Can Social Programs be Reliably Evaluated with NonExperimental Methods? Evidence on the Performance of Regression Discontinuity Design using PROGRESA data

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Abstract

In 1997 a social program called PROGRESA was introduced in Mexico. One of its goals was to increase schooling levels in rural areas by means of educational grants paid out to the mother. In order to evaluate the effects of PROGRESA the program was introduced using a design for a randomized experiment. In this paper we exploit the build in, but so far neglected, discontinuity in the eligibility rule that states you are eligible if you have a poverty index below a particular threshold value. We use the quasi-experimental Regression-Discontinuity design in order to estimate marginal average treatment effects. This marginal effect is precisely the parameter of interest in determining the effects of relaxing the eligibility criterium for PROGRESA through a raising of the threshold value. Our findings show substantial regional variation in treatment effects. Moreover, given that the RDD approach allows us to use only data from the treated sample, we are able to investigate the extend to which the introduction of the program had an effect on ineligible children in the localities it was introduced.

Keywords: Treatment effects, Regression Discontinuity, PROGRESA
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1 Introduction

In order to break the cycle of poverty and low human capital accumulation the Mexican government introduced a social program in the summer of 1997 named PROGRESA (Programa de Educación, Salud y Alimentación). The goal of the program was to alleviate current poverty and increase investment in human capital, health and nutrition by providing cash transfers, in-kind health benefits and nutritional supplements. In order to be eligible for these transfers the children needed to adhere to minimum schooling attendance rates and scheduled visits to the health clinic. The household also needed to be sufficiently poor, where the level of poverty was expressed by an index. The program was targeted towards the very poor in rural areas to maximize the impact on current poverty for a given program budget. Because of budgetary constraints, the program was introduced in phases. The necessity to introduce the program in phases was capitalized upon by phasing in the program along the lines of a randomized experiment where localities were either randomized to be in (treatment localities) or out (control localities). The resulting experimental data was used to evaluate program impacts regarding different outcomes related to schooling, health and nutrition[1]. With more and more Mexican families being covered by the program's horizontal expansion, the question arises what the impact would be of a vertical expansion, in particular a relaxing of the eligibility criteria along the household's poverty index. To answer this question we exploit the known but so far unused feature embedded in the eligibility rule by using the Regression Discontinuity Design (RDD). Different statistical tools to identify treatment effects are available, but the RDD approach is well suited to investigate this policy relevant question of what would happen if we were to relax eligibility criteria, precisely because it estimates local, or more accurately, marginal average treatment effects. Moreover, by not having to rely on the data collected for the control localities we can investigate to what extend it makes a difference if we limit ourselves to people within the sample of treated localities rather than compare people across the pool of treated and control localities, as is the case in the pure experimental approach. The paper is organized as follows. Section 2 describes the features of the program and the data collection process. Section 3 and 4 outline the RDD approach and alternative evaluation methods within a regression framework, respectively. Our findings are discussed in section 5. Section 6 concludes.

[1] See Skoufias (2001) for an extensive synthesis of all the available results.

2 Outline of the program and data collection

Due to budgetary constraints PROGRESA could not be implemented on a national scale from the start. The phased introduction of the program started in the summer of 1997 with phase I when roughly 140,000 households in over 3,000 localities were incorporated. By the end of phase 11 in the spring of 2000 the program included nearly 2.6 million families in over 72,000 localities[2]. To put these numbers in perspective, the number of people covered by the program can be expressed as approximately 40% of all rural families and about one ninth of all families in Mexico. In 1999 its costs consumed 0.2% of GDP. These numbers make clear that the program is a major component in the government's poverty relief effort. PROGRESA also meant a deviation from earlier practices where poverty relief programs often were not targeted (e.g. the tortilla subsidies) or deemed very susceptible to local influence. The program was also designed to simultaneously address education, health and nutrition and thus explicitly underscores the notion that poor health and/or nutrition can seriously affect school performance and could potentially hamper the intended educational improvements set out by the program. Since the prior of the Mexican government is that mothers know best, the benefits of the program were only paid out to mothers. The introduction of PROGRESA entailed a two step selection process. The first step was to select localities based on a marginality index computed using census data. Once a locality was deemed sufficiently deprived eligible household within the locality were then identified using a poverty index calculated using socio-economic data collected for all households in the locality. For the purpose of evaluating the program impacts 506 localities were sampled and split into 320 localities being randomized into the program (the treatment localities) and 186 being randomized out (the control localities). The 506 localities constitute 24,077 households.

2.1 The educational component of PROGRESA

PROGRESA aims to increase school enrollment, improve school attendance and raise school performance primarily by a system of educational grants and monetary support for the acquisition of school materials[3]. These grants increase by grade, both in primary and secondary school, to account for the increased opportunity costs of staying in school. For secondary school grades

[2] Table 5 contains an overview of the expansion of PROGRESA over time.

[3] See Table 6 for a description of the cash benefits.

girls receive a higher grant than boys to address the lower enrollment rates in secondary schools for girls relative to boys in the localities targeted by the program. The cash benefits also increase over time due to the biannual inflation adjustment, ensuring a stable real value of the cash benefits. To not completely erode the incentives to improve living standards through own initiative, the total amount of cash benefits a household can receive is capped. All children over 7 and under 18 years of age are eligible and cash benefits are paid out bimonthly during the school calendar. Two thirds of the cash benefits for school supplies are paid out at the start of the school year. The remaining benefit for school supplies is paid out half way through the school year. As noted before, benefits are exclusively paid out to the mother. To be eligible for these cash benefits the child needs to have a minimum school attendance rate of 85 % measured both monthly and annually. When attendance rates drop below 85% benefits will be revoked temporarily but eventually permanently. To provide a sense of the magnitude of the mean cash transfers in 1999 they can be expressed as constituting roughly one fifth of the mean value of consumption of poor households in the control localities. The average amount of benefits from the educational grants roughly equalled the average amount from the grants for food, although households with older heads of household receive a larger share of their cash benefits from grants for foods as they are less likely to have school aged children[4].

2.2 The health and nutrition components of PROGRESA

The health and nutrition component of the program consists primarily of a basic package of primary health care services but this is not the only component of the health and nutrition aspect. Families and communities also receive health and nutrition information and there are special nutritional supplements for pregnant women and breast feeding mothers. Young children also receive nutritional supplements. The health and nutrition component is mainly focussed on pre- and post-natal care. Regular scheduled and attended visits to the clinics are the crux and records that are being kept are used as proof of registration in order to receive the cash grants for food. If any one household member does not comply with the scheduled visits then the household is considered not to have complied and will not be entitled

[4]These calculations include households that did not receive any benefits due to non-compliance with the program requirements, or because of delays in the verification of the requirements or the delivery of the benefits.

to receive food support[5]. The health and nutrition information offered to families and communities takes place in the form of instructional meetings with a strong emphasis on preventive care. A large number of topics ranging from infectious diseases to family planning are also discussed.

2.3 Data collection

The targeting of PROGRESA takes place on two levels. Using census data (ENCASEH) a marginality index is computed to identify eligible localities based on empirical measures of poverty within the locality. Next, a subset of 506 localities was selected for the randomized social experiment. Using consecutive survey data (ENCEL) collected for each household in each of these localities a poverty index (*puntaje*) using poverty-related criteria was calculated for each household. This poverty index was then used to classify a household as poor and eligible to receive PROGRESA services, or non poor and ineligible for PROGRESA services. Finally, each of the 506 localities was then randomized into either the treatment or the control group. Randomization was implemented at the locality level because randomization at the household level *within* the same small rural locality would be problematic for obvious reasons. None of the eligible households living in the control localities received PROGRESA services. In the treatment localities all eligible households were offered PROGRESA services. Behrman and Todd (1999b) investigate the extend to which the treatment and control localities have different distributions for the observed characteristics. In terms of age, education, access to health care and income their findings show that the treatment and control groups do not indicate any systematic differences when based on locality means, which is to be expected as randomization was done at the locality level. When based on household level data, some statistically significant differences between the groups were found.

Data collected for the experiment consisted of the pre-program census survey (ENCASEH) in November 1997 and the pre-program household survey (ENCEL) in March 1998. Post-program household surveys (ENCEL) were held in all of the treatment and control localities[6]. Although very detailed household information was collected, the RDD approach identifies treatment

[5] Table 7 outlines the required visits to the health clinic. For individuals over 17 years of age the required number of visits is only once a year hence these requirements are almost always met.

[6] Table 4 displays the timing of the surveys and the amount of coverage in terms of households and individuals over time. Table 5 describes the overall expansion of PRO-

effects only of the discontinuity in the eligibility rule. For our analysis we thus only need information on the outcome measures (school enrollment and work incidence) and the qualifying threshold poverty scores that vary by region. The data that we have to our disposal consists of data collected for all households that were initially surveyed in October/November 1997 (Round 1). This is our pre-program data. The post-program data consists of 3 surveys that took place after the initiation of payments in July 1998. These are October 1998 (Round 3), June 1999 (Round 4) and November 1999 (Round 5). After that last survey round, the benefits of the program started to be distributed in the control communities.

GRESA over time.

3 Outlining the RDD approach

The RDD approach, which has originally been introduced by Thistlethwaite and Campbell (1960), exploits discontinuities in the probability of receiving treatment to identify treatment effects. The RDD approach has recently been used, for example, by Van der Klaauw (2001) to investigate the effects of financial aid on college enrollment, Black (1999) to evaluate the effect of elementary school quality on housing prices, Angrist and Lavy (1999) to identify the effect of class size on schooling attainment, Hahn et al (1999) to analyze the effect of an anti-discrimination law on minority employment in small US firms, by DiNardo and Lee (2002) to estimate the effect of unionization on establishment closure, and by Lee (2001) to analyze whether political incumbency provides an advantage in elections.

The RDD approach used in this paper is based on a discontinuity in the eligibility criterion. In order to be eligible for PROGRESA services one needs to have a poverty index that is sufficiently low. The localities for which the data was collected were grouped into seven broad geographical regions. For each region a separate discriminant analysis was performed to calculate this poverty index which resulted in a situation were different regions have different threshold scores to determine if one is eligible or not. This implies that for our purpose any application of the RDD approach will be conditional on region. The crux of the RDD approach is that one compares people just below and above the threshold score.

Suppose that school enrollment of a child is determined by the equation:

$$Y_i = \alpha_i + \beta T(S_i) + \epsilon_i, \tag{1}$$

where Y_i denotes enrollment of child i, and T_i is the treatment indicator that equals one if child i is eligible for PROGRESA services and zero otherwise. In our case, treatment depends on S_i, the poverty score. Of course, if one would only be interested in the program effects for those who are treated, the randomized experiment setup to evaluate PROGRESA would allow us to easily compare mean enrollment rates for eligible children in treatment and control localities. But by having experimental data and the discontinuity in eligibility we are able to investigate the performance of the RDD estimator. Moreover, since randomization was done at the locality level and Behrman and Todd (1999b) found some statistically significant differences between the groups when the unit of observation was at the household level, the RDD estimates could give an insight into the importance of their findings.

The RDD literature distinguishes between the so-called sharp and fuzzy designs. Let S_i denote the poverty index. If treatment assignment would be a stochastic function of S_i we would speak of a fuzzy design. Our case is one of a sharp design as treatment T_i is known to depend on S_i in a deterministic way[7]. Denoting the (region specific) threshold score by COS, we know families with a poverty score above COS are excluded from receiving PROGRESA services. The RDD approach uses the postulation that individuals with a poverty score just below the threshold score are similar in their observed and unobserved characteristics to individuals who have a poverty score just above the threshold score. Comparing a sample of individuals within a very small range around the threshold score will be similar to a randomized experiment at the threshold score. This is why the RDD approach is often referred to as a quasi-experimental design. For children with a poverty index around the threshold score it could be expected that

$$E[\alpha_i|S_i = COS + \Delta] \cong E[\alpha_i|S_i = COS - \Delta]$$

where Δ denotes an arbitrarily small number.

To see this, assume that $E[\alpha_i|S_i = S]$ and the conditional mean function $E[\epsilon_i|S]$ are continuous at COS. Assuming further that the treatment effect is constant across different individuals, it can be shown that the average treatment effect is identified by[8]:

$$\beta = lim_{S\uparrow COS}\left(E[Y_i|S_i = S]\right) \; - lim_{S\downarrow COS}\left(E[Y_i|S_i = S]\right) \tag{2}$$

When we allow the treatment effect to be heterogeneous across individuals (2) will identify the marginal treatment effect for the subgroup of individuals for whom eligibility changes discontinuously at COS.

We estimate $lim_{S\uparrow COS}\left(E[Y_i|S_i = S]\right)$ and $lim_{S\downarrow COS}\left(E[Y_i|S_i = S]\right)$ non-parametrically by using one-sided Kernel regressions, thereby following Hahn et al (2001) who show that this procedure is numerically equivalent to a local

[7] See for instance Van der Klaauw (2001) and Hahn et al (2001) for a detailed discussion on the sharp and fuzzy designs.

[8] See Van der Klaauw (2001) and Hahn et al (2001). In the case of a sharp design, $E[\epsilon|T, S] = E[\epsilon|S]$. In other words, S will capture any correlation between T and ϵ since S is the only systematic determinant of treatment status. The treatment parameter β could thus also be consistently estimated by the equation:

$$Y_i = \alpha + \beta T_i + c(S_i) + \nu_i,$$

where $c(S_i)$ is a control function that is continuous in S and represents a specification for $E[\epsilon|S]$. Typically $c(S_i)$ is specified as a higher order polynomial.

Wald estimator under certain conditions. Because of the poor boundary performance of standard kernel estimators we also explore local linear regressions (LLR) as suggested by Fan (1992)[9].

[9] Our results showed very similar estimates based on LLR versus RDD and hence these are not reported.

4 Outlining alternative evaluation methods within the regression framework

As an alternative to the RDD approach outlined above, one could also revert to a regression analysis to obtain a consistent and unbiased estimate of the effect of the program on individuals eligible for the program[10]. Restricting the sample to eligible households only (E=1), various estimators of program effects that control for observed individual, household and locality characteristics can be obtained by estimating a regressions equation of the form

$$Y(i,t) = \alpha + \beta_T T(i) + \beta_R R2 + \beta_{TR}(T(i) * R2) + \sum\nolimits_j \theta_j X_j + \eta(i,v,t)$$

where in our case Y(i,t) denotes the binary outcome indicator for individual i in period t, α, β, γ and θ are fixed parameters to be estimated, T(i) is a binary variable taking the value of 1 if the household belongs in a treatment community and 0 otherwise (i.e., for control communities), R2 is a binary variable equal to 1 for the second round of the panel (or the round after the initiation of the program) and equal to 0 for the first round (the round before the initiation of the program), X is a vector of household (and possibly village) characteristics and η is an error term summarizing the influence random disturbances[11].

The parameters α and β_T summarize the differences in the conditional mean of the outcome indicator before the start of the program whereas β_R and β_{TR} summarize differences after the start of the program. Specifically, the coefficient β_T allows the conditional mean of the outcome indicator to differ between eligible households in treatment and control localities before the initiation of the program whereas the rest of the parameters allow the passage of time to have a different effect on households in treatment and control localities. For example, the combination of parameters β_R and β_{TR} allow the differences between eligible households in treatment and control localities to be different after the start of the program.

The conditional mean values of the outcome indicator for treatment and control groups before and after the start of the program are as follows

[10] We are interested in the effect of eligibility and do not address the issue of take up.

[11] More than one round of observations after the start of the program can be easily accommodated by including an additional binary variables (e.g. R3, R4 and R5) along their interactions with the treatment dummy T.

$$[E(Y \mid T=1,\, R2=1,\, X)] = \alpha + \beta_T + \beta_R + \beta_{TR} + \textstyle\sum_j \theta_j\, X_j$$
$$[E(Y \mid T=1,\, R2=0,\, X)] = \alpha + \beta_T + \textstyle\sum_j \theta_j\, X_j$$
$$[E(Y \mid T=0,\, R2=1,\, X)] = \alpha + \beta_R + \textstyle\sum_j \theta_j\, X_j$$
$$[E(Y \mid T=0,\, R2=0,\, X)] = \alpha + \textstyle\sum_j \theta_j\, X_j$$

We can than easily compute the cross-sectional difference estimator (CSDIF) that compares differences in the means of the outcome variable Y between eligible people in the treatment and the control localities for the period(s) after the implementation of the program

$$CSDIF = [E(Y \mid T=1,\, R2=1,\, X) - E(Y \mid T=0,\, R2=1,\, X)] = \beta_T + \beta_{TR}$$

We can also easily compute the before and after estimator (BADIF) that compares differences in the means of the outcome variable Y for eligible people in treatment localities during the periods after and before the implementation of the program

$$BADIF = [E(Y \mid T=1,\, R2=1,\, X) - E(Y \mid T=1,\, R2=0,\, X)] = \beta_R + \beta_{TR}$$

Finally, we can also easily compute the double differences or difference-in-differences estimator (2DIF) that measures program impact by comparing differences in the mean outcomes between eligible people in treatment and eligible people in control localities in post survey rounds, with the differences in the mean outcomes between these groups in the pre program round

$$\begin{aligned} 2DIF = \; & [E(Y \mid T=1,\, R2=1,\, X) - E(Y \mid T=1,\, R2=0,\, X)] - \\ & \quad [E(Y \mid T=0,\, R2=1,\, X) - E(Y \mid T=0,\, R2=0,\, X)] \\ = \; & \beta_{TR} \end{aligned}$$

5 Findings and discussion

Although the RDD estimation methods we employed are known as nonparametric estimators, they do depend on the choice of a kernel and an appropriate bandwidth. Popular choices are the Gaussian kernel and Epanechnikov kernel, among many alternatives. The data at hand allows for several different comparisons. Let us define the group A that consists of those children who have a poverty index below the threshold score (i.e. are eligible) and who live in a locality where PROGRESA has been actually introduced. Similarly, define C as identical children who happened to live in one of the control localities. Finally, let B be those children who live in treatment localities who have a score above the threshold score (i.e. are not eligible) and D be children in the same situation who happened to live in one of the control localities[12]. To identify average treatment effects by exploiting the experimental nature of the data we compare conditional mean outcomes in group A with the conditional mean outcomes in group C. Since pre-program data was collected as well, we also compute a difference in differences estimate. In order to estimate marginal average treatment effects in the same spirit, we could apply the RDD approach using those children in group A and C with a poverty index close to the threshold score, where close is defined by our choice of kernel and bandwidth. However, we need not limit ourselves to only children in group C, as the RDD approach could just as well be applied to children in group A and B only. A third possibility would be to use children in group A, B and C[13]. The RDD approach applied to group A and C or A and B in both cases identifies the same marginal treatment effect and should produce similar results. When the results are very different this would imply the program has some substantial effect on children who are on the brink of having qualified to receive benefits, enough to make them different from their peers in the control localities. Furthermore, if the marginal treatment effects estimated by the RDD approach are very different from the average treatment effects as estimated by difference in differences, this would imply that treatment effects are not constant along the poverty index.

To provide an initial first investigation and to visualize the discontinuity in the eligibility rule we predict individual outcomes for different values of the poverty index using a very simplistic logit specification for our binary

[12] See figure 1 in appendix for a schematic overview of these different groups.

[13] One could run a check by applying the RDD approach to children in groups C and D. As these are only children living in control localities one would expect to find no treatment effects.

outcome variable with a constant, an eligibility dummy and a 4th order polynomial in the poverty index. These are displayed for region 3 to 6, for both the control and treatment localities and for round 1 (Nov97) and for round 3,4 and 5 combined (Oct98, Jun99, Nov99). We do this for both outcome measures, being school enrollment and work incidence[14].

5.1 Results within a conventional regression framework

5.1.1 The effects on school enrollment

Table 9 contains the results from a probit regression as outlined in section 4, using eligible children only. This regression allows us to identify the before-after (BADIF), the cross-sectional difference (CSDIF) and the difference in differences (2DIF) estimator. To do so we need the parameter estimates for the dummy indicating living in a treatment locality (T), the survey round indicators (R) and the interaction of these two (R_T). The other regression variables are used to control for possible confounding factors such as individual, family and village characteristics[15]. One advantage of the regression specification is that the t-values associated with the parameters provide direct tests of a number of hypothesis. For instance, the t-value of β_T provides a direct test of the equality in the conditional mean of the outcome variable, in this case school enrollment, between treatment and control localities before the initiation of the program. In this case it serves as a test for the randomness in selection of localities. With values for the t-statistic of 0.12, 1.21, 0.81 and 0.08 for boys aged 8 to 11, 12 to 17 and girls in the same age group respectively, we do not reject the null hypothesis that the pre-program conditional mean school enrollment rates in treatment and control

[14] To allow easy comparison to the RDD approach outlined in section 3 we use only the observations within a narrow bandwidth of 50 poverty index points around the cut-off score.

The parameter for the eligibility dummy, resulting in the discontinuity in the graph, is only significant at the 5% level for school enrollment in treatment localities located in region 6 in round 1, work incidence in treatment localities in region 3 for round 3,4 and 5 combined and for work incidence in treatment localities in region 5 in round 1. However, the main purpose is to the give a graphical reference for the RDD analysis.

[15] See table 11 for a complete listing of the included regressors and their meaning.

localities are identical. Given the small value of the β_T parameter estimate it also implies that the CSDIF estimate $(\beta_T + \beta_{TR})$ is almost identical to the 2DIF estimate, β_{TR}, except perhaps for 12 to 17 year old boys. One major advantage of the 2DIF over the CSDIF in measuring the mean direct effect of treatment is that the former controls for any pre-existing differences in the expected value of school enrollment between households in control and treatment localities. If mean school enrollment for eligible children would be lower in treatment than in control localities, and the program was effective in raising enrollment to equal levels, than CSDIF would not pick this up whereas 2DIF would. In a reverse situation CSDIF would overestimate the effect. Along similar lines 2DIF is preferred over BADIF as the former is able to yield an estimate of the effect net of any time trends or aggregate effects present in the data. Using 2DIF (β_{TR}) as our reference we find no significant mean direct treatment effect of PROGRESA on school enrollment for children aged 8 to 11, except for boys in survey round 5. This reflects a situation of very high pre-program (elementary school) enrollment rates for children in this age group of over 90%, combined with limited value to either market or home-economic activities[16]. In contrast, for children aged 12 to 17 we find significant and positive effects in the order of 3 to 4 percentage points for boys and roughly double that for girls. The effects are strongest in round 5, the last post-program survey round. This reflects a situation of much lower overall enrollment rates for secondary education that are in the order of 50% and the substantial earnings potential of children 12 to 17. Combined, these results seem to suggest that the transfers for young children are welcomed but have no behavioral impact, whereas for older children they do. It should be pointed out that all eligible families are poor and live in deprived localities so to suggest that the transfers spend on young children are a 'waste' is unfair as the program remains a poverty relief program.

5.1.2 The effects on work incidence

In the same table, table 9, an identical analysis was run using work incidence as the outcome variable. The results displayed in the final four columns show that for both boys and girls aged 8 to 11 we do reject the null hypothesis of equality in the conditional mean work incidence rates between treatment and control localities before the initiation of the program. This is displayed by the (positive) significant estimate of β_T. With regard to the mean direct effect of the program based on the 2DIF estimate (β_{TR}) we not surprisingly see

[16] See table 8 for pre- and post-program unconditional mean school enrollment and work incidence rates.

a mirrored, although less stark, image. When limiting ourselves to children aged 8 to 11 we find a significant negative,but very small effect, on work incidence for boys in round 3 only. Much like the case for school enrollment, the program seems to have no behavioral effects for young children. When looking at children aged 11 to 17 we only find significant negative effects for girls in survey round 5, the last post-program survey round. When comparing the order of magnitude of the effects, we find that in each post-program survey round the percentage point increase in school enrollment for boys is offset by a more or less equal percentage point reduction in work incidence. This also holds for young girls aged 8 to 11. The only exception are girls aged 12 to 17.

5.2 Results within the RDD framework

5.2.1 The effects on work incidence

Due to the fact that the threshold poverty index that determines eligibility differs by region, the RDD analysis has to be done on a per region basis. To allow a good comparison with the 2DIF results we separate the analysis for boys and girls in two different age groups, 8 to 11 and 12 to 17. When we further limit ourselves to a reasonably narrow bandwidth of 50 poverty points around the region specific threshold value we are left with too few observations to carry out the non-parametric analysis for boys and girls aged 8 to 11 separately and hence report results for boys and girls aged 12 to 17 only. Furthermore, region 12 and 28 in particular have too few observations as well. We limit our analysis to regions 3,4,5 and 6. As mentioned before, the RDD analysis uses children in eligible households just below the threshold index in treatment localities on the one hand and children in ineligible households just above the threshold in either treatment localities or control localities on the other. The latter results, based on eligible in treatment and ineligible in control, can be compared to the 2DIF estimates. The results based on children in eligible/ineligible households in treatment localities only, would be a way to mimic the experiment with a quasi-experiment. Focussing our attention to boys and using children from both treatment and control localities (Table 10 column 1), we find significant negative marginal treatment effects of roughly 5 percentage points in region 5 in survey round 3 and significant negative marginal effects of roughly double that in region 3 for both survey rounds 4 and 5. The estimated effects for the other rounds and regions are in general much smaller and in the case of region 6 even significantly positive in survey round 5. When using children in treatment localities only (second column) we find even stronger effects for region 3 in survey round 4, but on average the estimated effects are biased towards zero. For girls we find similar but slightly smaller effects. To compare our findings with the 2DIF and CSDIF estimates, we average first over region 3 to 6 by using the total number of people living in each region as weights and subsequently average over rounds 3,4 and 5. When we do this, we find an estimated effect of -0.022 for boys and -0.009 for girls when using the RDD estimates using eligible/ineligible children in treatment and control localities and -0.003.and -0.011 when using children in treatment localities only. The corresponding CSDIF estimate averaged over all rounds is -0.022 for boys and 0.002 for girls. The 2DIF estimates are -0.038 and -0.018.

5.2.2 The effects on school enrollment

When we look at the effect on school enrollment, the last four columns of table 10, and focus on boys we find significant positive effects of about 7 to 10 percentage points, except for region 6 where we find unusual strong marginal effects that seem to disappear when basing our analysis on eligible/ineligible children in treatment localities only. This seems to hold in general for the first two survey rounds, but some of the results carry through for the other rounds, in particular for region 3 in round 4 and region 5 in round 5. When we compare boys to girls we notice first off that the estimated effects are pretty comparable but are estimated with slightly more precision for girls and if anything moderately larger. More specifically, we on average find effects in the order of 12 to 17 percentage points for region 3, and 8 to 11 percentage points for region 5. However, when limiting ourselves to girls living in treatment localities only we find somewhat lower effects for the most part with the exception of region 3 in both survey round 3 and 5. As was the case for boys, the unusually large effects found for region 6 disappear when basing the analysis on children in treatment localities only. When we compare our findings with the 2DIF and CSDIF estimates by first averaging over region 3 to 6 by using the total number of people living in each region as weights and subsequently average over rounds 3,4 and 5, we find an estimated effect of 0.074 for boys and 0.118 for girls when using the RDD estimates using eligible/ineligible children in treatment and control localities. When using children in treatment localities only the corresponding estimated effects are 0.027 and 0.066. The corresponding CSDIF estimate averaged over all rounds is 0.072 for boys and 0.080 for girls. The 2DIF estimates are 0.054 and 0.082.

6 Conclusions

In this paper we investigated what the effect would be of relaxing the eligibility criterion of one of Mexico's most substantial poverty relief program. In order to be eligible to receive benefits under this program (PROGRESA) one needs to be sufficiently poor, expressed by a poverty index. For broad geographical regions there exist threshold scores which determine eligibility. This discontinuity in eligibility based on the poverty index is then exploited following the Regression-Discontinuity Design. Using only the poverty index we non-parametrically estimated the marginal average treatment effect for children that have a poverty index that is very close to the threshold value. Because the phased introduction of PROGRESA was taken advantage off by following a design for a randomized experiment, a rare added opportunity was created to also compare the outcomes of the RDD approach in a setting with experimental data at hand. It is important to stress at this point that annual fiscal constraints and logistical complexities associated with the operation of a social program like PROGRESA in very small and remote rural communities did not permit the program to cover all of eligible households at once. Rather than purposefully depriving households of program benefits for the purpose of the evaluation, the necessary sequential expansion of the program was used to select a comparable or control group from the set of households that are eligible for the program but have yet to be covered it. The resulting evaluation is the best way of determining wether scarce public funds are used effectively and efficiently towards the achievement of the short and long run objectives of the program.

Exploiting both the data from the control localities that were randomized out of participating in the early stages of PROGRESA as well as limiting ourselves to only the data available from the localities that were randomized in, the RDD approach should estimate the same average marginal treatment effect in both cases. We found that when limiting ourselves to data from the treated localities only, we almost always estimated smaller effects with less precision than when using the data from the control localities. This is much more so the case when analyzing school enrollment rather than work incidence. This gives rise to the notion that PROGRESA also had a (positive) effect on school enrollment of ineligible children in the localities where the program was introduced and who's poverty index was close to the qualifying threshold value. Further more, since the marginal effects estimated using the RDD approach based on children from both treatment and control localities -averaged over all regions and post-program rounds- are nearly identical to the corresponding CSDIF and 2DIF estimates, we find no indication that treatment effects are heterogeneous over the poverty index.

Although the program is poverty relief program and in a broader sense should also be viewed as such, we find the effects on work incidence to be rather small across all ages as are the effects on school enrollment for young children aged 8 to 11. However, for children aged 12 to 17 we find strong positive effects on school enrollment for both boys and girls which are highest for the latter and in the order of roughly 8 percentage points.

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Appendix

A Wald Estimator based on Kernel Regression

Let $Y^+ = lim_{S \downarrow COS} E[Y_i|S_i = S]$ and $Y^- = lim_{S \uparrow COS} E[Y_i|S_i = S]$. One way to get estimates of the limits Y^+ and Y^- is to use one-sided kernel regressions, which are given by

$$\hat{Y}^+ = \frac{\sum_{i=1}^n Y_i * \omega_i * K(u)}{\sum_{i=1}^n \omega_i * K(u)}$$

$$\hat{Y}^- = \frac{\sum_{i=1}^n Y_i * (1-\omega_i) * K(u)}{\sum_{i=1}^n (1-\omega_i) * K(u)}$$

where ω_i denotes the indicator function $I(S_i > COS)$. Let $K(u)$ be the uniform kernel, $K(u) = 1/2$ if $|u| \leq 1$ and $K(u) = 0$ otherwise, $u = \frac{S_i - COS}{h}$, and h is an appropriate bandwidth parameter. Hahn et al (1999) show formally that this estimator is numerically equivalent to an IV-estimator for the regression of Y_i on T_i, which uses ω_i as an instrument, applied to the sub sample for which $COS - h_- < S_i < COS + h_+$.

B Local Linear Smoother

The LLR estimator for Y^+ is given by $\hat{a}$ where

$$(\hat{a}, \hat{b}) = argmin_{a,b} \sum_{i=1}^{n} (y_i - a - b(S_i - COS))^2 K\left(\frac{(S_i - COS)}{h}\right) \omega_i$$

where ω_i again denotes the indicator function $I(S_i > COS)$. Furthermore, $K(\cdot)$ is a Kernel function and $h > 0$ is a suitable bandwidth. This estimator has been shown by Fan (1992) to have better boundary properties than the standard kernel regression estimator.

Similarly, the LLR estimator for Y^- is given by $\hat{a}$ where

$$(\hat{a}, \hat{b}) = argmin_{a,b} \sum_{i=1}^{n} (y_i - a - b(S_i - COS))^2 K\left(\frac{(S_i - COS)}{h}\right) (1 - \omega_i)$$

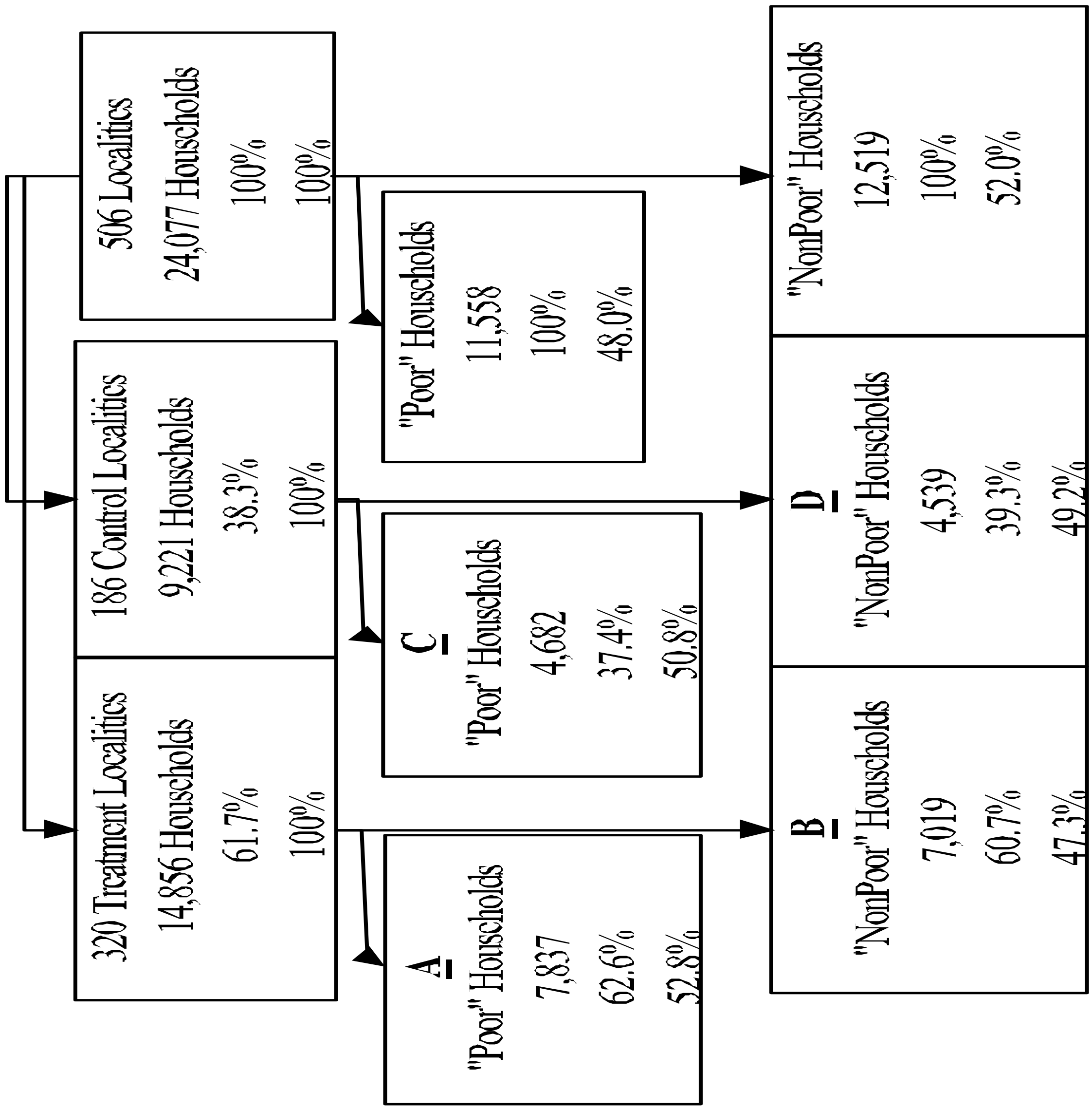


Figure 1. Number of "Poor" and "NonPoor" Households per Treated and Control locality. Evaluation Sample ENCEL98M[17].

[17] Source: Behrman and Todd (1999a)

Region Code	Name of the Region	Treatment	Control	Total
3	Sierra Negra-Zongolica-Mazateca	40	25	65
		12.50	13.51	12.87
		61.55	38.45	100.00
4	Sierra Norte-Otomí Tepehua	64	38	102
		20.00	20.54	20.20
		62.75	37.25	100.00
5	Sierra Gorda	139	80	219
		43.44	43.24	43.37
		63.48	36.52	100.00
6	Montaña (Guerrero)	21	10	31
		6.56	5.41	6.14
		67.72	32.28	100.00
12	Huasteca (San Luis Potosi)	3	4	7
		0.94	2.16	1.39
		42.95	57.05	100.00
27	Tierra Caliente (Michoacan)	45	25	70
		14.06	13.51	13.86
		64.29	35.71	100.00
28	Altiplano (San Luis Potosi)	8	3	11
		2.50	1.62	2.18
		72.75	27.25	100.00
Total		320	185	505
		100.0	100.0	100.0
		63.37	36.63	100.00

Table 2. Distribution of Localities Participating in the Experiment over Geographic Regions. Total Number of Localities: 505. Tabulations based on Census data ENCASEH97[18].

[18] Source: Behrman and Todd (1999b).

Region Code	Name of the Region	Treatment	Control	Total
3	Sierra Negra-Zongolica-Mazateca	1,502 11.84 57.13	1,127 14.77 42.87	2,628 12.94 100.00
4	Sierra Norte-Otomí Tepehua	2,252 17.76 60.00	1,501 19.68 40.00	3,754 18.48 100.00
5	Sierra Gorda	5,195 40.96 61.92	3,194 41.87 38.08	8,389 41.30 100.00
6	Montaña (Guerrero)	1,688 13.31 72.99	625 8.19 27.01	2,313 11.39 100.00
12	Huasteca (San Luis Potosi)	127 1.00 49.89	127 1.67 50.11	254 1.25 100.00
27	Tierra Caliente (Michoacan)	1,550 12.22 62.19	942 12.35 37.81	2,492 12.27 100.00
28	Altiplano (San Luis Potosi)	370 2.92 76.76	112 1.47 23.24	482 2.38 100.00
Total		12,682 100.0 62.44	7,629 100.0 37.56	20,311 100.0 100.00

Table 3. Distribution of Household Members over Geographic Regions. Total Number of Household Members (with pobre=1): 20311. Tabulations based on Census data ENCASEH97[19].

[19] Source: Behrman and Todd (1999b).

Survey round	Coverage	Non-Eligible (E=0) D Control(T=0)	B Treatment(T=1)	Eligible (E=1) C Control(T=0)	A Treatment(T=1)	Total Control(T=0)	Treatment(T=1)	Total
Pre-Program/Baseline Census/Survey								
ENCASEH-Nov97	Households	2.048	3.233	7.173	11.623	9.221	14.856	24.077
	Individuals	5.791	8.765	17.144	27.366	22.935	36.131	59.066
ENCEL-Mar98	Households	1.925	3.048	6.567	19.549	8.492	22.597	31.089
	Individuals	NA	NA	NA	NA	NA	NA	NA
Post-Program Surveys								
ENCEL-Nov98	Households	2.058	3.272	7.158	11.585	9.216	14.857	24.073
	Individuals	6.147	9.290	17.793	28.258	23.940	37.548	61.488
ENCEL-Jun99	Households	1.837	2.932	6.655	10.682	8.492	13.614	22.106
	Individuals	5.361	8.090	16.406	25.775	21.767	33.865	55.632
ENCEL-Nov99	Households	1.921	2.902	6.818	10.475	8.739	13.377	22.116
	Individuals	5.804	8.421	17.219	26.000	23.023	34.421	57.444

Table 4. Number of households and individual members covered in each survey round. The terms Eligible (E=1) and Non-Eligible (E=0) are based on the final list of eligible households constructed by the PROGRESA administration. The March 1998 ENCEL survey collected information at the individual level only for children between 0-6 years of age. No information was collected at the individual level for adult members[20].

[20] Source: Skoufias (2001).

Table 5. Expansion of PROGRESA Over Time. The treatment and control samples were taken from Phase 2. The control households were incorporated during phases 10 and 11[21].

	Phase 1	Phase 2	Phase 3	Phase 4	Phase 5	Phase 6	Phase 7	Phase 8	Phase 9	Phase 10	Phase 11
ENCASEH Survey	Oct-Dec 1996	Oct-Dec 1996 Oct-Dec 1997	Oct-Dec 1997	May-July 1998	Cleaning of Phases 1-3	Oct-Dec 1998	May-July 1999 (plus cleaning 6)	May-July 1999	May-July 1999	Oct-Dec 1999	Completion of 1999
Incorporation Date	Aug-Sept 1997	Nov/Dec 1997	Feb/Mar, May 1998	July-Sept 1998	October 1998	Nov/Dec 1998	May/June 1999	July/Aug 1999	Sept/Oct 1999	Nov/Dec 1999	Mar/April 2000
Localities Incorporated	3,369	2,988	4,334	25,568	5,432	8,151	3,290	9,758	2,801	6,523	131
Cumulative Localities	3,369	6,357	10,691	36,259	41,691	49,842	53,132	62,890	65,691	72,214	72,345
Households Incorporated	140,544	160,161	141,211	1,000,496	65,303	422,317	96,372	283,818	26,389	251,778	5,670
Cumulative Families	140,544	300,705	441,916	1,442,412	1,507,715	1,930,032	2,026,404	2,310,222	2,336,611	2,588,389	2,594,059
First Transfer	Sept/Oct 1997	Jan/Feb 1998	April-Aug 1998	Sept-Dec 1998	Nov-Dec 1998	Jan-April 1999	July-Aug 1999	Sept-Oct 1999	Nov-Dec 1999	Jan-Mar 2000	May-June 2000

[21] Source: Coady (2000).

	January-June 1998	July-December 1998	January-June 1999	July-December 1999
EDUCATIONAL GRANT PER CHILD				
(conditioned on child school enrollment and regular attendance)				
Primary:				
3rd grade	65	70	75	80
4th grade	75	80	90	95
5th grade	95	100	115	125
6th grade	130	135	150	165
Secondary:				
1st- male	190	200	220	240
2nd- male	200	210	235	250
3rd- male	210	220	245	265
1st- female	200	210	235	250
2nd- female	230	235	260	280
3rd-female	240	255	285	305
GRANT FOR SCHOOL MATERIALS PER CHILD				
Primary- September	-	In-kind	-	110
Primary- January	40	-	45	-
Secondary- September	-	170	-	205
GRANT FOR CONSUMPTION OF FOOD PER HOUSEHOLD				
(conditioned on attending scheduled visits to health centers)				
Cash Transfer	95	100	115	125
MAXIMUM GRANT PER HOUSEHOLD				
	585	625	695	750

Table 6. PROGRESA Monthly Cash Transfer Schedule (Nominal Pesos)[22].

[22] Source: Skoufias (2001).

Age Group	Frequency of Check-Ups
Children	
Less than 4 months	3 check-ups: 7 and 28 days, and at 2 months
4 months to 24 months	8 check-ups: 4,6,9,12,15,18,21 and 24 months with 1 additional monthly weight and height check-up
2 to 4 years old	3 check-ups a year: 1 every 4 months
5 to 16 years old	2 check-ups a year: 1 every 6 months
Women	
Pregnant	5 check-ups: prenatal period
During puerperium and lactation	2 check-ups: in immediate puerperium and 1 during lactation
Adults and youths	
17 to 60 years old	One check-up per year
Over 60 Years old	One check-up per year

Table 7. Annual Frequency of Health Care Visits Required by PROGRESA[23].

[23] Source: Skoufias (2001)

	School enrollment				Work incidence			
	Females		Males		Females		Males	
Age	control	treatment	control	treatment	control	treatment	control	treatment
Round 1								
8-11 yr	93.02	94.02	93.69	93.63	1.61	3.53	4.25	6.2
12-17 yr	47.17	48.07	55.6	56.78	10.35	13.17	35.36	37.75
Round 3								
8-11 yr	95.35	96.5	95.3	96.72	0.71	1.17	2.73	2.34
12-17 yr	47.25	53.75	53.88	58.38	7.49	7.79	27.86	26.8
Round 4								
8-11 yr	92.68	94.5	91.96	93.99	1.31	2.37	2.86	2.93
12-17 yr	54.64	60.94	59.85	63.27	6.46	7.24	23.89	23.07
Round 5								
8-11 yr	94.06	94.59	92.13	95.05	0.71	1.72	1.86	1.71
12-17 yr	53.51	60.89	57.63	62.76	7.34	6.85	25.83	23.9
Round 3-5								
8-11 yr	94.05	95.21	93.15	95.28	0.9	1.74	2.48	2.33
12-17 yr	51.78	58.44	57.08	61.45	7.1	7.3	25.89	24.6

Table 8. Unconditional mean school enrollment and work incidence rates

	School enrollment				Work incidence			
	Males		Females		Males		Females	
	8-11 yr (1)	12-17 yr (2)	8-11 yr (3)	12-17 yr (4)	8-11 yr (5)	12-17 yr (6)	8-11 yr (7)	12-17 yr (8)
T	0.0008	0.0264	0.0055	-0.002	0.0114	0.0155	0.0122	0.0196
	(0.12)	(1.21)	(0.81)	(0.08)	(2.14)*	(0.93)	(3.01)**	(1.89)
R3	0.0222	0.0155	0.0245	-0.0146	-0.0093	-0.0923	-0.0115	-0.0241
	(3.65)**	(0.83)	(4.48)**	(0.83)	(1.47)	(5.67)**	(2.57)*	(2.51)*
R3_T	0.0134	0.044	0.0029	0.0766	-0.0132	-0.0333	-0.0048	-0.0179
	(1.80)	(2.45)*	(0.41)	(4.27)**	(2.03)*	(1.68)	(0.82)	(1.66)
R4	0.004	0.0828	0.0091	0.0745	-0.0073	-0.1231	-0.0047	-0.0323
	(0.64)	(4.42)**	(1.43)	(3.88)**	(1.18)	(6.91)**	(1.12)	(3.64)**
R4_T	0.0111	0.0328	0.0054	0.075	-0.0091	-0.0335	-0.0034	-0.0117
	(1.67)	(1.77)	(0.76)	(3.74)**	(1.39)	(1.62)	(0.72)	(1.05)
R5	0.005	0.0656	0.0187	0.0643	-0.0156	-0.108	-0.0108	-0.0245
	(0.72)	(3.09)**	(2.69)**	(3.03)**	(2.29)*	(5.41)**	(2.09)*	(2.35)*
R5_T	0.0186	0.059	-0.0032	0.0946	-0.0115	-0.0466	-0.0008	-0.023
	(2.70)**	(2.86)**	(0.37)	(4.22)**	(1.30)	(2.06)*	(0.10)	(1.82)
mcharmis	-0.0617	-0.1268	-0.0653	-0.0992	0.0104	0.0163	-0.0067	-0.0447
	(2.46)*	(2.84)**	(2.94)**	(2.02)*	(0.82)	(0.47)	(1.04)	(3.33)**
MINDIG	-0.0044	0.056	-0.0121	0.0373	-0.0153	-0.0409	0.0017	-0.0208
	(0.38)	(1.86)	(1.11)	(1.21)	(2.18)*	(1.97)*	(0.31)	(1.68)
MSPANISH	0.0104	0.0421	0.0208	0.0365	0.0106	0.0208	0.0023	0.0174
	(1.14)	(1.53)	(3.28)**	(1.39)	(1.45)	(1.17)	(0.51)	(1.52)
mage	-0.0001	-0.002	0.0001	-0.0007	-0.0003	0.0017	0.0003	0.0001
	(0.16)	(0.17)	(0.25)	(0.75)	(2.78)**	(0.59)	(0.56)	(0.41)
MLIT	0.0195	0.0747	0.0278	0.0509	-0.0032	-0.0455	0.001	-0.0215
	(3.91)**	(5.62)**	(5.25)**	(3.63)**	(0.86)	(5.07)**	(0.43)	(5.01)**
MPRI	0.0118	0.0096	0.0133	0.0185	-0.002	-0.0065	-0.0012	-0.0042
	(2.11)*	(0.65)	(2.31)*	(1.17)	(0.36)	(0.50)	(0.31)	(0.57)
MSEC	0.0054	0.1087	0.0203	0.1723	0.0064	-0.1187	-0.0085	-0.0144
	(0.41)	(1.72)	(1.21)	(2.97)**	(0.49)	(2.44)*	(0.97)	(0.56)

Table 9. Results from a ML probit estimation on eligible only. Reported are marginal effects for continuous variables. Robust z statistics in parentheses (corrected for clustering at the locality level).

Table 9+	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
fcharmis	-0.0632 (2.57)*	-0.019 (0.39)	-0.0492 (2.39)*	-0.0602 (1.19)	-0.0045 (0.43)	0.022 (0.60)	0.0186 (1.92)	0.0257 (1.26)
FINDIG	-0.005 (0.36)	0.0603 (1.81)	-0.0088 (0.76)	-0.009 (0.21)	0.0068 (0.67)	-0.0525 (2.17)*	0.0031 (0.49)	0.0158 (1.13)
FSPANISH	0.0108 (0.91)	0.0242 (0.91)	0.0035 (0.41)	0.07 (1.70)	-0.0046 (0.50)	-0.0198 (0.95)	-0.0022 (0.43)	-0.0168 (1.45)
fage	-0.0005 (1.55)	-0.0002 (2.31)*	-0.0003 (0.97)	-0.0011 (1.18)	0.0006 (1.41)	0.0004 (2.63)**	-0.0001 (1.82)	0 (0.04)
FLIT	0.0196 (3.42)**	0.0499 (3.90)**	0.0118 (2.18)*	0.063 (3.92)**	-0.0054 (1.48)	-0.031 (3.32)**	-0.0041 (1.68)	-0.008 (1.56)
FPRI	-0.0031 (0.49)	0.0246 (1.69)	-0.0097 (1.61)	-0.0348 (2.25)*	0.0073 (1.33)	-0.0039 (0.32)	-0.0042 (1.44)	0.0014 (0.18)
FSEC	0.0032 (0.23)	0.1665 (3.16)**	0.0363 (2.38)*	0.1145 (2.04)*	0.0143 (1.15)	-0.0979 (2.25)*	0.0107 (1.11)	0.0057 (0.23)
indice	-0.0022 (0.70)	0.0187 (1.43)	-0.0027 (0.81)	-0.0042 (0.29)	0.0025 (0.99)	0.0179 (2.13)*	0.002 (0.99)	-0.0035 (0.69)
distance	0.0006 (1.51)	0.0054 (3.35)**	0.0006 (1.36)	0.0036 (2.26)*	0.0004 (1.09)	-0.0026 (2.83)**	0 (0.09)	-0.0012 (2.66)**
distsec	-0.0037 (2.51)*	-0.0262 (5.73)**	-0.0018 (1.69)	-0.0332 (5.75)**	0.0023 (2.72)**	0.0129 (4.88)**	0.0013 (2.20)*	0.0066 (4.20)**
c0_2	-0.0037 (1.66)	-0.009 (1.46)	-0.0056 (2.60)**	-0.0259 (3.67)**	0.0037 (2.89)**	0.0117 (2.38)*	0.0005 (0.49)	0.0024 (1.05)
c3_5	-0.0019 (0.98)	-0.0271 (4.21)**	-0.0038 (2.00)*	-0.0185 (2.62)**	0.0019 (1.28)	0.0179 (3.72)**	0.0012 (0.97)	0.0071 (2.81)**
m6_7	-0.0016 (0.51)	-0.0258 (2.68)**	-0.0015 (0.49)	-0.0012 (0.12)	0.0037 (1.60)	0.0201 (2.87)**	0.0008 (0.51)	0.0069 (1.83)
f6_7	0.001 (0.28)	-0.0255 (2.49)*	-0.0006 (0.17)	0.0011 (0.11)	0.0021 (0.93)	0.022 (2.97)**	0.0019 (1.18)	0.0038 (0.90)
m8_12	-0.0165 (7.72)**	-0.0113 (1.78)	-0.006 (2.57)*	-0.0014 (0.22)	0.0015 (0.80)	0.0038 (0.80)	-0.0025 (1.92)	0.0004 (0.16)

Table 9 [more] Results from a ML probit estimation on eligible only. Reported are marginal effects for continuous variables. Robust z statistics in parentheses (corrected for clustering at the locality level).

Table 9++	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
f8_12	-0.0072	-0.0103	-0.0095	0.002	-0.0046	0.0022	-0.0012	0.0038
	(3.22)**	(1.50)	(4.39)**	(0.30)	(2.28)*	(0.45)	(0.87)	(1.48)
m13_18	-0.0025	-0.0062	0.0005	-0.016	0.0008	0.0101	-0.0016	-0.0021
	(1.28)	(1.01)	(0.26)	(2.44)*	(0.64)	(2.08)*	(1.48)	(0.95)
f13_18	0.0038	-0.0061	0.0023	0.0028	-0.0011	-0.0036	0.0007	0.0046
	(1.79)	(1.00)	(1.12)	(0.42)	(0.63)	(0.76)	(0.73)	(1.87)
m19_54	-0.0076	-0.0085	-0.0023	-0.0175	0.0008	-0.0131	0.0002	-0.0121
	(3.34)**	(1.26)	(0.95)	(2.43)*	(0.34)	(2.64)**	(0.16)	(4.47)**
f19_54	-0.0014	0.0187	-0.0049	0.0144	-0.0019	-0.0211	0.0007	0.0018
	(0.60)	(2.57)*	(1.99)*	(1.89)	(0.91)	(3.96)**	(0.46)	(0.69)
m55p	-0.0002	-0.026	0.0076	0.0066	0.0052	0.0095	-0.0034	-0.0031
	(0.05)	(1.94)	(1.69)	(0.51)	(1.53)	(0.92)	(1.44)	(0.66)
f55p	0.0107	0.0345	0.004	0.0295	0.0002	-0.0037	0.0018	0.0099
	(2.37)*	(2.70)**	(0.83)	(2.11)*	(0.05)	(0.41)	(0.86)	(2.17)*
age10	-0.0044		-0.0034		0.006		0.0007	
	(1.32)		(1.16)		(2.58)**		(0.37)	
age11	-0.0131		-0.0187		0.0242		0.0058	
	(3.76)**		(5.30)**		(8.73)**		(3.09)**	
age13		-0.1392		-0.166		0.0924		0.0225
		(9.97)**		(12.82)**		(7.55)**		(3.33)**
age14		-0.2937		-0.3019		0.2281		0.0559
		(21.31)**		(23.51)**		(18.35)**		(8.02)**
age15		-0.4472		-0.4376		0.3871		0.0982
		(30.29)**		(33.52)**		(28.16)**		(11.90)**
age16		-0.5548		-0.5195		0.4991		0.15
		(36.74)**		(37.86)**		(33.92)**		(15.04)**
age17		-0.6992		-0.6302		0.619		0.1915
		(55.21)**		(50.93)**		(39.24)**		(16.78)**
Observations	25637	33242	24333	30794	25637	33242	24333	30794

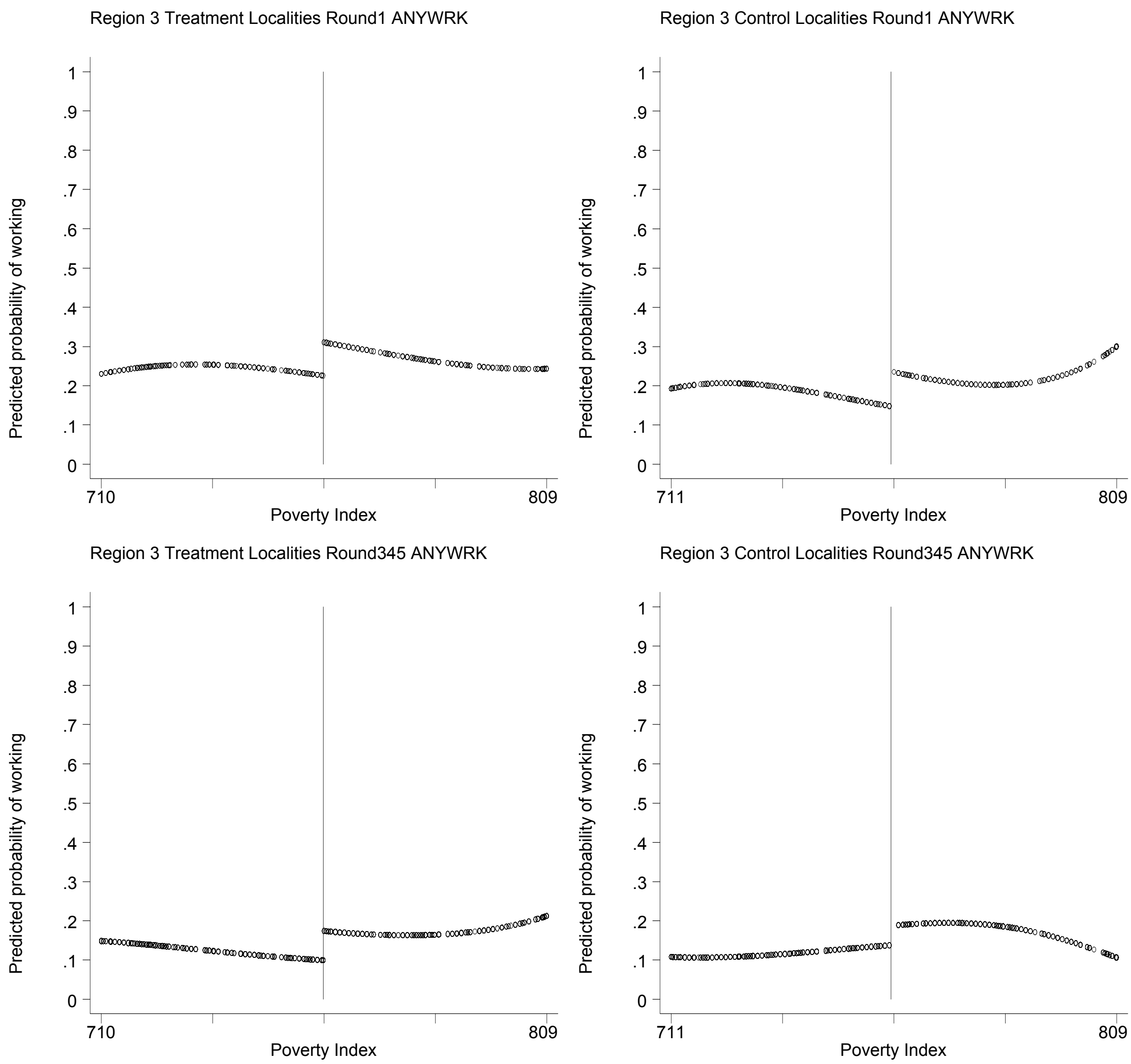
Table 9 [more]. Results from a ML probit estimation on eligible only. Reported are marginal effects for continuous variables. Robust z statistics in parentheses (corrected for clustering at the locality level).

	work incidence				school enrollment			
	Boys 12-17 yr RDD based on		Girls 12-17 yr RDD based on		Boys 12-17 yr RDD based on		Girls 12-17 yr RDD based on	
Round 1	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment
region 3	0.0510	-0.0105	0.0646	0.0264	0.0496	0.0210	0.0240	0.0160
	(0.0553)	(0.0611)	(0.0466)	(0.0530)	(0.0534)	(0.0625)	(0.0674)	(0.0677)
region 4	0.0293	-0.0159	0.0459	0.0165	0.0198	-0.0094	-0.0154	-0.0815
	(0.0505)	(0.0614)	(0.0331)	(0.0419)	(0.0492)	(0.0574)	(0.0508)	(0.0642)
region 5	-0.0098	-0.0241	-0.0230	0.0135	0.0635	-0.0132	0.0684	-0.0023
	(0.0279)	(0.0412)	(0.0193)	(0.0244)	(0.0309)*	(0.0427)	(0.0337)*	(0.0492)
region 6	-0.1340	-0.0657	-0.1548	0.1446	0.1938	-0.0858	0.2518	-0.1166
	(0.1306)	(0.0762)	(0.1046)	(0.0675)*	(0.1310)	(0.0714)	(0.0809)*	(0.0835)
Round 3								
region 3	-0.0700	0.0220	-0.0200	-0.0765	0.1060	0.0307	0.1267	0.1495
	(0.0526)	(0.0558)	(0.0325)	(0.0411)	(0.0568)	(0.0625)	(0.0577)*	(0.0653)*
region 4	0.0332	0.0015	0.0371	0.0110	0.0433	0.0388	0.0703	0.0104
	(0.0458)	(0.0550)	(0.0251)	(0.0295)	(0.0512)	(0.0604)	(0.0509)	(0.0640)
region 5	-0.0481	-0.0084	-0.0302	0.0039	0.0721	-0.0306	0.0797	0.0752
	(0.0254)*	(0.0372)	(0.0198)	(0.0264)	(0.0312)*	(0.0453)	(0.0327)*	(0.0487)
region 6	-0.0699	-0.0450	-0.1758	0.0097	0.2972	-0.0969	0.2536	-0.0759
	(0.1271)	(0.0644)	(0.1038)	(0.0454)	(0.1167)*	(0.0721)	(0.1023)*	(0.0714)
Round 4								
region 3	-0.1056	-0.1325	-0.0859	-0.0513	0.1171	0.1207	0.1672	0.1192
	(0.0558)*	(0.0613)*	(0.0434)*	(0.0449)	(0.0546)*	(0.0622)*	(0.0608)*	(0.0670)
region 4	-0.0561	0.0680	0.0633	-0.0462	-0.0025	-0.1217	0.0337	0.0655
	(0.0499)	(0.0494)	(0.0208)*	(0.0424)	(0.0527)	(0.0606)*	(0.0504)	(0.0647)
region 5	-0.0253	-0.0203	-0.0055	-0.0141	0.0402	0.0556	0.0950	0.0710
	(0.0246)	(0.0385)	(0.0155)	(0.0252)	(0.0337)	(0.0475)	(0.0355)*	(0.0521)
region 6	0.2372	0.0701	0.0766	0.1128	-0.0912	-0.0372	0.3249	0.0012
	(0.1401)	(0.0756)	(0.1099)	(0.0580)*	(0.1690)	(0.0783)	(0.1234)*	(0.0719)
Round 5								
region 3	-0.1119	-0.0650	0.0031	-0.0047	0.0896	0.1131	0.1575	0.1712
	(0.0560)*	(0.0611)	(0.0329)	(0.0367)	(0.0557)	(0.0615)	(0.0620)*	(0.0676)*
region 4	-0.0061	0.0027	-0.0059	-0.0623	0.0104	0.0360	0.0955	0.0895
	(0.0474)	(0.0541)	(0.0282)	(0.0448)	(0.0525)	(0.0658)	(0.0533)	(0.0667)
region 5	-0.0220	0.0105	-0.0221	-0.0073	0.1000	0.0931	0.1122	0.0477
	(0.0267)	(0.0389)	(0.0172)	(0.0274)	(0.0310)*	(0.0457)*	(0.0322)*	(0.0482)
region 6	0.2315	0.0676	0.0746	0.0408	0.3389	-0.0203	0.3985	-0.0371
	(0.0265)*	(0.0577)	(0.0177)*	(0.0306)	(0.1291)*	(0.0730)	(0.1002)*	(0.0748)

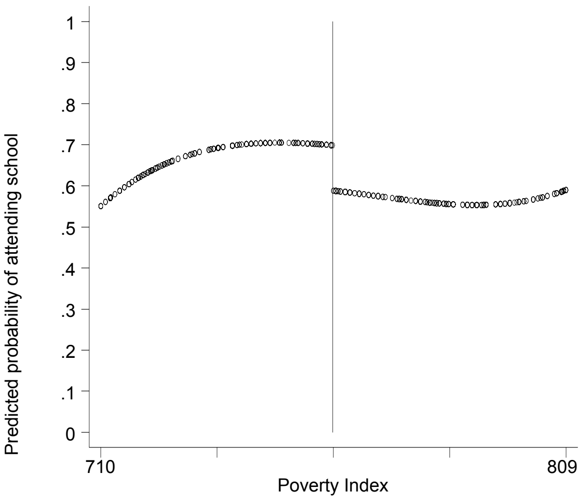
Table 10. RDD results based on treatment and control or treatment localities only. Bootstrapped s.e. (500x)

Variable Name	Meaning
T	Dummy indicating treatment locality
Rx	Dummy indicating round 'x'
Rx_T	Interaction of treatment locality indicator and round
mcharmis	Mother's characteristics are missing
MINDIG	Mother speaks indigenous dialect
MSPANISH	Mother speaks Spanish
mage	Mother's age
MLIT	Mother is literate (can read and write)
MPRI	Mother's highest schooling level is elementary schooling
MSEC	Mother's highest schooling level is secondary schooling
fcharmis	Father's characteristics are missing
FINDIG	Father speaks indigenous dialect
FSPANISH	Father speaks Spanish
fage	Father's age
FLIT	Father is literate (can read and write)
FPRI	Father's highest schooling level is elementary schooling
FSEC	Father's highest schooling level is secondary schooling
indice	Marginality index of the locality
distance	Distance in km to the municipality's center
distsec	Distance in km to the nearest secondary school
cx_xx	Number of children in the household between x and xx years old
mx_xx	Number of men in the household between x and xx years old
fx_xx	Number of women in the household between x and xx years old
m55p	Number of men over 55 in the household
f55p	Number of women over 55 in the household
age X	Age dummies

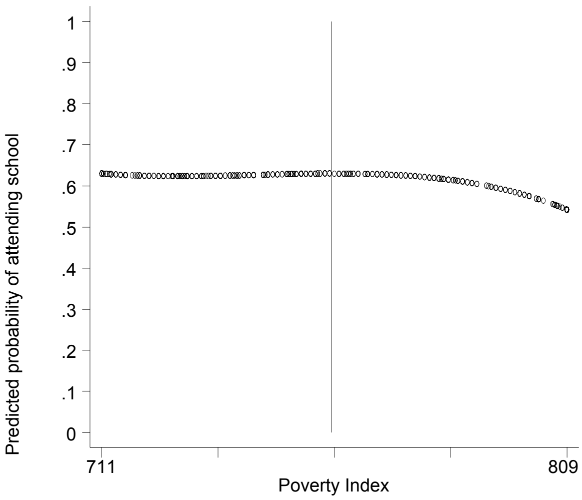
Table 11. Variable lists.


Region 3 Treatment Localities Round1 ANYWRK
Region 3 Control Localities Round1 ANYWRK
Region 3 Treatment Localities Round345 ANYWRK
Region 3 Control Localities Round345 ANYWRK
Predicted probability of working
Poverty Index
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1
710
711
809

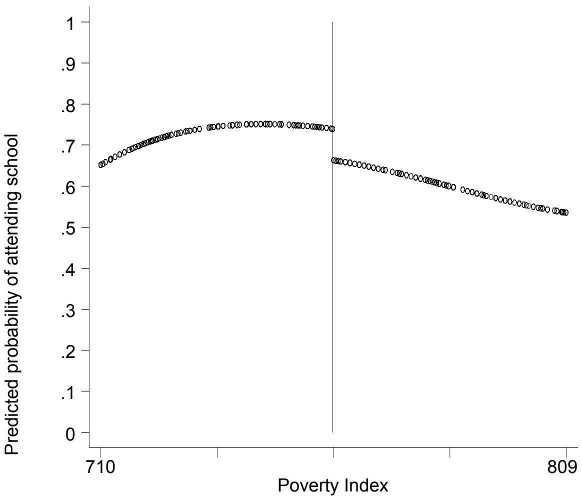
Region 3 Treatment Localities Round1 attS



Region 3 Control Localities Round1 attS



Region 3 Treatment Localities Round345 attS



Region 3 Control Localities Round345 attS