Impact Evaluation Design for MCC Water and Sanitation Interventions in El Salvador

Elaborated by:
Kristin Komives
Christine Poulos
Subhrendu Pattanayak

Submitted to the
Millennium Challenge Corporation
MCC-07-0124-CON-91

by Social Impact, Inc.
August 12, 2009

Table of Contents

Table of Contents 2
1. Overview of the W&S intervention and objectives of the evaluation 3
2. Project selection and challenges for evaluation 4
3. General approach to the evaluation 5
4. Research questions, indicators, and data collection instruments 7
5. Defining "treatment", "treated households", and "communities" 12
6. Sample design and sample size 15
7. Implications for measuring health impacts 18
8. Matching "communities" and sample selection 20
9. Data collection activities 24
10. References 26
Annex 1: List of eligible municipalities 28
Annex 2: Selection criteria 30
Annex 3: List and description of projects 32
Annex 4: Primary indicators 36
Annex 5: Water quality testing 37
Annex 6: Estimated Sample Size Required to Measure Diarrhea Reductions 44
Annex 7: Matching equation and results 49

1. Overview of the W&S intervention and objectives of the evaluation

In November of 2006, the Millennium Challenge Corporation (MCC) signed a five-year, $461 million Compact with the Government of El Salvador (GOES) to improve the lives of Salvadorans through strategic investments in education, public services, agricultural production, rural business development, and transportation infrastructure. The Government of El Salvador has set up a management unit called FOMILENIO to implement the five-year Compact from September 2007 to September 2012. Social Impact has been contracted by MCC to conduct an impact evaluation of the water and sanitation activities of the Compact.

The goal of the water and sanitation component of the Compact (originally budgeted at $24 million) is to enhance access to water systems for approximately 90,000 of the poorest inhabitants in the Northern Zone of El Salvador and to improve sanitation services for approximately 50,000 people. The component anticipates providing piped water or (in a few cases) public taps for households that previously did not have access to this level of service and latrines to all water project participants[1] who do not already have improved sanitation.

MCC and FOMILENIO expect the water and sanitation interventions to:

- increase household income by at least 10%,
- reduce morbidity from water-related illnesses[2], and
- reduce the time and cost spent on seeking or purchasing water[3].

Reductions in water-related disease and time spent collecting water are in turn expected to lead to reduced expenditures on health care and increased attendance at school and work. In the economic analysis prepared for the water and sanitation component of the Compact, three-quarters of the expected benefits are attributed to reductions in "coping costs", specifically the time costs associated with collecting water and the monetary cost of relying on alternative water sources (such as vendors) and storage systems.

This document lays out the plans for the impact evaluation of the water and sanitation program. The benefits of the water and sanitation projects will be measured with a rigorous non-experimental design that incorporates matching, a panel survey, difference in difference estimation, and econometric analysis. The sample size for the panel survey is powered to measure changes in one primary indicator of household welfare (household expenditure). The evaluation will also examine changes, albeit not necessarily with the same degree of precision, in coping costs, productive time use, diarrheal disease, school attendance, and access to and use of water and sanitation infrastructure. To the extent possible, we will examine the distribution of benefits and outcomes across gender and socio-economic groups. Data collection for the panel survey will be done by DIGESTYC,

---

[1] In this report, 'participant' is used to indicate any individual in a treated household or treated community.

2 Ex-ante project modeling projected that the number of times that a person is sick per year would decrease by 1.5.

3 Ex-ante project modeling projected that the number of hours per week that a household spends collecting water would decrease from 30 to 14 and the cost of water per cubic meter would decrease from $3.00 to $0.43.

the Dirección General de Estadísticas y Censos (General Directorate for Statistics and Census).

2. Project selection and challenges for evaluation

Sixty-two municipalities in the Northern Zone, classified as either "Extrema Pobreza Moderada" or "Extrema Pobreza Alta" by the national poverty map, were invited to submit proposals for water and sanitation projects (see Annex 1). To be considered eligible for the program, the proposals had to meet four criteria (Annex 2): (1) the municipality had to be eligible to participate, (2) both the community and municipality had to be willing to make a financial commitment to the project, (3) the community had to be organized and willing to work with the municipality, and (4) the estimated cost of the project could not exceed $850 per beneficiary. After projects that did not meet the eligibility critieria were excluded, a list of 68 projects remained and were declared eligible to enter the feasibility stage (see Annex 3)[4]. One of these projects was already in an advanced stage of elaboration (*carpeta*). As it will not follow the same project development process as the other projects, it has been excluded from the evaluation.

In the original economic analysis for the water and sanitation component of the Compact, it was assumed that all water projects would involve providing water to households that did not previously have improved services. The project application and selection process, however, generated a more diverse set of projects. Just under half of the projects propose to install water in communities that did not previously have improved water service. The rest will either extend an existing water system to additional households, improve an existing system, or both improve and extend an existing deficient system. The exact objective and scope of each project will only be clear after the feasibility stage, when final designs are ready. It is also unclear at this stage which projects will include latrine construction: most of the project descriptions did not mention latrine construction or current sanitation services.

The Compact W&S program poses some particular challenges for evaluating the impact of the investments.

- First, the program includes different types of projects, which will benefit households in different ways. For example, it is not obvious that projects that provide a new service option to a household without piped water service should be expected to have the same impact as projects that help households with connections get more out of a service they already have. For future policy discussions in El Salvador and within MCC, it could be very useful to understand the differential impacts of different types of projects. These more detailed research questions, however, would come at the cost of increasing the household survey sample size needed for the evaluation.

[4] There were also criteria established for the feasibility stage (see Annex 2). Projects that do not meet these criteria would not normally be considered eligible to proceed to the construction phase.

- Second, not all of the expected benefits of the water and sanitation program are relevant for all households. Whereas increases in income or reduction in water collection costs might be seen in all households, changes in school attendance rate relate primarily to households with school age children, and changes in morbidity and health expenditures from water and sanitation-related illnesses are expected to arise disproportionally in households with young children. This again has implications for sample design and sample size.
- Third, the benefits of water and sanitation investments have a seasonal character – households experience the benefits of service improvements in different ways at different times of year. Impacts on disease are most visible during the rainy season when diarrhea rates are highest. Impacts on coping costs (cash and time expenditure on water provisioning) are highest in dry season, when deficient pipe systems and shallow wells or streams are likely to experience water shortage problems.

In the original version of this design proposal, we discussed these challenges and their implications for the research design and the sample size. In subsequent discussions with MCC and FOMILENIO M&E staff, it became clear that keeping down the sample size and cost of the evaluation was a major concern and that we should give priority to the measurement of changes in household welfare and coping costs, over health impacts, in the final design. There are two reasons for this. First, coping costs accounted for the bulk of the expected benefits of water supply interventions in the economic analysis underlying the program. Second, diarrhea rates in El Salvador have dropped substantially in recent years due to a variety of public health interventions. This makes measuring changes in rates more difficult and more costly, and also is likely to mean lower than expected benefits from health improvements in these projects.

Because we will focus on changes in welfare and coping costs, benefits will be measured in the dry season only. Also, to keep down the sample size, we have decided to make the principle objective of the research the evaluation of the combined impact of the full set of water and sanitation projects. After project designs are finalized, we will assess to what extent it will be possible to compare impacts across project types with the existing sample (albeit with less precision than for the water and sanitations projects as a whole).

As implementation of the first round of feasibility studies has progressed, we have also become aware of a new set of challenges and threats to the evaluation. It now appears that the cost of water projects will be higher than anticipated and the number of participants lower. It is not yet clear whether this will affect the willingness of mayors and communities to make the required capital contribution or the ability of FOMILENIO to finance all of the feasible projects. It may also mean that project designs are changed to reduce costs. If some projects are dropped or significantly delayed, for this reason or another, it could affect the power of the evaluation.

3. General approach to the evaluation

The key to measuring the impacts caused by the water and sanitation interventions is to compare conditions *with* the interventions to conditions that would have prevailed *without* them. The counterfactual state is not naturally observable – we can never know what change would have occurred in program participants (the treatment group) if the program was not implemented. As it was not possible to apply randomization in the selection of water and sanitation projects in this case[5], the benefits of the water and sanitation projects will be measured with a rigorous non-experimental design that incorporates matching, pre- and post-implementation data collection, difference in difference estimation, and econometric analysis to estimate the counterfactual and address selection and other biases.

- Matching represents a credible non-experimental option for identifying comparison groups, and it has been used in several recent 'good practice' evaluations identified by Judy Baker (2000) as well as evaluations of various Social Investment Funds with water and sanitation components. We will use propensity score matching (PSM) (Rosenbaum and Rubin 1983; Heckman et al., 1997, 1998a, 1998b; Dehejia and Wahba, 1999, 2002) with existing secondary data to match communities before program implementation. PSM identifies control communities that have an equal probability of receiving the treatment. Matching ensures that the control or comparison areas are similar or identical to the treatment communities in terms of observable characteristics, thus serve as a comparison and provide measures of indicators in communities that are similar *but for* the treatment. With a good understanding of the selection process and with adequate secondary data, matching can address selection on observables.

- By collecting data before and after program implementation, we can apply a 'difference in difference' (DID) estimator (Heckman et al 1998). This estimator measures the treatment effect as the difference between the changes in indicators before and after the program among treatment recipients, on the one hand, and the changes in indicators before and after the program among control units, on the other. DID estimation helps control for residual confounding due to imperfect matches and selection bias from unobservable factors which differ between treatment and control communities and which may have an influence on the impact variables of interest.

To implement the DID estimator, we have planned for a panel survey in which the same households are interviewed in 2009 (year two baseline study) and 2011 and 2012 (follow-up surveys). The choice to implement a baseline and two follow-up surveys has a number of advantages over implementing a baseline and a single endline survey. The longer one waits to conduct the endline survey the greater the risk that 1) the measured impacts will be due at least in part to changes that are unrelated to the program under study (referred to as history and maturation effects) and 2) that households will forget key details about project implementation. There is also the risk that some control communities will receive W&S services from a different funding agency or program. On

[5] Once projects that did not meet the established criteria were deemed ineligible, there were not enough projects left to apply randomized selection.

the other hand, waiting longer to implement the endline study makes it possible to see impacts that might not materialize immediately (for example, the creation of water-based businesses) and to assess near-term indicators of long-term sustainability of the interventions. With a follow-up survey conducted both two and three years after the baseline (roughly 1.5 and 2.5 years after the implementation of the first projects), we both minimize risk and gain a view of longer-term impacts[6].

We will employ DID estimation in combination with multivariate regression analyses (Ravallion 2005) in order to control for individual and household level covariates, with adjustments for intra-cluster correlation due to design effects. During the survey process, we will collect information so that covariates can be controlled for in the econometric analysis.

While the evaluation will be based on the rigorous analysis of this panel survey data, we also plan to introduce limited qualitative analysis, to the extent feasible with the existing budget. In year three of the study, the Social Impact team itself will conduct a very limited set of structured interviews of national and local actors involved in the W&S program. The purpose of these interviews will be to gain insight into program implementation, beneficiary commitment and political support, which could ultimately affect the benefits of the project. If possible, we would also like to coordinate with those ultimately chosen to do a qualitative study of water and sanitation participants, so that the insight from this study can be used to help understand the results of the evaluation.

4. Research questions, indicators, and data collection instruments

Very few studies have measured changes in the household's costs of collecting, storing and treating water, or the income losses due to water-borne and water-washed illnesses (Pattanayak et al., 2008a). Furthermore, we are unaware of other rigorous impact evaluations that have measured education, gender, and poverty reduction impacts of W&S interventions. Bosch et al. (2000) did categorize W&S impacts on program participants into four groups: (1) health improvement; (2) education; (3) gender and social inclusion; and (4) income/consumption increases. The health impacts of W&S programs have also been studied frequently (see Fewtrell et al., 2005; Pattanayak et al., 2008a, 2008b; Rawlings and Schady 2002; and Galiani et al., 2005). However, the literature on the impacts of water and sanitation programs demonstrates a lack of scientific rigor in most evaluations, particularly with respect to (a) accounting for baseline levels and pre-intervention behaviors, (b) the inclusion of control groups, (c) explicit examination and control for confounders, and (d) detailed reporting and presentation of results (Poulos et al. 2006). In this study, we will examine the effect of the Compact's W&S program on the costs of collecting, storing and treating water, as well as the income losses due to water-borne and water-washed illnesses, which will constitute a significant contribution to the literature.

[6] In practice, it now appears that there will be as much as a one year delay between the implementation of the first and the last projects. Thus, we will consider time an important variable in our econometric analysis.

Below we describe the major research questions and indicators that we will use in this study. A preliminary list of indicators is also included in table form in Annex 3. In reading this section, it is important to recognize that the sample size calculations have been done based on one primary indicator of household welfare (household expenditure); thus, there will be more uncertainty about whether we will have the power to detect changes in other, complementary indicators.[7]

Household welfare indicators:

- Do W&S investments increase household expenditure or income?
- What are the consequences of W&S investments for expenditure patterns?

MCC and FOMILENIO expect the W&S program to increase incomes by 10% among project participants. In this study, household expenditure will be used as a proxy for income and the major indicator of household welfare. We will also measure income, but in the interest of keeping the survey of reasonable length, we will focus on aspects of income that we believe would be most directly related to W&S improvements and the time savings that such improvements could generate.

Because there is little information in the existing W&S literature about the impact of interventions on income and because of the inherent difficulties in accurately measuring income and expenditure, this study will also attempt to study the mechanisms through which one might expect W&S investments to generate income improvements. W&S investments could generate an increase in income through several channels:

- Working individuals could dedicate more productive hours to income-earning activities because, for example, they or their dependents are not ill as often or because they no longer have to spend as much time collecting water.
- Individuals who did not previously participate in income earning activities could enter the work force because they have more time available or because W&S investments present new opportunities for them (e.g. the possibility of starting a water-related business in the home; new businesses open in the village).
- Current productive activities in the households (e.g. agriculture or small business activities) could become more productive and/or profitable with a more reliable and less expensive water source.

To explore these channels, we will gather information about time use in productive activities and water collection, productive time lost due to illness or care-giving, and productive use of water at the household level.

In addition to (or in the place of) increasing income and expenditures, W&S investments could generate changes in patterns of expenditure. If households spend less on medical

[7] In an earlier version of this design, we planned to ensure sufficient power to also measure changes in health outcomes. After consultation with MCC and FOMILENIO, this idea was dropped due to budget constraints..

care due to reduced illness, or spend less on the purchase, storage, and treatment of water, then resources are liberated for other uses. If these resources are redirected to activities that contribute to socio-economic development (school fees, transportation, preventative health care, nutritious foods), then they could magnify the development effects of water interventions. To explore whether W&S interventions produce changes in patterns of expenditure, we will examine changes not just in total household expenditure, but also in major categories of expenditure.

Indicators of coping costs and cash expenditure on water:

- Do W&S interventions reduce coping costs?
- Do they reduce cash expenditures on water and on sanitation services?

Coping costs are the expenditures that households make in order to collect, store, and treat water (Pattanayak et al. 2005; Strand and Walker 2005). Closer, more reliable, and better quality water is generally expected to reduce these costs. We will measure expenditures on building and maintaining alternative water sources, delivery systems, and storage containers. We will also measure time spent collecting water and washing clothes at a source outside of the home as well as cash expenditures on water services, water vendors, and bottled water. Additionally, we will examine cash expenditures on sanitation services (e.g. use of public latrines, expenditures on latrine emptying and maintenance).

We expect the quantity of water used by households to increase with reliable piped service (e.g., see Strand and Walker 2005). Even if total expenditures on water do not change, the time or cash expenditure on water per unit of water used will decrease. Measuring the quantity of water used by households is very difficult. We will use questions about the size and number of containers of water collected, the storage units inside the home, and the time the tap is running to try to estimate changes in the quantity of water used by households before and after the interventions.

Health indicators

- Do W&S interventions reduce incidence of diarrheal illness?
- What factors (hygiene behavior, source and household-level water quality, household source choice) might explain the impact (or lack of impact) in this area?

While W&S improvements are related to reductions in a variety of water-borne, water-washed and water-related diseases, most rigorous impact evaluations in the sector have focused on diarrheal disease and acute respiratory illness. This study will focus primarily on diarrheal illness, but also collect information about cases of respiratory illness, stomach ailments, conjunctivitis, dengue, and fevers.

The primary health indicator will be a period prevalence of diarrhea, particularly in children under 60 months of age. Based on the predominant practice in the literature, the judgment of health experts and epidemiologists, and our team's previous experience measuring the health impacts of W&S interventions (Pattanayak et al. 2008a, 2008b), we

will define diarrhea as three or more episodes of liquid bowel movements within a period of 24 hours (with or without blood and/or mucus) during the two weeks prior to the survey.

This study will rely on self-reported morbidity to measure health status. While self-reported disease measures are vulnerable to measurement error due to the fact that respondents may fail to recall episodes or misdiagnose disease, these are the predominate health indicators in W&S impact evaluations and they are widely used by epidemiologists and health experts. The accuracy of the two-week recall period is well established (Kleinhau et al. 2003). It is important, however, to ensure comparability of measures taken in different years by conducting the panel household surveys in the same season each year. In this case, the surveys are planned for the April to May period each year. It is also important to control for other events that increase diarrhea, including weather events, natural disasters, and economic crises (Kleinhau et al. 2003). In El Salvador, the start date of the rains is an important indicator of when diarrheal rates will begin to increase. We will, therefore, collect information on the start dates of the rains in each survey year and ensure that matched control and treatment villages are interviewed during the same week, and that all villages are re-interviewed as close as possible to same week of the year in follow-up surveys.

The health effects that are associated with W&S interventions are produced through a variety of changes in household behavior and in living environments. To examine this, we will explore four issues that could contribute to the presence or absence of health improvements:

- *Source choice and use of multiple sources* For all households in the study, we will monitor self-reported use of community water sources to determine whether the introduction of an improved piped system leads to abandonment of other, unimproved sources.
- *Hygiene knowledge and behavior, water treatment, and use of sanitation facilities* For all households in the study, we will monitor understanding of good hygiene behavior. We will also examine whether sanitation facilities are being used and handwashing practiced. To study water treatment behavior, we will monitor self-reported water treatment practices (boiling, filtering, and use of PURIAGUA [a chlorine additive]) as well as test drinking water samples for residual chlorine levels in all interviewed households.
- *Household and source water quality* To assess the extent of bacterial contamination of water in households and at sources, the study will include laboratory tests of drinking water and source water in a sample of households and villages (see Annex 4).

Education indicators

- Do W&S interventions increase school enrollment among children aged 7 to 12?
- Do W&S interventions increase school attendance among children aged 7 to 12?

In the literature on W&S it is commonly suggested that in situations where children - particularly girl children - are responsible for water collection, a reduction in the time spent

collecting water could yield higher school enrollment and attendance. However, to our knowledge, no rigorous studies have yet examined whether these expected benefits of system improvements materialize. To examine this question, the survey will ask about school enrollment and school attendance over the two weeks prior to the survey, and the reasons for non-attendance.

Indicators of service, use, and sustainability

While impacts – the fundamental change experienced by participants – are the focus of this study, we will also measure some intermediate indicators that provide information about whether a project has been successfully implemented, what new options the project has opened for households, and whether service levels are maintained over the period of this study.

We will measure changes in:

- Access (e.g., the average distance from participants' homes to a water source and to sanitation facility or location),
- Quality and reliability of services (e.g., the number of hours of service, frequency of breakdowns),
- Use (e.g. household use of water and sanitation services)

Gender and social exclusion

How the benefits of W&S interventions are distributed across categories of participants is a question that has not received serious study through rigorous impact analyses. In this study, we will collect information on the gender and age of household members, as well as on the relative socio-economic status of households. This information will be used to analyze, to the extent feasible with the sample available, differences in selected impacts and outcomes across gender and socio-economic groups. In the case of gender, we will pay particular attention to differential impacts in areas of time use and school attendance. For socio-economic groups, we will look at differences in access improvements, use of clean water, as well as changes in coping costs and health, education and major welfare indicators.

Data collection instruments

The primary source of data on these indicators will be the household panel survey (Encuesta de Linea Base de Agua y Saneamiento, or ELBAS by its initials in Spanish). This survey will be administered to heads of household or adults older than 16 years old. Enumerators will give priority to interviewing the person in the household with the best knowledge of household expenditure and water use and collection. If the person is not present at the time of the first visit, enumerators will attempt to make an appointment and return again to interview the appropriate person, provided that this return visit is possible within the time that the survey team will be in the area. When possible, a second adult can also be included in the interview process, particularly for the questions related to work and agricultural output. The survey is designed to take between 1 and 1 ½ hours.

In addition to the household survey, there will also be a community survey for each "community" (see following section for definition) where selected households live. The community surveys will gather information about the local economy; price levels for food, basic commodities, and W&S-related expenditures; and about the history of public services in the area. In years four and five, these surveys will also include questions regarding the status of water sources and the W&S project implemented through the Compact. These surveys will be administered to one or more community-based organization or community leader. Price information will be collected in stores, from vendors, or in interviews with knowledgeable community members. The goal of the surveys is to provide some context for the information gathered in the household surveys, to track community-level changes that may affect outcomes, and to reduce the required length of the household survey questionnaire.

5. Defining "treatment", "treated households", and "communities"

Types of treatment

There are numerous different ways to define the "treatment" that is applied in this W&S program. The simplest approach is simply to say that the sum total of all activities undertaken in any project constitutes the treatment, thus ignoring the differences between the interventions in each community. If treatment is defined in this way, then the treatment effect that we measure with the evaluation will be an average effect across all types of interventions.

At the other extreme, we could consider defining four different treatments, as shown in Table 5.1 below. The implication of defining multiple treatments, however, is that it requires a research design that measures the program impact for each type of treatment, which in this case would mean close to doubling (in the case of two treatments) or quadrupling (in the case of four treatments) the sample size required for the survey. Aside from sample size considerations, there are some practical difficulties involved in precisely defining the treatments at this stage, as the final project designs are not completed, and FOMILENIO expects that some changes will be made to the designs during the feasibility assessments.

Table 5.1: Possible ways to define treatment

	New water projects	Projects that expand or improve existing water systems
No sanitation	A	B
Sanitation	C	D

Of the potential differences in treatment effects that we could analyze in this study, we consider that the most policy relevant and least studied in the existing literature is the comparison of effects between new piped water projects and the improvement or

expansion of existing projects. After generations of investment in W&S, many communities in developing countries now find themselves in need of what could be called a third stage of water supply improvements. They have passed the first stage of installing public point sources and a second stage of installing individual piped connections. But due to system failures or population growth, the households are not receiving the quality or reliability of service they demand, or some new households are excluded from connecting to the existing systems.

MCC and FOMILENIO have decided to include this third generation type project in the W&S program, but the rate of return calculations for the project are based on the expected benefits of new projects. For MCC and FOMILENIO, it is thus an interesting question whether the two types of water projects have similar impacts on income, school attendance, health, and coping costs. The comparison could also be interesting for the government of El Salvador as it plans for future water supply investments. We therefore see distinct advantages in building a test of two different treatments into this evaluation:

- Treatment A = "New" water projects (with or without sanitation)
- Treatment B = Projects that "improve, rehabilitate, or expand" existing systems (with or without sanitation)

To do independent tests of these two treatments, however, would require a larger sample size. Given concerns about costs of the evaluation and uncertainty about the final project designs, we have decided to focus on a design that will estimate the benefits of the full FOMILENIO W&S program as a single treatment (without differentiating between project type).[8] Nonetheless, within the limits of the final sample size, we will explore possibilities for evaluating differential impacts of the W&S interventions once project designs are finalized.

"Treated" households – or households that have received the treatment -- are defined as those households who had the potential to benefit from these water and sanitation projects. This means that they live within the service area of the new or improved water (and/or sanitation) system. Households that live within the service area, but do not connect to the service or see no change in their water or sanitation service after the project (e.g. water reliability, quality, hours of service from existing connection did not improve) are still considered treated households. This approach to defining treated households has important implications for the impact evaluation: projects that do not inspire households to connect or to agree to build latrines will have lower impacts than projects that achieve full coverage. One of the aspects of the project that is being evaluated is the "take up" and use of the services that is offered.

In defining treated households, we are also assuming that at least the short and medium-term benefits of community W&S improvements are only enjoyed by households living with the community. If a household moves out of the community, its members cease to benefit from the latrine or water connection they received. Households that have moved out of the intervention area between the baseline and follow-up surveys will therefore be dropped from the panel study.

[8] This choice was discussed with and accepted by FOMILENIO and MCC staff.

Treated "communities" are difficult to define in this case. The concept of community in rural El Salvador is associated with *caseríos*, which are residential areas that have a collective identity. Caseríos are not formal administrative units and do not exist in all areas. Each caserío is part of a "canton"[9], and a group of cantons forms a municipio. Most of the W&S projects are intended to serve a single caserío, but some projects will serve multiple caseríos or, in a few cases, one canton[10]. Thus, "project areas" are not equivalent to either caseríos, cantons, or municipios in this W&S program, and the water systems are not necessarily based in one "community".[11]

In addition to the conceptual problems involved in defining community, we face a practical constraint in defining treatment communities for sampling purposes: there is no information available about households at the caserío level or at the project area level. The lowest level at which census information is gathered is the census segment[12]. Each canton has one or more census segments[13]. In some cases, census segments are larger than project areas, and in some cases they are smaller. According to the information we were given before selecting the sample, census segments generally include about 100-125 households[14].

In this study, we ultimately chose to define treatment "community" at the level of the census segment. There are a number of advantages to defining community at the census segment level rather than at the project level or canton level:

- While "project area" would in many ways be the ideal choice of treatment community, we do not have the equivalent unit in control communities[15].
- A canton is usually much larger than a project area and will include many more non-beneficiary households than a census segment will.
- 2007 census data is available at the census segment level, so we had significant information available for matching treatment and control communities at this level.
- The General Directorate for Statistics and Census (DIGESTYC) is used to working with these census segments, and thus we are able to make use of many of their established procedures for sample selection and survey implementation.

---

[9] Some caseríos fall into more than one canton.

[10] In some cases the project descriptions simply mention that the service will be provided in a particular canton, but it is not clear if this means that the entire canton will be benefitted or some portion of the canton.

[11] This has implications for the management of these systems, as many systems will require collaboration of people from different "communities."

[12] This is the primary sampling unit for sample surveys in El Salvador. Its boundaries do not correspond to any political boundaries in most cases.

[13] In theory, each census segment is located in a single canton, but in practice there is debate about canton boundaries. We found therefore that a number of census segments span several cantons, at least according to the residents of the area.

[14] The rural census segments had about 120 houses, but the recent census showed that up to 20% of those houses can be unoccupied. For the purposes of the evaluation design, we assumed 100 households per census segment.

[15] We did make an effort to identify, through FISDL, W&S projects that have been formulated, but not submitted for FOMILENIO funding. This could have served as control "projects" for the study. However, there were not enough formulated but unsubmitted projects to have this be the basis for the matching.

6. Sample design and sample size

As described above, the impact evaluation will examine change in numerous indicators. Here we compute the sample size required to detect changes in the primary indicator of household welfare (measured by expenditure).

Indicators of incomes or expenditures are continuous. For continuous outcomes, the sample size required is given by (Purdon 2002):

$$n = \frac{d}{r}\left\{\frac{z_{\alpha/2} + z_{\beta}}{e}\right\}^2 \frac{\sigma^2}{a(1-a)} \quad [1]$$

where

- d (= 1+ ρ*(m − 1)) is the design effect due to intra-cluster correlation (ICC), where ρ is the ICC of subjects belonging to the same group, and m is the average number of subjects per cluster.
- a represents the proportion of the total sample that will be allocated to the intervention group;
- σ^2 is the baseline variance of the continuous indicator
- α is the significance level to be used in the statistical tests;
- 1 − β is the power of the study;
- r is the response rate; and
- e is the difference to be detected.

The d term in Equation [1] considers the effect of ICC on the sample size. ICC arises from design effects attributable to factors shared by observational units, thereby lowering the total amount of information available for statistical analysis and increasing the required sample size. By increasing the number of communities in the sample and reducing the number of households sampled per community (m), we can increase the amount of independent data. This will then lower the variance inflation factor, which is dependent on the ICC (ρ).

According to the 2007 household survey, average monthly household income for rural households is USD 284 (standard deviation=313) and the monthly household expenditures for rural households is USD 239 (standard deviation=170). An estimate of the ICC is obtained from analyzing the 2007 household survey data and observed at the department level. Table 6.2 shows alternative ICC estimates that range from 0.0147 to 0.0396. Given that the upper estimates in this range are expected to be a significant subset of our sample, we used ρ=0.03 in our initial calculations.

Table 6.1. Estimates of the Intra-cluster Correlation between Income and Expenditures

Description	ICC estimates (ρ)
Monthly household incomes, rural households in *departamentos* with the majority of projects	0.0187
Monthly household expenditures, rural households in departamentos with the majority of projects	0.0147
Monthly household incomes, rural households without access to private taps in departamentos with the majority of projects	0.0262
Monthly household expenditures, rural households without access to private taps in departamentos with the majority of projects	0.0396

Using these data and equation [1], the sample size necessary to detect a 10% increase in monthly household income would be about 5,900 and the sample size necessary to detect a 10% change in monthly household expenditures would be 2,473. The calculations assume an α equal to 90%, β equal to 80%, and 20 observations per cluster[16].

We assume an effect size of 10% since this is the expected effect size of the W&S component of the Compact. If we tested only for a 20% increase in expenditures, then the sample size would drop dramatically (see Figure 6.1). We would, however, be unlikely to be able to detect whether the W&S component produced the expected 10% increase in expenditures.

Based on these figures, we recommended focusing on household expenditures, as a proxy for household income in this study. In subsequent discussions with MCC, it was decided that we should assume the highest level of intra-cluster correlation that appears in the table above: ρ=0.04. We also assume a 10% attrition in each survey round (10% in the first follow up survey round in year 4, and an additional 10% in the last survey round) and add a contingency of 10% to each sample to account for possible changes in project design, problems with non-response or non-participants in treatment segments, or the loss of some clusters due to unforeseen circumstances.

Table 6.2 below shows how the sample size required to measure a 10% change in expenditures, assuming 0.04 ICC and with the contingencies described above, would change as the number of households per cluster changes. Consideration must be given to statistical, logistical, and financial matters in choosing how many clusters to include in the study. Statistically, it is usually advantageous to maximize the number of clusters as power is most directly affected by this number. At the same time, since we are going to compare community-level averages in treatment and control communities, a sufficient number of observations per cluster must be interviewed to ensure a representative sample is included for each cluster. Also, logistically and financially, it is usually more difficult and more expensive to recruit and survey more clusters than it is to interview more observations per cluster.

[16] The required sample size would be larger if there were more observations sampled per cluster and smaller if fewer observations were sampled per cluster.

Figure 6.1. Sample sizes required to detect different expenditure effect sizes (α=90%;β=80%;ρ=.003; and observations per cluster=20)

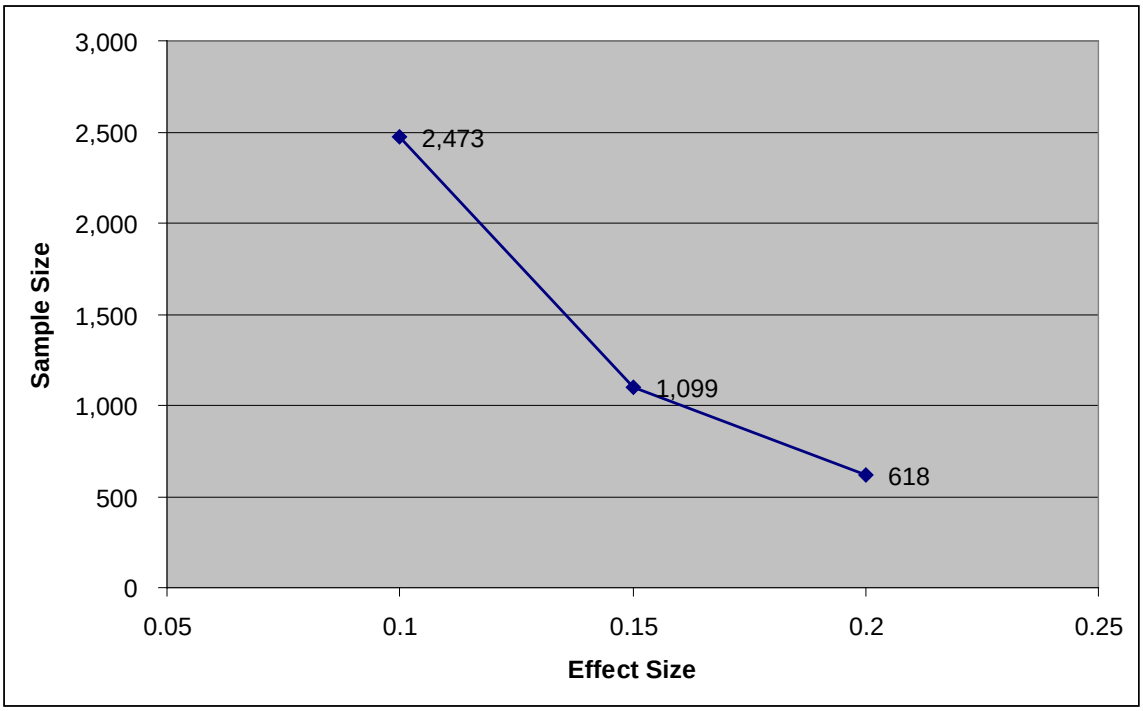


Table 6.2: Sample Size Required to Detect 10% Expenditure Effect in a General Population Sample (ICC=0.04)

Required sample size including 10% loss to follow up and 10% contingency[17]	Number of Communities	Obs. Per cluster (with 10% contingency and 10% loss to follow up)
2,745	189	15
2,973	164	18
3,202	147	22
3,431	135	25
3,660	126	29

Our original recommendation was to choose either 22 or 25 observations per cluster (see Memo of October 12, 2008), but because of concerns about the FOMILENIO Board rejecting a survey with a sample size this large, we ultimately adopted 18 observations per cluster and 164 communities. We also added an additional contingency -- 6 extra treatment segments and 6 extra control segments, for a total of 216 additional households – in order to provide extra cushion for the loss of projects during implementation or

[17] Note that the required sample size in the table does not exactly equal the number of communities or the observations per cluster because of how the 10% contingencies were calculated and added.

inaccuracies in the sample frame (see more below). The final sample size for the study is 3168, with 88 control and 88 treatment segments, each with 18 households.

7. Implications for measuring health impacts

The final sample is not designed to measure, on its own, the health impacts caused by the W&S interventions (See Annex 5 for a description of the sample size that would be required for this purpose). The primary objective of the research design would be to measure changes in expenditure and coping costs in a rigorous way. To address health in the evaluation, we propose to combine at least two sources of information to estimate health impacts (albeit not with the precision and power possible by adding a separate sample of households with children under three years old, as we had proposed in the original design).

First, as the general population sample described above is randomly drawn, we would expect that about 22% of households in the sample (roughly 600-700 households) will be households with children under three (based on data on the number of households with children under three in the census data). The sample of children under age three is important for measuring changes in the diarrhea rate because the vast majority of diarrhea cases occur in children under five. We are adopting a panel survey design, with two years between the first and second survey. To see changes in the diarrheal rate, we need to identify households that will have children under age five at the time of both the first and second survey. Households that now have children under three will still have children under five in two years.

These households with children under three would play two roles in our estimates of the health effects of the W&S interventions.

- Most importantly, the small sample of households with children under three would be our primary source of information about the time and monetary costs of illness – how much do these households spend on a curing diarrhea and how much productive time is lost? Both pieces of information are important inputs into the ERR for the water and sanitation projects.
- The sub-sample of households with small children may also be large enough to detect a very large decrease in diarrhea rates among this population. With a general population sample of about 3,400 households, for example, there should be a sufficient number of households with children under three years (expected n=755) to detect a diarrhea rate decrease of between 40-50% during the dry season (See Table 6.1). It is probably unrealistic to expect to see reductions of this magnitude in El Salvador given the progress that has been made in reducing diarrhea rates in recent years. Nonetheless, if there were to be a large change, it is likely that we would be able to detect it with this small sample of households under age three. It is important, however, not to expect too much from this small sample of children under three. Not only is the effect size that this sample could detect large, these effects would also be measured with a sample of at most six households with children under three per census segment. Since these are likely to be only a small fraction of the households with children under three in the

segment, we could not claim that the households in a community are representative of the effects in that segment.

Table 7.1: Sample Sizes Required to Detect Changes in Diarrhea Rates in Households with Children under five Years, by Effect Size and Baseline Diarrhea Rate (assuming about six observations per segment and 76 segments)

Effect Size	90% of April-May Average Estimated 2007 Diarrhea Prevalence in under 5s =17.4%*	90% of May - July Average Estimated 2007 Diarrhea Prevalence in under 5s = 26.5%*
35%	1,233	833
40%	919	623
50%	556	378
*Percentage of population under 5 who experience diarrhea at least once in this time period.		

Second, we propose to leverage existing epidemiological research that measures the health impacts associated with exposure to W&S interventions to estimate the health effects likely to have been realized as a result of the Compact's W&S investments. There have been several recent studies summarizing the epidemiological research on the health effects of water and sanitation. For example, Fewtrell et al. (2005) report a meta-analysis of studies evaluating the health impacts of W&S interventions. Our evaluation would measure the changes *in water and sanitation outcomes* (e.g., access and use of improved source) in the treatment and control areas through the general population panel survey, and then use existing epidemiological study results to calculate how these changes in outcomes are likely to influence health in the El Salvador case. As a test of this approach, information from the sub-sample of households with small children will be used to help assess the applicability of the existing epidemiological studies to the El Salvador case.

In addition to these two options, there is a third that is also available to us, but which would require additional resources. We could try to measure changes in diarrhea rates in control and treatment areas by studying information from the daily census kept at public health posts (municipal or sub-municipal level). These daily patient census records are kept for five years and include both the purpose of the visit as well as the residence of the patient. By examining a sample of census records in the clinics associated with our study population, we could compare the number of diarrhea-related visits by residents of treatment and control areas during a designated period in the baseline year, and in at least one follow up year. As diarrhea rates are highest in the mid-May-June period, we could focus the data collection efforts on records from this period. This information could be an important complement to the information about diarrhea rates gathered in the sub-sample of households under three and to the existing health department information on diarrhea rates at the municipal level.

As records are kept for five years, there is no need to make a final decision about this additional work now. The review could be done in either 2011 or 2012, parallel with the second or third year of the panel survey. Initial indications from MCC are that it would be a good idea to pursue this, thus we will begin in 2010 to examine budgetary and time

implications for this activity as well as to request the required permission from the Ministry of Health.

8. Matching "Communities" and Sample selection

The identification of the survey sample for the study involved a series of steps:

1. Identifying treatment "communities" (census segments)

The first step in the process was to identify the census segments that are expected to contain W&S projects. To do this, FOMILENIO M&E staff and staff of the Census division searched the census data base to find the census segments where the caseríos, barrios, lotificaciones, or cantons included in the project descriptions were located.

This proved to be quite difficult for a number of reasons. First, the information on caseríos is not systematically recorded in the census database. Rather the name of the caserío appears in the address field, *only if* a household reported the caserío when asked for the address. Some caseríos simply did not appear in the census data base. Others appeared but were associated only with a very small number of households. The list of identified treatment segments is thus likely to be missing some parts of project areas.

Second, in some cases, the information from the census database did not match the information in the project descriptions, for example, because according to the census database the caserío where the project was to take place was located in a different canton than was listed in the project description. FISDL, a GOES agency assisting in the implementation of the project, was asked to check these cases. In most cases, their reply was that the project description was correct. In a few cases, they corrected the project description.

In the end, a census segment was considered a "treatment segment" if any households in the segment listed the project community in their address. Nine projects (13% of the projects) had to be removed from the treatment sample frame because we were unable to identify any census segments associated with them. These projects are listed below.

CHALATENANGO	La Palma	El Aguacatal	Bella Vista
CHALATENANGO	San Rafael	San Rafael	Colonia Las Brisas
CHALATENANGO	Tejutla	Aguaje Escondido	Aguaje Escondido
SAN SALVADOR	El Paisnal	El Matazano	Central
SAN SALVADOR	El Paisnal	El Paisnal	La Fe y San Dieguito
CHALATENANGO	La Palma	El Aguacatal	Las Cruces
CABAÑAS	Dolores	Villa Dolores	Puebla Nueva I
CHALATENANGO	La Palma	Los Horcones	Cumbres de San José
LA UNION	Lislique	El Derrumbado	San José Las Tunas

2. Choice of treatment segments

Originally, we had planned to draw a random sample of treatment segments. However, as it turned out, we did not end up with many more treatment segments than the number required for this design (100 vs. 88). We chose instead to use clear criteria to reduce the set of possible treatment segments to the 88 that we needed. Segments were eliminated if:

- The project associated with the treatment segment was very small (had fewer than 100 participants)
- The segment had fewer than 55 identified households (which from DIGESTYC's past experience means that it would be hard to select a sample of 18 occupied houses)
- The segment was also included in either the connectivity or northern zone multi-purpose household survey samples (which would make selection of households for the water survey difficult) and elimination of the segment would not eliminate the entire project area from the survey sample.

Elimination on these criteria left a total of 90 segments. To reach 88, we also eliminated one treatment segment from each of the two largest project areas (the treatment segment from each project that contained the smallest number of households). Most of the final 88 treatment segments include only one project, but three segments encompass two different project areas.

As tables 8.1 and 8.2 show, this process of systematic elimination did not significantly alter the distribution of segments across project types or across departments.

Table 8.1 Number (and %) of treatment census segments with different types of projects

Project type	Available sample of 100 treatment segments	Final sample of 88 treatment segments
New water system	55 (55%)	48 (54.5%)
Rehabilitation of existing system	1 (1%)	1 (1.1%)
Expansion of existing system	7 (7%)	6 (6.8%)
Improvement of existing system	16 (16%)	14 (15.9%)
Improvement and expansion of existing system	21 (21%)	19 (21.6%)

Table 8.2 Distribution of treatment census segments by department

Department	Available sample of 100 treatment segments	Final sample of 88 treatment segments
CABAÑAS	15 (15%)	12 (13.6%)
CHALATENANGO	46 (46%)	40 (45.4%)
CUSCATLAN	6 (6%)	5 (5.7%)
LA UNION	9 (9%)	9 (10.2%)
MORAZAN	19 (19%)	18 (20.4%)
SAN MIGUEL	2 (2%)	2 (2%)
SANTA ANA	3 (3%)	2 (2%)

3. Selection of control communities

The third step in the selection of the sample was to select the control communities. In this case, the control group was determined by matching the chosen treatment census segments with control census segments *located within the municipalities that were eligible for the W&S program*. Propensity score matching was used to identify the control segments that were most similar to treatment segments on observable variables thought to predict likelihood of being chosen for inclusion in a W&S project area.

The steps in propensity score matching are as follows. First, estimate a logit model of whether or not a census segment is set to benefit from a project on a pooled sample of segments with and without projects. Second, calculate the propensity score for all communities in the pooled sample. Third, for each treatment community, find the non-treatment community with the closest propensity score[18]. Fourth, create a short-list of the non-treatment communities identified in the previous step to serve as the 'matched control' communities.

Alternative specifications of the selection model were judged on the basis of their prediction rates and their ability to reduce bias between control and treatment groups. The final model used the following variables to predict whether or not a census segment would be a treatment segment:

- Indicators of location
  - Average temperature
  - Dummy variables for department
- Characteristics of population and location
  - Population of municipality in which segment is located

[18] Treatment segments that were not chosen to be included in the sample were excluded as possible matches.

    - Density of settlement
    - Percent of surface area occupied by water bodies (an indicator of access to surface water)
    - % of households in segment relying on private well for water
    - Average household size (which is highly correlated with "rural area")
    - Average number of household members who had emigrated (an indicator of external resources available to the community)
    - % of households in segment with in-home business (an indicator of economic diversity and non-residential demand for water services)
- Indicators of inadequate water and/or sanitation
    - % of households in segment relying on unimproved water source
    - % of households in segment with piped water, but not receiving water every day
    - % of households in segment with no improved sanitation facilities
    - % of households in segment with composting latrines

Annex 6 includes the logistic regression results as well as tables showing how the matching reduces bias.

4. Selection of households

The final step in the selection of the sample was the selection of households. DIGESTYC staff visited all chosen treatment and control segments to map the segment and create a sample frame of occupied houses. Only occupied buildings that served as either a residence only or as both a business and a residence were retained in the sample frame. Eligible buildings were then listed according to the DIGESTYC standard numbering system (working from one side of the segment to the other), and grouped into blocks of four. Systematic sampling was used to draw six blocks of four (consecutive) residences each, for a total sample of 24 households.

In each block, the first three residences (in numerical order according to the DIGESTYC code) were visited by the enumerators on the day of the survey. If one of these three houses was found not to be eligible for the survey *because of a problem with the sample frame*, then the enumerator visited the fourth house as a replacement. Only one replacement house was available for each block.

If more than one household was found to live in a particular residence that had been selected into the sample, the primary household (the household of the owner or the household that had lived longest in the home) was chosen as the household to be interviewed. This was done because we want to have the best possible chance of finding interviewed households again at the time of next survey.

In some cases, occupied houses were stripped from the sample frame before the selection was made. This was done when the sample frame produced by DIGESTYC indicated that the treatment segment included a large number of households who did not live in the (anticipated) project area (according to the project description available at the time of the sampling). In order to increase the probability that the sample in "treatment segments" would comprise treated households, those households outside of the anticipated treatment area were eliminated.

When this was done, the sample frame for the matched control segment was also reviewed. If the elimination of some houses from the treatment frame changed the W&S coverage rate among eligible households in the treatment segment, then we evaluated whether the match between the matched treatment and control households could be improved by also eliminating households living in a particular caserío from the control segment.

A second reason that some residences were eliminated from the sample frame was to avoid overlap with other surveys. DIGESTYC was concerned that response rates or the quality of responses would go down if the same households were included in multiple surveys (particularly since this survey, the survey for the evaluation of the connectivity program, and part of the northern zone survey sample are all panel surveys). Thus, households chosen for the other surveys were eliminated from the sample frame before selection was done.

9. Data collection activities

Figure 9.1. Timing of data collection and report writing activities

	Year 2 (2009)	Year 3 (2010)	Year 4 (2011)	Year 5 (2012)
Household surveys, including residual chlorine tests	April - May		April - May	April - May
Community surveys and descriptions	April - May		April - May	April - May
Data entry and processing of survey data	May - September		May - September	May - September
Measures of source water quality	April - May			April - May
Measures of household water quality	April - May			April - May
Measures of water quality in piped systems by Ministry of Health[19]	Year round: treatment communities	Year round: control communities	Year round: all communities	
Qualitative interviews done by SI team		Timing to be determined		
Data collection from health posts		Preparatory work	Obtain necessary approvals	Timing to be determined
Reporting Baseline Interim Final	Dec		Dec	Dec

[19] Subject to continuing agreement by Ministry of Health officials

The timing of the household survey activities are planned as follows:

- Three survey rounds (in April – May of years 2, 4, and 5)
- Roughly 3168 surveys per round
- The content and length of the survey will be roughly the same in each year. In year four, however, some of the questions from year one will be replaced with questions about household participation in and opinions about the water project. These new questions will require some limited pre-testing.
- In all three years, enumerators will do tests of residual chlorine levels in the drinking water of all interviewed households.
- In first year, maps of sampled census segments including inhabited properties will be prepared in February-March.

Parallel to the household survey, a private firm will do testing for bacterial contamination in drinking water (at the household level) and at a selection of sources in the community. These tests will only be done in year two and year five. We currently expect to re-test the same, and same number of, communities and households in year five as were tested in year two; however, we will revisit this after reviewing the first year of results.

The timing of the community survey activities are planned as follows:

- Community surveys will be implemented at the same time as each of the three survey rounds, in each of the census segments included in the survey sample. These will involve surveys of a community leader about characteristics of the community and prices and services in the community. In years 4 and 5, the survey will also include questions regarding status of water sources and W&S project.

Assuming that the Ministry of Health and FOMILENIO approve the research design and budget for the records survey in health posts, this data collection effort will be done in the final year of the study. To prepare for this, initial tests of the methodology and budget estimates will be prepared starting in year three and approvals will be sought in year four. Ideally the data collection should be done in August-September of 2012, in order to include one additional rainy season (June-July) in the period studied. However, that may be too late to permit proper analysis of the results, in which case the study could be done earlier – between late fall 2011 and up to May/June 2012.

The exact timing of the qualitative assessments has yet to be determined, but should take place in year three in order to inform the interpretation of the quantitative data from the Year four and five surveys.

An analysis of the baseline surveys is slated for December 2009. A report of the interim survey findings, including qualitative data from the interviews, will be written in December 2011. The final report analyzing the year five survey, water quality measurements, and any data collected from the Ministry of Health will be written in December of 2012.

10. References

Alderman, H. 2000. Anthropometry. In Designing Household Survey Questionnaires for Developing Countries: Lessons from 15 Years of the Living Standards Measurement Study. M. Grosh and P. Glewwe, eds. Washington, DC, The World Bank. Pp. 251-272.

Baker, J.L. 2000. Evaluating the Impact of Development Projects on Poverty: A Handbook for Practitioners. The World Bank, Washington.

Bosch, C., K. Hommann, G. Rubio, C. Sadoff, and L. Travers. 2000. Water and Sanitation. Chapter 23 in A Sourcebook for Poverty Reduction Strategies Volume 2, pages 371-404. Washington, D.C.: World Bank.

Dehejia, R.H. and S. Wahba. 1999. Causal Effects in Nonexperimental Studies: Reevaluating the Evaluation of Training Programs. Journal of the American Statistical Association 94 (December): 1053–1062.

Diggle, P.J., K.-Y. Liang, and S.L. Zeger. 1994. Analysis of Longitudinal Data, Oxford Statistical Science Series. Oxford: Clarendon Press.

Donner, A., N. Birkett, and C. Buck. 1981. Randomization by Cluster – Sample Size Requirements and Analysis. American Journal of Epidemiology 114(6): 906–914.

Fewtrell, L., R.B. Kaufmann, D. Kay, W. Enanoria, L. Haller, and J.M. Colford. 2005. Water, Sanitation, and Hygiene Interventions to Reduce Diarrhoea in Less Developed Countries: A Systematic Review and Meta-Analysis. The Lancet Infectious Diseases. 5(1): 42–52.

Galiani, S., P. Gertler, and E. Schargrodsky. 2005. Water for Life: The Impact of the Privatization of Water Services on Child Mortality. Journal of Political Economy. 113: 83-120.

Heckman, J., H. Ichimura, and P. Todd. 1997. “Matching As An Econometric Evaluation Estimator: Evidence from Evaluating A Job Training Program.” Review of Economic Studies 64(4):605–654.

Heckman, J., H. Ichimura, and P. Todd. 1998b. “Matching As An Econometric Evaluation Estimator.” Review of Economic Studies 65:261–294.

Heckman, J., H. Ichimura, J. Smith, and P. Todd. 1998a. “Characterizing Selection Bias Using Experimental Data.” Econometrica 66(5):1017–1098.

Katz, J., V.J. Cary, S.L. Zeger, and A. Sommer. 1993. Estimation of design effects and diarrhea clustering within households and villages. American Journal of Epidemiology. 138(11): 994

Mansuri, G., and V. Rao. 2004. “Community-Based and –Driven Development: A Critical Review.” The World Bank Research Observer 19(1):1–39

Pattanayak, S.K., J.-C. Yang, C. Poulos, S.R. Patil, B. Arnold, and J.M. Colford. 2008b. Three years later: Environmental health impacts of a community-demand-driven water and sanitation program in rural Maharashtra, India. RTI International Working Paper 08-01, March 2008.

Pattanayak, S.K., J.-C. Yang, K.L Dickinson, C. Poulos, S.R. Patil, R.K. Mallick, J.L. Blitstein, and P. Praharaj. 2008a. Shame or subsidy revisited: Randomized evaluation of social mobilization for sanitation in Orissa, India. RTI International Working Paper 08-03, Aug 2008.

Poulos, C., S.K. Pattanayak, and K. Jones. 2006. A Guide to Water and Sanitation Sector Impact Evaluations. Doing Impact Evaluation Series No. 4. World Bank. < http://siteresources.worldbank.org/INTISPMA/Resources/383704-1146752240884/Doing_ie_series_04.pdf>

Prüss, A., D. Kay, L. Fewtrell, and J. Bartram. 2002. Estimating the Burden of Disease from Water, Sanitation, and Hygiene at a Global Level. Environmental Health Perspectives. 110 (5): 537-542.

Purdon, Susan. 2002. Estimating the Impact of Labour Market Programmes. National Center for Social Research Working Paper No. 3.

Ravallion, M. 2005. Evaluating anti-poverty programs. World Bank Policy Research Working Paper 3625. The World Bank, Washington, D.C.

Rawlings, L.B. and N.R. Schady. 2002. Impact Evaluation of Social Funds: An Introduction. The World Bank Economic Review 16 (2): 213-217.

Rosenbaum, P.R., and D.B. Rubin. 1983. “The Central Role of the Propensity Score in Observational Studies for Causal Effects.” Biometrika 70(1):41-55.

Smith, J. and P. Todd. 2005. Does Matching Address LaLonde’s Critique of Nonexperimental Estimators? Journal of Econometrics. 125:305-353.

Strand, J and I. Walker. 2005. Water Markets and Demand in Central American Cities. Environment and Development Economics. 10: 313-335.

Annex 1: List of eligible municipalities

Departamento	Municipio
CABAÑAS	DOLORES
CABAÑAS	GUACOTECTI
CABAÑAS	ILOBASCO
CABAÑAS	SAN ISIDRO
CABAÑAS	SENSUNTEPEQUE
CABAÑAS	TEJUTEPEQUE
CABAÑAS	VICTORIA
CHALATENANGO	AGUA CALIENTE
CHALATENANGO	AZACUALPA
CHALATENANGO	CITALA
CHALATENANGO	COMALAPA
CHALATENANGO	CONCEPCION QUEZALTEPEQUE
CHALATENANGO	DULCE NOMBRE DE MARIA
CHALATENANGO	EL CARRIZAL
CHALATENANGO	EL PARAISO
CHALATENANGO	LA PALMA
CHALATENANGO	LA REINA
CHALATENANGO	LAS FLORES (San José)
CHALATENANGO	NOMBRE DE JESUS
CHALATENANGO	NUEVA TRINIDAD
CHALATENANGO	SAN ANTONIO LA CRUZ
CHALATENANGO	SAN FRANCISCO LEMPA
CHALATENANGO	SAN IGNACIO
CHALATENANGO	SAN LUIS DEL CARMEN
CHALATENANGO	SAN MIGUEL DE MERCEDES
CHALATENANGO	SAN RAFAEL
CHALATENANGO	SANTA RITA
CHALATENANGO	TEJUTLA
CHALTENANGO	NUEVA CONCEPCION
CUSCATLAN	SUCHITOTO
LA LIBERTAD	SAN PABLO TACACHICO
LA UNION	ANAMOROS
LA UNION	BOLIVAR
LA UNION	EL SAUCE
LA UNION	LISLIQUE
LA UNION	NUEVA ESPARTA
LA UNION	POLOROS
LA UNION	SAN JOSE (de la fuente)
MORAZAN	ARAMBALA
MORAZAN	CACAOPERA

MORAZAN	CHILANGA
MORAZAN	CORINTO
MORAZAN	DELICIAS DE CONCEPCION
MORAZAN	EL DIVISADERO
MORAZAN	EL ROSARIO
MORAZAN	JOCOAITIQUE
MORAZAN	LOLOTIQUILLO
MORAZAN	MEANGUERA
MORAZAN	OSICALA
MORAZAN	PERQUIN
MORAZAN	SAN FERNANDO
MORAZAN	SENSEMBRA
MORAZAN	SOCIEDAD
MORAZAN	YAMABAL
SAN MIGUEL	CIUDAD BARRIOS
SAN MIGUEL	NUEVO EDEN DE SAN JUAN
SAN MIGUEL	SAN GERARDO
SAN MIGUEL	SAN LUIS DE LA REINA
SAN MIGUEL	SESORI
SAN SALVADOR	EL PAISNAL
SANTA ANA	SAN ANTONIO PAJONAL
SANTA ANA	SANTA ROSA GUACHIPILÍN

Annex 2: Selection criteria

Criteria for selection of project profiles for the feasibility stage

No	CRITERIOS	
1	MUNICIPIOS ELEGIBLES	De acuerdo al Índice Integrado de Marginalidad Municipal los municipios a participar son los de Extrema Pobreza Alta y Extrema Pobreza Moderada(Ver Anexo I A y B)
2	CONTRAPARTIDA	Acuerdo Municipal donde asegura el aporte del 10% en efectivo de la municipalidad para la inversión total del proyecto. Acta de Asamblea General donde asegura el aporte del 10% en efectivo o especie de la comunidad la inversión total del proyecto.
3	VIABILIDAD SOCIAL	COMUNIDAD ORGANIZADA: Personería Jurídica o Acta de Asamblea General donde muestren disposición de organizarse. Compromiso de la comunidad de participar en todo los pasos del proyecto, especialmente en las actividades de toma de decisiones. (Actas de Asambleas Generales) Compromiso comunitario-municipal para la administración, operación y mantenimiento de las obras y sistema. El mecanismo de comunicación será a través de las Asambleas Generales de las cuales se levantarán actas formalizando los acuerdos tomados.
4	COSTO POR BENEFICIARIOS	La razón entre el costo del proyecto y el número de beneficiarios sea como máximo $850.00 dólares por beneficiario

Beneficiario: Personas con acceso al servicio de agua potable y/o saneamiento básico como resultado de la intervención del programa de Cuenta del Milenio.

Conditions projects must meet in the feasibility phase

No	CRITERIOS	
1	PROYECTOS ELEGIBLES	Todos aquellos perfiles seleccionados con los criterios anteriores.
2	VIABILIDAD ECONOMICA Y FINANCIERA	Disposición de pago de tarifa mensual sostenible a ser establecida en conjunto comunidad-municipalidad-contratista en Estudio de Factibilidad. Las tarifas deben cubrir los gastos de operación y mantenimiento del sistema

No	CRITERIOS	
		Cada proyecto individual no deberá de exceder los $500,000. De proponerse obras de mayor costo, serán analizadas caso por caso por el Comité Técnico conformado por representantes de FISDL y FOMILENIO, teniendo como observador a la MCC. La Junta Directiva de FOMILENIO tomará la decisión de aprobar en base a los impactos esperados en la población a beneficiar.
3	VIABILIDAD TECNICA	Dotación de Diseño: 70 a 100 litros/persona/día para distribución domiciliar. Dotación de Diseño: Mínimo de 35 litros/persona/día para distribución por cantarera. Calidad del Agua: Potable Disponibilidad de Fuente de Agua para un periodo de 20 años Se construirán letrinas VIP (Letrinas de Foso Mejoradas) cuando las condiciones del terreno lo permitan (terreno no rocoso, nivel freático, etc.). En caso de no cumplirse estas condiciones, se proveerá letrinas tipo aboneras. Se deberá respetar los estándares establecidos por el Ministerio de Salud. Además deberá cumplir con el contenido de la Guía de Formulación de Carpetas Técnicas para Proyectos de Agua Potable del FISDL, ver extracto de los requisitos de la Guía de Formulación del FISDL Anexo II.
4	VIABILIDAD LEGAL	Propiedad de terrenos a favor de las municipalidades o carta compromiso de compra/donación/comodato de terrenos donde se encuentra la fuente de agua y donde se construirán las obras (que no tengan problemas legales o sociales) y autorización de servidumbres.
5	VIABILIDAD AMBIENTAL	Fuente de Agua: Capaz de suministrar la cantidad de 70 al 100 litros/persona/día para el periodo de diseño (20 años) Permisos MARN: Ficha de Impacto Ambiental y de ser necesario Estudio de Impacto Ambiental Plan de Manejo de Microcuenca

Annex 3: List and description of projects

Pobreza	Canton	Comunidad/Caserio/ Lotificacion	# de proyecto	Proyecto
Alta	Cañafístula	Vado de Lagarto	202700	Introducción de Agua en Comunidad Vado de Lagarto, Cantón Cañafístula
Alta	Curaren	Nueva Curaren	202660	Mejoramiento del Sistema de Agua Potable en Caserío Catarina, desde fuenta el Salamo, Cantón Paratao
Alta	Niqueresque	El Jocote	202680	Introducción de Agua Potable Caserío El Jocote, Cantón Niqueresque
Alta	Villa Dolores	Lotificación Puebla Nueva I y Puebla Nueva II	203820	Introducción de Agua Potable en Lotificación Puebla Nueva I y Puebla Nueva II
Alta	(A)Paratao	Catarina	202650	Mejoramiento del Sistema de Agua Potable en Caserío Catarina, desde fuenta el Salamo, Cantón Paratao
Alta	La Uvilla	La Uvilla	202640	Introducción de Agua Potable en Cantón La Uvilla
Alta	Paratao	El Papalon, el Limo	202690	Introducción de Agua Potable, Caserío El Papalon y Caserío el Limo, Cantón Paratao
Alta	San Pedro	San Juan	202630	Incorporación de Fuente Los Cuervos al Sistema de Agua Potable en Caserío San Juan del Cantón San Pedro.
Alta	Santa Marta	San Felipe	202670	Mejoramiento y Ampliación del Sistema de Agua Potable en Caserío San Felipe
Moderada	Izcatal	Izcatal	203810	Introducción de Agua Potable al Cantón Izcatal
Moderada	Potrero Batres	Potrero Batres	203800	Introducción de Agua Potable en los Cantón Potrero Batres
Alta	Agua Zarca	Agua Zarca	202620	Introduccion de Agua Potable en Canton Agua Zarca
Alta	Candelaria y La Junta	La Cuchilla, Irayoles, Chorosco	202380	Mejoramiento del sistema de agua potable a caseríos La Cuchilla, Irayoles, Chorosco y Cantón Candelaria.
Alta	El Morro	El Morro	202390	Ampliación y mejoramiento del Sistema de Agua Potable en Cantón El Morro
Moderada	El Tablon	Los Cruces	202770	Abastecimiento de Agua Potable Caserio Los Cruces, Cantón El Tablon
Moderada	El Aguacatal	Comunidad Bella Vista	202590	Mejoramiento del Sistema de Agua y Saneamiento de la Comunidad Bella Vista
Moderada	El Aguacatal	Las Cruces	202600	Mejoramiento del Sistema de Agua y Saneamiento de la Comunidad Las Cruces
Moderada	El Gramal	El Gramal	202580	Ampliacion y mejoramiento del Sistema de Agua Potable e Instalacion de alcantarillado Sanitario y Sistema de Tratamiento de agua residuales domesticas en el Cantón El Gramal
Moderada	El Tunel	El Tunel	202740	Mejoramiento del Sistema de Agua y Saneamiento del Canton El Tunel

Pobreza	Canton	Comunidad/Caserio/ Lotificacion	# de proyecto	Proyecto
Moderada	Los Horcones	Cumbres de San Jose, Cumbras del Gramal, La Montañita, La Paterna, El Terrero, Las Mesitas, El Panal, Rancho Quemado, Los Horcones, El Chupadero, Los Chaguites, El Guayabito y El Pino	202610	Mejoramiento del Sistema de Agua y Saneamiento del sistema mùltiple de las comunidades Cumbres de San Jose, Cumbras del Gramal, La Montañita, La Paterna, El Terrero, Las Mesitas, El Panal, Rancho Quemado, Los Horcones, El Chupadero, Los Chaguites, El Gua
Moderada	San Jose Sacare	San José Sacare	202720	Ampliacion y mejoramiento del Sistema de Agua Potable e Instalacion de alcantarillado Sanitario y Sistema de Tratamiento de agua residuales, domesticas en el Canton San Jose Sacare
Moderada	El Tigre	El Tigre	202460	Introduccion de Agua Potable en Cantón El Tigre
Moderada	Las Penas	Las Peñas	203860	Introduccion de Agua Potable Canton Las Peñas
Alta	Nombre de Jesús	Barrio El Calvario	202500	Mejoramiento del sistema de Agua Potable y saneamiento Barrio El Calvario
Moderada	Santa Rosa	La Cruz	202400	Introduccion del Servicio de Agua Potable en Colonia La Cruz, Canton Santa Rosa
Moderada	Sunapa	Conacastillo	202430	Introduccion del Servicio de Agua Potable en Caserio Conacastillo Canton Sunapa, Nueva Concepción
Alta	Los Naranjos y Santa Cruz	Los Naranjos y Santa Cruz	202760	Introduccion de Agua Potable y letrinizacion en los Cantones Los Naranjos y Santa Cruz
Alta	Piedras Gordas	El Pino	202410	Introducción de agua potable y saneamiento básico del Cantón Piedras Gordas y Caserío El Pino
Moderada	San Rafael	Colonia Las Brisas	202800	Mejoramiento del Sistema de Agua Potable y Letrinizacion en Caserio Las Brisas
Moderada	Chilamate	La Rastra	202560	Introducción de Agua Potable Caserío la Rastra, Cantón Chilamate
Moderada	Aguaje Escondido	Aguaje Escondido	202830	Intruccion de Agua Potable al Cantón Aguaje Escondido
Moderada	Aldeita	El Copinolito	202860	Ampliación y mejoramiento del sistema de agua potable en Caserío El Copinolito
Moderada	Concepcion	Concepción	202450	Introduccion de Agua Potable en Canton Concepcion
Moderada	El Salitre	El Salitre	202470	Introduccion de Agua Potable Canton El Salitre
Moderada	Los Martinez	Los Martínez	202520	Ampliación y mejoramiento del Sistema de Agua Potable en Cantón Los Martínez
Moderada	Quitasol	El Coyolito	202820	Introducción de Agua Potable al Caserio El Coyolito, Cantón Quitasol

Pobreza	Canton	Comunidad/Caserio/ Lotificacion	# de proyecto	Proyecto
Moderada	Colima	Colima	202420	Ampliacion y mejora del sistema de Agua Potable y saneamiento de la Comunidad Colima, Canton Colima
Moderada	Montepeque	El Franco	202490	Ampliacion del Sistema de Agua Potable Comunidad El Franco
Moderada	San Lucas	San Francisco	202440	Ampliacion de Sistema de Agua Potable y Saneamiento en Comunidad San Francisco, Canton San Lucas
Moderada	San Lucas	El Trapiche	202780	Ampliacion de Sistema de Agua Potable y Saneamiento en Comunidad El Trapiche, Canton San Lucas
Moderada	Albornoz	Albornoz	202730	Introduccion de Agua Potable y Saneamiento Basico en Carton Albornoz
Moderada	Villa de Bolivar	Area Urbana de Villa Bolívar	202710	Mejoramiento y ampliacion del sistema de agua potable en el área urbana de la Villa de Bolivar
Alta	Agua Fría	El Rulumbron	204070	Rehabilitacion del Sistema de Agua Potable en Caserio El Relumbron
Alta	El Derrumbado	San Jose Las Tunas	204060	Introduccion de Agua Potable en Caserio San Jose Las Tunas
Moderada	Monteca	La Pista	203940	Introduccion de Agua Potable en Caserio La Pista, Canton Monteca
Moderada	Palo Blanco	Los Fuentes, Los Velasquez, Piletas y Palo Blanco	204050	Introduccion de Agua Potable en Caserio Los Fuentes, Los Velasquez, Piletas y Palo Blanco
Alta	Chaguitillo	Chaguitillo	202750	Introduccion de Agua Potable en Canton Chagüitillo
Alta	Pueblo Viejo	Las Quebradas	203970	Introduccion de Agua y Saneamiento en Caserio Las Quebradas
Alta	Tierra Colorada	Las Pilas	203980	Introduccion de Agua Potable y Saneamiento en Caserio Las Pilas
Alta	Tierra Colorada	Pinalito	203990	Ampliacion del Sistema de Agua Potable y Saneamiento Basico en Caserio Pinalito
Alta	Tierra Colorada	Talchiga	204000	Introduccion de Agua Potable y Saneamiento en el Caserio Talchiga
Alta	Tierra Colorada	El Matazano	204010	Introduccion de Agua Potable y Saneamiento en Caserio El Matazano
Alta	El Pedernal	Los Vasquez	203830	Perforacion de Pozo y Ampliación del Sistema de Agua Potable en Caserio Los Vasquez, del Canton El Pedernal
Alta	Joya del Matazano	Los Hernandez	203850	Introduccion de Agua Potable en Caserio Los Hernández, Canton Joya del Matazano, Municipio de Chilanga
Alta	La Laguna	El Tablon, Los Plancitos, y casco urbano	203910	Introducción de Agua Potable La Laguna para CaseríosEl Tablón, Los Plancitos y casco urbano
Alta	Ojos de Agua	Ojos de Agua	203920	Mejoramiento del Sistema de Agua Potable al Canton Ojos de Agua
Moderada	Jocoaitique	Jocoaitique	204030	Mejoramiento del Sistema de Agua Potable del Casco Urbano
Moderada	Volcancito	Llano Liso	204020	Introduccin del Sistema de Agua Potable al Caserio Llano Liso

Pobreza	Canton	Comunidad/Caserio/ Lotificacion	# de proyecto	Proyecto
Moderada	La Joya	San Romero	203900	Abastecimiento de Agua Potable en Colonia San Romero, Canton La Joya
Moderada	Huilihuiste	Huilihuiste	203890	Mejoramiento de Sistema de Agua Potable, manantial El Ocote
Alta	San Juan	San Juan de La Cruz	203840	Mejoramiento de Sistema de Agua Potable en área céntrica en Cantón San Juan
Alta	Jardin	Jardín	203880	Perforacion de pozo e intruduccion de agua potable y saneamiento basico, Canton Jardin
Alta	Jardin	Caserio La Zincuya	204040	Perforacion de pozo e introduccion de agua potable y saneamiento basico, Caserio La Zincuya, Canton Jardin
Alta	San Sebastián	San Sebastián	203870	Perforación de pozo e introducción de agua potable y saneamiento básico
Moderada	El Matazano	Caserio Central	202810	Introduccion de Agua Potable en Caserio Central, Canton El Matazano
Moderada	El Paisnal	La Fe, Caserio Central San Dieguito	202510	Introduccion de Agua Potable en colonia La Fe y Caserio Central San Dieguito
Moderada	El Tablón	El Tablón	202840	Mejoramiento Sistema de Agua Potable Canton El Tablon

Annex 4: Primary indicators

Subject	Indicators
Expenditure	• Average total monthly household expenditures
Income	• Average total household income • Average household income from business • Average household income and individual income (by gender and age) from wage labor
Productive time use	• Average time spent on income-earning activities, by household and by gender • % of women / men / girls / boys engaged in wage labor • % of women / men / girls / boys working in a household business
Education	• School enrollment rate, for children ages 7 to 12, by gender • Average days of school attendance during the 2 weeks prior to the survey, by gender
Health	• Diarrhea rate among children under 5, and among all individuals, during 2 weeks prior to the survey • % of households with adequately chlorinated water
Coping costs	• Average time spent collecting water, by household and by gender and age of household member • Average monthly expenditures on water, total and by source • Average monthly expenditures on treatment, maintenance, alternative sources and storage
Coverage and access	• % of households with improved water service at the home, in the community • % of households with latrine or other improved sanitation service at the home • Average distance household travels to water source
Water use	• % of households using improved water service for drinking and cooking • % of households that still using unimproved water service for drinking and cooking • % of household that use household latrine for sanitation needs

Annex 5: Water quality testing

In all households interviewed for this evaluation, we will conduct a field test of residual chlorine levels in the household's drinking water supply. This test alone, however, is not enough to show whether or not water poses a health risk to the households. Non-chlorinated water may have no bacterial contamination, for example. To have better insight into the health risk posed by the water supply before and after the Compact investments, additional laboratory-based tests of water quality will be conducted in a sample of households. Also, the water quality in piped systems and some other non-piped sources will be tested in a subsample of communities.

These additional tests will be done in two ways. First, in 2009 (and hopefully again in 2011-2012) the Ministry of Health will test (through personnel of the Unidades de Salud) for bacteriological contamination in all piped water systems in the treatment communities. In November 2008, we provided a list of communities to the Ministry of Health, and these communities have been included in the regular testing schedule for 2009 (spread out through the year). The Ministry of Health has agreed to follow the same system to test all control communities with piped water systems in 2010. These tests will also be repeated in 2011-2012.

Second, a private firm will test drinking water for bacteriological contamination (Bacterias coliformes totals, Bacterias coliformes fecales, Eschericha coli) in 12 households (out of the 18 that that make up the sample in each census segment) and do between two and six tests of water sources in 22 randomly sampled[20] treatment communities and the 22 control communities matched to these treatments.

The tests of household drinking water will be done on stored water, if the household stores drinking water, or will be taken directly from the tap or pipe if the household does not store drinking water. For the tests of source water quality, the firm will search for the sources used by the 12 households included in the water quality testing program. They will then follow the following procedure to identify the number of sources to test and which sources to test:

Areas con agua por cañería:

- *Aparte de las pruebas en los hogares, se hacen 2 pruebas del agua en el sistema de agua por cañería. Estas pruebas se hacen en un chorro en conexiones domiciliares o en una cantarera. Para escoger donde hacer las pruebas, se escoge entre los 12 hogares visitados el más cercano al tanque y el más lejano. Se toma la prueba de la conexión domiciliaria o de la cantarera que usa ese hogar. A estas 2 pruebas, se hace no solamente las pruebas de bacterias coliformes totales, bacterias coliformes fecales, and eschericha coli, sino también una prueba de cloro residual (en laboratorio).*
- *Además, se hacen hasta 4 pruebas adicionales en otras fuentes usadas por los hogares en la comunidad. Para identificar las fuentes, en los 12 hogares, se pregunta por su fuente de agua de tomar. Si alguno dijera que la fuente de su agua*

[20] Randomly sampled from the 88 treatment communities included in the survey sample.

de tomar no es su conexión domiciliaria, sino otra fuente, se toma una prueba de esa fuente también (hasta un máximo de 4 fuentes adicionales).

En areas sin agua por cañería:

- *Se pregunta a los hogares visitados cual es su fuente de su agua de tomar (es decir la fuente del agua que tiene almacenado para ese propósito en la casa). Se toman pruebas en las primeras 6 fuentes mencionadas por los hogares visitados. En caso de usar dos hogares la misma fuente, no se toma dos pruebas de esa fuente, sino una.*

Testing will be done in April – May 2009 (and repeated in April – May 2012), as close as possible to the timing of the household survey. The lab technicians will fill out a small survey at each household visited to assist in the process of linking household survey results and the results of household-level and source-level water quality tests.

The survey and the list of communities selected for the additional water quality testing are included below.

The sample size of water quality tests will not necessarily give us sufficient power to draw conclusions about water quality across the whole sample. The idea is to get a sense of how important water quality problems are in the project and control areas and whether problems lie in source contamination, in in-house contamination, or both. Based on the results of both these tests and the residual chlorine tests, we can decide whether to repeat this level of testing or increase it in follow up surveys.

DEPARTAMENTO	MUNICIPIO	CANTON
SANTA ANA	SAN ANTONIO PAJONAL	AREA URBANA
SANTA ANA	SAN ANTONIO PAJONAL	EL TABLON
CHALATENANGO	COMALAPA	EL MORRO
CHALATENANGO	EL PARAISO	EL TABLON
CHALATENANGO	EL PARAISO	SANTA BARBARA
CHALATENANGO	EL PARAISO	VALLE NUEVO
CHALATENANGO	LA PALMA	EL GRAMAL
CHALATENANGO	LA PALMA	SAN JOSE SACARE
CHALATENANGO	LA REINA	EL TIGRE
CHALATENANGO	LA REINA	LAS PEÑAS
CHALATENANGO	NOMBRE DE JESUS	POTRERIOS
CHALATENANGO	NUEVA CONCEPCION	LOS CHILAMATES
CHALATENANGO	NUEVA CONCEPCION	SANTA ROSA
CHALATENANGO	NUEVA CONCEPCION	SANTA ROSA
CHALATENANGO	LAS FLORES	LAS LIMAS
CHALATENANGO	SAN LUIS DEL CARMEN	EL PITAL
CHALATENANGO	SAN MIGUEL DE MERCEDES	EL SALITRE
CHALATENANGO	SAN MIGUEL DE MERCEDES	PIEDRAS GORDAS
CHALATENANGO	SAN RAFAEL	SAN JOSE LOS SITIOS
CHALATENANGO	SAN RAFAEL	SAN JOSE LOS SITIOS
CHALATENANGO	SANTA RITA	SAN NICOLAS PIEDRAS GORDAS
CHALATENANGO	TEJUTLA	CONCEPCION
CHALATENANGO	TEJUTLA	LOS MARTINEZ
CHALATENANGO	TEJUTLA	QUITASOL
CUSCATLAN	SUCHITOTO	COLIMA
CABAÑAS	GUACOTECTI	EL BAÑADERO
CABAÑAS	SAN ISIDRO	POTRERO DE BATRES
CABAÑAS	VICTORIA	PARATAO
CABAÑAS	DOLORES	CAÑAFISTULA
CABAÑAS	DOLORES	EL RINCON
SAN MIGUEL	NUEVO EDEN DE SAN JUAN	SAN SEBASTIAN
MORAZAN	CACAOPERA	CALAVERA
MORAZAN	CHILANGA	EL PEDERNAL
MORAZAN	EL ROSARIO	OJO DE AGUA
MORAZAN	MEANGUERA	LA JOYA
MORAZAN	MEANGUERA	LA JOYA
MORAZAN	SENSEMBRA	EL RODEO
MORAZAN	SOCIEDAD	AREA URBANA
MORAZAN	SOCIEDAD	ANIMAS
MORAZAN	YAMABAL	SAN JUAN DE LA CRUZ
LA UNION	BOLIVAR	AREA URBANA
LA UNION	LISLIQUE	EL DERRUMBADO
LA UNION	POLOROS	BOQUIN
LA UNION	SAN JOSE	EL SOMBRERITO

FONDO DEL MILENIO

Pruebas de calidad de agua -- linea de base

Prueba de agua de fuentes

I. IDENTIFICACIÓN

1. Departamento: _____

2. Municipio: _____

3. Cantón: _____

4. Segmento: _____

Prueba de fuente

1. Tipo de fuente

1 Agua por cañería

2 Poliducto

3 Pozo con bomba de gasolina, diesal, electricidad, o energia solar

4 Pozo con bomba de mechate o manual

5. Pozo sin bomba, abierto o con pita y balde

6. Manantial o ojo de agua protegido

7. Manantial o ojo de agua con captacion abierta ("pozo")

8. Manantial o ojo de agua sin captacion o proteccion

9. Rio

10. Quebrada

11. Lago o represa

12. Otro (especifique) _____

2 Prueba de fuente

ID de prueba

Fecha de prueba

Hora de toma de prueba

Hora recibido en laboratorio

Fecha y hora del analisis

Persona que tomó la árueba

Para prueba en conexión domiciliaria o pozo privado….

de vivienda

Para otras pruebas…

Ubicacion georeferenciada:

Descripcion de detalla de la ubicacion de la fuente (y foto)

REPUBLICA DE EL SALVADOR

FONDO DEL MILENIO

Pruebas de calidad de agua -- linea de base

Prueba de agua almacenada en hogar

I. IDENTIFICACIÓN

1. Departamento: _____

2. Municipio: _____ _____

3. Cantón: _____

4. Segmento: _____

Vivienda # _____ (1-12)

Caserio / barrio _____

Direccion: _____

Jefe del hogar: _____

Correlativo de vivienda: _____

1 Tipo de recipiente?

1 Cantaro abierto
2 Cantaro con tapa o chorro
3 Cubeta abierto
4 Cubeta con tapa o chorro
5 Pitchel abierto
6 Pitchel con tapa o chorro
7 Churumba o pichinga abierto
8 Churumba o pichinga con tapa o chorro
9 Barril abierto
10 Barril con tapa o chorro
11 Pila
12 Otro _____
13 No almacena agua / usa directamente de chorro o poliducto.

Pase a 3

2 Preguntar al hogar si le han hecho algun tratamiento en casa el agua almacenada para tomar

(no incluye tratamiento que se pudiera haber hecho en la fuente)

Marcar todas los tratamientos que el hogar dice que ha hecho al agua almacenado en ese momento.

1 Hechar cloro
2 Hechar lejia
3 Hechar PURIAGUA
4 Hervir
5 Asolear
6 Filtrar
7 Otro

3 Prueba de agua almacenda (o del chorro en caso de respuesta 13 a pregunta 1)

ID de prueba _____

Fecha de prueba _____

Hora de toma de prueba _____

Hora recibido en laboratorio _____

Fecha y hora del analisis _____

Persona que tomo la prueba _____

Persona del hogar que contesto las preguntas _____

4 De donde viene el agua almacenada para tomar en este hogar?

1 Conexion domiciliaria (propio o del vecino)
2 Cantarera
3 Poliducto (propio o del vecino)
4 Pozo privado
5 Pozo publico
6 Ojo de agua o manantial (protegido o no protegido)
7 Rio o quebrada
8 Camion pipa
9 Agua de lluvia
10 Otro _____

5 Codigo(s) de la preuba hecho en la fuente(s) del agua de tomar de este hogar

Para hogares que utilizan caneria, anotar ID de la prueba hecha mas cerca del hogar.

ID de prueba _____

ID de prueba _____

Annex 6: Estimated Sample Size Required to Measure Diarrhea Reductions

As described in Donner et al. (1981) and Diggle et al. (1994), the size of the sample necessary for a community-based trial designed to measure change in a dichotomous variable can be calculated using Equation [1]:

$$n = \frac{d}{r}\left\{\frac{z_{\alpha/2}\sqrt{\frac{\pi(1-\pi)}{a(1-a)}} + z_{\beta}\sqrt{\frac{\pi_1(1-\pi_1)}{a} + \frac{\pi_2(1-\pi_2)}{(1-a)}}}{\pi_1 - \pi_2}\right\}^2 \qquad [1]$$

where

- d (= 1+ ρ*(m − 1)) is the design effect due to intracluster correlation (ICC), where ρ is the ICC of subjects belonging to the same group, and m is the average number of subjects per cluster.
- π2 (also equals p) indicates the approximate proportion of positive outcomes in the control group or the baseline prevalence rate.
- π1 (= p + e) indicates the approximate proportion of positive outcomes in the treatment group, where e is the effect size or the expected change in positive outcomes due to the intervention.
- a represents the proportion of the total sample that will be allocated to the intervention group;
- π = a*π1 + (1 − a)*π2;
- α is the significance level to be used in the statistical tests;
- 1 − β is the power of the study; and
- r is the response rate.

The d term in Equation [1] considers the effect of ICC on the sample size. ICC arises from design effects attributable to factors shared by observational units, thereby lowering the total amount of information available for statistical analysis and increasing the required sample size. By increasing the number of communities in the sample and reducing the number of households sampled per community (m), we can increase the amount of independent data. This will then lower the variance inflation factor, which is dependent on the ICC (ρ).

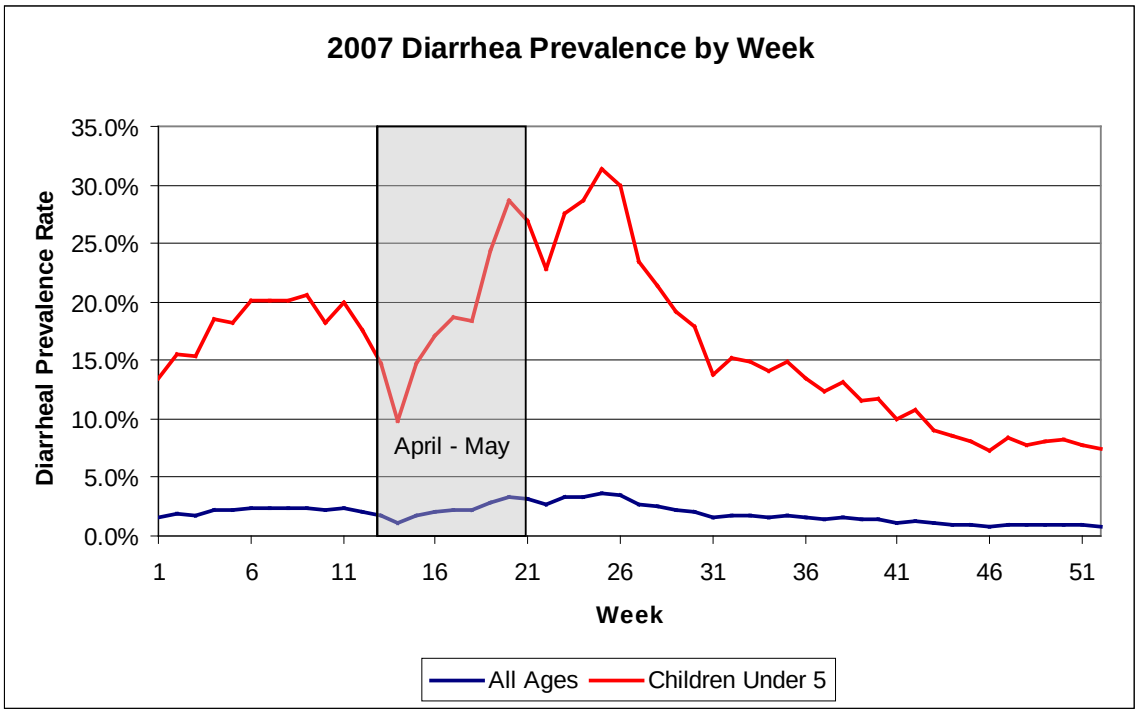
The table below summarizes the parameters that we use to estimate the sample size required to detect decreases in diarrhea prevalence. An estimate of the ICC is obtained from a review of the published literature. Katz and colleagues (Katz et al. 1993) examined the clustering of diarrhea rates at the village-level in several developing countries and provide measures of the design effect (d). With cluster sizes standardized to 50 households the design effect ranged from 1.38 to 4.73; employing the formula for d above, we calculated ICCs that ranged from 0.008 to 0.076. Hence, for our sample size calculations, we employed an ICC of 0.05, a conservative estimate within the range indicated by Katz et al.

Parameters used to estimate sample size required to detect decreases in diarrhea prevalence

Parameter	Value	Source
ρ	0.025	Katz et al. 1993
$\pi 2$ (also equals *p)*	19.3% diarrhea prevalence in children under 5 in April-May 2007	Vigilancia Epidemiológica diaria de SIBASI 2007 Census data
Effect size	30%	Fewtrell et al. 2005
a	50%	Assumption
α	90%	Assumption
β	80%	Assumption

The baseline diarrhea prevalence rate was calculated using data on diarrhea cases reported in 2007 to the Ministry of Health's Epidemiological Surveillance program and 2007 population from the 2007 Census. The baseline prevalence rate for municipalities that were eligible for the program was 4%, and the figure below shows how the prevalence in these municipalities varied by week in 2007. These prevalence rates are averages for all age groups, whereas the diarrhea prevalence will be higher among children under five, who bear the majority of the disease burden from inadequate water, sanitation, and hygiene (Prüss et al., 2002). The Ministry of Health's diarrhea data is not available by age group, so we estimate the diarrhea prevalence rate in children under five. Dr. Liliana Cruz indicated that approximately 85% of reported diarrhea is experienced in children under five (and even a higher percentage during peak periods). The 2007 census indicates that children under five make up about 11% of the population in the areas where we will be working. We estimate 2007 diarrhea prevalence in children under five to be 28%. The average diarrhea prevalence for April – May (the shaded area on the figure), the expected months for the baseline and endline surveys, was 2.3% for all ages and 19.3% for children under five. Since data from the Ministry of Health shows that diarrhea prevalence has been dropping over the past several years, we assume that the baseline prevalence rate will be 10% lower than these estimates (17.4%).

2007 Diarrhea Prevalence by Week in Municipalities Eligible to Participate in the Program



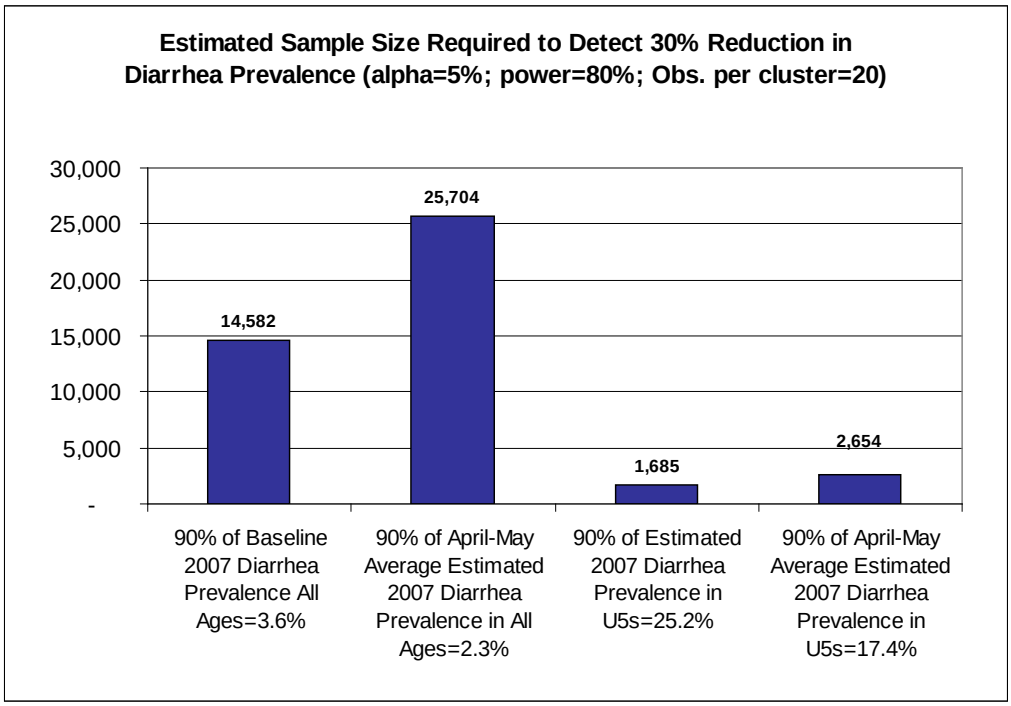
Source: Ministry of Health and 2007 Population Census

Notes: Prevalence for children under 5 is based on the assumption that children make up 11% of the population and that 85% of reported cases occur in children under 5.

As the MCC/FOMILENIO program does not specific a predicted reduction in diarrhea prevalence, we rely on a recent meta-analysis for the expected effect size. Fewtrell et al. (2005) finds that the impact of W&S interventions ranges from an approximately 15% to 50% reduction in diarrhea morbidity. Based on these findings, we assume a 30% effect size in our sample size calculations.

The next figure shows the advantages of measuring changes in diarrhea prevalence among children under five, as opposed to in the whole population. If the survey is conducted in April and May, the sample would require about 26,000 adults to detect a 30% reduction in the all-age diarrhea prevalence rate. A sample one-tenth that size, about 2,700 children under five, is required to detect a 30% reduction in the under five diarrhea prevalence rate. Thus, we suggest measuring diarrhea in children under five.

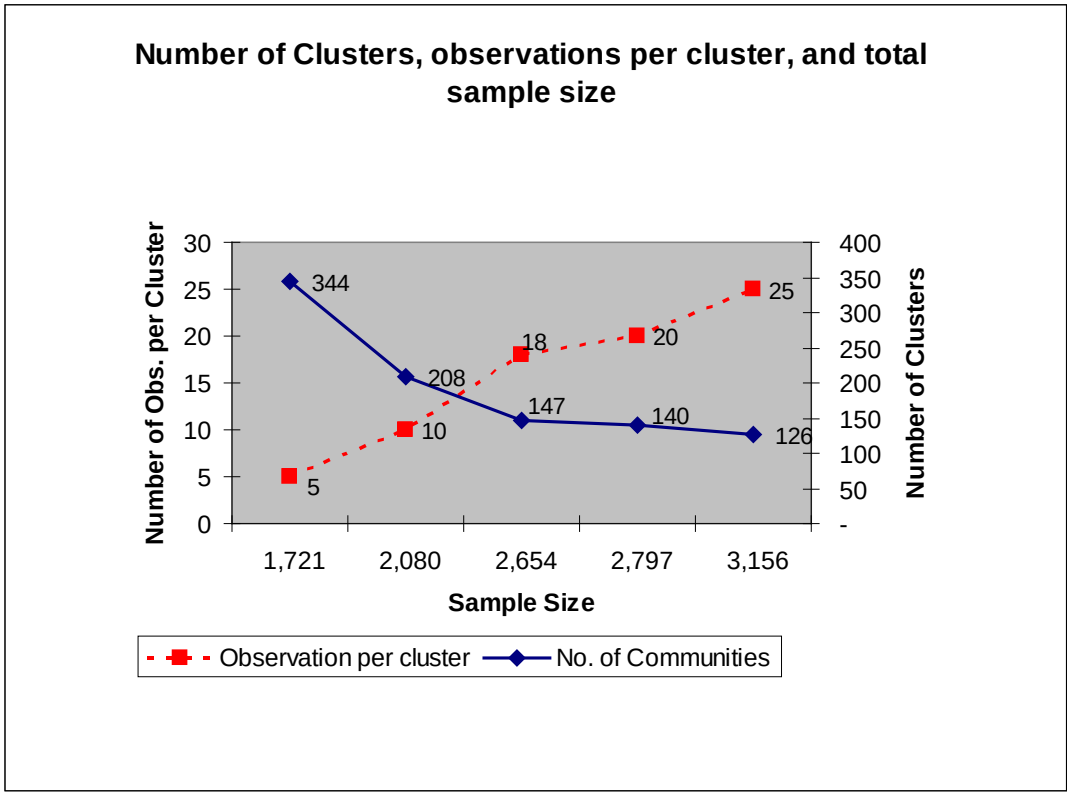
Estimated Sample Sizes required to Detect 30% Reduction in All-Age Diarrhea Prevalence and Under five Diarrhea Prevalence



Based on these data, we estimate that a sample of between about 1,700 to about 3,200 will be necessary to detect an intervention impact of 30% in a population with a baseline diarrhea prevalence of 17.4%[21], α equal to 90%, and β equal to 80%, which is indicated by a power calculation based on Equation [1]. This range reflects the fact that the sampling procedures must balance the number of clusters and the number of observations per cluster. Consideration must be given to statistical, logistical, and financial matters in choosing how many clusters to include in the study. Statistically, it is usually advantageous to maximize the number of clusters as power is most directly affected by this number. At the same time, since we are going to compare community-level diarrhea rates in treatment and control communities, a sufficient number of observations per cluster must be interviewed to ensure a representative sample is included for each cluster. Logistically and financially, it is usually more difficult and more expensive to recruit and survey more clusters than it is to interview more observations per cluster. The final figure shows the tradeoffs between the number of clusters and the number of observations in each cluster.

[21] This is the 90% of the 2007 diarrhea prevalence during the same period. As noted above, we use this level to reflect the fact that diarrhea rates have been declining in El Salvador for the past several years.

Trade-offs between Number of Clusters and Observations: Diarrhea Prevalence



Note: Estimate excluding contingencies.

Though diarrheal illness occurs primarily in children under 5, the longitudinal nature of this study requires that we sample households with children under 3 for the baseline survey so that our subjects remain under the age of 5 throughout the study. The 2007 household survey indicates that 22% of rural households have children under 3 years of age. Assuming that 80% of these households will be willing and able to participate in the baseline survey, each census segment, comprised of an average of 100 households, will have at most 18 eligible households. Thus, we use this as our maximum number of observations per cluster. At this level, the sample size required would be 2,654 observations (households with children under 3) from 148 clusters (74 treatment and 74 control).

Annex 7: Matching equation and results

Logistic regression for propensity score

```
Logistic regression                               Number of obs   =       1059
                                                  LR chi2(17)     =     136.34
                                                  Prob > chi2     =     0.0000
Log likelihood = -262.94233                       Pseudo R2       =     0.2059
```

treatment	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
Municip. population	-.0074157	.001615	-4.59	0.000	-.010581	-.0042503
Housing density	-.0016227	.0006476	-2.51	0.012	-.002892	-.0003534
Water area	.5318508	.3685632	1.44	0.149	-.1905198	1.254221
Average temp	-.1255031	.0588752	-2.13	0.033	-.2408964	-.0101098
Hh size	-1.029235	.278854	-3.69	0.000	-1.575779	-.4826911
% using unimproved w	1.60186	.6807669	2.35	0.019	.2675811	2.936138
% using Privatewell	2.3881	.6675804	3.58	0.000	1.079667	3.696534
% priv.piped,<7 days	1.040513	.473252	2.20	0.028	.1129565	1.96807
% no sanitation	.2329439	.8908739	0.26	0.794	-1.513137	1.979025
% comp. latrine	2.113224	.5830317	3.62	0.000	.970503	3.255945
% inhome business	-2.976308	1.454906	-2.05	0.041	-5.827871	-.1247449
Avg num emigrated	.1843335	.4095966	0.45	0.653	-.6184611	.9871281
Depid_1	-.8155275	.7607417	-1.07	0.284	-2.306554	.6754989
Depid_2	-.4854352	.3483113	-1.39	0.163	-1.168113	.1972424
Depid_7	-2.108303	.8104319	-2.60	0.009	-3.69672	-.5198856
Depid_8	-1.165856	.4104193	-2.84	0.005	-1.970263	-.3614492
Depid_9	-1.511512	.5287463	-2.86	0.004	-2.547836	-.4751881
_cons	6.041641	2.236102	2.70	0.007	1.658962	10.42432

Matched and unmatched samples – variables in and not in logistic regression equation

Variable	Sample	Mean Treated	Mean Control	%bias	%reduct |bias|	t-test t	t-test p>|t|
Dep 1	Unmatched	.03	.0182	7.7		0.82	0.411
	Matched	.03	.02	6.5	15.3	0.45	0.653
Dept 2	Unmatched	.46	.2848	36.7		3.68	0.000
	Matched	.46	.49	-6.3	82.9	-0.42	0.673
Dept 7	Unmatched	.02	.08917	-30.8		-2.40	0.017
	Matched	.02	.03	-4.4	85.5	-0.45	0.653
Dept 8	Unmatched	.19	.18471	1.4		0.13	0.896
	Matched	.19	.15	10.2	-656.6	0.75	0.454
Dept 9	Unmatched	.09	.13558	-14.4		-1.29	0.197
	Matched	.09	.08	3.2	78.1	0.25	0.801
Farmer	Unmatched	.41252	.37961	12.5		1.13	0.258
	Matched	.41252	.39068	8.3	33.6	0.60	0.550
% ag land	Unmatched	.47623	.4332	15.3		1.39	0.165
	Matched	.47623	.44382	11.6	24.7	0.84	0.402
animalhusb~y	Unmatched	.20876	.19981	3.8		0.37	0.714
	Matched	.20876	.19014	8.0	-108.1	0.58	0.565
inhomebusi~s	Unmatched	.07361	.09359	-22.8		-1.84	0.067
	Matched	.07361	.07853	-5.6	75.4	-0.40	0.688
mun_pop_2005	Unmatched	10202	19561	-67.4		-5.01	0.000
	Matched	10202	10654	-3.3	95.2	-0.46	0.646
Electricity	Unmatched	.78027	.77418	3.1		0.26	0.792
	Matched	.78027	.80324	-11.5	-276.9	-0.90	0.371

Variable	Sample	Mean Treated	Mean Control	%bias	%reduct |bias|	t-test t	t-test p>|t|
Housing dens	Unmatched	82.858	286.92	-46.4		-3.36	0.001
	Matched	82.858	87.029	-0.9	98.0	-0.20	0.841
Land	Unmatched	5.4842	3.8126	35.1		4.02	0.000
	Matched	5.4842	4.173	27.6	21.6	2.02	0.045
temp_avg	Unmatched	24.768	24.681	3.9		0.41	0.684
	Matched	24.768	25.011	-10.9	-178.9	-0.74	0.462
precip_avg	Unmatched	1872.2	1912.1	-20.6		-2.22	0.027
	Matched	1872.2	1859.6	6.5	68.4	0.44	0.657
hhsize	Unmatched	4.3365	4.5066	-29.9		-2.73	0.006
	Matched	4.3365	4.2993	6.5	78.2	0.52	0.600
nosanitation	Unmatched	.19104	.20639	-8.5		-0.72	0.470
	Matched	.19104	.16712	13.2	-55.9	1.06	0.289
Unimproved w	Unmatched	.16159	.13817	10.8		1.04	0.299
	Matched	.16159	.13716	11.3	-4.3	0.75	0.456
privatewell	Unmatched	.20433	.11542	45.3		4.65	0.000
	Matched	.20433	.19281	5.9	87.0	0.33	0.742
pipedwater	Unmatched	.5496	.65584	-33.9		-3.17	0.002
	Matched	.5496	.59564	-14.7	56.7	-0.98	0.327
Privpip 7days	Unmatched	.49039	.59115	-32.1		-2.95	0.003
	Matched	.49039	.54084	-16.1	49.9	-1.11	0.269
privatepip~y	Unmatched	.34941	.32706	7.1		0.65	0.513
	Matched	.34941	.37308	-7.5	-5.9	-0.51	0.610
nosanitation	Unmatched	.19104	.20639	-8.5		-0.72	0.470
	Matched	.19104	.16712	13.2	-55.9	1.06	0.289

```
                        |                                  |
Compost latr  Unmatched | .28526   .14464     57.8         |   6.02  0.000
                Matched | .28526   .31784    -13.4    76.8 |  -0.84  0.404
                        |                                  |
       sewer  Unmatched |  .0258   .08953    -39.5         |  -2.95  0.003
                Matched |  .0258   .01743      5.2    86.9 |   0.97  0.334
                        |                                  |
   dirtfloor  Unmatched | .36412   .37339     -4.3         |  -0.38  0.703
                Matched | .36412   .34563      8.6   -99.4 |   0.68  0.495
                        |                                  |
-------------------------------------------------------------------------------


-------------------------------------------------------------
         Summary of the distribution of the abs(bias)
-------------------------------------------------------------
                       BEFORE MATCHING
-------------------------------------------------------------
Mean           22.91773
Std. Dev.      17.97961
Variance       323.2665
Skewness        .745275
Kurtosis       2.711828
-------------------------------------------------------------
                       AFTER MATCHING
-------------------------------------------------------------
Mean           9.026587
Std. Dev.      5.382653
Variance       28.97295
Skewness       1.452191
99%     27.55772       27.55772       Kurtosis       6.238083
-------------------------------------------------------------


---------------------------------------------------------------
     Sample |    Pseudo R2      LR chi2        p>chi2
------------+--------------------------------------------------
  Unmatched |        0.187       128.80         0.000
    Matched |        0.057        15.80         0.941
---------------------------------------------------------------
```