduation gets larger as one moves to a more selective school (i.e. a school with

[46] The interquartiles correspond to having a cut-off score above 82 (q4), a cut-off score between 72 and 82 (q3), a cut-off score between 61 and 72 (q2), or a cut-off below 61 (q1).

a higher cut-off score). Among schools in the top quartile, I estimate that the offer of admission to a more competitive school reduces the probability of graduating and graduating on time, for the marginal applicant, by 4.8 and 7.5 percentage points respectively. In contrast, I do not find any effect of the offer of admission for those at the margin of admission to a school in the two bottom quartiles.[47] When focusing only in the schools in the top 5 percent of the cut-off score distribution (not shown here), I detect a negative effect of the offer of admission of about 7.3 percentage points on graduation and 10.1 percentage points on timely graduation. Therefore, it appears that most of the impact of the offer of admission on graduation and graduation on time is driven by the most selective schools (i.e. in the top quartile of the admission cut-off distribution).

Finally, I explore whether the effects of the offer of admission to, and enrollment in, a more competitive school might be different for those at the margin of admission to their top-choice school (i.e. their most-preferred option). I restrict the analysis to applicants who scored just below and just above their top-choice school. On average, 43 percent of the applicants in the analytic sample were offered admission to their most-preferred school. In Table 9, I present the results analogous to those in Table 2 but for focusing on the offer of admission to one's first-, second- or third-choice. I observe that applicants who scored just above the cut-off and were offered admission to their top-choice school have, on average, an estimated probability of graduating on time that is 5.9 percentage points less, and have an estimated probability of graduating within five years that is 3.6 percentage points less, than similar applicants who scored just below the threshold for that same school. Thus, effects for the top-choice school were particularly large. Effects on timely graduation and graduation are smaller, but still negative, for the offer of admission to one's second-choice school. I did not find statistically significant effects on graduation outcomes when estimating the impact of being just admitted or denied admission to one's third-choice school.

5. Threats to Validity

In this section, I discuss the main threats to internal and external validity of the inferences that can be drawn from my results.

The regression-discontinuity methodology has strong internal validity for inferences at the cut-off point for admission, providing its assumptions are met. Under this type of design, internal validity can be threatened if: (1) applicants can manipulate their position relative to the cut-off score or if schools choose cut-offs to admit particular applicants; and (2) there are statistically significant differences in baseline characteristics between applicants just below, and just above, the cut-off score. I have already shown evidence and argued that neither of them seems to pose a major threat in this particular setting. In addition, I must note that the process of student allocation

[47] When focusing on admission to IPN-affiliated schools, regarded as elite schools, de Janvry et al. (2013) found an increase of 7.7 percentage points in risk of dropout, proxied by the probability of taking the 12th grade standardized test.

implemented in Mexico City does not allow for crossovers of students between schools, which could bias the results. Applicants who were assigned during the first round cannot easily transfer to a different school that has no available seats. If they want to transfer to a school that still has seats open, they must meet the minimum cut-off score determined during the first round and go through a very time-consuming bureaucratic process. During the years under analysis, the only way to transfer to a different school was to re-take the entrance examination the following year.

Since not everyone answered all the questions of the background survey, one potential threat to internal validity of my findings concerns the potential for selection bias arising from those who did not answer them all. Comparing the unrestricted and the restricted samples I find very little difference in the average baseline characteristics, which leads me to believe that missing values are random.[48] When analyzing the balance of the covariates for applicants just below and just above the cut-off for admission, I found that there are no systematic differences between the two groups for alternative choices of bandwidth.

As in any regression-discontinuity design, the limited external validity is the main limitation of the findings presented in this study. Despite the internal validity of my estimates, I was only able to make causal claims for individuals at the margins of being offered a seat at an oversubscribed upper-secondary public school in Mexico City that did not require specifying a specialization field in the academic years under analysis. Given my sample restrictions, my inferences apply to applicants who took the entrance examination for the first time and were less than 25 years old when taking the examination. This implies that I cannot extrapolate my findings on how the offer to, and enrollment in, more competitive schools impacts subsequent upper-secondary graduation for students with very high, or very low, admission scores or who have taken the examination multiple times. In sum, unless stronger assumptions are imposed, my findings cannot then be generalized to applicants who score far from the admission cut-off, or to not-oversubscribed public schools, or private schools, or different places or years.

Another limitation to the external validity of my findings comes from the fact that I have no administrative records of enrollment and graduation for students attending schools from one particular subsystem in the State of Mexico. In contrast to other large subsystems, this subsystem has many small schools spread along the border of the Metropolitan Area. Many of those schools are not oversubscribed and tend to have low to medium cut-off scores. When creating the sharp samples, I omitted applicants that were offered admission to a school from this subsystem. For those assigned to any other subsystem under analysis, I considered their next most-preferred option to which they qualified and that was not from the subsystem with missing data. To assess the potential bias, I re-estimated the reduced-form model using the year a student took the 12th grade standardized examination as a proxy for graduation (low-stakes attached to the test).[49] Reduced-

[48] Tables available upon request.

[49] Taking the exam does not necessarily imply a student graduates, just that he or she is enrolled in 12th grade. There is anecdotal evidence of self-selection into the test. That is, some schools might ask their lower-performing students

form estimates from the regression, which uses the year of taking the 12th grade examination as a proxy for graduation and on-time graduation among applicants from the subsystem with missing data, are slightly smaller (-0.7 and -2.2 percentage points, respectively) than those for the sample with complete administrative data, but they have the same direction and statistical significance.

6. Discussion

My results indicate that applicants who barely passed the cut-off to a more competitive school (i.e. higher cut-off score and higher achieving peers) in Mexico City are less likely to graduate and to graduate on time than similar applicants who just missed the cut-off admission and were offered admission to, and enrolled in, a less competitive school. Taken together, my findings suggest that the most vulnerable students (i.e. males, students from low-SES families, those enrolled in public lower-secondary schools, and those with relatively low GPAs) who just make it into selective or elite schools (i.e. top quartile of the cut-off distribution), are at greater risk of not graduating. Overall, in this particular setting it seems that it is better, in terms of graduation outcomes, to be an average-ranking student in a school with lower- or average-achieving peers than the lowest ranking student in a more competitive school with higher-achieving peers.

As I mentioned before, I am only able to detect the net effect of the offer of admission to, and enrollment in, a school with a higher cut-off score and hence, higher achieving peers. However, peer composition may not be the only component of the educational production function that changes at the cut-off (Dobbie & Fryer, 2014). Other observable (i.e. resources, teacher quality) and unobservable (i.e. curriculum, academic rigor, parental involvement) characteristics of the schools may differ.

There is no consensus in the literature regarding the role of peers for the marginal students. On the one hand, peers and social interactions are important for the formation of skills and human capital (Sacerdote, 2011). On the other hand, being among the lowest-ranking students within a school, relative to the other peers, could have a negative effect (Cicala et al., 2011). First, marginal students are likely to have a lower class rank than they otherwise would have, so their confidence and motivation could be reduced by being at the bottom of the class (Barros & Salcedo, 2009). Thus, students at the bottom of the ability distribution at a given school will then be more likely to fail courses, fall behind, and eventually drop out of school. Second, teachers may devote less time and attention to the least academically skilled students in the classroom. If teachers teach to the mean of the class, students at the bottom of the ability distribution in a specific school will be exposed to more academic rigor than they would otherwise face. That is, instruction may be far above the level the marginal student can understand.

to not attend the day of the test to inflate school mean score. This might bias the estimates downward. Students attending UNAM-affiliated schools do not take the test.

In a similar case study to the one presented here, Pop-Eleches and Urquiola (2013) studied behavioral responses of students who participated in the Romanian centralized admission process and found that those who scored just above the cut-off to a ‘better’ school perceived themselves, on average, as weaker relative to their peers and felt marginalized. For the case of college students in Colombia, Saavedra (2009) noted that marginal applicants struggled to get through graduation relative to rejected applicants who attended less selective schools.

Below, I describe a potential mechanism that may undergird my results and discuss some policy implications of my findings.

6.1. **Reference-group Perspective**

As I mentioned before, the underlying rationale for allowing students to choose their schools is that they will benefit from being able to enroll in a school that best matches their interests and abilities. My results are consistent with the model proposed by Cicala et al. (2011) which suggests that the effect of the interacting environment with higher-achieving peers might depend on where a student falls in the ability distribution. That is, marginal applicants, who just passed the cut-off to gain admission to their top-choice school, are usually the ones at the bottom of the ability distribution and the ones that tend to have a harder time keeping up with the rest of the class. On average, marginal students have a lower class rank than they would have had in another school and might be less competitive in subsequent outcomes “even if their absolute level of achievement is unchanged” (Dobbie & Fryer, 2014, p.59).

This explanation is also consistent with sociology models showing that if relative ability matters, then admission into selective schools could adversely impact children who scored close to the cut-off. It relates to the sociology literature regarding relative deprivation and social comparison dynamics, such as the frog-pond model (Davis, 1966) or the big-fish-little-pond effect (Marsh & Parker, 1984; Marsh 1987). Davis (1966) applied the reference-group perspective to the study of college students and examined the relationships between academic achievement, GPA and career choice. He argued that an undergraduate's career aspirations were based in part on his assessment of his academic performance relative to that of his peers, concluding with the saying: “It is better to be a big frog in a small pond than a small frog in a big pond.” Similarly, the big-fish-little-pond effect (Marsh 1987) proposed that a student will have a lower academic self-concept and lower educational occupational aspirations in an academically selective school than in a non-selective school. Overall, this strand of literature points out that students form self-concepts about their academic abilities by a social comparison process, particularly with classmates. This might provide some insight on a potential mechanism driving the effects I found.

6.2. **Policy Considerations**

Although a merit-based student mechanism in upper-secondary education in Mexico City may be an efficient way to assign students to schools, in terms of costs and logistics, the negative effects of admission to a more competitive school on graduation outcomes for the marginal student calls into question the strategies implemented to keep on track and retain the lowest-ranking students within each school. Most of the current programs aimed at decreasing upper-secondary dropout in Mexico are designed to provide financial aid or economic incentives directly to students (like Oportunidades, Probems, and Prepa Si). Few and isolated efforts are focused on the supply side, such as providing counselling, tutoring and remedial education for those students who are falling behind. For instance, a school's resources that are spent on students who take longer to graduate may be better used in early programs to prevent grade retention. Recognizing that the most vulnerable students at the bottom of the ability distribution for a given school, particularly among the most selective schools, are at greater risk of falling behind and not graduating is an initial step to design better programs that target and support these groups.

Marginal students to a more competitive school can be divided into three groups: those who graduate on time, those who take more than 3 years to graduate but eventually do, and those who never graduate. Findings from earlier research suggest that there may be positive gains on achievement and subsequent educational or labor outcomes from admission to a more selective school (Dobbie & Fryer, 2014; Estrada & Gignoux, 2014; de Janvry et al., 2013; Jackson, 2010). In some cases, these gains may compensate for the negative effects of not graduating on time (i.e. having been held back) from a selective or elite school since, in the end, these students successfully receive a regular upper-secondary diploma from a selective school. However, the trade-off is not so clear for those who attend non-selective schools or those who attend a selective school but never graduate. In terms of the labor market, the most pervasive effects may be for the marginal applicants who, despite having high entrance scores and attending a selective school, never graduate from upper-secondary education. These school dropouts are the ones who in the long-term would be more affected, in terms of labor income, from the negative effect of scoring just above the admission threshold to a more competitive school and not graduating.

My findings show that there are some students with relatively high admission scores, and who are barely admitted into selective schools, that are facing difficulties graduating at all. Thus, the upper-secondary education system is losing high-ability students that could have probably graduated from a less competitive school. It would be desirable to design programs to prevent high-ability students from dropping out and which facilitates transfer to other schools without requiring them to re-take the entrance examination and repeat all the courses. In settings with imperfect information like this, allowing students to make post-enrollment transfers in order to complete their studies could alleviate the risks of receiving poor school matches and reduce the probability of dropping out of the system. Recent efforts in Mexico have focused on designing protocols to facilitate the transfer of academic credits between institutions, but there are still many challenges for those who want to switch to another school.

7. Conclusion

In this paper, I explore whether it matters to applicants' subsequent educational outcomes to which school they are offered admission to, and they enroll in. This is a difficult question to answer mainly because students self-select into schools of different characteristics. I address this obstacle by exploiting the student-allocation mechanism implemented in upper-secondary education in Mexico City. This mechanism creates a series of regression-discontinuity quasi-experiments that allow me to examine the average effects, for the marginal applicant, of the offer of admission, and enrollment in, a more competitive school. It also enables me to explore the heterogeneity in effects for different subgroups and at different points of the admission-score distribution.

I find that being offered admission to a more-preferred and higher-achieving upper-secondary school in Mexico City decreases the probability of graduating on time and of graduating within five years for individuals at the margins of being offered a seat at an upper-secondary public school in Mexico City, in the designated academic years. This suggests that applicants, who were otherwise equal in expectation at the end of 9^{th} grade, had statistically-different graduation outcomes in upper-secondary education as a result of admission to different schools. I show that the estimates for the impact of enrollment are very similar given the high compliance rate. In addition, my results indicate that the negative effect of scoring just above the threshold is larger for the marginal applicants who come from more vulnerable contexts (i.e. low-SES, public lower-secondary school, lower GPA) and are offered admission to a selective school (i.e. top quartile of the school cut-off distribution). The negative impact of being among the worst-performing students relative to the other peers in the class could operate through different channels. However, at this point I do not have enough information to explore the channels that are driving these effects. Further research is needed in this area.

While the magnitude and direction of the effects presented here may be particular to the case of Mexico City, additional work is needed to examine whether these negative effects exist in settings with different student allocation systems and educational institutions. Future work in this particular context might also undertake an examination of whether admission to a more competitive school has effects on longer-term outcomes, such as college attendance and labor income.

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Figure 1. Scatterplot of the bivariate relationship between school's cut-off score (School cut-off score) and the average score of applicants offered admission to that particular school (Peer mean score). Restricted to oversubscribed schools in the analytic sample.

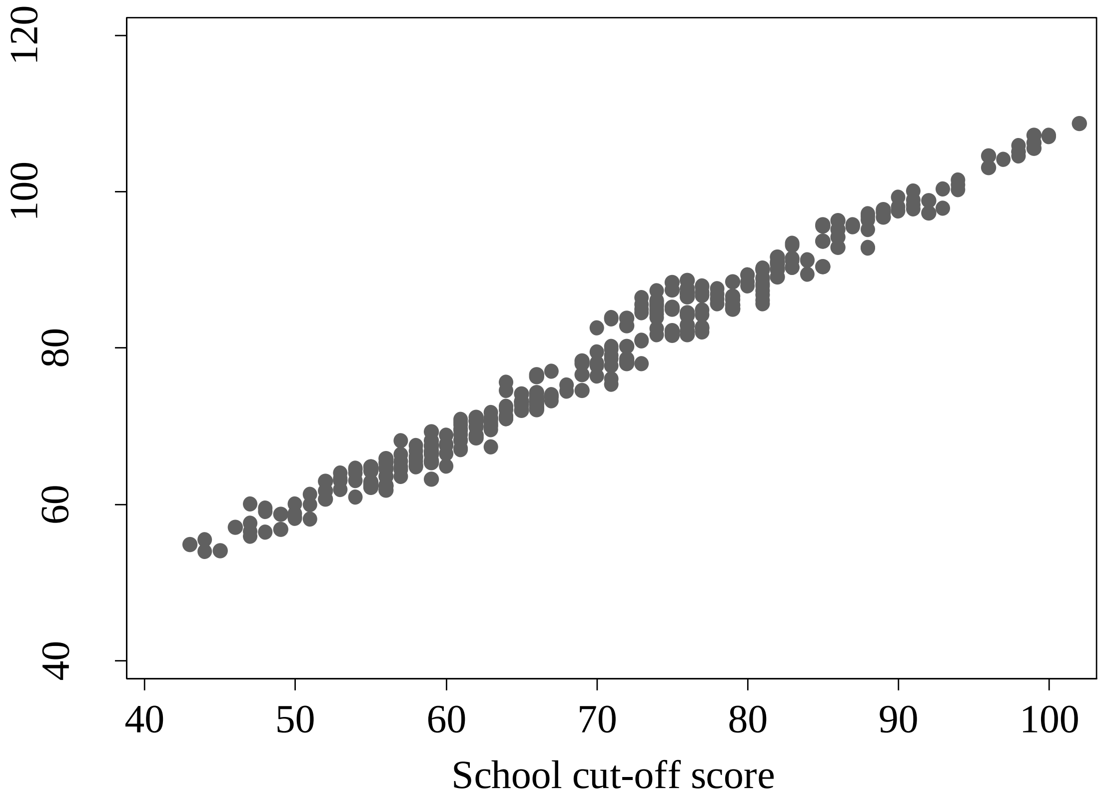
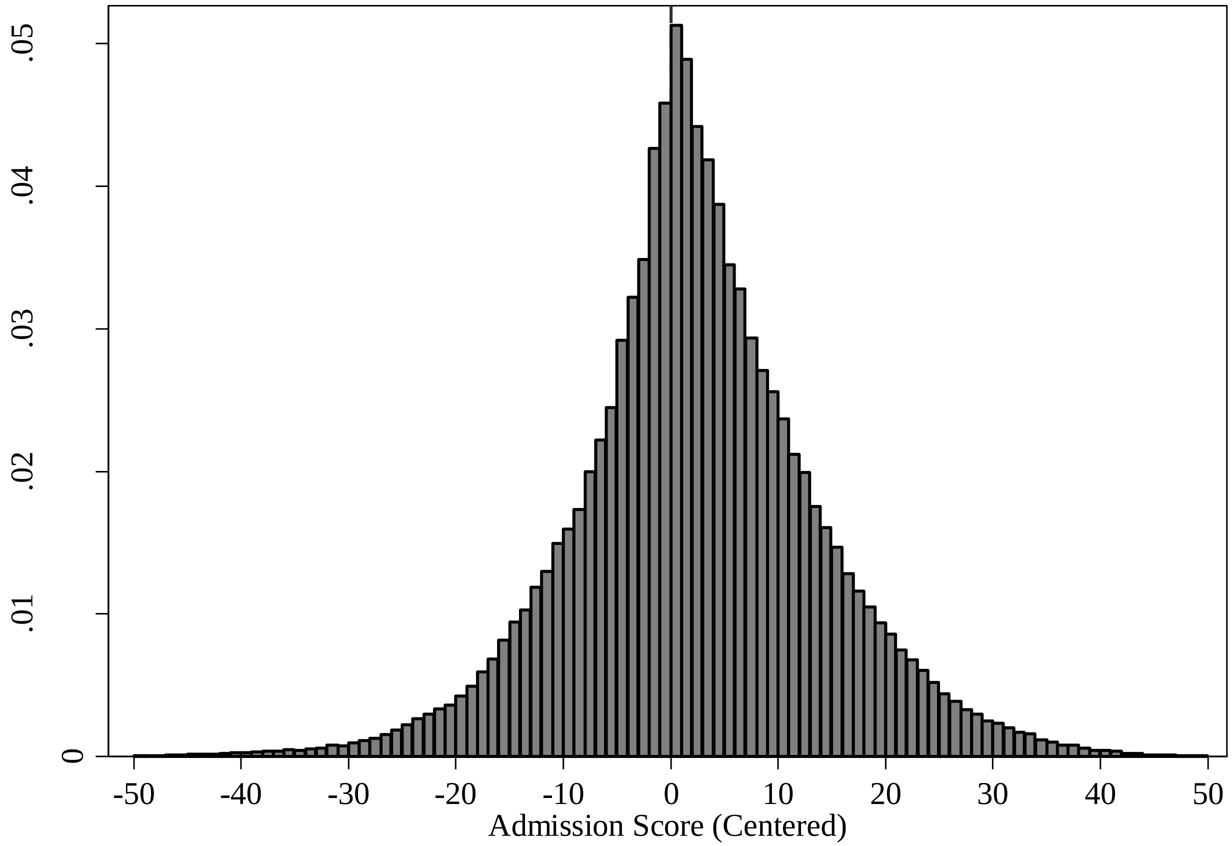


Figure 2. Sample distribution of the admission-score forcing variable relative to the admissions cut-off for a more competitive school.



Note: The score of each applicant is re-centered relative to the relevant threshold. Sharp sample for oversubscribed schools for which it was not necessary to specify a specialization field are pooled together.

Figure 2. Sample bivariate "reduced-form" relationship between the outcomes of interest GRADONTIME and GRAD and the forcing variable, SCOREc, displaying the observed discontinuity in each outcome, centered on the cut-off score (n=505,015)

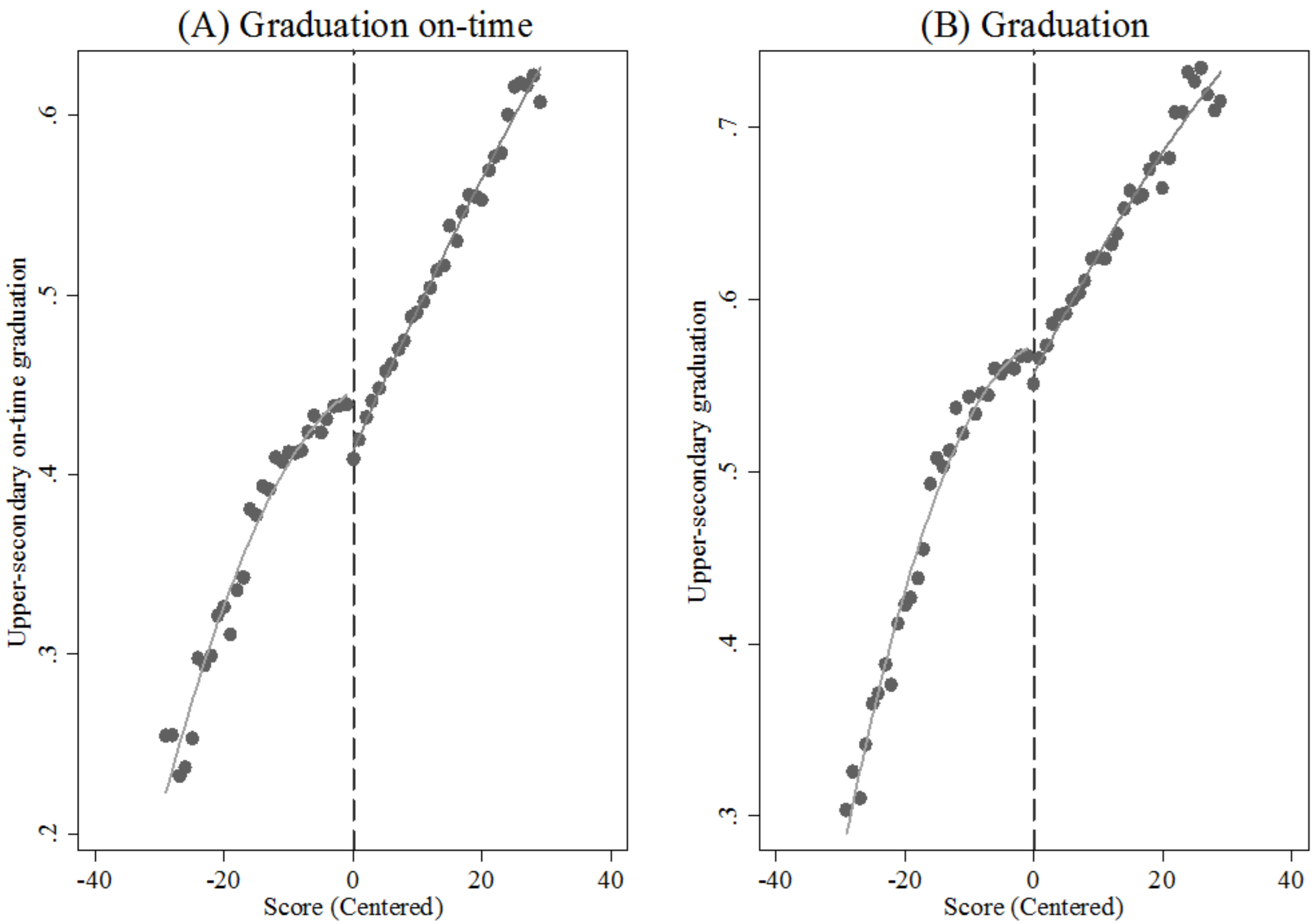


Figure 3. Sample bivariate “reduced-form” relationship between the outcomes of interest GRADONTIME and GRAD and the forcing variable, SCOREc, displaying the observed discontinuity in each outcome, centered on the cut-off score, within a 9 points bandwidth around the respective cut-off (n=271,655)

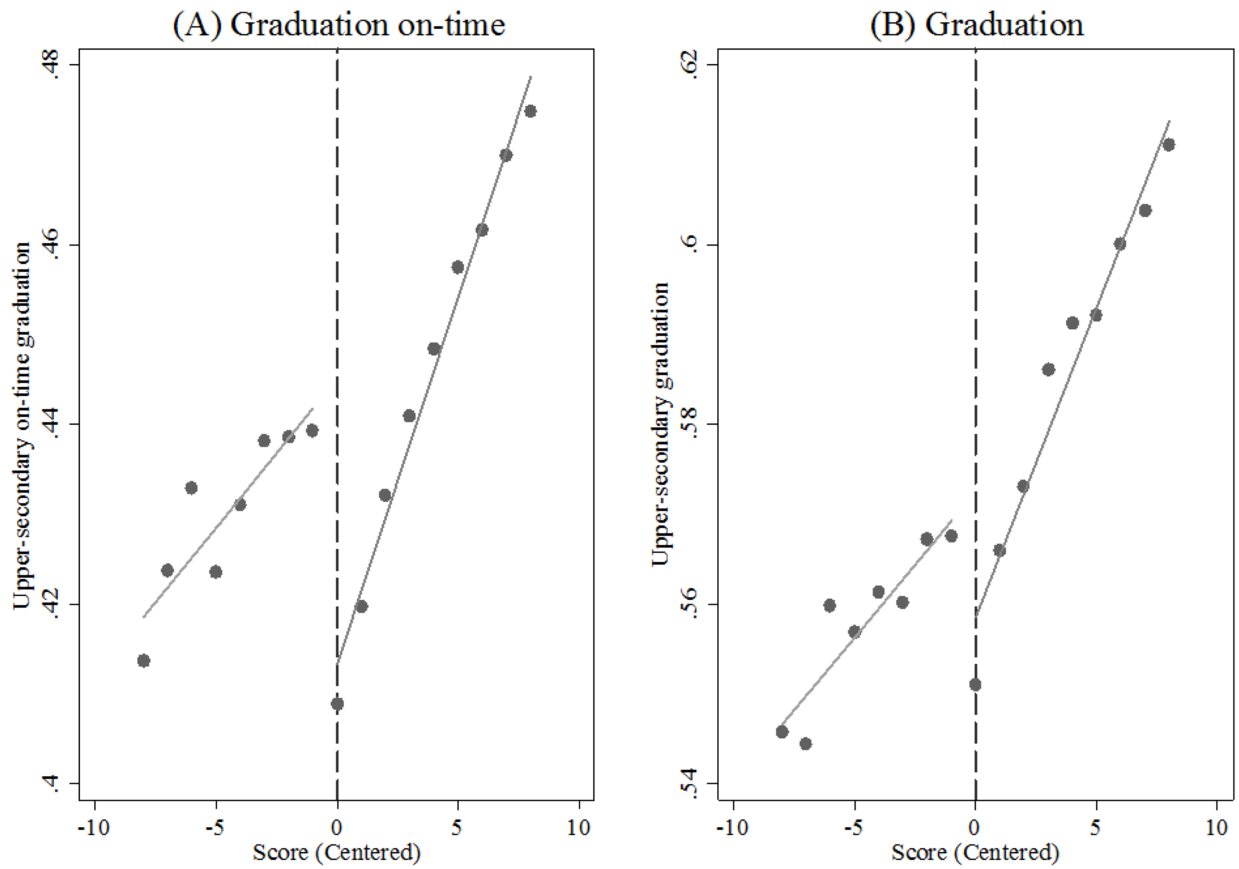


Figure 4. Enrollment in more-competitive oversubscribed school as a function of the re-centered admission score, in the analytic sample. (n=505,015)

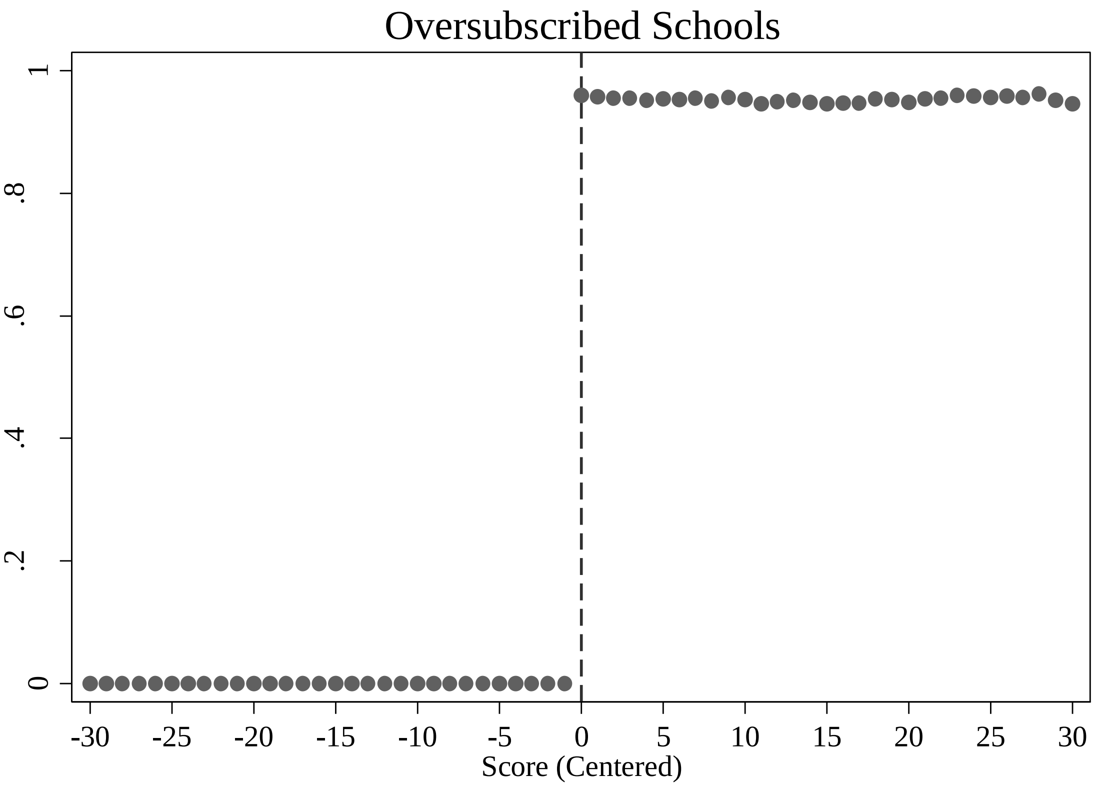


Table 1. Sample means (and standard deviations) describing applicants' selected characteristics, in different samples

	Sample			
	Full	Restricted	Analytic	Discontinuity
Panel A. Demographic and Socio-economic characteristics, at baseline				
Male	0.503	0.502	0.495	0.483
	(0.500)	(0.500)	(0.500)	(0.500)
Age (years)	15.636	15.315	15.203	15.193
	(1.583)	(0.923)	(0.720)	(0.705)
Indigenous	0.031	0.017	0.020	0.019
	(0.174)	(0.131)	(0.141)	(0.137)
Resident of Mexico City	0.776	0.891	0.915	0.920
	(0.417)	(0.312)	(0.225)	(0.219)
GPA in lower-secondary (out of 10)	7.212	8.332	8.379	8.391
	(2.518)	(0.848)	(0.837)	(0.815)
Private lower-secondary school	0.065	0.118	0.119	0.121
	(0.246)	(0.323)	(0.323)	(0.326)
Working	0.059	0.030	0.034	0.033
	(0.236)	(0.170)	(0.182)	(0.178)
Schooling of household head (years)	9.499	10.786	10.786	10.896
	(4.031)	(4.150)	(4.150)	(4.121)
SES index	0.004	0.397	0.430	0.471
	(1.575)	(1.597)	(1.693)	(1.687)
Low-SES family	0.551	0.436	0.440	0.429
	(0.497)	(0.496)	(0.496)	(0.495)
Oportunidades recipient	0.025	0.015	0.017	0.016
	(0.156)	(0.121)	(0.130)	0.126
Panel B. School preferences, at baseline				
Number options listed	9.283	9.935	9.996	10.295
	(3.712)	(3.817)	(3.800)	(3.809)
Rank of assigned option	3.373	3.580	3.557	3.659
	(2.972)	(2.888)	(2.872)	(2.822)
Score on entrance test	61.937	77.841	78.292	78.420
	(20.652)	(15.106)	(15.077)	(12.002)
Panel C. Graduation Outcomes				
On-time graduation (3 years)	0.423	0.451	0.468	0.454
	(0.494)	(0.498)	(0.499)	(0.498)
Graduation (5 years)	0.493	0.552	0.570	0.562
	(0.500)	(0.497)	(0.495)	(0.496)
Observations	*1,503,782*	*588,622*	*505,015*	*271,655*

Notes: The analytic and discontinuity sample include observations with no missing covariates. The discontinuity sample considers a bandwidth of 9 points. Some characteristics have missing values in the full and restricted sample.

Table 2. Estimated average difference in selected background characteristics (with the respective robust standard errors of the difference, in parentheses), at the cut-off used to determine the offer of admission.

	Bandwidth				
	3	6	9	12	15
Male	0.0011	-0.0006	-0.0021	-0.0022	-0.0030
	(0.0086)	(0.0047)	(0.0037)	(0.0032)	(0.0029)
Age	0.0091	0.0002	-0.0006	0.0002	0.0005
	(0.0120)	(0.0065)	(0.0053)	(0.0046)	(0.0042)
Indigenous	0.0014	-0.0007	-0.0007	-0.0005	-0.0000
	(0.0024)	(0.0013)	(0.0010)	(0.0009)	(0.0008)
GPA lower-secondary	0.0030	-0.0017	0.0049	0.0051	0.0043
	(0.0132)	(0.0072)	(0.0058)	(0.0050)	(0.0045)
Work	0.0012	0.0005	-0.0003	-0.0003	-0.0006
	(0.0031)	(0.0017)	(0.0013)	(0.0012)	(0.0010)
Private lower-secondary school	-0.0073	-0.0060*	-0.0041*	-0.0028	-0.0013
	(0.0054)	(0.0030)	(0.0024)	(0.0021)	(0.0019)
HH's schooling	-0.0776	-0.0797**	-0.0767**	-0.0727***	-0.0612***
	(0.0690)	(0.0377)	(0.0300)	(0.0260)	(0.0235)
SES index	-0.0358	-0.0143	-0.0188	-0.0191*	-0.0170*
	(0.0288)	(0.0157)	(0.0125)	(0.0108)	(0.0098)
Oportunidades	0.0013	0.0001	-0.0000	0.0002	0.0004
	(0.0021)	(0.0011)	(0.0009)	(0.0008)	(0.0007)
Resident	0.0043	0.0036*	0.0029	0.0020	0.0018
	(0.0038)	(0.0021)	(0.0018)	(0.0014)	(0.0013)
Observations	105,343	201,068	271,655	325,481	365,684

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors adjusting for clustering of students. Parameter estimates were obtained with a local-linear regression using an edge kernel with the listed bandwidth, and no additional covariates included. All models included school-cohort fixed effects.

Table 3. Estimated parameter estimates (with approximate *p*-values and associated robust standard error) summarizing the effect of an offer of admission to a more competitive school on the outcomes of interest, by the value of the forcing variable, without and with covariates

	Bandwidth				
	3	6	9	12	15
Panel A: On-time graduation, No controls					
OFFER	-0.0300***	-0.0324***	-0.0296***	-0.0290***	-0.0280***
	(0.0086)	(0.0047)	(0.0037)	(0.0032)	(0.0029)
OFFER×SCOREc	0.0101*	0.0064***	0.0050***	0.0041***	0.0038***
	(0.0056)	(0.0017)	(0.0010)	(0.0007)	(0.0005)
R-2 statistic	0.078	0.060	0.055	0.054	0.053
Observations	105,343	201,068	271,655	325,481	365,684
Panel B: On-time graduation, With controls					
OFFER	-0.0306***	-0.0321***	-0.0309***	-0.0302***	-0.0290***
	(0.0080)	(0.0043)	(0.0034)	(0.0030)	(0.0027)
OFFER×SCOREc	0.0107**	0.0079***	0.0061***	0.0055***	0.0052***
	(0.0052)	(0.0015)	(0.0009)	(0.0006)	(0.0005)
R-2 statistic	0.219	0.202	0.198	0.196	0.196
Observations	105,343	201,068	271,655	325,481	365,684
Panel C: Graduation, No controls					
OFFER	-0.0156	-0.0126**	-0.0095**	-0.0088**	-0.0081**
	(0.0112)	(0.0061)	(0.0049)	(0.0042)	(0.0038)
OFFER×SCOREc	0.0064	0.0054**	0.0033***	0.0024***	0.0018***
	(0.0073)	(0.0021)	(0.0012)	(0.0009)	(0.0007)
R-2 statistic	0.085	0.068	0.063	0.061	0.061
Observations	61,312	117,565	158,005	188,611	211,107
Panel D: Graduation, With controls					
OFFER	-0.016	-0.0124**	-0.0109**	-0.0105***	-0.0099***
	(0.0106)	(0.0058)	(0.0046)	(0.0040)	(0.0036)
OFFER×SCOREc	0.0067	0.0065***	0.0044***	0.0035***	0.0030***
	(0.0069)	(0.0020)	(0.0012)	(0.0008)	(0.0006)
R-2 statistic	0.188	0.171	0.167	0.166	0.166
Observations	61,312	117,565	158,005	188,611	211,107

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors adjusted for the clustering of students. Parameter estimates were obtained with a local-linear regression analysis using an edge kernel with the listed bandwidth. All models included school-fixed effects and an intercept. Additional controls included: male, age, indigenous, lower-secondary GPA, private lower-secondary school, working status, years of education of the household head, SES index, living in the metropolitan area of Mexico City, and receiving Oportunidades-Progresa (CCT).

Table 4. Parameter estimates (with approximate *p*-values and standard errors) summarizing the fitted probability of graduating and graduating on-time as a function of the offer of admission and the forcing variable, for selected non-linear specifications of the forcing variable, with controls.

	Specification of the Forcing Variable			
	(1) Linear	(2) Quadratic	(3) Cubic	(4) Quartic
Panel A: On-Time Graduation, With Controls				
OFFER	-0.0309***	-0.0306***	-0.0248***	-0.0177
	(0.0034)	(0.0057)	(0.0095)	(0.0176)
OFFER×SCOREc	0.0061***	0.0120***	0.0199**	0.0266
	(0.0009)	(0.0034)	(0.0096)	(0.0247)
OFFER×SCOREc^2		-0.0003	0.0017	0.0096
		(0.0004)	(0.0027)	(0.0115)
OFFER×SCOREc^3			0.0002	0.0005
			(0.0002)	(0.0019)
OFFER×SCOREc^4				0.0001
				(0.0001)
Bandwidth	9	9	9	9
R-2 statistic	0.198	0.198	0.198	0.198
Observations	271,655	271,655	271,655	271,655
Panel B: Graduation, With Controls				
OFFER	-0.0109**	-0.0117	-0.0134	-0.0201
	(0.0046)	(0.0075)	(0.0125)	(0.0233)
OFFER×SCOREc	0.0044***	0.0110**	0.0111	-0.0048
	(0.0012)	(0.0045)	(0.0127)	(0.0329)
OFFER×SCOREc^2		-0.0005	-0.0015	-0.0039
		(0.0006)	(0.0036)	(0.0152)
OFFER×SCOREc^3			0.0000	-0.0017
			(0.0003)	(0.0025)
OFFER×SCOREc^4				-0.0000
				(0.0002)
Bandwidth	9	9	9	9
R-2 statistic	0.167	0.167	0.167	0.167
N	158,005	158,005	158,005	158,005

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors adjusted for the clustering of students. Parameter estimates were obtained with a local-linear regression analysis using an edge kernel with the listed bandwidth. All models included school-fixed effects and an intercept. Additional controls included: male, age, indigenous, lower-secondary GPA, private lower-secondary school, working status, years of education of the household head, SES index, living in the metropolitan area of Mexico City, and receiving Oportunidades-Progresa (CCT).

Table 5. Estimated parameters (with approximate *p*-values and robust standard errors, in parentheses) describing the first-stage effects of being offered admission to a more competitive school on the probability that a student enrolls, without and with covariates.

	Bandwidth				
	3	6	9	2	15
Panel A: Enrollment, No Controls					
OFFER	0.9613***	0.9601***	0.9595***	0.9591***	0.9590***
	(0.0015)	(0.0011)	(0.0009)	(0.0009)	(0.0008)
R-2 statistic	0.892	0.904	0.905	0.905	0.905
Observations	105,343	201,068	271,655	325,481	365,684
Panel B: Enrollment, With Controls					
OFFER	0.9605***	0.9595***	0.9591***	0.9588***	0.9588***
	(0.0015)	(0.0011)	(0.0009)	(0.0009)	(0.0008)
R-2 statistic	0.894	0.906	0.907	0.907	0.907
Observations	105,343	201,068	271,655	325,481	365,684

*Notes:**** p<0.01, ** p<0.05, * p<0.1. Robust standard errors adjusted for the clustering of students. Parameter estimates were obtained with a local-linear regression analysis using an edge kernel with the listed bandwidth. All models included school-fixed effects and an intercept. Additional controls included: male, age, indigenous, lower-secondary GPA, private lower-secondary school, working status, years of education of the household head, SES index, living in the metropolitan area of Mexico City, and receiving Oportunidades-Progresa (CCT). All the F-statistic associated with the excluded instrument, not reported here, have very large values (>50).

Table 6. Instrumental-Variables estimates of the causal effect of attending a more competitive school on the probability that a student, at the margins of admission, graduates within 3 or 5 years.

	Bandwidth				
	3	6	9	12	15
Panel A: On-time graduation, No controls					
ENROLL	-0.0312***	-0.0338***	-0.0309***	-0.0303***	-0.0292***
	(0.0089)	(0.0050)	(0.0041)	(0.0036)	(0.0033)
R-2 statistic	0.035	0.036	0.035	0.036	0.037
Observations	105,343	201,068	271,655	325,481	365,684
Panel B: On-time graduation, With controls					
ENROLL	-0.0319***	-0.0335***	-0.0322***	-0.0315***	-0.0303***
	(0.0083)	(0.0046)	(0.0038)	(0.0033)	(0.0031)
R-2 statistic	0.151	0.149	0.149	0.150	0.150
Observations	105,343	201,068	271,655	325,481	365,684
Panel C: Graduation, No controls					
ENROLL	-0.0162	-0.0131**	-0.0099*	-0.0092**	-0.0085**
	(0.0116)	(0.0065)	(0.0053)	(0.0047)	(0.0043)
R-2 statistic	0.021	0.022	0.023	0.021	0.022
Observations	61,312	117,565	158,005	188,611	211,107
Panel D: Graduation, With controls					
ENROLL	-0.0167	-0.0129**	-0.0118**	-0.0109**	-0.0103**
	(0.0110)	(0.0062)	(0.0050)	(0.0044)	(0.0041)
R-2 statistic	0.111	0.110	0.111	0.112	0.112
Observations	61,312	117,565	158,005	188,611	211,107

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors adjusting for clustering at the student level in parentheses. The R-squared correspond to the specifications with controls. Parameter estimates were obtained with a local linear regression using an edge kernel with the listed bandwidth. All models include school-fixed effects. Additional controls include: male, age, indigenous, lower-secondary GPA, private lower-secondary school, working status, years of schooling of the household head, SES index, and Oportunidades-Progresa (CCT).

Table 7a. Reduced-form estimates of the offer of admission on the outcomes of interest including interactions of the offer with selected demographic characteristics of the applicant, with a bandwidth of 9 points.

	Specification				
	(1)	(2)	(3)	(4)	(5)
Panel A: On-time graduation, With controls					
OFFER	-0.0227***	-0.0309***	-0.02649***	-0.0331***	-0.1163***
	(0.0038)	-0.0036	(0.0037)	(0.0035)	(0.0154)
MALE	-0.0115***				
	(0.0032)				
MALE×OFFER	-0.0172***				
	(0.0031)				
WORK		-0.0219***			
		(0.0025)			
WORK×OFFER		-0.0126			
		(0.0099)			
LOW_SES			-0.0031		
			(0.0031)		
LOW_SES×OFFER			-0.0107***		
			(0.0032)		
PRIV_SEC				-0.0515***	
				(0.0050)	
PRIV_SEC×OFFER				0.0188***	
				(0.0050)	
GPA					0.2309***
					(0.0019)
GPA×OFFER					0.0102***
					(0.0018)
Bandwidth	9	9	9	9	9
R-2 statistic	0.198	0.198	0.198	0.198	0.198
Observations	271,655	271,655	271,655	271,655	271,655

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors adjusting for clustering at the student level in parentheses. Parameter estimates were obtained with a local linear regression using an edge kernel with the listed bandwidth. All models include school-fixed effects. Additional controls include: male, age, indigenous, lower-secondary GPA, private lower-secondary school, working status, years of schooling of the household head, SES index, and Oportunidades-Progresa (CCT).

Table 7b. Reduced-form estimates of the offer of admission on the outcomes of interest including interactions of the offer with selected demographic characteristics of the applicant, with a bandwidth of 9 points.

	Specification				
	(1)	(2)	(3)	(4)	(5)
Panel B: Graduation, With controls					
OFFER	-0.0043	-0.0109***	-0.0041	-0.0144***	-0.0852***
	(0.0049)	(0.0046)	(0.0049)	(0.0046)	(0.0217)
MALE	-0.0036				
	(0.0042)				
MALE×OFFER	-0.0139***				
	(0.0041)				
WORK		-0.0499***			
		(0.0111)			
WORK×OFFER		0.0014			
		(0.0117)			
LOW_SES			-0.0022		
			(0.0041)		
LOW_SES×OFFER			-0.0153***		
			(0.0041)		
PRIV_SEC				-0.1057***	
				(0.0068)	
PRIV_SEC×OFFER				0.0312***	
				(0.0068)	
GPA					0.1845***
					(0.0026)
GPA×OFFER					0.0089***
					(0.0025)
Bandwidth	9	9	9	9	9
R-2 statistic	0.167	0.167	0.167	0.167	0.167
Observations	158,005	158,005	158,005	158,005	158,005

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors adjusting for clustering at the student level in parentheses. Parameter estimates were obtained with a local linear regression using an edge kernel with the listed bandwidth. All models include school-fixed effects. Additional controls include: male, age, indigenous, lower-secondary GPA, private lower-secondary school, working status, years of schooling of the household head, SES index, and Oportunidades-Progresa (CCT).

Table 8. Reduced-form estimates of the offer of admission on the outcomes according to the location cut-off in the admission of the school cut-off score distribution, with a bandwidth of 9 points.

	Specification			
	(1)	(2)	(3)	(4)
	Interquartile 1	Interquartile 2	Interquartile 3	Interquartile 4
Panel A: On-time graduation, With controls				
OFFER	-0.0006	0.0127	-0.0108**	-0.0754***
	(0.0075)	(0.0088)	(0.0054)	(0.0058)
Bandwidth	9	9	9	9
R-2 statistic	0.200	0.187	0.176	0.177
Observations	23,971	47,317	98,631	101,736
Panel B: Graduation, With controls				
OFFER	0.0011	0.0161*	-0.0161**	-0.0476***
	(0.0113)	(0.0096)	(0.0071)	(0.0076)
Bandwidth	9	9	9	9
R-2 statistic	0.160	0.147	0.152	0.159
Observations	10,397	33,723	56,986	56,899

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors adjusting for clustering at the student level in parentheses. Parameter estimates were obtained with a local linear regression using an edge kernel with the listed bandwidth. All models include school-fixed effects. Additional controls include: male, age, indigenous, lower-secondary GPA, private lower-secondary school, working status, years of schooling of the household head, SES index, living in the metropolitan area of Mexico City, and receiving Oportunidades-Progresa (CCT). The interquartile 1 the sharp samples of schools with cut-of score of less than 61, the interquartile 2 includes schools with cut-off score between 61 and 72, the interquartile 3 corresponds to schools with cut-off scores between 72 and 82, and the interquartile 4 include schools with cut-off scores above 82 points.

Table 9. Reduced-form estimates of the offer of admission on the outcomes according to the ranking of the offer, with a bandwidth of 9 points.

	Subsample		
	First-choice	Second-choice	Third-choice
Panel A: On-time graduation, No controls			
OFFER	-0.0595***	-0.0325***	-0.0206***
	(0.0066)	(0.0081)	(0.0772)
OFFER×SCORE	0.0061***	0.0119	0.0112
	(0.0016)	(0.0185)	(0.0192)
R-2 statistic	0.082	0.125	0.178
Observations	110,309	34,490	14,368
Panel B: On-time graduation, With controls			
OFFER	-0.0562***	-0.0234*	0.0437
	(0.0060)	(0.0622)	(0.0786)
OFFER×SCORE	0.0068***	0.0053	0.0159
	(0.0015)	(0.0187)	(0.0191)
R-2 statistic	0.234	0.264	0.307
Observations	110,309	34,490	14,368
Panel C: Graduation, No controls			
OFFER	-0.0356***	-0.0264**	-0.0183**
	(0.0082)	(0.0189)	(0.0086)
OFFER×SCORE	0.0040**	-0.0013	0.0186
	(0.0020)	(0.0252)	(0.0256)
R-2 statistic	0.097	0.134	0.192
Observations	65,755	20,452	8,618
Panel D: Graduation, With controls			
OFFER	-0.0323***	-0.0139	0.0034
	(0.0077)	(0.0862)	(0.1052)
OFFER×SCORE	0.0051***	-0.0023	0.0167
	(0.0019)	(0.0244)	(0.0247)
R-2 statistic	0.205	0.229	0.287
Observations	65,755	20,452	8,618

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors adjusting for clustering at the student level in parentheses. The R-squared correspond to the specifications with controls. Parameter estimates were obtained with a local linear regression using an edge kernel with the listed bandwidth. All models include school-fixed effects and a constant. Additional controls include: male, age, indigenous, lower-secondary GPA, private lower-secondary school, working status, years of education of the household head, SES index, living in the metropolitan area of Mexico City, and receiving Oportunidades-Progresa (CCT).

Appendix

Appendix A. Subsystems and Institutions that integrate the COMIPEMS

Table A.1. Subsystems and Institutions that participate in COMIPEMS

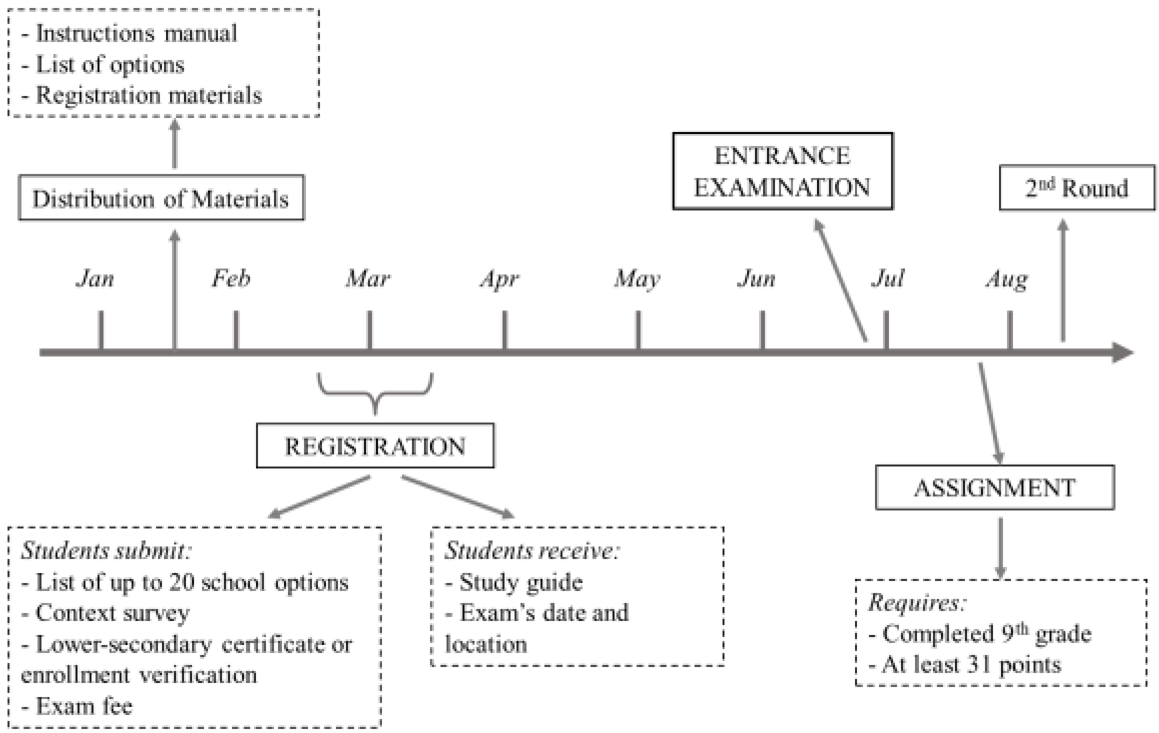
Model	Subsystem		Institutions
General	UNAM	Universidad Nacional Autonoma de Mexico	CCH, ENP
	UAEM	Universidad Autónoma del Estado de Mexico	Texcoco
	COLBACH	Colegio de Bachilleres	COLBACH
	DGB	Dirección General de Bachillerato	CEB
	SE	Secretaría de Educación del Estado de México	COBAEM
Technical	CONALEP	Colegio Nacional de Educación Profesional Técnica	DF, State of Mexico
Technological (General + Technical)	SE	Secretaría de Educación del Estado de México	CECYTEM
	DGETI	Dirección de Educación Tecnológica Industrial	CBTIS, CETIS
	DGETA	Dirección de Educación Tecnológica Agropecuaria	CBTA
	IPN	Instituto Politécnico Nacional	CET, CECYT

The main characteristics of each subsystem:

- *General:* prepares students with general knowledge to continue studying for post-secondary education. It is offered in three-year programs. Graduates receive a certificate that is necessary to enter higher education. There are several modalities: standard, open and distance, and mixed.
- *Technological:* covers the curriculum of general education to prepare students for post-secondary education. In addition, it gives students the opportunity to receive vocational training. This bivalent character allows graduates to continue into higher education and gives them also a technical diploma at a semi-professional level. Programs last three years.
- *Technical:* trains students to become professional technicians and gives them the option to continue into tertiary education. Programs are completed in three years.

Appendix B: **The COMIPEMS Admission Process**

B.1. Timeline



B.2. Registration Materials

The registration materials that are distributed to individuals who want to attend a public upper-secondary school in Mexico City include:

a) A manual with a detailed explanation of the process and the directory of available school options, including each option's cut-off scores from previous years.

b) A registration form to fill with personal information and the list of up to 20 preferred school options they were willing to attend, ordered by preference.

c) A context survey seeking information on the students' demographic, academic and socio-economic characteristics.

d) A bank deposit slip to pay the fee for sitting the standardized entrance examination (approximately 25 U.S. dollars).

C. Construction of Sharp Samples

In the setting under analysis, applicants to public upper-secondary schools are ranked based on their score on an entrance examination. Applicants may qualify for schools (i.e. be scored above the respective school's cut-off score) that they listed among their preferences, but not be offered admission if they have already received an offer of admission at a more preferred school.

According to Abdulkadiroğlu et al. (2014), the "sharp sample" for school S is the sample for whom offers of admission are sharp in the sense of being linked deterministically with S's forcing variable. Thus, the sharp sample for school S implies the union of different sets of applicants, those who ranked S and qualified, and those who ranked S and did not qualify.

In this particular case, considering the application risk sets, I constructed a "sharp sample" for each oversubscribed school and included fixed effects for each pair of schools combination in a given cohort (i.e. School-School-Cohort fixed effects), not necessarily the same preference set. For example, I grouped applicants that prefer A over B but some scored just above the cut-off for A and some scored just below the cut-off for A and got assigned to B.

Below, I explain the procedure I followed to construct the sharp samples, using a simple example.

Assume that, in 2006, with 3 applicants and 3 schools:

- Applicant 1 ranks schools in descending order: A, B, C
- Applicant 2 ranks schools in descending order: A, B, C
- Applicant 3 ranks schools in descending order: B, A, C

where cut-off(A)> cut-off(B) > cut-off(C).

Imagine that Applicant 1 is offered admission to school A, Applicant 2 is offered admission to B and Applicant 3 cleared the admission cut-off score for school C. Thus, all students prefer schools A and B over school C, but Applicant 3 prefers School B over A, instead of A over B. Then, given how the assignment algorithm works, it is not possible that Applicant 3 receives an offer of admission to School A after being rejected from B, because cut-off(A)>cut-off(B). If he gets rejected from School B, he will then be considered for School C, which is his next most-preferred school with available seats.

In this scenario, School B is the next most-preferred option to which Applicant 1 would qualify if rejected from School A. Thus, Applicant 1 will be included in the sharp sample for School A with a positive centered-score ($SCORE_1$-cut-off(A)>0). To control for the applicant's preference ordering, I include as a covariate in my regression models a dichotomous predictor to represent the applicant's School-School-Cohort combination A_B_2006, which indicates that for Applicant 1, School A is preferable to B in 2006. Similarly, School C will be the next most-preferred option to which Applicant 2 would qualify, so he will be considered in the sharp sample for School B with a positive centered-score ($SCORE_2$-cut-off(B)>0), and will have a corresponding value for the dichotomous covariate B_C_2006.

In addition, School B is the "most informative qualification" for Applicant 3. That is, the school with the lowest cut-off score that rejected him or the more-preferred to which he would have been more likely to be admitted. Then, Applicant 3 will be included in the sharp sample for School B with a negative centered-score ($SCORE_3$-cut-off(B)<0), and will include a corresponding value for the dichotomous covariate B_C_2006.[50] Analogously, School A is the most informative qualification for Applicant 2 so he will be included in the School A sharp sample with a negative centered-score ($SCORE_2$-cut-off(A)<0), and a corresponding value for the dichotomous covariate A_B_2006.

To summarize:[51]

- *Applicant 1* is included in the sharp sample for School A with SCOREc>0;
  applicant's preference is reflected in a value of 1 on the dichotomous covariate: A_B_2006; 0 otherwise.
- *Applicant 2* is included in the sharp sample for school A with SCOREc<0;
  applicant's preference is reflected in a value of 1 on the dichotomous covariate: A_B_2006; 0 otherwise.
- *Applicant 2* is also included in the sharp sample for school B with SCOREc>0;
  applicant's preference is reflected in a value of 1 on the dichotomous covariate: B_C_2006; 0 otherwise.
- *Applicant 3* is in the sharp sample for school B with SCOREc<0:
  applicant's preference is reflected in a value of 1 on the dichotomous covariate: B_C_2006; 0 otherwise.

As I mentioned, I follow the approach used in Pop-Eleches and Urquiola (2013) and combine the analysis of all discontinuities into a single analysis. Thus, I pooled all sharp samples into the analysis of a single discontinuity, and included suitable fixed effects to describe the combination of cohort and application risk sets (i.e. school-school-cohort).

Finally, since an applicant can be present in more than one sharp sample, I adjust the obtained standard errors for the clustering of records within the individual.

[50] As I mentioned before, although student 3 could be included in the sharp sample for A, there would be no applicant with the school-school-cohort fixed effect B_A_2006 that would be assigned to school A. Therefore, by construction, a more competitive school (i.e. higher cut-off score) is always a more preferred school with higher-achieving peers.
[51] Another approach is to include each applicant in the sharp sample of each school that they ranked. However, the strategy I follow is a more refined way to construct the sharp samples while controlling for the applicant's preference ordering. Ultimately, my analyses are restricted to individuals around the cut-off so those applicants that score away from the cut-off for a particular school are unlikely to change the results.

D. Definition and coding description of the variables

Table D.1: Definition and coding of the variables for the statistical analysis

Variable	Definition	Values
SCORE	Score on the standardized entrance examination	0 to 128
SCOREc	Distance of the score with respect to the cut-off (centered at 0)	-60 to 60
OFFER	Applicant was offered admission to a more competitive school in the sharp sample	yes=1, no=0
ATTEND	Applicant attended his or her assigned school	yes=1, no=0
GRADONTIME	Applicant graduated from upper-secondary education within 3 year of the offer of admission	yes=1, no=0
MALE	Applicant is a male	yes=1, no=0
INDIGENOUS	Applicant reported speaking an indigenous language	yes=1, no=0
AGE	Age of applicant at baseline	14 to 25
WORK	Applicant was working for a wage at baseline	yes=1, no=0
HH_SCHOOLING	Years of education for the highest level of education completed by the household head at baseline	no education=0, primary=6, lower-secondary=9, upper-secondary=12, university=17, graduate=20
GPA	GPA obtained in lower-secondary education	6 to 10
RESIDENT	Applicant lives in the metropolitan area of Mexico City	yes=1, no=0
SES_INDEX	Principal components index of access to durables goods (like car, computer, dvd) and services (like sewage, telephone, internet) at home	-6 to 6
LOW_SES	Applicant has SES below the median	yes=1, no=0
OPORTUNIDADES	Applicant's family receivee the Oportunidades-Progresa conditional cash transfer	yes=1, no=0
PRIVATE_LOWERSEC	Applicant attended a private lower-secondary school	yes=1, no=0