Oportunidades to Reduce Smoking in Mexico?

Mabel A. Andalón L.*

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Abstract

This paper investigates the causal effect of Oportunidades, a federal anti-poverty program in Mexico, on the smoking behaviors of its participants. The benefits of this program include sizable cash transfers, health information sessions and schooling. Affecting smoking is not a goal of this program. However, health economics research suggests that the Oportunidades intervention could substantially change smoking among poor Mexicans. Exploiting an exogenous jump in program participation by means of a fuzzy Regression Discontinuity Design, the evidence of this paper suggests a zero local average treatment effect on smoking among adults that participated in the program an average of four years. In contrast, Oportunidades might have increased slightly the smoking rates of participant adolescents. Finally, differential treatments by sex enable isolating the income effect of Oportunidades by estimating the program's impact on adult male smoking. The findings in this paper indicate a null income effect on adult smoking.

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I. Introduction

Cigarette smoking is the leading cause of preventable death among Mexican men and the second among women. The economic impacts of this health behavior include, but are not limited to, workplace productivity loss with associated wage reductions and treatment costs of the diseases related to smoking such as lung cancer and cardiac diseases. In the last decade, the Mexican government implemented several anti-smoking policies. Taxes were raised, health-warning labels and anti-smoking mass media campaigns were launched, and youth access restrictions to cigarettes and clean indoor-air laws were strengthened (Ibáñez-Hernández, 2005). Nonetheless, the prevalence of cigarette consumption among the population aged 12 to 65 slightly increased from 25.8 percent in 1988 to 26.4 percent in 2002 (Sáenz de Miera et al., 2007). In rural areas, the prevalence of smoking is calculated to be at about 14 percent (INEGI, 2004).

This paper investigates the causal effect of *Oportunidades,* a federal program in Mexico, on the smoking behaviors of its participants. The aim of this program is not to affect smoking directly, but to reduce poverty through investments in health, nutrition and education. Yet, based on a long line of health economics research there are several reasons to predict that the *Oportunidades* intervention will affect smoking among poor Mexicans. First, it provided households with subsidies that were very large compared to their baseline incomes. Based on estimated income elasticities that show that smoking is a normal good in Mexico (Jiménez-Ruiz et al., 2008), these subsidies are expected to increase smoking participation by about 9 percent. Second, *Oportunidades* provided participant women and adolescents in high school with health information sessions. Public health experts in Mexico agree that "providing information to the population regarding the health damages caused by tobacco smoking [is] an effective tool to reduce this behavior" (Lopez Antuñano, 2005). Extrapolating from studies of the effects of health information campaigns on smoking (Nuño-Gutiérrez et al., 2008), it is reasonable to predict that the program might reduce smoking participation by about 25 percent. Third, *Oportunidades* increased the schooling of adolescent participants, and a long line of health economics research suggests that increased schooling should improve health and reduce unhealthy behaviors like smoking. Based on Behrman et al., (2005) the schooling attainment of children after four years of participation in *Oportunidades* should have increased by 0.35 years.

From previous research, this is expected to reduce the prevalence of smoking among Mexican adolescents by 14 percent.

The contributions of this paper are the following. First, estimating the causal treatment effect of participation in *Oportunidades* on adolescent and adult smoking, documents the existence of unintended effects of the program. Furthermore, since smoking is an input in the production of health (Grossman, 1972) and health might reduce poverty via its effects on economic growth (Mayer, 2001; Fields, 2001), estimating the effect of the program on smoking elucidates the possibility of eliminating long-lasting poverty. Second, Gutierrez et al. (2005) and Duarte Gómez et al. (2005) have used propensity score matching (PSM) techniques based on retrospective data to estimate average treatment effects on smoking. In contrast, the approach in this paper consists on exploiting an exogenous jump in program participation at the poverty threshold for eligibility by means of a fuzzy Regression Discontinuity (RD) design (Imbens and Lemieux, 2007).[1] The RD is a quasi-experimental design that allows estimating a local average treatment effect (LATE) at the poverty threshold. This effect predicts what would happen with the smoking behaviors of non-participants if they were made eligible and decided to participate. As such, the LATE is particularly relevant when program expansions to cover better-off households via a small change in the cut-off for eligibility (van der Klaauw, 2008). Comparing the LATE with the average program effects previously analyzed, I investigate whether heterogeneous impacts of the program exist.[2] Third, this paper is the first to estimate the causal effects of *Oportunidades* by gender. Furthermore, because of the differential treatments between men and women in the *Oportunidades* program, and in the absence of peer effects, estimating the program's impact on adult male smoking enables isolating the income effect.

This paper’s identification strategy relies on the fact that only the households scoring below a poverty threshold (here normalized to zero) were eligible to participate in the program. As it is

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[1] The distinction between a sharp and fuzzy RD design is due to Trochim (1984). In both designs the probability of participation changes discontinuously at the threshold. In the sharp design this probability changes from zero to one while in the fuzzy design the magnitude of the probability change is smaller.

[2] Heterogeneous effects of *Oportunidades* on household consumption have been recently documented by Djebbari and Smith (2008).

clear from Figure 1, the eligibility rules induced a considerable discontinuity in program participation rates (denoted with open circles) at the eligibility cutoff among nearly identical individuals. Hence, comparing smoking behaviors of those who barely made it to be eligible to those who failed to be eligible potentially eliminates any confounding program selection and omitted variable biases. In consequence, it allows estimating the causal effect of *Oportunidades* on smoking at the threshold for eligibility. In principle, the negative effect of health information sessions and schooling might be offset by the positive income effect. Suppose it does not. Then, the program would have a positive effect on smoking and the discontinuity in program participation rates at the cutoff score would be echoed by a discontinuity in average smoking rates of the type shown in Figure 1 with x's. Smoking rates of individuals below the threshold would be much lower than smoking rates of individuals above the threshold. The discussion that follows demonstrates that the hypothetical average smoking rates are not representative of the true ones. This is shown in two ways. I first present graphical plots similar to Figure 1. Then I report the results of flexible parametric models that approximate the relationship between both program participation and eligibility and smoking and eligibility with an intercept shift at the threshold for eligibility.

The rest of the paper is organized as follows. Section II provides background on the *Oportunidades* program and discusses the pathways through which participating in this program is expected to change smoking behaviors. Section III scrutinizes the research design and the econometric methods used with the available data. Section IV presents the empirical findings for adults and adolescents. Section V addresses the validity of the design. Section VI isolates the likely effect of income on smoking among adults, and Section VII brings together the principal results.

II. Background on *Oportunidades*

2.1. Eligibility rules and identification strategy

Oportunidades, formerly known as PROGRESA, is the backbone of the social policy in Mexico. It currently covers approximately 25 million people, which represents 25 percent of the Mexicans in that country and 90 percent of the population living in extreme poverty (GEUM, 2008). *Oportunidades'* benefits include sizeable cash transfers conditional on households conforming to a set of coresponsabilities. The purpose of the later is to improve the human capital of the poor through investments in health, schooling and nutrition.[3]

At its original stage, this program was targeted to households in extreme poverty of rural marginalized localities with access to a school and a health clinic.[4] Due to budgetary restrictions, the program was randomly phased into communities over two years. In some communities it started in 1998 (treatment-98), and in the remaining communities, coverage started one year later (treatment-99). Within all the marginalized communities, the first step in the selection of beneficiary households involved a multi-dimensional approximation to the poverty condition (Skoufias et al., 1999). Using a discriminant analysis, several individual and households characteristics coming from a census survey conducted in 1997 (ENCASEH-97) were combined to generate a poverty score for each household.[5] Eligible households were those with poverty scores below a region-specific cutoff level. The rights and responsibilities of the eligible households were informed in a community assembly. In particular, *Oportunidades* required household withdrawal from pre-existing social programs. The assembly was also aimed to gather feedback from the community about families that needed to be eligible but were not and vice versa. Disapproval of eligibility status by other members of the community was an exception more than a rule (González de la Rocha and Escobar, 2001).

[3] The program provides nutritional supplements to infants, under-nourished children and pregnant and breastfeeding women (Skoufias and McClafferty, 2001). The impact of this benefit in the outcome of interest is only important if the food supplement releases resources that households can use to buy more cigarettes. This is the same effect that I expect to find as a result of the cash transfers, which I fully address in the paper.

[4] Localities were marginalized if their marginality index was very high or high. Other options of this index were very low, low and medium The marginality index was developed using the method of principal components, based on seven variables: 1) Share of illiterate adults (> 14 years) in the locality, 2) Share of dwellings without water, 3) Share of dwellings without drainage systems, 4) Share of dwellings without electricity, 5) Average number of occupants for room 6) Share of dwellings with dirt floor, and 7) Share of population working in the primary sector (Skoufias and McClafferty, 2001).

[5] The value of the poverty index depends on the following variables: a crowding index (number of people in the household/number of rooms), whether the head of household is female, whether the household has access to medical service, the total number of children in the household less than eleven years old, years of education of the household head, age of the household head, whether the household has a bath and whether the bath has running water, whether the floor of the house has a dirt floor, whether the household has a gas heating system, a refrigerator, a washing machine, whether assets include a vehicle, whether the home is in a rural area and region of residence dummies for the 19 census regions (Parker et al., 2006).

In July of 1999, some of the households in rural areas that were initially ineligible became eligible through a densification process. Such a revision was undertaken to increase the number of households with certain characteristics that were felt to be under-represented when the eligibility status was first determined. In principle, making comparisons of individuals below and above the two thresholds would enable testing for nonlinear impacts of the program at different poverty levels. Unfortunately, performing this test was hindered by the impossibility to get (or infer from the data) the region-specific thresholds that resulted from the densification process.[6] The quasi-experimental evidence presented in this paper is based on the threshold for eligibility in 1997.

The densification process shrank the discontinuity in program participation at the original threshold in treatment-98 communities, and eliminated the discontinuity in treatment-99 communities. This can readily be seen in Appendix Figure A1. Panel A shows pre-densification average participation rates in treatment-98 communities as a function of five equal-size categories of the poverty score on each side of the eligibility cutoff. Panel B plots post-densification average participation rates in treatment-98 communities. A comparison of these figures suggests that the densification process leaved unchanged the participation rates of the originally eligible. It is interesting to note that some of the individuals in non-eligible households were already participating before the densification took place. This was due to both the community assembly and ad-hoc changes in eligibility status in communities that were affected by natural disasters in the late nineties (Rubalcava and Teruel, 2003). Indeed, the densification process in 1999 only slightly increased the participation rates of the originally ineligible. Panel C in Figure A1 of the Appendix post-densification average participation rates in treatment-99 communities. In these communities there is not sharp discontinuity in participation rates for individuals in the neighborhood of the original threshold. This raises concerns regarding the validity of the RD identification in these communities. In consequence, the data used in this paper is limited to treatment-98 rural communities.

[6] In fact, little is known about the criteria actually followed to boost the eligible population. For a discussion see Buddelmeyer and Skoufias, 2003 and Rubalcava and Teruel, 2003.

2.2. Analysis of Potential Impacts of Oportunidades on Smoking

Oportunidades provides families with two subsidies: a food subsidy and a schooling subsidy. They are both handed directly to the mother of the household. The monthly amount of the food subsidy is of 150 pesos (about $15 dollars in 2002), which represents about 20 percent of the baseline household income. Households are encouraged to spend this money to improve nutrition, but they use it as they like. The food subsidy is disbursed conditional on (a) regular health clinic attendance by all family members, and (b) monthly attendance to health information sessions by the mother and adolescents in grades 10-12. Women have attended these sessions since the program started, and adolescents only during the school year since 2001 (De la Torre, 2005). The sessions cover 35 topics. Many aim to promote self-care and one of them covers addiction prevention (SDS, 2003).

Families with children are eligible to receive the schooling subsidy subject to children school attendance in one of the subsidy-eligible grade levels for at least 85 percent of days in a given month. Table 1 shows the schooling subsidy amounts and how they increase with grade level to offset the higher opportunity costs of working for older children. Scholarships are higher for girls as school attendance has traditionally been lower for them. Schooling benefits are permanently discontinued if a child fails a grade more than once.

The mechanisms through which the incentives of the program are expected to affect smoking can be derived from an economic model of addiction. Sophisticated economic models of rational addiction (Becker and Murphy, 1988) and time inconsistent preferences (Gruber and Köszegi, 2001) have been recently developed and tested. Nonetheless, a simpler model suffices to predict changes in smoking resulting from program participation. For instance, the myopic model of addiction by Mullahy (1985) suggests that, in a given time period t, the demand for cigarettes C, is a function of the stock of habits $\mathcal{H}$, the price of cigarettes P_c, the price of all other goods P_{OAG}, the individual's income I, schooling S and information ι:

$$C(t) = [\mathcal{H}(t), P_c(t), P_{OAG}(t), I(t), \mathrm{S}(t), \iota(t)] \quad (1)$$

Schooling and information could also be collapsed in a vector of covariates affecting both the production of nicotine services and the perceptions of health promotion. In any event, the benefits of *Oportunidades* are expected to affect smoking participation through an income effect, an information effect and a schooling effect. The rest of this subsection discusses the sign and magnitude of the expected changes based on available health economics empirical evidence.

The schooling and food subsidies of *Oportunidades* are expected to affect smoking participation through an income effect. An average of three years of participation in *Oportunidades* increased the average per capita income of those living in extreme poverty by nearly 28 percent (Rubalcava and Teruel, 2003). The income of the participants in my sample should have increased by 37 percent after four years of participation in the program. Relaxing the budget constraint not only allows buying more cigarettes, but also buying smoking quitting devices. It also gives people more access to health information. Therefore, an increase in income does not necessarily imply that smoking should increase. The sign and magnitude of the effect on smoking depends on whether smoking is normal or inferior. This remains an empirical question. In developed nations, early demand studies concluded that it was normal, but recent ones find it to be inferior (Chaloupka and Warner, 1999). In Mexico, Jiménez-Ruiz et al. (2008) estimate an "income elasticity" of household smoking participation of 0.25. To the extent that this figure is representative of the income elasticity in rural areas, *Oportunidades* subsidies would result in 9 percent increase in smoking participation, which represents about 1.5 percentage points.

Another channel through which *Oportunidades* might induce participants to choose healthier behaviors is health information. Assuming ex-ante incomplete information, economic theory predicts that providing information regarding the consequences of tobacco consumption and the addictive nature of tobacco would solve an information failure. Consequently, people would accurately estimate the costs of smoking and ultimately smoke less (i.e. the sign of $\iota(t)$ in equation (1) above would be negative). Public health experts in Mexico support this view. They agree that "providing information to the population regarding the health damages caused by tobacco smoking [is] an effective tool to reduce this behavior" (López Antuñano, 2005).

Recent evidence of the effects of a school anti-smoking campaign in Mexico involving parents, teachers and peers, points to a reduction in the rate of experimental smokers of about 50 percent and no effect on smoking among regular smokers (Nuño-Gutiérrez et al*.,* 2008). Data from the National Addiction Survey (2002) indicates that about 50 percent of smokers are light smokers (INEGI, 2004). This evidence suggests that the provision of information would modify the health perceptions of *Oportunidades* participants, increase the costs of smoking, and reduce smoking participation by about 25 percent.

The impact of the health information sessions is not expected to be confounded with other mass-media policies. This is because tobacco advertising in radio and television was banned in 2004, the same year in which health-warning labels increased from 25 to 50 percent of the back face of the cigarette packs (Sebrie, 2006), but smoking in this paper is measured in 2003. The small health-warning labels that were introduced nationally in 2000 are not likely to have been effective (Hammond et al. 2007); leaving enough room from the health information sessions to have an impact.

Oportunidades might have also affected cigarette consumption among adolescents through a schooling effect. "Years of formal schooling completed have been identified as the most important correlate of good health [and less unhealthy behaviors]" (Grossman, 2004; 32). Farrell and Fuchs (1982) claim that the observed correlation between health and education is mainly due to unobservable characteristics affecting both, investments in health and schooling (e.g. time preferences). An alternative explanation for the correlation is a causal effect from schooling to health or vice versa. Recent empirical evidence suggests a causal effect going from schooling to less unhealthy behaviors. Using the approach of instrumental variables, Currie and Moretti (2003), Kenkel et al. (2006), and De Walque (2007) find a negative effect of schooling on current smoking in the US. These studies, however, provide less guidance about the specific causal pathways involved.[7]

[7] Schooling affects health in three main ways. First, because education is an investment that raises the future level of income and consumption, educated individuals have more incentives to invest in their health (Becker 1993). Second, schooling improves the 'allocative efficiency' of individuals. That is, through more schooling people get access to health knowledge which makes them choose healthier behaviors. Third, schooling increases people's 'productive efficiency'. This is because they produce more health from the same amount of inputs (Grossman, 1972). Kenkel (1991) indicates that the productive efficiency pathway of schooling might induce healthier behaviors.

De Walque (2007) claims that an additional year of schooling beyond college decreases the probability of smoking by 6 percent. Behrman et al. (2005) simulate that participating in *Oportunidades* over an 8-year time period would increase the average educational attainment of children by 0.7 years. Because children in the current sample have participated in the program an average of 4 years, their educational attainment is about 0.35 years more than that of non participant children. Extrapolating from this evidence, it is possible to conclude that *Oportunidades* would decrease smoking participation by 2 percentage points, which represents a 14 percent reduction in smoking participation rates in rural Mexico. Although informative, this ad-hoc estimation should be taken with serious caution for the following reasons. First, it assumes that the effect of schooling on smoking is linear, but the combined evidence in Kenkel et al. (2006) and De Walque (2007) suggests that the marginal returns to schooling are positive but decreasing.[8] If so, because *Oportunidades* main impact is on middle school attendance, its effect on smoking would be higher. Second, it generalizes the instrumental variables estimations to the entire population in the US, and then to the Mexican population.

Table 2 summarizes the expected effects of the program. To the extent that there are no spillover effects of information, the overall effect of the program on men smoking should be at around 9 percent. Since the negative information and schooling effects can potentially be offset by the income effect, the theoretical effect among women and adolescents would be ambiguous. Based on the back-of-the envelope calculations, and in the absence of peer effects, participation in *Oportunidades* would expect to decrease the smoking prevalence among women by as much as 16 percent and among adolescents by as much as 30 percent.

Before I discuss the data and methods of this paper, one issue regarding program effects on smoking participation in relation to the dynamic aspects of smoking deserves further consideration. As DeCicca et al. (2008) highlight, because of addiction, current participation reflects past decisions regarding initiation and cessation. Accumulated evidence from both developed and developing countries suggests that these decisions occur in different points of the

[8] While Kenkel et al., 2006 find that high school completion decreases the probability of smoking by 25 percent; De Walque (2007) finds that finishing college decreases the probability of smoking by 16 percent.

life cycle. According to the 2002 Mexican Family Life Survey (MxFLS) data set, on average people start smoking at 18.4 years of age and quit smoking at 35.9 years. This evidence suggests that most of the program impacts would come about changes in smoking initiation among adolescents, and smoking cessation, among adults.

III. Data and Econometric Methods

3.1 Data

The data used in this paper come from the ENCASEH 1997 and ENCEL 2003 rural surveys. The ENCASEH provides baseline characteristics and household poverty scores. It was composed of two groups of communities: treatment-98 and treatment-99. I restrict attention to the individuals in treatment-98 communities because the densification process invalidated the RD design in treatment-99 localities (Appendix Figure A1, panel C). Treatment-98 are communities where the program started operating since 1998. The ENCEL 2003 is a follow-up survey containing self-reported information about smoking and other health behaviors. The empirical analysis is conducted on separate subsamples of adults and adolescents.

The LATE computed in this paper is informative about smoking behaviors of individuals participating in the program for an average of four years relative to those of non-participants. This is because program participants started receiving the benefits of the program between March of 1998 and December of 2000, and their smoking behaviors are measured in 2003. Smoking participants are those who answered ‘yes’ to the question: Do you currently smoke? Table 3 presents summary statistics of the estimation sample.

3.2 Econometric Framework

Let C_i be cigarette smoking in 2003, and T_i be an indicator for participation in *Oportunidades.* By definition, an individual is never simultaneously observed in both the treatment and the absence of treatment states. Therefore, $T_i = 1$ if an individual i belongs to a household that started receiving the benefits of the program in 2000 or before and continues receiving the benefits in 2003, and $T_i = 0$ if an individual belongs to a household that never received the benefits of the program. Let $C_i(1)$ be the outcome given treatment, and $C_i(0)$ the outcome in the absence of treatment. Then the actual outcome we observe is: $C_i = T_iC_i(1) + (1 - T_i)C_i(0)$. A common regression model representation expresses the outcome as a function of program participation and an unobserved error term representing all causes of cigarette consumption other than participation ε_i :

$$C_i = \alpha + \theta T_i + \varepsilon_i \qquad (2)$$

For simplicity, the previous expression excludes a vector of observable characteristics determining smoking other than treatment, but its inclusion is straightforward. The foremost parameter of interest in this paper, the causal effect of program participation on smoking, is given by θ. To the extent that program participation is exogenous, least squares estimation of equation (2) would produce an unbiased estimate of θ.

As discussed in section 2.1, *Oportunidades* gives households incentives in the form of cash transfers and coresponsabilities, and assigns them to a certain eligibility status based on their poverty scores. Program participation is voluntary. Based on expected benefits and costs of the program, households make the decisions to join the program or not. As such, some households might find it more beneficial to join the program than others; they may 'self-select into the program' (Heckman, 2008). In the *Oportunidades* context, the self-selection problem might be aggravated by the fact that the agents making the choice to join the program may be different from the agents receiving treatment. Note, for instance, that participant adolescents are younger and are more likely to be indigenous than non-participant youths (Table 3). If these differences persist in characteristics not observed by the econometrician, program participation endogeneity concerns would be at stage. Fortunately, the RD design used in this paper overcomes this problem and enables the estimation of an unbiased causal effect of θ: a local average treatment effect.

The identifying assumption in a RD design relies on the fact that program participation is a function of eligibility E (i.e. $T = fn(E)$). We also know that the first stage of the selection of eligible households was based on a known poverty score. In particular, households scoring below a predetermined cutoff poverty score (here normalized to zero) were eligible to participate in the program. That is, letting E_i be program eligibility, and P_i be the poverty index in 1997: $E_i = 1 \ if \ P_i < 0$ and $E_i = 0$. The list of eligible beneficiaries was finalized after getting feedback from the community, and was later changed when the densification process took place. The variables that leaded to these changes are unobserved by the econometrician implying that eligibility depends on the poverty score in a stochastic manner, but in such a way that the propensity of treatment is known to have a discontinuity at the threshold for eligibility. Given this feature, *Oportunidades* can be best characterized by means of a fuzzy Regression Discontinuity design. In contrast, a sharp RD design would require treatment to be a deterministic function of the poverty score (Trochim, 1984).

Estimating θ in a sharp RD design, requires continuity at the cutoff $E[\varepsilon_i|P_i = p]$. Local continuity requires that individuals just above and below the cut-off have similar average potential outcomes when receiving treatment and when not. This identifying condition implies that individuals in the neighborhood of the cutoff should share the same predetermined characteristics (i.e. there is local randomization). Judging from the eligibility rules, this assumption seems plausible, but cannot be taken for granted. As in other research designs, it is impossible to test this assumption for unobserved characteristics. However, in section 5 of this paper, four different tests provide evidence suggesting that observable characteristics of individuals close to the cutoff were nearly identical. Hence, following Hahn et al. (2001), the average treatment effect of *Oportunidades* if the sharp discontinuity in the probability of treatment was one would be given by the difference in smoking behaviors for individuals just below and just above the eligibility cutoff:

$$\theta^{sharp} = \lim_{p\uparrow 0} E[C_i|P_i = p] - \lim_{p\downarrow 0} E[C_i|P_i = p] \tag{3}$$

Since the probability of participation in *Oportunidades* at the threshold for eligibility does not jump from zero to one (see Figure 1), θ^{sharp} in equation (3) would not generally lead to correct inferences regarding an average treatment effect (van der Klaauw, 2008b). Nonetheless, assuming local conditional independence (i.e. individuals do not select into treatment on the basis of anticipated gains from treatment), a fuzzy RD design identifies an average treatment effect of participation precisely when the change in the probability of participation is less than one. This effect is estimated as follows:

$$\theta = \frac{\lim_{p\uparrow 0} E[C_i|P_i=p] - \lim_{p\downarrow 0} E[C_i|P_i=p]}{\lim_{p\uparrow 0} E[T_i|P_i=p] - \lim_{p\downarrow 0} E[T_i|P_i=p]} \tag{4}$$

In the presence of self-selection on the basis of expected gains from the treatment, Hahn et al. (2001) show that under a weaker local monotonicity assumption, the ratio (4) will instead identify a local average treatment effect (LATE) at the cutoff point. The local monotonicity assumption requires the individual level of treatment to be a monotonically increasing (or decreasing) function of the poverty score. This assumption is fundamentally untestable. However, it is expected that the probability of treatment would decrease as the poverty index increases (people become richer). If so, the denominator of equation (4) captures the (less than one) discontinuity in program participation at the threshold. As such, it magnifies the estimate in the discontinuity of smoking status given by (3) by the inverse of the fraction of compliers (Matsudaira, 2008). Compliers are individuals who were induced to participate in the program because their poverty score happened to be slightly below the cutoff score .The LATE differs from the average treatment effect estimated by Duarte Gómez et *al.* (2005) and Gutiérrez et *al.* (2005). The later effect is an estimation of the average effect of the program on compliers and people who participate in the program no matter where the threshold for eligibility is located.

3.3 Estimation

For the estimation of the LATE (as in equation is (4) above), suppose that cigarette smoking C_i measured in 2003, and program participation T_i (as defined in the previous section), can be expressed as a function of the 1997 poverty score P_i as follows:

$$C_i = \alpha_1 + D_i\pi_1 + m_1(P_i) + \varepsilon_{1i} \quad \text{where } E\left[v_1 \middle| P_, E\right] = 0 \text{ and } E_i = 1 \; if \; P_i < 0 \tag{5}$$

$$T_i = \alpha_0 + D_i\pi_0 + m_0(P_i) + v_{0i} \quad \text{where } E\left[v_0 \middle| P_, E\right] = 0 \text{ and } E_i = 1 \; if \; P_i < 0 \tag{6}$$

Under the assumptions that (a) $m_1(.)$ and $m_0(.)$ are continuous at $P = 0$, and (b) the parametrization of the $m(.)$ function is accurate; π_1 in (5) represents the size of the discontinuity in smoking and π_0 in (6) represents the size of the discontinuity in program participation. Note that π_1 and π_0 are the numerator and denominator of equation (4), respectively. Therefore, the causal effect of program participation on smoking is given by their ratio: $\theta = \pi_1/\pi_0$.

There are several ways to estimate θ. One approach is to use non-parametric and semiparametric estimations such as one-sided kernel, local polynomial regression and estimators based on partially linear model estimation with and without covariates (see, for example Haht et al., 2001; Porter, 2003; and Frölich, 2007). The approach in this paper is to use a flexible parametric model following DiNardo and Lee (2004) and Matsudaira (2008). Indeed, it is possible to estimate π_1, π_0, and hence θ, by means of a flexible parametric model. $m_1(.)$ and $m_0(.)$ in equations (5) and (6), represent a n degree polynomial in P, fully interacted with the indicator for eligibility $E_i = 1 \; if \; P_i < 0$ allowing the shape of the conditional expectation vary on either side of the cutoff. This parametrization is expressed as:

$$C_i = \alpha_1 + E_i\pi_1 + E_i \sum_{d=1}^{n} \varphi_{1d} \, (P_i)^d + (1 - E_i) \sum_{d=1}^{n} \varphi_{1p}{}' \, (P_i)^d + \varepsilon_{1i} \quad (7)$$

$$T_i = \alpha_0 + E_i\pi_0 + E_i \sum_{d=1}^{n} \varphi_{0d} \, (P_i)^d + (1 - E_i) \sum_{d=1}^{n} \varphi_{0p}{}' \, (P_i)^d + \varepsilon_{0i} \quad (8)$$

The estimation of π_1 and π_0 depends on the particular functional form of the model relating C_i and T_i to the eligibility score. For that reason, I will perform various specification checks varying the order of the polynomial on either or both sides of the cutoff. However, the preferred estimations will come from a specification where the order of the polynomial is chosen using the Schwarz (1978) criterion. This criterion, also known as the Bayesian Information Criterion (BIC), is commonly used to compare competing regression models. It penalizes a larger model for using additional degrees of freedom while rewarding improvements in goodness of fit (Baum, 2006).

Because this model is exactly identified I estimate it via two-stage least squares (TSLS). As Wooldridge recognizes (2002, 636) the LATE is identical to the instrumental variables estimator of θ in equation (2) when eligibility E_i is used as an instrument for participation T_i. Eligibility is a valid instrument for participation to the extent that it does not have an independent causal impact on smoking besides its effect through participation in *Oportunidades*. One can hardly argue against this assumption. Hence, the first stage of the TSLS estimates program participation as a function of eligibility and the interaction terms. This is analogous to estimating equation (8) above, implying that π_0 is the coefficient on the dummy for eligibility in the first stage. The instrument (eligibility) must be powerful in predicting a statistically significant discontinuity in participation at the threshold for eligibility. If not, $\hat{\pi}_0$ would be zero, and so we would have no variation to work with. Knowing that eligibility induces a discontinuity in program participation, the second stage of the TSLS uses predicted program participation along with the interaction terms to predict smoking. As such, θ is the coefficient on the dummy for participation in the second stage. The relationship between π_0, π_1, and θ implies that $\pi_1 = \theta * \pi_0$. The standard errors of π_1 are estimated from a reduced form specification of Y_i on the dummy for eligibility and the polynomial interaction terms (as in equation (7)). Robust standard errors are reported throughout. In that way, I account for the fact that members of the same family share the same poverty score, and allow for heteroscedasticity due to misspecification of the $m(.)$ function.

All the regressions in this paper exclude a vector of covariates. This is because θ is unchanged to the inclusion of smoking covariates when the identifying assumptions are met. In section 5 I prove that this is indeed the case. In addition, I also contrast the results of the regressions that exclude covariates with those including standard socio-demographic variables, state-level cigarette prices derived from barcode scanning in large food stores reported monthly by the Central Bank of Mexico and a dummy for state-level tobacco control laws.

IV. Results

In this section I present estimates of the effect of program eligibility on participation and smoking, and of the impact of participation in *Oportunidades* on the smoking behaviors of adults and adolescents.

4.1 Effect of eligibility on participation in Oportunidades

Represented with small diamonds, Figure 2 shows average program participation rates of adults (Panel A) and adolescents (Panel B) for five equally spaced poverty score categories on each side of the cutoff. A sizeable jump in program participation of adults at the eligibility cutoff of at least 40 percent is apparent in both figures. Table 4 presents estimations of the size of this jump. These are the coefficients on the dummy for eligibility coming from flexible parametric models where participation is modeled as a function of a full set of polynomial terms along with the eligibility indicator (see eq. (8)). The Schwarz preferred specification includes first-order polynomial terms on both sides of the cutoff. The predicted values of this model are superimposed to the average participation rates in Figure 2. In spite that the poverty score in the figure is truncated at +/- 150 points, the regressions are estimated on the entire range of data. This is because, under the assumption that the first-order polynomial is the true function of the underlying data, the LATE is "efficiently estimated using data that are both close to and far from [either side of] the discontinuity threshold" (DiNardo and Lee, 2004; 1400).

The Schwarz preferred estimation indicates that being eligible to participate in the program is associated with a 53 percentage point increase in the probability of participation for both adults and adolescents with standard errors of .028 and .034, respectively (first row, Table 4). The second-best specification, which includes linear polynomial terms to the left of the cutoff and quadratic to the right, suggests a 45 percentage point difference in participation rates around the threshold. Even thought the size of the discontinuity decreases as higher-order polynomials are included, the magnitude remains sizeable and significant. This evidence suggests that scoring below a predetermined cutoff had a sturdy impact on the probability of participation in *Oportunidades*. On the econometric side, the persistent and statistically significant discontinuity demonstrates the existence of a strong first stage relationship between eligibility (the instrument) and program participation. Hence, identifying causal effects using the RD design is not only justified, but powerful.

4.2. Effect of participation on smoking behaviors

Represented with x's, average adult smoking rates are plotted in Figure 2, Panel A. The similarity of the smoking rates on either side of the cutoff is visually apparent. Moreover, the predictions of the parametric estimates (also plotted in Figure 2 with dashed lines) point to a null effect of eligibility on smoking behaviors. In fact, the -0.003 coefficient together with the small confidence interval around the point estimate implied by the standard errors (s.e.: .011) provide strong evidence of a null effect of eligibility on smoking behaviors (Table 5). The point estimate of the effect of adult participation in *Oportunidades* on smoking is zero (-.005) and precise. In fact, the confidence interval suggests an effect going from -4 to 3 percentage points. The implications of this non-result are interesting, and will be discussed further in section 5 below.

Panel B of Figure 2 plots average program participation and smoking rates and their parametric predictions for adolescents. The parametric estimations of the effects are presented in Table 5.

The effect of eligibility on smoking participation is 1.4 percent, which is small and not statistically significant. The effect of program participation on smoking suggests that participating in the program increased adolescent smoking rates by 2.6 percentage points. This effect is not statistically significant but also less precisely estimated. Because the confidence interval is more on the positive side, smoking of current non-participants would increase slightly if the poverty threshold was moved to cover better-off households and the current non-eligibible became eligible and participate.

4.3. Specification checks

The previous subsection discussed RD results based on first order polynomial regressions. The conclusion that emerged is that, around the threshold for eligibility, adult and adolescent participants smoke at the same rates as non-participants. This is valid so long as the first order polynomial specification of the *m(.)* function is the true function of the underlying data. If the true functions do not belong to the class of first-order polynomials, the discontinuity estimates will in general be biased, and may lead to erroneous inferences of statistical significance (Lee, 2008).

Table 6 reports sensitivity estimates based on alternative model specifications. Based on the Schwarz criterion, the second-best specification includes a second order polynomial only to the right of the threshold. As before, the coefficients are not-significant. However, the sign of the coefficients is reversed. The confidence interval implied by the standard errors is similar to that of the preferred model (+/-5 percent). For completeness, I also report results from a model in which second order polynomial terms are included on both sides of the cutoff. The coefficients of this model remain not significant. Moreover, given that this model is far from being a good fit of the underlying data, it is not surprising that the standard errors increase. All in all, the specification checks presented here do not contradict the main finding of this paper: participation in *Oportunidades* did not affect current smoking rates of adults or adolescents.

V. Internal Validity of the RD estimates

This section's aim is to provide evidence in favor of a randomization around the *Oportunidades* eligibility cutoff, and hence, of the validity of the estimates. The RD impact estimates reported in the previous section are credible so long as the mean outcomes of individuals marginally above the threshold identify the true counterfactual of those marginally below the threshold. For that to be case, the individuals who barely made it to be eligible to participate in the program should be similar in observable and unobservable characteristics determining smoking behaviors to those who almost made it to be eligible. This is analogous to conducting a randomized experiment at the threshold (Hahn et al., 2001). It is precisely this randomization process what will be tested in this section.

As Lee (2008) emphasizes, randomization around the threshold is linked to how much control individuals have on the assignment variable (i.e. the poverty score). Manipulation can be complete or partial (McCrary, 2008). Complete manipulation occurs when the poverty score is entirely under the control of the agent. Partial manipulation occurs when the agent has some control of the assignment variable. Yet, the probability of receiving treatment lies somewhere between 0 and 1 due to an idiosyncratic element.

Partial manipulation of the poverty score might have occurred. Back in 1997, when the first *Oportunidades* survey was carried out, the interviewed families knew that an anti-poverty program was going to be implemented. It is possible that households (or interviewers) reported (registered) some of the variables with error aiming that this would impact the final eligibility assignment. However, complete manipulation was very unlikely because the variables determining the poverty score were not public information until after the program was evaluated for the first time in 1999.

Meaningful parameters using the RD design can be obtained even in the presence of partial manipulation as long as one can prove that the randomization around the threshold worked (Lee, 2008).The critical assumption is that each 'type' of person has an equal chance of scoring just below or just above the threshold. For that to be the case, conditioning on the 'type' of the individual, the density function of the poverty score should be continuous. Since the conditional density is not observable, testing for a discontinuity at the cutoff in the observable density function of the poverty score is informative about the randomization process. This is because continuity of the conditional density implies continuity of the poverty score density.

I test the null hypothesis of zero discontinuity in the density function of the poverty score at the threshold using the Wald test proposed by McCrary (2008), which I estimate through local linear density techniques.[9] Figure 3, plots the conditional expectation of this test along with confidence intervals for adults (top) and adolescents (bottom).[10] Both the graphical analysis and the point estimates (first row, Table 7) establish that the small discontinuity in the log of the baseline poverty score density for adults is not statistically significant. Nonetheless, the discontinuity in the log of the baseline poverty score density of adolescents is statistically significant.

The discontinuity of the density of poverty scores for adolescents casts doubt on the identifying assumption, but it does not prove lack of randomization.[11] I illustrate this point through an example. Suppose that individuals did not exercise any control over their poverty score, but interviewers did. In an attempt to make eligible to the program as many people as possible, poverty 'points' could have been given to those who barely failed the eligibility cutoff. This behavior would have caused the discontinuity of the density of poverty scores that we observe. In spite of that, the identifying assumptions would not be violated so long as points were given randomly to the non-eligible. In contrast, giving points based on characteristics unobserved by the econometrician would damage the research design. The remaining of this section discusses alternative tests that attempt to provide evidence against the hypothesis that the ineligible were made eligible based on unboservables.

[9] I would like to thank Justin McCrary for providing the Stata code to perform the McCrary (2008) test.

[10] Normal Q–Q plots for the t-test of the (true) null hypothesis of continuity, where t-tests stem from 1000 replications are available from the author upon request. They suggest that the normal distribution approximation is accurate as neither skewness nor fat tails are apparent. For details on this see McCrary (2008).

[11] In the same token, failing to reject the null hypothesis of zero discontinuity at the threshold is not conclusive of randomization around the threshold. See McCrary (2008) for a great example on this.

Testing whether the baseline characteristics of individuals in the neighborhood of the cutoff are similar is an alternative way to provide evidence in favor of pre-program randomization at the threshold.[12] As Lee (2008) recognizes, the sample average in a narrow neighborhood of the eligibility cutoff would in general be a biased estimate of the true conditional expectation function at the 0 threshold when that function has a non-zero slope. To address this problem, polynomial approximations (as in equation (7)) are used to generate simple estimates of the discontinuity of baseline characteristics at the threshold for eligibility. Failure to reject the null hypothesis of no discontinuity ensures that the conditional expectation of any baseline characteristic in the poverty score is continuous, which implies that the unobservable conditional density of the poverty score, of main interest to us, is continuous (Lee, 2008). Ideally, I would test for discontinuities of smoking behaviors before program intervention, but these data are not available in the baseline survey. Hence, I test the hypothesis of no discontinuities on a set of covariates that are correlated with smoking status, and other characteristics that are associated with poverty, but did not compose the poverty scores.

Each cell in Panel B of Table 7 represents the discontinuity estimates at the threshold for eligibility of the baseline variables defined in the first column of the table. For instance, the 0.006 coefficient at the intersection of the “female” row and the “adult” column suggests that the female rate before the program started was similar for observations “just above” and “just below” the threshold. The rest of the discontinuities at the cutoff are small and not statistically significant providing evidence in favor of the "randomization" at the threshold for both the adult and adolescent samples. The only exception to this rule is the age coefficient for adults. However, under the null hypothesis that the covariates around the threshold are balanced and independent, we would expect 5 percent of the discontinuity estimates to be statistically different from zero. Moreover, as I discuss below, controlling for age in the estimation of program effects on smoking behaviors does not change the main results of this paper.

The next diagnostic tests predicts smoking status as a function of poverty scores, a dummy for eligibility and all the available baseline covariates related to smoking, but excludes program

[12] This is equivalent to the standard test of randomization in an experimental design, using a test of the equality of the mean of every variable in covariates across treatment and control groups.

participation. These estimations contain all the information that could possibly predict smoking behaviors (aside from participation). Therefore, if individuals just above and just below the cutoff are nearly identical, baseline characteristics should not predict a discontinuity in smoking behaviors. These predictions are reported in Panel C of Table 7. The size of the discontinuities is tiny and not statistically different from zero in both samples, thus favoring the hypothesis of similarity of individuals around the threshold.

The last test involves estimating program effects with covariates included. In the presence of local random assignment, the point estimates of the impact of the program should be insensitive to the inclusion of any combination of baseline covariates (Imbens and Lemiux, 2007; Lee, 2008). In practice, if the covariates are correlated with the potential outcomes, tossing them in the regression may eliminate the biases that result from the inclusion of observations far away from the threshold, which would ultimately improve the precision of the estimates. The bottom part of Table 7 presents the estimates of the effect of *Oportunidades* on smoking participation, θ using a flexible parametric model that includes covariates. It is reassuring to see that the adult estimates are exactly the same as the ones where the covariates were excluded (“participation on smoking” column, Table 5). Furthermore, the standard errors in the model that includes covariates are slightly smaller, as expected. In the adolescent sample, not only the standard errors in the regression that includes covariates drop to zero, but also the point estimate.

This section showed that adults and adolescents around the neighborhood of the cutoff are nearly identical in terms of observable characteristics. The various tests supporting this argument also suggest that the discontinuity in the density of adolescent poverty scores was the result of a random sorting. The available evidence furnishes the non-testable hypothesis of equal unobservable characteristics. It furthermore suggests that individuals were locally randomized, and that the zero impact estimates on smoking behaviors reported in this paper are internally valid.

VI. Discussion

The evidence provided in this paper indicates that program participation in *Oportunidades* caused no effect on the smoking participation decisions of adults, and hence on health outcomes.

This conclusion is supported by both the point estimates and the narrow confidence intervals implied by the standard errors. From the local average treatment effect (LATE) of this paper it is possible to predict what would have happened with the smoking rates of 'compliers' in the absence of treatment. Putting it differently, the LATE tells us what would have happened to the smoking rates of non-participants had the threshold for eligibility being moved to the right in order to cover better-off households. This effect is relevant in programs like *Oportunidades* where the evidence of positive impacts increases the probability of program expansions.[13]

Matching individuals in treatment and control communities based on "pre-program characteristics" coming from retrospective information, Duarte Gómez et al. (2005) find a zero average treatment effect on the treated on smoking. This effect is the same as the LATE estimated in this paper. Consequently, the no effect of *Oportunidades* on smoking generalizes to the participant population.

Previous research regarding the average treatment effect on the smoking behaviors of participant youth is less conclusive. Gutiérrez et al. (2005) find no difference in the smoking rates of short-term (up to 3 years) participants and those of non-participants. Duarte Gómez et al. (2005) find that the smoking rates of short-term program participants were 26 percent lower than those of long-term (up to 5.5 years) participants. Combining this evidence suggests that long-term participation in *Oportunidades* increased the smoking rates of the average adolescent participant. However, Gutiérrez et al. (2005) find the average long-term effect on adolescent current smoking to be not statistically different from zero. The long-term local average treatment effects documented in this paper are also not statistically different from zero. Nonetheless, the point estimate is positive and less precisely estimated.

What processes were involved in causing this non-result? As program benefits where simultaneously given, disentangling each of the effects is possible to the extent that similar people received heterogeneous program benefits. For instance, suppose that eligible households were randomly assigned to two groups. Now suppose that both groups received cash transfers

[13] See De la Torre (2005) for the latest summary of these effects.

but only one was required to go to the information sessions. Applying the logic of difference-in-difference quasi-experimental estimators, program effect differences between these two groups could be causally attributed to the information sessions.

Differential treatments between women and men in *Oportunidades* can be used to isolate the income effect on smoking participation among adults. Households in *Oportunidades* were given cash transfers, but only women were required to attend health information sessions (Table 2). Therefore, program effects can be interpreted as income effects among men, and income-information effects among women. Moreover, taking the difference between the program impact effects of men and women participating sheds light on the magnitude of the information effect. This exercise is valid if (a) there are no reasons to believe that poor men and women in rural Mexico adjust their smoking behaviors differently to income and information shocks, and (b) there are no spillover effects of information. For the same reasons, differences in adolescent and women program effects can be interpreted as a schooling effect. However, women might be a poor counterfactual for adolescents given that these populations are at different points of their life cycle and economic incentives are expected to affect their smoking behaviors differently.

Table 8 reports the effects of *Oportunidades* on current smoking for men and women. The Schwarz preferred program impacts include only first order polynomials and are presented in column 1. The point estimates of the effect of *Oportunidades* on smoking among men, which can be interpreted as an income effect, are not statistically different from zero. Nonetheless, the standard errors imply an effect between +/- 10 percentage points, approximately. Program effects among women are also non significant and they are more precisely estimated. These conclusions are nearly insensitive to changes in model specification (second column) or the inclusion of covariates (third column). Furthermore, based on Appendix Figure A2 I fail to reject the null hypothesis of no discontinuity in the density of the poverty score for men and women. This evidence in favor of randomization at the threshold by gender suggests that the results in this section are internally valid.

The difference between program effects of men and women imply a zero health information effect. If so, information dissemination policies might not be the way to reduce cigarette smoking in Mexico. This result is interesting, but should be taken with caution for at least two reasons. First, because it assumes that men are indeed a good counterfactual for women. Second, because the lack of precision of the income effect of *Oportunidades* on smoking among men translates in information effects lying anywhere between +/- 12 percentage points.

VII. Conclusions

The goal of the *Oportunidades* program is to eradicate poverty in Mexico through investments in human capital in the form of schooling, nutrition and health. Previous research on the health impacts of this program found that short-term participation increased the utilization of public health clinics for preventive care (Gertler, 2000). Along with medical utilization, economic theory predicts that health outcomes are also determined by health behaviors such as smoking, exercising and eating healthy to avoid obesity (see Grossman, 1972).

This paper used program eligibility as an instrument for participation in *Oportunidades* to estimate the effect of this intervention on adult and adolescent smoking via a fuzzy Regression Discontinuity design. The benefits of the program include sizeable cash transfers, health information sessions and schooling. Based on economic theory predictions and previous empirical findings there were reasons to predict that *Oportunidades* would affect smoking among poor Mexicans through each of these benefits. The findings of this paper suggest, however, a zero local average treatment effect on adults that participated in the program an average of four years. This effect compares to the average treatment effects found in previous literature, suggesting that the impact of the program is homogeneous across the poor. Income, schooling and information jointly did not change smoking among adolescents. Because these estimates were less precisely estimated, the worst case scenario points to a slight increase of smoking among long-term adolescent participants.

Because of the differential treatments between men and women in the *Oportunidades* program, the income effect was isolated by estimating the program's impact on adult male smoking. The point estimate of the effect of *Oportunidades* on smoking among men was not statistically different from zero. Disappointingly, it was not precisely estimated. Therefore, it is not conclusive of a null income effect on smoking. The analysis by gender, however, indicated that health and income combined did not have an effect on the smoking behaviors of participant women.

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Table 1. Schooling Subsidy Amounts by Grade and Gender Handed to the Mother of Households in *Oportunidades* with Children Attending School (pesos per month in the second semester of 2002*).

Educational Level and Grade	Gender of Child who Attends School and Participates in *Oportunidades*	
	Boy	Girl
Elementary School:		
Grade 3	100	100
Grade 4	115	115
Grade 5	150	150
Grade 6	200	200
Middle School:		
Grade 7	290	310
Grade 8	310	340
Grade 9	325	375
High School:		
Grade 10	490	565
Grade 11	525	600
Grade 12	555	635

*Notes: *In 2002, 10 pesos were approximately equivalent to 1 US dollar.*

Schooling subsidies are given to poor eligible families with children in subsidy-eligible grade levels. To receive these subsidy children must attend school at least 85 percent of the days in a given month. In addition to schooling subsidies, participant households receive a food subsidy of about $15 dollars (in 2002). The food subsidy is given conditional on (a) regular health clinic attendance by family members, and (b) monthly attendance to health information sessions by the mother and adolescents in high school.

Source: Oportunidades Program Webpage: http://www.Oportunidades.gob.mx/informacion_general/main_ma.html

Table 2. Mechanisms Behind, and Sign of, the Potential Effect of Participation in *Oportunidades* on Smoking based on Differential Treatments among Adult Men, Adult Women and Adolescents.

Benefits of the Program	Mechanism behind a Potential Impact of the Program on Smoking	Expected sign of the Effect of Each Mechanism and of the Overall Effect for:		
		Adult Men	Adult Women	Adolescents
Food and schooling subsidies	1. Income effect	+	+	+
Health sessions providing information on addiction prevention	1. Allocative efficiency: Information makes people choose healthier behaviors.		-	-
Schooling	1. Allocative efficiency: Information at school makes people choose healthier behaviors more efficiency 2. Productive efficiency: schooling enables people choose healthier behaviors with the same amount of inputs.			-
Overall expected effect (in the absence of peer effects):		+	?	?

Table 3. Descriptive Statistics by Participation Status in *Oportunidades* for the Sample of Adults and Adolescents.

Variables	Adults			Adolescents		
	All	In *Oportunidades* ?: No	Yes	All	In *Oportunidades* ?: No	Yes
Panel A. Dependent Variables Measured in 2003						
Smoking rate	0.05	0.05	0.05	0.08	0.09	0.07
	(0.004)	(0.007)	(0.005)	(0.006)	(0.011)	(0.007)
Participation in *Oportunidades*	0.67	0	1	0.70	0	1
	(0.008)	(0)	(0)	(0.010)	(0)	(0)
Panel B. Demographic Variables Measured in 2003						
Female	0.73	0.73	0.73	0.53	0.55	0.53
	(0.008)	(0.013)	(0.009)	(0.011)	(0.020)	(0.013)
Age	40.73	42.04	40.08	17.02	17.24	16.93
	(0.214)	(0.429)	(0.239)	(0.037)	(0.071)	(0.044)
Married	0.89	0.85	0.92	n.a.	n.a.	n.a.
	(0.005)	(0.011)	(0.006)	(-)	(-)	(-)
Single	n.a.	n.a.	n.a.	0.88	0.85	0.89
	(-)	(-)	(-)	(0.007)	(0.014)	(0.008)
Indigenous	0.44	0.34	0.48	0.35	0.28	0.38
	(0.009)	(0.014)	(0.011)	(0.010)	(0.018)	(0.013)
Panel C. Covariates Associated with Smoking Measured in 2003						
No. years since started smoking	6.28	7.24	5.81	0.71	0.90	0.63
	(0.229)	(0.447)	(0.261)	(0.034)	(0.070)	(0.039)
Tobacco control laws	0.19	0.26	0.15	0.18	0.27	0.14
	(0.007)	(0.013)	(0.008)	(0.008)	(0.018)	(0.009)
Cigarette price (in pesos*)	12.27	12.28	12.27	12.23	12.26	12.21
	(0.015)	(0.025)	(0.018)	(0.020)	(0.034)	(0.024)
Panel D. Other Covariates Measured in 1997						
Social Program: Despensa	0.14	0.13	0.14	n.a.	n.a.	n.a.
	(0.006)	(0.011)	(0.007)	(-)	(-)	(-)
Social Program: Niños de Solidaridad	n.a.	n.a.	n.a.	0.20	0.17	0.21
	(-)	(-)	(-)	(0.009)	(0.016)	(0.011)
Land Property	0.63	0.69	0.61	0.68	0.71	0.66
	(0.009)	(0.015)	(0.011)	(0.011)	(0.020)	(0.013)
Cigarette price (in pesos*)	11.70	11.73	11.69	11.66	11.72	11.63
	(0.023)	(0.040)	(0.029)	(0.032)	(0.058)	(0.038)
Number of Observations	3370	1108	2262	2070	627	1443

*Notes: *One US dollar was approximately equivalent to 10.5 pesos in 2003 and 8 pesos in 1997.*

Niños de Solidaridad was a social program consisting on grants given to the children in isolated and marginalized communities to finish their elementary education. Despensas is a program that provides a monthly package of basic food products to very poor families for up to a year.

Standard errors are reported in parentheses.

Source: Own calculations based on ENCASEH 1997 and ENCEL 2003.

Table 4. Identification of Program Effects: Discontinuity Estimates of Participation in *Oportunidades* at the Poverty Score Eligibility Cutoff for Adults and Adolescents.

Polynomial order on both sides of the eligibility cutoff	Discontinuity Estimates of Participation at Cutoff for:	
	Adults	Adolescents
1	0.536[&]	0.536[&]
	(0.028)	(0.034)
2 right, 1 left*	0.458[‡]	0.454[‡]
	(0.037)	(0.044)
2	0.449	0.457
	(0.037)	(0.045)
3	0.408	0.407
	(0.047)	(0.058)
4	0.403	0.386
	(0.056)	(0.069)
5	0.414	0.380
	(0.066)	(0.083)
6	0.399	0.360
	(0.076)	(0.097)
7	0.376	0.351
	(0.085)	(0.113)
Number of Observations	3370	2070

*Notes: *This regression is estimated using a quadratic parametrization to the right of the cutoff for eligibility and a linear to the left.*

& are the preferred estimates based on Schwarz (1978).

‡ are the second best estimates based on Schwarz (1978).

Each cell comes from the first stage regression of the two-stage least squares (TSLS) estimation. In particular, each entry represents π_0 in equation 8 in the text. This is the dummy for eligibility coefficient of a flexible parametric regression of program participation on eligibility that also includes interactions of the dummy for eligibility and polynomial terms of the poverty score that defined eligibility. The order of the polynomial terms is specified in the first column of the table. Except for the estimates in the second row of the table, I use the same polynomial order to the right and to the left of the cutoff.

None of the regressions include covariates. Clustered standard errors are reported in parentheses.

Table 5. Effect of Eligibility on Program Participation and Smoking and Effect of Participation in *Oportunidades* on Smoking for Adults and Adolescents.
(Estimations based on the preferred flexible parametric specification)**

Estimates for:	Effect of:			Number of Observations
	Eligibility on participation Π_0*	Eligibility on smoking Π_1&	Participation on smoking $\theta = \Pi_1 / \Pi_0$‡	
Adults	0.536 (0.028)	-0.003 (0.011)	-0.005 (0.021)	3370
Adolescents	0.536 (0.034)	0.014 (0.019)	0.026 (0.036)	2070

*Notes: ** The preferred estimates are based on the Schwarz (1978) criterion. They are estimated using first order polynomials interacted with a dummy for eligibility to Oportunidades on both sides of the eligibility cutoff.*

** The effect of eligibility on participation corresponds to* π_0 *in equation (8) in the text. This is the coefficient of the dummy for eligibility coming from the First Stage of the TSLS regression of program participation.*

& The effect of eligibility on smoking corresponds to π_1 *in equation (7) in the text. That is the coefficient on the dummy for eligibility coming from a reduced form equation of smoking.*

‡ The effect of participation in Oportunidades on smoking is the local average treatment effect (LATE): $\theta = \pi_1/\pi_0$ *in equation (4) in the text. This is the participation coefficient of the second stage of the TSLS.*

None of the regressions include covariates. Clustered standard errors are reported in parentheses.

Table 6. Program Participation on Smoking for Adults and Adolescents: Sensitivity Estimates.
(Estimations based on the second and third best parametric specifications)**

Estimates for:	Polynomial order: First order to the left of the cutoff and second to the right	Polynomial order: Second order on both sides of the cutoff	Number of observations
Adults	0.004	0.076	3370
	(0.028)	(0.050)	
Adolescents	-0.008	0.094	2070
	(0.033)	(0.060)	

*Notes: ** Based on Schwarz (1978), the second best specification includes a first order polynomial term to the left and to the right of the cutoff and a second order polynomial term only to the right of the cutoff (see column 2). The third best specification includes second order polynomial terms on both sides (see column 3).*

All entries are local average treatment effects (LATE) of participation in Oportunidades an average of four years on smoking. These estimations come from the Second Stage of the TSLS regression and represent $\theta = \pi_1/\pi_0$ *in equation (4) in the text.*

None of the regressions include covariates. Clustered standard errors are reported in parentheses.

Table 7. Assessment of the Validity of the RD Design in the Sample of Adults and Adolescents: Discontinuity Estimates.

	Estimates for:	
	Adults	**Adolescents**
Panel A. Log Discontinuity		
Poverty Score that determined program eligibility	-0.043	-0.277
	(0.087)	(0.102)
Panel B. Discontinuity Estimates of Preset characteristics		
B.1 Correlated with smoking		
Female	0.006	-0.040
	(0.026)	(0.038)
Age	-2.108	0.013
	(0.829)	(0.120)
Literate	0.015	-0.018
	(0.027)	(0.017)
Married	0.036	n.a.
	(0.019)	(-)
Single	n.a.	0.001
	(-)	(0.007)
Indigenous	-0.026	-0.062
	(0.034)	(0.044)
Cigarette prices	-0.046	0.099
	(0.092)	(0.140)
B.2 Correlated with baseline poverty		
Despensa	-0.043	n.a.
	(0.025)	(-)
Niños de Solidaridad	n.a.	-0.025
	(-)	(0.039)
Land property	-0.011	-0.006
	(0.034)	(0.042)
Panel C. Discontinuity Estimates based on all available baseline covariates and excluding participation		
Effect of eligibility on Participation	0.005	0.000
	(0.012)	(0.019)
Panel D. Discontinuity estimates including covariates		
Effect of Program Participation on Smoking	-0.005	0.005
	(0.020)	(0.032)

Notes: The entries in Panels A, B and C represent the coefficient on the eligibility dummy of a regression similar to equation 6 in the text. It includes first order polynomial interaction terms of the poverty scores and the dummy for eligibility on both sides of the cutoff. Clustered standard errors of that coefficient are in parentheses.

*Panel **A** reports discontinuity estimates from the test proposed by McCrary (2008, 703).*

*The discontinuities in Panel **B** are for preset characteristics measured in 1997 from the ENCASEH-97. For this set of regressions estimates with up to second order polynomial terms are very similar and are available from the author upon request. Cigarette prices are state-level prices in 1997 published by the Central Bank of Mexico. See the notes in Table 3 for the definition of Niños de Solidaridad and Despensas.*

*Panel **C** includes age, gender, marital status, years of education, and cigarette prices measured in 1997. There were no tobacco control laws in place at that time.*

*Panel **D** reports the effect of participation in Oportunidades on smoking. This is the local average treatment effect (LATE): $\theta = \pi_1/\pi_0$ in equation (4) in the text, which corresponds to the participation coefficient of the second stage of the TSLS. The covariates included in this regression are age, gender, marital status, years of education, cigarette prices and state clean indoor air policies as of 2003.*

Table 8. Isolating the Income Effect of the Program:
Effect of Participation in *Oportunidades* on Smoking by Gender.
(Estimations based on the preferred and second best flexible parametric specifications)**

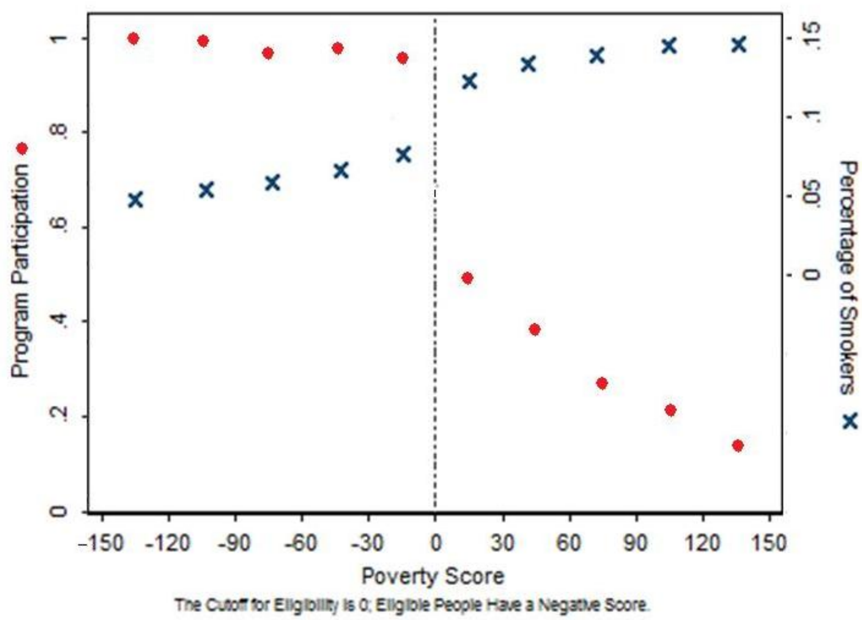
Estimates for:	**Polynomial order:** First order on both sides of the cutoff	First order to the left of the cutoff and second to the right	First order on both sides of the cutoff	**Number of observations**
Men	-0.011	0.014	-0.027	906
	(.063)	(.082)	(0.063)	
Women	-0.002	-0.001	-0.002	2406
	(0.011)	(0.013)	(0.011)	
Regression includes Covariates?	No	No	Yes	-

*Notes: ** Based on Schwarz (1978), the first best specification includes first order polynomial terms on both sides of the cutoff for eligibility (see columns 2 and 4). The second best specification includes first order a first order polynomial term to the left and to the right of the cutoff and a second order polynomial term only to the right of the cutoff (see column 3).*

All entries are local average treatment effects of participation in Oportunidades an average of four years on smoking: $\theta = \pi_1/\pi_0$ *in equation (4) in the text. They correspond to the participation coefficient of the second stage of the TSLS.*

Only the regressions in the fourth column include covariates. Clustered standard errors are reported in parentheses.

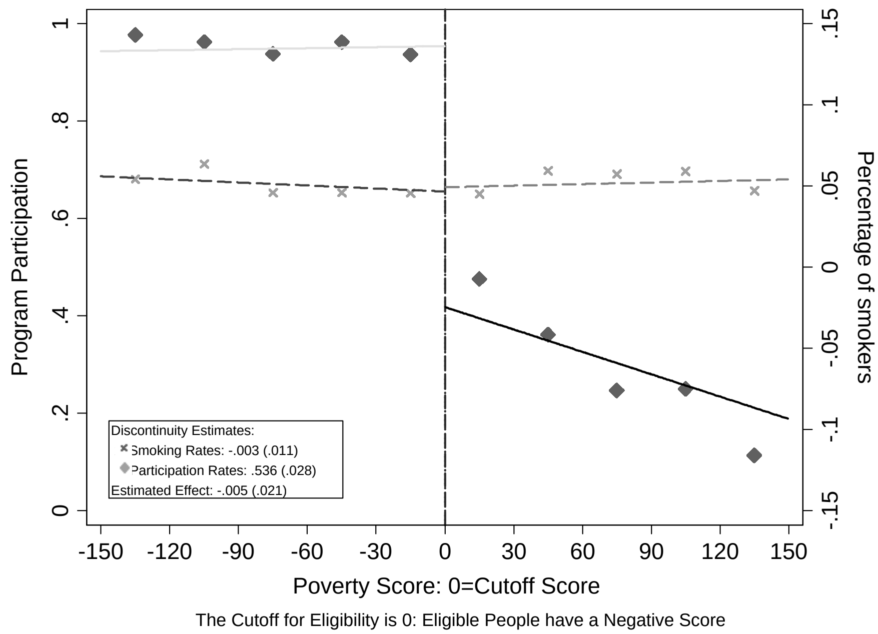
Figure 1. Raw Averages of Program Participation and Smoking for Adults as a Function of the Poverty Scores that Determined Eligibility.
(The individuals in this sample live in comunities where *Oportunidades* started operating in 1998)



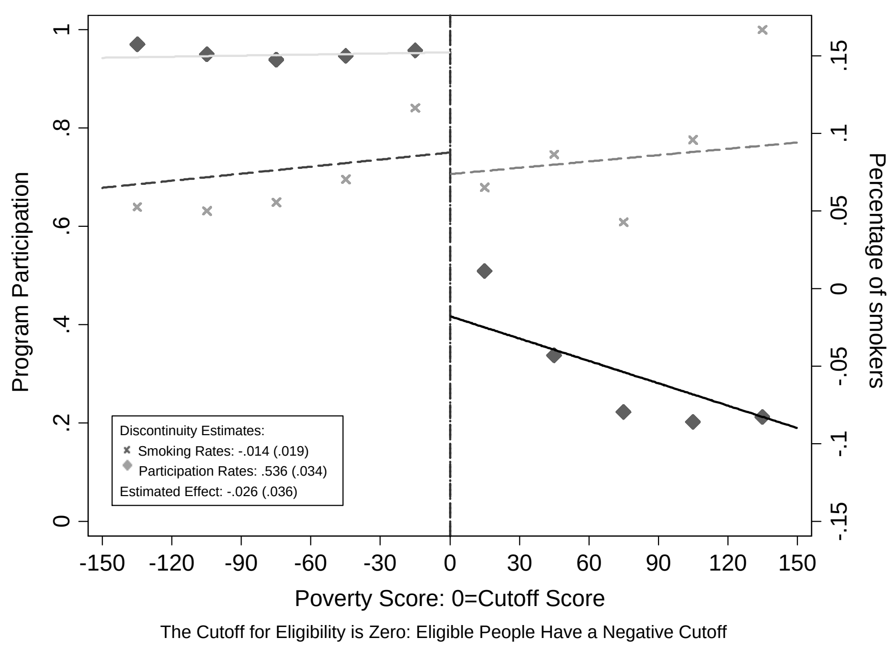
Notes: Average program participation and smoking participation rates are plotted as a function of five categories of the poverty score on each side of the eligibility cutoff. Participation rates are based on true data, but smoking rates are hypothetical.

Figure 2. Impact of Eligibility on Program Participation and Smoking and Effect of Program Participation on Smoking

Panel A. Discontinuity Estimates for Adults



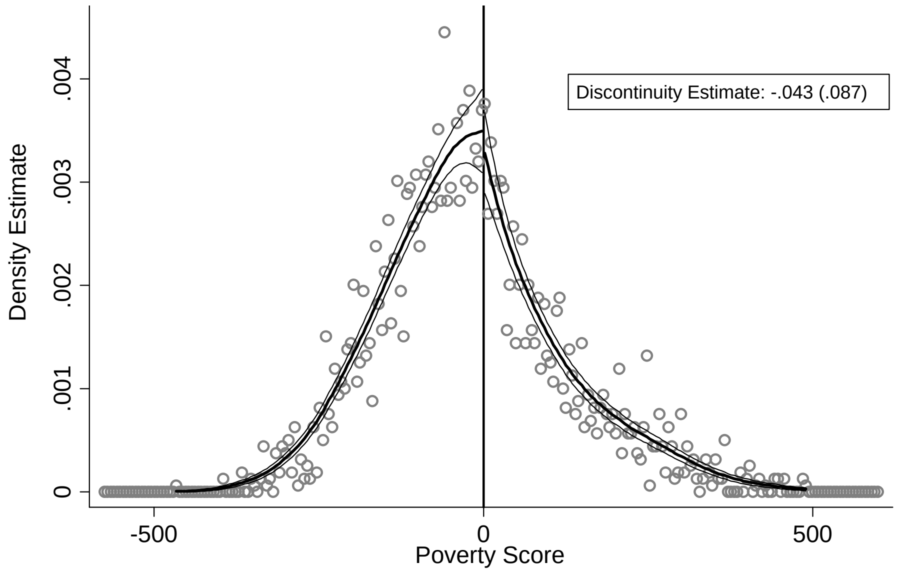
Panel B. Discontinuity Estimates for Adolescents



Notes: Average program participation (marked with) and smoking participation rates (marked with) are plotted as a function of five categories of the poverty score on each side of the eligibility cutoff. The lines are conditional expectations of specifications as in equations (7) and (8) in the text. They are estimated using the whole range of data with poverty scores of +/- 500. The first order polynomial approximation presented here is the Schwarz (1978) preferred specification for both Adults and Adolescents.

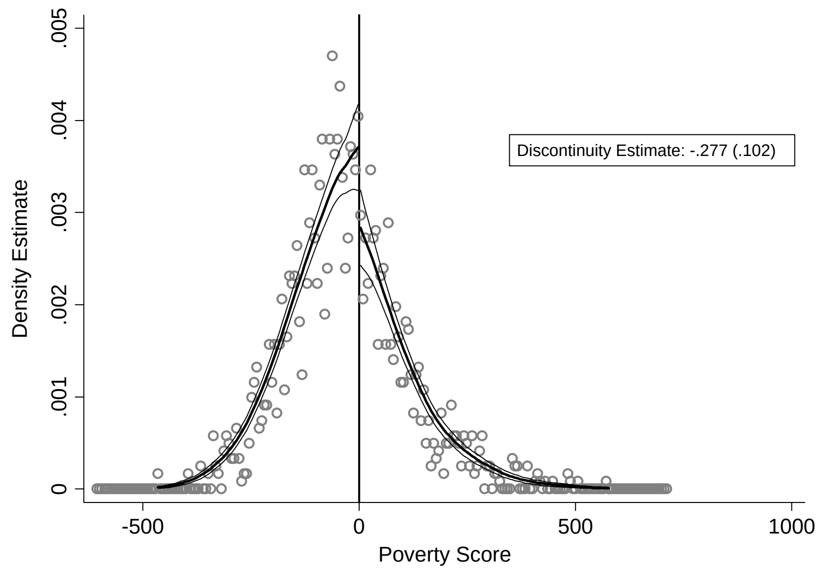
Figure 3. Density and Confidence Intervals of the Poverty Scores that Determined Program Eligibility for Adults and Adolescents: A test of the Validity of the RD Design.

Panel A. Discontinuity Estimates for Adults.



The Cutoff for Eligibility is 0; Eligible People Have a Negative Score.

Panel B. Discontinuity Estimates for Adolescents.



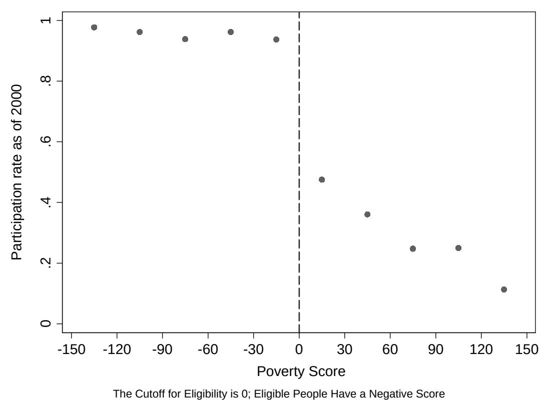
The Cutoff for Eligibility is 0; Eligible People Have a Negative Score.

Notes: This figures provide discontinuity estimates from the test proposed by McCrary (2008, 703).

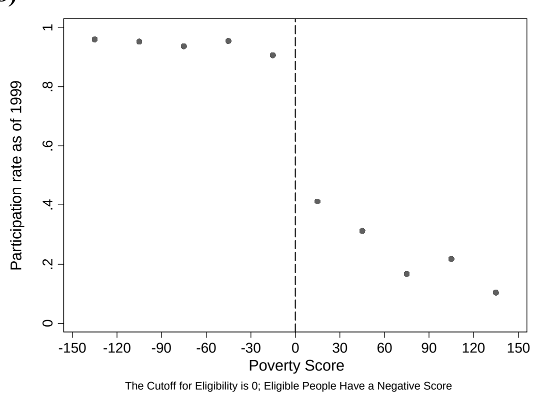
Standard errors are reported in parentheses.

Figure A1. Average Participation Rates Over Time in Different Communities as a Function of the Poverty Scores that Determined Program Eligibility.

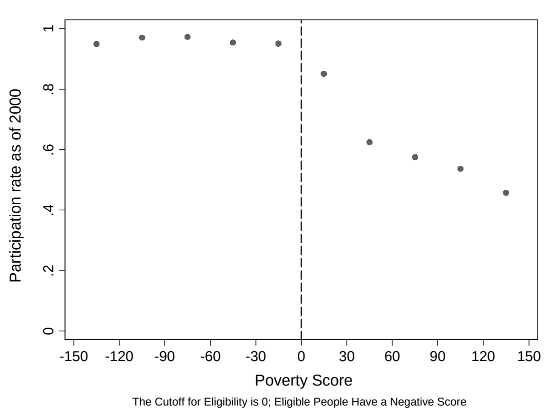
Panel A. Program Participation Rates in 2000 in Communities where the Program Started in 1998 (treatment-98)



Panel B. Program Participation Rates in 1999 in Communities where the Program Started in 1998 (treatment-98)



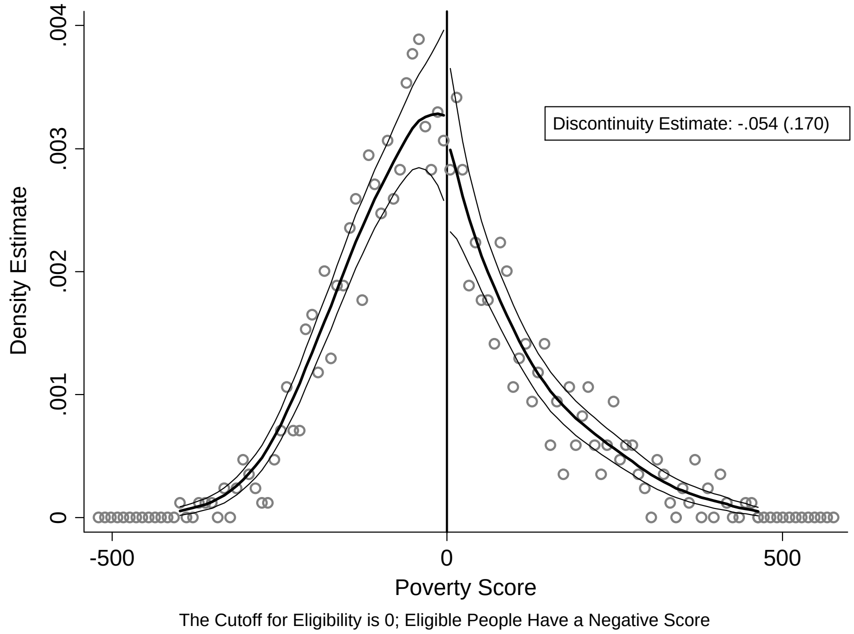
Panel C. Program Participation Rates in 2000 in Communities where the Program Started in 1999 (treatment-99)



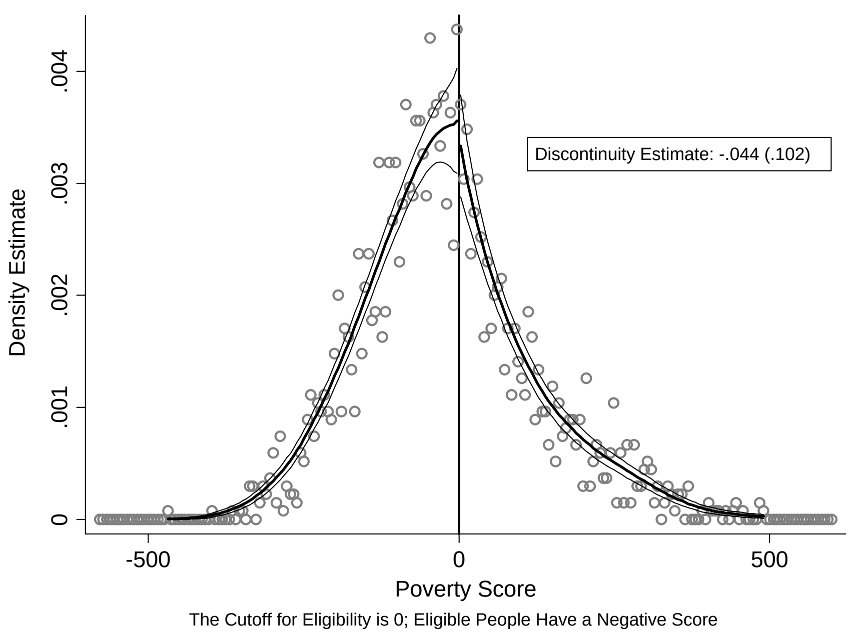
Notes: Average program participation rates are plotted as a function of five categories of the poverty score on each side of the eligibility cutoff.

Figure A2. Density and Confidence Intervals of the Poverty Scores that Determined Program Eligibility by Gender: A test of the Validity of the RD Design.

Panel A. Discontinuity Estimates for Men



Panel B. Discontinuity Estimates for Women



Notes: This figures provide discontinuity estimates from the test proposed by McCrary (2008, 703).

Standard errors are reported in parentheses.