Can Conditional Cash Transfers Improve Maternal Health and Birth Outcomes? Evidence from El Salvador's *Comunidades Solidarias Rurales*

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Abstract:

Although conditional cash transfers (CCTs) are traditionally evaluated in relation to child schooling and nutrition outcomes, there is growing interest in specifically examining maternal and reproductive health impacts. However, since data collection is not typically designed to evaluate these outcomes and sample sizes are often limited, there is a lack of rigorous evidence as to whether and through which pathways these effects may be realized. This paper uses regression discontinuity design and a unique implicit threshold to evaluate the impact of El Salvador's CCT program *Comunidades Solidarias Rurales* on a range of maternal and reproductive health outcomes: (1) prenatal care, (2) skilled attendance at birth, (3) birth in a health facility, and (4) postnatal care, using data collected by the International Food Policy Research Institute and its collaborators from women who entered the program in 2006 and 2007. Results indicate that robust impacts are found on outcomes at time of birth (skilled attendance and birth in facility), while no impacts are found on healthseeking behavior pre- and postbirth (prenatal and postnatal care). Potential impact pathways as well as the implications of these findings for program design are discussed in the conclusion.

Keywords: Conditional cash transfer, maternal health, El Salvador

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Please do not cite or distribute without permission. We are grateful to Myriam Margarita Beneke De Sanfeliu, Mauricio Arturo Shi Artiga, and others at FUSADES for helpful conversations and excellent data collection, to FISDL for funding and to Daniel Gilligan for contributions to methodology. All errors are our own.

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1. Introduction

Large-scale government conditional cash transfer (CCT) programs have become a mainstay in social protection and poverty reduction strategies throughout Central and South America and are increasingly being implemented in Sub-Saharan Africa and the Middle East (Fiszbein et al. 2009; Handa and Davis 2006; Lagarde, Haines, and Palmer 2007). Although CCTs are traditionally evaluated in relation to child schooling and nutrition outcomes, there is growing interest in specifically examining maternal health impacts. In fact, in a desk review of CCTs and health, Morris concluded that "in spite of the remarkable success of CCT programs in changing household behaviors, it is most unlikely that they have contributed anything to the global effort to reduce child and maternal mortality" (2010, 213).[1] Further, Morris suggested that in terms of health, the "greatest failure" of CCTs in Latin America and the Caribbean is "neglect of the very period in which the need for behavior modification is greatest: labor, delivery and the immediate postpartum recovery phase" (2010, 229) This claim is partially driven by lack of evidence, since data on maternal and reproductive health impacts has been either not collected or not analyzed in the context of most CCT evaluations. In most CCTs, in fact, the targeting of maternal health outcomes has been limited at best. The majority of current evidence is drawn from technical reports that examine maternal health as a secondary outcome, often descriptively. Recently there have been a few exceptions, which evaluate of the impact of CCTs on prenatal care, postnatal care, and skilled attendance at birth. Given the potential importance of, and the lack of evidence surrounding, the role of CCTs in maternal health, there is significant room for greater learning on where and why impacts are observed. This dialogue will be an important consideration as an increasing number of countries roll out, update, and modify CCT programs to move beyond targeting schooling and child nutrition.

In this paper, we contribute to the literature on the impacts of CCT programs on maternal health, focusing on the time around birth. We use a regression discontinuity design and a unique implicit

[1] Virtually no impact evaluations of CCTs have been conducted to determine effects on maternal mortality and morbidity per se because sample sizes would have to be quite large and beyond the scope of a traditional CCT evaluation. However, here we refer to maternal health indicators that have been linked to mortality, which include but are not limited to skilled attendance at birth, birth in a health facility, and pre- and postnatal care.

threshold to evaluate the impact of El Salvador's CCT program *Comunidades Solidarias Rurales* (CSR, formerly *Red Solidaria*)[2] on a range of maternal health outcomes at birth: (1) prenatal care, (2) skilled attendance at birth, (3) birth in a health facility, and (4) postnatal care. The CSR program, like many other CCTs in Latin America, consists of a monthly transfer typically given to female heads of households with children of school age and under five, conditional on school attendance and clinic visits. The data utilized for the analysis were collected in two rounds, in early and late 2008, by the International Food Policy Research Institute (IFPRI) in collaboration with the *Fundación Salvadoreña para el Desarrollo Económico y Social* (FUSADES) and the Government of El Salvador. We use a regression discontinuity design (RDD) combined with difference-in-difference (DID) methodology to determine the impacts of the program on the health outcomes listed above. An innovation in the paper is that we use an implicit variable as the forcing variable in estimating treatment effects. There was no single variable that determined program eligibility, so we used the methodology developed by de Brauw and Gilligan (2011) to construct the implicit forcing variable.

Our findings indicate that there are strong and robust impacts of CSR on skilled attendance at birth and on birth in a hospital setting; however, we find no impacts on prenatal or postnatal care. These results are robust to a number of sensitivity analyses, including varying the bandwidth, construction of kernels, addition of balancing control variables, and alternative constructions of variables measuring outcomes. In addition to income effects, it is possible that supply-side improvements and gains in women's decisionmaking agency are important factors leading to the results.

The paper proceeds as follows. In the next section, we discuss the pathways through which CCTs might impact maternal health. In the third section, we discuss details about the implementation of CSR relevant to the paper, and in the fourth section, we describe the data that will be used in the analysis. The fifth section includes a description of the regression discontinuity design used in this paper and other

[2] The program name was changed to *Comunidades Solidarias Rurales* in 2009 corresponding with the change of government, and we refer to the program throughout as such for consistency, even though the time period of analysis is in fact during the *Red Solidaria* phase of the program.

details of the methodology. The sixth section presents and discusses results, and the final section concludes with policy implications.

2. CCTs and Maternal Health: Impact Pathways and Current Knowledge

Although generally recognized for their broader poverty objectives, in many cases CCTs are well positioned to influence birth outcomes. We identify five distinct pathways through which CCTs may impact maternal health during pregnancy and birth. First, several CCTs, such as Mexico's *Oportunidades,* include free healthcare in the package of benefits, including prenatal care and care at delivery (Urquieta-Salomon et al. 2009). Especially in countries with high out-of-pocket health fees, the removal of user fees may be a particularly powerful incentive for women to use services. Second, programs may include prenatal or postnatal visits as part of the conditions for receiving the transfers, essentially engaging women in a contract to use health services. Third, programs may stimulate demand for care through health or nutrition training, often targeted toward women and offered in parallel with transfer payments. Fourth, CCTs may increase the supply of health services through investment in infrastructure and supply-side improvements of clinics in treatment communities. Fifth, economic theory suggests that CCTs will create an income effect: Household income increases with transfers from the CCT, as does the share of income that women control. As a result, one might expect increased demand for or utilization of health services in connection with pregnancy and birth. We do not expect these effects to be present in every CCT; their presence will likely depend upon the availability of services, the local context, and the program design. However, these potential pathways should be considered both in planning the programs and in explaining their effects.

With this framework in mind, we now turn to a review of current knowledge on the impacts of CCTs on maternal health and birth outcomes.[3] Within the broad spectrum of maternal health indicators, the majority of quantitative evidence is focused on health service utilization of pre- or postnatal care and delivery services, primarily from Mexico's *Oportunidades* (previously PROGRESA) program.[4] A recent

[3] We focus exclusively on CCTs and not payment waiver or voucher programs set up specifically for maternal health purposes, which pay women at facilities according to procedures obtained or give free or discounted services, since the latter are narrower and serve fundamentally different households from those typically targeted by CCTs.
[4] It is worth mentioning that qualitative evidence on CCTs and maternal health also exists. We use this evidence in our discussion of our findings, focusing here instead on the results of quantitative impact evaluations.

evaluation of *Oportunidades* on skilled attendance at birth among rural women concludes that in general, the program had no or little impact on attendance at birth, despite using a number of different matching and RDD evaluation techniques (Urquieta-Salomon et al. 2009). However, authors did find program impact on a select group of high-fertility women who had one birth just before and one just after program initiation. Other evaluations of *Oportunidades* have found that beneficiaries received 12.2 percent more procedures in prenatal care visits using a quality-of-care index as compared to nonbeneficiaries (Barber and Gertler 2009). The impact on the quality of care is attributed to the notion that women become more active and informed health consumers through their participation in CCTs. Previous research by the same authors found no significant impacts on use of prenatal care, number of visits, or proportion of women obtaining the minimum required visits, and attribute these finding to a lack of supply-side improvement in the health sector (Barber and Gertler 2008). Finally, *Oportunidades* has been found to increase rates of cesarean section at birth by 5.1 percentage points overall, 7.5 percentage points among women participating in the program for more than 5 months, and particularly among women giving birth in facilities run by social security and other government agencies (Barber 2009). Barber (2009) suggests that increases in disposable income drive this impact. However, several other explanations are also explored, including increased access to services and physician incentives.

Other evidence surrounding maternal health outcomes at birth comes from Honduras, Brazil, and India. Women receiving health vouchers in Honduras showed an 18 to 19 percentage point increase in 5 or more prenatal care visits; however, proportions were not balanced in the baseline and thus results were of questionable validity (Morris et al. 2004).[5] Reported in the same study, postnatal care visits within 10 days of birth in Honduras showed no statistically significant impacts (Morris et al. 2004). In contrast, an evaluation of Brazil's *Bolsa Alimentição* program found no significant impacts on either timing of first

[5] The baseline level of women receiving five or more prenatal visits among beneficiary households was 38 percent, while the control level was 49 percent. In addition, replication using routine government health facility data shows no impacts, which further calls into question the results (Morris et al. 2004).

prenatal visit or total number of visits; however, this result was expected, given that the sample size was relatively small, at 287 pregnant women (IFPRI 2003).

Lin et al. (2010) used two rounds of the India district-level household surveys to evaluate India's *Janani Suraksha Yojana* (JSY) program, a one-time cash incentive to encourage women to give birth in facilities. Results indicated that JSY had a positive impact on prenatal care, in-facility births or out-of-facility births with a skilled attendant, and in some models, a reduction of neonatal and perinatal deaths. In contrast, Lin and colleagues (2010) found that JSY had no effect on maternal deaths. Although JSY was the largest CCT running at that time in terms of beneficiaries, the authors noted targeting challenges and quality of healthcare as restricting factors in both program implementation and evaluation.

A related group of research examines the potential impact of CCTs on fertility decisions. The motivation for exploring potential linkages is related to the fear that CCTs that give benefits on a per-child basis, especially in relation to young children, may actually motivate families to increase fertility to gain program eligibility. Evidence from Mexico's *Oportunidades* and Nicaragua's *Red de Proteccion Social* indicate no program effects on fertility; however, research suggests that the *Programa de Asignacion Familiar* (PRAF) in Honduras increased fertility among eligible households by two to four percentage points (Stecklov et al. 2006). These differences are attributed to program design and the fact that PRAF both enrolled households contingent on new births and varied the transfer amounts according to the number of children in the household. Although fertility is an important and indeed primary maternal health indicator, the decisions surrounding fertility choices are complex. Due to our sampling strategy, it is not feasible to evaluate fertility choices in the present study of El Salvador. We therefore omit further discussion of fertility-related maternal health considerations.[6]

To summarize, the evidence concerning impacts of CCTs on maternal health is scarce and often piecemeal. In this paper, we contribute to this literature both by providing new estimates on the impacts

[6] Several papers study other issues under the broad concept of maternal health as it relates to participation in CCT programs. For example, Baird, McIntosh, and Ozler (2009) and Baird and others (2009) studied sexual behavior among teenage girls enrolled in a CCT in Malawi, in the context of HIV prevention. Other papers have studied the impacts of *Oportunidades* on the incidence of marriage dissolution, contraceptive use, and domestic violence (Bobonis, Castro, and Gonzalez-Brenes 2009; Lamadrid-Figueroa et al. 2008; Bobonis 2011).

of a CCT on a range of measures of maternal health and by exploring the mechanisms by which these impacts may have taken place. In the next section, we describe the context of El Salvador's CCT program.

3. El Salvador and *Comunidades Solidarias Rurales*

El Salvador is the smallest in geographic area and the most densely populated country in Latin America, bordered by Guatemala and Honduras to the north and the Pacific Ocean to the south. The gross national income per capita is US$3,370,[7] and approximately 30.7 percent of the population is below the poverty line (World Bank 2009). According to the most recent nationally representative survey, carried out in 2008, in the 5 years preceding the survey approximately 78 percent of women of reproductive age completed the 5 recommended prenatal visits, 84 percent gave birth in hospitals, and 54 percent completed postnatal visits in the 6 months after giving birth (FESAL 2009). These figures all showed increases from 2002/03, but they varied substantially within different regions and from rural to urban areas.

El Salvador began implementing CSR in 2005. The implementing government agency, *Fondo de Inversión Social para el Desarrollo Local* (Social Investment Fund for Local Development, FISDL), first carried out a census in each municipality to determine program eligibility and soon thereafter began distributing payments to each eligible household. Households were eligible for the health transfer if either a member was pregnant at the time of the census or a child residing in the household was 5 years of age or younger. To be eligible for education transfers, households were required to have children ages 6 to 15 residing in the household who had not completed primary school.[8] Transfers were conditioned on growth-monitoring visits every 2 months and vaccination status for children, and prenatal monitoring for pregnant women. Transfer amounts were $15 per month for households eligible for the health benefit, and $20 per month for households eligible for both the health and education benefits. In addition to monetary transfers, monthly information sessions (*capacitaciónes*) were offered at local village centers on topics such as education, nutrition, health, and women's or children's rights. Although attendance was taken at these sessions, attending was not a condition for receiving the transfer. Finally, in addition to household-level incentives, the government also implemented a series of supply-side improvements in the water and

[7] All dollar amounts are in U.S. dollars.
[8] For municipalities entering the program in 2008 and 2009, the upper end of this age range increased to 18 years.

sanitation infrastructure of the communities as well as making health systems investments. Improvements in health systems took place in almost all municipalities participating in CSR.

CSR is primarily geographically targeted. The program was initially rolled out in the poorest 15 municipalities in the country and now operates in 100 municipalities. Targeting occurred through a 2-step selection process. First, all of the municipalities in El Salvador were grouped by levels of extreme poverty, based on 2 indicators: the poverty rate, measured using data collected at the municipality level from 2001 to 2004; and the prevalence of severe stunting (the proportion of children more than 3 standard deviations below the mean height-for-age z-score) among first graders in the 2000 height census, using partitioned cluster analysis. The 2 highest poverty groups, termed "severe extreme poverty" and "high extreme poverty" by the government, were targeted for the program. The 32 municipalities in the severe extreme poverty group entered CSR in 2005 and 2006, and the 68 municipalities in the high extreme poverty group entered in 2007, 2008, and 2009. To determine the order of priority within each severe poverty group, municipalities were ranked from poorest to least poor using a municipality marginality index (IIMM in Spanish) within each group. The IIMM is a declining welfare index based on poverty, education levels, and housing conditions.[9] Therefore, the municipalities in the severe extreme poverty group entered the program first, followed by those in the high extreme poverty group; within the severe extreme poverty group, the 15 municipalities with the highest IIMMs entered CSR in 2005, whereas the remaining 17 entered in 2006. Important for the purposes of this paper are the municipalities that entered in 2006, having relatively lower IIMMs within the severe extreme poverty group, and the 15 municipalities entering in 2007, which have the highest IIMMs within the high extreme poverty group. These 2 groups will be used to construct the treatment and control groups. Because there were no additional targeting rules apart from those discussed above, the estimates in this paper can be considered intent-to-treat estimates.

[9] The extreme poverty groups and the IIMM were constructed independently, so that some municipalities in the poorest extreme poverty group have a lower IIMM score than some municipalities in the next poorest extreme poverty group.

4. Data

The data used for this paper were collected by FUSADES in collaboration with researchers at IFPRI, and households included in the sample were chosen explicitly to evaluate the impact of CSR on several indicators of infant and maternal health, education, and nutritional status, including some of the indicators used in this paper. The baseline data were collected in January and February of 2008, and a second survey was done between September and November of 2008.[10] The survey form included sections on household demographics, education, health, time allocation and off-farm labor, housing and consumer durables, agriculture, migration, other income sources, consumer expenditures, and community participation in programs, including CSR. The sample includes 100 *cantones* in 50 municipalities.

The sampling strategy for data collection was explicitly designed to ensure adequate sample sizes to examine outcomes specific to maternal and young child health. For the baseline survey, 15 households with children under 3 years old or with a pregnant woman resident and 15 households with children between the ages of 6 and 12 were selected randomly within each *canton* from census lists, for a total of 30 households per *canton*. For the second survey, all households that remained in these demographic groups were retained, and the sample was replenished to ensure a total sample size of approximately 3,000 households. Since one of the primary indicators was growth monitoring among children under a year old, the survey team visited health clinics in each of the municipalities to learn about recent births in each of the sample *cantones* between surveys. Households were randomly selected from among those identified during the health clinic visits as replacement households during the second survey.

To construct the sample for this paper, the key module in the survey specifically focused on maternal health and collected pregnancy histories for all women and adolescent girls over the age of 12. In the baseline survey, mothers were asked about all current and previous pregnancies occurring since the beginning of 2006, so that experiences prior to program implementation were available for all households. In the second survey round, households were asked only about pregnancies that had occurred in the past

[10] Third and forth survey rounds were collected in 2009 and 2010, however because of the phased roll in, they are not appropriate to use for this analysis.

12 months. Among mothers that lived in households interviewed in both rounds, we carefully examined the combined data on pregnancies to ensure that each pregnancy was included only once. The resulting sample included approximately 530 women with valid responses for attendance at birth and birth in facility, and approximately 494 women with valid responses for prenatal and postnatal care.

As previously mentioned, we use the subsample of municipalities that entered CSR in either 2006 or 2007 for this paper. In the language of program evaluation, the 2006 entry group can be considered the "treatment" group and the 2007 group the "control" group; we do not include births or pregnancies that took place after the 2007 entry group began receiving payments.[11] All of our impact estimates are based on the difference in differences. We define "before" and "after" as follows: For the 2006 entry group, we use the date of the first payment in each municipality as a cutoff between the before and after periods. For indicators measured at or after birth, we consider the birth preprogram if it occurred before the payment date and postprogram if it occurred after the payment date. To break up the control group into before and after periods as well, we use the median start date among the 2006 entry group, October 1, as the cutoff between the before and after periods (see Figure 4.1).[12] For prenatal care indicators, we define the cutoff period slightly differently, whereby the woman must be at least two months pregnant by the time of the initial municipality-specific payment date as a cutoff. We use two months as the threshold because by this time women are likely to be aware of the pregnancy and thus there is potential for behavior change such as initiating a health clinic visit or prenatal care.

[11] This assumption might seem to place significant limits on the data we are able to use. However, in both entry groups the program began late in the second half of the year, and so in practice few observations are dropped.
[12] Results in the paper are robust to minor changes in the cutoff date for the before and after periods in the control group.

5. Methodology and Key Indicators

Regression Discontinuity Framework

The regression discontinuity design (RDD) approach measures program impact by comparing outcomes between beneficiaries and nonbeneficiaries with eligibility criteria near a certain eligibility threshold. Under specific assumptions, RDD provides consistent estimates of program impact (or treatment effects) (Edmonds, Mammen, and Miller 2005).[13] First, the probability of treatment must vary discontinuously at the threshold. Intuitively, the sharp cutoff point serves as an instrumental variable that affects program participation but does not independently affect outcomes. Second, observations just above and below the threshold must be similar in both their observed and unobserved characteristics. Third, the outcome must be continuous at the threshold in the absence of the treatment. In other words, there should be no sharp break in outcome measures in the population at large for those just below and just above the threshold.

From the perspective of evaluating CSR, we know that specific criteria influenced the order of entry at the municipality level. By the end of 2006, all municipalities in the severe extreme poverty group had entered CSR, and in 2007, municipalities rated with high extreme poverty had begun to enter CSR. While each of the three assumptions listed above is assured to hold for the data, there is no explicit numerical threshold between the 2006 and 2007 entry groups, since the poverty groups were formed using a partitioned cluster means analysis.[14] To use RDD as an identification strategy, recall that the extreme poverty groups were chosen on the basis of two variables, the severe stunting rate among first graders and the poverty rate. We plot each municipality in the two extreme poverty groups on an axis measuring those two variables and measure the distance of each municipal score from each cluster mean score (Figure 5.1). We then construct the difference in distances from each cluster center for every municipality as an

[13] These conditions are relevant to the "sharp" RDD, where eligibility is determined entirely by a selection rule on observed characteristics. When this selection rule is only one determinant of program participation, a "fuzzy" RDD estimator may be used. The approach to targeting in CSR is consistent with the sharp RDD estimator.

[14] Moreover, the strict application of eligibility thresholds based on extreme poverty groups is consistent with the RDD methodology and renders other evaluation techniques, such as propensity score matching or covariate matching, infeasible.

implicit threshold; de Brauw and Gilligan (2010) demonstrated that the implicit threshold meets the three criteria above.

There is a further complication with applying RDD to the evaluation of CSR: Eligibility was determined at the municipality level rather than the household or individual level. Though outcomes are measured at the household level through the evaluation surveys, the unit of intervention is the municipality. Statistical power to differentiate impacts depends in part on the number of municipalities near the IIMM eligibility threshold in any given year, which is relatively small. Regardless of this limitation, the RDD evaluation approach has been increasingly implemented at a level of aggregation larger than the household or individual, and thus is not in itself a primary concern (Chay, McEwan, and Urquiola 2005; Leuven et al. 2007; Ludwig and Miller 2007; Van Der Klaauw 2008).

To construct RDD estimates, we generally follow the strategy of constructing estimates of changes in average outcomes using separate nonparametric, one-sided, kernel-weighted estimates of average outcomes for the treatment group, $\hat{\mu}\left(y_t^T\right)$, and comparison group, $\hat{\mu}\left(y_t^C\right)$, respectively:

$$\hat{\mu}\left(y_t^T\right)=\frac{\sum_{X\in T} y_{it} k\left(\frac{\left|X_j-c\right|}{h}\right)}{\sum_{X\in T} k\left(\frac{\left|X_j-c\right|}{h}\right)} \text{ and } \hat{\mu}\left(y_t^C\right)=\frac{\sum_{X\in C} y_{it} k\left(\frac{\left|X_j-c\right|}{h}\right)}{\sum_{X\in C} k\left(\frac{\left|X_j-c\right|}{h}\right)}, \quad (1)$$

where k is the kernel function, X_j is the eligibility criterion for the jth municipality, c is the threshold cutoff score determining eligibility, and h is the bandwidth. The estimated impact of the program is therefore $\hat{\beta}=\hat{\mu}(y_t^T)-\hat{\mu}(y_t^C)$.

To estimate the average outcomes, then, we must choose a kernel function, which can take several different forms. Since nonparametric kernels are subject to greater bias when estimating impacts near the threshold relative to other approaches (Hahn, Todd, and Van Der Klaauw 2001; Imbens and

Lemieux 2008), in this paper we work with the uniform kernel and a locally linear kernel. Nonetheless, we have estimated all results in the paper using nonparametric kernels, and they are largely similar. As a result, we discuss how we implement the uniform kernel and the locally linear kernel below.

Using the uniform kernel, the weight on each observation within the bandwidth is equivalent. As a result, one can simply estimate the program impact using a simple ordinary least squares (OLS) regression, which takes the form

$$y_i = \beta_0 + \beta_1 t_i + \beta_2 CSR_i + \beta_3 CSR_i \cdot t_i + \varepsilon_i, \quad (2)$$

where t_i is an indicator equal to 0 in the first year of data and 1 in the second year, CSR_i equals 1 if the municipality enters CSR during this period and 0 otherwise, ε_i is a random error term, and i indexes households. The regression is run only on observations for which $|X_i - c| \leq h$; if the difference between the indicator and the threshold is not within the bandwidth, the observation is dropped. The coefficient β_3 provides a DID estimate of the impact of the program on the change in the outcome over time.

Our second approach is to use a local linear regression (LLR) method to fit linear parametric functions to the outcome data on both sides of the threshold. Allowing for complete flexibility of the slopes, the DID version of this approach can be calculated by estimating a regression of the form

$$y_i = \beta_0 + \beta_1 t_i + \beta_2 D_j + \beta_3 D_j t_i + \beta_4 CSR_i + \beta_5 CSR_i \cdot t_i + \beta_6 CSR_i \cdot D_j + \beta_7 CSR_i \cdot D_j \cdot t_i + \varepsilon_i, \quad (3)$$

where $D_j = X_j - c$ is the distance of the eligibility criterion for the jth municipality from the cutoff threshold score for eligibility. The coefficient β_4 in this regression provides the LLR DID estimate of the impact of the program on the change in the outcome over time $\left(\beta_4 = \Delta_{DID}^{ATT-LLR}\right)$.

Because the RDD methodology is most effective for observations closest to the threshold, a bandwidth on the eligibility threshold X_j (the IIMM score or implicit extreme poverty group threshold) is set to restrict the sample to treatment and comparison municipalities close to the threshold. This restriction has the attraction of reducing bias in the estimated impacts because households closest to the threshold are more likely to differ only because of access to the program. However, a cost of this

restriction is a reduction in estimation sample and therefore a loss in statistical power. As a result, we experimented with several alternative bandwidths for each estimator to examine this tradeoff between bias and efficiency. Where the choice of bandwidth affected the impact estimate, we report the robustness of estimated impacts to alternative bandwidths.[15]

Outcome Indicators and Control Variables

We examine four main outcome indicators reflecting different stages of healthcare utilization over the pregnancy and birth periods: (1) adequate prenatal care, (2) skilled attendance at birth, (3) birth in a health facility, and (4) postnatal care. Adequate prenatal care is defined as at least five visits during the pregnancy as recommended by the Salvadoran Ministry of Health.[16] Skilled attendance at birth is defined as attendance by a general practitioner doctor or an obstetrician/gynecologist, along with a nurse. Birth in facility is defined as birth in a government or private hospital and excludes births taking place at health centers or at mobile health clinics. The receipt of postnatal care is defined as meeting with a health professional for a checkup within two weeks after giving birth.[17]

Although methodologically it is not necessary to include control variables in our estimation equations to identify the impact of CSR, we include several control variables in versions of our estimates both to ensure that their inclusion does not affect parameter estimates and to attempt to improve statistical significance by explaining some of the variance in the outcome. We do so by running alternative specifications, adding individual, household, and community-level control variables to the RDD estimators. The individual-level control variables included are the mother's age in years and splines for educational attainment and marital status, while household-level control variables are a count index of infrastructure services (piped water, flush toilet, and electricity) and an index of household asset holdings

[15] Imbens and Kalyanaraman (2009) demonstrated a method for computing an "optimal" bandwidth that balanced the tradeoff between theoretical bias in estimates and the sample size and resulting standard error estimates used in calculating treatment effects. However, their method was optimized for an individual-level forcing variable rather than a cluster-based forcing variable.

[16] However, note that results do not change when we consider alternative indicators of prenatal care utilization, including an indicator of any prenatal visit in the first four months or an indicator of the number of prenatal visits.

[17] Again note that results do not change when we consider an alternative indicator extending the time frame for a postnatal visit up to six weeks.

created using principal components analysis (Filmer and Pritchett 2001).[18] Finally, the log of the distance to a health center in kilometers is included to control for access to health services.

One requirement for RDD estimates to be valid is that control variables not be discontinuous on either side of the threshold (Edmonds, Mammen, and Miller 2005). We initially compute mean values of control variables on either side of the threshold (Table 5.1). We find no discontinuity at the threshold, so we can conclude that the explanatory variables are balanced on either side of the implicit threshold.

[18] These assets are included in the factor score: radio, TV, stereo, VCR, fan, computer, typewriter, sewing machine, vehicle (car or truck), boat, bicycle, cart/oxcart, motorcycle, refrigerator, stove, mill, blender, generator, solar panel, bed, and other furniture.

6. Results

We initially describe the proportion of mothers receiving adequate prenatal care, skilled attendance at birth, birth in a health facility, and postnatal care by entry group and by whether or not the care occurred pre- or post-treatment for the treatment group (Table 6.1). We find large increases in skilled attendance at birth and births in health facilities for the treatment group in the post-2006 round, while there are modest increases in both indicators among the control group. For example, skilled attendance at birth increases from 73.8 percent for the treatment group in the pre-entry period to 90.3 percent in the post-entry period, while the increase for the control group is from 63.3 percent to 65.9 percent. However, there are no positive changes in prenatal or postnatal care among the treatment group, which is at 75.4 and 23.2 percent of the sample, respectively, in the post-entry period. In fact, in the control group there are small increases in both of these indicators.

Tables 6.2 through 6.5 summarize the RDD results of each maternal health outcome, where the coefficient and standard errors are reported only for the indicator of CSR program impact (interaction between entry group and time period). Columns 1 through 3 report results from rectangular kernel estimates, first without controls and then adding individual and household controls, while columns 1a through 3a report the same for results for local linear estimates. Results are presented both among the full sample (first row) and subsequently narrowing the Euclidean distance bandwidth to 8 and then to 5 from the IIMM. Full regression results for the entire sample are included in the appendix (Tables A.1 through A.4) for each maternal health outcome. On average, our sample consists of women aged 25.77 (treatment sample) and 27.02 (control sample) years with less than secondary education who are in either legal or common-law marriages. Age is the only control variable that shows statistically significant difference within the full sample, and this difference is not observed when we restrict the sample to just the baseline.

We first show that the estimated coefficient representing the impact of CSR on adequate prenatal care is negative, but not statistically different than zero (Table 6.2), regardless of kernel or bandwidth. This result is consistent with graphical evidence (Figure 6.1); when we examine municipal averages, we essentially observe no difference in the relationship between adequate prenatal monitoring and transfers

associated with CSR. Although the program did not have an impact on whether women received adequate prenatal monitoring, we did find that almost all women did at least sign up for prenatal monitoring, and of those women who did not attend the minimum five visits to be considered adequate, almost all attended four (de Brauw et al. 2010).

We find particularly positive results when we examine the impact of CSR on skilled attendance at birth (Table 6.3). Graphically, we observe no change year to year at the threshold but large changes among mothers in municipalities close to the threshold (Figure 6.2), consistent with the point estimates of impacts that range from 12.3 to 17.4 percentage points, with the exception of the lowest bandwidth of local linear models. Descriptive statistics show that the bulk of this change is due to a shift in attendance at birth by midwives (*partera*s) to attendance by obstetrician/gynecologists and other medical doctors.

Perhaps not surprisingly, we also find a significant impact of CSR on a similar measure, births occurring in hospitals. Point estimates for impacts on births reported as taking place in hospitals largely mirror the results on skilled attendance at birth, though they are slightly larger, ranging from 15.3 to 22.8 percentage points (Table 6.4). Graphically, we observe a steeper relationship between the proportion of births in hospitals at the municipal level and the forcing variable among the 2006 entry group than we observe in the same relationship for the 2007 entry group (Figure 6.3). Although there is a significant correlation between births taking place in hospitals and births attended by skilled professionals, the two measures do not fully overlap. However, these results do both indicate that the program has a significant impact at the time of birth.

Finally, we estimate the impact of CSR on whether or not women obtain postnatal care (Table 6.5). We largely find negative point estimates that are not significantly different from zero. Local linear regressions on either side of the threshold nearly match at the threshold, indicating no program impact (Figure 6.4). Clearly if messages about the importance of receiving any postnatal care are part of CSR, they are either not getting through to women, or being confused with other health messaging, for example for growth monitoring.

In summary, then, we find impacts of CSR on measures of maternal health that are centered directly around the birth of the child; we demonstrate impacts on both skilled attendance at birth and whether or not births take place in hospitals. However, we do not find impacts on measures of maternal health before and after the birth, specifically whether or not women are getting adequate prenatal or postnatal care. In the next section, we discuss the pathways by which the positive findings may have occurred as well as the reasons we believe we do not find impacts on the latter two sets of variables.

7. Discussion and Conclusion

While CCTs are quickly becoming a mainstay of social protection and government welfare programs, there is little evidence about how these programs have affected maternal health and what design components in CCTs may lead to favorable outcomes. In the previous section, we demonstrate that CSR in El Salvador has had robust impacts on outcomes at the time of birth (skilled attendance and birth in facility), while it has had no impacts on healthseeking behavior before and after birth (prenatal and postnatal care). These results imply that there are important nuances to program design and implementation that must be taken into account for a CCT to successfully affect maternal health outcomes. For example, the main incentives and penalties, cash and conditions, are thought to drive many of the positive outcomes observed in children's schooling and nutrition in many countries. However, this formula may not hold for women's outcomes, since prenatal care is a condition for program recipients in CSR. Since CSR has had strong positive impacts on outcomes at the time of birth, alternative design components of CCTs may be very important in delivering positive results for maternal health.

As in many CCT programs, CSR did not condition its payments on either skilled attendance at birth or giving birth in a hospital setting. Therefore, the impact pathway is not immediately obvious. In considering potential impact pathways, we identify three ways in addition to the income effect and conditionality through which CSR had the potential to impact maternal health: (1) increase in health knowledge surrounding birth outcomes through *capacitaciónes* (demand for health services); (2) supply-side improvements in health facilities; and (3) gains in women's decisionmaking, which allow healthseeking for services that beneficiaries believe to be important. Although we are unable to isolate the impact pathway for each of these explanations, we are able to provide descriptive and supporting evidence for each in turn.

Capacitaciónes are an integral part of service delivery of CSR and are offered on a monthly basis at a local meeting point, such as a church, school, or government building. Although they are not officially required for program recipients, essentially all beneficiaries attend *capacitaciónes* (74.8 percent report attending in the last month in the second 2008 survey) and nearly all program recipients believe

they are conditions for payments, though they are not (97.5 percent in the second 2008 survey). Trainings are run by the implementing NGOs with curricula predetermined by FISDL. However, when we examine reports of ever having attended training on either infant and child health or family health, we do not find clear trends that earlier entry groups have higher exposure to these modules. In fact, overall the reports of ever having attended training on either subject are higher among the later entry group than among the early entry group (66.6 versus 52.8 percent for infant and child health; 45.1 percent versus 38.4 percent for family health). Therefore, although *capacitaciónes* may be contributing to overall increases in healthseeking behavior, we do not find a clear link to program impacts.

The second possible pathway is through the supply side, or increase in basic infrastructure and quality of health services in CSR communities. Health facility surveys were undertaken in the baseline and third survey; thus although the measurements are not strictly comparable to the time frame for this analysis, they are still indicative of trends in service improvements. We find that overall there have been improvements in availability and quality of health services; however, again, trends in communities among the early entry group are not significantly higher than those in the later entry group (de Brauw et al. 2010). We find that the number of skilled personnel (doctors of any kind and trained nurses) is higher, which suggests potential results for outcomes at birth, but we are not able isolate this impact.

Finally, it is possible that impacts are partially influenced positively by gains in women's decisionmaking agency and empowerment. Qualitative and ethnographic evidence conducted as part of the IFPRI evaluation (Adato et al. 2009) find that CSR increased women's decisionmaking agency through increasing the amount of cash under the control of women and through their participation in *capacitaciónes*. Therefore, although women's empowerment is notoriously hard to measure in quantitative impact evaluation, mixed-methods results imply that this pathway may be particularly important for maternal health outcomes, especially in combination with increased supply.

How can we design components of CCTs to increase likelihood of improved maternal health and birth outcomes? Although prenatal care has been a conditioning healthseeking behavior in CCTs in other countries (for example, Mexico, Honduras, and Brazil), it is not clear in general if conditionality is

effective at increasing the number of women receiving an adequate number of prenatal visits. Unfortunately, we are limited in our ability to ascertain whether the quality of prenatal care increased over these visits, as was found in the case of *Oportunidades* in Mexico (Barber and Gertler 2008). Despite this data limitation, perhaps more relevant is the lack of global evidence linking frequency, timing, or quality of care in prenatal visits to health gains for infants (Carroli et al. 2001; Villar et al. 2001). In contrast, these linkages have been well documented and established in the case of skilled attendance at birth and birth in a healthcare facility, which is reflected in inclusion of this target indicator as part of the Millennium Development Goals (WHO 2004, 2005).[19] Further, in a review of the ability of CCTs to increase the utilization of health interventions, Lagarde and colleagues (2007) concluded that although CCTs have done modestly well in encouraging healthseeking behavior and health service utilization, evidence with respect to actual health outcomes has been mixed. This finding leads us to propose that perhaps a more effective requirement in the case of El Salvador and similar programs may be enrollment in prenatal care in the first three months of pregnancy (instead of requiring four or five visits, varying by country), followed by a condition of skilled attendance at birth and one postnatal visit within two weeks of birth. The latter may be especially important in settings like El Salvador, where the rates of postnatal care are quite low. In addition, interactions with health services across the pre- and postnatal period have a greater opportunity to address a variety of maternal health concerns. For example, as part of postnatal care, family planning counseling and cervical cancer screening could be required, both of which are important components that may be omitted during prenatal care, when the focus is on the pending pregnancy and birth planning. To our knowledge, the only CCT program that includes behavior at the time of birth in its conditions (skilled attendance, for example) is India's JSY program, which was a one-time payment at the time of birth. Based on the success of JSY and evidence from El Salvador, conditions on behavior at the time of birth are a potential area of further exploration.

[19] However, note that there are ongoing debates surrounding many issues of skilled attendance, including level of training, technical assistance, provider attitudes, and the like. (Stanton 2008).

As a final note, as an increasing number of CCTs are rolled out, data collection and impact evaluation efforts should take into account not only child health but also the maternal health components of healthseeking and outcomes. Because of sample size limitations, attention to these components may require oversampling pregnant mothers in the baseline data or collecting more detailed information on fertility, prenatal care, and birth indicators. The lack of rigorous evidence on these outcomes, with the exception of findings across a variety of indicators from *Oportunidades* and JSY, are a limiting factor in advancing and making sound recommendations in this area. In light of the current public health and nutrition emphasis on the first two years of life as critical windows of opportunity for determining future health, education, and labor force outcomes, failing to include attention to maternal health is a missed opportunity.

8. References

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Table 5.1 Exogeneity of threshold point: Balancing control variables and IIMM

	Full sample			Baseline sample		
	CSR mean	Comparison mean	p-value	CSR mean	Comparison mean	p-value
Mother's age (in years)	27.0	27.4	0.466	25.98	27.28	0.112
Third cycle (up to 9th year =1)	0.20	0.19	0.450	0.11	0.17	0.104
Diploma (up to 12th year =1)	0.09	0.11	0.129	0.10	0.11	0.838
Never married (=1)	0.15	0.15	0.680	0.18	0.13	0.242
Separated/divorced/widowed (=1)	0.06	0.08	0.274	0.11	0.08	0.328
Services index (1-3)[1]	1.27	1.31	0.434	1.46	1.35	0.222
Asset index (continuous)[2]	-0.15	-0.09	0.196	-0.09	-0.11	0.865
Log distance to health center (in km)	3.52	3.52	0.981	3.43	3.51	0.557

Source: CSR baseline and second survey.

Notes: [1] Sum of indicators for (1) piped water, (2) electricity, and (3) toilet in house.

[2] Factor score for household assets including radio, TV, stereo, VCR, fan, computer, typewriter, sewing machine, vehicle, boat, bicycle, cart/oxcart, motorcycle, refrigerator, stove, mill, blender, generator, solar panel, bed, other furniture.

Table 6.1—Descriptive statistics of maternal health indicators

	Pre-2006 entry	Post-2006 entry	Sample size
Panel A. Adequate prenatal monitoring (5 or more visits)			
2006 entry group (treatment group)	0.768	0.754	269
	[0.424]	[0.432]	
2007 entry group (control group)	0.769	0.826	225
	[0.423]	[0.382]	
Panel B. Birth attended by skilled personnel			
2006 entry group (treatment group)	0.738	0.903	249
	[0.441]	[0.298]	
2007 entry group (control group)	0.633	0.659	287
	[0.483]	[0.477]	
Panel C. Gave birth in hospital			
2006 entry group (treatment group)	0.733	0.903	249
	[0.444]	[0.298]	
2007 entry group (control group)	0.623	0.633	281
	[0.486]	[0.485]	
Panel D. Mother went for postnatal checkup (first 2 weeks following birth)			
2006 entry group (treatment group)	0.259	0.232	226
	[0.439]	[0.426]	
2007 entry group (control group)	0.192	0.224	252
	[0.395]	[0.419]	

Source: CSR baseline and second survey.

Notes: Estimates use the baseline and second survey. Mean values with standard deviations reported below in [ ] brackets.

Table 6.2—RDD results for the impact of *Comunidades Solidarias Rurales*on the proportion of births with adequate prenatal care, comparing 2006 entry to 2007 entry

	Rectangular kernel			Local linear		
Sample	(1)	(2)	(3)	(1a)	(2a)	(3a)
Full sample *(N = 494)*	-0.070	-0.058	-0.065	-0.072	-0.061	-0.068
	(0.072)	(0.072)	(0.072)	(0.073)	(0.072)	(0.072)
R^2	0.003	0.017	0.015	0.004	0.018	0.025
Euclidean distance bandwidth (8) *(N = 405)*	-0.131	-0.114	-0.124	-0.120	-0.104	-0.114
	(0.085)	(0.085)	(0.083)	(0.089)	(0.089)	(0.086)
R^2	0.006	0.022	0.031	0.015	0.031	0.038
Euclidean distance bandwidth (5) *(N = 365)*	-0.112	-0.101	-0.108	-0.095	-0.083	-0.089
	(0.084)	(0.086)	(0.085)	(0.090)	(0.088)	(0.086)
R^2	0.006	0.024	0.032	0.010	0.028	0.037
Includes individual controls	N	Y	Y	N	Y	Y
Includes all controls	N	N	Y	N	N	Y

Source: CSR baseline and second survey.

Notes: Estimates use the baseline and second survey. Standard errors clustered by municipality in parentheses below coefficients. * indicates significance at the 10% level, ** significance at the 5% level, and *** significance at the 1% level. Bandwidth refers to the distance to the IIMM, where observations outside this distance are excluded from the sample. Control variables are listed in Table 5.1.

Table 6.3—RDD results for the impact of *Comunidades Solidarias Rurales*on the proportion of births with skilled attendance at birth, comparing 2006 entry to 2007 entry

	Rectangular kernel			Local linear		
Sample	(1)	(2)	(3)	(1a)	(2a)	(3a)
Full sample *(N = 536)*	0.139*	0.134*	0.123*	0.150*	0.142*	0.130*
	(0.074)	(0.070)	(0.069)	(0.075)	(0.072)	(0.070)
R^2	0.035	0.059	0.081	0.049	0.074	0.100
Euclidean distance bandwidth (8) *(N = 414)*	0.127**	0.141**	0.138**	0.125**	0.137*	0.136*
	(0.054)	(0.064)	(0.060)	(0.060)	(0.072)	(0.069)
R^2	0.033	0.069	0.096	0.074	0.109	0.136
Euclidean distance bandwidth (5) *(N = 365)*	0.174***	0.178**	0.164**	0.145*	0.152	0.13
	(0.057)	(0.073)	(0.075)	(0.086)	(0.099)	(0.097)
R^2	0.048	0.079	0.102	0.084	0.113	0.144
Includes individual controls	N	Y	Y	N	Y	Y
Includes all controls	N	N	Y	N	N	Y

Source: CSR baseline and second survey.

Notes: Estimates use the baseline and second survey. Standard errors clustered by municipality in parentheses below coefficients. * indicates significance at the 10% level, ** significance at the 5% level, and *** significance at the 1% level. Bandwidth refers to the distance to the IIMM, where observations outside this distance are excluded from the sample. Control variables are listed in Table 5.1.

Table 6.4—RDD results for the impact of *Comunidades Solidarias Rurales*on the proportion of births in hospitals, comparing 2006 entry to 2007 entry

	Rectangular kernel			Local linear		
Sample	(1)	(2)	(3)	(1a)	(2a)	(3a)
Full sample *(N = 530)*	0.160*	0.163**	0.153*	0.171*	0.171**	0.159**
	(0.081)	(0.076)	(0.076)	(0.083)	(0.078)	(0.077)
R^2	0.038	0.059	0.081	0.054	0.075	0.101
Euclidean distance bandwidth (8) *(N = 409)*	0.177***	0.195***	0.191***	0.174**	0.190***	0.189***
	(0.061)	(0.059)	(0.050)	(0.067)	(0.067)	(0.059)
R^2	0.035	0.068	0.096	0.071	0.102	0.132
Euclidean distance bandwidth (5) *(N = 360)*	0.223***	0.228***	0.214***	0.193**	0.201***	0.178**
	(0.052)	(0.052)	(0.052)	(0.075)	(0.073)	(0.069)
R^2	0.072	0.072	0.098	0.087	0.110	0.144
Includes individual controls	N	Y	Y	N	Y	Y
Includes all controls	N	N	Y	N	N	Y

Source: CSR baseline and second survey.

Notes: Estimates use the baseline and second survey. Standard errors clustered by municipality in parentheses below coefficients. * indicates significance at the 10% level, ** significance at the 5% level, and *** significance at the 1% level. Bandwidth refers to the distance to the IIMM, where observations outside this distance are excluded from the sample. Control variables are listed in Table 5.1.

Table 6.5 RDD results for the impact of *Comunidades Solidarias Rurales*on the proportion of births with postnatal care in first 2 weeks, comparing 2006 entry to 2007 entry

	Rectangular kernel			Local linear		
Sample	(1)	(2)	(3)	(1a)	(2a)	(3a)
Full sample *(N = 478)*	-0.059	-0.058	-0.059	-0.066	-0.063	-0.064
	(0.099)	(0.098)	(0.100)	(0.099)	(0.099)	(0.101)
R^2	0.005	0.013	0.014	0.014	0.023	0.024
Euclidean distance bandwidth (8) *(N = 367)*	-0.106	-0.101	-0.104	-0.098	-0.092	-0.094
	(0.118)	(0.117)	(0.117)	(0.116)	(0.118)	(0.119)
R^2	0.005	0.011	0.014	0.021	0.028	0.030
Euclidean distance bandwidth (5) *(N = 320)*	-0.094	-0.090	-0.093	-0.132	-0.124	-0.130
	(0.138)	(0.138)	(0.140)	(0.165)	(0.161)	(0.161)
R^2	0.003	0.011	0.015	0.017	0.026	0.030
Includes individual controls	N	Y	Y	N	Y	Y
Includes all controls	N	N	Y	N	N	Y

Notes: Estimates use the baseline and second survey. Standard errors clustered by municipality in parentheses below coefficients. * indicates significance at the 10% level, ** significance at the 5% level, and *** significance at the 1% level. Bandwidth refers to the distance to the IIMM, where observations outside this distance are excluded from the sample. Control variables are listed in Table 5.1.

Figure 4.1— Illustration of treatment and control groups for maternal health outcomes

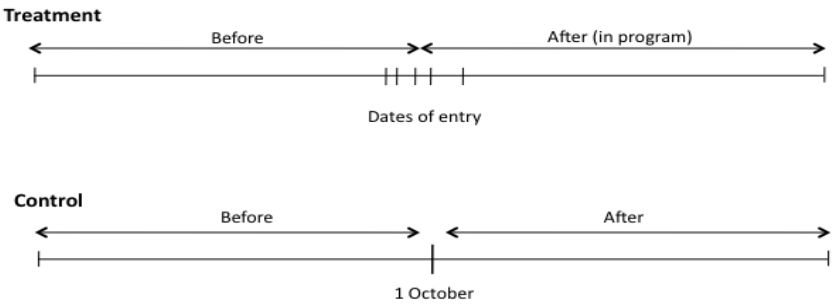


Figure 5.1—Difference in Euclidean distance between severe (*S, severa*) and high (*A, alta*) extreme poverty groups cluster threshold for all municipalities entering CSR in 2006 and 2007

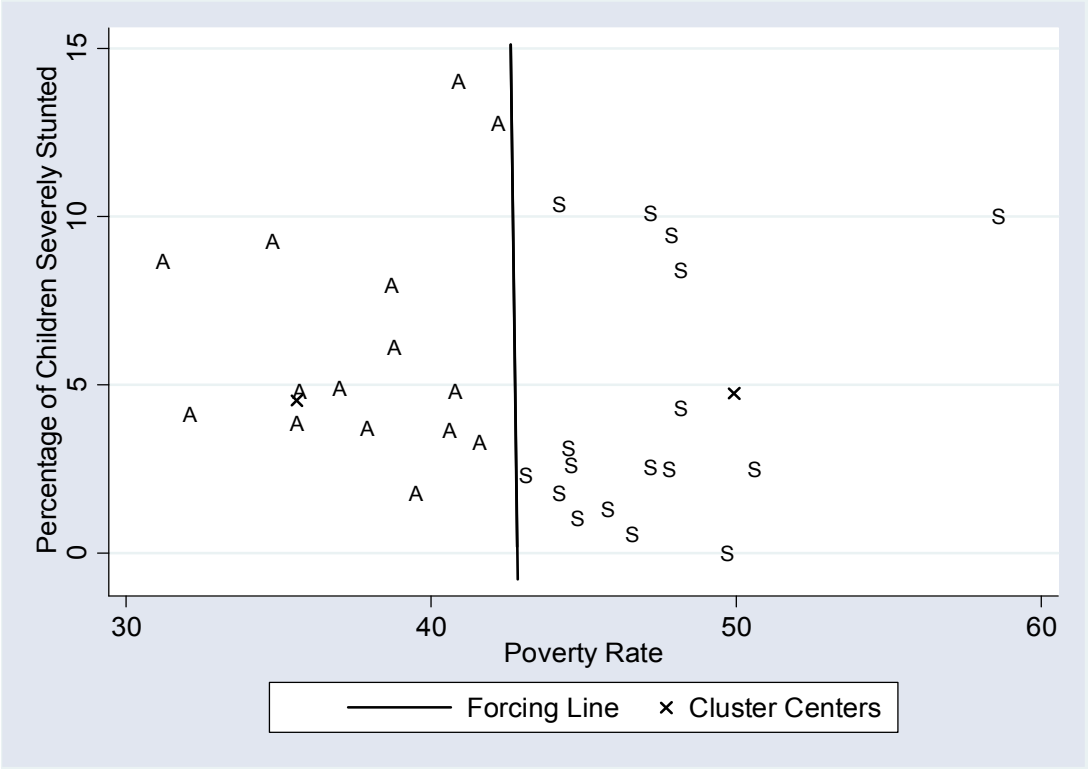


Figure 6.1—Change in women receiving adequate prenatal monitoring while pregnant by distance from implied cluster threshold, 2006 and 2007 entry groups

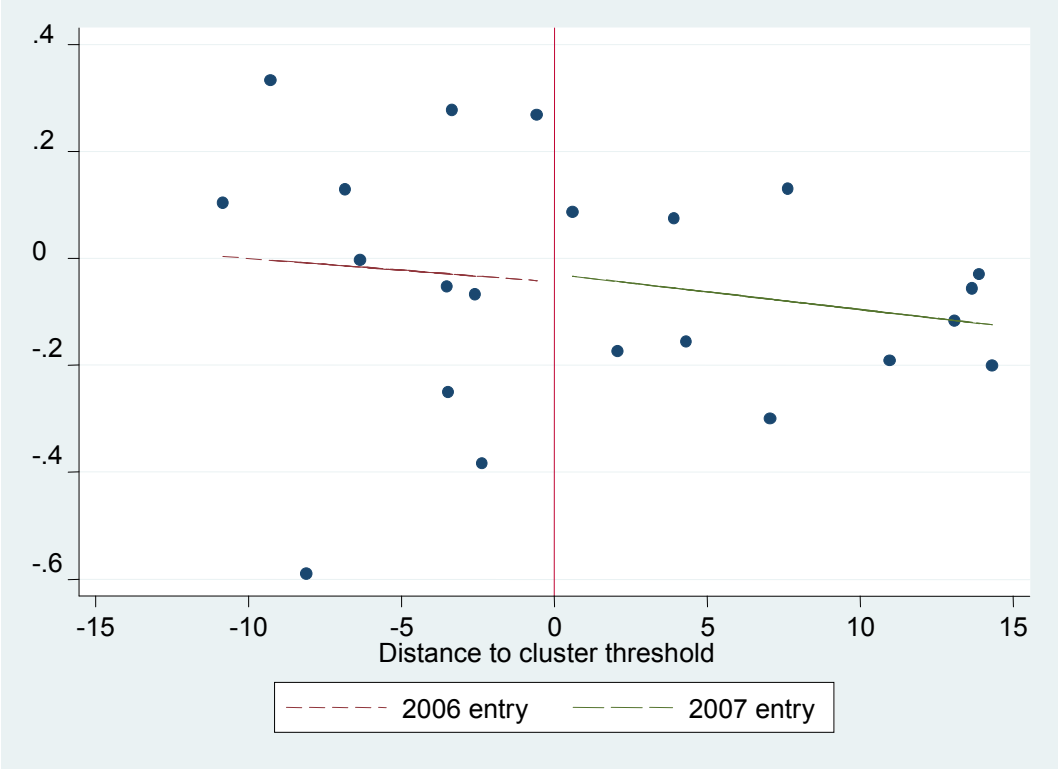


Figure 6.2—Change in the proportion of births with skilled attendants by distance from implied cluster threshold, 2006 and 2007 entry groups

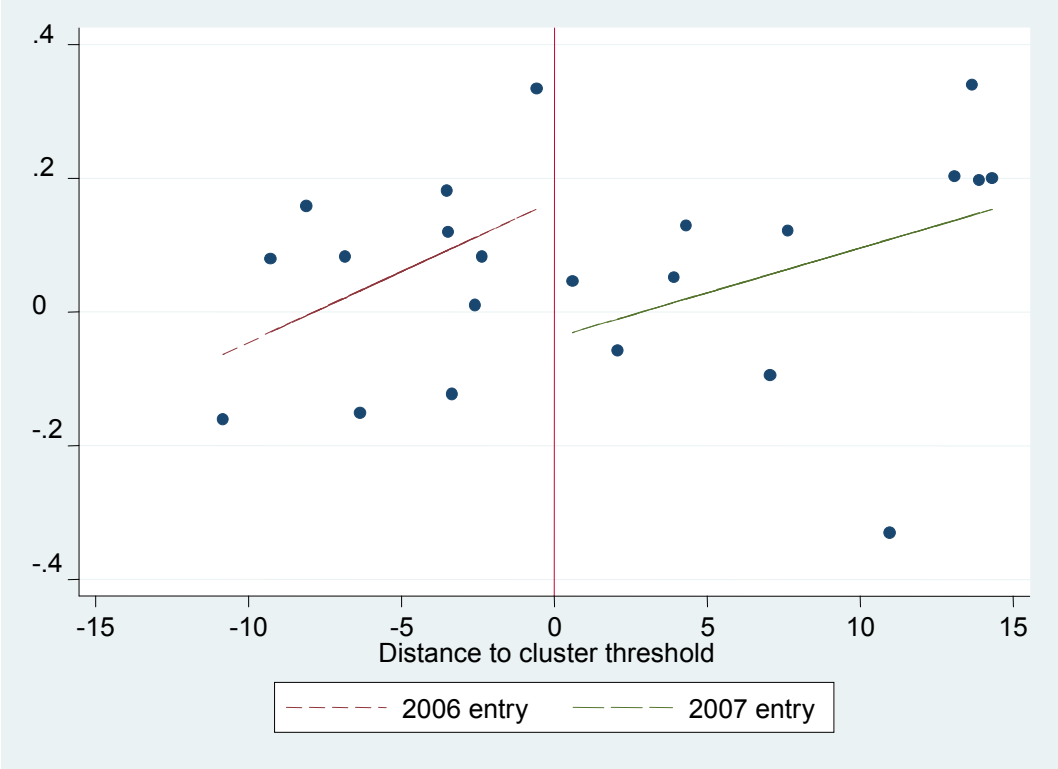


Figure 6.3—Change in the proportion of births in hospitals by distance from implied cluster threshold, 2006 and 2007 entry groups

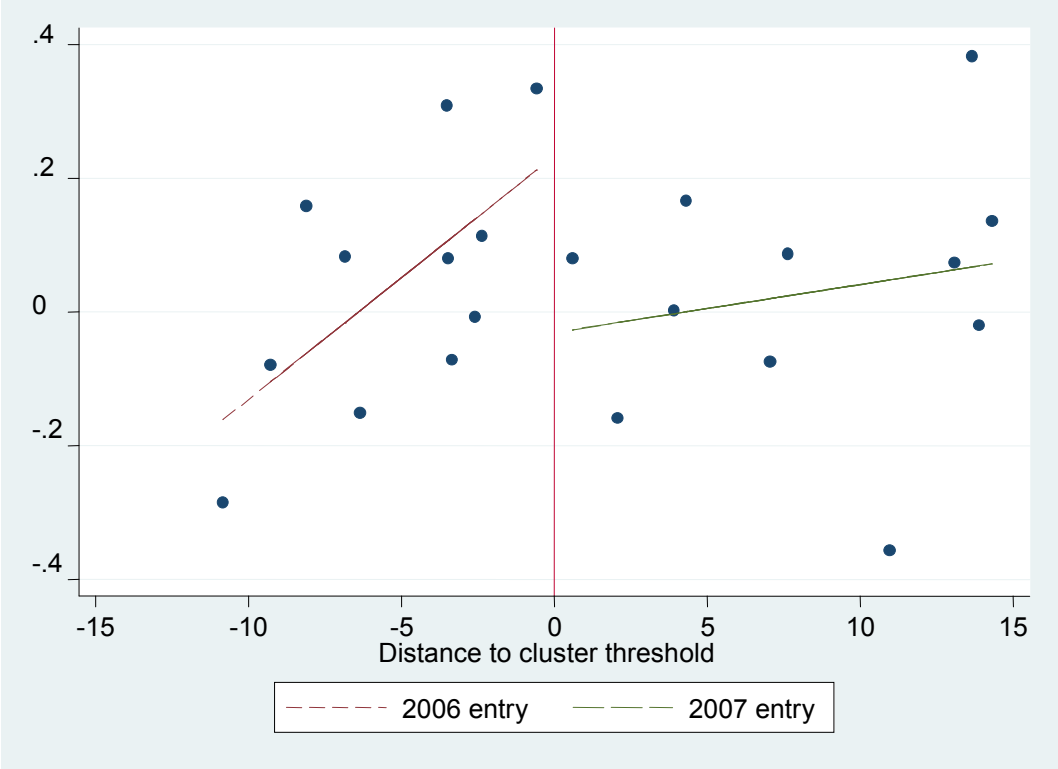
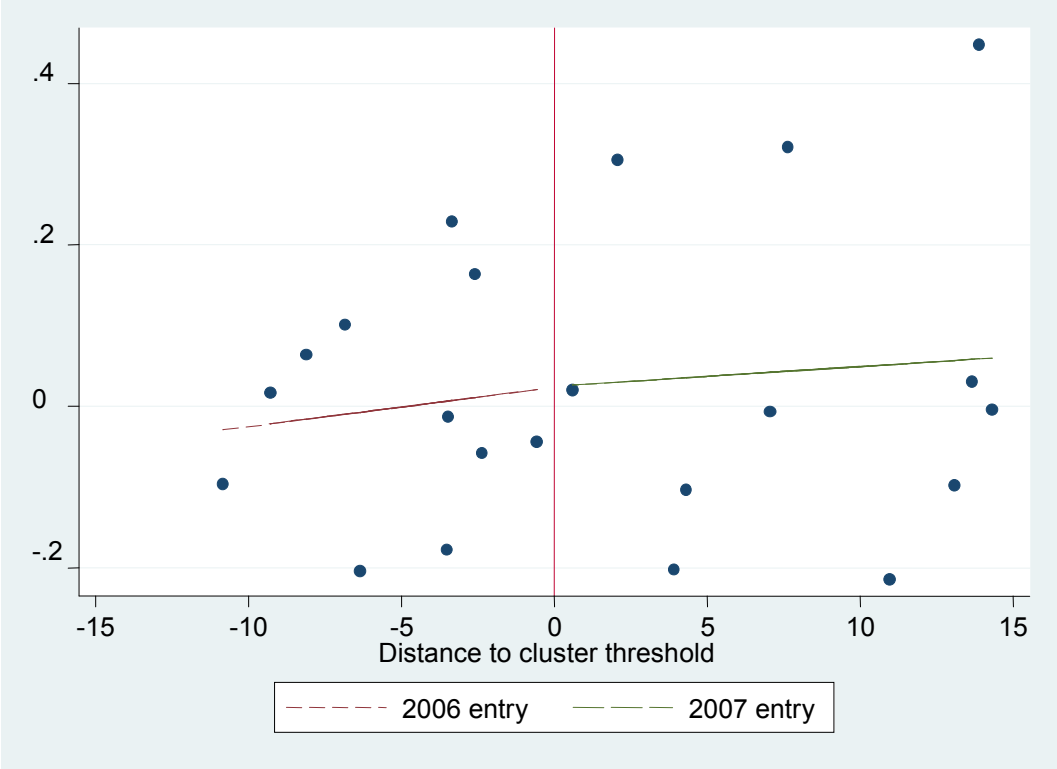


Figure 6.4— Change in the proportion of mothers receiving postnatal care by distance from implied cluster threshold, 2006 and 2007 entry groups



Appendix : Supplementary Tables

Table A.1. Full regressions for RDD results of the impact of *Comunidades Solidarias Rurales*on the proportion of births with adequate prenatal care, comparing 2006 entry to 2007 entry

	Rectangular kernel			Local linear		
	(A)	(B)	(C)	(A)	(B)	(C)
Variable	No controls	Ind controls	Full controls	No controls	Ind controls	Full controls
Time period	0.057	0.048	0.053	0.060	0.050	0.056
	(0.053)	(0.053)	(0.051)	(0.053)	(0.053)	(0.050)
Treatment group	-0.001	-0.011	-0.016	-0.055	-0.052	-0.063
	(0.038)	(0.038)	(0.041)	(0.061)	(0.060)	(0.064)
CSR (time period*treatment group)	-0.070	-0.058	-0.066	-0.072	-0.061	-0.068
	(0.072)	(0.072)	(0.072)	(0.073)	(0.072)	(0.072)
Distance to cluster threshold				-0.004	-0.004	-0.004
				(0.006)	(0.005)	(0.005)
Treatment group*distance to cluster threshold				-0.001	0.002	0.001
				(0.008)	(0.008)	(0.007)
Mother's age (in years)		-0.002	-0.002		-0.002	-0.002
		(0.004)	(0.004)		(0.004)	(0.004)
Third cycle (up to 9th year =1)		0.085	0.085		0.084	0.083
		(0.054)	(0.054)		(0.054)	(0.054)
Diploma (up to 12th year =1)		0.083*	0.084*		0.083*	0.082*
		(0.043)	(0.042)		(0.044)	(0.043)
Never married (=1)		-0.087	-0.088		-0.084	-0.085
		(0.070)	(0.069)		(0.070)	(0.069)
Separated/divorced/widowed (=1)		0.025	0.020		0.024	0.019
		(0.080)	(0.080)		(0.081)	(0.081)
Services index (1-3)[1]			0.035			0.037
			(0.027)			(0.027)
Asset index (continuous)[2]			-0.012			-0.012
			(0.026)			(0.026)
Log distance to health center (in km)			0.021			0.020
			(0.024)			(0.024)
Constant	0.769***	0.807***	0.689***	0.799***	0.839***	0.724***
	(0.025)	(0.121)	(0.153)	(0.040)	(0.127)	(0.158)
Sample size	494	494	494	494	494	494
R-square	0.003	0.017	0.023	0.004	0.018	0.025

Note: Estimates use the baseline and second survey and use the full sample. Standard errors clustered by municipality in parentheses below coefficients. * indicates significance at the 10% level, ** significance at the 5% level, and *** significance at the 1% level.

[1] Sum of indicators for (1) piped water, (2) electricity, and (3) toilet in house.
[2] Factor score for household assets, including radio, TV, stereo, VCR, fan, computer, typewriter, sewing machine, vehicle, boat, bicycle, cart/oxcart, motorcycle, refrigerator, stove, mill, blender, generator, solar panel, bed, other furniture.

Table A.2. Full regressions for RDD results of the impact of *Comunidades Solidarias Rurales*on the proportion of births with skilled attendance, comparing 2006 entry to 2007 entry

	Rectangular kernel			Local linear		
	(A)	(B)	(C)	(A)	(B)	(C)
Variable	No controls	Ind controls	Full controls	No controls	Ind controls	Full controls
Time period	0.027	0.038	0.033	0.027	0.040	0.037
	(0.062)	(0.059)	(0.063)	(0.060)	(0.058)	(0.060)
Treatment group	0.105	0.110	0.095	-0.019	-0.022	-0.069
	(0.075)	(0.071)	(0.067)	(0.145)	(0.139)	(0.137)
CSR (time period*treatment group)	0.139*	0.134*	0.123*	0.150*	0.142*	0.130*
	(0.074)	(0.070)	(0.069)	(0.075)	(0.072)	(0.070)
Distance to cluster threshold				0.000	0.000	-0.003
				(0.009)	(0.009)	(0.009)
Treatment group*distance to cluster threshold				-0.025	-0.026	-0.026
				(0.018)	(0.016)	(0.015)
Mother's age (in years)		0.003	0.003		0.003	0.003
		(0.002)	(0.002)		(0.002)	(0.002)
Third cycle (up to 9th year =1)		0.140***	0.116***		0.137***	0.111***
		(0.039)	(0.036)		(0.041)	(0.038)
Diploma (up to 12th year =1)		0.185**	0.124		0.180**	0.115
		(0.076)	(0.076)		(0.077)	(0.077)
Never married (=1)		0.076	0.071		0.090	0.087
		(0.068)	(0.066)		(0.071)	(0.070)
Separated/divorced/widowed (=1)		0.063	0.073		0.082	0.094
		(0.054)	(0.055)		(0.060)	(0.061)
Services index (1-3)[1]			0.039			0.0436*
			(0.027)			(0.025)
Asset index (continuous)[2]			0.026			0.025
			(0.029)			(0.029)
Log distance to health center (in km)			-0.041			-0.0444*
			(0.025)			(0.025)
Constant	0.633***	0.497***	0.617***	0.630***	0.491***	0.637***
	(0.044)	(0.086)	(0.116)	(0.107)	(0.121)	(0.165)
Sample size	536	536	536	536	536	536
R-square	0.035	0.059	0.082	0.049	0.074	0.100

Note: Estimates use the baseline and second survey and use the full sample. Standard errors clustered by municipality in parentheses below coefficients. * indicates significance at the 10% level, ** significance at the 5% level, and *** significance at the 1% level.

[1] Sum of indicators for (1) piped water, (2) electricity, and (3) toilet in house.

[2] Factor score for household assets, including radio, TV, stereo, VCR, fan, computer, typewriter, sewing machine, vehicle, boat, bicycle, cart/oxcart, motorcycle, refrigerator, stove, mill, blender, generator, solar panel, bed, other furniture.

Table A.3. Full regressions for RDD results of the impact of *Comunidades Solidarias Rurales*on the proportion of births in hospital, comparing 2006 entry to 2007 entry

	Rectangular kernel			Local linear		
	(A)	(B)	(C)	(A)	(B)	(C)
Variable	No controls	Ind controls	Full controls	No controls	Ind controls	Full controls
Time period	0.010	0.015	0.010	0.011	0.018	0.013
	(0.070)	(0.066)	(0.069)	(0.068)	(0.064)	(0.067)
Treatment group	0.110	0.112	0.095	-0.039	-0.042	-0.093
	(0.073)	(0.070)	(0.068)	(0.147)	(0.141)	(0.143)
CSR (time period*treatment group)	0.160*	0.163**	0.153*	0.171*	0.171**	0.159**
	(0.081)	(0.076)	(0.076)	(0.083)	(0.078)	(0.077)
Distance to cluster threshold				-0.002	-0.002	-0.005
				(0.010)	(0.009)	(0.009)
Treatment group*distance to cluster threshold				-0.024	-0.024	-0.024
				(0.018)	(0.017)	(0.016)
Mother's age (in years)		0.000	0.000		0.000	0.000
		(0.002)	(0.003)		(0.002)	(0.003)
Third cycle (up to 9th year =1)		0.135***	0.112***		0.132***	0.106**
		(0.041)	(0.037)		(0.042)	(0.039)
Diploma (up to 12th year =1)		0.167**	0.106		0.162**	0.096
		(0.073)	(0.071)		(0.073)	(0.073)
Never married (=1)		0.057	0.051		0.073	0.069
		(0.062)	(0.061)		(0.066)	(0.065)
Separated/divorced/widowed (=1)		0.033	0.045		0.053	0.067
		(0.052)	(0.053)		(0.060)	(0.062)
Services index (1-3)[1]			0.043			0.048*
			(0.026)			(0.024)
Asset index (continuous)[2]			0.027			0.028
			(0.031)			(0.031)
Log distance to health center (in km)			-0.036			-0.040
			(0.025)			(0.025)
Constant	0.623***	0.573***	0.669***	0.639***	0.587***	0.712***
	(0.040)	(0.086)	(0.108)	(0.111)	(0.128)	(0.166)
Sample size	530	530	530	530	530	530
R-square	0.038	0.059	0.081	0.054	0.075	0.101

Note: Estimates use the baseline and second survey and use the full sample. Standard errors clustered by municipality in parentheses below coefficeints. * indicates significance at the 10% level, ** significance at the 5% level, and *** significance at the 1% level.

[1] Sum of indicators for (1) piped water, (2) electricity, and (3) toilet in house.
[2] Factor score for household assets, including radio, TV, stereo, VCR, fan, computer, typewriter, sewing machine, vehicle, boat, bicycle, cart/oxcart, motorcycle, refrigerator, stove, mill, blender, generator, solar panel, bed, other furniture.

Table A.4. Full regressions for RDD results of the impact of *Comunidades Solidarias Rurales*on the proportion of births with postnatal care in first 2 weeks, comparing 2006 entry to 2007 entry

	Rectangular kernel			Local linear		
	(A)	(B)	(C)	(A)	(B)	(C)
Variable	No controls	Ind controls	Full controls	No controls	Ind controls	Full controls
Time period	0.032	0.028	0.027	0.034	0.028	0.026
	(0.062)	(0.061)	(0.061)	(0.062)	(0.061)	(0.061)
Treatment group	0.067	0.069	0.064	0.076	0.083	0.075
	(0.072)	(0.071)	(0.072)	(0.110)	(0.109)	(0.112)
CSR (time period*treatment group)	-0.059	-0.058	-0.059	-0.066	-0.063	-0.064
	(0.099)	(0.098)	(0.100)	(0.099)	(0.099)	(0.101)
Distance to cluster threshold				-0.008	-0.007	-0.008
				(0.006)	(0.006)	(0.006)
Treatment group*distance to cluster threshold				0.021	0.022	0.022
				(0.014)	(0.014)	(0.014)
Mother's age (in years)		-0.001	-0.001		-0.001	-0.001
		(0.002)	(0.002)		(0.003)	(0.003)
Tercer (=1)		-0.029	-0.033		-0.030	-0.035
		(0.070)	(0.071)		(0.070)	(0.072)
Bachillerato (=1)		0.110*	0.102*		0.110*	0.100
		(0.060)	(0.060)		(0.061)	(0.062)
Never married (=1)		-0.048	-0.050		-0.058	-0.059
		(0.046)	(0.046)		(0.042)	(0.041)
Separated/divorced/widowed (=1)		0.027	0.027		0.018	0.019
		(0.071)	(0.071)		(0.069)	(0.069)
Services index (1-3)[1]			0.008			0.006
			(0.018)			(0.018)
Asset index (continuous)[2]			0.007			0.009
			(0.020)			(0.021)
Log distance to health center (in km)			0.002			0.000
			(0.017)			(0.016)
Constant	0.192***	0.210**	0.196	0.253***	0.277**	0.277*
	(0.042)	(0.087)	(0.121)	(0.081)	(0.113)	(0.152)
Sample size	478	478	478	478	478	478
R-square	0.005	0.013	0.014	0.014	0.023	0.024

Note: Estimates use the baseline and second survey and use the full sample. Standard errors clustered by municipality in parentheses below coefficeints. * indicates significance at the 10% level, ** significance at the 5% level, and *** significance at the 1% level.

[1] Sum of indicators for (1) piped water, (2) electricity, and (3) toilet in house.

[2] Factor score for household assets, including radio, TV, stereo, VCR, fan, computer, typewriter, sewing machine, vehicle, boat, bicycle, cart/oxcart, motorcycle, refrigerator, stove, mill, blender, generator, solar panel, bed, other furniture.