

Time Varying Effects of Elite Schools: Evidence from Mexico City ^{*}

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Abstract

We examine whether the academic effects of marginal admission to elite science high schools vary by admission year, reflecting changes in school quality over time. Using administrative data from Mexico City’s centralized high school admission system between 2005 and 2009, we estimate year-specific regression discontinuity designs. We find that the effect on end-of-high-school math test scores declines steadily over the period—positive and significant in 2005, but statistically insignificant by 2009. This decline is not explained by changes in peer quality or other observable school inputs, which remain stable. Instead, we document a reduction in the value-added of elite schools, suggesting that changes in the productivity of school inputs drive the trend. Our findings highlight the time-specific nature of treatment effects and the limits of external validity, even in internally valid designs.

Keywords: School choice, Upper-secondary education, Education policy.

JEL codes: I21, I24, I28, J24.

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1 Introduction

In centralized education systems, high-performing schools—hereafter elite schools—attract a considerable share of applicants. These students often expect that elite schools’ advantages, such as better peers, superior infrastructure, and more qualified teachers, will translate into improved academic outcomes and, ultimately, better outcomes overall. Yet whether these expected benefits actually materialize—and under what conditions—remains unclear.

Recent research increasingly recognizes that the effects of elite schools may be heterogeneous. These studies have produced a range of estimates, reflecting differences in institutional context, student composition, and policy environment (e.g., [Pop-Eleches and Urquiola \(2013\)](#); [Abdulkadiroğlu et al. \(2014\)](#); [Dobbie and Fryer Jr \(2014\)](#); [De Groote and Declercq \(2021\)](#); [Beuermann and Jackson \(2022\)](#)). The literature recognizes that changes in school characteristics or in how those characteristics affect student outcomes can limit the generalizability of even internally valid causal estimates across periods or policy settings. For example, a policymaker considering whether to expand elite school capacity may find that past estimates reflect conditions or mechanisms that no longer apply.

This paper systematically examines whether—and why—the estimated effects of elite schools on academic outcomes vary across time. We study a fixed set of elite schools in Mexico City between 2005 and 2009, leveraging rich administrative and school census data from the city’s centralized high school admission system. By combining a sharp regression discontinuity design (RDD) with year-specific estimates, we trace changes in school effects over time and explore potential mechanisms underlying these trends. Our findings raise questions about the external validity of causal estimates in education research when time heterogeneity is not taken into account.

Estimating the impact of elite schools is complicated by the fact that admission is non-random: students who attend elite schools may differ systematically from those who do not. A growing literature addresses this selection problem using RDDs that exploit centralized admissions systems, where oversubscribed schools generate score-based cutoffs that effectively randomize access among students at the margin. Comparing outcomes of marginally admitted and rejected students yields credible causal estimates—though typically only near

the cutoff and for a specific cohort.

Yet many existing studies pool multiple cohorts to improve precision, often treating school effects as stable over time. However, if the relative quality of elite and non-elite schools shifts from year to year, such pooling may obscure meaningful variation and limit the relevance of findings for current policy decisions.

In particular, we examine the case of Mexico City’s centralized high school admission system from 2005 to 2009. Each year, approximately 250,000 students participate in a centralized matching process that assigns them to public high schools based on their preferences and a standardized admission exam score. The system’s structure and stability across years provides a rare opportunity to estimate year-specific school effects within a consistent institutional framework.

Our dataset combines three key administrative sources. First, we use application and placement records to identify students’ admission outcomes. Second, we track academic performance using standardized exit exam scores taken in the final quarter of high school. Third, we use yearly school census data to measure a wide range of school inputs, including peer composition, teacher qualifications, and infrastructure indicators. This rich, panel-like dataset allows us to estimate the impact of elite school admission on test scores and to investigate mechanisms driving any changes over time.

Three features of this setting enhance our empirical strategy. First, the centralized assignment process generates score-based cutoffs for oversubscribed elite schools, enabling a regression discontinuity design. Second, the large number of applicants allows us to estimate separate effects for each cohort with sufficient precision. Third, the availability of yearly input data enables us to explore how school-level changes correlate with the evolution of treatment effects.

Our identification strategy builds on the use of Regression Discontinuity Designs (RDDs) in centralized school admission systems. Oversubscribed elite schools set implicit cutoffs based on students’ composite scores, leading to clear discontinuities in the likelihood of admission. Students just above and just below each cutoff are plausibly similar in all respects except for school placement, allowing us to isolate the causal effect of elite school attendance for marginal applicants.

Rather than pooling cohorts to obtain a single average effect, we estimate the RDD separately for each admission year from 2005 to 2009. This approach allows us to track whether and how the effects of elite school admission evolve over time. Given that the assignment mechanism and institutional setting remain stable during this period, any changes in estimated effects are likely attributable to shifts in school quality or in how school characteristics translate into academic outcomes.

We focus on end-of-high school math test scores as our primary outcome, complemented by secondary analysis of dropout rates. Our analysis reveals that the academic benefits of elite school admission vary substantially across cohorts. The effect of marginal admission to an elite high school on students' math test scores is positive and statistically significant in 2005 but declines steadily, becoming statistically indistinguishable from zero by 2009. In contrast, the probability of dropping out increases for marginally admitted students and remains relatively constant across years. This stability in dropout effects helps rule out compositional changes as the primary driver of declining test score impacts.

To understand these trends, we examine whether changes in peer quality or observable school inputs explain the time pattern. We find that elite schools continue to offer consistently better observable inputs, but the magnitude of these differences does not change substantially over time. Instead, most of the decline in the impact of elite schools appears to stem from a reduction in how effectively those inputs translate into academic gains, potentially reflecting shifts in unobserved school characteristics. One plausible explanation is a major curricular alignment reform during our study period, which may have reduced the comparative advantage of elite schools by standardizing academic content across institutions.

Our work contributes to two main strands of the literature. First, it contributes to the extensive literature that studies the academic effects of elite/selective schools.¹ Our analysis extends this research by providing year-specific estimates across five consecutive cohorts, revealing substantial within-market variation over time. This evidence complements prior work, which often attributes heterogeneity in elite school effects to differences across markets

¹See [Clark \(2010\)](#); [Jackson \(2010\)](#); [Pop-Eleches and Urquiola \(2013\)](#); [Abdulkadiroğlu et al. \(2014\)](#); [Dobbie and Fryer Jr \(2014\)](#); [Lucas and Mbiti \(2014\)](#); [Abdulkadiroğlu et al. \(2017\)](#); [Dustan et al. \(2017\)](#); [Beuermann and Jackson \(2022\)](#); [Angrist et al. \(2023\)](#).

or institutional settings. Our results suggest that similar variation can arise within a single market as conditions evolve.

In the context of Mexico City, for example, [Dustan et al. \(2017\)](#) report large, positive, and statistically significant effects on math test scores for students marginally admitted to elite science schools based on two cohorts. Our analysis complements this work, by showing that the earlier positive effects are not stable: the impact of elite school admission declines and becomes statistically insignificant over time.

Second, we contribute to the literature on education production functions. As [Todd and Wolpin \(2003\)](#) highlight, policy effects such as those obtained using RDDs do not necessarily estimate production function parameters. Understanding whether and why elite school admission improves academic outcomes requires examining not just whether there is an effect, but also how school inputs interact to produce it. For example, some prior studies interpret the effect of marginal admission as primarily driven by improved peer quality (e.g., [Jackson, 2010](#); [Abdulkadiroğlu et al., 2014](#); [Dobbie and Fryer Jr, 2014](#)). However, in our setting, we observe a constant peer quality gain across cohorts, while the effect on test scores declines. This disconnect suggests that peer effects alone are not the primary driver of elite school impacts, or at least that their productivity may be changing over time.

More broadly, following the framework of [Altonji and Mansfield \(2018\)](#), school effects depend on individual-level inputs, group-level inputs (such as peers and teachers), and the productivity of those inputs. As either the level or productivity of group-level inputs may vary across time and context, it follows that causal estimates from one setting or period may not generalize to another. Our findings underscore this point: even when school inputs appear stable on the surface, the way they translate into academic outcomes may shift due to changes in curricula, institutional incentives, or unobservable factors.

The remainder of the paper is organized as follows. Section 2 provides institutional background on Mexico City’s high school admission system. Section 3 describes the data sources, sample construction, and outcome measures. Section 4 outlines the empirical strategy and presents evidence supporting the validity of the regression discontinuity design. Section 5 presents the main results. Sections 6 and 7 explore the mechanisms driving the time-varying effects. Section 8 concludes.

2 Institutional background

Elementary school in Mexico City is six years in length, middle school is three years, and high school is three years. The centralized high school admission process in Mexico City matches students with a middle school certificate to public high schools. Every year, the market has around 250,000 applicants applying for seats in around 600 high schools.

The timeline of the admission process is as follows. At the end of January, applicants receive a booklet that describes all the available public high schools in the market. Between late February and early March, students submit a rank-ordered list (ROL) of up to twenty high schools. At the end of June, students take a standardized admission exam. Exam scores are released at the end of July, and students are matched to high schools based on their exam scores, ROLs, and schools' available seats. The admission process remained unchanged during our study period (2005-2009).

The admission exam evaluates students on the material covered during middle school and in verbal and mathematical reasoning. The exam score takes integer values from 31 to 128. Students who score less than 31 are excluded from the admission process.

The matching process follows the serial dictatorship mechanism. This is a particular case of the student proposing a deferred acceptance mechanism where all schools use the same ranking of students. In Mexico City, all applicants are ranked based on their admission exam scores. The highest-scoring students are assigned to their first choices. Then, in admission score descending order, students are assigned to their highest-ranked schools with available seats.

Every high school in Mexico City belongs to one of nine subsystems. Subsystems are administrative units that manage a subset of schools. Two of the subsystems are considered elite: the IPN and the UNAM. High schools in the IPN and UNAM subsystems are affiliated with the country's two most prestigious public universities. This group of high schools are commonly referred as elite schools.

As an additional constraint, admission to any school affiliated with IPN and UNAM requires students to have a middle school GPA higher than 7/10. In practice, this constraint is not binding as the GPA requirement is satisfied by more than 90% of students each year.

Also, the minimum GPA to obtain a middle school certificate is 6/10.

From 2007 until 2014, during the last quarter of high school, students from public and private schools took a standardized exam that evaluated them on mathematics and Spanish. The government mainly used this test to assess high school-level performance. Students from UNAM-affiliated high schools did not participate in this exam. For the rest of the paper, we refer to this test as the exit exam.

3 Data

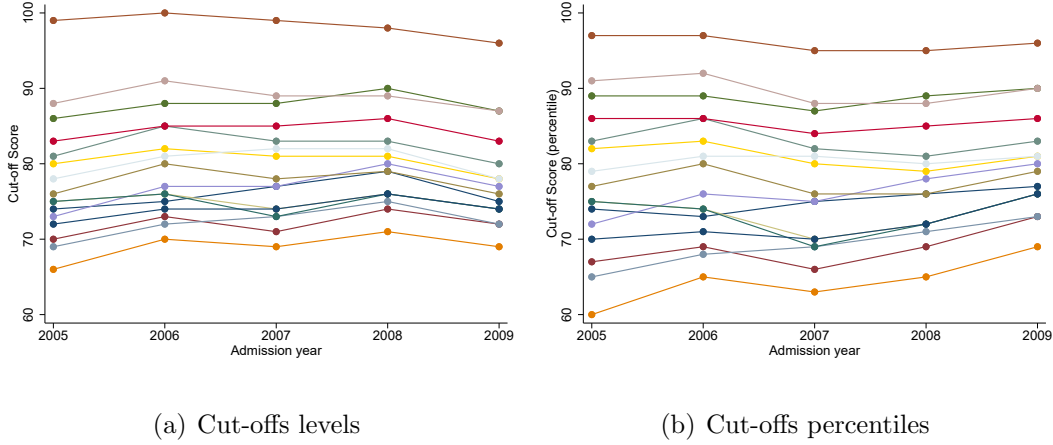
We have data on all participants in the centralized education market in Mexico City during 2005-2009. For each student we observe her ROL, admission exam score, GPA, and socio-demographics such as gender and parental education.

We combine the admission records with the high school exit exam records during 2008-2014 to measure outcomes. For each cohort of applicants we match students across datasets using their national IDs. We only work with students who participate for the first time in the admission process such that we only observe their outcomes at most once. We match students with their exam records between three to five years after application. Expected high school duration is three years.

Students admitted to UNAM-affiliated schools do not participate in the exit exam, so we do not have outcomes for these students. In the analysis we compare students admitted to IPN schools with students admitted to non-IPN and non-UNAM schools. Throughout the analysis period, the IPN subsystem has 16 affiliated schools that provide science oriented education. For the rest of the paper we will refer to these 16 high schools as elite high schools and all other high schools (excluding those affiliated to UNAM) as non-elite high schools.

Figure 1 shows the evolution of the sixteen elite schools' admission cut-offs during our study period. We define an admission cut-off as the lowest admission exam score of a student admitted to an over-subscribed school. All elite schools are over-subscribed each admission year. Panel (a) shows the admission cut-offs in levels. Panel (b) shows the admission cut-offs as percentiles of the test score distribution during a given admission year. We highlight two things from these figures. First, elite schools have relatively high admission cut-offs

Figure 1: Elite cut-offs



NOTE: This figure shows the admission cut-offs for each elite school over the period 2005-2009. Panel (a) shows the cut-offs in levels, where the score takes integer values from 31 to 128. Panel (b) shows the same cut-offs as percentiles of the distribution of test scores for each admission year.

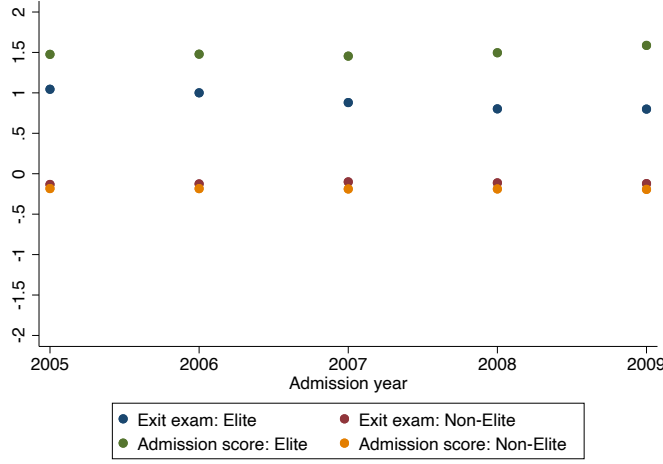
as these schools are heavily over-subscribed. Second, the admission cut-offs are stable over time, implying that the students' ability at the cut-offs has not changed much over our study period.

Our outcomes of interest are graduation/dropout and end-of-high school test scores. We consider a dropout a student assigned to a high school in the admission process who does not take the exit exam from three to five years later. For the students who do not drop out, we consider their performance on the mathematics and Spanish tests as a measure of skills at the end of high school.

We show some descriptive statistics in Figure 2. Students admitted to elite schools have higher exit exam scores than students at non-elite schools throughout our study period. They also have higher admission exam scores. The admission system creates stratification across schools by initial ability, and this has not changed much over time. However, this does not mean elite schools have a causal effect on exit exam test scores for any particular year. We use the empirical strategy outlined in the next section to separate the effect of elite schools on academic outcomes from what simply reflects the selection of better students.

Regarding school-level information, we obtain school characteristics from each year's school census (Formato 911). The school census tracks information on all schools in Mexico

Figure 2: Exams scores



NOTE: This figure shows the admission and exit exam scores for students assigned to elite and non-elite schools. We standardize each score within the distribution of scores for each admission cohort.

at the campus level. The close to 600 schools in our region of analysis are distributed into around 300 campuses, each belonging to one of the nine subsystems. The school census data includes information on teachers, students, and classrooms for each admission year.

4 Empirical strategy

4.1 Design

We want to estimate the effect of elite schools on academic outcomes for each admission year. However, since students are not randomly allocated to schools, they may self-select into elite schools for observable or unobservable reasons. To deal with the selection problem, we compare the outcomes of students who prefer elite schools to non-elite schools and are marginally admitted or rejected from elite schools (i.e., same ability). Under this design, we look to obtain internally valid estimates of the effect of admission to elite schools for students at the elite school's admission cut-offs. We define a school admission cut-off as the score of the last admitted student to an oversubscribed school. All elite schools are oversubscribed.

We follow [Kirkeboen et al. \(2016\)](#) strategy to estimate the effects of university majors and institutions in a centralized education market. We consider the case where there are only

Table 1: Stylized example of two applicants at the margin (RD Sample)

ROL	Institutions	Cut-off
1st best	Non-Elite	82
2nd best	Elite	78
3rd best	Non-Elite	76
4th best	Non-Elite	53
Application score=79		
Local Institution Ranking		
Preferred	Elite	Yes
Next-best	Non-Elite	No
Application score=77		
Local Institution Ranking		
Preferred	Elite	No
Next-best	Non-Elite	Yes

NOTE: This table provides an example where two applicants are on the margin of receiving an offer for an elite school and a non-elite school.

two institutions/subsystems, elite and non-elite. We compare the outcomes of students with the same local institution ranking (i.e., elite \succ non-elite), some of whom are admitted to their preferred institution while others gain admission to their next-best institution. Table 1 shows the types of applicants we include in our sample. This example considers two applicants with the same local institution ranking. The applicant with a score of 79 gains admission to her preferred institution, while the applicant with a score of 77 gains admission to her next-best institution.

Notice that students rank schools in cut-off descending order in our stylized example. The serial dictatorship algorithm guarantees that a student will never be assigned to a school that is not ranked in cut-off descending order. Therefore, we modify the observed ROLs to exclude all schools not ranked in cut-off descending order. ROLs subject to this modification would lead to the same equilibrium allocation as the unmodified ones.

We work with a sample of students with ROLs that list an elite school as their first best and a non-elite school as their next-best in the local institution ranking. We use a Regression Discontinuity Design (RDD) and focus on students close to the elite school admission cut-offs. Since there are 16 elite schools, we have 16 admission cut-offs. The intuition behind the identification strategy is that marginally admitted and rejected students from elite high schools have similar observable and unobservable characteristics.

Our empirical model is:

$$Y_{ik} = \mu_k + \alpha_1 \text{admit}_i + \alpha_2(S_i - \underline{s}_k) + \alpha_3(S_i - \underline{s}_k) \times \text{admit}_i + \epsilon_{ik}, \quad (1)$$

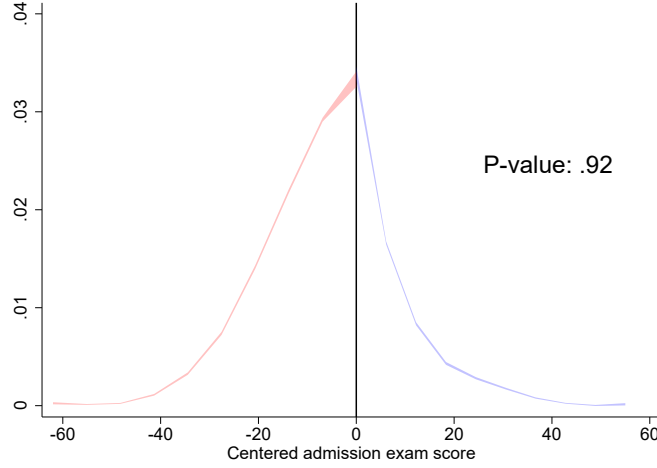
where index i is for individual, index k is for elite school admission cut-off. We have $K = 16$ elite schools and stack individuals with different elite cut-offs in the estimation sample. Our specification includes cut-off fixed effects μ_k as recommended by [Fort et al. \(2022\)](#) for the case of multi cut-off RDDs. We include a dummy variable for elite school admission admit_i . We denote the admission exam score S_i and \underline{s}_k is the elite school admission cut-off relevant to student i . In this specification $\text{admit}_i = 1$ when $S_i - \underline{s}_k \geq 0$.

The coefficient of interest is α_1 . It measures the intent-to-treat (ITT) effect of gaining marginal admission to an elite high school instead of a non-elite one. For the estimation, we use the optimal bandwidth obtained by following [Calonico et al. \(2014\)](#), which minimizes the mean square error. Within the optimal bandwidth, we estimate the parameters in Equation 1 using a local linear regression with a triangular kernel and cluster the standard errors at the admitted high school level. Since the admission process remains the same from 2005-2009, we follow the same design and empirical model for each admission cohort.

4.2 Validity

Before proceeding with the results, we provide evidence of the validity of the design. Following [Imbens and Lemieux \(2008\)](#), certain conditions need to be met to guarantee the validity of an RDD. Figure 3 shows the density of the centered admission score for the pooled sample for 2005-2009. There is no evidence that the density is discontinuous around the centered cut-offs, which indicates that manipulation of the running variable is unlikely. A formal

Figure 3: Density



NOTE: This figure shows the density of the centered running variable. The vertical line indicates the admission threshold.

statistical test does not reject the continuity of the density (p-value=0.92). As we estimate RDDs for each admission cohort, we also show evidence of a lack of manipulation for each cohort. We include these results in Appendix B.

As further support for the validity of the design, Table 2 presents results from estimating Equation 1 over predetermined covariates expected to be continuous around the centered admission cut-offs. For the pooled sample of years 2005-2019, being marginally admitted to an elite high school has no statistically significant effect on family income, gender, parents' education, or students' GPA in middle school. We report the estimates on predetermined covariates for each admission cohort in Appendix C.

Overall, we find no evidence of manipulation in the pooled sample or for each separate admission cohort, which we take as evidence of the validity of our design.

5 Results

We first show the results for the pooled sample of cohorts 2005-2009. Table 3 shows that marginal admission to an elite school has a positive and statistically significant effect on mathematics test scores, a non-statistically significant effect on Spanish test scores, and a negative and statistically significant effect on the probability of taking the exit exam (i.e.,

Table 2: Covariates

	Girl	GPA	Father	Siblings
RD_Estimate	0.012 (0.011)	0.002 (0.012)	0.011 (0.008)	0.009 (0.025)
Optimal BW	8.996	14.542	13.378	7.696
Mean	0.434	8.247	0.317	1.935
N	40,254	58,423	51,427	36,187

NOTE: This table shows RD estimates following the methods in [Calonico et al. \(2014\)](#) and the associated software package *rdrobust*. Each column indicate a different outcome variable. For each outcome we also report the associated optimal bandwidth, the average outcome for the marginally rejected students, and the number of observations. Robust standard errors in parenthesis.

Table 3: Pooled sample 2005-2009

	Math	Spanish	Test Taker
RD_Estimate	0.053*** (0.019)	-0.027 (0.021)	-0.072*** (0.010)
Optimal BW	12.611	10.355	12.659
Mean	0.143	0.137	0.601
N	31,070	27,532	53,088

NOTE: This table shows RD estimates following the methods in [Calonico et al. \(2014\)](#) and the associated software package *rdrobust*. Each column indicate a different outcome variable. For each outcome we also report the associated optimal bandwidth, the average outcome for the marginally rejected students, and the number of observations. Robust standard errors in parenthesis.

graduating). While pooling multiple years is a common approach in the literature to increase precision, it is important to consider that this may obscure heterogeneity in the effects across years. Our sample sizes are large enough to allow us to implement the same empirical design on a yearly basis.

To study whether the effect of being marginally admitted to an elite high school on

Table 4: Math

	2005	2006	2007	2008	2009
RD_Estimate	0.199*** (0.042)	0.178*** (0.038)	0.033 (0.040)	-0.023 (0.038)	-0.041 (0.040)
Optimal BW	11.576	13.308	13.006	11.565	12.615
Mean	0.111	0.102	0.144	0.179	0.161
N	4,766	5,663	6,482	6,800	7,258

H0: 2005=2006=2007=2008=2009, p-value: 0.000

Linear trend: coef -0.070, p-value 0.000

NOTE: This table shows RD estimates following the methods in [Calonico et al. \(2014\)](#) and the associated software package *rdrobust*. Each column indicate a different admission cohort. For each cohort we also report the associated optimal bandwidth, the average outcome for the marginally rejected students, and the number of observations. Robust standard errors in parenthesis.

test scores changes over time, we estimate Equation 1 separately for each admission cohort between 2005 and 2009. Table 4 presents estimated parameters for mathematics performance as the outcome variable. A clear pattern emerges: the effect is positive and significant in 2005 but steadily decreases and becomes not significant for later cohorts. We reject the equality of coefficients at conventional statistical significance levels and show that the coefficients have a statistically significant negative linear time trend. Consistent with our findings for the initial years of our sample, [Dustan et al. \(2017\)](#) also find positive and significant effects on mathematics performance for the 2005 and 2006 admission cohorts.² Our extended analysis over five years reveals that this effect is not time invariant, as it diminishes and loses statistical significance in later cohorts.

Table 5 shows that marginal admission to an elite science school does not have a statistically significant effect on Spanish scores for any admission cohort during the study period 2005-2009. We believe this is because the elite schools we study here are highly focused on providing science education. [Dustan et al. \(2017\)](#) also find no effects on Spanish scores for

²Their sample selection criteria are different from ours. Appendix E shows a replication of their results using their sample selection criteria.

Table 5: Effects on Spanish by year

	2005	2006	2007	2008	2009
RD_Estimate	-0.023 (0.050)	0.038 (0.043)	-0.026 (0.034)	-0.075 (0.052)	-0.038 (0.039)
Optimal BW	12.628	12.134	11.188	11.858	11.623
Mean	0.101	0.080	0.145	0.165	0.111
N	5,064	5,417	5,827	6,800	6,813

H0: 2005=2006=2007=2008=2009, p-value: 0.769

Linear trend: coef -0.016, p-value 0.255

NOTE: This table shows RD estimates following the methods in [Calonico et al. \(2014\)](#) and the associated software package *rdrobust*. Each column indicate a different admission cohort. For each cohort we also report the associated optimal bandwidth, the average outcome for the marginally rejected students, and the number of observations. Robust standard errors in parenthesis.

admission cohorts 2005 and 2006. We do not reject the equality of coefficients across time and find no statistically significant linear time trend in them.

Overall, the results show the time specificity of the estimated effects on mathematics performance. The next step is to understand why this effect experiences such a dramatic change over time.

6 Why did the effect change over time?

We only observe end of high school test scores for those who participate in the exit exam. Therefore, time varying effects on exam participation could potentially explain time varying effects on test scores. To assess this possibility, we estimate the year-specific effect of marginal admission to an elite school on the probability of taking the exit exam. We follow our empirical specification in Equation 1.

The results in Table 6 indicate that the estimated coefficients are negative, between 8 to 10 percentage points, and always statistically significant. However, there is no clear trend in the effects. Furthermore, we cannot reject the equality of coefficients over time, and

Table 6: Effects on graduation by year

	2005	2006	2007	2008	2009
RD_Estimate	-0.082*** (0.025)	-0.058** (0.025)	-0.064** (0.027)	-0.062*** (0.020)	-0.101*** (0.021)
Optimal BW	10.441	9.431	11.570	8.351	10.539
Mean	0.624	0.615	0.585	0.585	0.614
N	7,429	7,573	10,159	9,446	10,830

H0: 2005=2006=2007=2008=2009, p-value: 0.656

Linear trend: coef -0.004, p-value 0.562

NOTE: This table shows RD estimates following the methods in [Calonico et al. \(2014\)](#) and the associated software package *rdrobust*. Each column indicates a different admission cohort. For each cohort we also report the associated optimal bandwidth, the average outcome for the marginally rejected students, and the number of observations. Robust standard errors in parenthesis.

their linear trend is not statistically significant. We take this as evidence against differential selection over time. Under non-differential selection over time, methodologies to correct for bias would shift the estimates on test scores in the same direction without affecting the time trend on the effect on mathematics test scores.

A common interpretation in the selective school literature is that the RDD estimated parameter measures peer effects. The logic behind this interpretation is that marginally admitted students to selective schools experience increased peer quality relative to their counterfactual alternatives. For example, this occurs when selective schools admit students based on skill measures, as in Mexico City. Therefore, if the discontinuous jump in peer quality has changed over time, then we would expect time-varying effects on test scores.

In Table 7, we show the jump in peer quality for students marginally admitted to an elite school relative to their next-best alternative. We measure peer quality using the average admission exam score of all the admitted students to a school in a given year. Our results show that admission to an elite school implies an increase in peer quality for each year in our sample. Nevertheless, the estimated effect on peer quality is roughly constant over time. We do not reject the equality of the yearly parameters or find evidence of a linear trend in

Table 7: Peers exam

	2005	2006	2007	2008	2009
RD_Estimate	17.424***	17.303***	18.432***	18.341***	18.625***
	(0.923)	(0.999)	(0.966)	(0.917)	(1.002)
Optimal BW	10.334	10.614	10.206	11.509	14.063
Mean	63.906	66.282	65.576	66.457	62.585
N	7,429	8,155	9,534	11,871	13,753

H0: 2005=2006=2007=2008=2009, p-value: 0.853

Linear trend: coef 0.280, p-value 0.323

NOTE: This table shows RD estimates following the methods in [Calonico et al. \(2014\)](#) and the associated software package *rdrobust*. Each column indicate a different admission cohort. For each cohort we also report the associated optimal bandwidth, the average outcome for the marginally rejected students, and the number of observations. Robust standard errors in parentheses.

them. Therefore, changes in the average peer quality experienced by those admitted to elite schools are unlikely to be the primary driver of the decreasing effect on test scores.

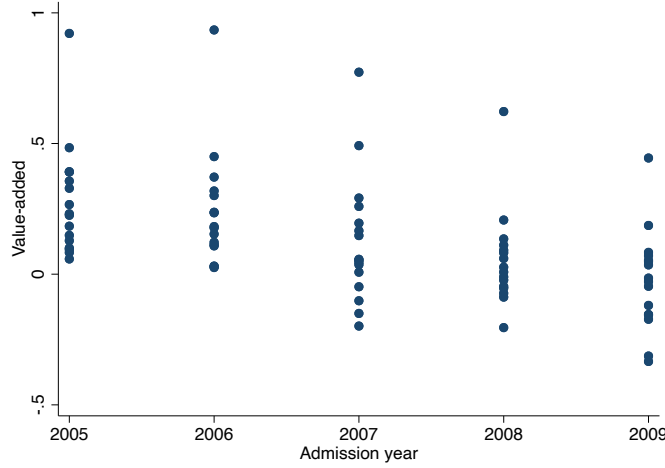
We next consider changes in school quality as an explanation for our time-varying effects. Access to elite schools could have a positive effect on test scores if they provide students with access to higher-quality schools relative to their next-best alternatives. Therefore, if the relative school quality between the first and next-best alternatives has been changing over time, the RDD estimated parameter would also change over time. To measure individual school quality, we estimate a value-added model, as shown in Equation 2.

$$Y_{it} = \sum_{j=0}^J \alpha_{jt} D_{ijt} + X'_{it} \Gamma_t + \nu_{it}, \quad (2)$$

where X_i is a vector that includes the standardized admission exam score and standardized middle school GPA. The outcome variable Y_{it} measures mathematics exam performance at the end of high school. Our parameters of interest are α_{jt} , which measure school quality for each school j and for each year t . For the value-added estimation, we do not restrict the sample to the RDD sample; instead, we use all available data.

In Figure 4, we show the value-added estimated parameters for the sixteen elite schools

Figure 4: Elite schools value-added ($\hat{\alpha}_{jt}$)



NOTE: This figure shows the evolution over time of elite schools value-added estimated parameters. Each dot represents a school quality estimate $\hat{\alpha}_{jt}$.

for each admission year. The value-added of elite schools consistently decreases every year. Note that the value-added parameter is estimated using the full population of students, not just those on the margin of admission. We take this as evidence of the average quality of elite schools decreasing over time.

We then use all the value-added estimated parameters as the outcome variable in our RDD estimations. We aim to capture if there is a discontinuous jump in school value-added between students marginally admitted to elite schools and those marginally rejected. Additionally, we aim to determine if the effect on school value-added has changed over time. In Table 8, we show that marginal admission to elite schools results in gaining access to schools with higher value-added. In addition, the effect on school quality is decreasing over time. We reject the equality of coefficients across time and find a negative and statistically significant linear trend in the estimated coefficients.

Since we have sixteen elite schools as preferred schools and multiple non-elite schools as next-best alternatives, another possible explanation for the time-varying effects could be that the weights of the different schools have been changing over time. This could be the case for the RDD estimates, as our parameter of interest compares a weighted average of elite schools against a weighted average of non-elite schools. Appendix D shows that for students marginally admitted to elite schools, the weights for different elite schools have been

Table 8: Value-added ($\hat{\alpha}_{jt}$)

	2005	2006	2007	2008	2009
RD_Estimate	0.270*** (0.025)	0.258*** (0.021)	0.133*** (0.026)	0.055*** (0.015)	0.013 (0.024)
Optimal BW	14.580	15.973	13.647	14.039	16.147
Mean	-0.033	-0.050	-0.044	-0.051	-0.076
N	9,151	10,333	11,274	13,737	14,937

H0: 2005=2006=2007=2008=2009, p-value: 0.000

Linear trend: coef -0.071, p-value 0.000

NOTE: This table shows RD estimates following the methods in [Calonico et al. \(2014\)](#) and the associated software package *rdrobust*. Each column indicate a different admission cohort. For each cohort we also report the associated optimal bandwidth, the average outcome for the marginally rejected students, and the number of observations. Robust standard errors in parenthesis.

mostly constant over time. As there are many next-best alternatives for students marginally rejected, we summarize the information by grouping schools into subsystems. Appendix D also shows that most of the next-best subsystems have a constant weight over time, except the two most popular ones, which experience small changes. However, when we repeat the RDD analysis excluding students admitted to the two most popular next-best subsystems, we still find decreasing treatment effects on mathematics test scores with a similar negative time trend as in our main specification. We take this as evidence that the small changes in school/subsystem weights over time are unlikely to explain the time-varying effects.

With this evidence, we conclude that the decreasing effects of marginal admission to elite schools on test scores are not due to changes in the composition of test takers over time or changes in peer quality. Instead, our results suggest that the difference in the quality of schools that treated and untreated students are exposed to is behind the time-varying effects.

Table 9: $Z'_{jt}\hat{\theta}_t$

	2005	2006	2007	2008	2009
RD_Estimate	0.018*** (0.003)	-0.101*** (0.010)	-0.063*** (0.014)	-0.070*** (0.012)	-0.073*** (0.019)
Optimal BW	11.075	13.817	12.180	11.817	11.603
Mean	0.012	-0.008	0.016	-0.005	0.004
N	7,812	9,297	10,488	11,500	11,296

H0: 2005=2006=2007=2008=2009, p-value: 0.000

Linear trend: coef -0.013, p-value 0.006

NOTE: This table shows RD estimates following the methods in [Calonico et al. \(2014\)](#) and the associated software package *rdrobust*. Each column indicate a different admission cohort. For each cohort we also report the associated optimal bandwidth, the average outcome for the marginally rejected students, and the number of observations. Robust standard errors in parenthesis.

7 Why did school quality change?

When estimating school value-added parameters, we estimate fixed effects that reflect school characteristics and their productivities. Changes in these characteristics or their productivity over time will thus affect the value-added estimates. We model this relationship as:

$$\hat{\alpha}_{jt} = Z'_{jt}\theta_t + \eta_{jt}, \quad (3)$$

where Z_{jt} is a vector of school characteristics that includes average peers' admission score, average peers' GPA, teachers per pupil, female teachers per pupil, full-time teachers per pupil, highly qualified teachers per pupil, and classrooms per pupil. We estimate Equation 3 to separate the part of value-added explained by observable school characteristics and their productivities ($Z'_{jt}\hat{\theta}_t$) from unobservable school characteristics ($\hat{\eta}_{jt}$). We then use these estimates as outcome variables in our RDD framework.

In Table 9 we show that marginal admission to elite schools is associated with an increase in the fitted value $Z'_{jt}\hat{\theta}_t$ for the 2005 admission cohort. However, for later cohorts the effect

Table 10: $\hat{\eta}_{jt}$

	2005	2006	2007	2008	2009
RD_Estimate	0.252*** (0.024)	0.358*** (0.026)	0.190*** (0.023)	0.123*** (0.015)	0.087*** (0.024)
Optimal BW	16.921	16.579	14.010	11.627	14.730
Mean	-0.044	-0.046	-0.057	-0.043	-0.081
N	9,747	10,349	11,525	11,500	13,390

H0: 2005=2006=2007=2008=2009, p-value: 0.000

Linear trend: coef -0.058, p-value 0.000

NOTE: This table shows RD estimates following the methods in [Calonico et al. \(2014\)](#) and the associated software package *rdrobust*. Each column indicate a different admission cohort. For each cohort we also report the associated optimal bandwidth, the average outcome for the marginally rejected students, and the number of observations. Robust standard errors in parenthesis.

becomes negative and monotonically decreasing. We reject equality of coefficients over time and we find they have a statistically significant negative linear trend.

The change in the effect on the fitted value can be due to time changes in school characteristics (Z_{jt}), time changes in productivities (θ_t), or both. In [Appendix A](#), we use each school characteristic as the outcome variable in the RDD specification and find no effect pattern or time trend. This implies that the same inputs (e.g., teacher ratios) became less productive in elite schools relative to non-elite schools during our study period.

A possible explanation behind the change in the productivity of school characteristics for elite and non-elite schools is that a curriculum alignment policy was in place during our study period. Notice that such a policy would not likely change the school characteristics levels but would change how productive the same levels are in creating school quality. Therefore, a curriculum alignment policy that imposes similar curriculums at elite and non-elite schools would align the productivities of school characteristics across sectors and decrease the treatment effect of marginal admission to an elite school.

We next consider the estimated residuals $\hat{\eta}_{jt}$, which we interpret as a measure of unobservable school characteristics that affect school quality. In [Table 10](#), we show the results of

our RDD estimations using $\hat{\eta}_{jt}$ as the outcome variable. We find that marginal admission to elite schools implies gains in unobservable school characteristics for all years between 2005-2009. However, this effect decreases over time. We reject equality of effects over time and find a statistically significant positive linear trend in coefficients. This suggests that both observed and unobserved aspects of school quality contributed to the declining time-varying effects on mathematics test scores.

The results in this section highlight that school value-added captures differences in school characteristics and their associated productivities. Since school characteristics and productivities can change over time, value-added can also change. Thus, if the effect of elite schools on academic outcomes is due to students gaining access to higher value-added schools, then this effect does not need to be constant over time. The same applies when comparing effects across different contexts, as the estimated parameters may not measure the same treatments.

8 Conclusions

The results presented in this paper indicate that the effect of being marginally admitted to an elite high school is not constant over time and relates to time changes in relative school quality between elite schools and their next-best alternatives. In the case of Mexico City, we find that over five years (2005-2009), the effect of marginal admission to elite science schools on mathematics test scores monotonically decreased and went from positive and statistically significant to not significant.

We explain the time-varying effect by showing that the gains in school quality from marginal admission to elite schools monotonically decreased during our study period. The gains in peer quality due to marginal admission did not change over time. Also, there were no time changes in the effect of marginal admission on exit exam test taking. A plausible explanation for the changes in school quality gains is a curriculum alignment policy in place during our study period. Such policy affected how school inputs mapped into school value-added. In addition, there were also changes in the school value-added part that were unexplained by observed school characteristics.

Our results contribute to a growing understanding that the effects of elite schools are

highly context-dependent. They suggest that the caution researchers apply when generalizing results across countries should also be extended to generalizations across time, even within the same institutional setting. This highlights the importance of understanding the specific mechanisms and policy environments at play when interpreting causal estimates.

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A Other school characteristics

Table 11: Change in average peers GPA

	2005	2006	2007	2008	2009
RD_Estimate	0.510*** (0.023)	0.494*** (0.024)	0.501*** (0.021)	0.475*** (0.023)	0.476*** (0.020)
Optimal BW	14.134	13.333	14.278	16.773	13.920
Mean	7.829	7.867	7.900	7.912	7.949
N	9,151	9,545	11,806	14,817	13,084

H0: 2005=2006=2007=2008=2009, p-value: 0.881

Linear trend: coef -0.007, p-value 0.298

Table 12: Teachers per pupil

	2005	2006	2007	2008	2009
RD_Estimate	0.024*** (0.005)	0.017*** (0.002)	0.047*** (0.004)	0.017*** (0.003)	0.020*** (0.003)
Optimal BW	15.559	13.150	11.558	20.510	13.734
Mean	0.056	0.051	0.026	0.053	0.050
N	9,415	9,297	9,927	15,963	12,745

H0: 2005=2006=2007=2008=2009, p-value: 0.000

Linear trend: coef -0.001, p-value 0.231

Table 13: Female teachers per pupil

	2005	2006	2007	2008	2009
RD_Estimate	0.004*	0.003*	0.014***	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Optimal BW	14.191	16.980	13.065	16.280	14.020
Mean	0.021	0.019	0.010	0.021	0.020
N	9,045	10,349	11,010	14,311	13,390

H0: 2005=2006=2007=2008=2009, p-value: 0.000

Linear trend: coef -0.001, p-value 0.135

Table 14: Full time teachers per pupil

	2005	2006	2007	2008	2009
RD_Estimate	0.010***	0.011***	0.010***	0.012***	0.009***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Optimal BW	13.681	12.126	13.795	14.412	13.422
Mean	0.012	0.011	0.011	0.011	0.011
N	8,647	8,873	11,010	13,282	12,745

H0: 2005=2006=2007=2008=2009, p-value: 0.356

Linear trend: coef 0.000, p-value 0.875

Table 15: High education teachers per pupil

	2005	2006	2007	2008	2009
RD_Estimate	-0.000 (0.000)	0.001*** (0.000)	0.001** (0.001)	0.000 (0.000)	0.004 (0.002)
Optimal BW	9.806	16.151	16.671	16.190	12.456
Mean	0.002	0.002	0.002	0.002	0.002
N	6,831	10,349	12,394	14,311	12,029

H0: 2005=2006=2007=2008=2009, p-value: 0.014

Linear trend: coef 0.001, p-value 0.115

Table 16: Classrooms

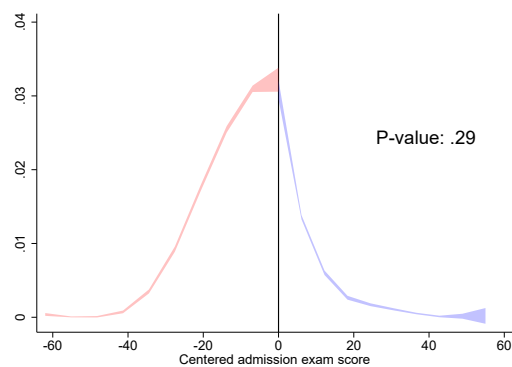
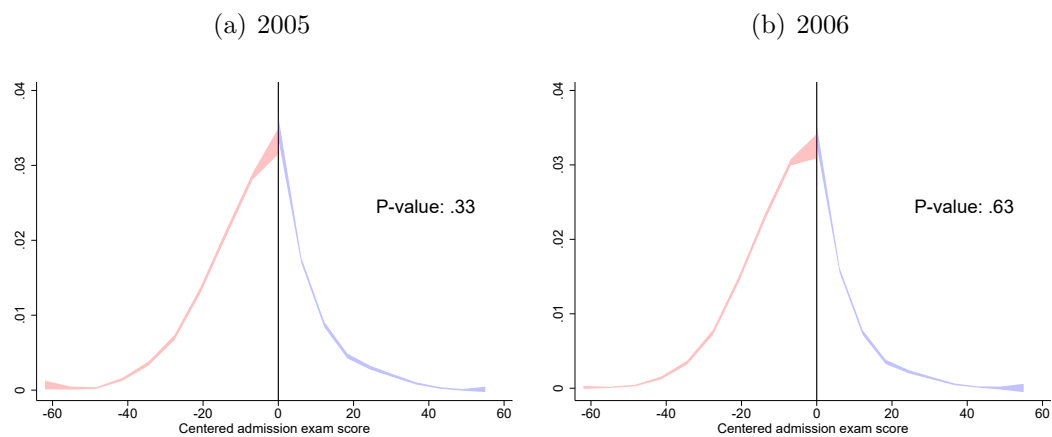
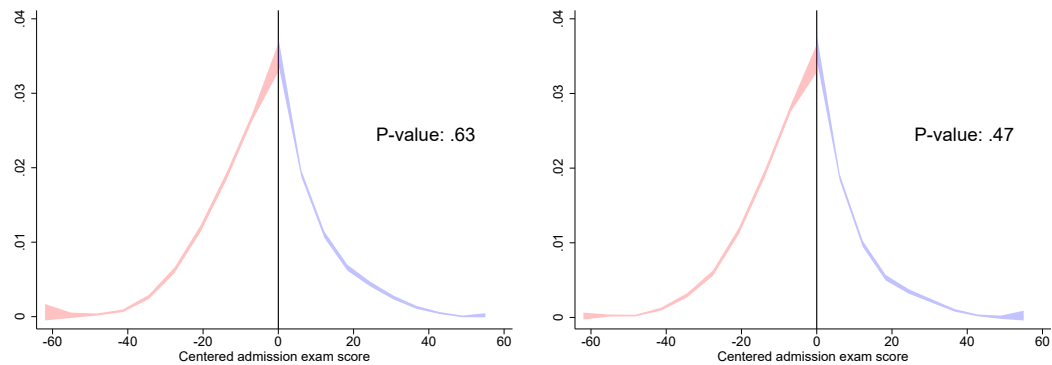
	2005	2006	2007	2008	2009
RD_Estimate	-0.002 (0.002)	0.004*** (0.001)	0.001** (0.001)	0.002** (0.001)	0.001 (0.001)
Optimal BW	13.817	10.645	14.069	12.807	12.602
Mean	0.023	0.022	0.022	0.021	0.021
N	8,647	7,955	11,525	12,126	12,029

H0: 2005=2006=2007=2008=2009, p-value: 0.023

Linear trend: coef 0.001, p-value 0.139

B Density by cohort

Figure 5: Density test



(e) 2009

C Covariates by cohort

Table 17: Girl

	2005	2006	2007	2008	2009
RD_Estimate	-0.017 (0.020)	0.014 (0.025)	0.012 (0.023)	0.022 (0.023)	0.025 (0.028)
Optimal BW	12.177	8.676	10.922	9.602	11.005
Mean	0.422	0.407	0.448	0.459	0.438
N	8,374	7,021	9,534	10,276	11,584

Table 18: GPA

	2005	2006	2007	2008	2009
RD_Estimate	0.009 (0.030)	-0.017 (0.030)	-0.014 (0.025)	0.042 (0.028)	-0.014 (0.034)
Optimal BW	10.759	10.573	12.818	11.679	11.138
Mean	8.205	8.218	8.254	8.258	8.325
N	7,429	8,155	10,740	11,871	11,584

Table 19: Father education

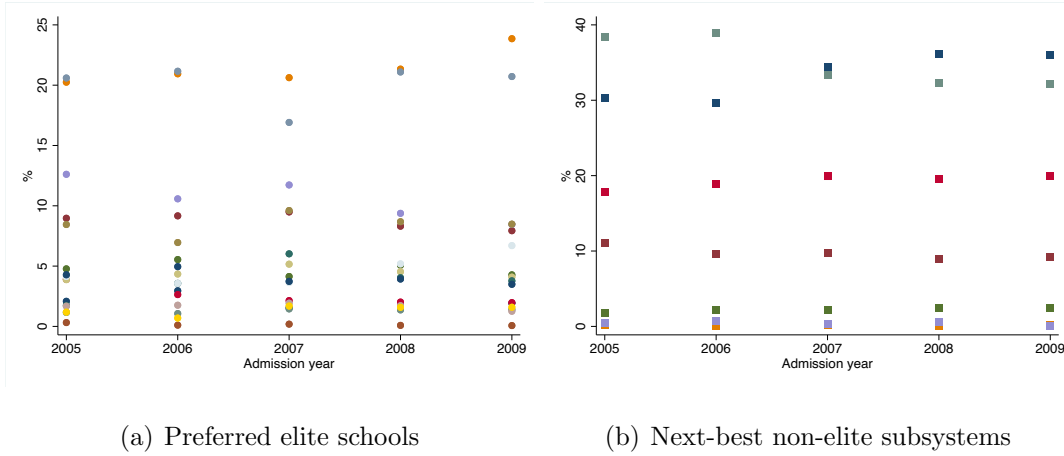
	2005	2006	2007	2008	2009
RD_Estimate	-0.010 (0.021)	0.026 (0.017)	0.020 (0.019)	0.019 (0.021)	-0.010 (0.021)
Optimal BW	9.483	16.071	10.152	8.908	9.780
Mean	0.308	0.288	0.328	0.343	0.346
N	6,398	9,825	8,767	8,708	9,228

Table 20: Siblings

	2005	2006	2007	2008	2009
RD_Estimate	0.027	-0.048	-0.025	-0.040	0.070
	(0.064)	(0.062)	(0.050)	(0.051)	(0.046)
Optimal BW	9.692	7.144	8.870	7.456	9.190
Mean	2.080	2.011	1.968	1.885	1.829
N	6,869	6,346	8,108	8,413	10,021

D First and next-best schools

Figure 6: Local school ranking (weights)



NOTE: This figure shows the weights of preferred elite schools and next-best non-elite subsystems for students within ten points of their respective elite school admission cut-offs. Panel (a) shows the weights of the sixteen elite schools over time (circles). Panel (b) shows the weights of the six non-elite subsystems over time (squares).

Table 21: Math restricted sample

	2005	2006	2007	2008	2009
RD_Estimate	0.195** (0.085)	0.086 (0.077)	-0.051 (0.078)	-0.075 (0.055)	-0.115* (0.063)
Optimal BW	8.825	11.636	11.068	10.445	10.385
Mean	0.132	0.123	0.149	0.192	0.193
N	1,576	2,083	2,408	2,666	2,443

H0: 2005=2006=2007=2008=2009, p-value: 0.000

Linear trend: coef -0.073, p-value 0.000

NOTE: This table shows RD estimates following the methods in [Calonico et al. \(2014\)](#) and the associated software package *rdrobust*. We restrict the sample to exclude students assigned to the two most popular non-elite subsystems when marginally rejected from an elite school. Each column indicates a different admission cohort. For each cohort, we also report the associated optimal bandwidth, the average outcome for the marginally rejected students, and the number of observations. Robust standard errors in parenthesis.

E Replication

In order to have an initial reference for our estimates, we replicate the results obtained by [Dustan et al. \(2017\)](#) using application cohorts 2005-2006. Table 22 presents the results of this exercise.

Table 22: Effects of elite assignment, 2005-2006

	Dropout	Math	Spanish
admit	0.094*** (0.017)	0.197*** (0.030)	0.028 (0.031)
N	17,850	11,959	11,216

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

score is slightly smaller than their result (their point estimate is 0.246) but is also statistically significant at 99%. Lastly, the effect on the Spanish test score is similar in magnitude, and it is also not statistically significant.