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IMPACT OF *OPORTUNIDADES* ON CONTRACEPTIVE METHODS USE IN ADOLESCENT & YOUNG ADULT WOMEN LIVING IN RURAL AREAS, 1997-2000[*]

Short title: Oportunidades and contraception in youths

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ABSTRACT

Oportunidades is a social program run by the Mexican government that seeks to improve education, health, nutrition, and living conditions of those living in extreme poverty. People supported by the program attend monthly health talks, which include information on contraceptive methods. Reduction in fertility, especially among youths, is deemed crucial to accomplish the program's goals. We analyze information on contraceptive method use among young women from the *Oportunidades* evaluation surveys conducted in the years 1997 to 2000. We present intention to treat effect estimates, and other estimates obtained by several statistical procedures performed to evaluate the impact of the program on contraceptive methods use by young women. To accomplish this, we took advantage of the experimental design setting that was implemented to evaluate the program in rural areas. We found that among women 20- to 24-years-old, the program increased the prevalence of contraceptive methods use by 5 to 10 percentage points after two years of exposure to the program. The impact appears to have occurred mostly to those with the lowest socio-economic level.

INTRODUCTION

Oportunidades, which started in 1997 as *Progresa* in rural areas, is a program run by the Mexican government to break the intergenerational cycle of poverty. It was designed to improve the education, health, nutrition, and living conditions of those in extreme poverty. In 2000 the program transformed into *Oportunidades*, and was expanded into a more general frame of actions by the government to promote social development, in what is called the *Contigo* strategy. *Oportunidades* became available in semi-urban areas in 2001, and in urban areas in 2002. By 2005, the program had enrolled 5 million families, with more than 25 million people across the country.

The program has four main components: 1) cash transfers for keeping children in school; 2) a health component, including a set of health promotion talks; 3) a free essential health care package known as PESS (for its name in Spanish, *Paquete Esencial de Servicios de Salud*), and 4) a nutritional supplementation program. Cash transfers to families vary according to the number of children they have and the age and gender of the children, with payments being higher for girls in higher education grades. These cash transfers are given to the female head of household and are conditioned on compliance with other aspects of the program, such as attendance at monthly health promotion talks.

In the area of reproductive health, these talks include information on family planning, prenatal care, alarm signs during pregnancy, and newborn care. The attendance at health promotion talks is a co-responsibility of the program participants (along with keeping the children in school and attending a set of check-up visits at the health services). Families that fail to attend health promotion talks may be excluded from the program, and therefore lose the benefit of the essential health care package.

Different procedures have been employed in rural and urban settings to identify families living in extreme poverty that may be eligible for the program. In rural areas, *Oportunidades* has identified areas with a high concentration of poor households and places where there are schools and health care facilities available for the implementation of the program. *Oportunidades* conducted a census of households in those rural areas, applying a questionnaire to estimate an eligibility score for each household. This assessment of eligibility includes a visual inspection of the household's characteristics. From these measures and assessments *Oportunidades* constructed a score based on family and households characteristics. Families under an eligibility score threshold receive further information and are enrolled in the program if they agree to fulfill some co-responsibilities, such as keeping children in school, having medical checkups, and attending health promotion talks. Local program administrators had some leeway in determining which families were eligible for the program – eligibility was not strictly according to the eligibility poverty score. Later program expansions might have added health facilities to new localities, while the original "experiment" focused on communities already with schools and health care facilities. This will impact the interpretation of the experimental effect – and maybe its relevance for assessing expansions of the program.

Potential program impacts

One of the strategies of the *Oportunidades* program is to strengthen the provision of an essential health care package. This package includes two important activities that relate to reproductive health. The first involves services for groups with specific needs, such as women who are pregnant, in the aftermath of giving birth, or breastfeeding. The second is monthly health promotion talks given to the heads of the beneficiary households, and since 2001 to students of middle-high education level. In the case of focused health actions, and particularly family planning, utilization of services is tightly linked to the key change in behavior, which is increasing contraceptive use in the beneficiary population.

During the time period covered in this study, only the person in charge of the care of children in the household (usually a woman) had to attend the health promotion talks. Health promotion talks have the potential of directly influencing the use of family planning methods by recruiting new users of these services, reinforcing the use of contraceptive methods among those who already use them, or by modifying the norm with regards to the size of the family. Other mechanisms by which the program may increase use of contraceptive methods among adolescents are increased access to health services and family planning methods, which are provided in the health services as part of the PESS; and increased school attendance, which has been documented as a positive impact of the program (Parker, Behrman and Todd 2005).

The use of contraceptive methods has a direct influence on fertility. Analyses of the decline in fertility in developing countries point to contraceptive methods use as the single most important related factor (Wohlschlagl 1991). The use of contraceptive methods also produces an impact in mother-infant mortality. Even though the exact nature of the relationship is more difficult to quantify, there is increasing evidence of health benefits related to an avoidance of births at a very early or very late maternal age, or to high parity in short periods, as well as a reduction in dependence on abortions as means of limiting fertility (Emond et al 2002; Ahman and Shah 2006).

According to the National Population Council (CONAPO 2000), there are approximately 22 million adolescents in Mexico (about one quarter of the entire population), and it is estimated that 36% of them were living in poverty in 2000. Adolescents living in poverty are vulnerable in terms of personal development. Developing in poor social conditions and at an economic disadvantage are decisive factors for early motherhood in developing countries (Welti 2000; Gigante et al 2004). CONAPO (2000) data also suggest that school drop out occurs at a much earlier age for children of women who are illiterate or have less than six years of education. Among children younger than 15 years of age, more than 50% of those children whose mother did not finish elementary school had already dropped out from school. In contrast, among children of mothers who finished elementary school, only 50% had dropped out by age 18. Children of mothers with few years in school start their sexual life earlier than any other group; one study showed that the odds of having first intercourse where inversely related to the years of maternal education (Dorius, Heaton and Steffen 1993).

Although the fertility rate in Mexican adolescents has declined in the last decades (Menkes and Suárez 2003), its decline has been smaller than the overall decline in fertility rate and therefore adolescent pregnancy now comprises a higher proportion of all pregnancies (Welti 2000; Menkes and Suárez 2003). This is likely due to poor knowledge and use of modern contraceptive methods.

Making contraceptives available for youth would likely reduce the probability of health risk behaviors, and hopefully would reduce the incidence of unwanted pregnancy and sexually transmitted diseases (STDs) in this population group as well. However, the impact of reproductive health programs within the adolescent population of Mexico is unknown. Only studies of birth-rate trends in the general population are being reported (Welti 1997, 2000; Menkes and Suárez 2003).

In our study population, all subjects (both in treatment and control areas) have free access to contraceptives, particularly condoms. Thus, program effects on contraceptive method use might arise primarily from the health information talks that adolescent women, their parents, or their partners attend rather than by improving access to contraceptives. Another possibility is that adolescents in treatment areas are more likely to stay in school than those in control areas, and this might provide them with some information on family planning or desire to use contraception to postpone pregnancy until they finish school. If the *Oportunidades* effect is mostly an "information" effect, then there could be spillover effects from the eligibles to the non-eligibles in the treatment communities. Additionally, the increased purchasing power of the program participants is likely to have a beneficial effect on non-participants if this increased income is spent locally. This would mean that some standard impact evaluations, like a difference-in-differences (d-in-d) approach or a Regression Discontinuity (RD) approach, might measure too small an effect.

The evaluation of *Oportunidades* has analyzed the impact of the program in different areas like education, nutrition, health, consumption and migration (Hernández-Prado et al 2005). Some studies have analyzed the impact of *Oportunidades* on knowledge and use of family planning methods. A study published by Huerta and Hernández (2000) analyzed information from the rural evaluations in 1998 and 1999. They found an increase in knowledge and use of family planning methods among women 20- to 49-years-old with partners. The impact of the program in the use of methods was larger among women 20- to 24-years-old and 40- to 44-years-old. A positive impact was also found in the study conducted by Hernández-Prado et al. (2005). Using d-in-d methods, they found an increase in the knowledge and use of family planning methods among reproductive age women with partners for the rural areas in the short and medium terms. On the other hand, their study only found a positive effect of the program in the short term (one year of exposure to the program) in the knowledge; there was no statistically significant effect on the use of family planning methods in urban areas.

Although the studies mentioned above have documented positive impacts of *Oportunidades* on knowledge and use of family planning methods, those studies have concentrated either on women over 20-years-old, or have included all women of reproductive age, without a specific analysis for adolescents. Given the importance of this group in terms of reproductive health, and the specific actions of the program aimed to this age group, it is necessary to evaluate the impact of *Oportunidades* on family planning among adolescents.

ANALYSES AND RESULTS

The objective of the following analyses is to estimate the effect of the *Oportunidades* program on the prevalence of use of contraceptive methods among adolescents and young adult women living in rural areas. Women were asked about their current use of contraceptive methods as well as their partners' current use, what kind of methods they were currently using,[†] and whether they had ever used methods. We define our main outcome as a dichotomous variable that indicates whether a woman currently uses modern contraceptive methods; the use of natural methods was not considered as actual contraceptive use (1=uses condom, pills, intrauterine device, or intramuscular injectable contraceptives; 0=none or others). A dummy variable indicates being included in a treatment locality (1=households in intervention community, 0=otherwise).

A poverty score was estimated for each household in the beginning of the program (1997) as a function of household characteristics. Based on this poverty score, the implementers of the program defined an arbitrary threshold for defining poverty. In theory, only households that were below the poverty line would actually be eligible for the program (see Appendix 1). In practice, results of the enrollment screening process were validated in community assemblies, and local administrators played a role on defining which households were finally enrolled into the program. Eligibility for the program is defined by a dummy variable (1=poor, 0=otherwise).

[†] The survey did not specifically ask for use of contraceptive methods during the last intercourse.

In the 1997 survey, information on contraceptive method use was only available for women who were informants for the entire household (usually the woman in charge of the care of children). Conversely, all women were interviewed and asked about reproductive health outcomes in the year 2000. We carry out two set of analyses. The first exploits the experimental design using data on 15- to 19-year-old adolescent women in the year 2000. The second set of analyses examines young adult women between 20 and 24 years of age in that same year. In the year 2000, a total of 2,735 women between 15 and 19 years of age answered the questionnaire on contraceptive methods use, and 389 were excluded from subsequent analysis because they were lacking the "poverty score" variable, 13% of women in the control areas were lacking this variable compared to 16% in treatment areas. For the 20- to 24-year-old group, the sample size was 2,682, but 452 subjects were excluded from subsequent analysis since they had no information on poverty score (18% of control area subjects vs. 16% of treatment area subjects). The rate of missing poverty data was not significantly different between treatment and control areas in any case. In conclusion, the sample size in the year 2000 was 2,346 for the *adolescent* group and 2,230 for the *young adult* group. In 1997, a total of 1,480 women 12 to 21 years of age were interviewed and responded to questions about contraceptive use. Of these, 812 were successfully re-interviewed in the year 2000; we refer to these subjects as the "panel" subset.

Descriptive analysis

Table I presents descriptive statistics of the study subjects at baseline (1997), according to assignment to a treatment community; these are presented separately for the *adolescent* and *young adult* groups, and are restricted to subjects who eventually were included in the year 2000 survey and answered the reproductive health questionnaire. Random assignment to the program (at community level) was performed at this time, and eligibility status was assigned before randomization. Mann-Whitney and χ^2 tests were performed to detect significant differences of the variables according to the treatment assignment. In addition, a logistic regression model of treatment assignment was implemented. A global test of significance showed that adolescents are indeed statistically different in treatment and control areas. However, results from the model presented in table I show that this difference is mainly due to age and poverty score. Consequently we adjusted for both of these variables in all the models presented in this paper. For the young adult group, the global test did not show a significant difference between the treatment and control areas.

Additionally, table II compares descriptive statistics between treatment and control areas in the year 2000. Prevalence of contraceptive methods use among adolescents was 3% in both control and treatment, whereas for young adults it was 19% and 23 % for control and treatment areas, respectively. The difference in contraceptive use prevalence between treatment and control areas was significant (p=0.02) in the young adult group, but not in the adolescent group.

Experimental design analyses

Several approaches for estimating the effect of the program are used on the year 2000 data. We estimated separate models for an "adolescent" group (15- to 19-year-olds) and the "young adult" group (20- to 24-year-olds).

Intent-to-treat analyses

This set of analyses takes advantage of the random allocation of the program to "treatment" and "control" areas; the results are presented in table III,[‡] which features three different strategies for estimating the program effect. The basic or simple approach (Model I) is an adjusted comparison of the proportions of women who reported using contraceptive methods in the treatment areas vs.

[‡] In all model tables, the first entry in every column presenting regression estimates is what would typically be considered the estimate of the treatment effect associated with the estimation procedure used for that column.

that in the control areas. We do this by using a linear regression model where the outcome is the dummy variable for current contraceptive methods use in 2000. The explanatory variables are the dummy for assignment to a treatment area, the $\log_e$ transformed poverty score up to the fourth power and dummies for state (beta coefficients for the latter two of set variables are not shown). Robust standard errors were calculated taking into account clustering at the locality level. The use of robust standard errors also controls for heteroskedasticity.

This model makes the assumption that randomization worked in the sense that confounding variables are equally distributed in treatment and control areas. This is a reasonable assumption based on descriptive statistics at baseline. It also makes no distinction between subjects who were residing in treatment areas but did not actually receive the treatment and those who did (non-eligibles vs. eligibles). Therefore, this is an estimate of the effect of the program at a community level. First, consider these community level effects as presented in the first two columns. The effect estimate in the 15- to 19-year-old group is very small (0.002), indicating that the effect of the program in this age group after two years of exposure to the program is negligible, and it is statistically indistinguishable from zero. However, the results for the older group showed the prevalence of contraceptive methods use to be 4.6 percentage points greater in treatment versus control areas.

In order to estimate the effect of the treatment, not at a locality level but on those individuals who would directly benefit from the program, we can compare the difference in prevalence of contraceptive methods use in eligibles of treatment areas vs. eligibles in control areas, controlling for inherent differences in prevalence of contraceptive methods use between treatment and control areas for non-eligibles. This is our preferred impact estimate, because it actually refers to the impact of the program on eligible women, as the group that we want to focus. Note that this approach can underestimate the true impact of the program, if there is a beneficial spillover from the eligibles to the non-eligible group. This effect estimate was obtained by means of a linear regression model with robust standard errors and clustering at the locality level. Explanatory variables included were the dummy variable for treatment, the dummy variable for eligibility to the program and the interaction between both, adjusting for poverty score, age, and state. The estimate of the program effect is the interaction coefficient, dubbed a *difference-in-difference* estimate or $\hat{\Delta}$ (Angrist and Krueger 1999), which is an estimate of how different between eligibles and non-eligibles, is the difference of prevalence of contraceptive methods use between treatment and controls (Equation 1).

$$\hat{\Delta} = (p_{te} - p_{ce}) - (p_{tn} - p_{cn}) \qquad (1)$$

Where $\hat{\Delta}$ is the *difference-in-difference* estimate, p denotes the prevalence of contraceptive methods use, subindex t denotes residency in a treatment area, subindex c denotes residency in a control area, subindices e and n denote eligibility and non-eligibility to the program, respectively.

Using this approach, we find that in the younger group that the effect of the program is small, in the order of a 1 percentage point difference in contraceptive methods use in treatment versus control eligible subjects (Model II in table III). However, the effect again appears to be much larger for the older group as we estimate a 5 percentage point difference for them. The standard error for this estimate is, however, relatively large, rendering it only significant at the 10% level. After obtaining our main impact estimates for adolescents and young adults, we conducted other analyses to check the consistency of our results. The last two columns of table III show the results of a model restricted to eligible subjects. Should there be no spillover between eligible and non-eligibles in treatment areas, this estimate should equal the difference-in-difference estimate (and the main effect for the variable treatment in the previous model should equal zero). As in the case of model II (our main impact estimates), the estimate obtained in this third model is not distinguishable from zero for the 15- to 19-year-old group, but it shows a statistically significant 7 percentage point increase in prevalence of contraceptive methods use in the treated vs. the non-treated eligibles in the 20- to 24-year-old group. This is very close to what is obtained by adding the main effect coefficient for "treatment" to the interaction coefficient between treatment and eligibility in the d-in-d model. It suggests, given the experimental design, that there may have been spillovers from the eligible to the non-eligible population in treatment areas.

Regression discontinuity analysis

As an alternative way to test the robustness of our results, the effect of the treatment was also evaluated by comparing the prevalence of contraceptive methods use among those who were just above or just below the poverty score cutoff point for eligibility, which was estimated to be a score of 752 (see appendix 1): we compare treatment area subjects who are just above vs. those who are just below the cutoff point at windows of varying width: 50, 100, and 150 points around. This strategy allows us to compare eligible vs. non-eligible subjects who have basically the same poverty, so that being eligible (and mostly enrolled) is their only difference (Hahn, Todd and Van der Klaauw 2001; Van der Klaauw 2002). Models were estimated by means of both OLS and a 2SLS regression models adjusted for poverty as indicator for the poverty score being below 752, age and state, with robust standard errors and clustering at the locality level. Table IV shows the results of these models in the young adult group. The program effect estimate is the coefficient on eligibility, interpreted as the adjusted difference in prevalence of contraceptive methods use between eligibles and non-eligibles in a particular window of poverty for the OLS model. In the 2SLS model, it is a Local Average Treatment Effect (LATE) that measures the impact of eligibility for those for whom being just below the cutoff score changed their eligibility status. In opposition to what we found in the ITT analysis, the models for young adults show large, negative, and statistically significant program effects for all four of the estimates using the smaller windows. With the largest window, the effects are still negative though they are not statistically significant. In the adolescent models, results (not shown) are very similar in both OLS and 2SLS approaches, showing a negative but very close to zero effect of the program. One possible explanation for these results is that the program does have an effect but it is not constant relative to poverty, being greater on those who are very poor and being perverse at poverty levels near the eligibility threshold. We estimated, by means of locally weighted scatterplot smoothing (lowess), the relationship between the poverty index and the proportion of contraceptive methods by treatment status, above and below the cutoff point for eligibility. The results are displayed graphically in Figure 1.

It is clearly visible that the strongest effect of the program is on the poorest of the poor. In fact, the prevalence of contraceptive methods use is at its highest (40%) below a poverty score of 550, being even higher than the prevalence in those with the highest poverty score. It is also important to note that the relationship between poverty and contraceptive method use for those not eligible for the program is similar for treatment and control areas, as would be expected. The curious and unexplainable drop in the prevalence of contraceptive methods use just below the cutoff point is responsible for the apparently negative effect of the program, and gives the impression of a strong discontinuity.. Figure 2 shows this same effect by plotting the estimated difference of prevalence of contraceptive methods use between treatment and control areas below the poverty threshold; for this graph a moving windows of 200 subjects was generated and the difference of proportions was estimated at each point. This graph strongly suggests that the program impacts were the largest for those well below the poverty score cutoff.

It is possible that the apparently negative effect of the program in the vicinity of the cutoff point is due to the violations of the cutoff point that occurred in the sample; 2.25 % of subjects above the cutoff point were defined as “eligible” by the implementers of the program. Conversely, 2.64 % of those below the cutoff point were considered “not eligible”. 68% of the “eligibles” above the cutoff point were concentrated in the vicinity (within 50 points) of the cutoff point. Furthermore, as explained in pages 8-9, local administrators played a role on defining which households were defined as eligible because of their perceived economic or social characteristics, regardless of their poverty score, which would mean that this misclassification is very likely endogenous.

Additional analysis: children of school age

The analyses presented so far indicate a positive impact of the program on the use of contraceptive methods among young adults but not among adolescents, as shown in our ITT effect estimates. Results also indicate that the effect of the program is larger among poorer individuals. It is also of interest to find some evidence on the mechanisms by which the program could impact the use for contraceptive methods. In order to do it, we estimated models that included information on the number of school age kids for each woman. Women who have children of school age are more likely to benefit from the program. This is due to the fact that they receive cash transfers for each child who attends school and are therefore more motivated to attend health talks. The program might have a larger effect on this group of women. However, they might also be the group of women who have the strongest desires to control fertility.

To test this hypothesis, we fit (by OLS) a model of contraceptive use including a dummy variable for having children of school age and its interaction with a dummy variable for treatment area as explanatory variables (the "simple" model). A second model evaluated the same hypothesis but restricted it to those women who were eligible for the program. Yet another model featured a triple interaction among children in school, treatment assignment, and eligibility.[§]

The results from these models appear in table V. In the d-in-d model approach, it is seen that among those women who do not have children, those in treatment areas have a 3 percentage point greater prevalence of contraceptive use than those in control areas. However, those who have children in treatment areas have a 10 percentage point greater prevalence than those in control areas (=.03+.07). If the model is restricted to eligibles, the difference between treatment and control areas is 5.1 percentage points in those with no children, and an additional 6.0 percentage points higher for those with children.

The d-in-d-in-d model in the last column of table V shows that the interaction between being in a treatment area and being eligible and having children in school is negative. The standard errors from this model, unfortunately, are too large to make any strong statement.

Additional analysis: "panel" subset

As another way to test the robustness of our program effect estimates, we analyzed contraceptive use information on the 812 women who had information in both 1997 and 2000. Even though this subsample is clearly not representative of the whole population, we considered it interesting

[§] This model was only estimated for 20- to 24-year-old women, as none in the adolescent group had children older than 5 years of age.

to evaluate the impact of the program in women who already were in charge of children at the beginning of the program. We estimated the following model by means of OLS:

$$y_{i,2000} - y_{i,1997} = \beta_0 + \beta_1 T_i + \beta_2 E_i + \beta_3 T_i E_i + \varepsilon_i \qquad (2)$$

Where $y_{i,2000}$ is a dummy for current contraceptive use as of 2000, and $y_{i,1997}$ indicates current contraceptive use as of 1997. T_i is a dummy designating assignment to treatment or control area of the i-th individual and E_i is an indicator of eligibility status for the i-th individual. This model provides an estimate of the program effect adjusted for all individual level variables (since both time-invariant observables and unobservables of the individual cancel out). The model features an interaction between treatment and eligibility, the d-in-d estimate being an estimate of the program effect. It tells us how the over time change in contraceptive use was different for eligibles in treatment areas than for eligibles in control areas. Note that we also include level eligibility and treatment area effects. These help control for differences in the rate of change in the two types of areas and for two types of people in the absence of the treatment.

We fit this model using 1) data from all women, and 2) only women who were aged 20 to 24 in the year 2000.[**] The results appear in table VI. The d-in-d estimates that were obtained show program impacts of 8.4 and 7.8 percentage points on the increase of contraceptive methods use between the years 1997 and 2000. These estimates are quite consistent to those obtained in other approaches, especially the ITT ones, our main effect estimates. The fact that they are more imprecise is likely due to the much smaller sample size of the panel subset (812 subjects).

[**] There were too few adolescents who were using contraceptives in both time periods (and especially in 1997) for a meaningful separate analysis.

To address the potential selective attrition problem (only 812 out of 1,480 women originally interviewed for contraceptive methods use were re-interviewed in 2000) , we fitted a logit regression model (results not shown) and found that attrition was related only to speaking an indigenous language and the poverty score; those who spoke an indigenous tongue were half as likely to become lost and being poor was related to a higher likelihood of being lost. This last finding is interesting since the impact of the program appears to be largest in the poorest of the poor (regression discontinuity analysis finding), which means that it is likely that the program impact in this particular sub-sample is underestimated. Attrition rates were not different between treatment and control areas. No differences between those who were lost and those who continued were found with regard to age, literacy, marital status, schooling and having a job. A global test on interaction term coefficients of the variables mentioned above and the dummy variable for treatment showed no significant differences in the attrition process between treatment and control areas (p=0.22).

DISCUSSION AND CONCLUSIONS

Using the ITT effect estimates as our main impact estimates, this study found no impact of the program on the contraceptive method use among adolescents aged 15 to 19 years. In contrast, the difference-in-difference estimate revealed a 5 percentage point increase in the contraceptive method use among young adult women aged 20 to 24 years. We found similar results when using other analytical techniques. Recall that the program impacts would most likely operate through the health information talks presented by *Oportunidades*. Over the time period of this study, adolescent women were not required to attend the health talks, and this could be a reason why the program appeared to have no impact on them. The young adults, however, were required to attend these talks and the program effects on contraceptive use were moderately sized and often statistically significant. An additional analysis in older women (25-29 year olds, results not shown) also find a positive, albeit smaller (3 percentage points), impact of the program, which constitutes additional evidence supporting our hypothesis.

Another issue is that the prevalence of sexual activity in adolescents may be lower than in older women. Although we have no data on actual sexual activity in the study subjects, the proportion of women who have a spouse is much lower in the adolescent group (≈10%) than in the young adult group (≈60%) (Table II). Adolescents with no sexual activity would not show an effect of the program on contraceptive methods use because they would not be susceptible of using contraceptives.

The finding of an impact on the use of contraceptive methods among young adults is consistent with the results of Huerta and Hernández (2000). In their study, those authors found a positive impact of the program, especially among women 20- to 24-years-old, although their analysis was restricted to women with a partner. Results are somewhat consistent with the findings of Hernández-Prado et al (2005), who found a positive impact of *Oportunidades* in the use of family planning methods among women of reproductive age (15- to 49-years-old) with a partner, although in that analysis there was no stratification by age. However, our analysis makes an important contribution to the evaluation or Oportunidades, while comparing the impact of the program on adolescents vs. young adults, finding only an effect in the latter group, and by identifying a higher impact of the program on the poorest women.

Since information on reproductive health variables in the year 1997 was collected mostly for household heads or women in charge of the household's children, the prevalence of contraceptive methods in the 1997 sample was much higher than that in the year 2000. However, data for the 812 informants who were subsequently interviewed in 2000 also shows that the increase in contraceptive method use in for them after two years of exposure to the program was almost 8 percentage points higher in treatment communities than in control communities. This is quite close to the effect we found in the ITT estimate obtained through d-in-d, and suggests that the program had basically the same impact .on women already in care of children in the year 1997.

The effect of the program on those women who bear school-aged children was larger; this could be due to the fact that women who have school-aged children are more motivated to attend the health talks in order not to lose the monetary transfers that they receive, but the d-in-d-in-d estimator was too inaccurate for us to reach a strong conclusion about this pathway.

The results show that, even though the effect of the program is greater for those who were eligible to receive the program, there could also be some residual impact through an overall improvement of the treatment communities. Although the model restricted to eligibles shows a 7 percentage point increase in contraceptive method use in those residing in treatment areas relative to controls, the d-in-d estimator only yielded a 5 percentage point increase (Table III). According to the latter model, among those not eligible to receive the program, those in treated areas had a 2 percentage point increase in contraceptive methods use relative to residents in control areas. Because of the experimental assignment, this could signify an overall improvement in access to contraceptive methods in the treatment communities or information spill-over from eligibles to non-eligibles. It could possibly be an important spillover effect, though it is somewhat small.

We conducted a regression discontinuity analysis as an alternative analytical technique to check the robustness of our impact estimates of the ITT using d-in-d methods, our preferred impact estimate. The results of the regression discontinuity analysis reveal an important differential impact of the program depending on poverty level. The strongest effect of the program is on the poorest of the poor. In fact, the prevalence of contraceptive methods use is at its highest (40%), for those below a poverty score of 550. This 40% rate is higher than the prevalence in those with the highest poverty score. Thus, it is mainly on the poorest subjects where the impact of the program takes place (Figure 2). In fact, for the poorest subjects the increase in contraceptive use prevalence was greater than 20 percentage points. The reason for this result is perhaps that the poorest people have a deeper commitment to the program since they are more economically dependent on it, and therefore they would be willing to assist to all health talks and follow their recommendations. By contrast, for those who are near the poverty (eligibility) threshold, the program did not appear to have an impact and it might even have been negative.

One limitation of this study is the fact that we did not distinguish between women who had initiated an active sexual life and those who had not. Moreover, since talk on contraceptive use is a delicate subject in rural areas in Mexico, it is possible that some under-reporting occurred. However, it is unlikely that our results are biased since randomization appeared to adequately balance treatment and control groups. In any case, since health promotion talks may lead young women to delay the onset of sexual activity (and therefore contraceptive use would diminish since there would be no need for it), it is likely that our results, at least in the adolescent group, are underestimates of the impact of the program in women with sexual activity. Moreover, in the young adult group, the proportion of women who cohabited with their partner was quite similar between treatment and control communities; therefore it is expected that the proportion of women with active sexual life is also similar. If that is the case, our results would not be biased. Additionally, a potential selection bias caused by attrition did not appear to be an issue in these data (see appendix 2).

The fact that the program had an impact on contraceptive method use among young adults but not among adolescents, and that this effect may have been due to health talks, gives support to the change in policy from program administrators in 2001. This change in policy consisted in implementing mandatory health promotion talks for all *Oportunidades*-enrolled adolescents attending high school.

While we have focused in this paper in the ITT d-in-d estimate as our main impact estimate, the various estimation procedures yielded very similar results, which gives us confidence that the program indeed had an impact on contraceptive methods use in the range of 5 to 10 percentage points. Although there was an underlying experimental design, the experiment may not have been perfect. For example, there was assignment of treatment at community rather than individual or household level, the fact that there were eligible and non-eligible subjects in each community, and possible contamination over time. These and other issues make it important to try different estimation procedures to see if they yield results different from the simple comparison. Our estimates are mostly intent-to-treat estimates, and they are likely less interesting than average treatment effects on the treated. This would require us to use data on actual enrollment rather than just eligibility to the program, with the difficulty being that enrollment would clearly be endogenous.

In conclusion, we have presented an ITT estimate of effect that suggests that *Oportunidades* increased the prevalence of contraceptive method use after two years of exposure to the program for young adult women, which was consistent with the findings of different estimation methods. The impact appears to have been much larger among the poorest of the poor. However, it had no discernible impact on adolescents aged 15 to 19 years. The effect of the program is likely to have operated through subjects' attendance to health talks, but at least part of this impact could reflect an overall improvement in access to contraceptive methods in the treatment communities.

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Table I. Descriptive statistics for study subjects in 1997.

	12 to 16 years of age								17 to 21 years of age							
	Control areas			Treatment areas					Control areas			Treatment areas				
Variable	N	$\mu^{‡}$	σ	N	$\mu^{‡}$	σ	p*	p **	N	$\mu^{‡}$	σ	N	$\mu^{‡}$	σ	p*	p **
Poverty score	855	743.00	144.37	1198	729.28	137.39	0.08	0.03	769	762.21	136.24	1182	755.08	128.58	0.67	0.27
Age (years)	855	14.24	1.21	1198	14.37	1.16	0.01	0.01	776	18.90	1.43	1182	18.97	1.38	0.30	0.26
Literacy †	855	0.95	0.22	1198	0.95	0.22	0.83	0.59	776	0.88	0.32	1180	0.88	0.32	0.85	0.13
Currently attends school? †	851	0.53	0.49	1191	0.54	0.50	0.48	0.26	763	0.06	0.24	1168	0.09	0.28	0.03	0.01
Has a job? †	853	0.08	0.27	1196	0.12	0.32	<0.01	0.08	776	0.17	0.37	1179	0.17	0.37	0.89	0.58
Has a spouse? †	845	0.06	0.24	1190	0.05	0.22	0.14	0.09	772	0.50	0.50	1180	0.52	0.49	0.35	0.40
Number of children	855	0.03	0.18	1198	0.02	0.13	0.09	<0.01	776	0.60	0.85	1182	0.66	0.85	0.08	0.91
Number of children of school age	855	0.00	0.00	1198	0.00	0.00	--	--	776	0.01	0.11	1182	0.02	0.12	0.67	0.86
Currently uses contraceptives? †	36	0.19	0.40	35	0.17	0.38	0.80	--	292	0.32	0.47	449	0.31	0.46	0.68	0.10

† Dichotomous variable, 1=yes, 0=no. ‡ Arithmetic mean for numeric variables, proportion for dichotomous variables. *Mann-Whitney test for numeric variables,$\chi 2$ for dichotmous. ** p value from multiple logistic regression model where treatment assignment is the outcome, robust standard errors with clustering at locality level were calculated. Global test of significance: p=0.02 for 12-16 yo; p=0.17 for 17-21 yo.

Table II. Descriptive statistics for study subjects in the year 2000.

Variable	15 to 19 years of age							20 to 24 years of age						
	Control areas			Treatment areas				Control areas			Treatment areas			
	N	μ‡	σ	N	μ‡	σ	p*	N	μ‡	σ	N	μ‡	σ	p*
Poverty score	968	746.59	143.66	1378	731.49	136.83	0.04	873	761.11	137.99	1357	752.85	129.21	0.45
Age (years)	968	16.98	1.32	1378	17.04	1.27	0.31	873	22.02	1.47	1357	22.07	1.42	0.47
Currently attends school? †	952	0.27	0.44	1343	0.32	0.47	0.01	855	0.05	0.28	1344	0.03	0.17	0.09
Has a job? †	950	0.15	0.35	1341	0.13	0.34	0.30	860	0.14	0.35	1344	0.12	0.32	0.35
Has a spouse? †	706	0.11	0.31	1022	0.09	0.29	0.33	655	0.57	0.49	1006	0.62	0.48	0.02
Number of children	968	0.21	0.73	1378	0.16	0.55	0.03	873	1.26	1.29	1357	1.36	1.30	0.07
Number of children of school age	968	0.00	0.03	1378	0.00	0.07	0.50	873	0.24	0.54	1357	0.26	0.55	0.29
Currently uses contraceptives? †	968	0.03	0.17	1378	0.03	0.17	0.89	873	0.19	0.39	1357	0.23	0.42	0.02

† Dichotomous variable, 1=yes, 0=no.

‡ Arithmetic mean for numeric variables, proportion for dichotomous variables.

* Mann-Whitney test for numeric variables, $\chi 2$ for dichotomous.

Table III. Intent-to-treat effect estimates. All models are OLS.

	Model I Simple, 2000		Model II Diff-in-Diff		Model III Only eligibles 2000	
	15 to 19	20 to 24	15 to 19	20 to 24	15 to 19	20 to 24
Treatment×eligible			0.009	0.049		
			[0.015]	[0.036]		
Treatment (1=treatment area, 0=control area)	0.002	0.046	-0.003	0.021	0.004	0.070
	[0.008]	[0.022]*	[0.013]	[0.029]	[0.009]	[0.027]*
Eligible (1=eligible, 0=non-eligible)			-0.026	-0.03		
			[0.017]	[0.037]		
Age (years)	0.023	0.029	0.023	0.029	0.019	0.039
	[0.003]**	[0.006]**	[0.003]**	[0.006]**	[0.004]**	[0.008]**
$\log_e$(poverty score)	0.045	-0.02	-0.018	-0.018	-0.042	-0.058
	[0.034]	[0.088]	[0.056]	[0.134]	[0.120]	[0.355]
$\log_e(\text{poverty score})^2$	0.04	-0.142	-0.052	-0.148	-0.572	2.917
	[0.142]	[0.345]	[0.157]	[0.357]	[1.161]	[3.117]
$\log_e(\text{poverty score})^3$	-0.305	-0.362	-0.107	-0.386	-1.737	11.842
	[0.160]	[0.572]	[0.204]	[0.656]	[3.350]	[7.539]
$\log_e(\text{poverty score})^4$	-0.604	0.167	-0.284	0.156	-1.649	10.063
	[0.428]	[1.056]	[0.482]	[1.135]	[2.824]	[4.913]*
Constant	-0.379	-0.633	-0.361	-0.616	-0.335	-0.895
	[0.056]**	[0.129]**	[0.056]**	[0.132]**	[0.071]**	[0.180]**
Observations	2346	2230	2346	2230	1323	1146
R-squared	0.03	0.05	0.03	0.05	0.03	0.08

Robust standard errors in brackets

* significant at 5%; ** significant at 1%

Adjusted for state, clustering at locality level.

Table IV. OLS and 2SLS regression models of contraceptive methods use among 20 to 24 year old women in treatment areas, with varying poverty score window width. In 2SLS models *Eligibility* was instrumented by an indicator of the individual being below the poverty score cutoff point used to determine eligibility for most households.

	50 point window		100 point window		150 point window	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Eligible (1=eligible, 0=non-eligible)	-0.224	-0.218	-0.179	-0.173	-0.088	-0.105
	[0.087]*	[0.107]*	[0.070]*	[0.080]*	[0.063]	[0.066]
Age (years)	-8.413	-8.3	-4.565	-4.513	-1.384	-1.49
	[6.711]	[6.808]	[1.704]**	[1.739]*	[0.677]*	[0.691]*
$\log_e$(poverty score)	55.187	55.336	22.5	22.608	4.56	4.35
	[66.903]	[66.896]	[16.250]	[16.289]	[5.881]	[5.875]
$\log_e(\text{poverty score})^2$	0.01	0.010	0.023	0.023	0.031	0.031
	[0.015]	[0.015]	[0.012]	[0.012]	[0.011]**	[0.011]**
Constant	0.394	0.384	-0.05	-0.055	-0.372	-0.362
	[0.394]	[0.412]	[0.286]	[0.290]	[0.266]	[0.266]
Observations	280	280	487	487	652	652
R-squared	0.08	0.08	0.08	0.08	0.07	0.07

Robust standard errors in brackets

* significant at 5%; ** significant at 1%

Adjusted for state, clustering at locality level.

Table V. Effect of the program according to having at least one children of school age. All models are OLS.

	D-in-D	D-in-D Only eligibles	D-in-D-in-D
Treatment×eligibility×children in school			-0.027
			[0.110]
Treatment×children in school	0.073	0.060	0.082
	[0.051]	[0.059]	[0.096]
Treatment×eligibility			0.041
			[0.037]
Eligibility×children in school			0.003
			[0.081]
Treatment (1=treatment area, 0=control area)	0.03	0.051	0.011
	[0.022]	[0.028]	[0.030]
Children in school? (1=yes, 0=no)	0.13	0.119	0.133
	[0.040]**	[0.046]**	[0.074]
Eligibility (1= yes, 0=no)			-0.030
			[0.038]
Age (years)	0.013	0.020	0.013
	[0.006]*	[0.009]*	[0.006]*
Constant	-0.308	-0.492	-0.291
	[0.130]*	[0.186]**	[0.134]*
Observations	2230	1146	2230
R-squared	0.07	0.1	0.07

Robust standard errors in brackets

* significant at 5%; ** significant at 1%

Adjusted for state and a fourth degree polynomial of the log of poverty score, full set of estimates is available from the corresponding author on request. Clustering at locality level.

Table VI. OLS models of the impact of the program on the change in contraceptive methods use between 1997 and 2000.

	All women	20- to 24-year-old in 2000
	D-in-D	D-in-D
Treatment×eligible	0.084	0.078
	[0.080]	[0.090]
Treatment (1= treated, 0=control)	0.058	0.074
	[0.061]	[0.065]
Eligibility (1=eligible, 0=non-eligible)	-0.037	0.003
	[0.065]	[0.073]
Constant	-0.013	-0.042
	[0.050]	[0.053]
Observations	812	705
R-squared	0.01	0.01

Robust standard errors in brackets

* significant at 5%; ** significant at 1%

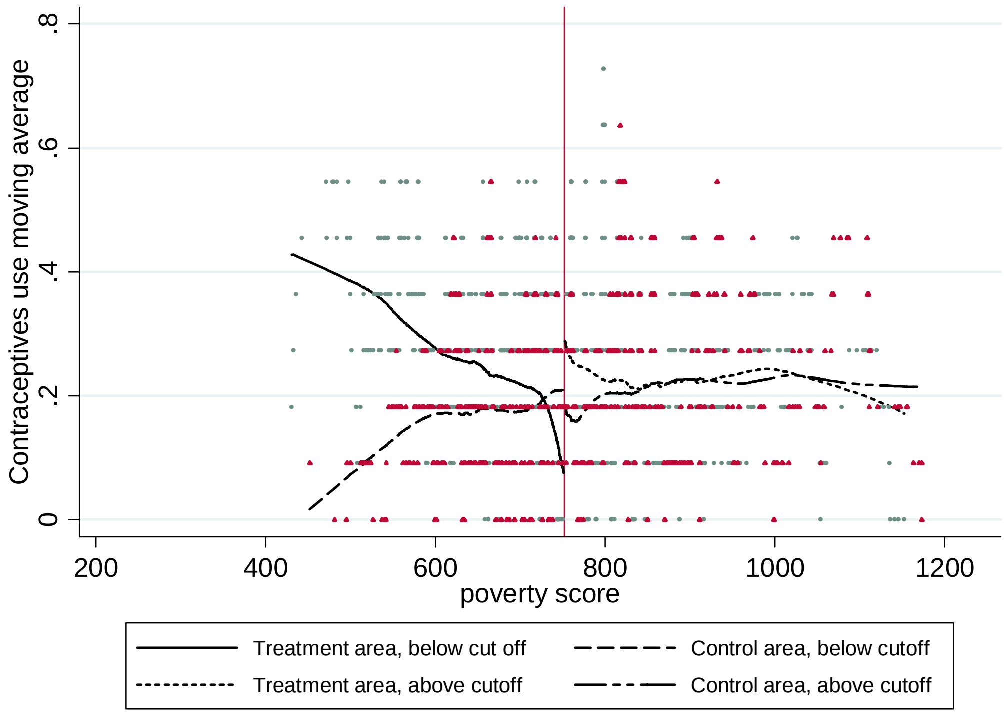

Figure 1. Relationship between the poverty index and use of contraceptive methods by treatment area. Separate lowess curves were plotted for treatment and controls above and below the cutoff point for eligibility.

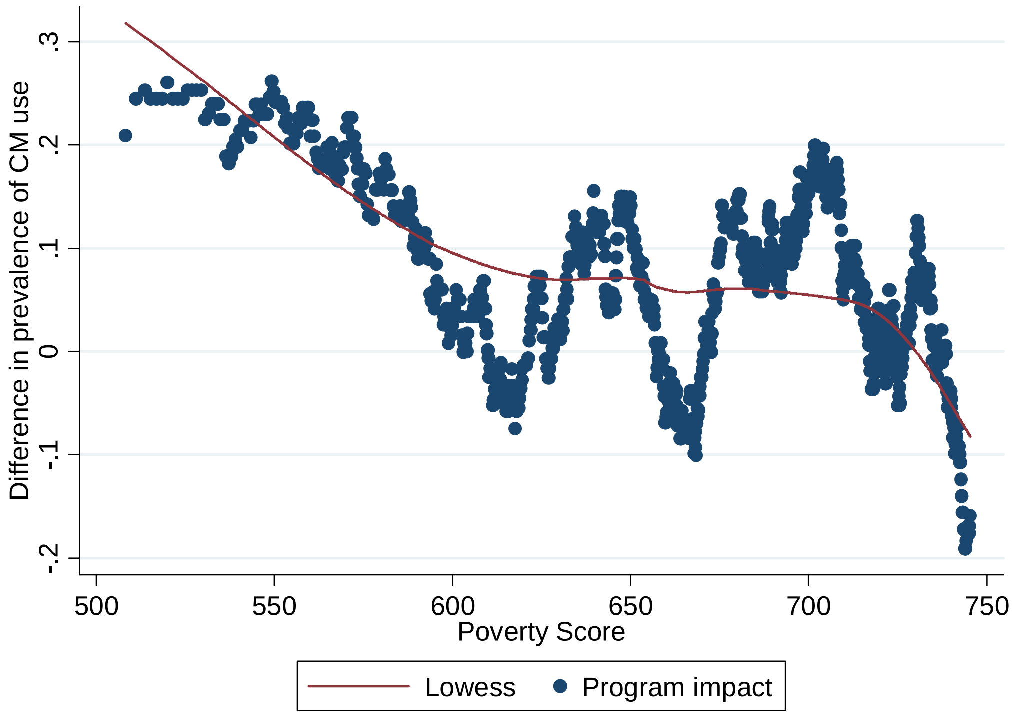

Figure 2. Intent-to-treat effect of the program by poverty index. Each dot represents an estimate of the difference in prevalence of contraceptive methods use between treatment and control areas in a moving window of 200 observations.

APPENDIX 1. DEFINING THE CUT-OFF POINT FOR ELIGIBILITY

Eligibility to the program was defined as living in a household below a particular threshold of the poverty score index calculated by the implementers of the program. Unfortunately, the actual value of the threshold was not available for us. Therefore, in order to estimate the cutoff point we performed a discriminant analysis of the data where the group variable was "eligible", and the discriminatory variable is the poverty score. The discriminant analysis generates a predicted grouping variable according to the discriminatory variable. By sorting the predicted grouping variable, one can find the value that minimizes misclassification, which is the best cutoff point that can be estimated from the data.

The cutoff point that minimized misclassification obtained from the discriminant analysis was 752. Using this cutoff point, only 3.23% of the subjects were misclassified; that is, were assigned to being "eligible" or "non-eligible" by criteria other than the poverty score. Figure A1 shows evidence of a sharp discontinuity in eligibility about the estimated cutoff point even though some misclassification is apparent.

APPENDIX 2. SAMPLE ATTRITION

To evaluate the possibility of selective attrition, we studied baseline characteristics of all women in the 12-21 years age range in 1997, regardless of them completing the reproductive health questionnaire. We defined attrition as a dummy variable where 0 designates women who were re-interviewed in the year 2000 survey, and 1 indicates those who were lost. We obtained descriptive statistics according to attrition status and treatment area assignment. Results are shown in table A1. We found that those who were lost were more likely to be attending school in 1997, however this difference was very similar in both treatment and control areas.

We fitted a logit model of attrition (table A2) and found that those who were married, those who were attending school in 1997 and those who were younger were less likely to be re-interviewed in the year 2000, however, the attrition process did not appear to be different in treatment and control areas, as all treatment×baseline variable interaction terms were not statistically significant. Additionally, a global test of significance for this interaction term was also not significant (p=0.43). This result appears to rule out potential selection bias due to attrition.

Table A1. Subject characteristics by treatment area and attrition status in 1997.

	Control areas						Treatment areas					
	Re-interviewed			Lost			Re-interviewed			Lost		
Variable	N	μ	σ	N	μ	σ	N	μ	σ	N	μ	σ
Has a spouse? †	4290	0.16	0.37	1166	0.15	0.35	6391	0.17	0.37	2042	0.15	0.36
Has a job? †	4319	0.16	0.37	1175	0.12	0.33	6416	0.18	0.39	2065	0.16	0.36
Currently attends school? †	4293	0.31	0.46	1168	0.46	0.50	6374	0.32	0.47	2050	0.46	0.50
Any schooling?	4326	0.95	0.23	1179	0.93	0.26	6411	0.94	0.23	2061	0.92	0.26
Literacy †	4328	0.93	0.25	1180	0.90	0.29	6420	0.93	0.25	2065	0.91	0.29
Speaks an indigenous language?†	4324	0.27	0.45	1171	0.26	0.44	6405	0.28	0.45	2057	0.31	0.46
Age (years)	4330	16.20	2.64	1180	15.11	3.04	6424	16.27	2.64	2066	15.03	3.01
Poverty score	4296	735.24	144.95	1176	719.56	144.51	6422	723.22	139.77	2065	713.98	143.57

† Dichotomous variable, 1=yes, 0=no.

Table A2. Logit model for attrition (1=lost, 0=re-interviewed). Variables are baseline (1997) characteristics.

Variables	β	SE
Treatment area †	0.177	[0.571]
Has a spouse? †	0.504	[0.124]**
Has a job? †	-0.028	[0.126]
Currently attends school? †	0.419	[0.089]**
Any schooling?	-0.231	[0.239]
Literacy †	-0.400	[0.221]
Speaks an indigenous language?†	-0.134	[0.098]
Age (years)	-0.144	[0.020]**
Poverty score	-0.002	[0.000]
Has a spouseXtreatment area	0.061	[0.170]
Has a jobXtreatment area	0.092	[0.154]
Currently attends schoolXtreatment area	-0.114	[0.114]
Any SchoolingXtreatment	0.191	[0.324]
LiteracyXtreatment	-0.010	[0.289]
Indigenous languageXtreatment	0.235	[0.131]
AgeXtreatment	-0.041	[0.027]
Poverty scoreXtreatment	0.001	[0.000]
Constant	1.768	[0.343]**
Observations	13665	

Robust standard errors in brackets, estimates are state-adjusted.
* significant at 5%; ** significant at 1%
Global test of significance for interaction terms: p=0.43

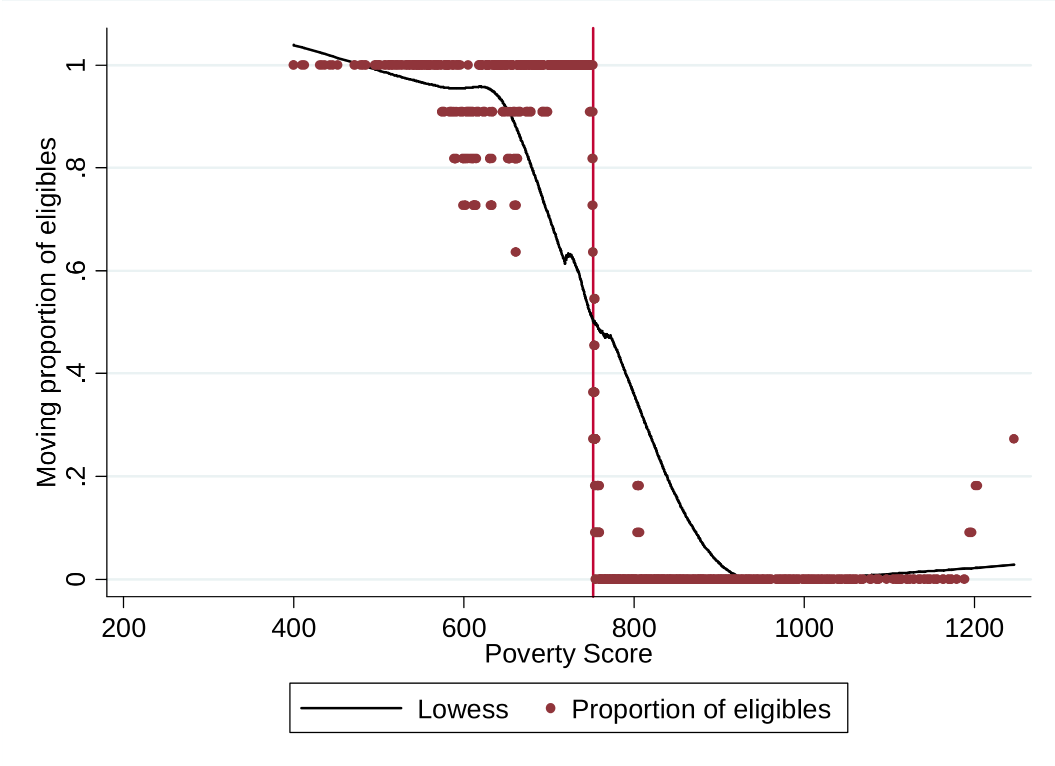

Figure A1 Moving average of the dummy variable for eligibility (1= eligible, 0=otherwise) by poverty score. Vertical line marks a poverty score of 752, the estimate of the cutoff point for eligibility status.