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April 2008

WP-08-102

MEASURE Evaluation is funded by the U.S. Agency for International Development (USAID) under terms of Cooperative Agreement GPO-A-00-03-00003-00 and is implemented by the Carolina Population Center at the University of North Carolina in partnership with Constella Futures, John Snow, Inc., Macro International Inc., and Tulane University.

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Printed on recycled paper

This working paper series is made possible by support from the U.S. Agency for International Development (USAID) under Cooperative Agreement No. GPO-A-00-03-00003-00. The opinions expressed are those of the authors, and do not necessarily reflect the views of USAID or the U.S. government.

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Impact of *Oportunidades* on skilled attendance at delivery in rural areas

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Acknowledgements:

We acknowledge the support of the Oportunidades Evaluation Division for the data collection of information used in this paper, access to databases and clarification on data collection and operation of the program procedures. This analysis is an extension of a first analysis of impact of Oportunidades on skilled attendance at delivery conducted with economic support from Oportunidades. This study was conducted with funds provided by MEASURE Evaluation, a Cooperative Agreement (GPO-A-00-03-00003-00) between the U.S. Agency for International Development and the Carolina Population Center. The views presented herein are those of the authors and not of the sponsoring agency.

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ABSTRACT

Oportunidades (formerly Progresa) is a conditional cash-transfer program run by the Mexican federal government aimed to break the inter-generational cycle of poverty, which among other activities, provides free delivery attendance for women enrolled in the program. Skilled attendance to delivery has been identified as an effective strategy to reduce maternal mortality. In this paper, we assess the impact of Oportunidades on skilled attendance to delivery, taking advantage of the experimental design implemented for the evaluation of this program in rural areas

The main results of the study indicate that Oportunidades had, at best, only a small effect on skilled attendance at delivery in treatment communities. The effect of the program, however, appears to be higher for women with a poverty score near to the eligibility cut-off point, whereas it seems not to have an effect on women in the poorest households. We also find that the program had larger effects on those women who had one birth just prior to the experimental treatment and another birth subsequent to the experimental treatment. However, the impacts of the program seem to be null or even negative when comparing enrolled vs. non-enrolled women in intervention areas. These results should lead to a review about the strategies used by Oportunidades to increase skilled attendance to delivery.

Introduction

Mexico's maternal mortality ratio of 63.3 per 100,000 live births per year in 2005 (Secretaría de Salud, 2006) appears high when compared with other countries of similar or lower economic development. Given its importance as a public health problem as well as a commitment to the Millenium Goals, the Mexican health sector has begun to implement several strategies to reduce maternal mortality. Most importantly, there is now an explicit focus on increasing skilled attendance at delivery as a preventive strategy by health sector institutions. Skilled attendance is defined as the attendance at a delivery by skilled personnel under conditions that allow the provision of quality delivery services. It has been identified as an effective intervention to reduce maternal mortality (Graham, Bell & Bullough, 2001).

Among several strategies adopted by the Mexican health sector, the Human Development Program Oportunidades *(*previously Progresa) plays an important role. Oportunidades is the most important strategy of the Mexican government to improve living conditions of poor population. A key aspect of the program includes efforts to improve health of mothers and children through the provision of skilled attendance to delivery. While the impact of Oportunidades on several other reproductive health outcomes has been documented, there is little information about the impact of this program on the incidence of deliveries with skilled attendance among the beneficiary population. The objective of this paper is to assess the impact of Oportunidades on skilled attendance to delivery in rural areas through the application of a variety of evaluation techniques, taking advantage of the experimental design implemented for the evaluation of this program in rural areas.

The main results of the study indicate that Oportunidades had, at best, only a small effect on skilled attendance at delivery in treatment communities. The effect of the program, however, appears to be higher for women with a poverty score near to the eligibility cut-off point, whereas it seems not to have an effect on women in the poorest households. The program appears to have had a larger effect on the relatively high fertility women who had one birth just prior to the experimental treatment and another subsequent to the experimental treatment. However, the impacts of the program seem to be null or even negative when comparing enrolled vs. non-enrolled women in intervention areas.

Background

Oportunidades is a conditional cash transfer program that started in rural areas as Progresa in 1997. 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. Its aim is to improve the education, health, nutrition, and living conditions of populations in extreme poverty and to break the intergenerational cycle of poverty. By 2005, the program had enrolled 5 million families in urban and rural areas, containing more than 25 million people across the country.

Since the beginning of its operations, the program has worked through three major components (Secretaría de Desarrollo Social, 1997):

a) The program provides cash transfers to families for children attending school. These payments vary according to the number of children in the family, and their age and gender. Payments are larger for girls in higher education grades. These cash payments are made to the female head of household, and they are conditioned on compliance with the attendance of children at school and other responsibilities.

b) In the area of health, the program offers an essential health care package that includes among its activities pregnancy and delivery care for women enrolled in the program. Health care is provided either by the Ministry of Health or by the Mexican Institute of Social Security (IMSS-Oportunidades). Whether the Ministry of Health or IMSS-Oportunidades provides services depends on the institution responsible for health care in each locality. The program also includes a series of health promotion talks that are presented monthly by personnel of the

Ministry of Health and IMSS-Oportunidades. In the area of reproductive health, these talks include information on family planning, prenatal care, alarm signs during pregnancy, and newborn care. In the case of delivery attendance, health institutions are responsible for providing, as part of the essential health package of Oportunidades, delivery attendance in their facilities. Attendance at the health promotion talks and to medical checkups are a co-responsibility of the family, and are requirements for continued enrollment in the program and receipt of the cash payments.

c) In the area of nutrition, the program distributes nutritional supplements with minerals and vitamins for all children under 2 years old, pregnant or lactating women, and undernourished children 2-4 years old.[1]

Oportunidades focuses on families living in extreme poverty. To identify families to be admitted to the program in rural areas, program administrators selected areas with high concentrations of poor households that also already had schools and health care facilities available for the implementation of the program. They then conducted a census of households in those areas and applied a survey questionnaire that enabled them to construct an eligibility score for each household. The eligibility assessment also included a visual inspection of the household's characteristics. Oportunidades constructed the eligibility score based on family and household characteristics. Families with a score below a program-determined threshold were considered eligible to participate in the program. As an additional step, the eligibility status results were validated in a community assembly. Families deemed eligible received further information about the

[1] In recent years, Oportunidades added a component of support to senior adults and a savings fund for youth.

benefits and requirements of the program and were offered to be enrolled. Nearly all eligible households in participant localities agreed to comply with the eligibility rules and co-responsibilities of the program; the refusal rate was only 3% (Orozco, Parker & Hernández, 2000). It is necessary to keep in mind that the cutoff point for eligibility to the program was defined by the administrators of the program and varied by region of the country (Skoufias, Davis & Behrman, 2000).

Oportunidades may increase the proportion of deliveries with skilled attendance through several mechanisms. First, women in incorporated households should receive free delivery attendance as part of the essential health package included in the program (either through the Ministry of Health or the IMSS-Oportunidades), as long as they continue to comply with the co-responsibilities of the program and make at least five prenatal care visits. Women who are not in Oportunidades may have to pay for delivery attendance at public hospitals. Second, Oportunidades may increase skilled delivery attendance through an income effect, because the cash transfers may motivate a higher demand of skilled attendance, either in private or public facilities. Third, the health promotion talks may increase the contact of women with the health services providers. This likely provides more information about care during pregnancy, and it may increase the likelihood of having a birth inside a medical unit.

Maternal mortality has shown a reduction in Mexico, falling 12.7% between 1995 and 2000 and 12.8% between 2000 and 2005 (Secretaría de Salud, 2006). The role of social and health programs to reduce poverty and to improve health conditions of the poor population has been identified as a key factor to reach the Millenium Goals (Torres & Mújica, 2004). In addition, skilled attendance at delivery has also increased in

Mexico, reaching 74.2% of all births by 2004 (Dirección General de Información en Salud, 2005). Improving the availability and use of essential obstetric care and ensuring skilled attendance to deliveries are two of the main strategies recognized as effective approaches for reducing maternal mortality (Donnay, 2000). Both strategies are among the actions of Oportunidades.

Several studies have documented positive impacts of Oportunidades in rural and urban areas in the area of education (Parker, 2003; Parker, 2005; Parker, Behrman & Todd, 2005; Todd, Gallardo-Garcia, Behrman & Parker, 2005; Behrman, Parker & Todd, 2005), consumption (Attanasio & Di Maro, 2005; Angelucci, Attanasio & Shaw, 2005), and nutritional status (Rivera, Sotres, Habitch, & Villalpando, 2004). In the area of health, the evaluations indicate that Oportunidades has lead to a reduction in the number of episodes of disease and an increase in the use of health services among the beneficiary population (Gutiérrez, Bautista, Gertler, Hernández & Bertozzi, 2005; Bautista, Martínez, Bertozzi & Gertler, 2003). In the area of reproductive health, several studies have documented a positive impact of Oportunidades in the knowledge and use of family planning methods and antenatal care. (Huerta & Hernández, 2000; Hernández et al., 2005b)

So far, no study has analyzed in detail the impact of Oportunidades on skilled attendance. The only analysis of the impact of the program on this outcome was descriptive and found no differences in the overall proportion of deliveries attended by physicians or in medical units in rural or urban areas (Hernandez et. al. 2005b). This paper examines the impact of Oportunidades on skilled attendance in more detail, through the application of more rigorous program effect estimation techniques.

Data

Since its inception, Oportunidades considered the need to carry out evaluations of the program. To assist with this, in rural areas an experimental design was implemented to assess the impact of the program. This analysis uses data sets designed for this evaluation. Some rural localities were initially randomized to have families eligible to enroll in the program in early 1998 while other localities were randomized to be control areas. A total of 320 localities where defined as intervention localities, and 186 as controls in seven of the states where the program started operations in 1997 (Guerrero, Hidalgo, Michoacán, Puebla, Querétaro, San Luis Potosí, and Veracruz).

A first survey, called the *Encuesta de Características Socioeconómicas de los Hogares 1997* (Encaseh 97) collected basic information from households that was required for classifying households as eligible for the program or not. This information was collected in both intervention and control localities between October and November 1997, before the random assignment of communities to treatment and control areas. Panel surveys called *Encuesta de Características de los Hogares* (Encel) collected information in two rounds per year in 1998, 1999, and 2000. The first round in 1998 (Encel 1998) constitutes, along with the Encaseh 97, the baseline of the evaluation.

Enrollment of households into the program started in intervention localities in 1998. Starting in August of 1999, households in control areas became eligible to enter the program. By 2000, all localities in the control group had been incorporated in the program (see Figure 1). Because of this enrollment of the control sample into the program, one can only use the experiment to measure impacts between January 1998 and July 1999.

The eligibility status of a household was determined by the administrators of the program through the estimation of an eligibility score and the validation of results in a community assembly. Consequently, eligibility status does not exactly match the criterion of having a poverty score under the eligibility cutoff point. Likewise, since the decision to participate was made voluntarily by eligible households, actual enrollment does not match the condition of eligibility. The distinctions among these variables (the eligibility status according to the eligibility score and cutoff point, the eligibility status according to the program administrator and community criteria, and the actual enrollment to the program) are part of our empirical analysis.

Fertility histories, including type of attendant at delivery, were collected in the Encel 1998, and in the first follow-up round of the Encel 2000. This information was provided typically by the woman in charge of the care of children in the household, and in some cases by other women of reproductive age (15 to 49 years old). Table 1 shows the number of households included in the analysis. Out of 24,077 households in the baseline survey, 20,493 were observed in the 2000 follow-up survey (Encel 2000), which represents a follow-up rate of 85.1%. The follow-up rate did not vary by treatment area or eligibility status.

This analysis includes only households with at least one woman reporting a birth in either the Encel 1998 or the Encel 2000 evaluation surveys. The unit of observation of the study is the birth; and the outcome of interest, skilled attendance to delivery, is defined as whether or not the delivery was either attended by physicians or nurses or in a health care facility. The two Encel surveys contain information on skill attendance for births to women 15 to 49 years old. The baseline information used in this analysis includes births that occurred between January 1996 and July 1997, while the follow-up information refers to births that occurred between January 1998 and July 1999. Although the Encel 2000 had information on births occurring up to 2000, we only used information up to July 1999 because some households in the control group were enrolled into the program starting August 1999. Thus, the analysis sample included 3,280 households with women who had births in the baseline measure and 2,715 households with women who had births in the follow up measure, as can be seen in the last two columns of Table 1. In this analysis we use information on eligibility status, age, state of residence, schooling of women and women's ability to speak an indigenous language as covariates.

A source of concern was whether households that were not followed in the Encel 2000 were systematically different than those that were followed up. If so, that could introduce a bias in the results. To explore this possibility, we conducted an attrition analysis using the variables mentioned above and other household characteristics (see results in Table A in Appendix). The results do not show any compelling evidence that the loss to follow up is related differentially to characteristics of the households in treatment and control areas. This supports the assumption that the advantages of the baseline experimental design could carry over to 1999.

Another concern was if the participation on the program could affect the decision of women to get pregnant. Among women in the evaluation sample, the decision to have children after the program started may be influenced by the program. This difference may be explained by observable and by non-observable characteristics, which would suggest the potential for selection bias in the sample. To analyze this, we conducted an analysis on women in the evaluation sample, assessing if the area of residence (intervention or control localities) or the interaction between the area of residence and other baseline characteristics, including having a birth in the pre-intervention period (January 1996 to July 1997) could be related to the probability to have a birth in the post-treatment period (January 1998 to July 1999). We find no significant association between living in an intervention or control area (T_j) or the interaction of living in an intervention area with other baseline households characteristics (T_j*X) on the probability of having a birth in the post-intervention period for women who had a birth in the pre-intervention period. This suggests that the decision of having a birth in the post-intervention period did not appear to be affected by living in an area where the program was available. We find, however, that in intervention areas there is a positive and significant association between having a birth in the pre-intervention period and the probability of having a birth in the post-intervention period (Appendix, Table B).

We conducted a comparison of the baseline characteristics of women and households in the intervention and control groups. For that comparison we will use pre-intervention data from the Encaseh 1997 merged into our sample of analysis. Table 2 presents baseline characteristics of households (n=2,715) and women (n=2,732) included in the follow-up from the analysis sample. We compared characteristics of households and women in intervention vs. control areas for eligible and non-eligible households using regression analysis including as covariate a dummy variable for state, and adjusted by clustering at the locality level. We find that eligible and non-eligible households in treatment and control localities have similar characteristics. We only find marginally significant differences (at the 10% level) in the proportion of households with migrants between intervention and control groups in eligible households. We also compared baseline characteristics of households (n=3,280) and women (n=3,280) of eligible vs. non-eligible groups, in intervention and control areas, included in the baseline measure from the analysis sample (see Table C in Appendix). Overall, we do not find significant differences between treatment and control groups. There were a few exceptions, like the differences in the number of children under 12 and 18 years old in intervention vs. control groups among non-eligible households at the 5% and 10% level, respectively, and the difference in the mean age of women in the intervention vs. control groups for both eligible and non-eligible women, which were statistically different at the 10% and 5% level. Finally, in order to identify systematic differences in the several samples for the analysis, we replicated this analysis with households with women who had births in both the baseline measure and the follow-up measures (n= 541 households, n=541 women). We found no significant differences between the intervention and control groups (see Table D in Appendix).

Table 3 shows the proportion of births with skilled attendance at baseline and after the intervention started among the eligible and non-eligible individuals, for intervention and control groups. This analysis was conducted for all births from women who had a birth in any of the periods under study, and for women who had births in both periods. The post-intervention proportion of births with skilled attendance included information on the last two births reported by women as long as they occurred in the period of study. In intervention areas, 46.3% of eligible women who had a birth between January 1996 and July 1997 received skilled attendance at delivery, while this proportion was 68.9% for the presumably wealthier non-eligible women. Similar proportions were found in control areas. Interestingly, there was a decrease in the proportion of deliveries with skilled attendance between the pre and post interventions measures among eligible women in intervention areas (-3.03%) but the decline was larger in control areas (-10.45%). When we consider only the sample of women who had a birth in both waves of the study (see the lower section of Table 3), the proportion of births with skilled attendance among eligible women after the start of operation of the program shows an increase of 7.05% in intervention areas and a decrease of 4.11% in control areas.

Methodology

We take advantage of the experimental design implemented in rural areas to estimate different program effects on women's use of skilled attendance at delivery. We start by estimating intention to treat (ITT) in the overall population and on those eligible for the program. Under the assumption of exogenous enrollment, we also estimate average treatment on the treated (ATT). We examine potential endogeneity of both eligibility status and program enrollment using difference-in-difference (DID) estimation strategies and, in the case of eligibility to the program, instrumental variables approaches. The features of the program eligibility process made possible to use a regression discontinuity analysis (RDA) approach to estimate program effects (Angrist & Krueger, 1999; Hahn, Todd & Van der Klaauw, 2001). The analytic approaches are described in detail below.

Intention to treat and targeted intention to treat estimates

Random experimental designs are recognized as the most powerful experimental designs, in terms of their ability to estimate impacts of a program given that randomization ensures that the intervention and control groups are expected to be similar in observable and non-observable characteristics, excepting for the presence of the program in the intervention group. A simple comparison between women in treatment and control areas would be enough to estimate an ITT effect of the program. The main assumption of this approach is that treatment assignment took place independently of the potential outcomes with and without the treatment. Even if this were the case, it may be possible to obtain more accurate estimates of the treatment effect by controlling for pre-assignment variables. By randomization, these pre-treatment variables are unrelated to the actual treatment status. This assumption is known as the *conditional independence assumption*. The post-intervention information is sufficient to identify the impact of the program. Therefore, using the information on all women in the evaluation sample who had births in the post-intervention period (January 1998 to July 1999), we estimate the following empirical specification using ordinary least squares (OLS):

$$y_{ij} = X_{ij} \cdot \beta + \delta \cdot T_j + \mu_{ij} \qquad (1)$$

The dependent variable y_{ij} takes the value 1 if the birth of woman i of locality j had skilled attendance, and zero if not; T_j is a dummy variable indicating whether locality j is a treatment area; X_{ij} is a vector of individual and household exogenous characteristics measured at the baseline[2]; β and δ are parameters to be estimated; and μ_{ij} is the error term. We include the X_{ij} in this equation to control for some particular forms of differences between treatment and control areas that were not perfectly eliminated by the randomization. The coefficient δ in equation (1) is our ITT estimator. It measures the average difference in the proportion of women with skilled attendance between treatment and control areas after controlling for the linear effects of the variables in X_{ij}. It makes no distinction between the impact of the program on the eligibles and the non-eligibles within the treatment area.

We are also interested in examining the effect of Oportunidades on the program's target population; that is, on those deemed eligible to receive the intervention. For this group of women, one can estimate another type of ITT effect that we call the *focused or*

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[2] The vector X_{ij} includes age (using dummy variables for the following categories: 15-19, 20-34, and 35 or more years old), schooling (using dummies for elementary and middle school), speaking an indigenous language, and a polynomial for the poverty score, including the natural log of poverty score and its square.

targeted ITT effect. In order to examine the differential effect of the program on the eligible group, we can estimate the following equation for all women who had births in the post-intervention period (January 1998 to July 1999) using OLS:

$$y_{ij} = X_{ij} \cdot \beta_1 + \beta_2 \cdot T_j + \beta_3 \cdot E_{ij} + \delta \cdot (T_j \cdot E_{ij}) + \mu_{ij} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2)$$

where E_{ij} is a dummy variable indicating the woman is in an eligible household. In equation (2), our impact estimate δ measures the average difference in the proportion of women with skilled attendance between treatment and control areas for eligible women compared to non-eligible women, after controlling for the linear effects of the variables in X_{ij}. In other words, δ indicates the effect of the program on the eligibles relative to the effect on the non-eligibles. An advantage of this approach is that it provides a measure of the intent to treat effect on the subset of women who were the target of the program. It also permits an examination of potential spillover program effects on the non-eligible group of women under the assumption of perfect randomization (denoted in the model by the term β_2). One can also estimate equation (1) only on the sample of eligible women in the treatment and control areas to measure this focused ITT effect.

However, one concern is that eligibility status (E_{ij}) be endogenous to the main outcome of interest, as final eligibility status was largely influenced by eligibility according to the poverty score, which was determined prior to the experimental assignment. It is likely that our outcome of interest (skilled attendance at delivery) is correlated with general family welfare conditions and considering that the final eligibility status was validated in a community assembly, it is possible that eligibility was influenced by unobserved factors also related to family welfare and the outcome. Therefore, the eligibility status might not be exogenously determined, which would bias estimates from equation (2). In order to deal with the potential endogeneity of eligibility, we use an instrumental variables approach and conducted Durbin-Wu-Hausman tests for exogeneity of eligibility.

One can instrument E_{ij} through a dichotomous variable Z_{ij} that takes value 1 if the poverty score (PS) of a household is lower than the threshold or cutoff point established by the program managers, and zero otherwise. Because the actual value of the threshold used by the program administrators was not available, we estimated the cutoff point using a discriminant analysis where the group variable was being eligible, and the discriminatory variable was the PS. The cutoff point that minimized misclassification obtained from the discriminant analysis with the 24,077 households in the baseline evaluation survey was 752. Using this cutoff point, only 3.23% of the households were misclassified. Further details of this analysis can be found in Lamadrid-Figueroa, et al. (2006). We used a two-step-least-square procedure to re-estimate equation (2) with Z_{ij} and $T_j \cdot Z_{ij}$ as instruments for E_{ij} and $T_j \cdot E_{ij}$. The new estimator provides a *local average treatment effect* (LATE) (Imbens & Angrist, 1994). It measures the impact of the program on the potential eligible women among those women who modify their eligibility status as a result of a change in Z_{ij} .

The panel structure of the data enables to deal with the potential endogeneity problem using DID estimators. This approach would enable us to remove time-invariant unobserved community characteristics that could be potential sources of endogeneity. To do this, we take advantage of the skilled attendance at delivery information in pre-treatment time reported at the baseline round (Encel 98). In order to consider the same window of time, for pre-treatment time we include births from January 1996 to July of

1997, and for post-treatment time births from January 1998 to July 1999. The sample of analysis was restricted to eligible women. We estimated the following model,

$$y_{ijt} = X_{ijt}\beta_1 + \beta_2 T_j + \beta_3 t_t + \delta(T_j \cdot t_t) + \mu_{ijt}, \quad \text{if the woman was eligible } (E_{ij} = 1) \ldots\ldots\ldots(3)$$

where the subscript t denote the time period. The dummy variable t_t takes value 1 for the follow-up time period and value of 0 to indicate the baseline time period; μ_{ijt} represents the stochastic error term. The vector X_{ijt} includes only baseline characteristics. In equation (3), β_3 measures change in the skilled attendance between the baseline and follow-up in non-treatment areas; and δ measures the differential change in skilled attendance at delivery women in treatment localities experienced between baseline and follow-up relative to the change experienced by women in non-treatment localities, that is, δ is the DID estimator of the targeted ITT effect after controlling for pre-treatment differences between the eligible groups in the treatment and control areas.

To conduct the DID analysis, we only considered one birth per woman in the post-intervention period. For women with two births in the post-intervention period, we chose the last birth that occurred during January 1998 to July 1999. We fit equation (3) with OLS for eligible women using an unbalanced panel (women with births reported in any of the pre- or post-intervention periods), and also with a balanced panel (women with births in both pre- and post-intervention periods). For the latter group, we also fit the model in equation (3) controlling for fixed effects at the individual level, as follows:

$$y_{ijt} = F_{ij} + X_{ijt}\beta_1 + \beta_2 T_j + \beta_3 t_t + \delta(T_j \cdot t_t) + \mu_{ijt}, \quad \text{if the woman was eligible } (E_{ij} = 1) \ldots(4)$$

where F_{ij} captures fixed effects at the individual level. In this model, δ is the DID estimator of the targeted ITT effect after controlling for pre-treatment differences between the eligible groups in the treatment and control areas, but also controlling for unobserved fixed differences between individuals.

It is possible that there were post-treatment differences taking place in treatment and control localities that were not caused by the program. In that case, we can use information from non-eligible women to help control for this possible confounding effect. To do this, we estimate the following difference-in-difference-in-difference (DIDID) model for all women with births in the pre and post-intervention periods:

$$y_{ijt} = X_{ijt} \cdot \beta_1 + \beta_2 \cdot T_j + \beta_3 \cdot t_t + \beta_4 \cdot E_{ij} + \lambda_1 \cdot (T_j \cdot t_t) + \lambda_2 \cdot (T_j \cdot E_{ij}) + \lambda_3 \cdot (E_{ij} \cdot t_t) + \delta \cdot (T_j \cdot E_{ij} \cdot t_t) + \varepsilon_{ijt}, \quad (5)$$

Notice that λ_1 measures the difference in the before-and-after change between treatment and control areas that occurred in the non-eligible group. In the absence of spillover program effects on the non-eligible group, these are post-intervention changes in the two areas not due to the program. The term ($\lambda_1 + \delta$) measures the difference in the before-and-after change between treatment and control areas for eligible women. Therefore, δ is our measure of program effect. It measures the differential change in the proportion of women with skilled delivery attendance before and after the intervention on the eligible group compared to the non-eligible women. We estimated equation (5) using OLS for the balanced and unbalanced samples, and controlling for fixed effects at the individual level. In order to adjust for the potential endogeneity of E_{ij}, we used instrumental variables with Z_{ij} as an instrument and fit the model with the unbalanced and balanced datasets, and adjusting for fixed effects at the individual level.

As indicated above, the first step used by program managers to define the eligibility status of a household was to generate a poverty score (PS) on the basis of data on household conditions collected through a survey questionnaire and visual inspection of the dwelling. Households below a cutoff threshold were deemed eligible in principle for participating in the program. Households with PS above the cutoff point were deemed non-eligible in principle. Final eligibility status, however, was determined in community assemblies where results from the PS-based classification were validated. In the vast majority of cases, community assemblies confirmed the household's eligibility status (Skoufias, Davis & Behrman, 2000). We take advantage of this mechanism to estimate program effects using a *regression discontinuity analysis* (RDA) approach. We compare women who are just below the cutoff point to women who are just above the cutoff point (Hahn, Todd & Van der Klaauw, 2001; Van der Klaauw, 2002).

The RDA approach is intuitively appealing: given that treatment localities were selected because of their overall condition of poverty, we would not expect large differences in observed and unobserved characteristics between resident women located immediately above and those immediately below the cutoff point, except for the fact that those below were deemed eligible for the program. For this RDA approach to work, it must be the case that program eligibility is discontinuous at the threshold and that the cutoff point is exogenous for the groups of women that will be compared. Figure 2 shows the distribution of the proportion of eligible households according to their PS considering the cutoff point obtained in the discriminant analysis described above.[3] For the RDA, we use observations with PS that are within "windows" of varying width centered at a score of 752. The sample of analysis is then defined by:

[3] We replicated the discriminant analysis to identify an eligibility cutoff point with the sample of households with women who had births pre- or post-intervention. The analysis revealed a sharp change in eligibility status at a PS of 754. Results of the RDA did not differ using the cutoff point of 752 or 754, and therefore only the ones with the cutoff point of 752 are presented.

$W=1(|PS_i-752|\leq\eta_k)$

for η_k ε {20, 50, 75, 100, 120}.

The use of different size windows in the RDA poses a trade-off between bias and precision. With smaller windows, one may have less bias because the women are more similar in unobserved characteristics. At the same time, however, using a smaller window means one would also have fewer observations and, consequently, less precision. Similarly, with larger windows one would have more precision, but one runs the risk of comparing less similar women who might differ significantly and systematically in unobserved characteristics. In the empirical analysis, we present results for a variety of windows.

For each of the windows, we estimate the following model using OLS:

$y_{ij} = X_{ij} \cdot \beta_1 + \delta \cdot E_{ij} + \varepsilon_{ij}$ if treatment area=1 and $W=1(|PS_i-752|\leq\eta_k)$……….(6)

Where δ is our impact estimate for the targeted ITT, and measures the average difference in the proportion of women with skilled attendance between eligible and non-eligible women in treatment areas after controlling for the linear effects of the variables in X_{ij} for women in each of the windows of size W.

We also treated eligibility status as a potentially endogenous variable and estimate model (6) by instrumental variables using Z_{ij}, the poverty-score-based indicator of eligibility status, as the instrument for actual eligibility, E_{ij}. It should be kept in mind that, by using instrumental variables, this is a LATE valid for women whose actual eligibility status would had been modified by changes in the PS.

The estimation of equation (6) assumes that the window was small enough so that we could consider women on either side of the threshold were quite similar. As an alternative, for each of the windows, we use a DID estimator that exploits the panel data we have available. The model to estimate, fit controlling for fixed effects at the individual level, is specified as:

$y_{ijt} = F_{ij} + X_{ijt} \cdot \beta_1 + \beta_2 \cdot E_{ij} + \beta_3 \cdot t_t + \delta \cdot (E_{ij} \cdot t_t) + \varepsilon_{ijt}$ if treatment area=1 and W=1(|PS$_i$-752|≤η_k) …………………………………………..……(7)

where F_{ij} indicates fixed effects for woman i at locality j. In equation (7), OLS estimation controlling by women-level fixed-effects will generate that δ be the targeted ITT effect purged of time-invariant women-specific unobservables.

We also estimated a locally weighted regression (lowess regression) in order to describe by treatment status, the behavior of skill attendance for delivery at different levels of PS and above and below the cutoff point for eligibility, in the follow-up observation period.

Program effect on enrollees.

In addition to using information on eligibility to the program, one can further estimate the impact of the program on those actually enrolled in the program. Because there were no enrolled women in control areas, the analysis of the program effect on enrollees was conducted initially only in treatment areas, which is presented in detail. We then replicated the analysis including women in control areas as well in order to increase the number of observations for some groups of reference as the poor.

Define R_{ij}, as a dummy variable that takes value 1 if a household was enrolled in the program since early in 1998 and zero otherwise. Then, using information on all women who had births in the post-intervention period in treatment areas, we estimate the following model:

$$y_{ij} = X_{ij} \cdot \beta_1 + \delta \cdot R_{ij} + \mu_{ij} \text{ ....(8)}$$

In equation (8), our program impact estimate, δ, measures the difference in the proportion of women with skilled attendance at delivery between those who actually received the program and those who did not in treatment areas, controlling the effect of a set of covariates (X_{ij}) that includes their poverty score. This model will enable us to examine the average program effect on the treated (ATT) if R_{ij} is exogenously determined.

We modified equation (8) replacing the continuous poverty status variables with a dummy variable (Z_{ij}) which takes value 1 if the household was located under the poverty score-based eligibility cutoff point, and zero otherwise. We estimated a model that includes as covariates the poverty-based eligibility status (Z_{ij}), and a set of characteristics excluding povery score (U_{ij})[4] with the whole sample in treatment areas:

$$y_{ij} = U_{ij} \cdot \beta_1 + \beta_2 \cdot Z_{ij} + \delta \cdot R_{ij} + \mu_{ij} \text{ ....(9)}$$

In equation (9), our impact estimate δ measures the difference in the proportion of women with skilled attendance at delivery between those who actually received the program and those who did not in treatment areas, controlling the effect of a set of covariates (U_{ij}) and their poverty score-based eligibility status (Z_{ij}). This model will also enable us to examine the average program effect on the treated (ATT) if R_{ij} is exogenously determined.

[4] The vector U_{ij} includes age (using dummy variables for the following categories: 15-19, 20-34, and 35 or more years old), schooling (using dummies for elementary and middle school), and a dummy for speaking an indigenous language.

Actual enrollment to the program (R_{ij}) may be endogenous to the skilled attendance decision process. Since the family freely decided whether to enroll in the program or not, it could have been influenced by unobserved factors related to the outcome of interest. For instance, households with a higher motivation to improve family members' human capital and health might have a higher propensity to enroll into the program and also to use skilled attendance at delivery. Simple estimation methods that ignore the effect of common unobservables would produce biased estimates of program effect in equations (8) and (9).

As we did with the case of eligibility, we also use DID estimators to deal with the potential endogeneity of enrollment. We use panel data to estimate the following DID model:

$$y_{ijt} = U_{ijt}\beta_1 + \beta_2 Z_{ij} + \beta_3 t_t + \beta_4 Z_{ij} \cdot t_t + \delta_1 R_{ij} + \delta_2 R_{ij} \cdot t_t + \mu_{ijt}, \quad \ldots\ldots\ldots(10)$$

This specification was fit with the unbalanced panel, and also with the balanced panel adjusting for fixed effects, as follows:

$$y_{ijt} = F_{ij} + U_{ijt}\beta_1 + \beta_2 Z_{ij} + \beta_3 t_t + \beta_4 Z_{ij} \cdot t_t + \delta_1 R_{ij} + \delta_2 R_{ij} \cdot t_t + \mu_{ijt}, \quad \ldots\ldots\ldots(11)$$

In equations (10) and (11), δ_2 is our program impact estimate and it measures the additional change across time in the proportion of women with skilled attendance to deliveries for those enrolled in the program relative to the change in those non-enrolled, in treatment areas.

In order to explore the effect of enrollment on skilled attendance to delivery among poor individuals (defined as those located under the poverty score-based eligibility cutoff), we fitted two additional models. First we fit a cross-sectional model with all women in treatment areas, as follows:

$$y_{ij} = U_{ij} \cdot \beta_1 + \beta_2 \cdot Z_{ij} + \delta_1 \cdot R_{ij} + \delta_2 \cdot Z_{ij} \cdot R_{ij} + \mu_{ij} \quad \text{.........(12)}$$

In equation (12), our impact estimate δ_2 measures the differential effect of enrollment on poor women relative to the effect on non-poor women.

Second, taking advantage of the panel information, we fit a DIDID model with all women in treatment areas, with the following specification:

$$\begin{aligned} y_{ijt} &= U_{ijt}\beta_1 + \beta_2 Z_{ij} + \beta_3 t_t + \beta_4 Z_{ij} \cdot t_t + \delta_1 R_{ij} + \delta_2 R_{ij} \cdot t_t + \delta_3 R_{ij} \cdot Z_{ij} \\ &+ \delta_4 (Z_{ij} \cdot R_{ij} \cdot t_t) + \mu_{ijt} \end{aligned}, \quad \text{.........(13)}$$

The DIDID analysis attempts to control for the potential endogeneity of the enrollment to the program, and examines whether the differential effect of the program on poor women, relative to non-poor women, varies across time. In equation (13), δ_4 is an estimate of that differential effect. We also fit equation (13) with the balanced panel, adjusting for fixed effects, as follows:

$$\begin{aligned} y_{ijt} &= F_{ij} + U_{ijt}\beta_1 + \beta_2 Z_{ij} + \beta_3 t_t + \beta_4 Z_{ij} \cdot t_t + \delta_1 R_{ij} + \delta_2 R_{ij} \cdot t_t + \delta_3 R_{ij} \cdot Z_{ij} \\ &+ \delta_4 (Z_{ij} \cdot R_{ij} \cdot t_t) + \mu_{ijt} \end{aligned}, \quad \text{.........(14)}$$

As an alternative strategy to control for endogeneity of enrollment, we use a RDA approach and estimate the DID models presented in equations (10) and (11) using observations with poverty scores that are within “windows” of varying widths centered at a PS of 752.

Results

<u>Intention to treat, targeted intention to treat, and local average treatment effect estimates</u>

Table 4 presents the results of models estimated using only cross-sectional follow-up (post-intervention) data. The first two columns of estimates present the ITT and the targeted ITT results that correspond to equations (1) and (2), respectively, described in the previous section. After controlling for exogenous differences between treatment and control areas, we find that, on average, the program increased the probability of using skilled attendance at delivery by 2.8 percentage points in the treatment areas and among eligible women. These program effects, however, are not statistically different from 0 at the 10% significance level. In column 2 of Table 4, the non-significant estimate of 0.9% for the area of residence suggests that the program had no spillover effects on the non-eligible group. The third column of Table 4 indicates that after controlling for potential endogeneity of eligibility, the program increases the probability of using skilled attendance at delivery by 4.8 percentage points. The program effect is still not significantly different from 0 at the 10% level, although it is important to recall that IV methods introduce imprecision in the estimation. We used a Durbin-Wu-Hausman test to examine the hypothesis that eligibility and the interaction between eligibility and treatment area (E_{ij} and $E_{ij}{\cdot}T_{ij}$) are exogenous. The test results did not provide evidence to reject the hypothesis of exogeneity. In all models presented in Table 4, higher schooling was positively associated with skilled attendance and speaking an indigenous language was negatively associated with it. In order to explore if the program had an impact on the poorest women, we replicated the ITT estimation using a sample of women with a poverty score below or equal to 577.[5] We found no statistically significant impact estimates at the 5% or 10% levels for the ITT or targeted ITT using OLS or instrumental variables, although it is important to note that the sample size reduced to only 446 women (results are available from the authors upon request).

We take advantage of the panel structure of the data to estimate DID models as an alternative estimation strategy to control for unobserved time-invariant pre-existing systematic differences between the treatment and control groups that could be a source of biases in the program estimates. To be able to conduct the DID analysis for the post-intervention period, we only considered one birth per woman, taking the last one that the woman had in the post-intervention study period (between January 1998 and July 1999). We run DID models using two samples of births: the first column of Table 5 shows results from pooling all last births observed in the baseline (January 1996-July 1997) with all births in the follow-up (January 1998-July 1999) periods. We call this the unbalanced sample. The second column presents results from the sample of women who had births in both the baseline and the follow-up periods, which is called the balanced sample.

The upper panel of Table 5 presents the estimated program effects from targeted ITT models as specified in equations (3) and (4). The first row presents results from the simple specification of the DID model while the second row presents results from further adding fixed effects controls at the individual level to the model. As we can observe in Table 5, using the unbalanced sample, we estimated that the ITT program effect on eligible women was 4.8 percentage points, however, not significantly different from 0 at the 10% level. Using the balanced sample we estimated a larger ITT program effect of

[5] All women with a poverty score of 577 or less were considered eligible by both the program administrator's criteria and by the communities' assemblies.

11.4 percentage points, which is significantly different from 0 at the 5% level. Adding individual-level fixed effects only changed the magnitude of the program effect slightly. These results suggest that the program had an effect on women with a recent birth delivery experience.

In order to assess the impact of the program controlling for post-treatment differences between treatment and control areas that were not caused by the program, we also estimated DIDID models as proposed in equation (5). The results are presented in the lower panel of Table 5, showing the estimates of parameter δ, which is our measure of program effect. In the first row of the first column, when the model is estimated with OLS using the unbalanced sample, we found an ITT program effect (δ) of 3.5 percentage points. The estimate is, however, insignificant at the 10% level. This effect increases to about 15 percentage points when using the balanced sample and controlling for fixed effects (first and second rows, second column, of the lower panel). These effects are also not significantly different from 0 at the 10% level. Controlling for potential endogeneity of E_{ij} using IV methods in the unbalanced sample (row 3 of the lower panel, column 1), we obtained an estimate of 8.7 percentage points but still not significant at the 10% level. However, we found an effect of about 18 to 19 percentage points with the balanced sample (rows 3 and 4 of the lower panel, column 2), which is significant at the 10% level.

Results from the RDA approach to estimate program effects on the eligible women are presented in Table 6. This table presents results of equations (6) and (7), with program effects estimates from simple OLS, two-steps-least-squares, and panel DID models applied to different samples of women defined by windows of varying width centered on the threshold of eligibility based on the PS. In the first column of Table 6, simple OLS estimates show a small targeted ITT effect in the range of -1.2 to 3.5 percentage points, but all of them not significantly different from 0 at the 10% level. As shown in column 2, controlling for potential endogeneity of E_{ij} using IV increases the magnitude of program effect to a range of 4.9 to 25.2 percentage points, but all of them still not significantly different from 0 at the 10% level. It is important to notice that the magnitude of the LATE increases as the width of the window narrows, suggesting larger program effects for those women closer to the poverty score threshold point. Similar point estimates and pattern is found for DID estimates as presented in the third column, but with an ITT effect of 26.1 percentage points at of window of size 30, being different from 0 at the 10% significance level.

Figure 3 shows the results of the locally weighted regression (lowess regression) analysis. The first panel of this figure shows the results of this analysis in the post-intervention period. For this period, skilled attendance in intervention areas (solid line) was higher than in control areas (dashed line) for the eligible group (with a PS lower than the threshold) for all levels of PS, with the exception of the very low. Note that the positive effect of the program appears to be driven by the higher prevalence of skilled attendance in the control area just above the poverty score cutoff. Consistently with what is shown in Table 8, as the window around the cutoff point widens, the use of skilled attendance by those above the poverty score cutoff point becomes much more similar in treatment and control areas, resulting in a RDA estimate that falls as the window size expands. The lower panel of figure 3 shows results of the locally weighted regression in the pre-intervention period. The comparison of the pre- and post-intervention charts shows that the program seems to have a positive effect on eligible women with a PS closer to the eligibility threshold. The program seems to have a protective effect as there was an overall decline in skilled attendance in both intervention and control areas: We can observe that skilled attendance for eligible women in intervention areas did not decline as much as it did in control areas. This effect is clear for eligible women with a poverty score closer to the threshold point.

<u>Program effect on enrollees.</u>

The estimation of impact effects based on actual enrollment poses two difficulties. First, due to the design of the evaluation, we do not have enrolled women in non-treatment areas. Second, in the sample of analysis in treatment areas, there are only 128 eligible women according to the administrator's criteria, and 180 eligible according to the poverty score based-eligibility status who are not enrolled to the program. Consequently, non-treatment areas provide no additional information about enrollment, and in treatment areas, we have a reduced number of observations of eligible women – by any criteria – not enrolled in the program. For these reasons, we decided to estimate the models using only women in treatment areas.

Table 7 shows the program effect estimates based on actual enrollment on treatment areas only. In the first row of this table, using the cross-sectional approach proposed in equation (8), we found a negative effect of -8.5 percentage points, which is significantly different from cero at the 5% confidence level. In the second row, a program estimate of -10.9 percentage points, significantly different from 0 at the 5% confidence level, was obtained when fitting a cross-sectional model adjusting by poverty score-based eligibility status (Z_{ij}), as proposed in equation (9). These results of the negative effect of enrollment are contrary to our expectations. However, we suspect it could be the result of the potential endogeneity of actual enrollment to the program (variable R_{ij}). That is, those women who are more likely to enroll in the program due to unobserved factors are those less likely to use skilled birth attendance. Once we adjust for the potential endogeneity of enrollment using a DID models, as proposed in equations (10) and (11), we still obtain negative program effect of -12.3 percentage points for the unbalanced panel (significantly different from 0 at the 5% confidence level). The program effect becomes negative but not significant estimate of -5.8 percentage points when using the balanced panel and adjusting for individual fixed effects (third row of table 7).

When we examine the differential effect of the program on the poor, that is on women with a poverty score below the program-determined threshold, by estimating equation (12), we found a program effect of 3.3 percentage points, but not significantly different from 0 at the 10% confidence level (fourth row of table 7). When fitting the DIDID model proposed in equations (13) and (14), we found negative although non-significantly different from 0 impact estimates of -6 and -9 percentage points, when using the unbalanced and the balanced panels respectively (fifth row of table 7).[6,7]

[6] We also estimated equations (8) to (14) using women from treatment and non-treatment areas, as a way to increase the number of poor women not enrolled in the program. We added a dummy variable for treatment area (T_j). We obtained negative impact estimates in the cross-sectional models (equations (8) and (9)) of -5 percentage points, not significantly different from 0 at the 10% confidence level. Positive impact estimates of 10 and 7.7 percentage points were obtained in the DID analysis with the unbalanced and balanced samples, respectively, both not significantly different from 0 at the 10% confidence level. When we explored the impact of the program on the poor in a cross-sectional model (equation (12)), we found a positive impact of 10.8 percentage points, significantly different from 0 at the 5% level. We also found positive impact estimates of 8.4 and 23.2 percentage points in the DIDID model (equations (13) and (14)) with the

Table 8 shows the results from the RDA conducted the DID model with actual enrollment (R_{ij}) as proposed in equations (10) and (11) for windows of different sizes around the poverty based eligibility cutoff point of 752, using OLS for the unbalanced panel and adjusting for fixed effects at the individual level with the balanced panel. With the unbalanced panel (first column), DID program effect estimates ranged from -6 to -16 percentage points, and the only statistically different from null were those over 13.1 percentage points obtained from the wider windows of 100 and 120. Using DID models on balanced samples as an alternative strategy to control for endogeneity (second column), we obtained program effects in the range of -10.9 to 7.4 percentage points, but none of them significantly different from 0 at the 5% or 10% level. [8]

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unbalanced and balanced samples, respectively, both non-significantly different from 0 at the 10% confidence level.

7 One concern in this analysis was if the program could have a different effect among women who had their first birth either in the pre-treatment or in the post-treatment observations periods. To address this issue, we obtained DID and DIDID estimates as proposed in equations (3), (5), (12) and (13), with a subsample of primiparous women. We found no program effects significantly different from 0 at the 10% confidence level in most models, with the exception of the DID comparing enrolled and non enrolled women in intervention areas and adjusting by poverty score-based eligibility status (equation 10), where we found a negative impact estimate of the program of minus 25 percentage points, significantly different from 0 at the 5% confidence level. These results suggest that the program had no effect among women who had their first reproductive experience during the observation period of analysis.

[8] When replicating this RDA including women from control areas, we found impact estimates ranging from -5.6 to 5 percentage points with the unbalanced panel, and from 6.1 to 12.2 percentage points with the balanced panel, none of them significantly different from cero at the 10% confidence level.

Discussion

A previous study has identified a reduction of maternal mortality ratios in areas served by Oportunidades (Hernández et al., 2005a). However, we know little about the mechanisms by which the program may reduce this kind of death. A possible explanation is that the program induces an increase in the use of modern delivery care. This paper evaluates the impact of Oportunidades on skilled attendance at delivery in rural areas of Mexico during the first years of the program. We take advantage of the experimental design implemented for the purpose of evaluating the program. However, even though there was a random allocation of localities to the intervention and control groups, there may be self-selection of households to participate in the program, as was pointed out by Berhman and Hoddinot (2005). Therefore, we used analytical strategies, including DID, DIDID, and RDA to control for this potential endogeneity.

Women enrolled in the program have levels of skilled attendance lower than the national average, recorded at 74.2% in 2004 (Dirección General de Información en Salud, Secretaría de Salud, 2005), which suggests that there was an important window of opportunity to increase skilled attendance at delivery among the poor population targeted by the program.

The results based on the analysis of eligibility to the program indicate that the program had, at best, a small effect on women in intervention areas. The effect of the program appears to be higher for women with a poverty score nearer to the eligibility cut-off point. The results also indicate that the program is not reaching the poorest women in the intervention areas. Either differential responses due to a heterogeneous targeted population or to differential reach of program services are likely explanations, and these constitute challenges for the program.

We also found that the program had a higher effect on women who had a birth in both the pre- and post-intervention study periods. These women have had the experience of a birth, but are also a group with an overall lower proportion of births with skilled attendance. On the other hand, the program seems to have a null effect on women who are going through their first birth experience. These results identify groups of women for whom the program should focus future efforts to increase skilled attendance at delivery.

When we examined the impact of the program on enrollees, by comparing enrolled vs. non-enrolled women in treatment areas only, we found a negative effect, which remains significant even after controlling for the potential endogeneity of the enrollment using DID methods. Although this negative effect becomes non-significant when we analyzed only women who had a birth in both the pre- and post-intervention study periods, and when we focus only on poverty score-based eligible women, in general we find negative or null effects of the program on skilled attendance to delivery for enrollees compared to non-enrollees in intervention areas.

This study has some limitations that should be considered in the interpretation of results. The time period of analysis is short, using data collected from 1997 to 2000, when the program was in its first years of operation. It is possible that trends and impacts of Oportunidades on skilled attendance in rural areas may have varied in subsequent years as the program matured.

Other limitations of the study are related to the sources of information. Skilled attendance at delivery is provided by either the Ministry of Health or by social security providers. We did not have information on the characteristics of the providers available to women in the study localities. Therefore, it was not possible to examine whether the program effect varied by the characteristics of the service providers in the localities.

These results should lead to a revision of the strategies used by Oportunidades to increase skilled attendance to delivery. Qualitative studies in Mexican rural populations have shown that the woman's decision on where a delivery should be attended is heavily influenced by significant-others, especially her mother-in-law (Hussein, Jentsch & Hernandez, 2003). Oportunidades, however, directs its incentives to pregnant women. It will be important for the program to redesign its strategies to include those household members who heavily influence the decision about place of delivery. Likewise, the program should increase efforts on groups of women where Oportunidades shows no impact.

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Figure 1. Enrollment rate of eligible women in the rural evaluation sample by bimester, 1998-1999.

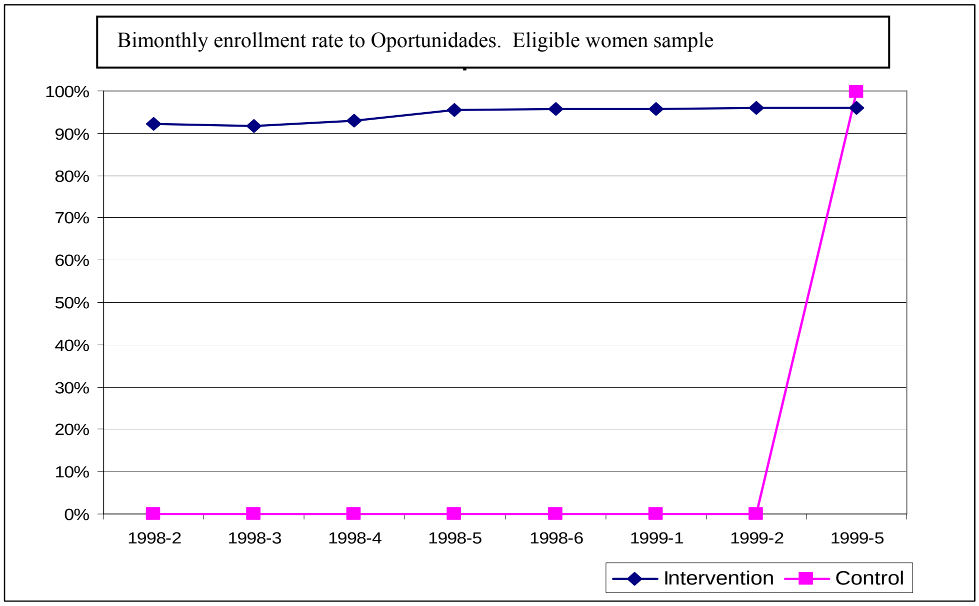


Figure 2. Moving average of the dummy variable for eligibility (1= eligible, 0=otherwise) by poverty score. Vertical line marks a poverty score of 752, the estimate of the cutoff point for eligibility status.

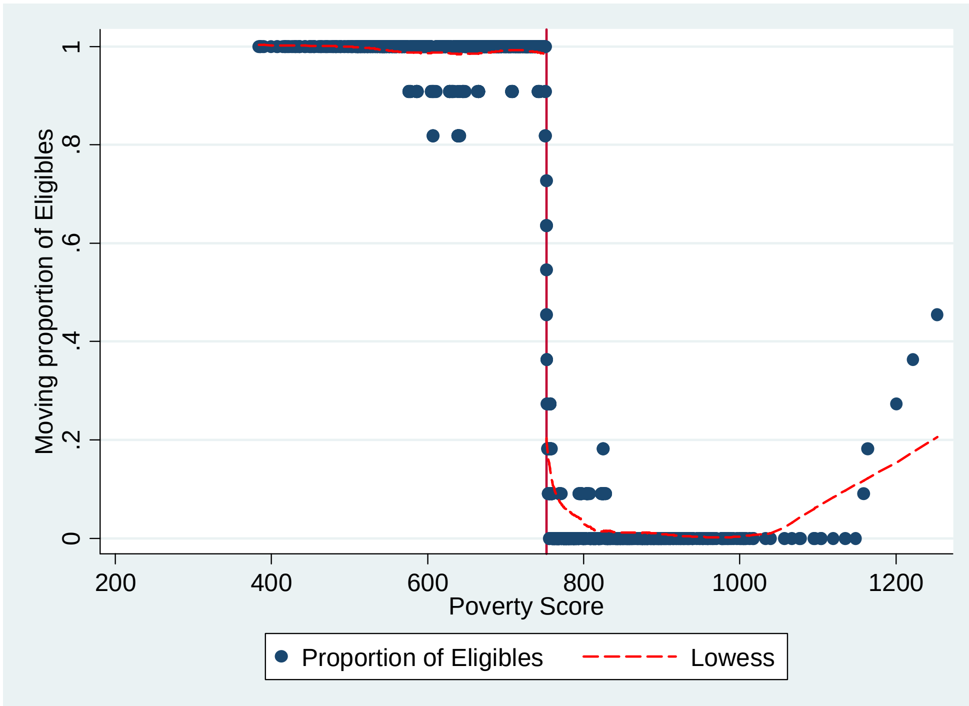
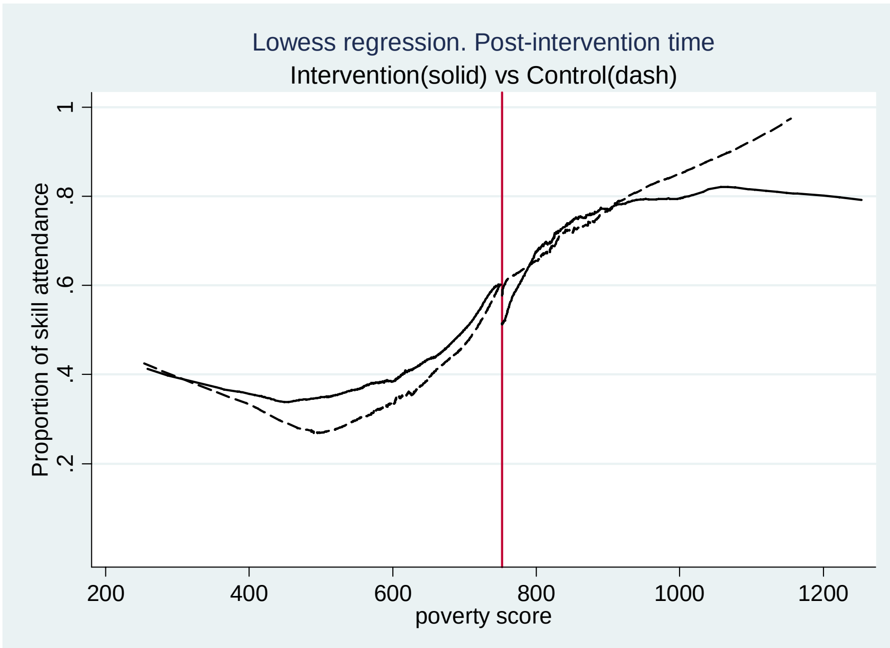


Figure 3. Association between the poverty index and skilled attendance to delivery by treatment area. Separate lowess curves were plotted for treatment and controls above and below the cutoff point for eligibility.



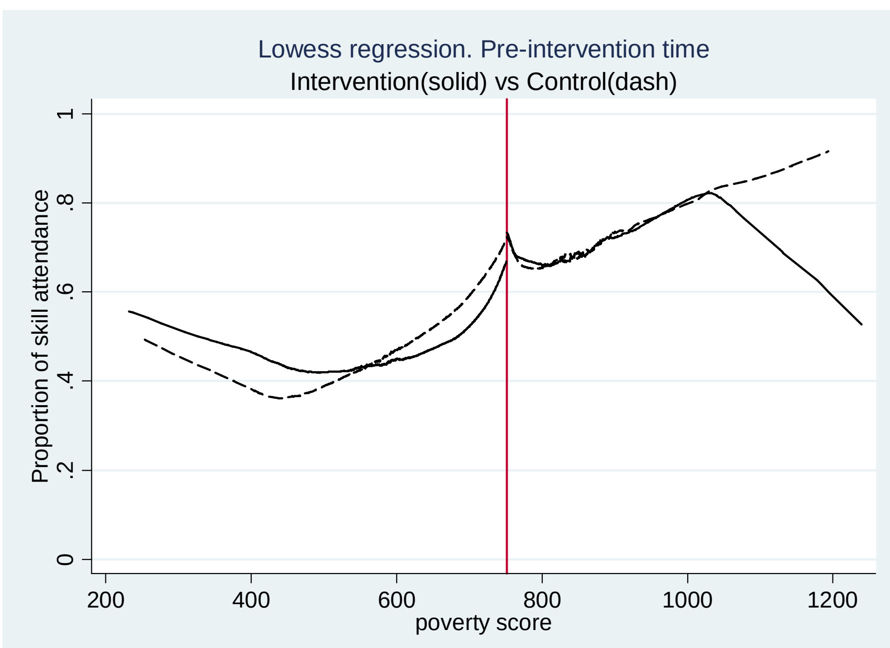


Table 1. Number of Households in the Sample

	All evaluation sample		Analysis sample	
	Baseline (Encel 1998)	Follow-up (Encel 2000)	Baseline† (Encel 1998)	Follow-up (Encel 2000) ††
Intervention localities	14,856	12,509	2,011	1,659
Eligible households	7,837	6,685	1,531	1,155
Non-eligible households	7,019	5,824	480	504
Enrolled in the Program	---	7,767	---	1,222
Control localities	9,221	7,984	1,269	1,056
Eligible households	4,682	4,099	925	701
Non-eligible households	4,539	3,885	344	355
Total sample of households	24,077	20,493	3,280	2,715

†: Analysis sample includes all households with women with a birth between January 1996 to July 1997

††: Analysis sample includes all households with women with a birth between January 1998 to July 1999

Table 2. Baseline Characteristics of Follow-up Sample (Those with Births from January 1998 to July 1999)

	Intervention		Control	
	Eligible[a]	Non eligible[b]	Eligible[c]	Non eligible[d]
Household characteristics				
N	1155	504	701	355
Eligibility (poverty) score	633.91(84.78)	839.31(91.19)	632.54(80.89)	846.59(97.56)
% Enrolled to *Oportunidades*	95.67	23.21	--	--
Household size (number of persons in the household)	6.19(2.33)	5.30(2.59)	6.16(2.42)	5.47(2.74)
Women living in the household / Household size	0.50(0.16)	0.51(0.15)	0.51(0.16)	0.58(0.16)
Number of women 15 years old or older	1.43(0.77)	1.75(1.02)	1.43(0.78)	1.76(1.03)
Number of children of age 12 or younger	3.14(1.62)	1.73(1.43)	3.10(1.66)	1.81(1.53)
Number of persons of age 18 or younger	3.81(2.06)	2.43(1.87)	3.78(2.02)	2.62(2.02)
Number of persons of age 65 or older	0.13(0.39)	0.17(0.47)	0.11(0.36)	0.18(0.46)
Number of indigenous members/Household size	0.34(0.36)	0.21(0.35)	0.38(0.38)	0.21(0.37)
Number of literate members/number of household members 5 years of age or older	0.49(0.22)	0.66(0.22)	0.48(0.23)	0.67(0.22)
% Households with migrant last 5 years	0.7	3.1	1.8[€]	2.5
% Households with a person with disability	3.9	2.3	2.7	3.3
% Households that had social security	6.1	7.1	4.5	10.4
% Households that own farm animal	31.2	40.5	28.5	37.5
Women's Characteristics				
N	1158	507	708	359
Age	27.82(7.34)	25.04(7.47)	27.11(7.83)	25.61(8.71)
Education				
% Without instruction	28.7	9.9	30.4	11.7
% Elementary	65.8	68.4	63.1	66.3
% Secondary o higher	5.5	21.7	6.5	22.0
% Indigenous language	47.2	25.0	50.3	26.7

Standard deviations in parenthesis
Comparison (a-c): [€] significant at 10%, [€€] significant at 5% . Test Ho by regression analysis adjusted by clustering at the locality level.
Comparison (b-d): [£] significant at 10%, [££] significant at 5% . Test Ho by regression analysis adjusted by clustering at the locality level.

Table 3. Proportion of Births with Skilled Attendance at Delivery

Time	Birth during:	Intervention areas				Control areas			
		Eligible[a]		**Non-eligible[b]**		**Eligible[c]**		**Non-eligible[d]**	
		%	n	%	n	%	n	%	n
Pre-Intervention*	January 1996-July 1997	46.31	1531	68.96	480	48.54	925	72.38	344
Post-Intervention**	January 1998-July 1999	43.28	1,183	71.04	518	38.09	722	71.66	367
Δ pre-post		-3.03		2.08		-10.45		-0.72	
Only women with births in both study periods									
Pre-Intervention*	1996 to July 1997	28.87	284	60.71	56	32.19	146	60	55
Post-Intervention**	1998 to July 1999	35.92	284	69.64	56	28.08	146	70.91	55
Δ pre-post		7.05		8.93		-4.11		10.91	

Comparison (a-c): € significant at 10%, €€ significant at 5% . Comparisons made using linear regression, adjusted by clustering at the locality level.

Comparison (b-d): £ significant at 10%, ££ significant at 5% . Comparisons made using linear regression, adjusted by clustering at the locality level.

n: Denotes all births in the analysis sample

Source of data: *Encel 1998, **Encel 2000

Table 4. Cross-Sectional Effects on Skilled Attendance at Delivery, Using Follow-up Sample; ITT, Targeted ITT and LATE Estimates

	OLS		2SLS
y={1:qualified, 0:non qualified]	ITT	Targeted ITT	LATE (eligibility status)†
Area x Eligible status		0.028	0.048
		[0.038]	[0.039]
Area {1:intervention, 0:control}	0.028	0.009	-0.004
	[0.027]	[0.035]	[0.036]
Eligible status {1:eligible, 0=non eligible }		-0.088	-0.088
		[0.033]***	[0.037]**
Age 1 {1:15-19 years old, 0: 20-34 years old}	0.048	0.041	0.042
	[0.040]	[0.040]	[0.039]
Age 2 {1:35 years or more}	0.018	0.017	0.016
	[0.021]	[0.021]	[0.021]
Schooling 1 {1:primary, 0:none}	0.092	0.091	0.092
	[0.024]***	[0.024]***	[0.024]***
Schooling 2 {1:secondary or more}	0.216	0.215	0.216
	[0.038]***	[0.038]***	[0.037]***
Indigenous language	-0.153	-0.152	-0.152
	[0.030]***	[0.030]***	[0.030]***
Ln poverty score	-3.959	-2.039	-2.35
	[2.121]*	[2.161]	[2.206]
Ln poverty score^2	0.343	0.186	0.211
	[0.163]**	[0.167]	[0.171]
Constant	11.61	5.839	6.783
	[6.906]*	[6.999]	[7.128]
Observations	2790	2790	2790

* significant at 10%; ** significant at 5%; *** significant at 1%

Robust standard (by locality) errors in brackets

Table 5. Program Effects on Skilled Attendance at Delivery; Difference-in-Difference and Difference-in-Difference-in-Difference Results

	Targeted ITT. Difference-in-difference models†	
	Unbalanced panel¥	Balanced panel¥¥
OLS	0.048	0.114
	[0.031]	[0.048]**
Fixed effects	--	0.118
	--	[0.047]**
Observations	4315	860
	Targeted ITT and LATE Estimators. Difference-in-difference-in-difference models†	
	Unbalanced panel¥	Balanced panel¥¥
OLS	0.035	0.144
	[0.050]	[0.097]
Fixed effects	--	0.152
	--	[0.097]
IV	0.087	0.184
	[0.056]	[0.109]*
IV Fixed effects	--	0.191
	--	[0.108]*
Observations	6001	1081

* significant at 10%; ** significant at 5%; *** significant at 1%

Robust standard (by locality) errors in brackets

† using T_j; All regressions include controls for age, schooling, indigenous language, poverty score, poverty score square, and dummies by state. Full set of estimates available from the corresponding author on request.

All IV models were instrumented using Z_{ij} {1 if score<752}, $Z_{ij}\cdot T_j$, $Z\cdot t_t$, $Z_{ij}\cdot t_t \cdot T_j$

To conduct the DID analysis we only considered one birth per woman. For women with two births in the post-intervention period, we considered the last birth that occurred between January 1998 and July 1999. Therefore, the number of births in this table differs from the ones presented in Table 3.

¥: Include all women for each wave with any birth reported.

¥¥ Include only women with births in both pre- and –post- intervention periods.

Source of data: Encel 1998, Encel 2000

Table 6. Regression Discontinuity Analysis; Program Effects on Skilled attendance at Delivery Based on Eligibility to the Program (Coefficients of E_{ij})

	Cross sectional		Targeted ITT
	Targeted ITT	LATE	Panel Data
	OLS	2SLS	DID
Window 20 (n=228)			
(panel n=38)	0.016	0.252	0.192
	[0.113]	[0.195]	[0.198]
Window 30 (n=327)			
(panel n=58)	0.002	0.115	0.261
	[0.098]	[0.140]	[0.150]*
Window 50 (n=501)			
(panel n=83)	0.006	0.107	0.165
	[0.076]	[0.102]	[0.124]
Window 75 (n=693)			
(panel n=106)	0.033	0.107	0.125
	[0.059]	[0.076]	[0.109]
Window 100 (n=888)			
(panel n=151)	0.035	0.095	0.095
	[0.053]	[0.068]	[0.095]
Window 120 (n=998)			
(panel n=177)	-0.012	0.049	0.056
	[0.049]	[0.061]	[0.094]

* significant at 10%; ** significant at 5%; *** significant at 1%

Robust standard errors in brackets.
All regressions control for age, schooling, indigenous language, log poverty score, log poverty score squared, and dummies at state level.
IV model: eligible (E_{ij}) was instrumented by Z_{ij}\{1 if score<752\}.
The panel analyses included fixed effects at individual level. Analysis conducted with balanced panel.

Source of data: Encel 1998, Encel 2000

Table 7. Cross-Sectional, Difference-in-Difference and Difference-in-Difference-in-Difference Analysis for ATT. Models Using Actual Enrollment (R_{ij}) [†]. Treatment Areas Only

	Program impact estimate	Program impact estimate
Cross-sectional analysis adjusted by poverty status	-0.085 [0.038]**	---
n	1,701	
Cross-sectional analysis adjusted by eligibility status (Z_{ij})	-0.109 [0.045]**	---
n	1,701	
	Unbalanced panel[¥¥]	Balanced panel[¥]
Difference-in-difference models	-0.123 [0.054]**	-0.058 [0.088]
n	3,676	340
Models exploring the effect on the eligible according to the poverty score		
Cross-sectional analysis adjusted by eligibility status (Z_{ij}) and its interaction with enrollment (Z_{ij}*R_{ij})	0.033 [0.086]	---
n	1,701	
	Unbalanced panel[¥¥]	Balanced panel[¥]
Difference-in-difference-in-difference models	-0.060 [0.071]	-0.091 [0.168]
n	3,676	340

* significant at 10%; ** significant at 5%; *** significant at 1%

Robust standard (by locality) errors in brackets

[†] All regressions were controlled by time, area, age, schooling, indigenous language, and dummies by state. Fixed effects at the individual level are used for the balanced panel. Full set of estimates available from the corresponding author on request.

To conduct the DID analysis we only considered one birth per woman. For women with two births in the post-intervention period, we considered the last birth that occurred between January 1998 and July 1999. Therefore, the number of births in this table differs from the ones presented in Table 3.

[¥]: Include all women for each wave with any birth reported. [¥¥]: Include only women with births in both pre- and – post- intervention periods.

Source of data: Encel 1998, Encel 2000

Table 8. Regression Discontinuity Analysis; Effects on Skilled Attendance at Delivery Based on the Difference-in-Difference with Enrollment to the Program (Coefficients on R_{ij}) in Treatment Areas Only

	Unbalanced panel	Balanced panel
Window 20 (n=434)	-0.06	0.074
(panel n=38)	[0.096]	[0.233]
Window 30 (n=632)	-0.069	-0.069
(panel n=58)	[0.087]	[0.188]
Window 50 (n=992)	-0.036	-0.014
(panel n=83)	[0.074]	[0.169]
Window 75 (n=1374)	-0.102	-0.033
(panel n=106)	[0.067]	[0.167]
Window 100 (n=1786)	-0.131	0.005
(panel n=151)	[0.067]*	[0.131]
Window 120 (n=2052)	-0.160	-0.109
(panel n=177)	[0.064]**	[0.124]

* significant at 10%; ** significant at 5%; *** significant at 1%

Robust standard errors in brackets.

All regressions control for age, schooling, indigenous language, and dummies at state level.

The balanced panel analyses included fixed effects at individual level.

Source of data: Encel 1998, Encel 2000

Appendix

Table A. Attrition Analysis. Logit Coefficients for a Model of Follow-up Interview Using Baseline Characteristics; Includes Information from Households in Baseline Sample that Were Followed or Lost in the Encel 2000 Sample

	y={1=follow up, 0=drop out}
Treatment area (T) {1:intervention, 0:control}	24.426
	[42.054]
Eligibility status (E) {1:eligible, 0=non eligible }	-0.004
	[0.010]
Household size (number of persons in the household)	0.021
	[0.006]***
Number of women/ Household size	0.054
	[0.025]**
Number of women older than 15 years of age	-0.009
	[0.006]
Number of children under 12 years of age	-0.014
	[0.006]**
Number of persons 18 years old or younger	0.007
	[0.006]
Number of persons 65 years old or older	0.008
	[0.008]
Number of indigenous members/household size	0.029
	[0.018]*
Literacy members/number of persons 5 years old or older in the house	-0.016
	[0.020]
Number of labor active persons	0.002
	[0.006]
Average age of household members	0.001
	[0.000]
Household has a migrant	0.009
	[0.021]
Household with person with disability	-0.022
	[0.015]
Household is owner of farm animal	0.025
	[0.009]***
Household that has social security	0.003
	[0.018]
ln poverty score	-8.45
	[13.971]
ln Poverty score^2	1.311
	[2.170]
ln Poverty score^3	-0.068

	[0.112]
Interactions	
Treatment area*eligibility status (T*E)	0.023
	[0.015]
T*Household size	0.004
	[0.008]
T* Number of women/ Household size	-0.03
	[0.031]
T* Number of women older than 15 years of age	0.01
	[0.008]
T* Number of children under 12 years of age	0.015
	[0.007]**
T* Number of persons 18 years old or younger	-0.018
	[0.008]**
T* Number of persons 65 years old or older	-0.004
	[0.011]
T* Number of indigenous members/household size	-0.034
	[0.029]
T* Literacy members/number of persons 5 years or older in the house	0.079
	[0.032]**
T* Number of labor active persons	-0.011
	[0.009]
T* Average age of household members	0
	[0.001]
T* Household has a migrant	0.003
	[0.025]
T* Household with person with disability	-0.011
	[0.021]
T* Household is owner of farm animal	0.008
	[0.014]
T* Household that has social security	-0.03
	[0.030]
T*ln poverty score	-11.935
	[19.888]
T*ln poverty score^2	1.928
	[3.125]
T*ln poverty score^3	-0.103
	[0.163]
Constant	18.867
	[29.932]
Observations	24077
R-squared	0.03
F test: T= T*X'=0	1.34
Prob > F	0.154

Robust standard errors in brackets. Included fixed effects at state level
* significant at 10%; ** significant at 5%; *** significant at 1%

Table B. Analysis of Potential Sample Selection Bias; Logit Results of Model of Having a Birth in Post-Intervention Time

	y = {1:any birth 98-July 99}
Treatment area (T) {1:intervention, 0:control}	22.253
	[39.459]
Eligible (E) {1:eligible, 0:non eligible}	-0.171
	[0.121]
Area * eligible	0.099
	[0.152]
Household size (number of persons in the household)	-0.01
	[0.048]
Number of women older than 15 years of age	-0.285
	[0.070]***
Number of children under 12 years of age	0.194
	[0.053]***
Number of persons under than 18 years of age	-0.057
	[0.060]
Age 1 {1: 15 to 19 years of age;0: 20 to 34 years of age }	-1.549
	[0.155]***
Age 2 {1: 35 years of age or more}	-1.138
	[0.088]***
Schooling 1 {1:primary, 0:none}	-0.143
	[0.093]
Schooling 2 {1:secondary o more}	-0.358
	[0.144]**
Indigenous language	0.15
	[0.104]
Number of children (until 1997)	-0.038
	[0.020]*
ln poverty score	7.212
	[9.822]
ln poverty score^2	-0.603
	[0.755]
Child 96-97 {1: if woman had a child in 96 or before August 97, 0=otherwise}	0.071
	[0.180]
Interactions	
T * Household size	-0.074
	[0.063]
T* Number of women older than 15 years of age	0.158
	[0.088]*
T * Number of children under 12 years of age	0.058
	[0.066]
T * Number of persons under than 18 years of age	0.004
	[0.076]
T * age 1 (15 to 19 years of age)	-0.27

	[0.226]
T * age 2 (35 years of age)	0.15
	[0.121]
T * schooling 1 (primary school)	0.043
	[0.116]
T * schooling 2 (secondary or higher)	0.021
	[0.176]
T * indigenous language	-0.044
	[0.123]
T * Number of children	-0.001
	[0.027]
T* ln poverty score	-6.771
	[12.132]
T* ln poverty score^2	0.512
	[0.933]
T*Child 96-97	0.462
	[0.233]**
Constant	-21.752
	[31.945]
Observations	15,975
Chi2 test: T = T*X'=0	15.72
Prob > chi2	0.401

Robust standard errors in brackets. Included fixed effects at state level.
* significant at 10%; ** significant at 5%; *** significant at 1%
Source: Encel 2000

Table C. Baseline Characteristics of Baseline Sample

	Intervention		Control	
	Eligible[a]	**Non eligible[b]**	**Eligible[c]**	**Non eligible[d]**
Household characteristics				
N	1531	480	925	344
Eligibility (poverty) score	616.01 (87.01)	823.34 (102.37)	614.98(88.13)	835.44 (108.21)
% Enrolled to *Oportunidades*	94.91	24.58	--	--
Household size (number of persons in the household)	6.48 (2.38)	5.7 (2.60)	6.48(2.27)	6.00 (2.65)
Women living in the household / Household size	0.50 (0.17)	0.50 (0.17)	0.50 (0.17)	0.49 (0.16)
Number of women 15 years old or older	1.40 (0.72)	1.67 (1.03)	1.40 (0.76)	1.63 (0.96)
Number of children age 12 or younger	3.45 (1.57)	2.28 (1.41)	3.46(1.46)	2.55 (1.51) [££]
Number of persons of age 18 or younger	4.14 (2.04)	2.90 (1.86)	4.14(1.95)	3.17 (1.94) [£]
Number of persons of age 65 or older	0.11 (0.37)	0.19 (0.48)	0.10 (0.33)	0.16 (0.43)
Number of indigenous members/Household size	0.29 (0.34)	0.17 (0.30)	0.28 (0.35)	0.13 (0.26)
Number of literate members/number of household members 5 years of age or older	0.48 (0.21)	0.58 (0.17)	0.48 (0.20)	0.60 (0.17)
% Households with migrant last 5 years	2.02	3.1	1.4	2.3
% Households with a person with disability	4.1	2.3	2.5[€]	1.7
% Households that had social security	4.9	8.9	4.5	13.1
% Households that own farm animal	31.6	37.8	26.8	39.8
Women's Characteristics				
N	1531	480	925	344
Age	29.04 (6.92)	27.00 (7.12)	29.20 (6.91) [€]	28.13 (7.50) [££]
Education				
% Without instruction	26.7	10	25.6	10.2
% Elementary	67.8	64.2	67.4	66.9
% Secondary o higher	5.6	25.8	7.0	23.0
% Indigenous language	42.4	21.9	40.5	18.6

Standard deviations in parenthesis
Comparison (a-c): [€] significant at 10%, [€€] significant at 5% . Test Ho by regression analysis adjusted by clustering at the locality level.
Comparison (b-d): [£] significant at 10%, [££] significant at 5% . Test Ho by regression analysis adjusted by clustering at the locality level.

Table D. Baseline Characteristics of Women with Births in Both Baseline and Follow-up Measures (Balanced Panel)

	Intervention		Control	
	Eligible[a]	**Non eligible[b]**	**Eligible[c]**	**Non eligible[d]**
Household characteristics				
N	284	56	146	55
Eligibility (poverty) score	622.26(86.29)	840.10(85.00)	624.51(85.58)	842.27(97.93)
% Enrolled to *Oportunidades*	95.42	33.93	--	--
Household size (number of persons in the household)	6.31(2.31)	5.80(2.33)	6.28(2.44)	5.90(2.62)
Women living in the household / Household size	0.50(0.16)	0.50(0.16)	0.48(0.17)	0.50(0.15)
Number of women 15 years old or older	1.34(0.68)	1.60(1.03)	1.28(0.59)	1.58(0.89)
Number of children age 12 or younger	3.48(1.62)	2.55(1.51)	3.47(1.70)	2.63(1.60)
Number of persons of age 18 or younger	4.04(2.11)	2.98(1.81)	3.93(2.15)	3.23(1.98)
Number of persons of age 65 or older	0.14(0.41)	0.19(0.51)	0.07(0.28)	0.11(0.36)
Number of indigenous members/Household size	0.36(0.34)	0.22(0.32)	0.34(0.34)	0.25(0.34)
Number of literate members/number of household members 5 years of age or older	0.44(0.20)	0.56(0.15)	0.41(0.20)	0.59(0.18)
% Households with migrant last 5 years	0.7	1.7	1.3	1.8
% Households with a person with disability	2.8	1.7	2.0	0
% Households that had social security	4.9	3.6	4.1	11
% Households that own farm animal	27.1	41.0	28.0	40.0
Women's Characteristics				
N	284	56	146	55
Age	27.54(6.34)	26.25(5.71)	27.91(6.06)	26.05(6.62)
Education				
% Without instruction	31.1	7.3	36.3	12.8
% Elementary	65.8	76.7	58.2	65.4
% Secondary o higher	3.1	16.0	5.5	21.8
% Indigenous language	54.2	30.3	47.9	36.3

Standard deviations in parenthesis

Comparison (a-c): [€] significant at 10%, [€€] significant at 5% . Test Ho by regression analysis adjusted by clustering at the locality level.

Comparison (b-d): [£] significant at 10%, [££] significant at 5% . Test Ho by regression analysis adjusted by clustering at the locality level.