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Do Larger School Grants Improve Educational Attainment? Evidence from Urban Mexico*

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Abstract

We study the effects of increasing cash grants on the education attainment of low-income middle and high school students in Mexico. Starting in 2009, the Oportunidades conditional cash transfer (CCT) program increased the average grant in middle and high school by 27 percent for females and 30 percent for males in 263 of 630 urban localities in the country. Using administrative data sources and a difference-in-difference identification strategy, we find that students in households with larger grants exhibit lower dropout rates in middle school, and increase high school graduation by up to 33.5 percent. Effect sizes do not vary substantially by gender or baseline academic ability as measured by a standardized test. The expected future income from additional schooling exceeds the cost of the grants by a ratio of more than two-to-one. The patterns we observe are consistent with an elastic demand for schooling, suggesting that increasing the school grant component of conditional cash transfers may be an efficient way to boost educational attainment for low-income students.

Key Words: Cash transfers, school dropout, high school graduation, Mexico, Progresa, Oportunidades, Prospera

JEL Codes: I25, H53, I38

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I. INTRODUCTION

In previous decades, Latin American countries progressed substantially towards universalizing access to and graduation from primary education. From the early 1990s to late 2000s, primary school completion rates increased from 65 to 76 percent. Although progress has also been made for secondary education, the road to universalization at this level still looks lengthy, particularly for the poor. Conditional upon having completed primary education, secondary graduation rates in the region increased from 32 to 46 percent over the same period. However, graduation gains at secondary levels were concentrated disproportionately amongst the non-poor. Even though graduation increased for both groups, the graduation gap between the richest and poorest income quintiles increased from 19 to 28 percentage points (Bassi, Busso, Muñoz, 2013).

Conditional cash transfer (CCTs) programs encourage school enrollment, attendance, and completion for children in poor households. There is a large body of literature showing that CCTs tend to have positive impacts on school enrollment and attendance both at primary and secondary school levels, although impacts at primary level are generally modest, driven by already high levels for these outcomes in most countries.[3] Only a few CCTs have been evaluated regarding their impact on high school completion rates, even though graduation, rather than years of schooling *per se*, might be a more relevant determinant of better labor market outcomes.[4]

From a purely educational point of view—leaving aside impacts on consumption, CCTs are more transfer-efficient at secondary than primary level (Garcia and Saavedra, 2017). Thus, it would be tempting for policy-makers to increase the size of secondary level transfers, possibly by reducing benefits at primary level to balance fiscal concerns. However, CCTs might face diminishing returns to scale for educational outcomes. The CCT literature offers practically no guidance on this respect. To our knowledge, only Filmer and Schady (2011) have addressed this issue by means of a trial specifically designed to that end. In their evaluation of the

[3] Garcia and Saavedra (2017) present the most recent meta-analysis for CCTs impacts on educational outcomes. Forty-seven CCTs impact evaluations (for 31 countries) met their quality criteria. Fifty-three percent of them were implemented in Latin America, 32 percent in Asia and 15 percent in Africa.

[4] There is a body of literature analyzing the "sheepskin effect", whether an educational degree yields higher returns that the same amount of studying without the possession of a certificate. Hungerford and Solon (1987) and Jeager and Page (1996) are seminal papers for this topic.

Cambodian Educational Sector Support Project, they conclude that a small CCT transfer had a substantial impact on school attendance at 7th grade—the first year of secondary education—that could not be improved by a somewhat larger transfer. Beyond the fact that this result might be specific to the Cambodian context, the transfer differential analyzed might have been insufficient to overcome potential large fixed costs to secondary school attendance[5]. Consistent with Filmer and Schady's (2011) conclusion, by means of a CCT meta-analysis, Garcia and Saavedra (2017) conclude that transfer size does not have a significant impact on educational outcomes, a finding that contradicts their theoretical model.

In this paper, we study the effects of larger cash grants for middle and high school students in the context of Mexico's national CCT program Oportunidades. [6] Starting in 2009, middle and high school grants for new beneficiaries in a sub-set of 263 out of 630 urban localities were increased by 27 percent for females and 30 percent for males, financed through the elimination of primary school transfers in those same localities. [7] We employ administrative data spanning seven years to analyze effects on proxies of dropout and high school graduation using a difference-in-difference strategy that compares changes in educational outcomes for new enrollees in 2008 and 2009 across treatment and comparison localities. Our paper makes a contribution to the CCT literature in an area of high potential impact for policy making. It is novel in analyzing the educational impact of a large change in the size of the cash transfer in middle and high school, and provides evidence on the impact of a CCT on a relatively unexplored outcome, namely high school graduation.

Our main finding is that larger grants make students substantially less likely to drop out of middle school, and increase their high school graduation by 9.2 percentage points for females and 8.6 percentage points for males. Relative to the grant increase, these effect sizes suggest that the demand for schooling is elastic, and imply that households perceive a positive return to

[5] Filmer and Schady (2011) describe that the amount transferred by the Cambodian program was very small compared to those transferred by other CCTs: 2 percent of the consumption of the median recipient household in Cambodia, while the comparable value is 22 percent for recipients of Oportunidades (nowadays Prospera) in Mexico.

[6] The Oportunidades CCT program began as Progresa in 1997, was renamed Oportunidades in 2002, and became Prospera in 2014. Given that the policy change studied in this paper took place under Oportunidades, we use this name to refer to the program.

[7] See Dávila Lárraga (2016) for a detailed description of the Prospera Conditaional Cash Transfer Program, including the pilot Urban Model studied in this paper. While the relative increase in grants was slightly higher for males, the transfer levels remained higher for females. In addition to school grants, the graduation grant "Jovenes con Prospera" was also increased by 29.5 percent under the Urban Model.

education. Furthermore, the expected benefits from future earnings outweigh the cost of the larger grants by a ratio of more than two-to-one. We hypothesize that the larger cash grants may have helped low-income students overcome the opportunity cost of schooling during an economic crisis that unfolded as these students entered middle school.

Our study complements and expands the existing empirical and theoretical work on the relationship between CCTs and educational outcomes. In particular, evidence on impacts of CCT programs on high school graduation is relatively scarce, and focuses primarily on rural areas, as shown in a recent review by Molina-Millan et al. (2016). An exception is the analysis of the long-term impacts of the Ecuadorian unconditional cash transfer (Araujo, Bosch and Schady, 2016) that uses regression discontinuity around the eligibility cutoff to compare the school attainment of young adults at ages 19-25, six years after their families' eligibility was determined. The authors find a modest impact on high school completion: an increase of 1-2 percentage points, from a counterfactual of 75 percent.

Existing experimental evidence from Oportunidades comes primarily from its original rollout into rural areas starting in 1997. No similar experimental evidence is available for the second phase of program implementation that began in 2002, when Oportunidades was expanded into urban areas using the same basic grant structure as in rural areas. The evaluations from Oportunidades in rural areas documented modest impacts on educational variables. Behrman, Parker and Todd (2009, 2011) found that six years after the original experiment, an 18-month differential exposure had no significant effect on grade progression for children ages 9-11 and resulted in 0.2-0.5 more grades of schooling for children who were 15-21 in 2003. Despite higher schooling among the older cohort, no impacts were found on reading, writing and math tests. Using difference-in-difference matching estimates, the authors compared the original treatment group to a non-experimental comparison group and found positive and significant effects in progression rates of (15 percent for boys and 7 percent for girls) amongst children 0-8 at baseline and 0.5 to 1 more years of schooling for those who were 9-15 at baseline.

This and other evidence raise the question as to the extent that CCT programs could be more effectively designed to have larger impacts on schooling outcomes. Given the already high pre-program enrollment rates in primary school, particularly in urban areas, one of the ideas floated

in the academic and policy circles was focusing cash transfers exclusively on educational levels where enrollment was lower. De Janvry and Sadoulet (2006) quantified two sources of inefficiencies in the Oportunidades program design: (a) paying people for what they were already doing, i.e. enrollment in primary school, which also absorbed the largest proportion of total transfers paid; and (b) paying transfers that are relatively too low (or too high) compared to the cost of the action they are inducing. Attanasio, Meghir and Santiago (2012) estimated a structural model with the program experimental data to show that increasing middle school and high school grants while eliminating the primary school ones in a budget neutral manner could have large positive effects on enrollment in the upper levels and minimal impacts on enrollment in the primary school grades.

The remainder of the paper is structured as follows. In the next section we provide a brief overview of the Mexican educational system, the Oportunidades CCT program, and the Urban Model pilot that introduced the changes to the school grant structure that we study here. Section III presents the conceptual framework, with a model that relates a household's decision to invest in education to changes in the cost of education given by changes to the school grant. Section IV discusses the administrative data sources used in the analysis and the analysis sample. Section V presents our empirical strategy, and section VI discusses our main findings. In section VII we conduct robustness checks of our main specifications, section VIII presents a simple cost-effectiveness analysis, and section IX concludes.

II. THE URBAN MODEL: CONTEXT AND INTERVENTION

The Oportunidades CCT program is Mexico's largest anti-poverty program, covering 6 million households with a budget of $4.5 billion USD (1.6 percent of the national budget) in 2015 (Dávila Lárraga, 2016). Oportunidades conditions transfers to poor households on compliance with "co-responsibilities" in health and education, intended to promote investments in human capital. The CCT payments are divided in three components (nutrition, health and education), of which the school grants make up the largest amount for most families with school-aged children. School grants in middle and high school vary by gender and grade, with larger transfers for females in higher grades. The total transfer amounts per family are capped, with a monthly

maximum of $1,825 Mexican pesos (approximately $114 USD[8]) for households with no high school students, and $2,945 Mexican pesos (approximately $184 USD) for families with high school students.

The Mexican education system is organized into four mandatory levels: (a) early childhood education for children ages 3-5 is comprised of three grades, preschool, pre-K and kindergarten; (b) primary education for children 6-12 consists of grades 1-6; (c) middle school for children 13-15 and has three grades; and (d) high school for children ages 16-18 that also consists of 3 grades. This paper focuses on the latter two levels, middle school and high school[9]. According to INEE (2016), for the 2014-2015 school year, net enrollment rates in early childhood education were 71.9 percent, increased to 98.6 percent for the primary level and decreased in a sustained manner thereafter, to 87.6 percent by middle school and to 57 percent by high school. There is substantial heterogeneity behind these numbers. For example, net enrollment in high school was the lowest in the state of Guerrero, at 47.3 percent and highest in Mexico City, at 86.4 percent.

Starting in 2009 the Oportunidades program revised various features of its operation in urban areas. This responded to evidence that suggested that the program needed to better adapt its design to the reality of urban areas. Gonzales-Flores, Heracleous and Winters (2012) examined six years of urban program data (2002-2007) and documented that beneficiaries in urban areas abandoned the program at rates that were twice as high as those of beneficiaries in rural areas. The reasons for leaving the program in approximately three out of four cases were related to non-compliance of conditions. The remaining beneficiaries left the program due to administrative reasons. The authors also identified an increase over time of dropouts among the poorest 30 percent of eligible households. Similar to what Alvarez, Devoto and Winters (2008) had shown earlier with rural data, the authors documented that in addition to the very poor, beneficiaries on the right-hand-side tail of the poverty distribution were also more likely to fail to comply with conditions compared to those in the middle. The authors hypothesized that amongst the less poor, the transfer value was too low relative to their opportunity cost to comply with conditions. They also found that administrative changes in the program´s operational

[8] Assumes exchange rate of $16 Mexican Pesos per USD as an approximate average in July 2015.

[9] *Educación Secundaria* and *Educación Media Superior*, are the Spanish names for middle and high school, respectively, in Mexico.

processes that increased participation costs for the beneficiaries could translate into large numbers of families being removed from the program´s roster.

The set of changes to program design features in urban areas was named the pilot Urban Model of Oportunidades (henceforth, the Urban Model). It was approved as part of the operations manual in December 2009 and included reforms in five areas (Oportunidades, 2009). The new model: (1) adjusted the health and nutritional components of the program to the urban epidemiology; (2) transferred the payment process to financial institutions; (3) opened new enrollment points for its urban beneficiaries; and (4) piloted an alternative targeting model in a small sub-set of areas. The focus of this paper is on a fifth component of the Urban Model: the changes to the school grants scheme, which proposed a budget-neutral change in the size of school grants. Specifically, grants were eliminated for primary education. In turn, its amount was increased by 22 to 40 percent amongst children enrolled in the next six years of school (middle-school and high school). The Oportunidades graduation grant (Jovenes con Oportunidades) was also increased by 30 percent. Table 1 compares the school grant schemes for 7^{th} to 12^{th} grades under the standard grant model and the Urban Model in 2015 for males and females.

An experimental impact evaluation was built into the original rollout of the Urban Model in two cities of central Mexico, Puebla and Ecatepec. Households joining the program for the first time were randomly assigned into one of three groups: a control group that would receive the traditional school grant scheme, a first treatment group that would receive the Urban Model described above (Treatment 1), and a second treatment group that, in addition to the Urban Model, would receive a payment based on academic performance (Treatment 2). Attanasio and Espinosa (2010) and Espinosa (2014) analyzed the results of this experiment, finding that after 18 months of implementation, the Urban Model resulted in positive and significant impacts on school enrollment only for girls and only in one of the two cities, Ecatepec. The magnitude of the impact was of 5-7 percentage points. No impacts were found amongst boys in Ecatepec or amongst boys or girls in Puebla. In both cities, eliminating the primary school grant had no effect on enrollment in this level. The authors noted that the performance pay was not adequately communicated to the students in the experiment so, for the purposes of the analysis, they merged the two treatments into one.

Espinosa (2014), in turn, analyzes the two treatments separately. The presence of impacts of the Urban Model varies across cities and also depends on whether the author groups children by age or by grade. She documents that the Urban Model led to a decrease in primary school enrollment of 3-4 percentage points in Ecatepec and Puebla from a baseline enrollment level of over 95 percent in the sample (which falls to 50 percent by middle school). In Ecatepec, she also found that Treatment 1 resulted in a decrease of enrollment amongst 16-18-year-old boys of 10 percentage points while Treatment 2 increased enrollment for girls ages 16-18 by 10 percentage points. In Puebla, she found that both treatments decreased enrollment of 13-15-year-old boys by about 8 percentage points. For girls in this age group, she also found a negative impact of both treatments; however, it is less robust across specifications and smaller in magnitude. It is worth noting that none of these impacts prevail when the data is analyzed by grade and not by child's age. The Ecatepec and Puebla experimental evaluation of the Urban Model focused only on two cities and had a short exposure period. To the extent to which the decision to enroll in school depends on its opportunity cost, it likely did not capture much of the spatial heterogeneity in the conditions of the local labor markets for the youth. In summary, it provides evidence at the very local level, but the effects may or may not extrapolate to other settings. Our analysis is a complement to this evidence.

Starting in September 2009, the Urban Model was implemented in a total of 263 out of 630 urban localities in the country.[10]. Between 2009 and 2013, all eligible new Oportunidades beneficiaries who were incorporated into the program roster in these 263 localities (treatment localities, henceforth) received benefits under the Urban Model scheme. Eligible Oportunidades beneficiaries incorporated into the program in this period in all other urban localities retained the traditional grant scheme (comparison localities), as did families enrolled before 2009 in treatment localities. Starting in 2014, and given that there was no conclusive evidence on the impacts of the change in school grants under the Urban Model at that date, Oportunidades decided to discontinue it (Dávila Lárraga, 2016). However, households originally enrolled under the Urban Model retained that benefit structure.

[10] Urban localities are those with a population of 15 thousand or more in 2009 and that met other eligibility conditions related to the supply of health and education services. The only urban localities excluded where those in the sample of an alternative health scheme that was also being evaluated as part of the Urban Model.

III. CONCEPTUAL FRAMEWORK

In this section, we propose a simple model of the relationship between grant amounts and schooling. A household maximizes the net present value of its only-child consumption stream (c_t) by deciding whether to send her/him to school ($s_t = 1$) or to work ($s_t = 0$). If the child is sent to school, her/his current human capital level h_t will increase by a school quality parameter $\alpha(h_t)$. If the child is sent to work, her/his human capital will yield wages $w(h_t)$. To send a child to school, the household must spend a fixed cost m, partially subsidized by the government through the school grant τ.

The household solves:

$$\max_{C} \sum_{t=0}^{N} \beta^t c_t$$

$$subjet\ to: c_t + s_t(m - \tau) = (1 - s_t)w(h_t)$$

$$and: h_{t+1} = h_t + \alpha(h_t)s_t$$

Letting V represent future consumption stream given h, this is equivalent to choosing s_t such that:

$$\max_{s_t \in \{0,1\}} [-m + \tau + \beta V(h_t + \alpha(h_t))\ ;\ w(h_t) + \beta V(h_t)]$$

Let φ be the household policy function:

$$\varphi(h; m, \tau, w, \alpha) = \beta v(h) - w(h) - m + \tau$$

Where $v \equiv \beta[V(h_t + \alpha(h_t) - V(h_t)]$. For any level of h, the child will be sent to school ($\varphi > 1$) if the present value of the returns to studying outweighs the costs of doing so—opportunity and direct net costs.

A permanent increase in the school grant under the Urban Model implies that:

$$\frac{\Delta\varphi}{\Delta\tau} = 1 + \beta\Delta v$$

Thus, the likelihood of going to school would increase more than proportionally to a reduction in the cost of schooling, as long as the return to studying is positive. Notice also that, as v is an increasing function of α, $\frac{\Delta\varphi}{\Delta\tau}$ is an increasing function of school quality.

IV. DATA AND SAMPLE

Our analysis of the Urban Model on school attainment uses data from three different administrative sources: (1) the Oportunidades beneficiary roster; (2) the Oportunidades survey of households' socioeconomic characteristics (ENCASEH) that is used to determine program eligibility; and (3) the Ministry of Education registries of standardized tests (ENLACE). Each of them is described below.

The Oportunidades roster is a household-level database that tracks compliance of program conditions such as school attendance and health check-ups down to the level of each individual household member. The roster contains six observations per individual per year. The operational rules of the program require that compliance with conditions is reported and cash transfers are disbursed every two months. From the moment a household is enrolled in the program, the roster includes bimonthly registries of program transfer. When an observation is missing, it means that the program has suspended the transfer—either temporarily or permanently—during that period. This can happen because of (a) administrative faults[11]; (b) the household fails to fulfill its conditions for more than two consecutive bimonthly cycles; or (c) the household is no longer eligible for the program.

When transfers are suspended due to administrative faults, the beneficiary (usually the female head of household) has to approach the Oportunidades representatives in her locality in order to resolve any inconsistency. In the second case, the program can suspend the monetary transfer until the fulfillment of the condition is again certified, in which case, transfers are reinstated. In the third scenario, a household becomes ineligible if following a reassessment of its socioeconomic conditions, the household fails to meet minimum criteria for continued eligibility[12]. In all of the cases described above, the roster will show a missing observation for one or more bimonthly cycles, resulting in that particular household having fewer than six observations per year. Data from the Oportunidades roster were available for the period between 2007 and 2013, covering two cohorts of new Oportunidades beneficiaries prior to the

[11] These faults include main beneficiary failing to collect her transfer or make any bank account transaction in two consecutive bimonthly cycles, an existing dispute over the transfers that household members should be receiving, among others.

[12] The periodic re-evaluation of socioeconomic conditions through the so called "recertification" process updates the household level information to confirm whether: (a) household per capita income is larger than an upper threshold known as the permanent socioeconomic verification line; or (b) household per capita income is larger than a lower threshold known as the minimum welfare line, but has no household members younger than 22 years old or women of reproductive age (15-49 years old).

start of the Urban Model (2007 and 2008), and tracking the first Urban Model cohort (2009) through three years of middle school (2010-2012) and the first year of high school (2013).

Our second data source is the ENCASEH, a socio-economic survey that determines eligibility to Oportunidades. It is first collected amongst all potential beneficiary households prior to their enrollment and later repeated, at least once every eight years, as part of the recertification process. The survey collects household and individual level data on health, education, employment, income, social program participation, consumption, and demographic characteristics. Prior to enrollment, the ENCASEH data are used to estimate per-capita income and determine eligibility. We use the ENCASEH as a source of baseline information, so for each individual in our sample we take the ENCASEH that was available immediately prior to the moment his or her household was enrolled in the program.

Our third source of data is the Ministry of Education (SEP) registries of standardized tests. The National Achievement Evaluation in School Facilities test (known as ENLACE by its Spanish acronym) is a mandatory examination taken by all students in each of the middle school grades and in their last year of high school, in both public and private schools. The test assesses two subject areas, language and math, and is meant to yield a general student assessment. Scores have no impact on student grades. For students completing high school, ENLACE is not an entry exam for college enrollment. Between 2008 and 2015 ENLACE aimed to cover all students in their last year of high school. In addition, between 2008 and 2013, ENLACE was administered to all students in first, second and third grades of middle school.

We merge the three data sets described above. The Oportunidades roster and the ENCASEH share common identifiers at the household and individual levels. The SEP-ENLACE registries were merged using the national personal identification number (CURP), which yielded an incomplete match, as described below. With this data, we construct proxies for school dropout and high school completion.

Neither dropout nor graduation are observed from official school records. However, the Oportunidades roster has information about program exit, which can be used to construct proxies for school dropout through 2013 as follows:

(1) CCT official exit: This variable comes from the program roster and captures exit from the Oportunidades program based on non-compliance with the school enrollment condition using official program registries. While this measure is closely linked to actual school dropout, it is only reported in the program roster for students in middle school and not for those in high school.

(2) CCT panel exit: This variable is constructed from the program roster and includes CCT official exit. It takes the value of 1 when a student exits the panel before the third bimonthly cycle of 2013 (when the 2009 cohort of new beneficiaries were completing the 10^{th} grade). This measure captures exits from the CCT program for any reason, not only non-compliance with the school enrollment conditionality because of dropout. We include this measure as a complement to the CCT official exit variable since it captures program exit at the critical juncture of middle to high school transition, including dropping out of school. We argue that these two estimates of dropout, the CCT official exit and CCT panel exit, represent upper and lower bounds of the true levels of school dropout, respectively.

At the high school level, in turn, we construct two proxy indicators for high school graduation using data through 2015-2016, when the 2009 cohort was expected to graduate from high school:

(3) CCT graduation grant: Upon high school graduation, Oportunidades beneficiaries are eligible for a graduation grant valued at approximately $400 USD. The graduation grant is not automatic, and must be claimed by the beneficiary upon providing proof of graduation. Given that not all beneficiaries claim the grant (~96 percent as reported by program directors), but that graduation is a pre-requisite, this can be thought of a lower-bound estimate for true graduation rates. Graduation grant data are available through February 2016, by when the 2009 cohort of new beneficiaries would have had approximately eight months to claim the grant after graduating from high school.

(4) Graduation test: All high school students are required to take the ENLACE exam at the end of their last year of high school. We matched the Oportunidades roster to the SEP-ENLACE data and constructed a dummy variable equal to 1 for all students who took the test. The merge was based on the CURP, the individual national identifier, and 15

percent of CURP were missing in the Oportunidades roster[13]. As a result, this variable is a likely lower-bound estimate for true graduation rates.

While the selection of the 263 treatment localities was not well documented, it appears that the program prioritized localities based mainly on operational convenience at the time of the Urban Model's implementation. In these localities, the grant scheme was applied exclusively to newly eligible families with children enrolled in middle and high school starting 2009.[14] Existing beneficiaries, as well as new enrollees in non-Urban Model (control) localities, remained under the traditional grant scheme. It is important to note that the eligibility criteria for program enrollment remained the same in Urban Model and comparison localities[15]. However, recruitment efforts to enroll new beneficiaries was not uniform across time and space. In 2009, the program expanded its outreach efforts in Urban Model localities, thus increasing the proportion of new beneficiaries that were enrolled in those localities relative to comparison localities.

Our analysis focuses on the effects of increased grants for students who were starting 7th grade (i.e. middle school) as new Oportunidades beneficiaries during the 2009-2010 school year.[16] The start of the school year coincides with the fifth bimonthly payment cycle (corresponding to the September-October cycle in the program roster database), so we limit the sample to enrollments in the fifth bimester of 2009 (20095). We focus on this cohort for the following reasons. First, students in treatment localities had the longest exposure to the Urban Model of any cohort, allowing us to study the long-term effects of the increase in school grants. Second, unlike new Oportunidades beneficiaries in grades 8 and higher, the cohort of 7th graders was not subject to attrition through previous years of middle and high school. Third, this cohort was directly preceded in 2008 by new program entrants in the 7th grade under the traditional grant scheme in treatment and comparison localities. The 2008 cohort of new beneficiaries (enrolled during the fifth bimester), just one-year senior, are closest in terms of grade and age and thus

[13] Attrition based on CURP is not correlated with treatment status. However some baseline characteristics of the sub-sample of students without CURP are different from the analysis sample, suggesting a potential alteration to the sample composition.

[14] Note that while the Urban Model officially started in the third bimester of 2009, most families with school-age children enroll as of the fifth bimester of 2009, at the start of the school year. Our analysis sample thus includes families enrolled in the later period.

[15] See Dávila Lárraga (2016) for a description of the enrollment process.

[16] We exclude students in four cities (Reynosa, Puebla, Juarez and Ecatepec) that piloted an alternative targeting model in 2009. Puebla and Ecatepec were also the sites of the original Urban Model experiment described in Attanasio and Espinosa (2010) and Espinosa (2014).

make the most comparable baseline in the context of a difference-in-difference analysis. Finally, we focus on treatment effects for the 2009 cohort due to data availability. Only new participants enrolled in 2009 or before would have completed high school by 2015 and been able to claim the Oportunidades graduation grant during the 2015-2016 school year. Collection of the graduation grant for new participants entering middle school in 2010 onwards is not observed in the available data.

After applying our inclusion criteria, the analysis sample consists of 19,902 new Oportunidades beneficiaries in 461 localities enrolled in the first year of middle school in 2008 and 2009. Of these, the sample includes 10,844 students from 246 Urban Model localities and 9,058 students in 216 comparison localities who maintained the traditional grant scheme. The sample is also about equally split between gender, with 10,049 females and 9,853 males. As noted above, the program's recruitment efforts intensified in Urban Model localities in 2009, resulting in 9,816 new beneficiaries in 246 localities in 2009 compared to 1,028 new beneficiaries in 110 localities the year before. The enrollment pattern in comparison localities was opposite, with 7,127 new beneficiaries in 185 localities in 2008, and 1,931 new beneficiaries in 163 localities in 2009. We additionally use the 2007 cohort of new enrollees in treatment and comparison localities to conduct falsification tests.

V. IDENTIFICATION STRATEGY

We estimate the effects of the increased grants on our proxies for high school dropout and graduation using a difference-in-difference model which compares the changes in outcomes for new program beneficiaries in intervention localities between 2008 and 2009, with changes in outcomes for new enrollees in non-intervention localities during the same time period. Our baseline period is September-October 2008, when all new beneficiaries received the same grant scheme.

To minimize the potential serial correlation in time-series data and simplify the analysis, we collapse all variables to one observation per individual for the difference-in-difference estimation, and present separate Cox proportional hazard model estimates (Cox, 1972) using the complete time series.

We estimate a standard difference-in-difference specification:

$$Y_{ijt} = \Phi_t + \Phi_j + \delta D_{ijt} + X'_{ijt}\beta + \varepsilon_{ijt}$$

Where Y_{ijt} is the schooling outcome for individual i in locality j and enrollment cohort t. Φ_t is a time fixed effect equal to 1 for the 2009 cohort and 0 for the 2008 cohort, and Φ_j is a locality fixed effect equal to 1 for treatment localities and 0 otherwise. D_{ijt} is a dummy variable equal to the interaction between Φ_t and Φ_j, that is equal to 1 for individuals enrolled in 2009 in treatment localities. δ is the treatment parameter of interest, interpreted as an average treatment on the treated effect (ATOT), the average effect of the Urban Model on schooling outcomes in intervention localities. X'_{ijt} is a vector of individual covariates including age, parental education and household characteristics (number of household members, assets in the household, whether the household has piped water inside the home, electricity, domestic gas, the number of rooms, and whether the family owns the dwelling). Given that previous analysis of the Urban Model identified different impacts by gender (Attanasio and Espinosa, 2010), we estimate separate regressions for males and females. Robust standard errors, clustered at the locality level, are presented in parenthesis.

We also use Cox proportional hazard models (Cox, 1972) to estimate the impact of larger school grants under the Urban Model scheme on the likelihood of dropping out of school and of graduating from high school, using the time-series data. These models' framework requires two possible outcomes for each beneficiary on the sample, in our case: the individual is enrolled in school (right-censored) or not (failed) and the individual has graduated from high school (failed) or not (right censored). The proportional hazard model assumes that the fraction of the beneficiaries that dropped out (or graduated) after bimonthly cycle t, relative to those that still enrolled (did not graduate) in that cycle is:

$$h(t) = h_0(t)\exp(\delta\Phi_j + X'_{ij}\beta)$$

Where $h_0(t)$ is the baseline hazard of dropping out (graduating) from school after bimester t. We assume that the effect of the covariates are constant over time, and that the hazard ratio for two observations is independent of time *t*. Following the same notation as in the difference-in-difference model, Φ_j is a locality fixed effect equal to 1 for treatment localities and 0 otherwise, and δ is the treatment parameter of interest and X'_{ijt} is a vector of individual covariates that includes the same variables as described before. Consistent with the earlier strategy, we estimate separate models for males and females. Standard errors are clustered at the locality level.

VI. RESULTS

Table 2 presents descriptive statistics of the change in outcomes and difference-in-difference coefficients for the pre-treatment cohorts of 2007 and 2008 in treatment and comparison localities. We observe that the pre-treatment trends are balanced in all outcomes at the 5 percent significance level. For females, the CCT panel exit and graduation test outcomes are different at the 10 percent level, but run in the opposite direction from the expected effects under treatment. That is, we observe a positive pre-intervention trend for panel exit, our proxy for school dropout in middle school and first year of high school, and a negative trend for graduation test, our proxy for high school graduation. A majority of the demographic indicators and assets are balanced in the pre-treatment period.

Table 3 presents our main difference-in-difference results. Effects of the Urban Model on our first proxy of school dropout in middle school, official exit, is depicted in column 1 for females, and in column 5 for males. Results show that larger school grants have a large effect on reducing the probability of dropping out of middle school. As a result of the Urban Model, the likelihood of officially exiting the Oportunidades roster due to non-compliance of schooling conditions in middle school declines by 2.4 percentage points for females and 4.1 percentage points for males. Counterfactual exit rates are 4.0 percent and 5.7 percent, respectively, in comparison localities. In other words, students with larger cash grants have dropout rates that are between 60 and 70 percent lower than comparison localities. It is worth noting that official exit rates in the control group are of a similar magnitude as official dropout rates reported by the education sector in middle school for 2013 (4.5 percent for females and 6.5 percent for males)

(INEE, 2014). The joint significance test for the pre-trends analysis are not statistically significant at conventional levels, lending credibility to the above results (p-values are presented in the second to last column of Table 3). We also calculate the price elasticity of demand as the ratio of the percent change outcome and the percent change in the grant amount. The estimated elasticity for official exit is -2.60, implying that the relationship between the relative increase in transfers and school dropout in middle school is elastic.

Our second proxy for school dropout, CCT panel exit, includes students who exit the beneficiary roster unaccounted for in middle school and the first year of high school, and as such captures school dropouts as well as beneficiaries who leave the program for a host of other reasons but may remain enrolled in school. As such, this proxy vastly over-estimates true school dropout rates and should be interpreted as the effect of increased grants on remaining in the CCT program, rather than school. We include this variable in the analysis because it is available for the key transition year between middle and high school, while the official exit outcome is only registered for middle school students. Our estimates show that the Urban Model results in a reduction of panel exit by 14.4 percentage points for females and 11.1 percentage points for males. Relative to counterfactual panel exit in comparison localities, this represents a decline in the probability of leaving the program through the first year of high school of 22.0 percent and 15.5 percent for females and males, respectively. It is interesting to note that while the elasticity of demand for school dropout as measured by official exit appears to be elastic, the elasticity of demand on the panel exit outcome is below 1, suggesting that beneficiaries' demand for program exit for any reason is slightly price inelastic.

We next analyze whether larger school grants result in increased school attainment as measured by our two proxies of high school graduation. The impact of the Urban Model on the likelihood of collection of the Oportunidades graduation grant increases by 11.4 percentage points for females and 8.6 percentage points for males, over counterfactual graduation grant collections of 26.4 percent and 19.9 percent respectively, suggesting a relative increase in grant collection for both genders of the order of 43 percent. The impact of the Urban Model on our second proxy outcome, taking the ENLACE test in the final year of high school, is of 9.2 percentage points for females and of 8.6 percentage points for males, representing a relative increase of 27.1 percent and 33.5 percent respectively. Given that students take the ENLACE

test one to two months prior to graduation, and that the graduation grant requires proof of graduation but not all beneficiaries collect the grant, we again interpret these effects as upper- and lower-bound estimates of the Urban Model's effect on high school graduation. The elasticity for graduation proxies ranges between 1 and 1.59, suggesting that demand is elastic.

While the above estimates provide average treatment on the treated effects of the Urban Model, we can also express impacts as hazard ratios. Results of the Nelson-Aalen estimates of the cumulative hazard function are presented graphically in Figures 1 to 4, and the hazard ratios for the 2009 (treatment) cohort are presented below the difference-in-difference coefficients (fourth row) in Table 3. In Figures 1 to 4, the function estimates for students that enter the program during the pre-treatment period (Pre-20085) are on the left side of the panel. We observe that students from the treatment and comparison localities have the same dropout and graduation probabilities over time. However, on the right-hand panel for the treatment period (Post-20095), we observe that students incorporated under the Urban Model have a significantly lower probability of dropout and a higher probability of graduation over time, compared to those incorporated under the traditional grant scheme in 2009 in comparison localities. The figures also highlight the high dropout rates that occur in the transition between middle school and high school, and the effectiveness of the larger grants in preventing dropouts at that critical juncture.

The hazard ratios for the 2009 cohort are presented in the fourth row of Table 3. As expected from the graphical analysis, the (exponential) coefficient for the treatment variable is significantly different from one for most of the outcomes with the exception of official exit for females. For female students, the estimated hazard ratio for the other outcomes are significant at the 1 percent level, with a ratio of 0.55 for panel exit, and 1.60 and 1.45 for the graduation grant and test graduation outcomes, respectively. That is, females from treatment localities are 45 percent less likely to drop out of school based on panel exit and between 60 and 45 percent more likely to graduate from high school based on our two proxy measures. The estimated hazard ratios for males are all statistically significant at the 1 percent level, with ratios of 0.87 and 0.48 for dropout proxies, and 1.73 and 1.52 for graduation outcome proxies. In other words, males from Urban Model localities are 13 percent less likely to drop out of middle school based on the official exit outcome, and 52 percent less likely to leave the program prior to the first year of high school based on the panel exit outcome. Males are 52 to 73 percent more likely to graduate

high school based on the graduation grant and graduation test proxies, respectively. For the graduation proxy outcomes, results of the hazard models are consistent with the difference-in-difference estimates, albeit of a larger magnitude.

We next use data from the ENLACE test to explore whether there are heterogeneous impacts of the Urban Model across students of diverse academic ability. Our measure of ability are the students' scores in the ENLACE test when they were in 7th grade. We construct percentile dummies of the internally standardized ENLACE scores and include an interaction of the treatment variable, the time dummy and a dummy equal to one if the ENLACE percentile was equal to *X* (where *X = 99, 98…1*) and to zero otherwise. Results are presented graphically on the left-hand-side panel of Figure 5. The vertical axis represents the parameter estimates of the triple interaction effect and on the horizontal axis are the ENLACE percentiles. The right-hand-side panel of the same figure illustrates the probability of graduating from high school as a function of the same ENLACE percentiles. Results are presented for ENLACE language scores, ENLACE math scores, and for ENLACE total scores. We estimate effects for males and females separately.

Two patterns arise from the analysis in Figure 5. First, the triple interaction effect is never significantly different from zero in any segment of the ENLACE distribution. In other words, the effect of the Urban Model on high school graduation appears to have been the same across different ability levels as measured by ENLACE. On the other hand, the 7th grade ENLACE score is positively associated to the likelihood of high school graduation, which validates the assumption of using this variable as a proxy for academic ability.

VII. ROBUSTNESS TESTS

The key identifying assumption for a causal interpretation of the difference-in-difference results is that the change in dropout and completion rates in comparison localities is an unbiased estimate of the counterfactual change. While the "parallel trends" assumption required by the difference-in-difference model cannot be tested, we conduct a number of validity checks with cohorts of newly enrolled beneficiaries in the period before the start of the Urban Model in the fifth bimester of 2009. In order to document validity of this assumption, we test whether trends

in the treatment and comparison localities were the same in the pre-treatment period (Table 4). For each of the models, we cannot statistically reject the hypothesis that the pre-treatment bimonthly cycle dummies are the same for treatment and comparison localities. This suggests that our proxies for dropout and completion had parallel trends across treatment groups in the period before the Urban Model was implemented. The complete set of interaction coefficients on our schooling outcome proxies for newly enrolled cohorts between the 5th bimester of 2007 (20075) and 1st bimester of 2009 (20091) are presented in Table 4.[17] Furthermore, Panel A of Figures 1-4 presents the survival analysis using a Cox proportional hazard model for the 2008 baseline cohort (5th bimester) in treatment and comparison localities. We observe that for both the fixed effects estimates in Table 4 and in the graphical analysis, in the absence of treatment, dropout and graduation patterns for new beneficiaries in treatment and comparison localities follow very similar trends and are never significantly different. The statistical equivalence of the change in outcomes in treatment and comparison localities in the pre-intervention period lends credibility to the "parallel trends" assumption, that is, that in the absence of treatment, outcomes in Urban Model localities would have followed the same trend as in comparison localities.

We additionally implement a falsification test that replicates the difference-in-difference model in the pre-treatment period, comparing outcomes between newly enrolled students in middle school in the 2008 and 2007 cohorts, falsely and purposively assuming that those enrolled in 2007 constitute the control group and those in 2008 the treatment group. Results are presented in Table 5. In all but one of the models the interaction between treatment and time yields small point estimates that are not significantly different from zero. For the graduation test outcome in the sub-sample of females, the coefficient is marginally significant at the 10 percent level, but goes in the opposite direction. These results, coupled with the graphic analysis in Panel A of Figures 1-4, lend credibility to a causal interpretation of the effects of the Urban Model on our proxy measures of dropout and graduation for middle and high school.

[17] There were no new enrollments between the first and fifth bimesters of 2009 or in the 6th bimester of 2008. There are no payments for school grants in the 4th bimester of each year (summer break).

VIII. COST-BENEFIT ANALYSIS

In this section we compare the estimated income from increased educational attainment to the marginal cost of increased scholarships under the Urban Model.[18] We estimate benefits as the additional income from the average increase of 0.671 years of schooling, assuming a return to schooling of 8 percent per year (Ordaz, 2007 and Morales-Ramos, 2011) and an income of 2.12 and 2.55 minimum salaries for middle and height school graduates, respectively.[19] In 2016, the minimum daily salary was $73.04 Mexican Pesos.[20] We assume that high school graduates do not continue to higher education, and work until a retirement age of 65, so benefits are accrued over 47 years.[21] The marginal cost of the scholarship is the present value (2016) of the difference between the Urban Model scholarships and the traditional grant scheme (Table 1).[22] Using the graduation test outcome as our preferred estimates (Table 3 columns 4 and 8 for women and men, respectively), and assuming a discount rate of 12 percent, we find that the benefit to cost ratio is 2.22 for women and 2.34 for men. That is, the expected benefits in terms of labor market returns to increased education are more than twice the cost of increasing the middle and high school grants by 27 percent and 30 percent for females and males, respectively. These results are robust to assuming half the total number of years of employment (benefit to cost ratio of 2.08 and 2.19 for women and men, respectively), and also hold when taking the bottom 95 percent confidence interval of the additional years of schooling attributed to the Urban Model (benefit to cost ratio of 1.95 and 2.16 for women and men, respectively).

IX. CONCLUSION

While the effects of conditional cash transfers on primary school enrollment, attendance and dropout are well documented, few studies address their impact on longer-term outcomes like

[18] A complete cost-benefit analysis including a full accounting of the Urban Model's benefits and costs is outside the scope of this paper. On the cost side, this would include the opportunity cost of increased middle and high school student's time in school, the marginal costs to the educational system for delivering additional years of schooling, and the potential effects on primary school students from the elimination of grants at that level, amongst others. Benefits include potential delays in fertility and other non-pecuniary benefits from increased educational attainment. Given that data are not available to inform the parameters required for a full accounting, we estimate a simple benefit to cost ratio of the expected labor market returns to increased education relative to the marginal cost of the increased grants. We assume no general equilibrium effects on wages in local markets.

[19] Survey of Labor Trajectories https://www.gob.mx/cms/uploads/attachment/file/98540/Presentacion-Trayectorias_Laborales.pdf accessed November 19, 2017.

[20] Minimum general salary as reported by the National Commission for Minimum Salary: http://www.conasami.gob.mx/pdf/salario_minimo/2016/historico_2016.pdf, accessed November 19, 2017.

[21] We assume that individuals work full time (22 days per month) starting at age 18 and until retirement at 65.

[22] We estimate the net present value of the increased grants under the urban model using grants corresponding to the period 2009-2015 and includes the graduation grant.

high school graduation. The literature is even scarcer regarding how changes in the amount of school grants affect educational outcomes, particularly in middle and high school, where dropout rates are much higher. We provide evidence that larger cash grants for middle and high school students in Mexico's national CCT program reduced dropouts and increased high school graduation rates.

Starting in 2009, middle and high school grants for new beneficiaries in a sub-set of 263 urban localities were increased by 27 percent for females and 30 percent for males. New beneficiaries in all other urban localities continued to be enrolled under the traditional grant scheme. We use administrative data to analyze effects of the change in grants on proxies of dropout and high school graduation. We estimate a difference-in-difference strategy that compares changes in educational outcomes for new enrollees in 2008 and 2009 across treatment and comparison localities.

Our most conservative estimate is that high school students under the urban model increase high school graduation rates by 27.1 percent for females and 33.5 percent for males. There is no evidence of differential effects across students based on baseline academic ability. The price elasticity of demand for high school graduation ranges from 1 to 1.59, suggesting that the demand for education in this population is elastic with respect to the grant. Based on the predictions of the model presented in section 3, this result suggests that the perceived returns to schooling by beneficiary households is positive.

An interesting pattern that emerges from the difference-in-difference analysis and hazard models is that the graduation rate for students in comparison localities in the 2009 cohort is lower than both the 2009 treatment group as well as the 2008 cohorts. A year-by-year analysis of number of years of education acquired (Table A1), suggests that this may be the result of substantially higher dropout rates during secondary school and in the critical transition to high school in comparison localities. Since the entry of the 2009 cohort entered secondary school during the 2009 economic crisis in Mexico, we speculate that larger school grants may have helped buffer the negative consequences of the 2009 recession, helping low income households compensate for outside labor market opportunities and signaling the value of education.

In addition to increasing school grants in middle and high school, the Urban Model also eliminated grants in primary school. The objective was to make the alternative grant scheme cost-neutral for the Oportunidades program by re-allocating grant funds designated for primary-aged children to households with middle school and high school students. Unfortunately, we are not able to identify effects of the Urban Model on primary school enrollment using available administrative data sources, since outcomes for children in treatment localities are not registered. However, with near universal enrollment in primary school and based on available evidence, the effects for primary enrollment in the urban context are thought to be small (Attanasio and Espinosa, 2010 and Espinosa, 2014).

Relative to existing literature on the effects of grant sizes in the context of conditional cash transfer programs, our results show substantial long-run effects on reducing school dropout and increasing graduation. Given the timing of the introduction of the increased grants, coinciding with a severe economic crisis, additional research is required to assess whether alternative grant structures remain cost-effective in other settings. Nevertheless, our results suggest that it may be worthwhile for CCT programs to revisit the optimal grant scheme. Adjustments to the grant structure, including larger grant amounts, may provide a promising policy lever for improving educational outcomes amongst low-income students.

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Tables and Figures

Table 1: Oportunidades School Grants (2015-2017)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Female				Male			
Grade	Traditional Grant (MXN)	Urban Model Grant (MXN)	Difference	% increase under Urban Model	Traditional Grant (MXN)	Urban Model Grant (MXN)	Difference	% increase under Urban Model
7th	540	675	135	0.25	515	640	125	0.24
8th	600	735	135	0.23	540	680	140	0.26
9th	660	805	145	0.22	570	715	145	0.25
10th	990	1,365	375	0.38	865	1,225	360	0.42
11th	1,055	1,365	310	0.29	925	1,225	300	0.32
12th	1,120	1,365	245	0.22	980	1,225	245	0.25
Grad. Grant	4,890	6,333	1,443	0.30	4,890	6,333	1,443	0.30
Average over all grades	827.5	1051.67	224.17	0.27	732.5	951.67	219.17	0.30

Source: Prospera (https://www.gob.mx/cms/uploads/attachment/file/206673/MONTOS_MENSUALES_PRIMER_SEMESTRE_2017.pdf), last accessed November 18, 2017). School grants are paid every two months over a 10-month period, for a maximum of 5 transfers per year conditional on compliance with conditionalities. The Graduation Grant corresponds to the "Jovenes con Oportunidades" grant.

Table 2: Pretreatment balance (2007-2008)

VARIABLES	Female			Male		
	Δ Treatment	Δ Control	Diff in Diff	Δ Treatment	Δ Control	Diff in Diff
CCT official exit	-0.025	-0.015	-0.009	-0.028	-0.018	-0.010
	(0.011)	(0.014)	(0.018)	(0.014)	(0.019)	(0.024)
CCT panel exit	0.010	-0.055	0.065*	-0.014	-0.015	0.002
	(0.033)	(0.017)	(0.037)	(0.028)	(0.020)	(0.035)
Graduation grant	0.007	0.048	-0.041	0.021	0.003	0.018
	(0.030)	(0.017)	(0.034)	(0.025)	(0.021)	(0.033)
Graduation test	-0.046	0.030	-0.076*	-0.008	-0.020	0.011
	(0.032)	(0.022)	(0.034)	(0.032)	(0.023)	(0.040)
Age	-0.024	-0.093	0.069	0.024	-0.119	0.143**
	(0.057)	(0.033)	(0.066)	(0.056)	(0.036)	(0.067)
Father's Education (years)	-0.361	0.183	-0.544**	-0.002	0.094	-0.096
	(0.245)	(0.114)	(0.27)	(0.196)	(0.113)	(0.226)
Mother's Education (years)	0.226	0.502	-0.276	0.276	0.443	-0.167
	(0.252)	(0.163)	(0.030)	(0.205)	(0.158)	(0.259)
Household size	-0.041	-0.121	0.080	0.081	-0.184	0.265
	(0.101)	(0.175)	(0.202)	(0.116)	(0.148)	(0.188)
Own home	-0.020	-0.124	0.104*	0.033	-0.140	0.173***
	(0.047)	(0.038)	(0.061)	(0.040)	(0.036)	(0.054)
Number of rooms	0.114	-0.037	0.151*	0.119	-0.017	0.135*
	(0.047)	(0.072)	(0.086)	(0.056)	(0.047)	(0.073)
Sanitary service	0.041	0.025	0.016	0.067	0.015	0.051
	(0.031)	(0.017)	(0.035)	(0.032)	(0.018)	(0.037)
Water	0.085	0.077	0.008	0.098	0.074	0.024
	(0.037)	(0.036)	(0.051)	(0.038)	(0.032)	(0.050)
Electricity	0.009	0.020	-0.011	0.039	0.017	0.022
	(0.016)	(0.011)	(0.020)	(0.013)	(0.009)	(0.016)
Domestic gas	-0.080	-0.040	-0.040	-0.100	-0.037	-0.062
	(0.046)	(0.025)	(0.052)	(0.043)	(0.025)	(0.050)
Refrigerator	-0.027	0.002	-0.029	0.009	0.007	0.003
	(0.037)	(0.032)	(0.049)	(0.036)	(0.026)	(0.044)
Washing Machine	0.009	-0.002	0.011	0.010	0.006	0.004
	(0.016)	(0.009)	(0.018)	(0.019)	(0.007)	(0.020)
VCR	0.032	0.054	-0.022	0.064	0.052	0.013
	(0.035)	(0.017)	(0.039)	(0.026)	(0.018)	(0.032)
Telephone	-0.016	-0.031	0.016	-0.043	-0.024	-0.019
	(0.021)	(0.016)	(0.026)	(0.023)	(0.015)	(0.028)
Vehicle	0.014	0.008	0.006	0.011	0.002	0.009
	(0.015)	(0.008)	(0.017)	(0.018)	(0.007)	(0.019)

Note: Standard errors (in parenthesis) clustered at the community level. * significant at 10%, ** significant at 5%, *** significant at 1%

Table 3: Difference in difference and Hazard models

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Female				Male			
	Dropout Proxies (Middle school)		Graduation Proxies (High School)		Dropout Proxies (Middle School)		Graduation Proxies (High School)	
	CCT official exit	CCT panel exit	Graduation grant	Graduation test	CCT official exit	CCT panel exit	Graduation grant	Graduation test
Treatment*Time	-0.024**	-0.144***	0.114***	0.092***	-0.041***	-0.111***	0.086***	0.086**
	(0.010)	(0.032)	(0.034)	(0.034)	(0.013)	(0.032)	(0.029)	(0.035)
Treatment	-0.009	0.045*	-0.010	0.020	0.004	0.024	0.003	0.013
	(0.008)	(0.026)	(0.027)	(0.030)	(0.011)	(0.027)	(0.024)	(0.029)
Time	0.017**	-0.005	-0.088***	0.094***	0.016**	-0.036*	-0.079***	-0.076***
	(0.007)	(0.022)	(0.021)	(0.020)	(0.008)	(0.019)	(0.016)	(0.020)
Hazard Ratio 20095	0.979	0.547***	1.600***	1.448***	0.870***	0.483***	1.725***	1.518***
	(0.052)	(0.088)	(0.228)	(0.125)	(0.045)	(0.070)	(0.231)	(0.154)
Elasticity (Dif-Dif)	-2.60	-0.95	1.59	1.00	-2.85	-0.62	1.44	1.12
Control mean	0.040	0.655	0.264	0.339	0.057	0.712	0.199	0.256
Observations	10,049	10,049	10,049	8,431	9,853	9,853	9,853	8,485
Pre-trends p-value	0.542	0.243	0.752	0.128	0.006	0.673	0.990	0.620
R-squared	0.014	0.069	0.052	0.072	0.017	0.068	0.042	0.054

Note: All regressions include sex, age, parents' education and household characteristics. Columns (4) and (8) sample only includes students with CURP. Standard errors (in parenthesis) clustered-robust at the community level. Hazard seeform (in parentheses) clustered at the community level. * significant at 10%, ** significant at 5%, *** significant at 1%. The price elasticity of demand for dropout is calculated with the average grant increase in middle school of 23% for females and 25% for males. The price elasticity of demand for graduation is calculated with the average grant increase in middle and high school of 27% for females and 30% for males.

Table 4: Pre-trends analysis

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Female				Male			
	Dropout Proxies (Middle school)		Graduation Proxies (High School)		Dropout Proxies (Middle School)		Graduation Proxies (High School)	
	CCT official exit	CCT panel exit	Graduation grant	Graduation test	CCT official exit	CCT panel exit	Graduation grant	Graduation test
Treatment*20075	0.029	0.108	0.024	-0.081	0.005	-0.003	0.004	-0.003
	(0.034)	(0.123)	(0.104)	(0.126)	(0.044)	(0.107)	(0.094)	(0.134)
Treatment*20076	0.030	-0.037	0.079	-0.018	-0.013	-0.090	0.020	0.047
	(0.041)	(0.135)	(0.110)	(0.160)	(0.055)	(0.138)	(0.123)	(0.163)
Treatment*20081	0.070	0.052	0.105	-0.113	0.079	-0.040	0.022	-0.004
	(0.070)	(0.158)	(0.141)	(0.198)	(0.077)	(0.139)	(0.124)	(0.162)
Treatment*20082	0.094	0.084	0.156	-0.063	-0.132	0.141	-0.042	0.094
	(0.072)	(0.209)	(0.182)	(0.210)	(0.097)	(0.165)	(0.165)	(0.223)
Treatment*20083	-0.069	0.163	0.036	-0.008	0.082	0.070	-0.090	0.329
	(0.082)	(0.208)	(0.199)	(0.217)	(0.133)	(0.250)	(0.230)	(0.231)
Treatment*20085	0.013	0.156	-0.007	-0.127	0.016	-0.053	0.037	0.000
	(0.028)	(0.119)	(0.101)	(0.136)	(0.042)	(0.108)	(0.098)	(0.129)
Treatment*20091	-0.025	0.127	0.001	-0.238	-0.002	-0.056	-0.016	-0.090
	(0.047)	(0.157)	(0.128)	(0.155)	(0.070)	(0.114)	(0.111)	(0.149)
Control mean	0.041	0.681	0.261	0.345	0.059	0.725	0.216	0.282
Observations	7,568	7,568	7,568	6,387	7,521	7,521	7,521	6,546
F-test	0.865	1.441	0.586	0.752	0.708	0.841	0.314	0.716
F-p-value	0.534	0.188	0.768	0.628	0.665	0.553	0.947	0.658
R-squared	0.010	0.035	0.031	0.050	0.010	0.032	0.024	0.036

Note: All regressions include bimester and community fixed effects. Columns (4) and (8) sample only includes students with CURP. Standard errors (in parenthesis) clustered at the community level. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5: Falsification Test (pre-treatment period)

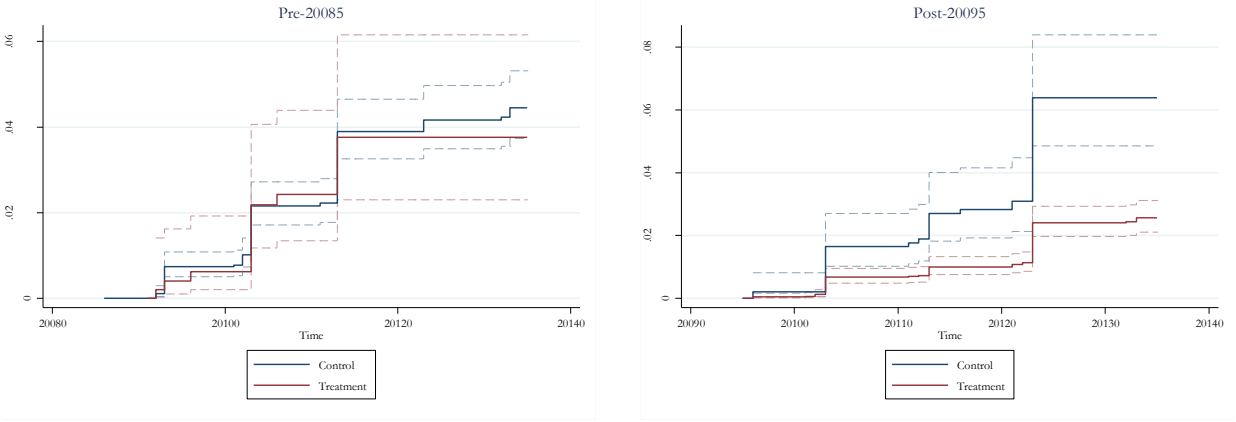
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Female				Male			
	Dropout Proxies (Middle school)		Graduation Proxies (High School)		Dropout Proxies (Middle School)		Graduation Proxies (High School)	
	CCT official exit	CCT panel exit	Graduation grant	Graduation test	CCT official exit	CCT panel exit	Graduation grant	Graduation test
Treatment*Time	-0.013	0.051	-0.034	-0.064*	-0.015	-0.017	0.033	0.023
	(0.017)	(0.034)	(0.033)	(0.036)	(0.022)	(0.033)	(0.032)	(0.038)
Treatment	0.006	0.004	0.018	0.084***	0.017	0.044	-0.030	-0.010
	(0.016)	(0.031)	(0.025)	(0.031)	(0.020)	(0.032)	(0.028)	(0.033)
Time	-0.015	-0.052***	0.042**	0.016	-0.016	-0.014	0.002	-0.022
	(0.014)	(0.015)	(0.016)	(0.021)	(0.019)	(0.019)	(0.019)	(0.021)
Hazard Ratio 20085	1.185***	0.862	0.963	1.067	1.120*	1.158	1.058	1.082
	(0.076)	(0.219)	(0.113)	(0.098)	(0.071)	(0.235)	(0.132)	(0.122)
Control mean	0.041	0.674	0.267	0.350	0.060	0.724	0.215	0.280
Observations	6,603	6,603	6,603	5,584	6,558	6,558	6,558	5,736
R-squared	0.015	0.060	0.054	0.088	0.017	0.050	0.038	0.047

Note: All regressions include sex, age, parents' education and household characteristics. Columns (4) and (8) sample only includes students with CURP. Standard errors (in parenthesis) clustered-robust at the community level. Hazard seeform (in parentheses) clustered at the community level. * significant at 10%, ** significant at 5%, *** significant at 1%.

Figure 1: Nelson- Aalen estimate of the cumulative hazard function for dropouts

Female

CCT official exit



CCT panel exit

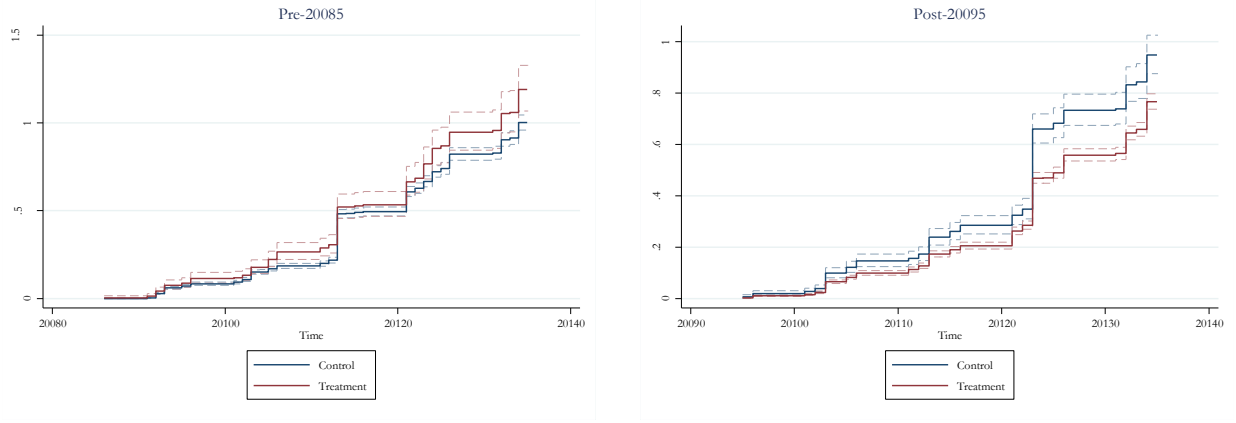
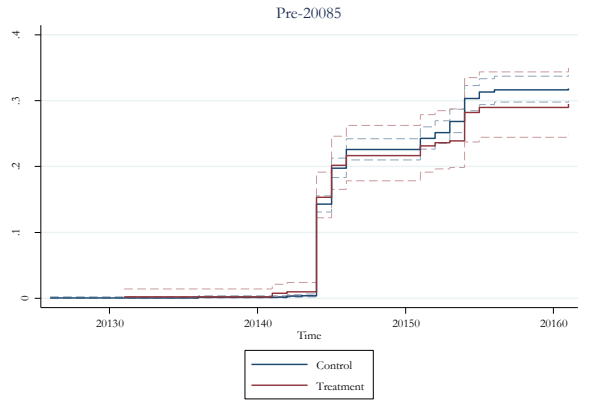
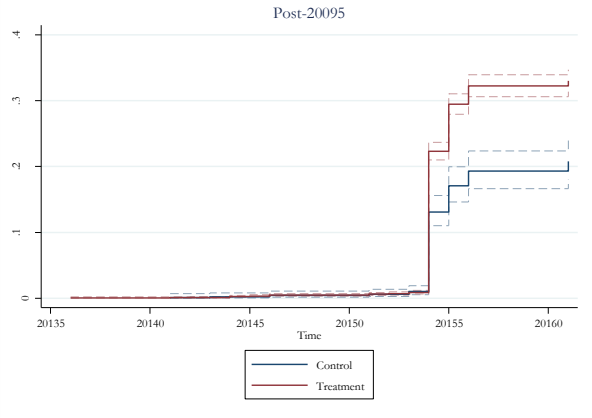


Figure 2: Nelson- Aalen estimate of the cumulative hazard function for graduation

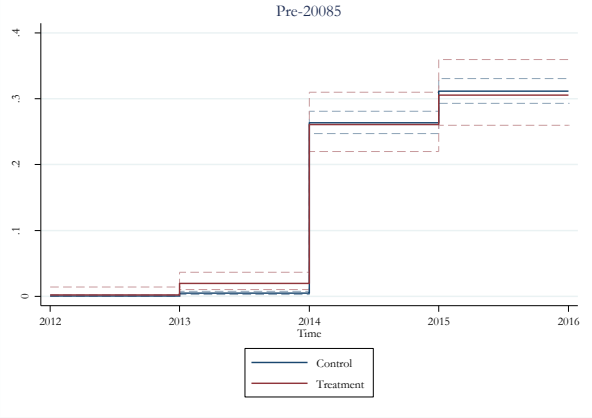
Female

Graduation grant





Graduation test



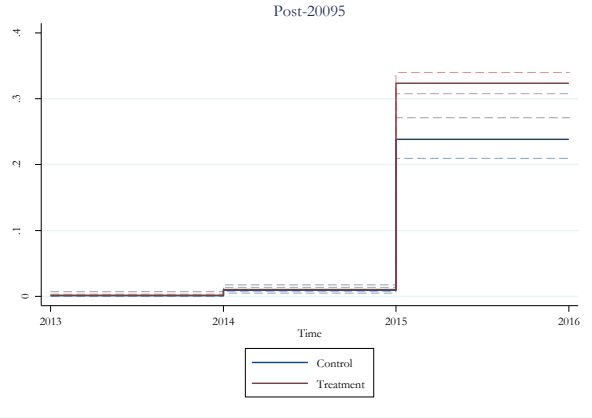
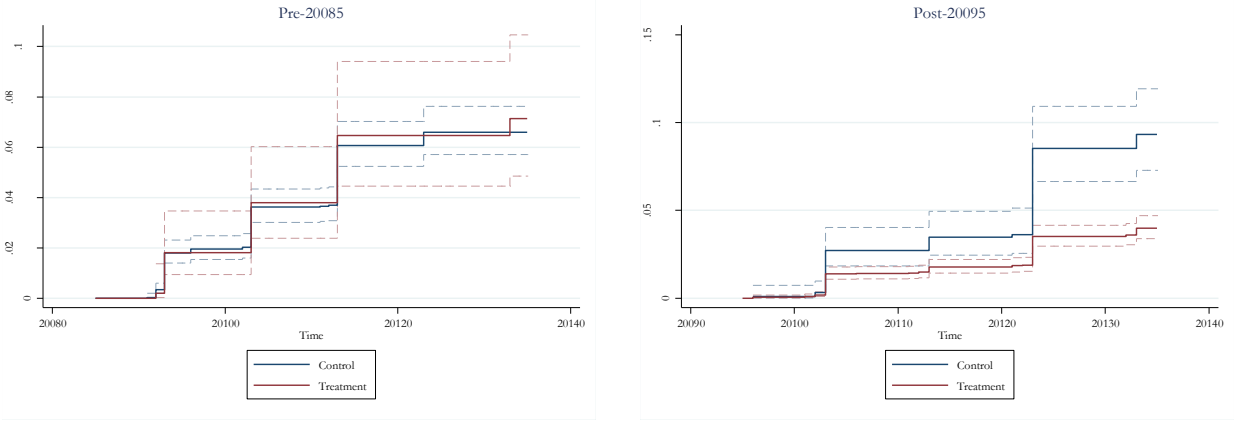


Figure 3: Nelson- Aalen estimate of the cumulative hazard function for dropouts

Male

CCT official exit



CCT panel exit

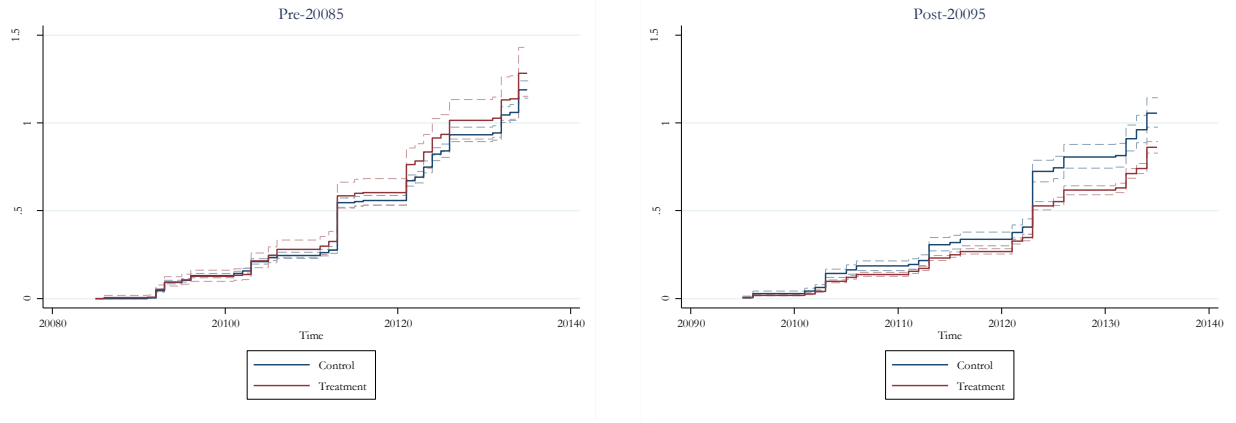
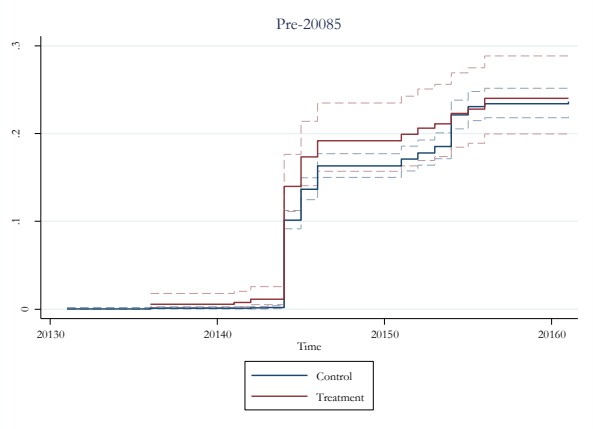
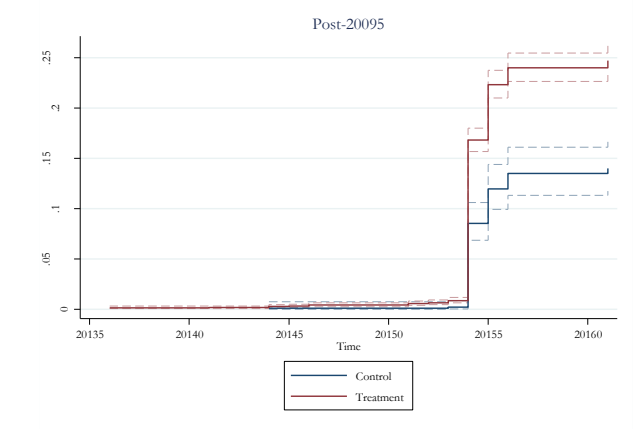


Figure 4: Nelson- Aalen estimate of the cumulative hazard function for graduation

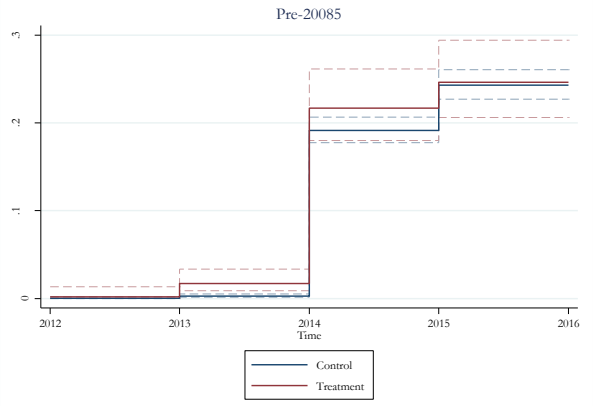
Male

Graduation grant





Graduation test



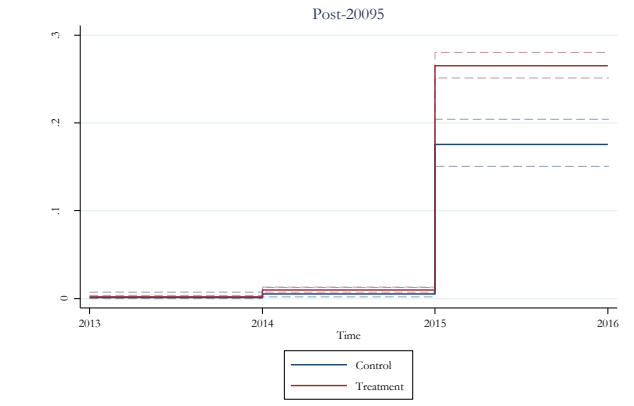
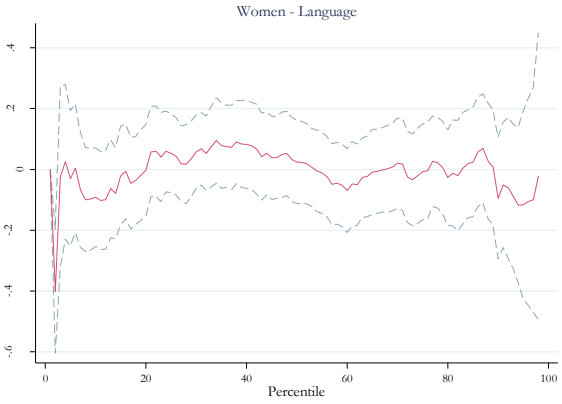
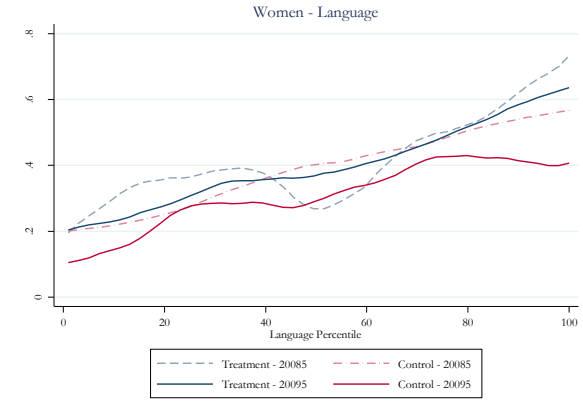
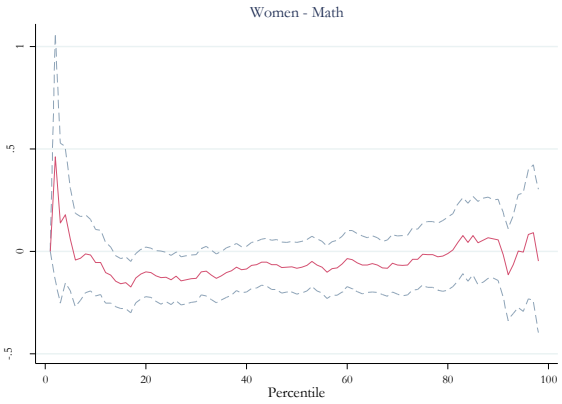


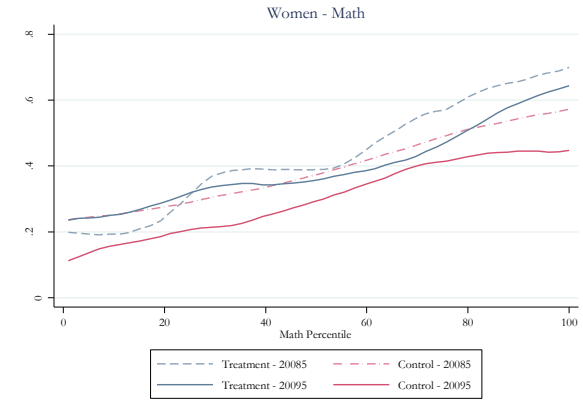
Figure 5: Heterogeneity of impact of the Urban Model on High School Graduation (Graduation Test) by baseline ENLACE test (percentile in first year of middle school)

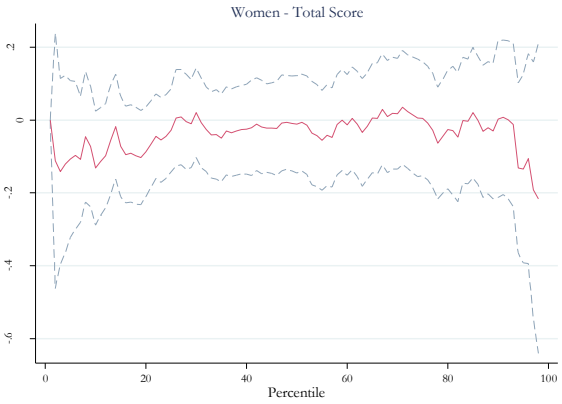
Female











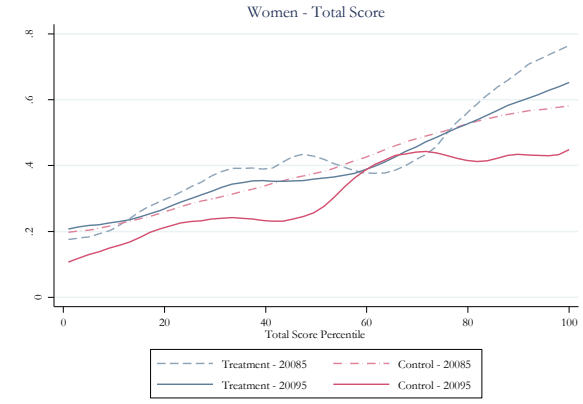
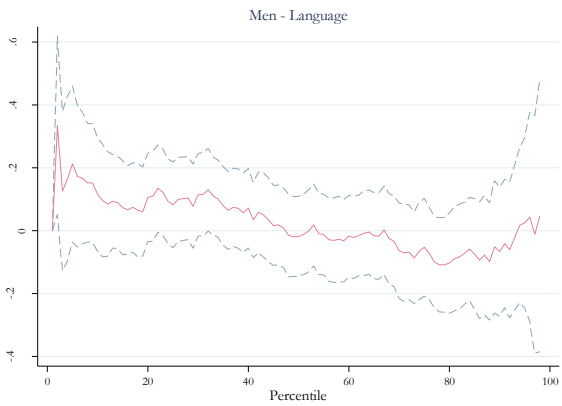
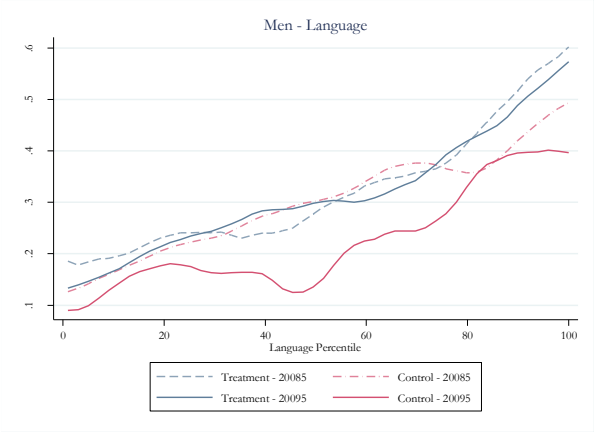
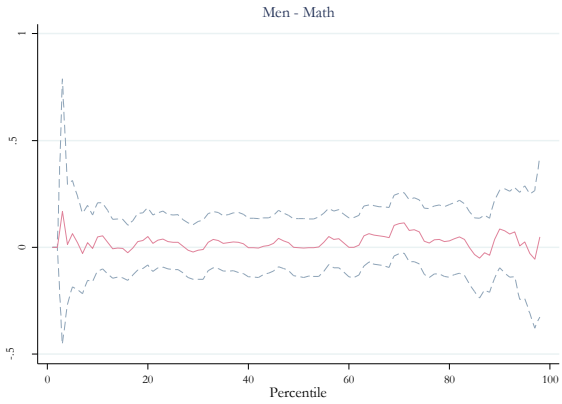


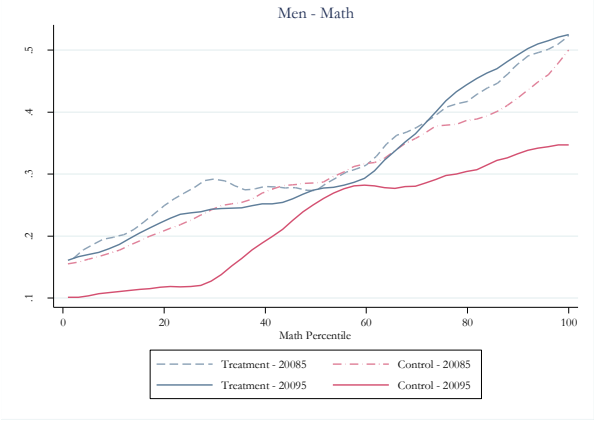
Figure 5: Heterogeneity of impact of the Urban Model on High School Graduation (Graduation Test) by baseline ENLACE test (percentile in first year of middle school)

Male









Appendix

Table A1: Distribution of years of schooling for pre- and post intervention cohorts

Education Years	20085 (PRE)		20095 (POST)	
	T	C	T	C
6	7.88	8.22	8.26	12.17
7	10.51	9.65	10.88	13.83
8	24.32	25.00	19.15	26.98
9	14.59	12.95	12.67	9.48
10	5.45	6.64	5.86	6.42
11	2.53	2.60	8.36	6.94
12	34.73	34.94	34.81	24.18
Mean Years of Schooling	9.46	9.48	9.61	9.02

Table A2: Estimated Years of Education under Urban Model

	Years of Education	
VARIABLES	(1)	(2)
Treatment*Time	0.616***	0.671***
	(0.117)	(0.127)
Treatment	-0.019	-0.004
	(0.104)	(0.112)
Time	-0.460***	-0.586***
	(0.077)	(0.082)
Control mean	9.379	9.254
Observations	19,902	17,744
R-squared	0.006	0.007

Note: Standard errors (in parenthesis) clustered-robust at the community level. *** p<0.01, ** p<0.05, * p<0.1

Table A3: Cost Effectiveness Analysis for Urban Model Cash Grants in Middle and High school

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Present value of Urban Model incremental transfers (2016). Interest rate = 12%.	Estimated additional Schooling Years from Urban Model (Table A2)	Value of additional year of schooling (return of 8%; minimum salary in 2016 of 73.04 pesos per day (INEGI))	Yearly Return to Urban Model [2*3]	Years of Employment (18 to 65 years old) - assumes no continued education	Net present value of returns to Urban Model (2016). Discount rate = 12%.	Benefit/Cost Ratio [6/1]
Expected return for beneficiary women (43.1% high school graduation)	$6,372.34	0.671	$3,556.21	$1,707.34	47	$14,158.66	2.222
Expected return for beneficiary men (34.2% high school graduation)	$5,670.80	0.671	$3,497.18	$1,601.15	47	$13,278.08	2.341

Table A4. Cost Effectiveness Sensitivity Analysis

	Sensitivity Analysis - NPV with half of employment years (23.5)	Benefit/Cost Ratio with 23.5 years of employment	Sensitivity Analysis - NPV with lower bound of estimated additional schooling years (95% CI)	Benefit/Cost Ratio at lower bound impact of 95% confidence interval of program impacts on education
Expected return for beneficiary women (43.1% high school graduation)	$13,235.82	2.08	$12,445.22	1.95
Expected return for beneficiary men (34.2% high school graduation)	$12,412.63	2.19	$12,238.62	2.16