



Should program graduation be better targeted? The other schooling outcomes of Mexico's Oportunidades[☆]

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ABSTRACT

A large literature on Conditional Cash Transfers programs assesses the effects of becoming a beneficiary. However, the consequences of losing the benefit due to program graduation are largely unstudied. This paper replicates the eligibility score employed over 2010–15 by Mexico's Oportunidades for a large household survey. Using a Regression Discontinuity Design around the threshold for program graduation, it shows that losing this additional incentive had a negative effect on high school attendance for lower secondary school aged students in urban, and upper secondary school aged ones in rural areas. The results suggest that the graduation thresholds are chosen too low.

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1. Introduction

Since the inception of Mexico's Progresa in 1997, cash transfer programs, either conditional (CCT) or unconditional, have been one of the primary areas of research in the development literature. The focus has been almost exclusively on the various effects of program implementation on a variety of outcomes on the individual or household level, or some aggregate unit (e.g. the village). While this is undoubtedly the single most important treatment, the literature thus far has ignored the effects of program graduation. Most cash-transfer programs establish a mechanism by which beneficiaries lose the benefit if their socioeconomic characteristics improve above a certain level. In this, governments have to strike a delicate balance: On the one hand, they need to control expenditures by graduating beneficiaries who are no longer deemed in need of the program, but, on the other, they have to make sure that the loss of benefit does not eradicate the gains made.

This paper is to my knowledge the first study that looks at the effects of program graduation on one of the target outcomes of

conditional cash transfer programs, namely school attendance. It does so for the well-studied Mexican flagship program Oportunidades. The findings show that program graduation significantly reduces school attendance at the lower secondary school level (grades 7–9) for urban students and at the upper secondary level (grades 10–12) for rural ones. These results suggest that the effect on human capital formation should be better targeted when designing program graduation. Given the popularity of CCTs in middle-income countries, they also call for more exercises of this kind for other countries.

The paper employs a regression discontinuity design (RD) around the known threshold for program graduation, based on individual and household level data collected by Mexico's 2015 Intercensal Survey. This is combined with information on the recertification process during 2011/12. Since the data do not contain information on a household's beneficiary status, the results presented have to be interpreted as an intention to treat effect (ITE), rather than an average treatment effect (ATE). By this measure, lower secondary school attendance is reduced by around five percentage points for urban students, and over 14 percentage points for rural students at the upper secondary level. However, when combined with other information on program coverage, the estimated average treatment effects increase to 26% and 16%, respectively. Given the general characteristics of the program's implementation in the late 1990s and early 2000s, the vast major-

[☆] The findings, interpretations, and conclusions expressed in this work are exclusively the author's responsibility, and do not form part of the official statistics or positions of Mexico's National Institute of Statistics and Geography (INEGI), nor of the National System of Statistical and Geographical Information (SNIEG).

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ity of households in the sample can be expected to have been beneficiaries since the first half of the 2000s or even since the 1990s. The results, therefore, strongly suggest that the conditional part of the cash-transfer program continues to play an important role even for households who have benefitted from it for a long period of time. In this context, the study also makes an important contribution to the emerging literature on the longer term effects of cash-transfer programs.

In order to keep the language simple, I will refer to the program simply as Oportunidades. As the reader may be well aware, it was initially instituted under the name Progresa, which it held until after the change in Mexico's federal government in late 2000. In 2014, over a year after the old governing party PRI returned to power, the name was changed yet again to Prospera. I will refer to it as Oportunidades, since the treatment of interest (benefit loss due to recertification) occurred during 2011 and 2012 when the program was still operating under that name.

In the next section, I will provide a description of the program and the recertification process. This is followed by a review of the literature relevant to this study. Section four provides the theoretical background necessary to derive meaningful hypotheses. Section five discussed the RD methodology employed, and section six explains the data used and how the methodology is implemented in the present setting. I then present the results, including robustness checks. I conclude this study with a discussion of how to interpret the findings.

2. Background

Mexico's Conditional Cash Transfer program Oportunidades began operating in 1997, under its first name Progresa, in a few rural localities. In 1998, 506 additional localities were selected, of which 320 were randomly assigned to treatment, while the remaining 186 localities entered the program 18 months later. It was this random protocol that made it the probably most researched and well-documented government program outside high-income countries. Levy (2006) gives an in-depths account of the program's inception and an exhaustive review of its earlier literature. In a nutshell, Oportunidades provides cash payments to families conditional on children's school attendance and that all family members visit health clinics on a regular basis. First solely focused on rural areas and children of mandatory school age, in 2001 the program started expanding into urban areas and to provide additional subsidies for children attending school beyond ninth grade. While the program's name changed with the two changes of party in government, it maintained its principal characteristics with relatively minor changes.¹ In order to keep the discussion to a manageable length, at continuation I will only go over the program rules directly relevant to this study.

Eligibility to Oportunidades is determined by a proxy means test. It computes an index that estimates per-capita household income based on a number of easily observable characteristics. These are captured by a questionnaire called *ENCASEH* (Encuesta de Características Socioeconómicas y Demográficas de los Hogares) that is administered during visits to the household. If the estimated income is below a pre-defined minimum welfare line (LBM, by its Spanish acronym) the household qualifies. The same index is used to determine if households currently participating in the program should lose the benefit due to improvements in their socio-economic status. For the latter purpose, called recertification, households' eligibility was supposed to be reassessed, at first,

every three years. If the household continued to be below the LBM, the program was continued. If it was above the LBM, but still below a second, higher, line, the household was put into a regime with reduced benefits. Importantly, the stipends for post-primary education were not affected by this reduction. If the household moved above the second line, benefits were terminated.

The basic setup of this process has not changed since. However, the three yearly recertification proved to be excessively burdensome since it required that each year one-third of beneficiaries, spread out over the entire country, be interviewed. In 2010, in addition to updating the underlying model used to determine eligibility and permanence to the one used in this study,² the recertification process was changed to a five yearly rotation. Now, each year, households comprising around one-fifth of the beneficiary population are interviewed in a determined number of localities. The order of localities visited was determined by their score on the Social Gap Index (*Índice de Rezago Social*), calculated by Mexico's National Council for the Evaluation of Social Policies (*CONEVAL*). This index gives each locality in the country a social gap score (very low, low, medium, high, very high) according to its performance on a multi-dimensional poverty measure. Households in localities with the lowest gap were put through recertification first.

This process, however, proved to be highly controversial as during the first two years, according to official data, 26.11% of households that went through it were graduated from the program. It was stopped in late 2012 when a new federal administration entered office. In my conversations with government officials working for the program, the graduation rate of 26% was mostly attributed to the comparatively high levels of development in these localities. The model used to determine eligibility was again changed in 2015 (based on the 2014 round of the ENIGH), and recertification was reinstated at the locality level, but without the grouping by the Social Gap Index. During the second half of the Peña Nieto administration (2012–2018), according to the same officials, around 16% of households were dropped from the program each year. The upshot for the present paper is that the results presented apply for localities with relatively high socioeconomic development: 84% of observations in the sample live in municipalities³ with very low or low gaps.

The model used to estimate household per-capita income during 2010–15 consists of a linear regression with the logarithm of per-capita household income (excluding government transfers) as the dependent variable. Different models, based slightly different characteristics, were estimated for rural and urban areas. Following the standard definition used in Mexico, rural refers to localities with less than 2500 inhabitants, and in this paper urban to those with more. The full log-linear model with all included variables and their coefficient values is shown in Table 1. This model will be discussed in more detail below.

3. Existing literature

Oportunidades is likely the most researched public policy outside the rich world. Even the original randomized setup continues to spawn new studies after more than 15 years. The earlier studies focused on outcomes directly targeted by the program, such as household consumption, or children's health status and school attendance. The more recent literature looks at a wide variety of other outcomes, such as, for example, deforestation (Alix-Garcia, McIntosh, Sims, & Welch, 2013), health status of elderly household members (Behrman & Parker, 2013), investment on productive

¹ However, the program's future is uncertain. The administration of Andrés Manuel López Obrador has announced its discontinuation, but at the time of writing these lines transfers are still being paid.

² The model used during late 2010–15 was based on information collected in the 2010 *Encuesta Nacional de Ingresos y Gastos de los Hogares* (ENIGH).

³ The locality level index is not observed.

Table 1

Coefficient values of logarithmic income per-capita model used to assess eligibility and graduation.

	Urban	Rural
Intercept	8.245	7.389
Household head and spouse:	0.066	0.137
Have completed primary, but not secondary school		
Household head and spouse:	0.257	0.313
Have completed secondary school or higher		
Household dependency ratio	−0.034	−0.06
Number of women in household aged 15–49	−0.027	
Logarithm of the total number of HH members	−0.737	−0.624
Number of HH members who are employees	0.24	0.374
Number of HH members who work independently	0.172	0.101
Number of HH members who work in a employment-like, but unpaid capacity	0.06	
Number of HH members with Seguro Popular coverage	−0.009	
At least one HH member has employer provided health insurance	0.224	0.388
HH works independently & at least one HH member has employer provided health insurance	0.055	0.219
Most of the dwelling has solid floors		0.096
Most of the dwelling has covered floors	0.135	0.302
Dwelling is owner occupied	0.035	
Dwelling is rented	0.183	0.186
Total number of rooms	0.051	0.024
Wood or coal are used as cooking fuel	−0.112	−0.271
Dwelling has exclusive water toilet	0.015	0.074
Does not have refrigerator	−0.023	−0.121
Does not have telephone landline	−0.072	
Does not have motor vehicle	−0.23	−0.197
Does not have VHS/DVD/Blue Ray player	−0.128	−0.111
Does not have computer	−0.288	
Does not have electric or microwave oven	−0.115	−0.114
Some adult HH member did not have breakfast, lunch or supper	−0.1	
due to lack of resources (Food Security 1)		
Some adult HH member had only one meal a day or none at all	−0.058	
due to lack of resources (Food Security 2)		
Food Security 1 or Food Security 2		−0.096
Receives international remittances	0.078	0.279
Municipal social gap index	−0.047	−0.071
Locality with 100,000 inhabitants or more	0.058	
Locality with 15,000–100,000 inhabitants	0.054	

Notes: Information provided by SEDESOL. The predicted outcome of the linear model is $\log(\text{income per-capita})$. This proxy means test was used during 2011–15.

assets (Gertler, Martinez, & Rubio-Codinsa, 2012), mothers' human capital investment (Dubois & Rubio-Codina, 2012), or the program's interaction with adverse income shocks (Adhvaryu, Nyshadham, Molina, & Tamayo, 2018).

Starting with Behrman, Parker, and Todd (2011), which considers a time frame of roughly five and a half year, the literature started to look at the program's longer-term impacts.⁴ Some of these studies still base their identification on the randomized roll-out and compare the effects on differentially exposed age-groups. The paper just mentioned is of interest given its focus on schooling and work. The authors first compare outcomes in 2003 in response to the 18 month lag in exposure due to the randomized roll-out. The second exercise consists of comparing each of these two groups to a control group that had never been exposed, using difference in differences matching estimators. In both cases, the authors find significant improvements in school performance for the group with longer exposure to the program. Though somewhat weaker, they also find a reduction in work for boys. A similar exercise by the same authors (Behrman, Parker, & Todd, 2009) for younger children who were not of school age at the start of the program, but should have

benefitted from the nutritional and health components, shows that it significantly reduced the age of primary school entry. For a somewhat longer term, Rodríguez-Oreggia and Freije (2016) look at similar outcomes in 2007 for beneficiaries who were 5–15 years old in 1998. They do not find results on employment, wages or inter-generational mobility (though their sample suffers from high attrition). For a much longer time horizon, Kugler and Rojas (2018) find great educational attainment after 17 years of exposure. They also find higher subsequent rates of employment. Not using the experimental data, Parker, Rubalcava, and Teruel (2012) employ the 2002, 2005, and 2009 rounds of the Mexican Family Life Survey (MxFLS) for the sample of individuals who were 10–14 years old in 1997. Results are presented for difference in differences estimates with a variety of different matching methods, showing a consistently big (around 5 percentage points) and significant increase of the probability of attending college. The labor market results are more mixed, with some evidence of a higher probability of working, but no results regarding hours worked, or wages and benefits received. Looking further into the future, McKee and Todd (2011) use the existing evidence of the program's human capital effects, and data on current 25–40 year olds, to simulate its long-term effects on earnings. They conclude that while its does increase mean earnings, it will only have limited effects on earnings inequality.

Moving beyond Mexico, the longer-term effects of other CCT programs in Latin America have also been assessed for a number of other countries. One interesting study (Baird, McIntosh, & Özler, 2016) looks at a program in Malawi that was only in place for two years. It finds that the positive effects quickly dissipated after it had been discontinued. However, results differ for Nicaragua's *Red de Protección Social*. Here, localities were randomly assigned to either receiving the benefit over a three year period starting in either 2000 or 2003. Barham, Macours, and Maluccio (2016) find that early exposure increased total schooling by half a grade, and also very significantly increased performance in standardized test scores. Another Central American country, Honduras, also had a time-limited CCT in place between 2000 and 2005 (*Programa de asignación Familiar*). Treatment was randomized across 70 poor municipalities over three different supply and demand side interventions. Molina-Millán, Macours, Maluccio, and Tejerina (2018) use the basic CCT intervention to show that exposure led to significantly improved educational outcomes, except for indigenous children, and to a higher rate of international migration. Ham and Michelson (2018) look at the different treatments to find that the traditional CCT component was only effective when combined with investments in schools and health facilities. For the Colombian program, *Familias en Acción*, Baez and Camacho (2011) conduct a difference in differences estimation based on matching, followed by, and somewhat mirroring the strategy followed here, a regression discontinuity design based on the eligibility score (*Sisben*). The results from both methods show a significant positive impact on school completion, but no effect on test scores. A similar approach is taken by Araujo, Bosch, and Schady (2016), who show that Ecuador's *Bono Desarrollo Humano* program, after ten years, had no effect on test scores, but increased secondary school completion for females by around two percentage points. Using a RD design, Bosch and Schady (2019) are only able to find a small negative effect for adult women on work in the formal sector, but not on beneficiaries' labor force participation overall. Glewwe and Kassouf (2012) show, using data from Brazil's School Census over the 1998–2005 period, that Brazil's *Bolsa Escola/Familia* increased enrollment and grade promotion, while lowering dropout rates.

In this paper, I compare outcomes for rural and urban areas. Given that the original randomization of Progreso only applied to rural localities, there is comparatively little research on its effect in urban areas. Behrman, Gallardo-García, Parker, Todd, and Vélez-Grajales (2012) show that the program's expansion to

⁴ A thorough review on the longer-term impacts of CCTs in Latin America can be found in Molina-Millán, Barham, Macours, Maluccio, and Stampini (2016).

localities with more than 2,500 inhabitants had similar effects to those found in rural settings. Specifically, it increased school enrollment and attainment, increased time devoted to homework for girls and working rates of boys. There is also a surprising dearth on studies on CCTs performance in targeting poor households. In an early study, before the urban expansion, Skoufias, Davis, and de-la Vega (2001) find that Progresa successfully targeted the poorest households and localities. For the urban expansion, Coady and Parker (2009) assesses targeting as a function of household self-selection and administrative selection. It concludes that most of the targeting can be attributed to the former.

Looking at voluntary dropout, Álvarez, Devoto, and Winters (2008) argue that the program's conditionality imposes higher compliance costs on relatively better-off households, increasing their likelihood to drop out. This increased overall program targeting on the poor. A similar argument is made by Heinrich and Brill (2015), who look at changes in the application requirements (and eligibility age) to South Africa's Child Support Grant. They find that the resulting interruptions in program receipt lowered female educational attainment and increased risky behavior by adolescents. However, these studies differ from the present one, in that they look primarily at households' voluntary self-selection out of the program in question. The discussion and results presented at continuation look at the effect of becoming ineligible to the benefit due to changes in household characteristics. The literature on this topic is all but missing. The only somewhat related study is Villa and Nino-Zarazúa (2014), who use the Mexican Family Life Survey (MxFLS), a longitudinal dataset with a first round collected in 2002 and follow-up rounds in 2005/06 and 2011/12, to estimate graduation probabilities.

4. Theory and hypotheses

The conditional nature of a CCT benefit is motivated by a possible disparity between the level of human capital investment chosen by parents, and the level that would be in the child's own best interest. This disparity may either be the result of a misalignment between parents' and children's preferences, or of a lack of information about the long-term benefits of such investments. In the former case, parents are willing to compromise their child's future earnings (and hence future consumption) for a higher level of present-day consumption by forcing him/her to seek gainful employment or to work in a family business or on family owned land. In the latter case, parents intrinsically care about their child's future, but do not appreciate the long-term benefits of educational and health investments.

Fig. 1 illustrates this problem for the trade-off between investment in a child's education and household (present-day) consumption. The latter represents the *additional* consumption derived from the child's labor market participation. The equilibrium outcome is determined by the wage rate available to children relative to other household income and the size of the transfer payment. The situation is shown for three different scenarios: The first one, (a) in the top-left panel, corresponds to a relatively low wage being available to the child; while the second one, (b) in the top-right panel, corresponds to a medium child wage; and the last one, (c) at the bottom, to a high wage. The solid lines show the budget constraint for potential program beneficiaries. In order to qualify for the CCT payment, and hence to increase present-day consumption, households have to provide the child with a minimal level of educational investment. This usually corresponds to school attendance—which is observable to the government. However, as shown in the graph, this may still fall short of the child-optimal level of education; for example, if parents require the child to work after school instead of doing homework or dedicating time to at-home study. When the

benefit is lost, the upper part of the budget constraint returns to the dash-dotted line. The dotted indifference curves correspond to child-optimal preferences, while the dashed curves show parent-optimal preferences (be it because they do not fully internalize their child's utility, or because of misinformation).

Panel (b) shows the advantage of CCTs: When the opportunity cost of educational investment is high, which is the case when the child could command a relatively high wage, educational investment in the absence of the program would be low. A large enough transfer would entice parents to keep their child in school (of course, point 2 could also be slightly above the kink in the budget constraint). However, as shown in panel (c), if the wage is very high compared to the transfer amount, this incentive may not be strong enough to change parents' behavior. This mirrors the argument made in Angelucci and Attanasio (2009), that the officially reported participation rate among eligible urban households of only 50% may partially be the result of the program's lower attractiveness in such areas. While in panel (c) parents opt to forego the benefit, panel (a) shows the case when conditionality is superfluous. Here, the wage that the child could receive is so low that parents opt for a level of educational investment that exceeds the minimal level required by the CCT even before its implementation. In this case, an unconditional program would have exactly the same effect.

The role of parental income (or any other sources of household income) are not explicitly modeled in Fig. 1. But it is drawn for a relatively poor household. If the household is richer, and consumption is high in the absence of the child's additional income, parental indifference curves will be almost horizontal (that is, the marginal contribution of an additional unit of consumption to parents' utility is so low that it would only offset a vanishingly small amount of education lost). In that case, the equilibrium point will be at the corner of the budget constraint with the y-axis, with maximal education and zero additional consumption. For households slightly below that level of income, the situation is similar to the one depicted in panel (a): Parents opt for a level of educational investment above the minimum required by the CCT.

The main hypothesis this paper tests is whether the representative household at the graduation threshold finds itself in the situation depicted in panel (b). Since the program defines the minimal educational investment as school attendance, the movement from point 2 to point 3 can be interpreted as dropping out of school. Finding a statistically significant negative causal effect of program graduation on school attendance would confirm this hypothesis. Not finding such an effect means that the representative household is either in the situation depicted in (a), or the one in (c).

5. Methodology

First proposed by Thistlewaite and Campbell (1960), regression discontinuity designs have gained prominence in economics starting in the 1990s. Over the years, a large variety of different RD estimators have been proposed. The approach employed here consists of using nonparametric regressions to determine the expected values of the outcome of interest at the boundary points on either side of the threshold, conditional on the assignment variable. This method has gained prominence since Hahn, Todd, and Van-der Klaauw (2001), given the problems with standard nonparametric kernel regressions in this context, proposed local regressions⁵ to estimate the boundary points. The treatment effect is then calculated as the difference between the two. While Hahn et al. (2001) showed that the local regression is less biased than other nonparametric

⁵ Local regressions were first proposed by Fan (1992).

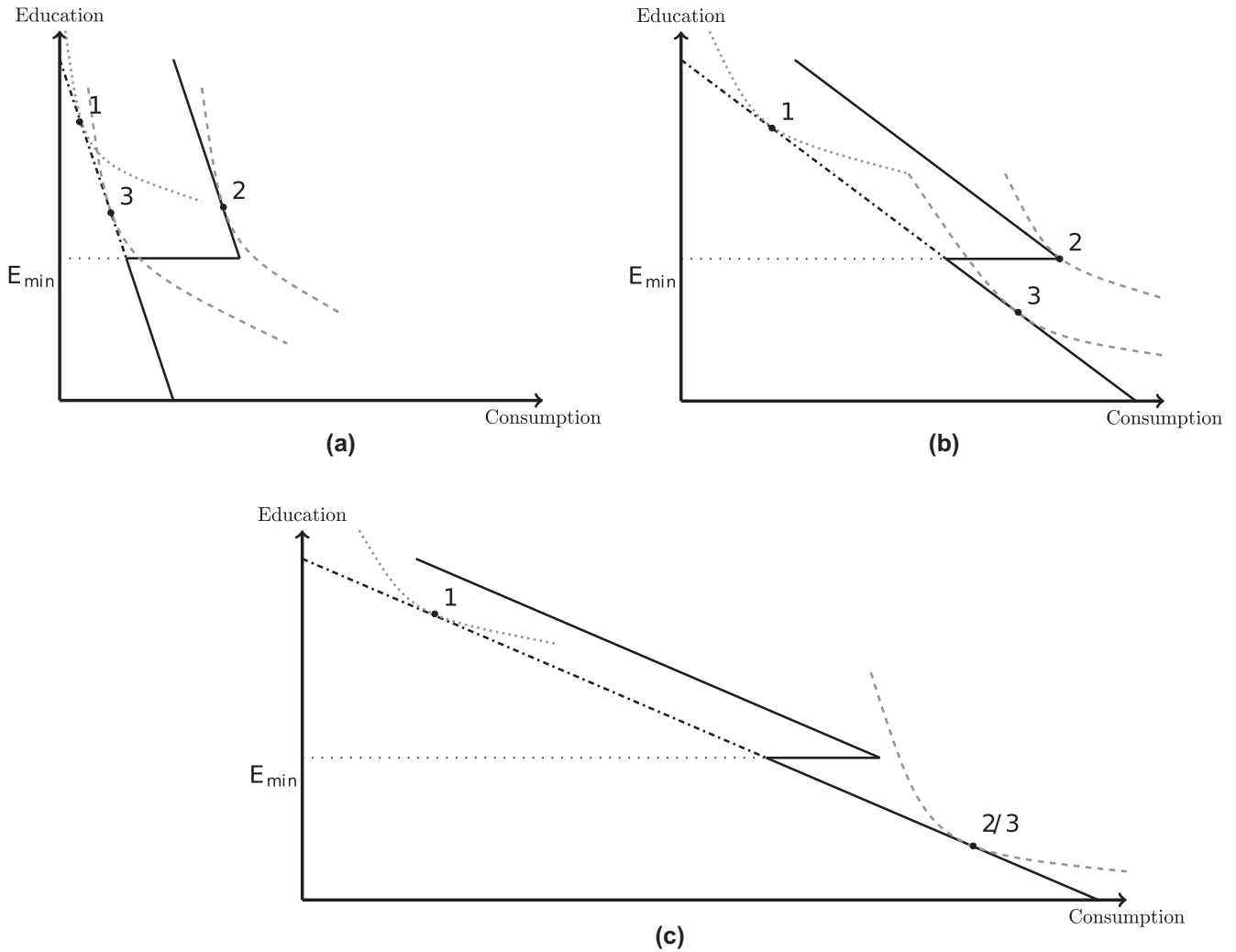


Fig. 1. Effect of loss of benefit on educational attainment for different child wage rates. This graph shows the effects of a loss of benefit on educational attainment and household consumption for different wage rates for minors: (a) a low wage; (b) a medium wage, and (c) a high wage. The solid lines show the budget constraint with the Oportunidades benefit, which is paid if the child's educational attainment is above the minimum E_{min} . The dash-dotted lines show the change in the budget constraint after the benefit is removed. The dotted indifference curves correspond to child-optimal preferences, while the dashed ones correspond to parent-optimal preferences. Point 1 would be the child-optimal allocation in the absence of the benefit; point 2 and 3 correspond to parent-optimal allocations with and without the benefit, respectively.

methods, it only yields consistent estimates under fairly strong assumptions. Of crucial importance here is the bandwidth employed in the estimator: While a larger bandwidth will result in more precise estimates (since it is based on more observations), it also increases the potential bias. This tradeoff between bias and precision is central to all nonparametric estimators. This study employs a nonparametric local regression and, following the recommendations in [Lee and Lemieux \(2010\)](#), shows results for different neighborhoods and polynomials. It will also show the distribution of the assignment variable, give a graphical impression of the discontinuity, and assess the continuity of potential confounding variables at the threshold.

To fix ideas, following [Calonico, Cattaneo, and Titiuk \(2014b\)](#) the sharp RD estimator can be expressed as:

$$b_{RD}^S = \alpha_y^+ - \alpha_y^-, \quad (1)$$

where the y and subscript denotes the outcome of interest, and the superscripts refer to the boundary values at the threshold from the left (-) and right(+). The value of α_y^+ is obtained as the estimator \hat{a}_y of the local (linear) regressions:

$$(\hat{a}_y, \hat{c}_y) = \underset{a_y, c_y}{\operatorname{argmin}} \sum_{i=1}^N \{y_i - a_y - c_y(x_i - \tau)\}^2 K\left(\frac{x_i - \tau}{h}\right) I[x_i \geq \tau]$$

Here, τ denotes the threshold, and $K(\cdot)$ is a kernel function of a type to be chosen by the researcher. N are all observations in the data or in neighborhood around the threshold, and h is the bandwidth (which, of course, allows for some observations to be completely excluded). α_y^- is obtained similarly to the left of the threshold, i.e. changing the identity function to $I[x_i < \tau]$.

In addition, results will be presented for bias-corrected estimates and with p-values based on robust standard errors, both also proposed in [Calonico et al. \(2014b\)](#). This method, instead of subtracting the estimated bias term from the confidence intervals, derives the asymptotic variance for the bias-corrected point estimate. This variance term takes into account the additional variation introduced by the correction, allowing for robust standard errors and thus confidence intervals with improved finite sample properties.⁶

⁶ See also [Calonico, Cattaneo, and Titiuk \(2014a\)](#) for a detailed overview of the different methods available and Monte Carlo results of their performance.

6. Data and implementation

The principal data source is Mexico's 2015 Intercensal Survey (INEGI, 2015), *Encuesta Intercensal*, collected between March 2–27, 2015. Since the 19th century, Mexico conducted a full census in all years ending in zero, with the occasional deviation from this pattern due to internal or external strife. In 1995 and 2005, the country also carried out an additional census (called *Conteo*) with a slightly shorter questionnaire. This practice was again changed in 2015, when instead of a full census the National Institute of Statistics and Geography (INEGI, by its Spanish acronym) decided to conduct a large household survey with a detailed questionnaire.⁷ The *Encuesta Intercensal* has a sample size of 6.1 million households, designed to be representative of all localities with more than 50,000 inhabitants.

The single most important reason to use the *Encuesta* is its large sample size. Results will be presented for different groups: Rural males and females and their urban counterparts. The large sample size provides sufficient thickness of observations around the threshold in each group to draw meaningful conclusions. Empirical researchers often face an important trade-off between large sample sizes and more observable characteristics, with smaller surveys being able to afford larger and more detailed questionnaires. The present study is no exception: The large sample comes at the price of not being able to observe a household's beneficiary status. The largest survey available that would contain all the relevant information (the already mentioned ENIGH) suffers from a comparatively small sample size (the 2014 round contains a total of 21,400 households) that renders RD estimation infeasible. The estimations are, therefore, conducted as a sharp RD based on the permanency threshold and can be interpreted as an ITE, rather than an ATE. For a fuzzy RD estimator, the expression in (1) would be divided by the (similarly estimated) change in the treatment at the threshold:

$$b_{RD}^F = \frac{\alpha_y^+ - \alpha_y^-}{\alpha_d^+ - \alpha_d^-}, \quad (2)$$

The denominator of which has to be between zero and one. The estimates presented can thus be thought of as a lower bound on the true treatment effect. In this sense, the lack of information on beneficiary status works against the econometrician by biasing the estimates towards zero.

The two groups of interest for this study are individuals aged 13–15 and 16–18. The typical Mexican student enters lower secondary school (*Secundaria*) after six years of primary school at age 12, and upper secondary school (*Preparatoria*) at age 15, to graduate at age 18. The data contain the age of each person surveyed as of March 2015, but not the exact date of birth. But since they were collected about 3 months before the end of the school year the majority of students in 12th grade can be assumed to be 18 years old at that point. Likewise, most students in 10th grade can be assumed 16 years old; most of ninth graders 15 years, and most seventh graders 13 years. The population of interest consists of all children in the respective age bracket who live in localities that participated in the recertification process during 2011/12,⁸ and who have successfully graduated from primary school or lower secondary school, respectively. The sample also excludes children who are not living with at least one of their parents, since their eligibility to *Oportunidades* is unclear without more information.

⁷ The questionnaire follows the same design as the extended questionnaires used in earlier censuses for a subsample of the population.

⁸ I use ENCASEH data on the universe of recertified household to determine which localities took part.

There are two sources of noise that will increase the “fuzziness” around the threshold, and hence work against finding any significant effects. The first one is the time lag between recertification in 2011/12 and observation in 2015. However, given the nature of the included characteristics, discussed above and listed in Table 1, they can be expected not to change very much over 3–4 years. Households' demographic characteristics have been adjusted to take this time lag into account.⁹ On the positive side, some time lag between recertification and observation seems necessary to obtain any significant results, since parents are unlikely to pull their children out of school the day they lose the benefit. Secondly, the *Encuesta* has information, with almost identical wording as in the ENCASEH, on all characteristics used with the exception of whether the household owns a VHS or DVD player. In order to make up for this lack of information, predicted values for DVD/VHS ownership are constructed from dwelling characteristics, ownership of durable goods, and availability of other services that are observable in both, the ENIGH and the *Encuesta*, and do not enter the proxy means test. I estimate a probit model with the ENIGH to predict the probability of DVD/VHS ownership, which I then apply it to the *Encuesta*.¹⁰ This correction, while not ideal, is not expected to affect the results in any significant manner. The weight given to DVD/VHS ownership in the index is fairly small, and will therefore only add some additional noise to the index.

The index is designed as a proxy measure for per-capita household income, based on easily observable household characteristics. The parameters were estimated by regressing the logarithm of income, as observed in the ENIGH, on the characteristics listed in Table 1. The index itself is defined as the predicted monthly per-capita income in 2010 Mexican Pesos (MXN) (i.e. by using the predicted value from the linear model in an exponential function). In order to qualify for the benefit, an urban household had to score less than 1,243.15 MXN, and a rural one less than 716.17 MXN. This paper, however, does not use the qualification threshold for new beneficiaries, but the second, higher, threshold used to determine whether the benefit is discontinued. The corresponding values here are 1,538.29 MXN for urban, and 1,145.65 MXN for rural households. According to CONEVAL, these compare to an extreme per-capita poverty line of 699.65 MXN for rural, and 996.04 MXN for urban households in October 2010. The corresponding lines for relative poverty are 1,356 MXN for rural and 2,155.07 MXN for urban households. Thus, while eligibility is put slightly above the extreme poverty line, the benefit is lost when a household is still considerably below the relative poverty line. In this context, it is also important to point out that no other Mexican social program uses the ENCASEH questionnaire to determine eligibility. There is thus no risk that other programs may act as confounding factors by employing a very similar assignment mechanism. Using the index and the values for permanency, each household's predicted distance to the cutoff is computed. It is this distance measure that constitutes the assignment variable in the RD design. Fig. 2 shows as histogram of this distance for all minors aged 16–18 in the sample, up to a distance of 1000 MXN. Given how the index is constructed, and that it is meant to proxy for income, it is not surprising that it follows a log-normal distribution (it would be worrisome if it did not). The graph shows about 80% of all observations with the right tail cut-off.¹¹ There is no visible jump or any other discontinuity at the threshold.

⁹ For most localities, the data shows that almost all households went through recertification in the same year. For the localities that had interviews conducted in both years, the year with most interviews was taken used to adjust the demographic characteristics.

¹⁰ Regression results of the model are presented in the appendix.

¹¹ The survey stratification oversamples households in smaller, and on average poorer, localities.

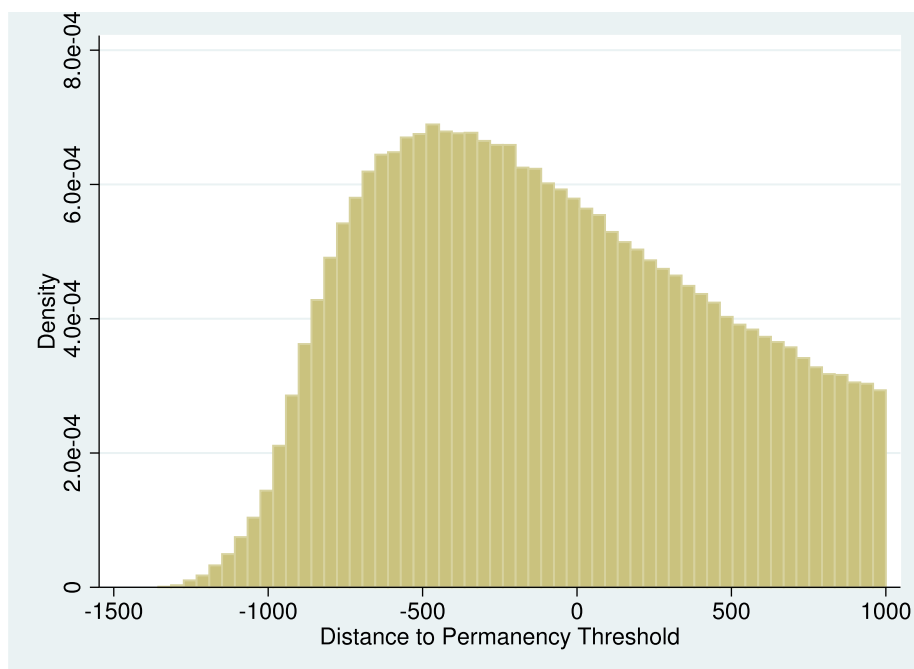


Fig. 2. Distribution of the distance to the permanency threshold by household.

The principal specification employs observations within a ± 100 MXN neighborhood around the permanency threshold, using a local linear regression. Results will also be presented for wider neighborhoods (data windows) and higher local polynomial regressions. Taking a closer look at the different outcomes of interest, Table 2 shows summary statistics for the eight different groups of minors aged 13–15 and 16–18 within ± 100 MXN of the permanency threshold. The first line in each of the four panels shows the outcome of school attendance at their corresponding level. On average, around 90% of those aged 13–15 attend lower secondary school, and 69% of males and 75% of females aged 16–18 upper secondary. The next four outcomes capture a person's primary occupation during the week prior to the interview. There are always a few percent fewer children who primarily study than are attending school. This indicates that some of those who do attend school, do not consider this their primary activity. Unsurprisingly, more of the older children are primarily in paid work. Also, more males than females are working, while the latter are more commonly found in household chores. Close to 10% of older, and 5%–6% of younger, males do neither work nor study. This number is slightly lower for females. The remaining variables shown will be used to assess their smoothness at the threshold. They capture whether or not a child is fully or partially indigenous; his/her parents average years of education (measured as the average of the household head and the spouse or domestic partner, and only the former if no spouse/domestic partner is present); a binary variable indicating that no spouse or domestic partner of the household head is present; the number of other minors in the household; a binary variable indicating a nuclear family household; the total number of household members; the age of the household head; a binary variable indicating that at least one household member owns agricultural or grazing land; a binary variable indicating that the household suffers from food vulnerability¹²; and reported household income excluding transfers.

¹² Defined someone in the household having to forgo meals or eat less for lack of economic resources.

7. Results

Estimation results will be presented in several steps, starting with the local linear regression RD estimates on school attendance for observations within a ± 100 MXN neighborhood around the permanency threshold. Two subgroups are of interest here: Firstly, male and female youth may be facing different labor market conditions. In terms of the model in Fig. 1, they may be facing different wage rates, leading to different decision on their educational investment. Secondly, it is of interest to distinguish between rural localities on the one hand, and urban ones on the other. For one, larger localities may offer different (likely higher) wages compared to rural ones. Again, following Fig. 1, this would affect the decision on educational investment. Moreover, Oportunidades works differently in rural localities (i.e. those with fewer than 2,500 inhabitants) than in urban ones. The program was only extended beyond rural areas in 2002. And as discussed above, the model used to estimate household per-capita income differs between the two, as do the thresholds established for eligibility and permanency. As a final step, results will be presented taking into account all four subgroups.

This is followed by an analysis of the effect of program graduation on respondents reported primary activity in the week prior to the survey interview. The options are to be studying, working (including active job search), being dedicated to household chores, or be neither working nor studying. These outcomes are likely much noisier than school attendance. One can, for example, attend school, yet report work as one's primary activity. Similarly, one may declare to be primarily studying without actually attending school. The main objective of this exercise is to establish, as implied by Fig. 1, whether or not any reduction in school attendance is associated with an increase in work.

Next, I will present a number of robustness checks to assess the validity of the RD approach. The single most important identification assumption is that only the probability of receiving treatment changes discontinuously at the threshold, and, as a consequence, all the outcomes affected by it. In particular, any other variable that

Table 2
Descriptive statistics for principal outcomes by group.

	Rural						Urban					
	Males			Females			Males			Females		
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
<i>13–15 Year Olds:</i>												
Attends School	5697	0.91	0.29	5467	0.93	0.25	14,282	0.89	0.31	13,887	0.92	0.27
Work	5687	0.07	0.25	5457	0.02	0.14	14,256	0.09	0.28	13,871	0.03	0.16
No Work	5687	0.06	0.23	5457	0.03	0.17	14,256	0.05	0.22	13,871	0.04	0.20
Household Work	5687	0.00	0.07	5457	0.04	0.20	14,256	0.01	0.09	13,871	0.04	0.20
Studies	5687	0.87	0.33	5457	0.91	0.29	14,256	0.85	0.35	13,871	0.89	0.31
Indigenous	5603	0.25	0.43	5375	0.25	0.43	14,062	0.23	0.42	13,693	0.23	0.42
Av. Educ. Parents	5697	7.54	2.63	5468	7.52	2.69	14,287	7.93	2.82	13,892	7.94	2.86
No Spouse	5697	0.18	0.38	5468	0.19	0.39	14,287	0.21	0.41	13,892	0.22	0.41
Num. Oth. Minors	5697	1.52	1.11	5468	1.56	1.16	14,287	1.61	1.12	13,892	1.62	1.11
Nuclear HH	5697	0.84	0.37	5468	0.83	0.38	14,287	0.80	0.40	13,892	0.80	0.40
HH Size	5697	4.80	1.42	5468	4.87	1.52	14,287	4.93	1.52	13,892	4.92	1.52
Age Head	5697	42.91	7.60	5468	43.14	7.75	14,287	42.23	7.60	13,892	42.22	7.68
Agricultural Land	5697	0.15	0.36	5468	0.16	0.36	14,287	0.05	0.21	13,892	0.05	0.21
Food Vulnerability	5697	0.39	0.49	5468	0.38	0.49	14,287	0.41	0.49	13,892	0.40	0.49
HH Income	4781	5821	15,036	4,599	5629	4730	12,814	6715	4899	12,445	6713	10,094
<i>16–18 Year Olds:</i>												
Attends School	4346	0.69	0.46	4060	0.75	0.44	10,857	0.69	0.46	10,435	0.74	0.44
Work	4339	0.26	0.44	4054	0.09	0.28	10,851	0.28	0.45	10,417	0.13	0.33
No Work	4339	0.09	0.29	4054	0.05	0.22	10,851	0.08	0.27	10,417	0.06	0.23
Household Work	4339	0.00	0.07	4054	0.15	0.36	10,851	0.01	0.10	10,417	0.12	0.32
Studies	4339	0.64	0.48	4054	0.71	0.45	10,851	0.63	0.48	10,417	0.70	0.46
Indigenous	4266	0.26	0.44	4005	0.26	0.44	10,674	0.24	0.43	10,311	0.25	0.43
Av. Educ. Parents	4348	7.18	2.80	4062	7.23	2.78	10,866	7.68	2.95	10,440	7.65	2.94
No Spouse	4348	0.18	0.38	4062	0.19	0.40	10,866	0.23	0.42	10,440	0.24	0.43
Num. Oth. Minors	4348	1.45	1.16	4062	1.45	1.19	10,866	1.50	1.15	10,440	1.51	1.16
Nuclear HH	4348	0.78	0.41	4062	0.77	0.42	10,866	0.76	0.43	10,440	0.72	0.45
HH Size	4348	4.91	1.56	4062	4.92	1.57	10,866	4.99	1.58	10,440	5.03	1.60
Age Head	4348	45.97	7.72	4062	46.00	7.76	10,866	44.95	7.65	10,440	45.05	7.65
Agricultural Land	4348	0.19	0.40	4062	0.18	0.39	10,866	0.06	0.24	10,440	0.06	0.23
Food Vulnerability	4348	0.42	0.49	4062	0.39	0.49	10,866	0.42	0.49	10,440	0.40	0.49
HH Income	3746	6151	17,102	3401	5762	5089	9769	6995	5047	9392	6834	5278

may have an effect on the outcomes of interest must be continuous around the threshold. This smoothness assumption will be tested by running the same RD design on household characteristics that (i) could affect school attendance, and (ii) are observed in the data. Another important test is whether the results presented are sensitive to the data window or the functional form specification used (\pm MXN100 in predicted per-capita income around the threshold and a local linear regression). I will, therefore, also present results for different data windows and higher local polynomial regressions. A final check consists of a visual inspection of the discontinuity in the outcome variable. It would be a concern if the discontinuity was the product of a sudden increase or drop right before or after the threshold in an assignment function that shows otherwise a constant slope.

The results tables show the estimated size of the bias-corrected discontinuity at the threshold, based, unless otherwise noted, on a local linear regression. Below, in parentheses, p-values based on robust standard errors are shown. The bottom of table or panel shows the number of observations effectively included in the local regression to the left and to the right of the threshold, followed by the bandwidths used in the regression and for the bias correction, respectively.

7.1. Main results

Table 3 shows the paper's principal results on the intention to treat effect of program graduation on school attendance. Taking the whole sample into account, or dividing the sample by gender, in columns 1–3 a statistically significant effect is only detectable for 13–15 year olds (i.e. in lower secondary school). However, once the sample is divided into rural and urban subsamples in columns 4

and 5, the picture changes. The effect for 13–15 year olds is entirely driven by the urban observations: The magnitude of the effect increases from 3.77 percentage points to 5.67 and its statistical significance increases from the five-percent to the one-percent level. The estimated effect in rural areas, on the other hand, is almost zero. In terms of the theory from Fig. 1, these results correspond to a situation in which the opportunity cost of the child not working is high enough in urban areas for the transfer to have the intended effect. In rural areas it is still too low for that age group.

For 16–18 year olds, I now find a negative effect of 13.87 percentage points on school attendance in rural areas, significant at the one-percent level. The estimated effect in urban areas is almost exactly zero. Referring back to Table 2, it is not surprising that a statistically significant effect for the whole sample could only be found for the younger cohort, given that the sample consists of over twice as many urban than rural observations. The implications in terms of the theory behind CCTs is that as children get older, their opportunity costs of not working increases. This moves rural children to a high enough potential wage for the program to have its intended effect. However, the foregone wage for urban households is so high that the transfer is not enough to entice a change in behavior.

Dividing the rural and urban subsamples further by gender, columns 6–9, only confirms the previous results, the point estimates for males and females are almost identical. One can also go a little further, and directly test for the equality of the estimated effects between the different subsamples using an equality of means test. Comparing effects in rural and urban areas for the two age groups, the null-hypothesis of equally sized effects can be rejected with a p-value of 0.0404 (z-score of 2.047) for children aged 13–15, and with a p-value of 0.0102 (z-score of 2.5682) for 16–18 year-olds.

Table 3
Results For School Attendance.

	All			Rural	Urban	Rural		Urban	
	All	Male	Female	All	All	Male	Female	Male	Female
13–15 Years:									
RDD Effect	−0.0377**	−0.0461**	−0.0270	0.0051	−0.0567***	0.0021	0.0096	−0.0559**	−0.0497**
p-value	(0.0238)	(0.0459)	(0.1699)	(0.8184)	(0.0050)	(0.9489)	(0.7626)	(0.0387)	(0.0489)
Obs. left	3998	2262	2461	2362	2590	1258	1030	1627	1426
Obs. right	3809	2124	2392	2328	2542	1248	996	1508	1410
Bandwidth Regression	20.08	22.36	25.08	42.28	18.32	44.24	36.74	22.33	20.33
Bandwidth Bias	33.67	37.04	43.03	62.21	31.50	65.05	54.47	35.77	36.12
16–18 Years:									
RDD Effect	−0.0371	−0.0066	−0.0495	−0.1387***	0.0003	−0.1432**	−0.1496***	0.0163	−0.0091
p-value	(0.1248)	(0.8347)	(0.1181)	(0.0022)	(0.9928)	(0.0253)	(0.0086)	(0.7087)	(0.7962)
Obs. left	4492	2764	2453	1131	3237	607	564	1558	2189
Obs. right	4448	2857	2318	1102	3209	599	554	1651	2068
Bandwidth Regression	30.12	36.75	32.90	26.73	30.25	27.87	27.79	29.40	40.76
Bandwidth Bias	52.46	58.95	55.59	49.18	47.53	47.40	52.52	46.81	62.35

Notes: Results show bias-corrected estimates for discontinuity using local linear regression; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Robust p-values in parentheses.

Table 4
Results for Main Activity in the Past Week.

	Male				Female			
	Paid Work	No Work	HH Work	Studying	Paid Work	No Work	HH Work	Studying
13–15 Years								
Urban:								
RDD Effect	0.0213	0.0215	0.0150*	−0.0530*	0.0191	0.0059	−0.0080	−0.0299
p-value	(0.3362)	(0.3141)	(0.0538)	(0.0942)	(0.1200)	(0.7220)	(0.5823)	(0.2659)
Obs. left	1958	1754	1808	1691	2310	2220	2504	1768
Obs. right	1809	1617	1656	1571	2286	2217	2478	1744
Bandwidth Regression	26.85	23.87	24.53	23.18	33.26	32.11	36.25	25.25
Bandwidth Bias	41.48	37.83	35.48	35.39	55.91	48.37	55.04	43.59
16–18 Years								
Rural:								
RDD Effect	0.0646	0.0093	0.0158*	−0.0910	0.0391	0.0546**	0.0450	−0.1335**
p-value	(0.2959)	(0.8169)	(0.0785)	(0.1930)	(0.3188)	(0.0408)	(0.4180)	(0.0389)
Obs. left	595	640	571	574	555	442	563	573
Obs. right	588	632	560	562	544	429	552	561
Bandwidth Regression	27.25	29.37	26.09	26.35	27.32	21.90	27.74	28.29
Bandwidth Bias	44.72	45.84	30.44	41.73	45.01	37.49	46.63	46.65

Notes: Results show bias-corrected estimates for discontinuity using local linear regression; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Robust p-values in parentheses.

Given the very similar estimates for males and females in the two groups with statistically significant results, it is not surprising that the null-hypothesis of equality cannot be rejected. The corresponding statistics are a p-value of 0.865 (z-score of 0.167) for urban 13–15 year old children, and a p-value of 0.99 (z-score of 0.0749) for rural ones aged 16–18.

Table 4 shows the corresponding results for a child's main activity in the week prior to the survey interview. These are only of interest for the subsamples with statistically significant results on school attendance, i.e. 13–15 year old urbanites, and 16–18 year old rural dwellers. As discussed above, these outcomes are much noisier than school attendance. Hence, statistical significance is much lower. The important result is that the estimated effects on being primarily dedicated to study are very similar to the ones on school attendance and negative throughout (though only statistically significant for rural females). With only one exception, which has a negative estimate that is almost zero, the point estimates for all other activities are positive. Some additional insights can be gleaned by looking only at sign and magnitude of the effect. With the exception of rural females, the biggest increase in the principal activity is in paid work. For both urban

and rural males, though smaller in magnitude, the increase in household chores is significant at the ten-percent level. This is striking, given the generally low level of males dedicated to this task. It is also of interest that for rural females the principal activity with the largest increase is to be neither studying nor working. This last result is statistically significant at the five-percent level.

7.2. Robustness

I will now directly address a number of possible caveats that could cast doubt on these results. In the interest of space, and given the similarity in the effects on school attendance for males and females, the two genders will be pooled. To assess the smoothness of possible confounding variables, Table 5 shows RD results (using the same model as for the outcomes of interest) for characteristics that may have a direct effect on school attendance. These characteristics are shown in Table 2, and are briefly discussed at the end of Section 6. Out of 20 separate estimations, two mutually exclusive samples and ten different outcomes, only two yield results that are statistically significant at the ten-percent level-

Table 5
Results for Other Characteristics.

	Indigenous	Educ Parents	No Spouse	Other Minors	Nuclear HH	HH Size	Age Head	Agri Land	Food Vul	HH Income
<i>13–15 Years</i>										
<i>Urban:</i>										
RDD Effect	0.0122	−0.1814	−0.0275	−0.1184*	−0.0068	−0.0045	0.8648*	0.0016	0.0279	−624.12
p-value	(0.5750)	(0.2266)	(0.1999)	(0.0908)	(0.7335)	(0.9592)	(0.0709)	(0.8735)	(0.3535)	(0.2595)
Obs. Left	5039	5216	4873	3285	5602	4610	3645	5174	3937	2185
Obs. Right	4874	5053	4712	3144	5386	4457	3480	5011	3834	2152
BW Regression	35.89	36.53	34.18	22.92	39.19	32.32	25.39	36.26	27.82	17.31
BW Bias	56.41	60.37	56.64	40.18	59.57	48.82	41.66	55.94	43.04	38.13
<i>16–18 Years</i>										
<i>Rural:</i>										
RDD Effect	−0.0037	−0.0776	0.0053	−0.0451	−0.0224	0.0553	−0.3502	−0.0153	0.0343	49.81
p-value	(0.9258)	(0.8022)	(0.8789)	(0.7119)	(0.5610)	(0.7365)	(0.6978)	(0.6624)	(0.4566)	(0.9252)
Obs. Left	1607	1203	1648	1223	1516	1244	1009	1814	1615	1229
Obs. Right	1581	1189	1619	1202	1500	1236	988	1779	1590	1224
BW Regression	38.42	28.60	38.71	28.94	35.76	29.61	24.23	42.58	37.96	34.40
BW Bias	61.08	43.86	60.44	43.72	57.25	47.38	38.90	63.96	57.37	53.40

Notes: Results show bias-corrected estimates for discontinuity using local linear regression; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Robust p-values in parentheses.

Table 6
Results for School Attendance at Different Polynomials and Data Windows.

	13–15 Year Olds Urban			16–18 Year Olds Rural		
	±50	±100	±200	±50	±100	±200
<i>First Order</i>						
RDD Effect	−0.0625***	−0.0567***	−0.0235*	−0.1155*	−0.1387***	−0.1199***
p-value	(0.0046)	(0.0050)	(0.0579)	(0.0595)	(0.0022)	(0.0033)
Obs. left	2151	2590	7184	669	1131	1421
Obs. right	2128	2542	6891	665	1102	1421
Bandwidth Regression	15.20	18.32	50.15	15.92	26.73	33.48
Bandwidth Bias	26.12	31.50	83.94	24.57	49.18	61.44
<i>Second Order</i>						
RDD Effect	−0.0690**	−0.0596***	−0.0204	−0.0950	−0.1465**	−0.1162***
p-value	(0.0101)	(0.0036)	(0.1132)	(0.2342)	(0.0100)	(0.0077)
Obs. left	2878	4963	13616	771	1514	2697
Obs. right	2763	4803	12865	741	1499	2608
Bandwidth Regression	20.15	34.80	93.91	18.20	35.72	62.29
Bandwidth Bias	27.79	52.32	128.75	24.55	48.81	86.32
<i>Third Order</i>						
RDD Effect	−0.0655**	−0.0632***	−0.0615***	−0.0859	−0.1448**	−0.1134**
p-value	(0.0444)	(0.0070)	(0.0016)	(0.3016)	(0.0201)	(0.0144)
Obs. left	2935	6481	9094	1152	2080	3962
Obs. right	2821	6233	8760	1134	2048	3730
Bandwidth Regression	20.56	45.29	63.59	27.31	49.09	91.58
Bandwidth Bias	26.33	57.87	86.78	34.54	61.33	114.55

Notes: Results show bias-corrected estimates for discontinuity using local polynomial regression of first to third order; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Robust p-values in parentheses.

which is the number one would expect from randomly generated data.

In Table 6, the analysis is extended to different data windows and higher order local polynomials. The size of the data window is halved, to \pm MXN50, and doubled to \pm MXN200. Local polynomial regressions are estimated up to the third order. The estimates in the top panel for the \pm MXN100 data window are the same as those in columns 4 (for 16–18 year-olds) and 5 (for 13–15 year-olds) of Table 3. The results are fairly robust in terms of the estimated magnitude of the effect at the threshold. Using a higher order polynomial on a narrower data window compromises statistical significance, but does not alter the point estimates very much. Wider data windows, in turn, require a higher order polynomial to yield precise results (this is particularly the case for urban areas, where the extension of the window results in a much larger bandwidth compared to the rural case).

Lastly, Figs. 3 and 4 provide a visual impression of the discontinuity, based on a third order polynomial. In Fig. 3, the assignment function is relatively flat on either side of the threshold, and shows a clear downward shift at the discontinuity. In Fig. 4, the assignment function changes its slope at various points. This curviness may be the result of fewer observations in rural areas. Observing the steep increase in the function only right before or only right after the threshold would be a concern. However, observing it on both sides simultaneously and with a similar slope at the boundary, as is the case in Fig. 3, is expected when threshold falls on a segment with a steep positive slope. Fig. 4.

8. Discussion and conclusions

Conditional Cash Transfer programs were established to provide welfare payments while, at the same time, better aligning parents

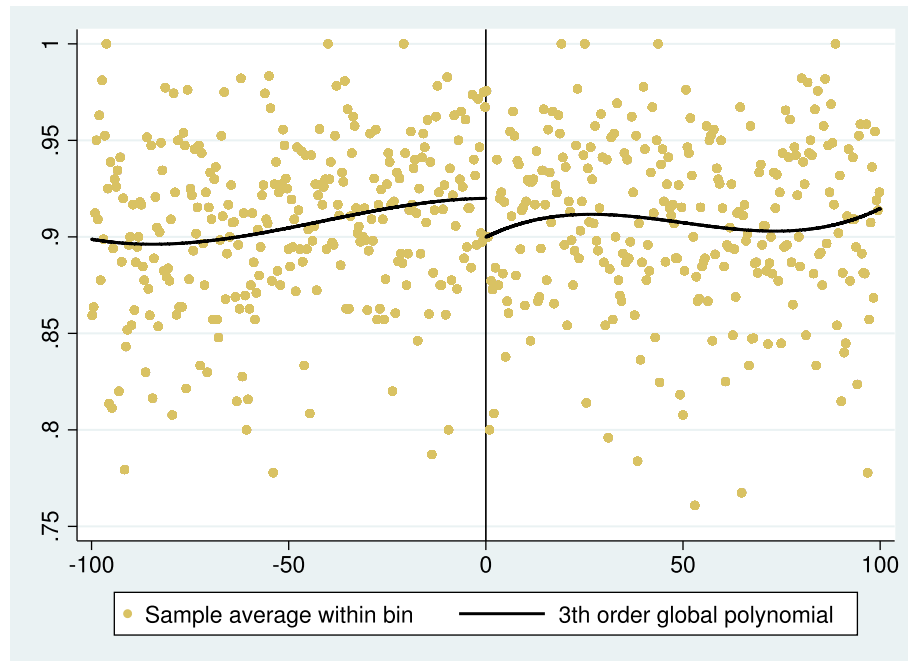


Fig. 3. Discontinuity for school outcome with 3rd order polynomial for urban 13–15 year-olds. Notes: Graph shows discontinuity at threshold based on a 3rd-order global polynomial. The y-axis shows the probability of school attendance, and the x-axis shows the distance to the threshold of the estimated household per-capita income in MXN.

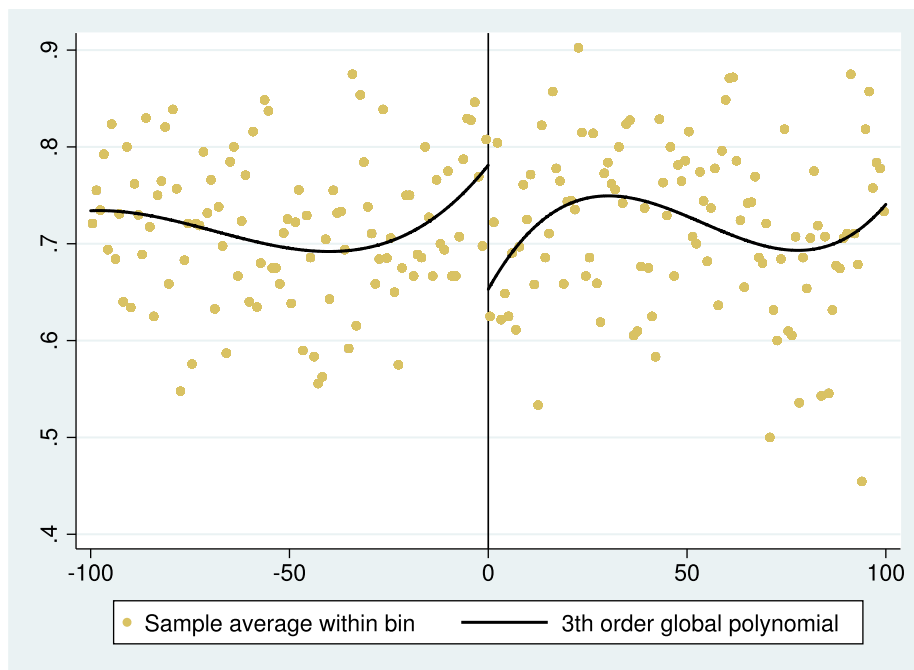


Fig. 4. Discontinuity for school outcome with 3rd order polynomial for rural 16–18 year olds. Notes: Graph shows discontinuity at threshold based on a 3rd-order global polynomial. The y-axis shows the probability of school attendance, and the x-axis shows the distance to the threshold of the estimated household per-capita income in MXN.

incentives with the long-term interests of their children. When households are poor, combining a large enough welfare payment (relative to the prevailing wage rate for minors) with a requirement for some minimum investment in children's human capital, can effectively increase school attendance. As household income increases, the need for welfare support as well as for children's labor income will decrease. The decision at which income level CCTs should be phased out needs to take both aspects into account. When program graduation results in lower school attendance this income threshold may have been chosen too low.

This study assessed the effect of program graduation from Mexico's flagship social protection program Oportunidades during the 2011/12 recertification cycle. It showed that it resulted in a significant reduction in school attendance. The point estimates of the intention to treat effect (ITE) indicate a drop of five percentage points for the younger cohort in urban areas and of over 14 percentage points for the older rural students. In order to get an idea of the size of the average treatment effect (ATE), estimates of the magnitude of the denominator in expression 2 in Section 6 are needed. Under the assumption that all (or close to all) households

to the right of the threshold living in localities that went through recertification were effectively dropped from the program, the proportion of beneficiary households to the left can be used as a (rough) estimate of the denominator. These proportions can be calculated with a high degree of accuracy, given the large sample size in the Encuesta Intercensal 2015, and the availability of official program data. According to the latter, among the urban households that were recertified during 2011/12, 67,304 fell within MXN 100 below the permanency threshold. The corresponding number for rural households is 52,139. Using data from the Encuesta Intercensal, and applying sampling weights, the total number of households that fall within this distance, i.e. including those who were not beneficiaries, should have been 313,079 and 61,209, respectively. This implies pre-certification levels of coverage in this bracket of 21.5% in urban and 85.18% in rural localities. This, in turn, yields an urban ATE of 26.37% and a rural ATE of 16.28%. While these number contain a lot of noise, they do contain some useful information: Firstly, the size of the effect on younger urban, and older rural students is likely much more similar than the ITE estimates suggest (and the urban ATE may even be larger than the rural one). Secondly, the magnitude of the effect is more important than suggested by the ITEs.

Why are there no significant results for younger rural and/or older urban students? The results for these groups in Table 3 have very low point estimates and very large p-values, implying that there is indeed no effect. The model in Fig. 1 provides a plausible explanation. As discussed above, it has been argued the much lower participation rate among eligible households in the program is at least partially the result of its lower attractiveness in urban areas. This situation is captured in panel (c): A high opportunity cost of school attendance (i.e. foregone wages for a child's work) relative to the benefit paid renders the program ineffective. It seems reasonable to assume that attainable wages are increasing in a child's age. The results, therefore, suggest that the representative urban household at the permanency threshold finds itself in the situation depicted in panel (b) for children aged 13–15, and in panel (c) for children aged 16–18. For rural households the situation is different. While the wage is also likely to increase in a child's age, the overall wage level is lower than in urban areas. This implies that for 13–15 year old children the situation of the representative rural household at threshold is captured in panel (a): A very low opportunity cost of school attendance means that children continue to attend lower secondary school even if the benefit is removed. Once they reach upper secondary school age, and the attainable wage increased, their situation changes to the one in panel (b). While the data employed in this study do not allow to directly test for these explanations, the results are at least consistent with the basic theory behind CCTs. That is, the results confirm the predicted effect from the theory laid out in Fig. 1. A direct test would require the ability to observe potential wages for each child.

Some corollary results are also notable. The small, but statistically significant increase (albeit only at the ten-percent level) in the proportion of younger males in urban and older males in rural areas that dedicate themselves primarily to household chores may be explained by the decision of the household to free up the time of some other household member to participate in wage labor. While not explicitly modeled, this result is in line with the argument on Fig. 1. A second result, which is not captured by the model, is the increase in rural upper secondary aged females that end up neither working nor studying in response to the loss of benefit. This may be explained by extending to model to account for leisure, or allow for different parental preferences with respect to female educational attainment. If rural wages are low for females, but leisure is preferred to further education, women aged 16–18 may simply

transition to idleness once the monetary incentive of school attendance is removed. Both results are worthwhile pursuing further in future research.

Overall, the results imply that to the extent to which human capital formation is a primary objective of Oportunidades, the graduation thresholds may have been chosen too low. These results were obtained using RD methods which, though they allow for the consistent estimation of treatment effects at the discontinuity, are by design very local. With the data available, it is infeasible to determine the income level at which these effects would disappear. From a policy perspective, one would also want to take other factors into account when deciding on the optimal graduation thresholds. These include the trade-off between the welfare aspects of Oportunidades and the goals related to greater human capital formation. That is, raising the graduation bar would result in payments being made to households that were thus far not considered poor enough to merit them. The impact of these additional expenditures on the overall government budget and hence the financing of other programs would also need to be considered. A detailed discussion of all these considerations, most of which are of a normative nature, is beyond the scope of this study.

Generally, some form of program graduation policy forms part of most CCTs' rules of operation, though actual enforcement of these rules seems to vary. Colombia recertifies the beneficiaries of its "*Más Familias en Acción*" CCT with every actualization of its general proxy means tests "*Sistema de Selección de Beneficiarios de Programas Sociales*" (SISBEN). While according to the pertinent legislation these actualization are supposed to happen every three years, thus far they only took place in 2003, 2012, with the next one envisaged for 2020. Similarly, beneficiaries of Peru's CCT "*Jun-tos*" are supposed to have the information in their *Clasificación Socio-Económica* (CSE) updated every three years to determine permanence in the program. Ecuador's "*Registro Social*", used to assess eligibility to the "*Bono de Desarrollo Humano*" CCT and other social programs is updated every five years. For Brazil's *Bolsa Familia*, beneficiary households are required to update their self-reported income every two years. One thing all these programs have in common is that continued participation in the program is solely based on the household's socio-economic or poverty status. For example, the eligibility thresholds for the *Bolsa Familia* benefits are set at the poverty and extreme poverty lines. The results in this paper strongly suggest that more research is needed assess the human capital effects of CCTs at these thresholds. Moreover, since the effects of program graduation have been largely neglected by the literature there is also scope for many other outcomes of interest, such as consumption patterns, poverty risks, or geographical mobility.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A

See Table A.1.

Table A.1

Results for determinants of having VHS or DVD equipment.

	Urban	Rural
Apartment	0.123** (0.0555)	0.910*** (0.315)
Solid Walls	0.157*** (0.0462)	0.103** (0.0470)
Solid Roof	0.0814** (0.0338)	0.0864* (0.0453)
Kitchen	0.0960* (0.0508)	−0.0292 (0.0653)
Public Sewer	−0.0137 (0.0324)	0.0846* (0.0445)
Number Light Bulbs	0.0429*** (0.00449)	0.0542*** (0.00871)
Shower	0.0181 (0.0357)	−0.0607 (0.0541)
Water Tank	0.0398 (0.0271)	0.0482 (0.0530)
Cistern	0.0805** (0.0340)	0.155** (0.0745)
Boiler	0.210*** (0.0268)	0.0642 (0.0590)
Water Pump	0.0628* (0.0327)	0.0619 (0.0585)
Pay TV	0.187*** (0.0239)	0.143*** (0.0434)
Radio	−0.0949*** (0.0296)	−0.208*** (0.0523)
Washer	0.368*** (0.0266)	0.315*** (0.0415)
Constant	−1.149*** (0.0629)	−1.092*** (0.0693)
Observations	14,210	5248

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