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Opportunities for Higher Education: The Ten-Year Effects of Conditional Cash Transfers on Upper-Secondary and Tertiary Enrollments

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Abstract

This paper investigates the effect of the conditional cash transfer program Oportunidades on enrollment in higher education ten years after the initiation of the program. We use data from the International Food Policy Research Institute (IFPRI) and employ a Regression Discontinuity approach. We find that 1997-eligible households had higher 2007 high-school and college enrollment relative to non-eligible households. However, the program's benefits are heterogeneous depending on school access which we proxy by distance. Further, we find positive tertiary education benefits for individuals in treated households who were too old to qualify for benefits directly, indicating potential positive externalities for individuals residing in eligible households. Finally, our findings suggest that one possible mechanism for these educational benefits is this effect of Oportunidades on educational aspirations related to relaxing budget constraints, changing perceptions on the returns to education, and reducing marginal costs of college attendance.

Keywords: Economic Development, Mexico, Secondary Education, Tertiary Education, School Access, Aspirations, Regression Discontinuity Design.

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1. Introduction

Conditional cash transfer (CCT) programs, of which the Oportunidades program in Mexico is a pioneer, now operate in over 50 countries worldwide. CCT programs provide regular cash payments to poor households with the condition that they support the education and health of their children. These programs typically include conditions such as requiring participating families to enroll their children in school and maintain good attendance, as well as receiving regular health check-ups and proper vaccinations.

Numerous positive benefits are associated with CCT programs, including a broad range of improved schooling and health outcomes. The United Nations described CCTs as "*one of the most significant developments in global social policy since the expansion of social security in industrialized countries.*"[2] The World Bank allocated $2.4 billion in 2009 alone to help countries develop CCT programs, and the Inter-American Development Bank invested over $8 billion in them from 2000 to 2010 [30]. Recent studies find that participation in Oportunidades increases birth weight, height-for-age and reduces the prevalence of stunting [15] as well as improves motor development, receptive language and cognitive outcomes [16]. Oportunidades also reduces the age of school entry, increases grade progression, increases primary school enrollment and completion, as well as enhances the likelihood of enrolling in secondary education [5, 8, 12].

Behrman, Parker, and Todd [6, 7, 9] evaluate the effects of Oportunidades on educational attainment six years after its initiation by exploiting the randomized rollout of the program's pilot phase. The authors compare the 2003 educational outcomes of treated communities where the program was initially offered in 1997 with those of control communities where the program was withheld for 18 months. Using experimental and difference-in-differences methods, they find robust, significant benefits of Oportunidades on schooling.

In this paper, we investigate the extent the educational benefits of Oportunidades extend to higher education by looking at 2007 educational outcomes, ten years after program initiation. We exploit the discontinuity in program participation at the program's

eligibility cutoff and use a Regression Discontinuity (RD) approach to estimate the effects of Oportunidades on enrollment in upper secondary and tertiary education. We do this not only for individuals covered by the program but also for adult household members who did not directly qualify for program benefits. To our knowledge, no existing empirical work investigates the educational externalities of these programs on individuals who reside in a treated household but do not themselves qualify for program benefits. Further, while numerous papers estimate CCTs' effect on health outcomes and primary education, only a few look at secondary education and none examine the impact on tertiary schooling.

We find that individuals in Oportunidades eligible households had higher 2007 enrollment in both secondary and tertiary school relative to those above the eligibility cutoff. That is, our results suggest that Oportunidades has an impact on educational outcomes beyond the directly targeted grades of 3-12. We further find that these benefits differ depending on the level of school access. When we control for school access as proxied by walking time and distance to school, we see that those who live closest to school realize the largest benefits from the program. We also find positive higher education benefits for individuals in treated households who are too old to directly qualify for benefits themselves – suggesting positive educational externalities within treated households. Finally, we find that Oportunidades appears to increase educational aspirations in eligible households. This offers one potential mechanism through which the program is affecting higher educational outcomes. The improvement in aspirations appears to be, in part, due to a relaxation of the household's budget constraint, increased exposure to positive role models and a better understanding of education cost/benefits.

The rest of the paper is organized as follows. Section 2 provides background on the Oportunidades program and describes the data. Section 3 presents the research design and the econometric methods used in this paper. Section 4 presents the empirical findings, while section 5 concludes.

2. Oportunidades and Education

Mexico's Oportunidades program, formerly named Progresa and currently Prospera, aims to target poverty by improving nutrition, health, and education through the means of cash transfers to eligible households. These transfers are accompanied by the requirement that all family members receive regular health checkups and that the mother and children in grades 10-12 attend monthly health information sessions. Education is targeted through another subsidy, which requires that children maintain a minimum 85% monthly attendance rate in the eligible grade levels 3-12. Each child qualifies for a separate school subsidy, which can be discontinued if the child fails a class more than once. The scholarships, approximately between 10 - 70 USD per family, are given in monthly installments and increase with grade level to account for the higher opportunity cost of school attendance. In middle school, scholarships are higher for females than for males, as female school attendance tends to decline for that age group dramatically. The scholarships for upper-secondary school can be given directly to attending adolescents.

Oportunidades was implemented in two phases, a randomized pilot phase beginning in 1997, followed by the full rural rollout of the program 18 months later in 1998. Program eligibility was determined using a marginality index. Communities were grouped into seven broad geographical regions, and a separate analysis was performed for each region to calculate a region-specific marginality index score. Using a discriminant analysis, the score was calculated based on individual and household characteristics gathered from census surveys [34]. Families are eligible for the program if their household marginality index is lower than that of their region-specific mean.[1]

2.1. Data

The data for the program comes from the Oportunidades External Evaluation administered by the International Food Policy Research Institute (IFPRI). These data include baseline household and community surveys conducted in 1997 before the program was

[1] The original calculated marginality index was the inverse—it increased with level of poverty. For ease of exposition, we modify it so that our measure decreases with poverty. We also normalize the index to have mean zero and a standard deviation of one.

implemented, with subsequent rounds occurring in 1998, 1999, 2000, 2003 and 2007. In this paper, we employ the 1997 and 2007 rounds. These surveys followed 24,000 households from 506 communities in the seven states of Guerrero, Hidalgo, Michoacán, Puebla, Querétaro, San Luis Potosí and Veracruz. Of these 506 communities, 320 were treated during the pilot phase, and 186 were treated 18 months later. The surveys contain specific modules on individual, family, and community characteristics including household marginality index scores, school enrollment status and educational aspirations. Of the 24,000 households initially surveyed, 16,093 (89,251 individuals) appear in both 1997 and 2007.[2]

In Mexico, basic education is divided into three stages: primary school (*primaria or elementary*), comprised of grades 1-6 for students aged 6 to 12 years old; Junior high (*secundaria or middle school*), consisting of grades 7-9 for students aged 12 to 14 years old; and upper secondary (*preparatoria/técnica, or high school´*), comprised of grades 10-12 for students aged 14+. *Primaria* and *secundaria* are mandatory by law, while upper secondary is not. For upper secondary school, students have a choice between a *technical* program (*técnica*) that provides vocational training with the aim of receiving employment directly upon graduation, or a *preparatoria* program that prepares the student to continue their education at a university. Tertiary education (college) the choice is between Licenciatura and Normal school. Lincenciatura follows the US education model with a 4-year undergraduate level bachelor's degree. Normal or "teachers' college" trains upper secondary graduates to be teachers.

To examine how the effect of Oportunidades changes depending on school access we supplement our data with data collected from the Directorate General of Higher Education for Professionals of Education (DGESPE) which contains addresses for all Normal Colleges in Mexico. These addresses were turned into GPS locations to determine the distance from community centers to closest Normal College.

Table 1 summarizes the 1997 pre-treatment and 2007 post-treatment educational en-

[2]To check if attrition affects our results, we conducted all the analysis using attrition weights as in Behrman et al. (2009, 2011). Our findings remain qualitatively the same when we use these weights. We also compare baseline characteristics between households who remained in sample and those who atritted. The two groups appear largely similar. Results available upon request.

rollment for households above (ineligible) and below (eligible) the eligibility cutoff. Eligible households are poorer and are less likely to be enrolled in school at all grade levels. However, the enrollment rate of the eligible group increases more rapidly between 1997 and 2007 than that of the wealthier ineligible group. Table 1 does not include information for technical school enrollment in 1997, as it was not part of the questionnaire that year. Overall, enrollment rates are similar to the national average for rural households in Mexico.

Table 1: Educational Statistics

Panel A. Dependent variables measured in 1997	age	Male			Female		
		all	In Optunidades? yes	no	all	In Optunidades? yes	no
Preparatory School Enrollment	14-18	0.047 (0.212)	0.042 (0.201)	0.134 (0.341)	0.040 (0.196)	0.034 (0.181)	0.151 (0.358)
Tertiary School Enrollment	19-28	0.015 (0.120)	0.012 (0.108)	0.053 (0.223)	0.012 (0.111)	0.009 (0.093)	0.062 (0.242)
	28+	0.010 (0.098)	0.007 (0.084)	0.052 (0.222)	0.006 (0.079)	0.004 (0.066)	0.036 (0.186)

Panel B. Dependent variables measured in 2007	age	Male			Female		
		all	In Optunidades? yes	no	all	In Optunidades? yes	no
Technical School Enrollment	14-18	0.004 (0.060)	0.004 (0.060)	0.000 0.000	0.008 (0.090)	0.008 (0.091)	0.000 0.000
Preparatory School Enrollment	14-18	0.152 (0.359)	0.151 (0.358)	0.296 (0.460)	0.171 (0.377)	0.168 (0.374)	0.386 (0.490)
Tertiary School Enrollment	19-28	0.011 (0.104)	0.010 (0.097)	0.047 (0.212)	0.012 (0.110)	0.010 (0.101)	0.064 (0.244)
	28+	0.006 (0.076)	0.004 (0.062)	0.038 (0.190)	0.003 (0.054)	0.002 (0.041)	0.023 (0.150)

Table 2 reports 1997 baseline household characteristics according to eligibility status. Here we see that eligible and ineligible households differ on various dimensions. Eligible households have more household members but fewer rooms. They are also less likely to have access to water, electricity and, own land, as well as have lower rates of literacy and educational attainment. Eligible households are however more likely to have a mother and father in the home. This is most likely because richer rural households are more likely to see the migration of a parent.

Table 2: Household Characteristics by Oportunidades Eligibility

Panel C. Covariates associated with school enrollment in 1997	Male			Female		
	all	In Optunidades? yes	no	all	In Optunidades? yes	no
Literacy	0.758 (0.428)	0.750 (0.433)	0.928 (0.258)	0.684 (0.465)	0.674 (0.469)	0.868 (0.338)
Educational Attainment	3.163 (3.184)	3.053 (3.087)	5.593 (4.174)	2.867 (3.089)	2.762 (2.988)	5.102 (4.174)
Meets With Child's Teacher	0.223 (0.416)	0.225 (0.418)	0.165 (0.372)	0.375 (0.484)	0.381 (0.486)	0.236 (0.425)
How Often Meets With Teacher	0.441 (0.497)	0.446 (0.497)	0.244 (0.435)	0.339 (0.474)	0.342 (0.475)	0.206 (0.407)
School Expenses	188.250 (534.372)	185.436 (526.729)	275.882 (738.972)	331.818 (1,761.491)	259.854 (1,069.181)	2,175.319 (7,128.316)
Father at Home	0.548 (0.497)	0.552 (0.497)	0.451 (0.497)	0.491 (0.499)	0.495 (0.499)	0.391 (0.488)
Mother at Home	0.619 (0.485)	0.624 (0.484)	0.512 (0.499)	0.546 (0.497)	0.551 (0.497)	0.443 (0.496)
Rooms in Home	1.924 (1.162)	1.870 (1.123)	3.099 (1.371)	1.932 (1.173)	1.877 (1.131)	3.099 (1.425)
Access to Water on Land	0.371 (0.483)	0.362 (0.481)	0.576 (0.494)	0.381 (0.486)	0.371 (0.483)	0.588 (0.492)
Access to Water in Home	0.070 (0.255)	0.063 (0.243)	0.217 (0.412)	0.072 (0.258)	0.065 (0.246)	0.216 (0.412)
Home has Electricity	0.738 (0.440)	0.727 (0.445)	0.978 (0.148)	0.742 (0.437)	0.731 (0.443)	0.982 (0.134)
Members in Houshold	6.508 (2.664)	6.582 (2.668)	4.888 (1.983)	6.508 (2.690)	6.587 (2.694)	4.817 (1.971)
Owns Land	0.663 (0.473)	0.661 (0.473)	0.710 (0.454)	0.655 (0.475)	0.653 (0.476)	0.700 (0.458)
Age	24.201 (20.156)	23.861 (20.092)	31.688 (20.104)	24.304 (20.066)	23.938 (19.996)	32.112 (19.964)

3. Empirical Strategy

Much of the previous research evaluating the impact of Oportunidades employs difference-in-difference methods exploiting the randomized rollout of the program's pilot phase. However, ten years after the program's initiation, the randomized implementation essentially equates to a randomized duration of treatment, rather than a randomized treatment. Consequently, difference-in-difference methods exploiting the randomized implementation are not ideal for examining the longer-run treatment effects of this program. Indeed, Behrman, Parker, and Todd [9] demonstrate that outcomes for households treated under the two phases of the program converge over time. In addition, participation in Oportunidades exhibits a sharp discontinuity around the eligibility cutoff, in that 97% of all eligible households were treated by the program. Figure 1 illustrates this sharp discontinuity. We, therefore, exploit this discontinuity to examine the effect of Oportunidades on educational outcomes using a sharp regression discontinuity (RD) approach.

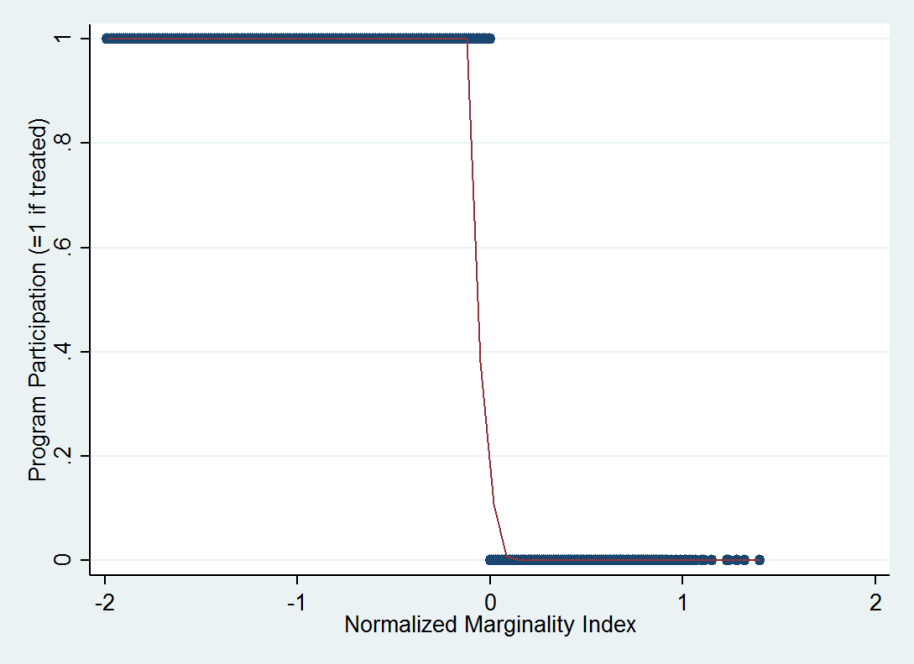

Figure 1. Raw averages of program participation as a function of the Normalized Marginality Index that determined eligibility

Buddelmeyer and Skofias [11] demonstrate the validity of the RD design for evaluating Oportunidades by comparing results found using difference in differences (DD)

methods to those using an RD approach. They conclude that the RD estimates of program impact agree with experimental estimates using DD. Recent economic studies using an RD design estimate the impact of Oportunidades on prevalence of overweight [4], deforestation [3], contraceptive use [24], child health [25], child labor, and school attendance [11]. An RD approach effectively means that we are limited to estimating the local average treatment effects (LATE) of Oportunidades. Nonetheless, this is policy relevant as it indicates what would happen with upper secondary and tertiary enrollment if the program expanded to cover slightly better off households, by changing the eligibility threshold.

We employ a quasi-experimental RD design to measure the long-term impact of Oportunidades on enrollment in higher levels of education. Our specification is

$$Y_{ij}^{a} = \alpha_i{}^{a} + \beta_{rd}{}^{a} E_{ij}{}^{a}\left(MI_{ij}\right) + \gamma_j^{a} + \varepsilon_{ij}^{a} \tag{1}$$

$$E_{ij}\begin{cases} 1 & if\ MI_{ij} < Ths_j \\ 0 & otherwise \end{cases},$$

where Y_{ij}^{a} is the outcome variable of interest observed in 2007 (i.e. enrollment rate in preparatoria, técnica, or college) for individual i in community j. The superscript a indicates membership in one of the three 2007 age cohorts: 14-18, 19-28, and 28+ years old. These age groups correspond to 1997 age groups of 4-8, 9-18, and 18+ years old. Individuals over the age of 28 in 2007 would have been adults at the time of the program's initial rollout. These individuals, therefore, would not be direct beneficiaries of the program even if they resided in a treated household. Consequently, any effect on these individuals would be indirect.

Ths_j represents the eligibility threshold score in community j. $E_{ij}{}^{a}$ is an indicator equal to one if individual i was eligible for Oportunidades in 1997, and zero otherwise. The eligibility indicator $E_{ij}{}^{a}$ is a function of the 1997 normalized marginality index MI_{ij} and Ths_j. Because the eligibility threshold varies by community, we also include a vector of community fixed effects, γ_j^{a}, to allow for an accurate comparison of individuals where community characteristics differ based on the threshold score. β_{rd}^{a} is the regression discontinuity parameter to be estimated and captures the cohort specific

effect of Oportunidades on our educational outcomes of interest. Since the treatment ${E_{ij}}^a$ depends on MI_{ij} in a deterministic way, the sharp RD design must be used.[3] Using the sharp design gives

$$E(Y_{ij}|MI_{ij} \; = \; Ths_j \; - \Delta) \simeq E(Y_{ij}|MI_{ij} \; = \; Ths_j \; + \; \Delta)$$

Individuals just to the left or right of the threshold score can be thought of as identical since, without the treatment, the unconditional mean values of Y are the same. It can then be shown that for a sharp RD the average treatment effect can be found by taking the difference between the mean values of Y from the eligible and ineligible.

$$\beta_{rd} \; = \; Y^- \; - \; Y^+ = \lim_{MI \; \to Ths} E\left(Y_{ij} \; \middle| \; MI_{ij} \; = MI\right) - \lim_{Ths \to MI} E(Y_{ij}|MI_{ij} = MI) \tag{2}$$

where we assume that $E\left(\alpha_i \; \middle| \; MI_{ij} \; = MI\right)$ and the conditional mean function $E\left(\varepsilon_{ij}^a \; \middle| \; MI_{ij}\right)$ are both continuous at $MI \; \to Ths$. If, however, ${\beta_{rd}}^a$ varies across individuals, then β_i identifies the local average treatment effect (LATE) for the subgroup of individuals around the threshold point. One-sided kernel regressions are used to find the unconditional mean estimates for each outcome variable.

$$Y^- = \; \frac{\sum_{i=1}^{n} Y_i * I_i * K(u_i)}{\sum_{j=1}^{n} I_i \; * K\left(u_i\right)} \quad and \; Y^+ = \; \frac{\sum_{i=1}^{n} Y_i * (1 - I_i) * K(u_i)}{\sum_{j=1}^{n} (1 - I_i) * K\left(u_i\right)}$$

Where I_i equals 1 if $MI_{ij} \leq Ths_j$, $K(u_i)$ is the kernel, $u_i \; = \frac{MI_{ij} - Ths_j}{h}$ and h is the bandwidth. Bandwidth and kernel choices follow that of Mccarry [27] and Imbens & Lemieux [22]. Kernel-weighted seemingly unrelated estimation and triangular kernel are used in the reported results. As results can vary widely based on bandwidth choice, only those found significant using robust standard errors at multiple bandwidths are considered.

A required assumption for identification in the RD framework is that the discontinuity in our education outcomes of interest is the result of the discontinuity in treatment. Consequently, we must assume that no other educational determinant also changes dis-

[3]Results produced using a fuzzy regression discontinuity were similar to those found using a sharp design. Results are available upon request.

Table 3: School Enrollment Rates β_{rd}

	Combined	Male	Female
Preparatory School	-0.0556	-0.0817	0.109
14 to 18	(0.0635)	(0.105)	(0.112)
Technical School	0.0160**	0.0139***	0.0230*
14 to 18	(0.00811)	(0.00522)	(0.0130)
Tertiary	-0.00211	0.00758	-0.0307
19 to 28	(0.0205)	(0.0283)	(0.0318)
Tertiary	0.0188***	0.0259***	0.0119**
28+	(0.00511)	(0.00859)	(0.00576)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

continuously at the eligibility threshold. To check this assumption, we test for a discontinuity around the eligibility threshold for several 1997 covariates likely correlated with education using equations (1) and (2). The covariates we examine are those often employed in general education models [7, 9, 8] and include age, sex, father at home, mother at home, number of rooms, access to electricity, access to water, owns land, and literacy. For each covariate, we fail to reject that it is continuous around the eligibility threshold, supporting the required identification assumption. We also perform a placebo test by checking for outcome discontinuities at false thresholds. We find no program effect at false thresholds, which further supports that our estimates are causally identified.[4]

4. Results

Table 3 reports the effects of Oportunidades on school enrollment estimated from equation (1) separately for males and females.

The first and second panels of Table 3 report the estimated effects of Oportunidades on technical and preparatory school enrollment separately. Interestingly, Oportunidades appears to improve enrollment in technical school but not in preparatory school for 14 to 18-year-olds just below the eligibility cut-off, relative to those just above it. Below we explore school access as a possible explanation for the differential effects of the program on preparatory relative to technical school. Participation in the program increases the likelihood of technical school enrollment by approximately 1.39% and 2.3% for males

[4]Results from this covariate analysis can be found in Appendix A.

and females, respectively[5].

The bottom two panels of Table 3 report the estimated effect of Oportunidades on tertiary school enrollment for individuals between the ages of 19 and 28, and those over 28. Eligibility for Oportunidades significantly improves the likelihood of tertiary enrollment for individuals older than 28 years old. Household program eligibility increases the likelihood that males and females in this age cohort were enrolled in tertiary school by approximately 2.59% and 1.19%, respectively. Interestingly, these individuals were over the age of 18 at the beginning of the program. Therefore, they did not directly receive benefits for their schooling. This suggests that Oportunidades exerts positive educational externalities within eligible households. This may be due to relaxing household's budget constraint by subsidizing the education of younger household members and thus allowing for increased expenditure on the education of older household members. It may also be due to the program affecting the educational aspirations of all members of eligible households regardless of being direct beneficiaries. Further, our results suggest that Oportunidades has an impact on educational outcomes beyond the program's targeted grades of 3-12.

4.1. School Access

While CCT programs provide incentives for educational attainment, the extent that increased incentives translate into increased enrollment also depends on the existing school infrastructure available to beneficiaries. Table 3 seems to indicate that Oportunidades increased enrollment in technical school, but had little to no effect on enrollment in preparatory school. However, if there are no preparatory schools in the area, then improved incentives may not be enough to increase enrollments in preparatory school.

In our data, less than 3% of communities reported a preparatory school in their area, while over 97% of preparatory age children indicated that walking was the only way to get to school. As travel time increases, the transaction cost of attending school increases, and thus the net benefit of the program decreases. To account for this, we modify equation

[5]While the point estimates are different for males and females their difference is not statistically different from zero.

Table 4: Preparatory School Enrollment Rates by Travel Time to School β_{rd}

Age	Combined	Male	Female
10 mins	0.138**	0.110	0.163*
14 to 18	(0.0632)	(0.0917)	(0.0850)
15 mins	0.257*	0.670*	0.0491
14 to 18	(0.152)	(0.358)	(0.210)
30 mins	0.139	0.265	0.00496
14 to 18	(0.0897)	(0.168)	(0.139)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

(2) to estimate the effect of Oportunidades on preparatory school enrollment conditional on travel time to school,

$$Y_{ij}^{a,t} = \alpha_i{}^{a,t} + \beta_{rd}{}^{a,t} E_{ij}{}^{a,t} \left(MI_{ij}\right) + \gamma_j^{a,t} + \varepsilon_{ij}^{a,t} , \tag{3}$$

where the superscript t indexes travel time to school for travel times up to 10 minutes, 15 minutes, and 30 minutes. Table 4 reports results from estimating equation (3) for 14-18 year olds.[6]

By breaking up the sample by travel time to school, we see that those who live closest to school realize the largest benefits from the program and that these benefits decrease as the travel time increases. The effect of the program on males living within 10 minutes of a school is not statistically significant. This is likely due to the fact that most males in this group are already enrolled regardless of eligibility. On the other hand, Oportunidades significantly improves preparatory high school enrollment for females reporting a travel time of 10 minutes or less to the nearest school. This is consistent with the additional incentives given to females as well as their lower likelihood of being enrolled in the absence of the program. As travel time increases, the likelihood of preparatory enrollment decreases. However, the program benefits appear to sufficiently mitigate that added cost for males living within 15 minutes of the nearest preparatory school. For those living within 15 minutes of school, males just below the eligibility cutoff are 67% more likely to

[6]The data only collected information on distance to preparatory school. We are thus not able to account for distance to technical. However, the large number of technical schools suggests that access to them may not pose a problem for our sample individuals.

Table 5: Normal School Enrollment Rates by Distance to School β_{rd}

	Combined	Male	Female
20 miles	0.0948*	0.689**	0.00101
19 to 28	(0.0576)	(0.289)	(0.0484)
60 miles	0.0543	0.501*	0.0351
19 to 28	(0.0598)	(0.301)	(0.0560)
120 miles	0.0261	0.216	0.0142
19 to 28	(0.0334)	(0.157)	(0.0383)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

be enrolled in preparatory school than those just above the cutoff. Interestingly, program eligibility has no statistically significant effect on females living within 15 minutes of a preparatory school. This may be due to households being more reluctant to send females to school at longer distances. Social norms and concern over the increased vulnerability of females to physical harm and violence may be related to this. Consequently, it is possible that the transaction costs of female enrollment may increase with travel time at a faster rate than that for males. For travel time over 30 minutes, the program is unable to compensate for the increased cost of distance and has no statistically significant effect on preparatory enrollment for both genders.

We also observed in Table 3 that there seemed to be no college enrollment affect for individuals aged 19 to 28. If, however, we follow the same logic of that for preparatory schools, this may be due to a lack of access. Using information from the DGESPE, we can generate GPS location data and join that with IFPRI community center location data to find the distance to closest Normal College. We can then use equation 3 as we did before allowing for differential access to Normal Colleges,

$$Y_{ij}^{a,d} = \alpha_i{}^{a,d} + \beta_{rd}{}^{a,d} E_{ij}{}^{a,d} \left(MI_{ij}\right) + \gamma_j^{a,d} + \varepsilon_{ij}^{a,d} , \tag{4}$$

where the superscript d indexes distance to school for distances up to 20 miles, 60 miles, and 120 miles. Table 5 reports results from estimating equation (4) for 19-28 year olds.

From Table 5 we see that just like with preparatory schools those who live closest to a Normal College are seeing the largest effect and the increase in enrollment diminishes with distance. Also while we see a strong effect for males, there is no significant effect for females.

Table 6: Educational Aspirations Past high school β_{rd}

	Combined	Male	Female
All	0.341*	0.179*	0.477*
	(0.178)	(0.107)	(0.276)
14 to 18	0.567**	0.929*	0.470***
	(0.260)	(0.519)	(0.0429)
19 to 28	0.128**	0.114*	0.222***
	(0.0643)	(0.0679)	(1.30e-16)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

4.2. Educational Aspirations

We find that Oportunidades improves enrollment in education. There are a few potential mechanisms that may underlie this program effect. Improved educational aspirations offer one possible mechanism that can account for some of the pathways leading to higher educational outcomes.

Poor households tend to have lower educational expectations for their children who themselves also tend to have lower personal education goals [29, 32, 31]. Recent studies suggest three important sources for low aspirations. First, low-income households may suffer from psychological distress due to liquidity constraints [33, 21]. Second, low aspirations can also be caused by limited or no exposure to positive role models in disadvantaged environments [14, 17, 23]. Third, they may lack information concerning returns to education, caused by low schooling [19, 14]. A CCT program, therefore, may result in higher aspirations through relaxing a household's budget constraint, delivering information about returns to education, and increasing exposure to positive role models through interactions with professionals and local leaders.

In 2007, the survey asked the head of household about the level of education they believed each household member would achieve. Using this information, we construct a variable equal to one if the household head believed individual i would achieve some level of post-secondary schooling. We use this variable as a proxy for the household's post-secondary educational aspirations for individual i, and use it as the dependent variable in equation (1). Table 6 reports the estimated effects of Oportunidades on educa-

Table 7: Educational Aspirations Mechanisms

Panel A: Expenditures on Schooling	Combined	Male	Female
14 to 18	-1544.3	268.2	-2601.0
	(1494.2)	(354.8)	(2325.4)
19 to 28	1012.3**	1053.3***	549.3
	(508.8)	(51.72)	(1027.1)
Panel B: Attends Community Meetings	Combined	Male	Female
14 to 18	0.134*	0.00656	0.142
	(0.0809)	(0.140)	(0.145)
19 to 28	0.0554	-0.0732	0.312*
	(0.0882)	(0.142)	(0.187)
Panel C: Meetings with Teacher	Combined	Male	Female
14 to 18	-0.121	0.144	-0.202
	(0.149)	(0.199)	(0.169)
19 to 28	-0.00209	0.0142	-0.145
	(0.285)	(0.157)	(0.153)
28+	-0.0544	0.0199	-0.110
	(0.0606)	(0.0555)	(0.104)
Panel D: How Often Meets with Teacher	Combined	Male	Female
14 to 18	1.044***	1.314**	0.406*
	(0.221)	(0.525)	(0.240)
19 to 28	-0.0670	-5.35e-16***	-0.0268
	(0.381)	(3.17e-17)	(0.426)
28+	0.272*	0.184**	0.326*
	(0.157)	(0.0828)	(0.192)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

tional aspirations for adults and children in the household.[7] Oportunidades had large and statistically significant effects on the household's educational aspirations for its members. Household heads just below the eligibility cut-off were 57% more likely than those just above the cut-off to report post-secondary educational aspirations for their 14 to 18-year-old household members. They were also 11.4% and 22% more likely to report post-secondary aspirations for their 19 to 28-year-old male and female household members, respectively. Exploring some of the aforementioned mechanisms can reveal some of the likely pathways for this increase in aspirations.

One possible channel through which Oportunidades increases educational aspirations is by alleviating budget constraints through the income transfer, which in turn, may in-

[7] Due to the limited number of responses, we could not estimate the aspiration effects for individuals aged 28+ years.

crease a poor household's belief that higher educational achievement is attainable. To explore this possibility, we examine the effect of program eligibility on schooling expenditures by estimating equation (1) with age-cohort-specific education expenditures as the dependent variable. These results are reported in Panel A of Table 7. Here we see that Oportunidades exhibits no statistically significant effect on schooling expenditures for 14 to 18-year-olds. Since this age group qualifies for direct education benefits from the program, this finding suggests that instead of increasing overall expenditure on this group's education, the program's education subsidy displaces the resources the household would have otherwise spent on education, freeing them up for other uses. Conversely, living in an Oportunidades-eligible household significantly increases spending on education for males 19-28. Again, individuals in this age cohort are too old to qualify for the program's subsidies directly. This result, therefore, suggests that the program's income transfer and education subsidies relax the household's budget constraint such that education expenditures increase for non-beneficiary household members. This result complements our previous findings on improved tertiary attainment, and aspirations.

Chiapa et al. [12] find that Oportunidades improves parental aspirations due to increased interaction with medical professionals and improved understanding on returns to education. Similarly, we explore the role Oportunidades may have had in improving aspirations by increasing exposure to positive role models, as well as the perception of the returns to education. We do so by estimating equation (1) using participation in community meetings and meeting with teachers as dependent variables. Panel B of Table 7 reports the effect of Oportunidades on attendance at community meetings. Membership in an eligible household appears to increase the likelihood of female participation in community meetings by approximately 31% for 19 to 28-year-olds and 13.4% for the 14 to 18 group. The estimated effect for 14-28 females is not statistically significant, but the significant pooled effect appears to be driven by females in the sample.

Panels C and D of Table 7 report the effect of Oportunidades on the probability of meeting with a teacher and the number of meetings conditional on having at least one meeting. According to Panel C, living in an Oportunidades-eligible household has no statistically significant effect on the likelihood that an individual meets with his or her

teacher. However, as reported in Panel D, conditional on meeting with the teacher, individuals in households just below the eligibility cutoff have more meetings for their 14 to 18-year-olds than those just above the edibility cutoff. Males 14 to 18 have, on average, one more meeting with their teacher and females 14 to 18 have almost half a meeting more. The program had no statistically significant effect on teacher meetings for 19 to 28-year-olds, which is intuitive since individuals in this age cohort were adults for the entire sample period. Increased interaction with teachers and community leaders likely increases households' interaction with positive role models and influences their perception of the returns to higher education. Our findings are at least suggestive that these two channels also offer an explanation for the aspirations effect of Oportunidades, which in turn may translate into part of the program's positive effect on higher education outcomes.

5. Conclusion

This paper investigates the long-term effects of participation in Mexico's conditional cash transfer program Oportunidades on school enrollment rates. Our findings suggest that households that were eligible for the program in 1997 had higher enrollment rates for technical school, preparatory, and college in 2007.

We find that participation in Oportunidades also increases educational aspirations in the household, which suggests one channel for the positive higher education effects we observe. Previous studies find similar effects on aspirations from CCT programs [12, 18]. This effect is believed to be partly due to the required education information workshops and increased interaction with community leaders ([12, 33, 26]. In these workshops, beneficiaries interact with community leaders and learn the importance of health and education. Previous work demonstrates that reducing misconceptions about education is an important mechanism for promoting educational outcomes [10, 23, 28]. Additionally, overestimating the costs and underestimating the benefits of higher education reduces aspirations in both parents and children [20].

Unlike previous work on the relationship between CCTs and aspirations, we also examine the effect that Oportunidades has on education aspirations for non-beneficiary

adults in treated households. We find that expenditures on schooling increase for 18 to 28-year-olds, suggesting that this program not only increases the household's education aspirations for its children, but also for their parents and other adult household members. This finding highlights an important positive externality of CCTs.

Finally, in the rural areas that our study covers, there is a significant difference in school access. Most communities in our sample had at least one technical school, while only eleven communities had preparatory schools. Oportunidades offers paid grants for upper secondary enrollment, yet if there is no school nearby to enroll in, the ability of individuals to take advantage of this program is hampered. When we control for school access, we find clear program benefits for those with sufficient access to school for both preparatory and Normal schools.

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Table A.8: Covariates by Age Group β_{rd}

	14 to 18			19 to 28			28+		
	Combined	Male	Female	Combined	Male	Female	Combined	Male	Female
Household Size	-0.00607	0.212	-0.0789	-0.353	-0.349	-0.211	-0.290	-0.265	-0.303
	(0.306)	(0.532)	(0.731)	(0.265)	(0.277)	(0.393)	(0.219)	(0.206)	(0.244)
Rooms	-0.234	0.0614	-0.0897	-0.0684	-0.440**	0.0426	-0.0679	-0.105	-0.110
	(0.237)	(0.578)	(0.551)	(0.252)	(0.193)	(0.321)	(0.166)	(0.149)	(0.175)
Water in home	0.0219	-0.0483	-0.0849	-0.0247	-0.0252	0.0230	-0.00691	-0.0122	0.00137
	(0.0635)	(0.116)	(0.213)	(0.0491)	(0.0528)	(0.0599)	(0.0344)	(0.0279)	(0.0400)
Electricity	0.00521	0.0229	0.00295	-0.0243	-0.0173	-0.0289	-0.0137	0.000546	-0.0206
	(0.00809)	(0.0201)	(0.00379)	(0.0182)	(0.0144)	(0.0301)	(0.0130)	(0.0113)	(0.0156)
Owns land	-0.0646	0.0116	-0.201	-0.0154	-0.0646	0.0260	-0.0325	-0.0303	-0.0461
	(0.0841)	(0.155)	(0.181)	(0.0838)	(0.0723)	(0.117)	(0.0450)	(0.0421)	(0.0465)
Age	-0.0763	-0.249	-0.0664	-0.0511	-0.714**	0.351	-1.375	-0.560	-2.800**
	(0.239)	(0.514)	(0.565)	(0.292)	(0.311)	(0.540)	(1.041)	(1.074)	(1.313)
Sex	-0.163**	0.0796	0.00476	0.111*	0.0211	0.0143	-0.00138	-0.00694	0.00521
	(0.0779)	(0.0745)	(0.0998)	(0.0609)	(0.0211)	(0.0442)	(0.0181)	(0.0114)	(0.0302)
Father at home	0.00532	0.0101	0.0349	-0.0232	-0.0131	-0.0522	-0.00759	-0.0103	-0.00127
	(0.0123)	(0.00862)	(0.0337)	(0.0215)	(0.0207)	(0.0347)	(0.0106)	(0.0133)	(0.00980)
Mother at home	-0.0198	-0.127	-0.000398	0.0615	0.0413	0.126	-0.0174	-0.0931	0.0667
	(0.0510)	(0.105)	(0.140)	(0.0507)	(0.0427)	(0.0910)	(0.0758)	(0.0909)	(0.126)
Literacy	0.0320	0.0672	0.0610	0.00524	-0.0144	0.0156	0.0393*	0.0576***	0.0161
	(0.101)	(0.223)	(0.264)	(0.0176)	(0.0156)	(0.0349)	(0.0203)	(0.0219)	(0.0298)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Appendix A. Validity Tests

Determination of pre-program randomization is performed by examining individuals around the cut-off and seeing if they are similar. We do this by estimating equation 2 using covariates as the depending variable to determine if these characteristics also exhibit a discontinuity around the eligibility threshold.

Table A.9: Placebo test β_{rd}

	Combined	Male	Female
High School 14 to 18	-0.0337	0.000389	-0.0611
	(0.0343)	(0.0461)	(0.0431)
Preparatory 14 to 18	-0.0292	-0.00171	-0.0672
	(0.0335)	(0.0473)	(0.0534)
Technical 14 to 18	0.00124	-0.00390	-0.00231
	(0.00762)	(0.00457)	(0.0142)
Tertiary 19 to 28	-0.00486	-0.00368	-0.00544
	(0.00544)	(0.00767)	(0.00809)
Tertiary 28+	0.000619	0.000976	0.00152
	(0.00148)	(0.00235)	(0.00163)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The covariates used are those from the general education model used in other research [8, 9]. The covariates from the general education model are age, sex, father at home, mother at home, rooms, electricity, water, owns land, and literacy. Failure to reject the null hypothesis of no discontinuity confirms that the conditional expectation of the covariates is continuous across the marginality index. The covariates for enrollment show that there are none that would cause the increased enrollment rates found. If anything, those above the threshold have more favorable education conditions; providing evidence in favor of randomization at the eligibility threshold. Thus is does not appear that a discontinuity in the covariates is not driving the discontinuity observed in our outcome variables.

The next test estimates the program effects with the covariates included. The estimates of the program impact will be unaffected by the inclusion of any covariates if the local randomization assumption is meet. With the inclusion of the covariates, there is little change in the coefficients of the estimates for males or females. There is a slight change in significance as the standard errors become smaller because the addition of covariates improves the precision of the estimates.

For our last diagnostic test, we run a placebo test in which we take the median of the normalized marginality index on one side of the threshold score and run another RD estimation using that as our threshold. Doing this shifts the threshold score from zero to -2.099. With this as the new threshold score, we rerun all the regressions.

The results are insignificant, suggesting that only the real threshold is where we see an effect. The threshold score is then moved to the other side and three other random points with similar results. Figure A.1 is with the original threshold and shows the discontinuity that we find, yet when we move this threshold, we get Figure A.2 and the discontinuity vanishes. The lack of significant RD estimates at false thresholds strengthens the argument that the discontinuity in enrollment rates is due to the discontinuity in participation in Oportunidades. The assumption of local randomization around the eligibility cutoff has been strengthened by the evidence in this section, indicating that the positive impact effect on school enrollment is internally valid.

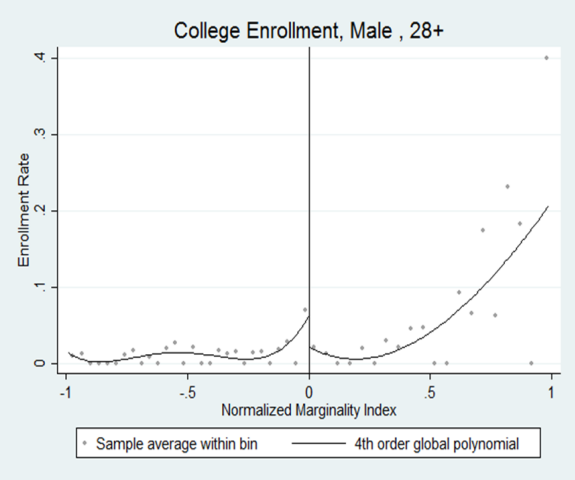

Figure A.1

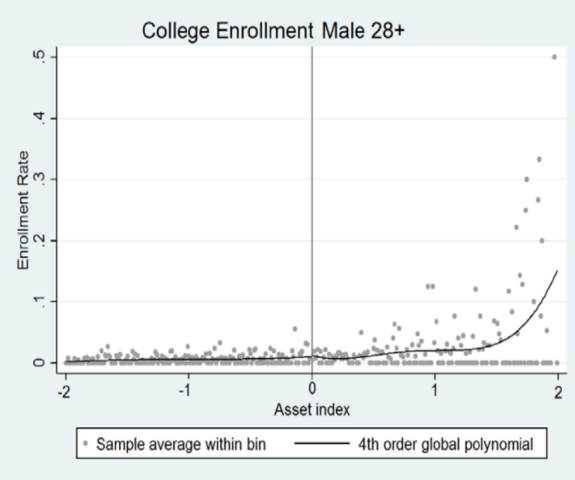

Figure A.2