Can large, untargeted conditional cash transfers increase urban high school graduation rates? Evidence from Mexico City's Prepa Sí

Andrew Dustan*
Department of Economics
Vanderbilt University

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Abstract

This paper estimates the effects of a massive, minimally targeted conditional cash transfer program in Mexico City's public high schools on graduation rates, test scores, and school choice. Using a difference-in-differences approach that exploits variation in eligibility between students and cohorts within a high school, I find that this program had no appreciable effect on high school completion. The results are sufficiently precise to rule out policy-relevant effect sizes. Null effects persist for subgroups that could be candidates for a targeted program. End-of-high school exam scores are apparently unaffected by the program and effects on high school choices by eligible students are minimal. There is no evidence for heterogeneous effects with respect to implicit or explicit cost of attendance, suggesting that liquidity constraints are not a key driver of high school dropout in this urban setting. These results highlight the challenges of using cash to improve academic outcomes in cities.

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1 Introduction

The developing world is urbanizing at a rapid pace, at the same time as it experiences significant growth in primary and lower secondary school completion rates.[1] As a consequence, many educational systems are turning their attention to high school completion, with the goals of increasing student learning and providing pathways to formal employment and higher education. But high school completion rates remain low. In many Latin American countries, which are highly urbanized compared to other developing regions, only about half of students graduate from high school (UNESCO Institute for Statistics 2010). One consequence is that one in five Latin American youth ages 15 to 24 is a so-called *nini*, neither studying nor working (de Hoyos, Popova, and Rogers 2016). Two-thirds of these ninis live in urban areas. They typically drop out of high school to work in the informal sector, become unemployed at some point, and then lack the high school diploma (and associated skills) needed to obtain stable, formal employment. The prevalence of ninis is not only an economic concern, but also a potential driver of organized crime and other social problems in the region (de Hoyos et al. 2016).

As a response to this phenomenon and increasing concern about the need to develop a skilled, educated workforce, Mexico recently made high school compulsory. But finding the optimal policy for encouraging high school completion in urban areas, where labor market opportunities are more immediate and the real value of conditional cash transfers (CCTs) is lower than in rural areas, has proved difficult. While policy experts in the region point to promising evidence in favor of carefully-structured transfer schemes (Barrera-Osorio, Linden, and Saavedra, forthcoming) or targeted non-monetary interventions,[2] public funding overwhelmingly flows to broad CCTs as the primary anti-dropout tool. Andrés Manuel López Obrador, elected as President of Mexico in 2018, has even begun implementing an untargeted CCT for all public high school students in the country. Supporting rhetoric often centers on liquidity constraints: students drop out due to school fees and transportation fares, as well as the need to generate income for the family's other immediate needs. Implicit in this argument is that the binding liquidity constraint is sufficiently common—or that effective targeting is sufficiently costly—to justify a universal CCT. Whether such a program will actually increase high school graduation rates, particularly in urban areas, is open for debate.

[1]The proportion of developing nations' population living in urban areas has nearly doubled since 1960 and neared 50% by 2011 (World Bank and International Monetary Fund 2013).

[2]Examples of such non-monetary interventions include programs that build socioemotional skills (Cook et al. 2014; Heller et al. 2017), prevent teen pregnancy (Steinka-Fry, Wilson, and Tanner-Smith 2013), and provide mentoring to at-risk students (Rodriguez-Planas 2012), as summarized in de Hoyos, Popova, and Rogers (2016).

This paper provides evidence on the potential effectiveness of universal urban CCT schemes by evaluating *Prepa Sí* ("High School Yes"), Mexico City's version of such a program. First implemented in 2007, it pays students monthly for being enrolled in a public high school. The amount of these transfers is similar to the costs of attending a public school. The main eligibility criteria are that the student must attend a public high school located in the Federal District (*Distrito Federal*, or DF) and must also reside in the DF, rather than the contiguous suburbs located in the State of Mexico (*Estado de México*, or EdoMex) that form the Mexico City metropolitan area. The residency requirement generates within-school variation in eligibility because many students living in EdoMex are assigned to DF schools through a centralized, citywide admissions consortium that places students on the basis of merit and student preferences. I combine the cross-sectional within-school variation in eligibility with the timing of the program's introduction to estimate the effects of eligibility on high school completion, 12th grade standardized test scores, and students' choice of high schools. This difference-in-differences approach controls for both high school-by-year shocks and geographically local shocks that affect neighboring DF and EdoMex residents, so that identification comes from within-high school changes in outcomes between DF and EdoMex residents. This is made possible by administrative data covering the universe of several cohorts of students applying to public high schools in the Mexico City metropolitan area, merged with exit exam records that provide a proxy for graduation, as well as supplementary administrative data on program take-up.

The key finding is that eligibility for Prepa Sí, despite increasing the probability of receiving the transfer in the student's first year by between 46 and 60 percentage points, did not increase high school graduation rates. The 95% confidence interval for this effect is sufficiently narrow to rule out increases of 1.1 percentage point or higher in the probability of completing high school. To investigate whether targeting the transfers to students on the basis of observables would have increased the average effect on completion, I estimate program effects for several subgroups that would likely be targeted if such an effort were undertaken. In all cases, estimated impacts are bounded close to zero. Furthermore, eligible students who faced higher costs of attendance in the form of school fees and commuting distance did not experience increases in graduation probability compared to eligible students with lower costs. This casts further doubt on the role of liquidity constraints in driving dropout.

To investigate the possibility that within-school spillovers between eligible and ineligible students are biasing the estimated program effect downward, I use a related difference-in-differences design to compare EdoMex residents attending DF schools (i.e. students who are ineligible but attend school with many eligible students) with neighbors attending a school in the EdoMex (where no student is eligible for the program). There is no evidence for positive spillovers on ineligible students attending school with eligible ones, as might occur if resources previously allocated to DF students were reallocated to those students who were ineligible for the newly-introduced Prepa Sí. As an additional test for direct and spillover effects of the program, I use a difference-in-discontinuities design that exploits admissions cutoffs arising from the assignment mechanism to estimate the effect of assignment to a DF school (instead of an EdoMex school) for marginal students. Again, I find no evidence of positive effects on high school completion.

Next, the effects of the program on end-of-high school test scores and self-reported academic effort are examined. There is, again, no evidence of positive impacts. Taken together, the evidence suggests that this CCT did not improve educational outcomes despite its massive scale. Furthermore, examining school choices for students who selected schools at a time when Prepa Sí's existence was known, it does not appear that eligible students responded by choosing more expensive, competitive, or distant schools. This is in contrast with Avitabile, Bobba, and Pariguana (2017), who find evidence of substitution toward more expensive schools among very poor (Oportunidades-eligible) students in Mexico City's suburbs. This again suggests that liquidity constraints are not the main driver of school-going behavior among Prepa Sí's beneficiary population, although I will discuss further how Oportunidades and Prepa Sí could differ in their effects on choice.

This paper advances our limited understanding of how educational CCTs perform in urban high schools, particularly with respect to completion rates. The literature on educational CCTs is vast: Fiszbein and Schady (2009) provide a thorough review of the early evidence, Saavedra and García (2012) conduct a meta-analysis of impacts, Glewwe and Muralidharan (2016) describe several recent studies, and Millán et al. (forthcoming) review the evidence on long-term impacts. Each of these reviews points out the ample evidence that CCTs can increase attendance and enrollment. Most of the reviewed evaluations, and nearly all of those using randomized controlled trials (RCTs), primarily benefit rural families. Furthermore, there is a paucity of evidence on high school *completion* effects, which are important beyond shorter-run attendance effects. A notable exception is parallel work by De Hoyos, Attanasio, and Meghir (2019), which finds no evidence of any impact for a national high school scholarship program in Mexico during a similar time period to the one studied in the present paper.

The existing literature focused on CCTs for urban high school students is inconclusive with respect to their efficacy in increasing graduation rates. Barrera-Osorio et al. (2011) find that a one-year pilot program in urban Colombian high schools increases attendance and re-enrollment for the next school year. Barrera-Osorio, Linden, and Saavedra (forthcoming) reports high school graduation effects among students who were in high school at the time of the program's introduction. These effects were small, positive, and statistically insignificant, except in the case of a treatment that incentivized re-enrollment, where the effect was marginally significant. Quasi-experimental evidence is also mixed, but suggests the potential for positive impacts on completion. Behrman et al. (2012) study the urban expansion of Mexico's Oportunidades, which entered the poorest (peri-)urban municipalities and did not follow a randomized rollout. They find positive effects of Oportunidades receipt on years of schooling after two years of exposure, but effects on completion are not reported. In Colombia, Baez and Camacho (2011) present mixed and somewhat sensitive findings of a national anti-poverty CCT's effects on completion in urban areas. In a much different context, an evaluation of the pilot Opportunity NYC–Family Rewards program in New York City found no effect on high school graduation rates (Riccio et al. 2013). Compared to the state of the current literature, Prepa Sí offers an opportunity to study the effects of an urban CCT that operates at scale, in high school only, for up to the duration of the student's enrollment. These characteristics suggest that the results may be informative about the likely impacts of implementing a scaled program elsewhere, although the concluding discussion will point out areas of the program design that, if modified, could lead to better performance in improving educational outcomes.

The remainder of the paper proceeds as follows. Section 2 provides institutional background on education in Mexico City and the Prepa Sí program. Section 3 sets forth the empirical strategy for evaluating this program. Section 4 describes the data and Section 5 presents results. Section 6 concludes.

2 Institutional background

This section gives a brief overview of high school dropout in Mexico. It then explains the structure of the high school system in the Mexico City metropolitan area and provides background on the Prepa Sí program and relevant national CCTs and scholarship programs. Several of these institutional details provide the basis for the empirical strategy that is employed to estimate the effects of Prepa Sí.

2.1 High school dropout in Mexico

Mexico's public education system consists of six years of primary school (*primaria*), three years of middle school (*secundaria*), and three years of high school (*media superior*). High school became compulsory in 2012, immediately after the period addressed in this paper, but this requirement is not yet enforced vigorously. High school completion among those who initially enroll at this level is quite low. Nationally in the 2010-2011 school year, the average annual dropout rate in all grades (10 through 12) of high school was 14.9%, resulting in a completion rate of 62.2% (Subsecretaría de Educación Media Superior and Consejo para la Evaluación de la Educación Medio Superior A.C. 2012). The dropout rate in the Mexico City metropolitan area is higher than the national average: the DF, which forms the core of the metropolitan area, had a dropout rate of 18.5% during this period. The vast majority of high school dropout takes place within the first two years. In the set of DF public high schools examined in this paper, an estimated 63% of all dropout takes place within the first year and 87% occurs within two years.[3] The primary policy challenge, then, is keeping students from dropping out early in their high school careers rather than finding ways to push enrolled-but-lagging students to complete their requirements and graduate.

In a national survey of students who had dropped out of high school, the most-reported reason for leaving school was a lack of money for school supplies, travel, and associated fees (Subsecretaría de Educación Media Superior and Consejo para la Evaluación de la Educación Medio Superior A.C. 2012; Kattan and Székely 2014). This option was selected as the most important reason by 36% of students and was mentioned by 50%. While the second most-mentioned reason was a dislike of studying (appearing on 17.8% of lists), the third most-mentioned motive was also economic: a belief that working was more important than studying (mentioned by 13% of dropouts). The average annual cost of high school, including materials and uniforms but excluding transportation costs, for Prepa Sí-eligible students in this paper's sample (DF residents assigned to DF public high schools) was 3,208 pesos (about $290). Transportation costs vary, but round-trip transportation on the subway system during this time period would cost about 750 pesos per year and students riding the bus instead of, or in addition to, the subway would have spent considerably more. As a point of reference, self-reported family income of eligible students in this sample is about 4,300 pesos per month, similar to the average total annual cost of attendance.

2.2 High school in the Mexico City metropolitan area

Mexico City's public high schools enroll more than a quarter of a million new first-year students each year. Public schools are divided into nine "subsystems" that are spread throughout the metropolitan area. These include two elite, university-affiliated subsystems; other traditional, academic subsystems; and multiple technical and vocational subsystems where students can study trades and technical fields while pursuing a high school diploma that is

[3]The procedure used to estimate these dropout figures is described in Appendix B.

considered valid for university admission. In all, there were more than 300 campuses offering over 600 programs during the sample period being studied.

In 1996, these subsystems collaborated to form the Metropolitan Commission of Public High School Institutions (COMIPEMS), with the goal of creating a centralized process for assigning students to high schools. The schools belonging to COMIPEMS are located throughout the metropolitan area, which includes the DF and the EdoMex. In many cases the border between the DF and EdoMex is almost indistinguishable, and travel between the two entities is extremely common. Public and private mass transit now operates across borders seamlessly to integrate most of the COMIPEMS area (Dustan and Ngo 2018). About 29% of students residing in the EdoMex attend schools in the DF, and half of EdoMex students within three kilometers of the border attend DF schools. In contrast, very few DF students attend school in the EdoMex.

Students who wish to attend a public high school in Mexico City must participate in the annual COMIPEMS admissions process. First, in the spring of ninth grade, the final year of middle school, students receive a catalog of schooling options that they can request. Second, they turn in a ranked list of up to 20 preferences, along with a demographic survey. Third, they take a standardized entrance exam, which assesses both aptitude and knowledge of several content areas. Finally, after a computer has graded the exams, the assignment process occurs. This process ranks all students according to their exam scores, and then proceeds down the ranked list, assigning each student to his most-preferred school that still has a seat available in its pre-reported capacity. Thus high-scoring students are assigned to their top choices, while students with lower scores may be assigned to less-preferred options because their top choices fill up earlier in the process. Any student who was not assigned during the automated process must choose from schools that still have seats remaining in a later, separate process.[4]

2.3 Prepa Sí and other educational CCT programs

The low rate of high school completion has long been a concern for education officials in the DF. In order to address this problem, the DF government began the Prepa Sí program at the beginning of the 2007-2008 school year. The political process that led to its introduction was rapid, such that families were essentially unable to anticipate its existence during previous school years. Created with the stated intention of increasing high school completion rates

[4]Students unhappy with their assignment or who otherwise want to attend a high school other than the one to which they were assigned must wait until the next year's admissions cycle. Among applicants assigned during the automated process, this is uncommon: only 6% of students in the sample used in this paper's main analysis retake in the following year.

and raising student grades, the core component of the program was a cash transfer paid to students enrolled in high school.

The program was minimally targeted. In order to receive the transfer, students needed to legally reside in the DF, attend a public high school located in the DF, and have a grade point average of 6.0/10 or higher, which is a low bar since a grade below 6 is considered failing. Monthly payments were made directly to students, who had bank cards that allowed them to withdraw the cash. There were 10 monthly payments each year, with no payments during the two summer months. The amount of the transfer depended on the student's current grade point average: 6.0 to 7.5 averages received 500 pesos per month (about $46), 7.6 to 9 received 600 pesos per month ($56), and 9.1 to 10 received 700 pesos per month ($65).[5] Students were not allowed to obtain federal educational CCTs at the same time as Prepa Sí, but in the inaugural 2007-2008 school year, this rule was essentially unenforced (Ligeia Ravest, Pina, and Segura 2008). Prepa Sí had features that distinguished it from other educational CCTs. It required students to take part in a variety of community- and service-oriented events, although actual student participation was low and this conditionality was not effectively enforced (Ligeia Ravest, Pina, and Segura 2008). It also provided accidental injury insurance to all participating students. In its second year of operation (2008-2009), the program was expanded to provide monthly payments of the same amounts to beneficiaries who had graduated from high school and were attending their first year of public university within the DF.

Families of eligible students could apply for Prepa Sí themselves, although schools often provided application assistance and disseminated information about the program, particularly at its inception. Once students were enrolled, payments were not contingent on attending a minimum proportion of classes. Schools could report that students were no longer attending and have them dropped from the Prepa Sí beneficiary list, but this was uncommon. Instead, Prepa Sí staff asked schools to update enrollment information each semester, although it appears that most updating actually occurred between school years.

Prepa Sí's coverage rate in the population of eligible students is uncertain because both the numerator and denominator of this ratio are difficult to measure precisely. The number of beneficiaries who are actually eligible is different from the reported number of beneficiaries because, as a contemporary evaluation of Prepa Sí's implementation and the empirical section of this paper show, some ineligible students manage to receive transfers (Ligeia Ravest, Pina, and Segura 2008). The number of eligible students–those enrolled in DF high schools who

[5]This rule for determining the transfer amount results in a discontinuous change in payment amount for students near the 7.6 and 9.1 point cutoffs. I use this discontinuity to test for an effect of eligibility for a higher transfer amount on high school completion in Appendix C, but the estimated confidence intervals are quite wide and include zero.

also reside there–is also unknown because the annual school census does not disaggregate school enrollment by state of residence. I estimate the coverage rate for eligible students to be 68.6% in the 2007-2008 school year.[6] This rate appears to be relatively stable during the period of study.[7] Incomplete take-up could be due to a number of factors: dropping out early in the year prior to signing up, being unaware of the program or having trouble signing up, or receiving a federal CCT that (officially) precluded the student from enrolling in Prepa Sí.

Prepa Sí was introduced into an environment where other federal transfer programs already existed and were expanding, but their coverage in the DF was low compared to Prepa Sí. The flagship national anti-poverty CCT program, called Oportunidades at the time under study here, had already expanded to poor urban municipalities (Behrman et al. 2012). But it left students attending DF high schools essentially uncovered, as will be demonstrated below for the sample of interest. The Secretariat of Public Education also operated transfer programs during the period of study, although the coverage of these programs was low compared to Prepa Sí. Prior to the 2007-2008 school year, much of the federal CCT budget for public high schools in urban areas was dedicated to merit-based scholarships for students with exceptionally high grade point averages. Coverage was low: in the 2006-2007 school year, 2,438 scholarships were budgeted for students in DF high schools, compared to a student population of about 330,000—a coverage rate of 0.7 percent (Secretaría de Educación Pública 2006).

The Secretariat of Public Education rolled out what would eventually be called the nationwide High School Scholarship Program (PROBEMS) during the 2007-2008 school year. PROBEMS subsumed the federal merit-based scholarship program and introduced additional CCT components intended to serve high-need students without access to Oportunidades. The timing coincided with Prepa Sí's introduction, but the design and implementation of the two programs were not coordinated and they were not intended to be complementary. The payment structure of these need-based transfers was similar to that of Prepa Sí, except that the monthly amounts were slightly higher on average (625 to 790 pesos in most cases), did not depend on grade point average, increased slightly with grade progression, and favored

[6] This coverage rate is estimated in the following way. First, I estimate the number of students in DF high schools who are DF residents by computing the proportion of students assigned to DF high schools who were residents in the 2007 COMIPEMS cycle (72.4%). Second, I multiply this number by the total reported enrollment in public DF high schools in the 2007-2008 school census (342,536), forming the denominator. Third, I match the 2007-2008 Prepa Sí beneficiary list to the 2007 COMIPEMS cohort and find that 90.6% of the matched beneficiaries resided in the DF when registering for the COMIPEMS exam. Multiplying this residency rate by the number of beneficiaries (187,665) forms the numerator.

[7] The coverage rate cannot be computed for 2008-2009 and onward because of Prepa Sí's addition of transfers for first-year university students, who are not reported separately. The empirical analysis in this paper indicates that coverage rates in the 2007-2008 and 2008-2009 cycles were very similar.

girls over boys by 50 pesos. Students attending approved public high schools could apply for this program through an online portal, which asked them to self-report information about household structure, income, and physical assets. Transfers were then allocated to students on the basis of a proprietary weighting formula. Because all information was self-reported and students knew that financial need determined priority, it is likely that PROBEMS was imperfectly targeted. Students needed to reapply for the program each year.

PROBEMS covered about 8% of public high school students nationally in the 2007-2008 school year, a rate that had only increased to 9% by 2010-2011 (Secretaría de Educación Pública 2014). Estimated initial coverage rates were similar for public high schools in the DF (11%) and EdoMex (13%). Thus while PROBEMS represented an increase in federal educational CCT support during the time that Prepa Sí was introduced, this expansion affected a relatively small proportion of the student population. Prepa Sí failed to enforce its stated rule of excluding students receiving federal transfers, including PROBEMS, in the 2007-2008 school year. PROBEMS did not disqualify Prepa Sí recipients because it did not have access to the beneficiary list (Universidad Autónoma Metropolitana 2008). As a result, significant "double-dipping" was possible in that year and noted anecdotally by school directors (Ligeia Ravest, Pina, and Segura 2008). This issue was rectified in subsequent years of the program, which was reflected in a decrease in the proportion of students in DF high schools who received a PROBEMS transfer to about 3.5%. While PROBEMS does not disaggregate beneficiary counts by student state of residence, this decline is roughly consistent with PROBEMS receipt among DF residents falling to near zero while remaining steady for EdoMex residents. This would reflect eligible students preferring the guaranteed Prepa Sí payments to the PROBEMS transfers that had similar monetary amounts but that were more difficult to obtain—that is, students knew that applying for PROBEMS did not guarantee selection as a beneficiary, and that awards were for a one year duration so that reapplication would be necessary for the next year.

The National School of Technical Professional Education (CONALEP), a large system of vocational high schools that operates nationwide, was essentially excluded from PROBEMS coverage. It operated its own scholarship program during the entire period under study, in which recipients were selected by a school-level committee based on need. Scholarships were paid once per semester and were lower in value than both Prepa Sí and PROBEMS, with students with grade point averages below 8.5 receiving (in 2009) about 1,100 pesos per semester (i.e. 220 per month), those above 8.5 and below 9.5 receiving about 2,200 (440 per month) and the most outstanding students receiving about 4,400 (880 per month) (Colegio Nacional de Educación Profesional Técnica 2009). Nationally, the scholarship program provided the equivalent of year-long awards to 7% of their student population in 2007, increasing somewhat to 10% by 2012 (Colegio Nacional de Educación Profesional Técnica 2007, 2012).[8] A different system of schools that operates exclusively in the Mexico City metropolitan area, the National Polytechnic Institute (IPN), was covered by PROBEMS but also offered scholarship support under a wide variety of programs during this time period. Scholarship amounts varied by program; they covered approximately 9% of students in 2007 and had increased coverage to 13% by 2012 (Instituto Politécnico Nacional 2009, 2012). IPN rules prohibited students from simultaneously receiving federal support and an IPN scholarship.

Because both CONALEP and IPN scholarships were allocated internally rather than through a centralized system, it is possible that scholarship committees in DF high schools belonging to these systems reacted to the introduction of Prepa Sí by reallocating scholarships from DF students to EdoMex students, who were ineligible for Prepa Sí. The empirical analysis will explicitly address this possible positive spillover onto Prepa Sí-ineligible students.

3 Empirical strategy

This section describes the main empirical approach taken by the paper: a difference-in-differences design comparing multiple cohorts of similar DF and EdoMex residents. It then details the use of a similar design to estimate spillovers onto ineligible students, as well as two complementary empirical approaches. The first uses admission cutoffs to generate exogenous variation in DF school assignment. The second uses variation in costs of attending different DF high schools to test for differential effects of Prepa Sí on students facing different costs. Finally, the strategy for estimating effects on school choice is discussed.

3.1 Estimating the effect of Prepa Sí eligibility

The primary goal of this paper is to estimate the effect of Prepa Sí eligibility on the probability of completing high school. Eligibility is not randomly assigned, but the straightforward program rules governing eligibility make it possible to identify students who are almost certainly eligible, because they both live in the DF and have been assigned to a high school there. It is also possible to identify a set of students assigned to DF high schools who are almost certainly *ineligible* because they live in the adjacent EdoMex. Thus the cross-sectional comparison will be between eligible and ineligible students within the same high school. The student-level microdata contain cohort-level variation in exposure to Prepa Sí

[8] CONALEP reports the number of semester-long scholarships rather than number of beneficiaries. Year-long equivalent coverage is computed as (number of semester-long scholarships) / (2 × enrolled students).

as well: the 2005 cohort was minimally exposed to the program because they were already in their third year of high school when it was introduced, and 87% of cohort-level dropout has already taken place after two years of high school, as explained in the previous section. Thus we expect a near-zero impact of Prepa Sí on the 2005 cohort and consider them as the pre-treatment cohort.[9] The 2006 cohort was exposed beginning in its second year of high school, while the 2007 and 2008 cohorts in the data were exposed for the duration of high school, including the crucial first year (10th grade) where dropout is most pervasive. The primary empirical approach is a difference-in-differences design where, within each DF high school, the between-cohort changes in outcomes of DF residents are compared to those of EdoMex residents. If Prepa Sí eligibility affects student outcomes, then these outcomes should evolve differently between DF and EdoMex students when moving the 2005 cohort to *partial* exposure (2006) and then *full* exposure (2007-08) cohorts. These definitions of exposure will be used throughout the paper.

Because the data do not contain multiple pre-Prepa Sí cohorts, a potential threat to this empirical strategy is that even within a high school, the outcomes of DF and EdoMex students were trending differently in this period. Furthermore, geographically localized shocks in the DF or EdoMex could coincide with Prepa Sí's introduction. In order to mitigate these related concerns, I limit the sample to focus on students residing in postal codes that are close to the DF-EdoMex border. Figure 1 illustrates this strategy. Because the measurement of distance from student residence to the border is necessarily from the postal code centroid and not the precise location of the student's home, conservative definitions of "close" tend to exclude geographically large postal codes even when they are contiguous to the border. Examining the map, a reasonable trade-off between inclusion of relevant postal codes and exclusion of areas far from the border is 3 kilometers. Robustness to other choices of "close" will be shown as well.

Beyond limiting the sample geographically, I group postal codes on each side of the border by dividing the border into segments of equal length. Each postal code is assigned to the segment that is closest to its centroid. Segment-by-cohort fixed effects are included in the regression to account for the effects of any localized shocks. These shocks could either be economic or changes in unobserved demographic composition of the students themselves. Segments 5 kilometers in length are used in the body of the paper, but results are highly robust to changes in segment length, as will be shown. The estimated specification is as

[9] The graduation proxy used in this paper is only available beginning in 2008, which corresponds to the 2005 COMIPEMS cohort.

follows:

$$y_{ipbmst} = \alpha_p + \mu_m + \nu_{st} + \gamma_{bt} + \boldsymbol{X}_i\boldsymbol{\beta} + \delta_1 \left(DF_p \times partial_t\right) + \delta_2 \left(DF_p \times full_t\right) + \varepsilon_{ipbmst}, \quad (1)$$

where i indexes the student, p is the postal code of residence, m is the middle school attended, b is the closest border segment, s is the high school to which the student was assigned, and t is the cohort. The outcome of interest, y, is high school completion in most cases. In addition to postal code and middle school fixed effects, the model includes fixed effects to account for high school-by-cohort shocks that affect DF and EdoMex students similarly, as well as fixed effects allowing for border segment-cohort shocks that affect students on both sides of the border segment similarly. A vector of covariates $\boldsymbol{X}_i$ controls for student-level characteristics. The corresponding coefficient vector $\boldsymbol{\beta}$ is allowed to vary by cohort in a robustness check. The coefficients of interest are δ_1 and δ_2, which give the average effect of Prepa Sí eligibility on DF residents ($DF_p = 1$) in the partially and fully-exposed cohorts, respectively. Standard errors are clustered on two dimensions: the assigned high school s and the student's middle school m.[10]

3.2 Addressing spillovers

A further potential concern with this strategy is that the existence of Prepa Sí led to spillovers onto EdoMex residents who were attending DF high schools with eligible students. For example, CONALEP and IPN schools may have reallocated their institutional scholarships toward EdoMex students, or Prepa Sí may have resulted in higher attendance by DF students, causing congestion.[11] Such spillovers could bias the estimated effect of Prepa Sí either positively or negatively. In order to assess the possibility that such spillovers exist and are meaningfully large, I apply a similar difference-in-difference strategy to compare EdoMex residents who reside in the same postal code as each other but attend high schools in either the DF or EdoMex:

$$y_{ipmst} = \alpha_{pt} + \mu_{mt} + \nu_s + \boldsymbol{X}_i\boldsymbol{\beta} + \delta_1 \left(DFHS_s \times partial_t\right) + \delta_2 \left(DFHS_s \times full_t\right) + \varepsilon_{ipmst}. \quad (2)$$

This specification includes high school fixed effects and allows for postal code-by-cohort and middle school-by-cohort shocks. The parameters of interest, δ_1 and δ_2, give the difference-in-differences effects of Prepa Sí on EdoMex students who are assigned to a high school within

[10]Clustering on these dimensions results in slightly more conservative standard errors than two-way clustering on high school and border segment or high school and postal code.

[11]To address directly the possible bias from scholarship reallocation in CONALEP and IPN schools, the main effects will be re-estimated on a subsample that excludes these schools.

the DF ($DFHS_s = 1$), compared to residents of the same postal code who are assigned to EdoMex schools. Dropping the student demographic vector $\boldsymbol{X}_i$ and using these characteristics as outcome variables, the δ coefficients are then informative about whether selection of students into DF schools was changing over this period. This allows us to assess whether the Prepa Sí effects estimated in Equation 1 are likely to be influenced by trends in selection of the EdoMex control group.

A complementary empirical approach using difference-in-discontinuities to estimate both the direct and spillover effects of Prepa Sí is explained and implemented in Appendix D. This design relies on the fact that the COMIPEMS assignment mechanism results in cutoff exam scores for each oversubscribed school, generating variation in school assignment for students with scores near the cutoff. Some students' choice lists are such that a more-preferred school is in the DF and a less-preferred school is in the EdoMex, or vice versa, so that whether they attend a DF high school depends on whether they meet the cutoff score. Differencing the regression discontinuity effects of DF high school admission between cohorts gives an estimated impact of Prepa Sí students near this assignment margin. While this approach benefits from a clear source of exogenous variation in high school location, it produces estimates that are less precise and that are specific to the subsample of students whose school preferences and scores place them near the threshold between DF and EdoMex high schools.

3.3 DF resident difference-in-differences

To further investigate the possible role of Prepa Sí in reducing dropout among students with relatively high costs of attending high school, a complementary empirical strategy using only DF students is employed. This continuous difference-in-differences approach estimates the differential change in completion rates over time with respect to the assigned high school's annual fees and its distance from the student's home. If Prepa Sí induces students with high costs of attendance to remain in school, then students at higher-fee and more-distant high schools should have a relatively larger increase in completion rates post-Prepa Sí than otherwise similar students attending less-expensive, closer schools. The basic specification is:

$$\begin{aligned} y_{ipmst} = \alpha_{pt} + \mu_{mt} + \nu_s + \boldsymbol{X}_i \boldsymbol{\beta} + \theta cost_{pst} + \tau_1 partial_t + \tau_1 full_t \\ + \gamma_1 \left(partial_t \times cost_{pst}\right) + \gamma_2 \left(full_t \times cost_{pst}\right) + \varepsilon_{ipmst}, \end{aligned} \tag{3}$$

where $cost_{pst}$ is either annual attendance fees of school s in cohort t's first year or distance from postal code p to school s. This specification controls for postal code-by-cohort, middle school-by-cohort, and high school fixed effects, as well as student covariates. The identifying assumption is that, in the absence of Prepa Sí's introduction, completion rates would have evolved similarly between students with different levels of *cost*, conditional on the flexible postal code- and middle school-level trends. Because students attending the same high school live in different postal codes, when *cost* refers to distance from home, it is possible to augment equation 3 by including high school-by-cohort fixed effects. In this case, the identifying assumption is conditional on these flexible high school-level trends as well.

3.4 Estimating effects on school choice

Eligibility for Prepa Sí might be expected to induce changes in school choice, particularly for students who were liquidity constrained, as in Avitabile, Bobba, and Pariguana (2017). This is, in itself, a potentially important aspect of CCTs that is worthy of exploring. But school choice effects would also affect the interpretation of the estimated completion effects. The 2008 COMIPEMS cohort was aware of Prepa Sí at the time of application, and this cohort is included in the sample when estimating completion effects. Thus if Prepa Sí affected school choice for the 2008 cohort, then it may have changed the composition of DF students assigned to each school, potentially inducing changes in within-school completion rates even in the absence of a direct effect of Prepa Sí on dropout. Thus estimating choice effects is important for understanding the validity of the completion results.

Choice effects can be estimated using a strategy similar to the one used for estimating completion effects. Because data on high school completion are not needed, only the COMIPEMS data which includes students' choice portfolios, the set of years can be expanded: the 2005 through 2007 cohorts form the pre-Prepa Sí group, while the 2008 through 2010 cohorts are the post-Prepa Sí group. Postal code-specific linear time trends in choice outcomes can also be included in the specification:

$$y_{ipbmst} = \alpha_p + \mu_m + \gamma_{bt} + \kappa_p t + \boldsymbol{X}_i \boldsymbol{\beta} + \delta \left(DF_p \times post_t \right) + \varepsilon_{ipbmst}. \tag{4}$$

where $post_t$ is a dummy variable equal to 1 for cohorts 2008 through 2010. The coefficient δ gives the estimated effect of Prepa Sí's introduction on the specified school choice outcome, accounting for both flexible border segment trends and linear postal code-level trends that allow postal codes belonging to the same border segment to follow different time paths.

4 Data

The empirical strategy is implemented using matched data from four sources. The first is a database of all COMIPEMS participants from the years 2005 through 2010. The 2005-2008 cohorts are used for the graduation and test score analyses, while 2005-2010 cohorts are used when assessing the effects of Prepa Sí on school choice. This database provides the independent variables for the analysis. It includes each applicant's state and postal code of residence, which I match to geographic coordinates (for the centroid of the postal code) to determine distance from the DF border. Also included for each applicant are the full list of school preferences, COMIPEMS exam score, and assigned high school from the automated assignment process. Middle school grade point average and sex are available for all students in the sample. Most students turned in a demographic survey as well, which contains parental education and family income, among other measures. Family income is likely quite noisy because it is estimated by the student filling out the survey.

Data on take-up of Prepa Sí come from public beneficiary lists made available by the DF government for each school year. These are matched to the COMIPEMS database using full student names (i.e. two family names and all given names). Full names are unique within a cohort in 98% of cases and thus provide a good basis for forming reliable matches. To assess the viability of matching on the basis of names, I matched the 2007-8 and 2008-9 Prepa Sí beneficiary lists back to the 2004 through 2008 COMIPEMS databases, which resulted in a 94% match rate. Given that 7% of Prepa Sí beneficiaries in 2007-8 were enrolled in non-traditional high school options outside of the COMIPEMS system (Ligeia Ravest, Pina, and Segura 2008), and thus may not have taken part in the COMIPEMS process, the true match rate among students in COMIPEMS-participating schools almost certainly exceeds 94%.

Data on high school fees come from an annual school census. These data are reported by an administrator for each school and include basic fees and costs for required materials and uniforms. These fees are not uniform across schools, even within a subsystem. In particular, technical and vocational schools may have quite different fees to cover trade-specific materials (Avitabile, Bobba, and Pariguana 2017).

Finally, a proxy for high school completion is obtained from the 2008 through 2012 databases of students taking the 12th grade ENLACE exam, which is a low-stakes standardized test in math and Spanish applied to students who are in their final year of high school. While this does not provide a perfect individual-level measure of graduation, Avitabile and de Hoyos (2018) and Dustan, de Janvry, and Sadoulet (2017) show that it tracks graduation closely. These data are matched to the COMIPEMS database using national identification numbers. Matching 2005-2008 COMIPEMS cohorts with 2008-2012 ENLACE data captures students from 2005-2007 cohorts who took up to five years to graduate and students from the 2008 cohort who took up to four years to graduate. Few students graduate after more than four years of entering high school (recall that high school consists of three grades). The ENLACE test scores themselves are also used as an outcome in the analysis, when assessing whether Prepa Sí had an effect on learning. A random subset of schools were selected to give a supplementary questionnaire to all students taking the 12th grade ENLACE. The questionnaire data are used to investigate whether, conditional on not dropping out before taking the ENLACE, self-reported student behavior changed as a result of Prepa Sí eligibility.

Several sample restrictions are imposed for both conceptual and data availability reasons. First, only COMIPEMS-takers who are completing middle school at the time of participation are included. This excludes middle school graduates and older adults, who are less likely to actually enroll in high school after the competition. Second, students attending private middle schools are excluded because they often go to a private high school even after participating in the COMIPEMS assignment process, which results in higher apparent dropout among this group in the data because some private high schools do not administer the ENLACE.[12] Third, only students assigned to a school during the automated assignment process, rather than being left unassigned, are included because I do not observe whether and where unassigned students enrolled. Fourth, one of the two elite high school subsystems, the Universidad Nacional Autónoma de México (UNAM), is officially autonomous of the government and chooses not to administer the ENLACE. Thus students assigned to an UNAM high school are excluded. Finally, a small number of students choose and are assigned to a late-entry track at a set of schools in the Colegio de Bachilleres (COLBACH) subsystem, which is a group of non-elite high schools that follow a traditional curriculum. These late-entry students take the ENLACE at a rate close to zero in most years, indicating an administrative decision by the schools not to administer the exam to them. They are excluded.

Table 1 summarizes key variables for COMIPEMS cohorts 2005 through 2008 belonging the three groups relevant to the main analysis: DF residents assigned to DF schools (i.e. Prepa Sí eligible from 2007 cohort onward), EdoMex residents assigned to DF schools (the control group), and EdoMex residents assigned to EdoMex schools (the group used to assess spillover effects). All groups are restricted to students living within 3 kilometers of the border, which is the sample selection criterion for the main difference-in-differences analysis

[12] Many private school students select only elite high schools and then remain in the private school sector if rejected from these options. Fewer than 2/3 of private high school graduates in the Mexico City metropolitan area graduated from a school that administered the ENLACE during this time period. See Appendix B, Section V of Dustan, de Janvry, and Sadoulet (2017) for more details.

reported in the paper.[13] EdoMex residents attending DF high schools are slightly positively selected on most dimensions, compared to their DF resident counterparts. The former group has 0.1 higher normalized average entrance exam score, slightly higher middle school GPA, and similar family income and parental education.[14] They are also more likely to take the ENLACE exam by 5 percentage points and have higher ENLACE exam scores. In terms of preference over schools, these groups are similar: 75% request a school belonging to an elite subsystem as their first choice and they choose schools with cutoff scores (the minimum score that can result in assignment to that school) that are more than half of a standard deviation above the student-level mean score.

Comparing columns 2 and 3, we see that EdoMex residents who select into DF high schools are different than those who stay in their state for high school. The former group has 0.44 standard deviations higher COMIPEMS exam score on average and slightly higher middle school GPA. The sex composition is also different: while 54% of students traveling to the DF for school are male in this sample, only 44% of those staying in the EdoMex are male. Those attending EdoMex high schools pay 1,303 pesos more per year in fees, on average. Close to half of this difference is explained by assignment to IPN-affiliated schools, whose fees are much lower than average and which, with one exception, are located in the DF. They also have higher family income and parental education. These groups take the ENLACE at a similar rate, but those assigned to DF schools score significantly higher on this exam.

5 Results

This section makes the empirical case that Prepa Sí eligibility had no positive impact on educational outcomes and little discernible effect on school choices. First, support is given for the validity of the within-school difference-in-differences design. Moving to the main finding, access to Prepa Sí does not appear to have had any positive effect on the probability of high school completion. This same pattern of tightly-bounded null estimates persists in several subgroups toward which a strategically-targeted CCT might be directed. Positive spillovers onto EdoMex residents are not driving the null effects, and within-DF responses to Prepa Sí are also consistent with null program effects. There are no apparent effects on average ENLACE test scores. Finally, Prepa Sí had little or no effect on DF students' school choices.

[13] The geographically unrestricted summary statistics are in Appendix Table A.1.

[14] The normalized COMIPEMS score is computed by subtracting off the mean score among *all* COMIPEMS-takers in that year and dividing by the standard deviation.

5.1 Demographics and take-up

The key assumption underlying this difference-in-differences approach is that, within a DF high school, the completion rates of DF and EdoMex residents would have evolved identically in the absence of Prepa Sí's introduction. This assumption is not testable, but we can show that the demographic composition of students from these two groups was evolving very similarly during this period. Table 2 shows the results of estimating Equation 1 on the preferred sample of students residing within 3 km of the border, where the indicated demographic characteristics in the column headers are the dependent variables (and all demographic variables are excluded as regressors).[15] The reported coefficients show how, conditioning on the high school to which students were assigned, the differences in mean characteristics between students from the two states evolved over the 2005 to 2008 sample period. The coefficients are all small and 16 of 18 are statistically insignificant at the 10% level. Thus it seems that, conditional on high school assignment, any pre-existing differences in observables between groups remained nearly constant during this period.

Estimated take-up rates of Prepa Sí for first-year high school students in this sample, based on matching COMIPEMS records to the Prepa Sí beneficiary lists, are summarized in Table 3. The results indicate that take-up was far from complete for incoming cohorts and that, in the first year, there was some leakage to EdoMex residents. The regression estimates are from cross-sectional regressions of Prepa Sí receipt on a dummy for DF residence, boundary segment and high school fixed effects, and the indicated covariates. Column 1 shows take-up in the 2007-2008 school year among students who took the COMIPEMS exam in 2007. A lack of program compliance in Prepa Sí's first year is evident: 18% of the EdoMex residents in the sample are found on the beneficiary list, despite being ineligible. While some of these matches could be due to students moving across the DF border or sharing a name with an eligible student, the fact that this 18% figure declines to only 6% in 2008 (Column 4) indicates that most of this is due to noncompliance. Within a high school and border segment in the 2007 cohort, DF students were 46 percentage points more likely than their EdoMex counterparts to be Prepa Sí beneficiaries (the estimated DF take-up rate is 64%, close to the 69% take-up rate estimated in Section 2.). This gap is robust to including demographic covariates in Columns 2 and 3.[16] The gap in take-up with respect to place of residence was larger for the 2008 COMIPEMS cohort, as shown in Columns 4 through 6. Most of this 60 percentage point difference is due to better compliance among EdoMex

[15] The observation count in the regression results differs slightly from the corresponding count in the summary statistics because singletons are dropped in the multi-way fixed effects estimation procedure, as described in Correia (2016).

[16] About 12% of observations are lost in Column 3 due to missing survey data.

students, however: the take-up rate for apparently eligible students is 66%. To conclude, while it does not appear that take-up of Prepa Sí was close to complete, in both years there were large gaps in take-up between eligible and ineligible first-year students.[17]

5.2 Effects on high school completion

Despite the fact that DF residents in this sample were much more likely to enroll in Prepa Sí, Table 4 shows that there is no evidence of effects of eligibility on high school completion. Column 1 shows estimates from Equation 1 using the preferred sample, including students within 3 kilometers of the border. The estimated impacts of Prepa Sí on the high school completion proxy, ENLACE-taking, among eligible students are slightly *negative*, -1.3 percentage points for partial exposure and -1.1 for full exposure. The 95% confidence intervals are sufficiently narrow to rule out positive effects larger than 1.1 percentage points. Conditional on assigned high school and border segment, the completion gap between DF and EdoMex residents in 2005 in this sample was just 2 percentage points, in favor of EdoMex students. The estimates suggest that this small gap persisted after Prepa Sí's introduction. Adding covariates in Columns 2 and 3 results in similar, statistically insignificant estimated impacts. Allowing covariates to have cohort-specific coefficients in Column 4 has little effect on the results.

Recall that, beginning with the 2008 COMIPEMS cohort, participating students were aware of Prepa Sí's existence and may have responded by changing their school choices and obtaining different high school assignments. This possibility will be explored empirically later in this section. In order assess whether these potential compositional changes are driving the observed completion results, columns 5 through 8 show results from a specification that allows separate effects for the always-exposed cohorts: the 2007 cohort, who could not react to Prepa Sí in making their school choices and the 2008 cohort, which could. The estimated effects on high school completion remain consistently small, negative, and insignificant, with the 95% confidence intervals ruling out effects in 2007 and 2008 larger than 0.6 and 0.7 percentage points, respectively.

These small, negative, null estimated impacts are robust to changes in the specification and sample definition. Appendix Figure A.2 shows the estimated effects of Prepa Sí for different lengths of DF border segments used for defining the border segment-by-year fixed

[17] Further evidence on the extent to which students may have "gamed" the eligibility criteria is presented in Appendix Figure A.1, which plots the (normalized) number of COMIPEMS participants by year from 2005 to 2010, separately by state. We see no stark divergence in state-specific trends following Prepa Sí's 2007 introduction. If the rate of students claiming DF residence had increased sharply, we might suspect that EdoMex students were claiming DF residence to appear eligible, despite the fact that COMIPEMS and its database in fact had no connection to Prepa Sí.

effects. The estimates correspond to the specification used in Table 4, Column 2. The point estimates are nearly identical over the range of segment lengths, and the 95% confidence intervals include 0 in every case except the 2006 cohorts for segment lengths of 9 km and 11 km. Appendix Figure A.3 shows estimated effects for different definitions of which postal codes are "close" to the DF border and thus included in the estimation sample. Again, the point estimates are essentially unchanged over the range of definitions and the 95% confidence intervals include 0.[18]

If we are willing to assume that Prepa Sí eligibility only affected high school completion through its effect on Prepa Sí take-up, instrumental variables can be used to estimate the average effect of take-up on completion. This could be a useful policy parameter for understanding the potential effects of a universal high school CCT with minimal or nonexistent barriers to enrollment. To implement the IV approach, I define a Prepa Sí take-up dummy variable equal to one if the student appears on the beneficiary list in his first year of high school. This variable is zero for all 2005 cohort students. For 2006 students this is also zero, but because these students were still partially exposed to Prepa Sí in their second year, this cohort is dropped from the sample. The two instruments are *DF* × *2007 cohort* and *DF* × *2008 cohort*. The second stage regresses ENLACE-taking on predicted take-up.[19]

Table 5 shows the results. Column 1 presents the first stage estimates for the sample limited to 2005 and 2007 cohorts, Column 2 limits the sample to 2005 and 2008 cohorts, and Column 3 includes both full exposure cohorts. As expected, the estimated effects of Prepa Sí eligibility on take-up are almost identical to the cross-sectional estimates in Table 3: 46.0 percentage points for 2007 and 59.8 percentage points for 2008. Columns 4 through 6 present the second stage estimates for the 2007 cohort, 2008 cohort, and full samples. In each case, the estimated IV effect of Prepa Sí take-up on ENLACE-taking is small, negative, and statistically insignificant. This is unsurprising, given the negative and insignificant reduced-form effects. The 95% confidence intervals rule out large positive effects of take-up: for the full sample (Column 6), the upper bound of the 95% confidence interval is 0.7 percentage points.

In contrast to many CCT and scholarship programs, Prepa Sí was untargeted with respect to socioeconomic characteristics and academic performance. In principle, it is possible that there were positive effects for subgroups who had relatively large proportions of students

[18] Removing the small proportion of students who reported receiving Oportunidades benefits in middle school produces almost identical results in Appendix Table A.2, as expected given the low rate of Oportunidades participation. Including middle school-by-cohort fixed effects increases standard errors on Prepa Sí eligibility by more than 50% because the vast majority of students attend middle school in their state of residence, but point estimates remain close to zero and statistically insignificant, as reported in Appendix Table A.3.

[19] The second stage is $ENLACE_{ipbmst} = \alpha_p + \mu_m + \nu_{st} + \gamma_{bt} + \boldsymbol{X}_i\boldsymbol{\beta} + \delta \widehat{take\text{-}up}_i + \varepsilon_{ipbmst}$.

near the margin of dropout and who would be induced to complete school by Prepa Sí. To investigate this possibility, Table 6 estimates the effects for groups that the program could in fact target using existing administrative data. Column 1 replicates the full-sample result from Column 2 of Table 4, for comparison. Columns 2 and 3 show no evidence of positive effects for either boys (who should arguably be targeted because they face a greater pull from the labor market) or girls (who might be targeted for reasons such as incentivizing delays in pregnancy). Like the full sample results, estimates are consistently negative and rule out modest positive effects on completion.

Targeting on other dimensions does not seem promising, either. Students without a parent who graduated high school and students whose self-reported family income is 3,000 pesos a month or less (below the median) show no evidence of positive impacts from Prepa Sí. Nor do students with middle school grade point averages of 8 and above, which puts them in the top half of the GPA distribution. Thus it seems that targeting on observable characteristics while keeping the present transfer amount is unlikely to result in a program with positive average effects on high school completion among the beneficiary population. We cannot infer from these results, however, whether or how much maintaining the program budget constant and allocating larger transfer amounts to targeted individuals would improve the effectiveness of the program.

5.3 Are null completion effects driven by federal CCTs?

A potential concern when interpreting the null effect of Prepa Sí on high school completion is that federal educational CCTs served as substitutes. As explained in Section 2.3, Oportunidades had already undergone its urban expansion during this sample period, and the PROBEMS program rolled out at the same time as Prepa Sí. If a large proportion of EdoMex students attending DF high schools received these transfers, then the null effect simply indicates that Prepa Sí was no more effective at reducing dropout than relatively similar programs administered by the federal government. While I do not have access to administrative microdata on federal program participation during this period, this section shows that even under extreme assumptions about the proportion of beneficiaries who resided in the EdoMex, Prepa Sí resulted in a large increase in transfer receipt among DF residents.

Coverage of Mexico's flagship CCT, Oportunidades, was negligible in this sample, both in the DF and EdoMex. This is seen both in the COMIPEMS microdata and in contemporary Secretariat of Social Development documents pertaining to educational outcomes of Oportunidades beneficiaries. The COMIPEMS demographic survey asks students if they have received Oportunidades during middle school, a good proxy for whether they are eligible for benefits in high school. Table 7 shows the evolution in the conditional Oportunidades receipt gap, as estimated using Equation 1. In 2005, self-reported coverage among DF students in the sample was essentially 0%. Self-reported coverage was just 1 percent for students residing in the EdoMex. This 1 percentage point gap did increase for later cohorts, but only slightly, by less than one percentage point. These persistently small gaps are robust to including covariates in Columns 2 and 3. Similar to these results, Mancera Corcuera, Priede Schubert, and Serna Hernández (2012) report that 0.9% of ENLACE-takers in 2008 (corresponding mostly to the 2005 COMIPEMS cohort, which was minimally exposed to Prepa Sí) in DF high schools were Oportunidades recipients, a number that rose to just 1.6% in 2011 (corresponding mostly to the 2008 COMIPEMS cohort, which was fully exposed).[20]

Under the extreme assumption that *all* Oportunidades beneficiaries in DF high schools in 2008 were DF residents, the upper bound on Oportunidades coverage among DF residents in the 2008 ENLACE is 1.3% (0.9% total coverage / 71% DF resident representation among DF high school ENLACE-takers). Making the opposite extreme assumption for 2011 (that all Oportunidades beneficiaries in DF high schools were EdoMex residents), this upper bound is 5.1% (1.6% / 32%). The sum of these two figures, 5.1 + 1.3 = 6.4 percentage points, is an upper bound estimate of how much Oportunidades coverage could have increased in DF high schools for EdoMex residents compared to DF residents.

A similar exercise can be performed for PROBEMS, which began its rollout in the same year as Prepa Sí. A quarterly report from PROBEMS tabulates the number of current beneficiaries by state of high school attended, as of December 2009 (Subsecretaría de Educación Media Superior 2009). This is a time period by which "double-dipping" would have been eliminated, so that PROBEMS beneficiaries likely were not receiving Prepa Sí. Combining the count of beneficiaries attending DF high schools with a later summary on total beneficiary count for the 2009-10 school year, I estimate a 5.5% coverage rate for students in DF high schools.[21] Suppose that all of these beneficiaries are EdoMex residents, meaning that no DF resident chose to apply to and receive PROBEMS. This would imply a coverage rate of 19.8% for EdoMex students in DF high schools, representing an upper bound on the extent to which PROBEMS increased EdoMex resident transfer receipt compared to DF residents.[22]

[20] Mancera Corcuera, Priede Schubert, and Serna Hernández (2012) matched the ENLACE database to Oportunidades beneficiary rolls and identified beneficiaries as those who appeared on the rolls at least once during the school year in which the test was administered.

[21] Of the 283,850 beneficiaries in the PROBEMS report, 18,648 (6.6%) attended DF high schools. The report has a lower beneficiary count than the total number of beneficiaries reported for the 2009-2010 school year, 298,100, because some students are added and dropped from the beneficiary list throughout the year. I thus estimate the total number of beneficiaries in DF high schools to be $298,100 \times (18,648/283,850) = 19,584$ students. The total number of students enrolled in DF high schools in the 2009-10 school year was 358,651, so $19,584/358,651 = .055$.

[22] For the 2007 COMIPEMS cohort, 27.6% of students assigned to DF high schools were EdoMex residents,

This upper bound is likely far too conservative: the estimated PROBEMS coverage rate in EdoMex high schools during this same year was 10.1%, about half of the upper bound.

Under the given extreme assumptions on the evolution of Oportunidades and PROBEMS coverage rates, these federal programs would have increased the CCT coverage gap between EdoMex and DF residents in DF high schools by 26.2 percentage points. The Prepa Sí take-up rate for the 2008 COMIPEMS cohort was estimated to be 59.8% higher for DF residents in the analysis sample, as reported in Table 3.[23] Assuming that the 26.2% federal program gap applies to the analysis sample, the combined effect of Prepa Sí and both federal programs was a $59.8 - 26.2 = 33.6$ percentage point increase in CCT coverage for Federal District residents compared to EdoMex residents. Prepa Sí thus represented a significant increase in the availability of CCTs for high school students, even under conservative assumptions about other programs.

How does this conservative estimate of Prepa Sí's impact on the probability of CCT receipt change our conclusion about the effect of CCTs on high school completion? Again focusing on the 2008 cohort, recall from Column 5 of Table 5 that the IV estimate of Prepa Sí take-up's effect on ENLACE-taking is -1.5 percentage points ($SE = 0.0138$). Because there is one endogenous regressor (take-up) and one instrument (*DF* × *2008 cohort*), this estimate is the ratio of the reduced-form effect of eligibility and the first stage effect of eligibility on take-up (59.8 percentage points). If we instead assume that DF students in the 2008 cohort were only 33.6 percentage points more likely to benefit from a CCT, then rescaling the IV estimate by .598/.336 gives an estimated completion effect of CCT take-up of -2.7 percentage points. Maintaining the standard error from the IV coefficient, the 95% confidence interval for the effect of CCT take-up on high school completion is $[-5.4, 0.0]$ percentage points.

5.4 Assessing spillover effects on EdoMex residents

This section shows that the null estimated effects of Prepa Sí are unlikely to be driven by spillovers between DF and EdoMex high school peers. Table 8 reports estimates from Equation 2 comparing completion rates between EdoMex residents in the same postal code but assigned to high schools in different states. Column 1 shows that ENLACE-taking evolved almost identically between the two groups as Prepa Sí was introduced. For fully-exposed cohorts, the estimated effect of exposure to Prepa Sí is -2 percentage points and the 95% confidence interval rules out positive effects larger than 1 percentage point. If

a rate that is similar across cohorts. So 5.5%/27.6% = 19.8%.

[23] The estimated difference was 46.0% for the 2007 cohort, but this is the cohort for which receiving both Prepa Sí and PROBEMS was possible. I focus on the 2008 cohort here because it corresponds more closely to the federal program take-up bounds estimated above.

there were positive spillovers of Prepa Sí within DF high schools, we would expect to see positive estimated effects of DF assignment in the period following Prepa Sí's introduction. Columns 2 through 8 show that demographics evolved similarly between students assigned to schools in each state. The only statistically significant differences are small: the proportion of these students requesting elite schools as their first choice trended down slightly (between a 2 and 3 percentage point decline compared to the trend for students remaining in the EdoMex). Appendix Table D.1 and Appendix Figure D.1 present the estimated spillover (and direct) effects from the difference-in-discontinuities analysis described in Section 3.2. These estimates are less precise than the difference-in-differences estimates. We fail to reject the null of no Prepa Sí effect, except for a negative estimated spillover effect for fully-exposed students when a very wide bandwidth is used.

To explore whether the estimated Prepa Sí effects on DF students is biased downward due to reallocation of institution-level scholarships in the subsystems where such scholarships are relatively prevalent, Appendix Table A.4 replicates the DF-EdoMex resident comparison from Table 4 while omitting students assigned to CONALEP and IPN schools. Removing these subsystems should increase the estimated impact of Prepa Sí if this positive spillover mechanism was operating in the excluded schools. On the contrary, the estimated effect of partial exposure becomes more negative by 1.1 percentage points and, in specifications including covariates, reaches statistical significance at the 10% or 5% level. The estimated effects for the fully exposed cohorts remain stable near -1 percentage point and are statistically insignificant. It is unclear why the partial exposure coefficient is statistically significantly negative, although it is worth noting that I find that the difference between partial exposure coefficients for the CONALEP-IPN sample and its complement is statistically insignificant ($p = 0.23$) under the specification in Column 2 (full results not reported here).

5.5 Within-DF differential impacts

High school completion among DF residents facing longer commutes and higher school fees did not improve relative to their peers facing lower costs of attendance. Table 9 reports the results of estimating Equation 3 for the sample of students residing and attending school in the DF. Columns 1 and 2 show that higher-fee students in more-exposed cohorts did not fare significantly better than their neighbors attending lower-fee schools. The point estimates are very close to zero and, noting from Appendix Table A.1 that the student-level standard deviation of fees in this sample is about 2,200 pesos, the 95% confidence interval rules out differential effects of full exposure larger than 1 percentage point for students with a 1 standard deviation difference in fees. Columns 3 through 5 show that commute distance-based heterogeneous responses to Prepa Sí are small. Given the 4.6 km standard deviation in commute distance, even the 95% confidence interval for the positive estimate of full exposure (Column 4) rules out differential effects greater than 2 percentage points for students with a 1 standard deviation difference in distance. Adding high school-by-cohort fixed effects results in zero estimated effect and allows us to rule out differential effects greater than 1 percentage point for such students. Appendix Tables A.5 and A.6 show similar patterns of small, null results among children of low-educated and low-income parents, casting further doubt on the role of Prepa Sí in overcoming a liquidity constraint for students with high costs of attendance and low resources.

5.6 Effects on test scores and other end-of-high school measures

Just as there is no evidence for a high school completion effect of Prepa Sí, this section shows that the CCT likely had no appreciable impact on learning among inframarginal students. For these students, additional income might be expected to lead to better academic performance by reducing hours worked in the labor force or increasing expenditure on educational inputs. Table 10 shows estimates from the within-school difference-in-differences specification (Equation 1) with ENLACE score as the dependent variable. Panel A shows results for the math portion of the exam, while Panel B shows results for Spanish. Column 1 presents estimates for the full sample. In both subjects, there is no evidence for a positive effect of Prepa Sí eligibility. The 95% confidence interval for full exposure rules out effects larger than 0.04 standard deviations for math and 0.06 standard deviations for Spanish. Columns 2 through 6 show estimated impacts for various subgroups, again finding no evidence of positive impacts, although the confidence intervals include large effects in some cases. Appendix Table A.7 disaggregates the full exposure group into 2007 and 2008 cohorts, again showing no evidence of an effect for any cohort. Thus it seems unlikely that there were meaningful inframarginal impacts on students who would have stayed in school regardless of whether Prepa Sí were available.

A randomly-selected subset of high schools administers a questionnaire to ENLACE-takers, allowing for further comparisons of outcomes among inframarginal students. While the set of questions varies over time, I have consistent data for ENLACE years 2008 through 2011 on whether students work, how much they study, and their cumulative grade point average. Estimated effects of Prepa Sí on these outcomes, presented in Table 11, provide no support for positive effects on student effort or reductions in labor force participation. Column 1 shows no statistically significant change in the number of days per week on which the student does homework, with the 95% confidence interval ruling out changes larger than

0.15 days for fully exposed cohorts. Column 2 shows marginally significant *negative* effects on hours spent per day doing homework, but these reductions are small, less than six minutes. Estimated effects on grade point average in Column 3 are small and insignificant, ruling out increases larger than 4% of a standard deviation. Columns 4 and 5 show no evidence of a reduction in the probability of current employment or of having worked for more than a year at a current job, but we cannot rule out either modest positive or negative effects on these outcomes. In sum, there is evidence that Prepa Sí did not increase students' academic effort and a lack of evidence on labor market participation.

5.7 School choice effects

It appears that the school choice effects of Prepa Sí were quite modest for the eligible population. Table 12 presents the estimates of Equation 4 on the pre- (2005-7) and post-introduction (2008-10) cohorts.[24] Panel A shows the average effects on characteristics of the student's first choice school. Estimates from a model without postal code time trends (Columns 1 through 4) suggest no meaningful or statistically significant change in terms of annual fees (6 pesos/year, less than 1% of a standard deviation of the outcome variable in 2007, with the 95% confidence interval ruling out changes larger than 2.6% of a standard deviation); a slight, statistically significant shift toward *closer* schools (0.11 km or 2.2% of a standard deviation closer); a small but statistically significant 1 percentage point decrease in the probability of choosing a school in the DF; and a marginally significant 0.02 standard deviation increase in normalized school cutoff score. Columns 5 through 8 add postal code-level linear time trends to allow for pre-existing trends in choices unrelated to Prepa Sí's introduction and that are not common to all postal codes in a border segment. Adding these trends changes the point estimates little: the effect on distance becomes marginally significant, while the DF and cutoff score effects become statistically insignificant.

Going beyond effects on students' first choice, which are usually elite schools, Figure 2 shows the estimated effects separately for students' first five choices and the school to which they are actually assigned (conditional on being assigned during the computerized assignment process). Panel A corresponds to the full choice sample in Table 12 and includes linear time trends, as in columns 5 through 8. As with the case of the first choice, it is possible to rule out meaningfully large increases in demand for more expensive, distant, or more competitive schools in choices further down the list. This is the case as well for the assigned school (regression estimates also given in Table 12, Panel B), although DF students are on average assigned to schools with a small but statistically significant 3% of a standard

[24]Appendix Table A.8 provides summary statistics for this sample.

deviation higher cutoff score after Prepa Sí's introduction.

This analysis is repeated for students from less-educated and lower-income households in Figure 2 Panels B and C, respectively.[25] To the extent that these families are particularly liquidity-constrained, they should display changes in demand toward more expensive and distant schools. Instead, the pattern of results is similar to those from the full sample. Thus while Avitabile, Bobba, and Pariguana (2017) point to binding liquidity constraints affecting school choice, there is little evidence for this behavior in the population served by Prepa Sí. One key difference between Oportunidades—the program examined in Avitabile, Bobba, and Pariguana (2017)—and Prepa Sí is that while most Oportunidades beneficiary families have already been receiving benefits for some time by the end of middle school, Prepa Sí was a new program that, by design, did not make transfers prior to high school. If potential Prepa Sí beneficiary families were uncertain about their future eligibility or the amount and timing of transfers, or if they simply were unaware of the program during middle school, their school choice response would be muted compared to the better-understood Oportunidades program. The findings in Avitabile, Bobba, and Pariguana (2017) thus suggest some caution in interpreting the Prepa Sí results as ruling out the possibility for a differently-designed transfer program to affect school choice. A simpler possibility is that Oportunidades beneficiaries are much poorer than the students whom I identify as "poor" using the self-reported income in the COMIPEMS questionnaire and are thus more likely to be liquidity-constrained, but I do not have the requisite data to explore this possibility.

6 Conclusion

Why was Prepa Sí ineffective at increasing graduation rates, even in subpopulations that might be served by a targeted intervention? While we do not observe variation in program characteristics over time or across students that allows for estimating their importance, we can consider a number of potential factors. One is the amount of the transfer, which is similar to the average level of school fees and transportation costs, but small compared to the opportunity cost of foregoing labor earnings. The average urban teen wage, including formal and informal sector work, in Mexico in 2011 was about 25 pesos/hour (Instituto Nacional de Estadística y Geografía 2011). This puts the value of the transfer at between 20 and 28 hours of wages, which may be too little to discourage dropout. A sharp increase in the amount of the transfer would be expensive and would need to be offset by decreasing the number of beneficiaries through targeting of individuals expected to be at the margin of dropout. The availability of outside options in the labor market is likely to be a general

[25] Table 12 is reproduced for these groups in Appendix Tables A.9 and A.10.

problem for urban educational CCT programs for teenagers.

Another possible issue is that the transfer is made directly to the student, rather than a parent. While this design aspect may be positive if students are more likely to spend the transfer on school expenses than their parents, it does not incentivize parents to monitor their children and enforce enrollment and progression through high school.

The lack of active monitoring of attendance by schools is another potential factor, as students are in many cases able to enroll and then collect payments until the end of the semester or beyond. This is a puzzling design choice, given that the high schools participating in this program are geographically concentrated and relatively few in number, facilitating cost-effective monitoring that would pay for some of its costs by dropping absentee students. An alternative would be an approach similar to Barrera-Osorio et al. (2011), who find that delaying transfers until re-enrollment for the next year or making them contingent upon graduation are both effective in raising attendance. It is worth noting, however, that there is only weak evidence that any of these variations increased high school graduation rates among students who received the intervention while in high school.

It may also be that Prepa Sí is able to increase enrollment and attendance, but that completion effects are elusive because graduation requires students to fulfill specific academic requirements beyond attending school, such as passing a set of required courses. Unfortunately, the administrative data used in this paper do not allow me to estimate effects on these intermediate outcomes. While there is likely some value in programs that induce students to delay dropout, de Hoyos, Popova, and Rogers (2016) point out that the lack of a high school credential has important negative consequences in the labor market and precludes advancement to higher education either directly after high school or in the future.

Mexico is now instituting an untargeted high school CCT at the national level, with characteristics quite similar to Prepa Sí. The evidence in this paper suggests that such a program, by itself, is unlikely to be effective in increasing graduation rates or improving academic outcomes as reflected in exam scores. If the goal is indeed facilitating high school completion, then further experimentation with programs combining targeted financial support with complementary interventions aimed at helping students to make progress toward fulfilling graduation requirements appears necessary.

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8 Figures

Figure 1: Public high schools and postal codes in COMIPEMS region

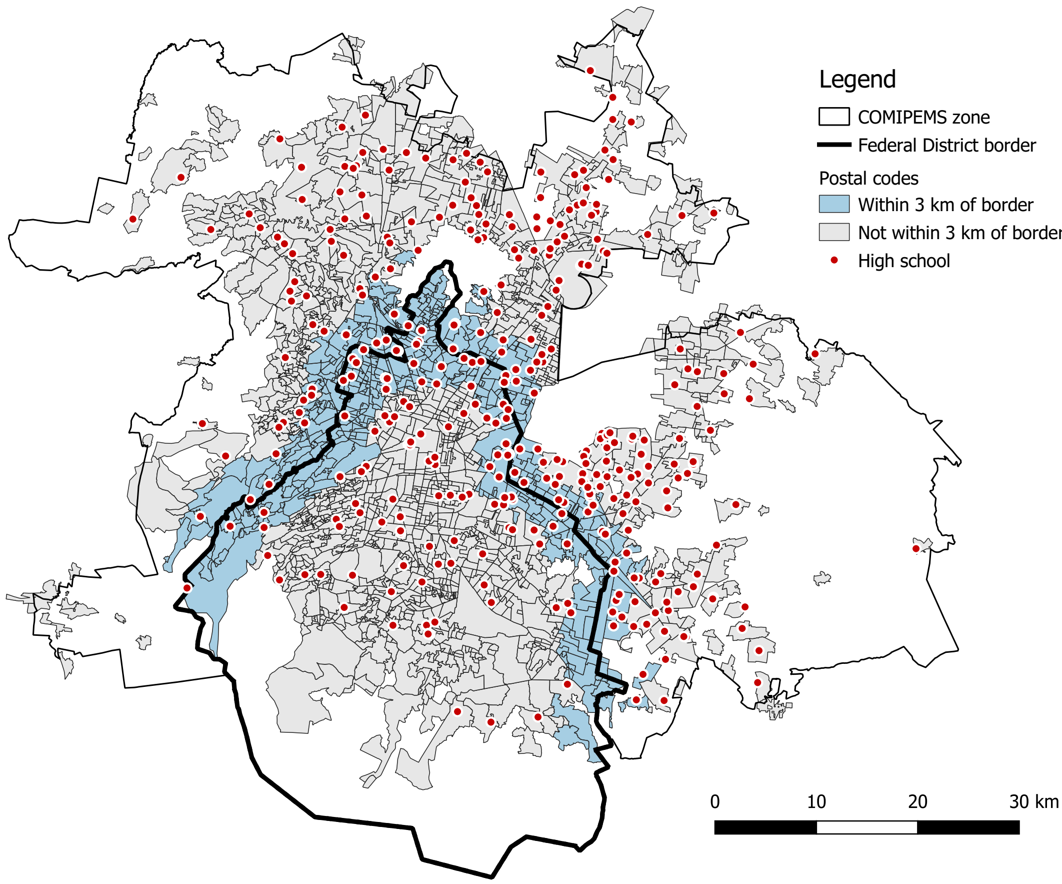
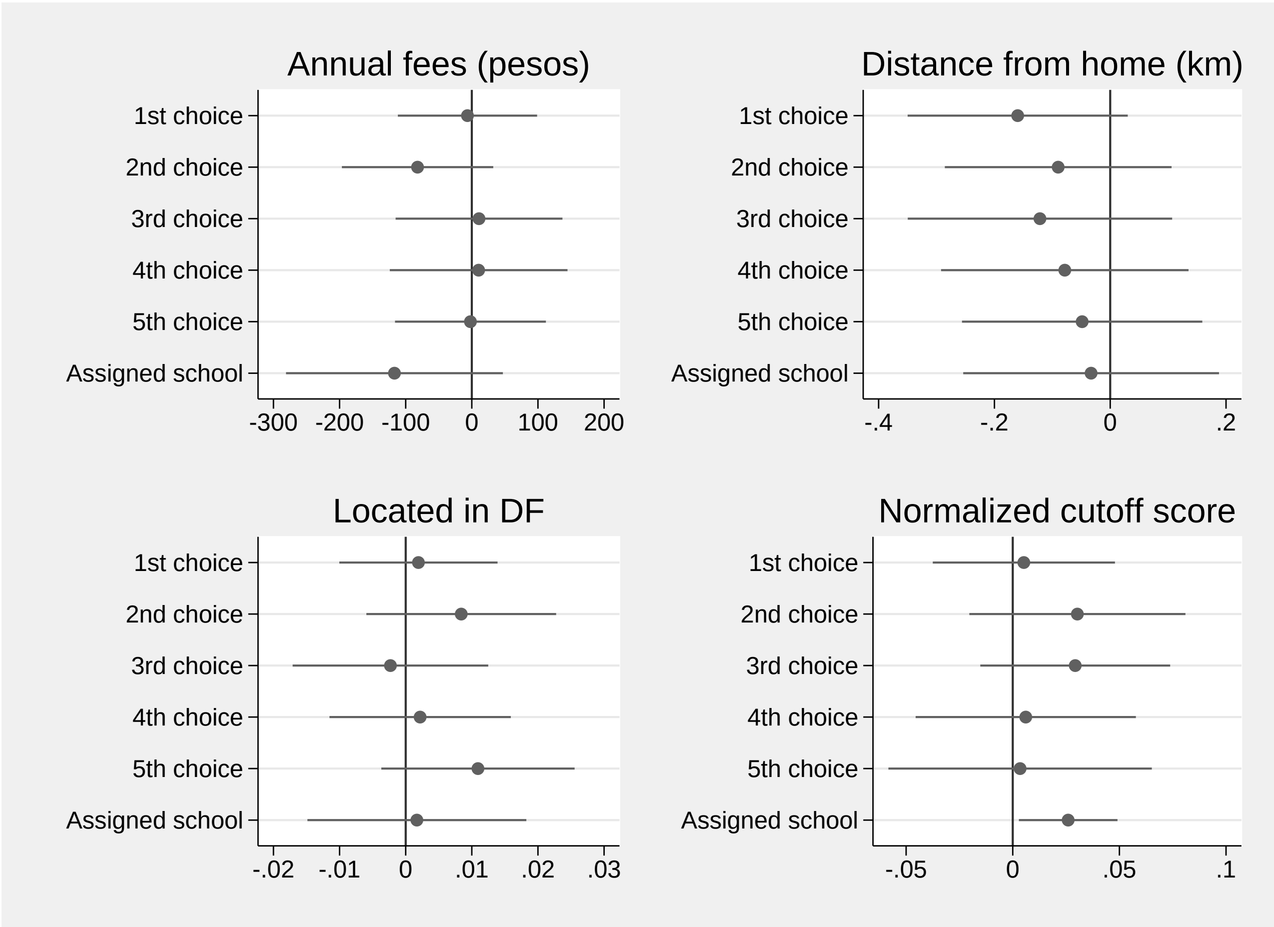
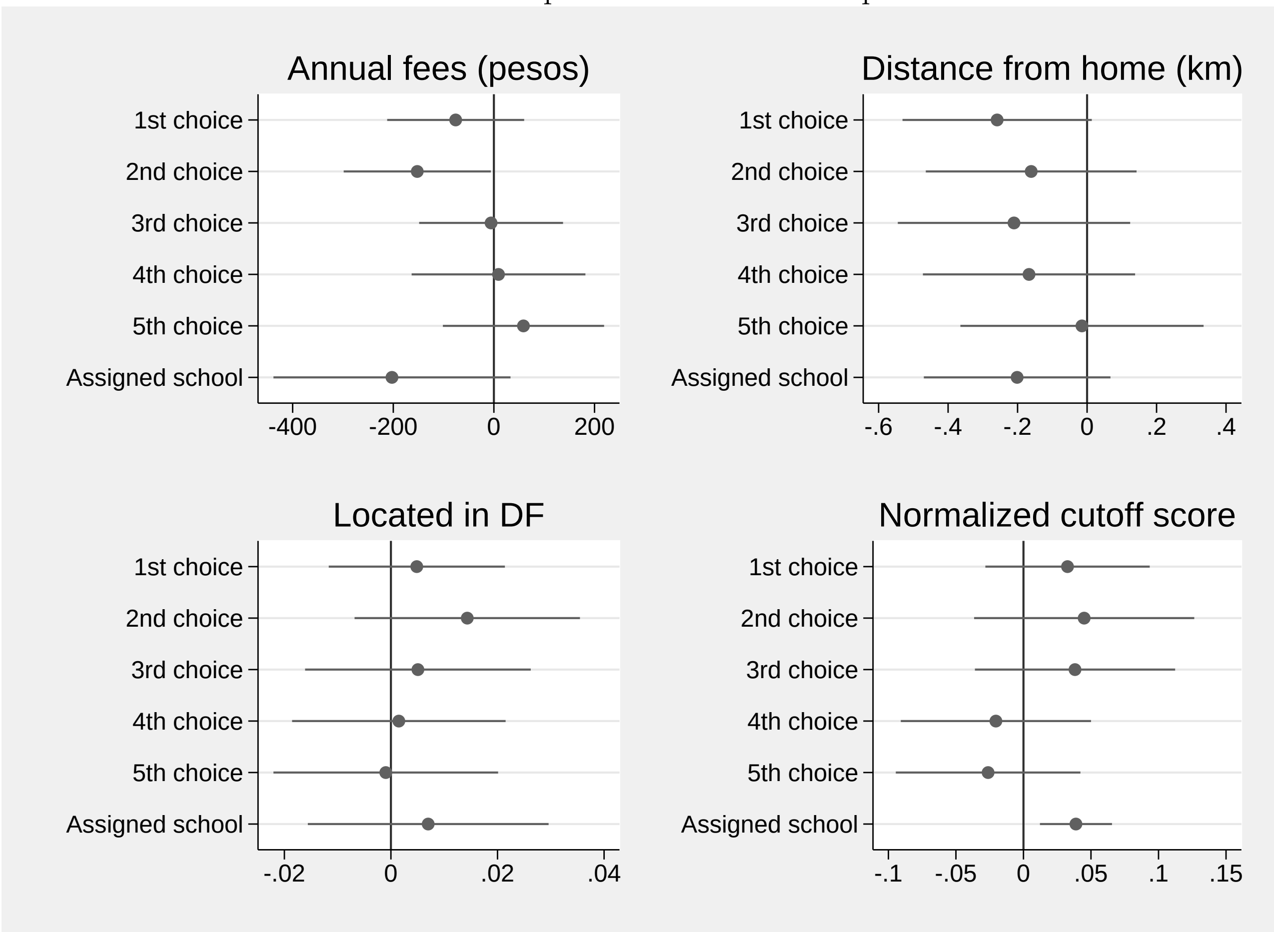


Figure 2: Estimated effect of Prepa Sí on characteristics of chosen and assigned schools, 2005-2010 cohorts

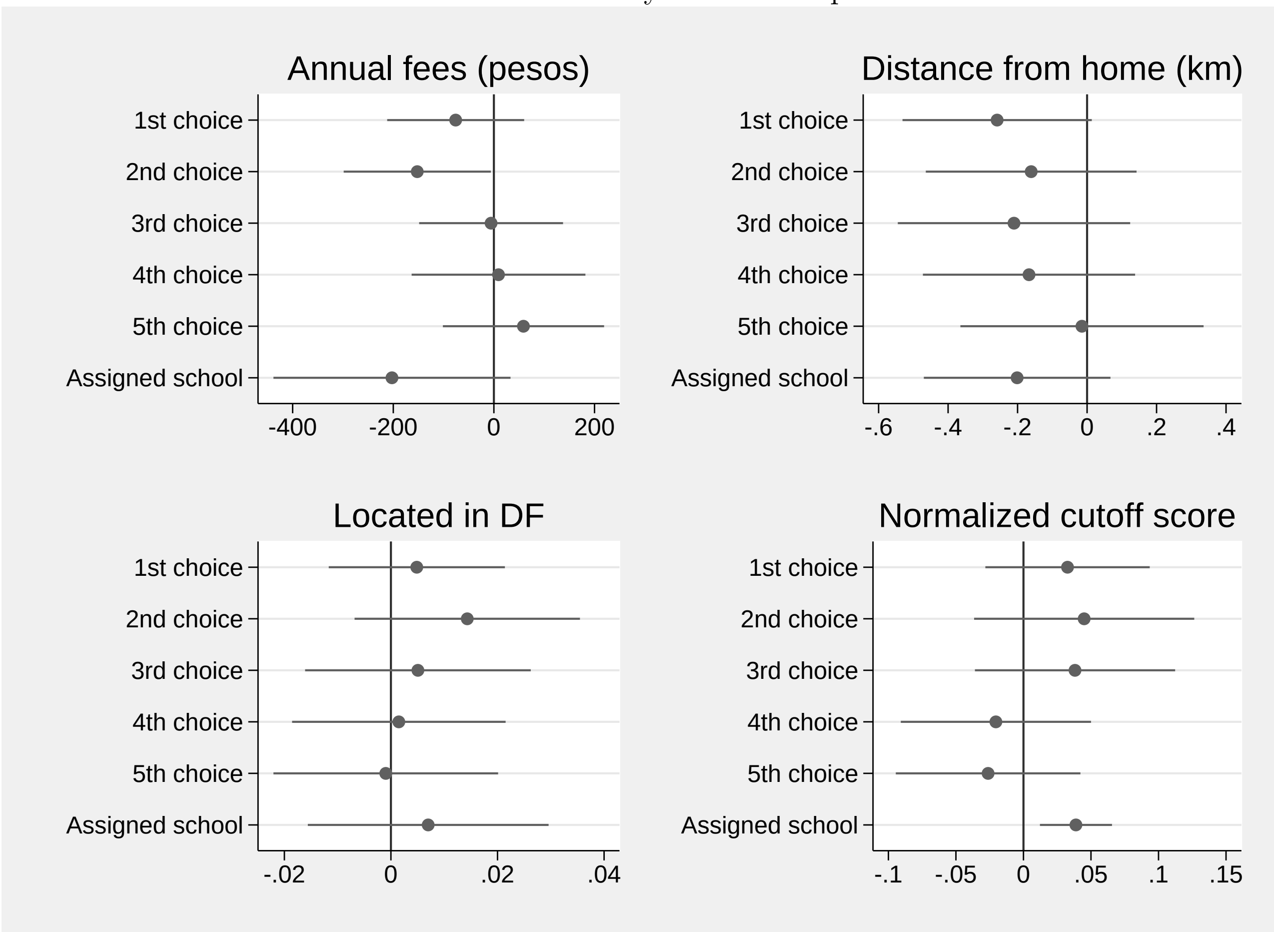
Panel A. Full choice sample



Panel B. Low parental education sample



Panel C. Low family income sample



Note: Circles are estimated coefficients of the Prepa Sí effect on the indicated outcome from Equation 4 ($\hat{\delta}$), which correspond to the specification estimated in Columns 5 through 8 of Table 12 and include border segment-by-cohort, postal code, and middle school fixed effects; postal code-level linear trends; and student normalized COMIPEMS score, middle school GPA, and dummy for male. Sample is restricted to students in postal codes within 3 kilometers of the DF-EdoMex border. For example, "Annual fees (pesos), 1st choice" gives the estimated average effect on the annual fees charged by the student's first choice. Lines are the corresponding 95% confidence intervals. Low parental education sample is limited to students whose parental education is less than high school. Low family income sample is limited to students whose self-reported family income is below 2,500 pesos per month.

9 Tables

Table 1: Summary statistics by place of residence and location of assigned high school, 2005-2008 cohorts

	(1) DF resident, DF high school	(2) EdoMex resident, DF high school	(3) EdoMex resident, EdoMex high school	(4) p-value, (1) - (2)	(5) p-value, (2) - (3)
Normalized COMIPEMS score	0.15 [0.91]	0.25 [0.93]	−0.29 [0.76]	0.00	0.00
Middle school GPA	7.97 [0.80]	8.13 [0.82]	8.02 [0.78]	0.00	0.00
Male	0.50 [0.50]	0.54 [0.50]	0.44 [0.50]	0.00	0.00
Parental education (years)	10.40 [3.21]	10.23 [3.25]	9.49 [3.12]	0.00	0.00
Income (pesos/month)	4341.05 [3284.24]	4371.25 [3293.37]	3695.06 [2953.83]	0.24	0.00
Number of siblings	2.04 [1.36]	2.15 [1.40]	2.36 [1.52]	0.00	0.00
Annual fees, assigned school (pesos)	3208.37 [2956.78]	2740.39 [2352.09]	4043.25 [2788.12]	0.00	0.00
Distance to assigned school (km)	5.70 [4.76]	8.11 [5.69]	4.67 [4.99]	0.00	0.00
Student has matched ENLACE result	0.50 [0.50]	0.55 [0.50]	0.56 [0.50]	0.00	0.00
Normalized ENLACE math score	0.10 [1.05]	0.23 [1.06]	−0.12 [0.92]	0.00	0.00
Normalized ENLACE Spanish score	0.03 [1.02]	0.10 [1.01]	−0.08 [0.95]	0.00	0.00
Annual fees, first choice school (pesos)	1291.06 [2145.23]	1348.16 [1879.81]	2366.39 [2770.53]	0.00	0.00
Distance to first choice school (km)	6.49 [4.81]	8.68 [5.35]	6.43 [5.22]	0.00	0.00
Elite school as first choice	0.23 [0.42]	0.31 [0.46]	0.01 [0.10]	0.00	0.00
Normalized cutoff score of first choice	0.60 [1.10]	0.56 [1.08]	−0.02 [1.37]	0.00	0.00
Observations	51612	28433	42674		

Note: Standard deviations in brackets. Sample is restricted to students in postal codes within 3 kilometers of the DF-EdoMex border. Exchange rate from Mexican pesos to US dollars was approximately 10.8 in 2005.

Table 2: Parallel trends in covariates between DF and EdoMex residents assigned to DF high schools

	(1) Normalized COMIPEMS score	(2) Middle school GPA	(3) Male	(4) Parental education (years)	(5) Income (1000 pesos/mo)	(6) Number of siblings	(7) Distance to assigned school	(8) Elite first choice	(9) Normalized cutoff score of first choice
DF × partial exposure	0.03**	0.01	0.02*	0.02	0.02	0.01	0.03	0.00	−0.01
	(0.012)	(0.022)	(0.012)	(0.073)	(0.085)	(0.033)	(0.073)	(0.010)	(0.025)
DF × full exposure	0.02	0.00	0.01	−0.02	−0.06	−0.00	−0.02	−0.01	−0.02
	(0.012)	(0.018)	(0.011)	(0.065)	(0.073)	(0.031)	(0.065)	(0.010)	(0.025)
Observations	79746	79746	79746	71244	70300	71569	78858	79746	79746
Adjusted R^2	0.657	0.204	0.114	0.175	0.117	0.071	0.629	0.239	0.235
Mean of dependent variable (2005, DF)	0.16	7.95	0.51	10.20	4.07	2.15	5.73	0.73	0.48
SD of dependent variable (2005, DF)	0.95	0.81	0.50	3.30	3.13	1.42	4.84	0.45	1.18
2005 conditional DF-EdoMex difference	−.01	−.11	−.03	0.28	0.07	−.13	−1.81	0.03	0.1

Note: Dependent variables in column headers. Models correspond to Equation 1 and include assigned high school-by-cohort, border segment-by-cohort, postal code, and middle school fixed effects. Sample is restricted to students in postal codes within 3 kilometers of the DF-EdoMex border. The "2005 conditional DF-EdoMex difference" is the coefficient on DF in a regression of the outcome on DF residence, high school and border segment fixed effects, for the 2005 cohort.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the high school and middle school levels in parentheses.

Table 3: Prepa Sí take-up rate in 10th grade for students assigned to DF high schools

	(1) 2007 cohort	(2) 2007 cohort	(3) 2007 cohort	(4) 2008 cohort	(5) 2008 cohort	(6) 2008 cohort
Resides in DF	0.460***	0.458***	0.466***	0.598***	0.592***	0.598***
	(0.0290)	(0.0289)	(0.0295)	(0.0244)	(0.0249)	(0.0249)
Normalized COMIPEMS score		0.019**	0.017**		0.011	0.012
		(0.0076)	(0.0081)		(0.0072)	(0.0073)
Middle school GPA		0.063***	0.061***		0.042***	0.039***
		(0.0077)	(0.0079)		(0.0054)	(0.0053)
Male		0.021***	0.019**		0.028***	0.029***
		(0.0077)	(0.0082)		(0.0065)	(0.0070)
Distance to assigned school (km)		−0.004***	−0.003***		−0.005***	−0.006***
		(0.0010)	(0.0011)		(0.0014)	(0.0015)
Elite school as first choice		0.054***	0.051***		−0.011	−0.007
		(0.0177)	(0.0180)		(0.0131)	(0.0141)
Normalized cutoff score of first choice		−0.009	−0.012*		0.001	−0.000
		(0.0068)	(0.0068)		(0.0059)	(0.0059)
Parental education (years)			0.000			−0.000
			(0.0013)			(0.0012)
Income (1000 pesos/month)			−0.002			−0.004***
			(0.0010)			(0.0011)
Number of siblings			−0.004*			−0.005*
			(0.0023)			(0.0025)
Observations	20063	19815	17270	21089	20843	18452
Adjusted R^2	0.221	0.232	0.234	0.368	0.374	0.380
Take-up rate among EdoMex students	0.18	0.18	0.19	0.06	0.06	0.06

Note: Dependent variable is a dummy variable for whether the student is on the Prepa Sí beneficiary list in the first school year after COMIPEMS assignment. Column headers correspond to COMIPEMS cohorts. Models include high school and border segment fixed effects. Sample is restricted to students in postal codes within 3 kilometers of the DF-EdoMex border.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the high school and middle school levels in parentheses.

Table 4: Effect of Prepa Sí eligibility on ENLACE-taking rates in DF high schools

	(1) Took ENLACE	(2) Took ENLACE	(3) Took ENLACE	(4) Took ENLACE	(5) Took ENLACE	(6) Took ENLACE	(7) Took ENLACE	(8) Took ENLACE
DF × partial exposure	−0.013	−0.018	−0.019	−0.017	−0.013	−0.018	−0.019	−0.017
	(0.0118)	(0.0115)	(0.0122)	(0.0128)	(0.0118)	(0.0115)	(0.0122)	(0.0128)
DF × full exposure	−0.011	−0.013	−0.011	−0.008				
	(0.0089)	(0.0081)	(0.0089)	(0.0101)				
DF × 2007 cohort					−0.011	−0.015	−0.014	−0.010
					(0.0110)	(0.0108)	(0.0112)	(0.0125)
DF × 2008 cohort					−0.012	−0.010	−0.009	−0.005
					(0.0105)	(0.0086)	(0.0102)	(0.0110)
Normalized COMIPEMS score		0.044***	0.040***			0.044***	0.040***	
		(0.0034)	(0.0041)			(0.0034)	(0.0041)	
Middle school GPA		0.210***	0.212***			0.210***	0.212***	
		(0.0046)	(0.0046)			(0.0046)	(0.0046)	
Male		0.043***	0.043***			0.043***	0.043***	
		(0.0050)	(0.0053)			(0.0050)	(0.0053)	
Distance to assigned school (km)		−0.004***	−0.004***			−0.004***	−0.004***	
		(0.0006)	(0.0007)			(0.0006)	(0.0007)	
Elite school as first choice		0.013**	0.007			0.013**	0.007	
		(0.0066)	(0.0071)			(0.0066)	(0.0071)	
Normalized cutoff score of first choice		0.004	0.004			0.004	0.004	
		(0.0026)	(0.0029)			(0.0026)	(0.0029)	
Parental education (years)			0.005***				0.005***	
			(0.0007)				(0.0007)	
Income (1000 pesos/month)			0.003***				0.003***	
			(0.0006)				(0.0006)	
Number of siblings			−0.003**				−0.003**	
			(0.0015)				(0.0015)	
Covariate-by-year interactions				X				X
Observations	79746	78858	68727	68727	79746	78858	68727	68727
Adjusted R^2	0.065	0.162	0.164	0.164	0.065	0.162	0.164	0.164
Baseline ENLACE-taking rate (2005, DF)	0.51	0.51	0.52	0.52	0.51	0.51	0.52	0.52
2005 conditional DF-EdoMex difference	−.02	−.02	−.02	−.02	−.02	−.02	−.02	−.02

Note: Models correspond to Equation 1 and include assigned high school-by-cohort, border segment-by-cohort, postal code, and middle school fixed effects. Sample is restricted to students in postal codes within 3 kilometers of the DF-EdoMex border. The "2005 conditional DF-EdoMex difference" is the coefficient on DF in a regression of the outcome on DF residence, high school and border segment fixed effects, for the 2005 cohort.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the high school and middle school levels in parentheses.

Table 5: Instrumental variables estimates of Prepa Sí take-up effect on ENLACE-taking rates in DF high schools

	(1) Took up Prepa Si	(2) Took up Prepa Si	(3) Took up Prepa Si	(4) Took ENLACE	(5) Took ENLACE	(6) Took ENLACE
DF × 2007 cohort	0.460***		0.460***			
	(0.0283)		(0.0286)			
DF × 2008 cohort		0.598***	0.598***			
		(0.0245)	(0.0244)			
Took up Prepa Si				−0.030	−0.015	−0.020
				(0.0231)	(0.0138)	(0.0138)
Normalized COMIPEMS score	0.004	0.000	0.004	0.045***	0.043***	0.042***
	(0.0037)	(0.0034)	(0.0037)	(0.0044)	(0.0043)	(0.0035)
Middle school GPA	0.036***	0.025***	0.040***	0.208***	0.215***	0.215***
	(0.0040)	(0.0030)	(0.0036)	(0.0048)	(0.0049)	(0.0047)
Male	0.016***	0.020***	0.024***	0.048***	0.048***	0.047***
	(0.0040)	(0.0032)	(0.0035)	(0.0072)	(0.0063)	(0.0056)
Distance to assigned school (km)	−0.002**	−0.003***	−0.003***	−0.004***	−0.005***	−0.004***
	(0.0006)	(0.0008)	(0.0007)	(0.0009)	(0.0008)	(0.0007)
Elite school as first choice	0.019**	−0.008	0.010	0.013	0.013	0.008
	(0.0080)	(0.0062)	(0.0075)	(0.0101)	(0.0089)	(0.0074)
Normalized cutoff score of first choice	−0.004	0.000	−0.003	0.003	0.005	0.004
	(0.0029)	(0.0026)	(0.0030)	(0.0042)	(0.0038)	(0.0032)
Observations	38512	39539	59365	38512	39539	59365
Adjusted R^2	0.465	0.547	0.453	0.152	0.163	0.161
Kleibergen-Paap rk Wald F	264.09	593.27	372.31			

Note: Columns 1 through 3 are first-stage estimates where the endogenous dependent variable is Prepa Sí take-up, defined as appearing on the beneficiary list in the first year of high school. Columns 4 through 6 are corresponding second-stage estimates, where "Took up Prepa Sí" is the predicted value from the first stage and $DF \times$ *2007 cohort* and $DF \times$ *2008 cohort* are the excluded instruments. Columns 1 and 3 are limited to 2005 and 2007 cohorts. Columns 2 and 4 are limited to 2005 and 2008 cohorts. Columns 3 and 6 include 2005, 2007, and 2008 cohorts. Models include assigned high school-by-cohort, border segment-by-cohort, postal code, and middle school fixed effects. Sample is restricted to students in postal codes within 3 kilometers of the DF-EdoMex border.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the high school and middle school levels in parentheses.

Table 6: Effect of Prepa Sí eligibility on ENLACE-taking rates in DF high schools, by subgroup

	(1) All students	(2) Male	(3) Female	(4) Parental education < HS	(5) Family income < 3000 MXN/mo	(6) Middle school GPA ≥ 8
DF × partial exposure	−0.018	−0.012	−0.016	−0.002	−0.002	−0.015
	(0.0115)	(0.0179)	(0.0139)	(0.0180)	(0.0202)	(0.0148)
DF × full exposure	−0.013	−0.018	−0.003	0.000	0.005	−0.015
	(0.0081)	(0.0125)	(0.0129)	(0.0130)	(0.0169)	(0.0106)
Observations	78858	40462	38177	36741	27959	39146
Adjusted R^2	0.162	0.168	0.152	0.159	0.157	0.097
Baseline ENLACE-taking rate (2005, DF)	0.51	0.50	0.53	0.49	0.50	0.66
2005 conditional DF-EdoMex difference	−.02	−.01	−.03	−.03	−.03	0

Note: Column headers represent sample restrictions. Model corresponds to Column 6 of Table 4. Sample is restricted to students in postal codes within 3 kilometers of the DF-EdoMex border. The "2005 conditional DF-EdoMex difference" is the coefficient on DF in a regression of the outcome on DF residence, high school and border segment fixed effects, for the 2005 cohort.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the high school and middle school levels in parentheses.

Table 7: Evolution of previous Oportunidades beneficiary status in DF high schools

	(1) Oportunidades recipient	(2) Oportunidades recipient	(3) Oportunidades recipient
DF × partial exposure	−0.004	−0.004	−0.003
	(0.0028)	(0.0028)	(0.0029)
DF × full exposure	−0.007***	−0.007***	−0.007***
	(0.0024)	(0.0024)	(0.0025)
Normalized COMIPEMS score		−0.002	−0.001
		(0.0010)	(0.0010)
Middle school GPA		0.004***	0.003***
		(0.0008)	(0.0008)
Male		−0.000	0.000
		(0.0011)	(0.0011)
Distance to assigned school (km)		0.000	0.000
		(0.0002)	(0.0002)
Elite school as first choice		−0.002	−0.001
		(0.0018)	(0.0018)
Normalized cutoff score of first choice		−0.001	−0.000
		(0.0007)	(0.0007)
Parental education (years)			−0.001***
			(0.0002)
Income (1000 pesos/month)			−0.001***
			(0.0002)
Number of siblings			0.003***
			(0.0007)
Observations	70497	69725	67448
Adjusted R^2	0.077	0.078	0.081
Baseline proportion receiving Oportunidades (2005, DF)	0.00	0.00	0.00
2005 conditional DF-EdoMex difference	−.01	−.01	−.01

Note: Outcome variable is a dummy variable for whether the student reports, on the COMIPEMS questionnaire, having been an Oportunidades beneficiary during middle school. Models correspond to Equation 1 and include assigned high school-by-cohort, border segment-by-cohort, postal code, and middle school fixed effects. Sample is restricted to students in postal codes within 3 kilometers of the DF-EdoMex border. The "2005 conditional DF-EdoMex difference" is the coefficient on DF in a regression of the outcome on DF residence, high school and border segment fixed effects, for the 2005 cohort.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the high school and middle school levels in parentheses.

Table 8: Comparison of EdoMex residents admitted to DF vs. EdoMex high schools

	(1) Took ENLACE	(2) Normalized COMIPEMS score	(3) Middle school GPA	(4) Male	(5) Parental education (years)	(6) Income (1000 pesos/mo)	(7) Number of siblings	(8) Distance to assigned school	(9) Elite first choice	(10) Normalized cutoff score of first choice
DF HS × partial exposure	0.01	0.02	0.00	−0.01	0.00	−0.07	0.03	−0.14*	−0.02**	−0.03
	(0.014)	(0.020)	(0.017)	(0.012)	(0.083)	(0.078)	(0.037)	(0.082)	(0.010)	(0.029)
DF HS × full exposure	−0.02	0.02	0.01	−0.01	0.05	0.05	0.00	0.00	−0.03***	−0.00
	(0.016)	(0.021)	(0.014)	(0.010)	(0.074)	(0.071)	(0.028)	(0.079)	(0.010)	(0.034)
Observations	70144	70144	70144	70144	62679	61811	62973	69717	70144	70144
Adjusted R^2	0.072	0.571	0.185	0.096	0.164	0.123	0.068	0.629	0.252	0.299
Mean of dependent variable (2005, DF schools)	0.55	0.28	8.11	0.55	10.05	4.08	2.25	8.04	0.72	0.42
SD of dependent variable (2005, DF schools)	0.50	0.96	0.82	0.50	3.32	3.13	1.46	5.69	0.45	1.19
2005 conditional DF high school-EdoMex high school difference	−.02	0.45	0.15	0.13	0.39	0.43	−.11	3.85	0.25	0.39

Note: Column headers are dependent variables Models correspond to Equation 2 and include assigned high school, postal code-by-cohort, and middle school-by-cohort fixed effects, as well as student-level controls for normalized COMIPEMS score, middle school GPA, distance to assigned school, and elite school as first choice. Sample is restricted to students in EdoMex postal codes within 3 kilometers of the DF-EdoMex border. The "2005 conditional DF high school-SoM high school difference" is the coefficient on DF high school in a regression of the outcome on DF high school, postal code and middle school fixed effects, for the 2005 cohort.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the high school and middle school levels in parentheses.

Table 9: Differential impacts of Prepa Sí eligibility on ENLACE-taking rates with respect to high school fees and distance, DF sample

	(1) Took ENLACE	(2) Took ENLACE	(3) Took ENLACE	(4) Took ENLACE	(5) Took ENLACE
Annual fees (1000 pesos) × partial exposure	−0.001	0.000			
	(0.0026)	(0.0024)			
Annual fees (1000 pesos) × full exposure	−0.004	−0.004			
	(0.0038)	(0.0034)			
Distance from home (km) × partial exposure			0.000	0.001	−0.001
			(0.0009)	(0.0011)	(0.0010)
Distance from home (km) × full exposure			0.001	0.002**	0.000
			(0.0009)	(0.0010)	(0.0008)
Distance from home (km)	−0.004***	−0.004***	−0.004***	−0.005***	−0.003***
	(0.0004)	(0.0004)	(0.0007)	(0.0008)	(0.0007)
Postal code FE	X		X		
Postal code-by-cohort FE		X		X	X
High school FE	X	X	X	X	
High school-by-cohort FE					X
Middle school FE	X		X		
Middle school-by-cohort FE		X		X	X
Cohort FE	X		X		
Observations	154436	153942	154436	153942	153942
Adjusted R^2	0.137	0.138	0.137	0.138	0.144
Baseline ENLACE-taking rate (2005)	0.50	0.50	0.50	0.50	0.50

Note: Models correspond to Equation 3 and include student-level controls for normalized COMIPEMS score, middle school GPA, and elite school as first choice, along with the fixed effects indicated. Sample is limited to students with DF residence.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the high school and middle school levels in parentheses.

Table 10: Effect of Prepa Sí eligibility on ENLACE test scores in DF high schools, by subgroup

Panel A. Math scores	(1) All students	(2) Male	(3) Female	(4) Parental education < HS	(5) Family income < 3000 MXN/mo	(6) Middle school GPA $\geq$ 8
DF × partial exposure	−0.018	−0.019	−0.034	0.052	0.006	−0.029
	(0.0219)	(0.0336)	(0.0279)	(0.0397)	(0.0430)	(0.0264)
DF × full exposure	0.006	0.011	−0.011	0.023	0.036	0.000
	(0.0195)	(0.0286)	(0.0267)	(0.0331)	(0.0396)	(0.0240)
Observations	40515	19625	20676	17935	13540	25562
Adjusted R^2	0.553	0.562	0.509	0.519	0.515	0.568
Baseline mean (2005, DF)	0.17	0.39	−0.04	0.02	−0.01	0.40
2005 conditional DF-EdoMex difference	−.04	−.04	−.03	−.06	−.06	−.04

Panel B. Spanish scores	(1) All students	(2) Male	(3) Female	(4) Parental education < HS	(5) Family income < 3000 MXN/mo	(6) Middle school GPA $\geq$ 8
DF × partial exposure	−0.018	−0.022	0.001	−0.001	−0.026	−0.038
	(0.0261)	(0.0420)	(0.0362)	(0.0420)	(0.0461)	(0.0287)
DF × full exposure	0.009	−0.014	0.035	0.029	0.012	0.002
	(0.0226)	(0.0347)	(0.0275)	(0.0355)	(0.0401)	(0.0294)
Observations	40578	19667	20696	17950	13557	25583
Adjusted R^2	0.416	0.423	0.412	0.403	0.397	0.410
Baseline mean (2005, DF)	0.08	0.04	0.11	−0.06	−0.07	0.30
2005 conditional DF-EdoMex difference	−.02	0.01	−.06	−.03	−.04	−.01

Note: Column headers represent sample restrictions. Models correspond to Equation 1 and include assigned high school-by-cohort, border segment-by-cohort, postal code, and middle school fixed effects, as well as student-level controls for normalized COMIPEMS score, middle school GPA, distance to assigned school, and elite school as first choice. Sample is restricted to students in postal codes within 3 kilometers of the DF-EdoMex border. The "2005 conditional DF-SoM difference" is the coefficient on DF in a regression of the outcome on DF residence, high school and border segment fixed effects, for the 2005 cohort.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the high school and middle school levels in parentheses.

Table 11: Effect of Prepa Sí eligibility on ENLACE questionnaire outcomes in DF high schools

	(1) Days/week did homework	(2) Hours/day did homework	(3) Grade point average	(4) Currently works	(5) Has worked 1+ year
DF × partial exposure	−0.050	−0.098*	−0.040	0.032*	0.006
	(0.0728)	(0.0507)	(0.0332)	(0.0174)	(0.0124)
DF × full exposure	0.010	−0.068*	−0.020	0.012	0.007
	(0.0752)	(0.0361)	(0.0242)	(0.0245)	(0.0185)
Observations	13072	13092	13030	13104	13065
Adjusted R^2	0.112	0.087	0.382	0.056	0.040
Mean of dependent variable (2005, DF students)	4.22	2.25	7.95	0.13	0.06
SD of dependent variable (2005, DF students)	1.50	1.04	0.71	0.34	0.23
2005 conditional DF-EdoMex difference	0.01	0.05	−.01	−.03	−.01

Note: Column headers represent dependent variables. Models correspond to Equation 1 and include assigned high school-by-cohort, border segment-by-cohort, postal code, and middle school fixed effects, as well as student-level controls for normalized COMIPEMS score, middle school GPA, distance to assigned school, and elite school as first choice. Sample is restricted to students in postal codes within 3 kilometers of the DF-EdoMex border assigned to high schools that administered the ENLACE context questionnaire. The "2005 conditional DF-SoM difference" is the coefficient on DF in a regression of the outcome on DF residence, high school and border segment fixed effects, for the 2005 cohort.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the high school and middle school levels in parentheses.

Table 12: Effect of Prepa Sí introduction on characteristics of first choice and assigned schools, 2005-2010 cohorts

Panel A. First choice school characteristics	(1) Annual fees	(2) Distance from home	(3) Located in DF	(4) Normalized cutoff score	(5) Annual fees	(6) Distance from home	(7) Located in DF	(8) Normalized cutoff score
Federal District × 2008-10 cohorts	6.30	−0.11**	−0.01***	0.02*	−6.55	−0.16*	0.00	0.01
	(24.832)	(0.046)	(0.003)	(0.010)	(53.209)	(0.092)	(0.006)	(0.022)
Postal code trends					X	X	X	X
Observations	301553	298375	301567	301567	301553	298375	301567	301567
Adjusted R^2	0.108	0.249	0.236	0.205	0.109	0.249	0.236	0.205
Mean of dependent variable (2007, DF)	1048.86	6.67	0.96	0.80	1048.86	6.67	0.96	0.80
SD of dependent variable (2007, DF)	2126.71	4.89	0.20	1.02	2126.71	4.89	0.20	1.02
2007 conditional DF-EdoMex difference	−479.75	−.45	0.23	0.28	−479.75	−.45	0.23	0.28

Panel B. Assigned school characteristics	(1) Annual fees	(2) Distance from home	(3) Located in DF	(4) Normalized cutoff score	(5) Annual fees	(6) Distance from home	(7) Located in DF	(8) Normalized cutoff score
Federal District × 2008-10 cohorts	−75.12	−0.06	0.01	0.03***	−117.00	−0.03	0.00	0.03**
	(55.099)	(0.056)	(0.004)	(0.006)	(82.968)	(0.113)	(0.008)	(0.012)
Postal code trends					X	X	X	X
Observations	251209	248636	251293	301567	251209	248636	251293	301567
Adjusted R^2	0.192	0.138	0.352	0.692	0.194	0.138	0.353	0.692
Mean of dependent variable (2007, DF)	2951.81	6.15	0.88	−0.51	2951.81	6.15	0.88	−0.51
SD of dependent variable (2007, DF)	3282.13	5.13	0.32	1.05	3282.13	5.13	0.32	1.05
2007 conditional DF-EdoMex difference	−247.66	−.11	0.39	0.16	−247.66	−.11	0.39	0.16

Note: Dependent variables in column headers. Models correspond to Equation 4 and include border segment-by-cohort, postal code, and middle school fixed effects; postal code-level linear trends (where indicated); and student normalized COMIPEMS score, middle school GPA, and dummy for male. Sample is restricted to students in postal codes within 3 kilometers of the DF-EdoMex border. The "2007 conditional DF-EdoMex difference" is the coefficient on DF in a regression of the outcome on DF residence and border segment fixed effects, for the 2007 cohort.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the postal code and middle school levels in parentheses.