Microfinance and Home Improvement: Using Cross-Sectional Surveys to Measure Program Impact on Qualitative Events

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Abstract: From a one-time cross-sectional survey, we create a backcast panel data set based on the yearly timing of discrete upgrades in dwellings in the history of 218 Guatemalan households with access to microfinance. We used fixed effects estimation to analyze the timing of dwelling improvements such as the construction of cement block walls to replace adobe walls, cement floors to replace dirt floors, tiled roofs to replace corrugated iron roofs, and new purchases of land, relative to the timing of microfinance borrowing and the inflow of foreign remittances. We find that microfinance and remittances "Granger cause" modest increases in the probability of dwelling upgrades, and that the magnitudes of these changes in the probability of home improvement are smaller than those realized after the inflow of foreign remittances. We develop a test for possible endogeneity in the timing of borrowing decisions based on using the geographical rollout of the credit program as a source of exogenous variation.

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1. Introduction

There has been much written recently by development economists about the need for rigorous and systematic appraisal of the effectiveness of anti-poverty programs in developing countries (for example, Armendáriz de Aghion and Morduch, 2005; Easterly, 2006). Yet researchers and practitioners seeking to ascertain the true impact of development programs face a daunting task. Accurately measuring program impacts has historically been both time-consuming and costly, especially for small institutions that seek accurate measures of their effectiveness. Moreover, many institutions would like to evaluate the effectiveness of their programs *ex-post* to implementation, creating problems with the establishment of baseline surveys, control groups, and other means of identification. These obstacles have resulted in a dearth of rigorous analysis in trying to obtain bona fide impacts of many types of development programs, including the most popular and widespread today, microfinance.

In this paper we present a methodology for ascertaining welfare changes brought about by development programs that may be applicable in a variety of contexts. The identification in this methodology comes from differences in the time at which households gain access to a treatment, and so relies on this rollout process being unrelated to the variables being used to measure impact. In many cases the methodology may be able to yield insights into changes associated with the implementation of programs based on merely a single cross-sectional survey of program participants, *ex-post* to program implementation.
Our methodology uses a single cross-sectional survey to create a retrospective panel data set (or "backcast panel") based on discrete, memorable events in the history of households. Analyzing the timing of these events with respect to the timing of treatment allows for an assessment of changes in important welfare variables within a window surrounding the treatment.

We apply this methodology to studying the effects of a microfinance program in rural Guatemala and compare these effects to those of remittances sent to the households by relatives working in the United States. We study discrete changes in the probability of major dwelling improvements, upgrades of walls, roofs, floors, the installation of indoor toilets, and the purchase of new land, using a linear probability estimator that incorporates household and year fixed-effects. Our results show that borrowing for enterprise expansion via microfinance exhibits modest positive effects on some dwelling upgrades, but the effects are small relative to the larger changes associated with remittances sent from abroad via immigration of a family member. Moreover, we find that the changes realized in dwellings after borrowing are significantly greater within the subset of households in our sample that send migrants to the United States, implying that development interventions such as microfinance exhibit significantly stronger impact on households that have a predilection for self-improvement based on other observable actions. We also find that it appears to be actual *borrowing* rather than simple *access* to credit that is associated with these changes; analyzing the timing of dwelling upgrades relative to introduction of the credit program in a household's village yields no significant results stemming from mere access to the program within the village. Furthermore, we are able to check for endogeneity in the timing of borrowing decisions using the geographical rollout of the microfinance program into new areas over time.

The estimation technique presented in this paper is quite general, and applicable to a wide variety of contexts. Furthermore, it offers some advantages relative to alternative approaches, though researchers must exercise care in both implementation and interpretation of results.

Historically, researchers have often sought to obtain the effects of development programs through waves of surveys taken over multiple years, using baseline surveys as a reference point for subsequent changes in household variables. Such methods can provide unbiased estimations of program impact when properly carried out in the presence of a well-chosen control group. However, this approach is typically both time-consuming and costly, and can delay obtaining valuable information about program effects for years after a study is initiated. Many institutions have not undertaken a baseline survey of treatment and control groups before program implementation, usually a prerequisite for identification using this methodology.

In other cases researchers have used instrumental variable techniques to try to identify impacts. By using a third variable that is correlated with program access, but uncorrelated with the dependent variables of interest, the use of instrumental variables overcomes problems of endogeneity to allow for theoretically unbiased estimates. The main difficulty with the use of instrumental variables is logistical; instruments, if they are available, differ from one situation to the next. Furthermore, finding instruments that are strongly correlated with program access in a particular context, but uncorrelated with impact variables, also requires substantial ingenuity, complicating the use of a standardized instrumental variable approach. In the context of microfinance, finding convincing instrumental variables for credit access or actual borrowing has often proved to be a frustrating exercise for researchers (Armendáriz de Aghion and Morduch, 2005). What is more, instruments vary in their strength of correlation with program access; weak instruments yield imprecise estimates of true impact magnitudes.

To overcome these problems, the use of randomized field experiments has become increasingly common in ascertaining the impacts of many types of poverty intervention programs (Duflo, 2006). Randomized field experiments have become increasingly popular because they allow for a maximum degree of exogeneity in treatment and control, allowing researchers to overcome the often-thorny issues of self-selection, endogeneity, and omitted variable bias. Yet despite the advantages of randomized field experiments, such studies face their own set of difficult issues. To create the control group needed for the identification of treatment impact, it is necessary that some who desire access to the treatment (such as microfinance, health, or education) remain untreated for a specified time so that impact can be measured on an equivalent treatment group relative to the control group. This difference in the timing of treatment is usually justified by a constraint on the institution's ability to treat all agents immediately, and in some cases a random lottery can be the fairest way to determine the queuing rule. Nonetheless randomized implementations are complex to execute, and particularly when we try to measure the Treatment Effect on the Treated of a voluntary program (and so must let the selection mechanism work before randomizing), can be logistically infeasible in some environments.

Because any synthetic research structure is difficult to maintain for a long period of time, randomized experimental studies tend to be of limited duration. This introduces several practical problems for the evaluation of interventions such as microfinance, which may take years to realize their full effects on the household. First, one can capture only those impacts which have transpired during the duration of the study. Second, the tendency for untreated units to "bleed" from the study by seeking treatment from other sources, lessens the difference between the treatment and control groups and hence the apparent impact of the treatment. Third, outcomes measured from short-term policy trials are subject to the influence of time-specific economic shocks occurring within the relatively narrow timeframe of the experiment. If these time-specific shocks, like the ups-and-downs of regional economic activity, are complementary to a treatment (as is typically the case with microfinance), the use of a narrow window in any type of study may yield imprecise (yet still unbiased) estimates of the magnitude of program impacts, and understate the standard errors of these estimates.

Lastly, because limited-duration field experiments represent a snapshot of program impact over a short time frame, they are unable to capture important dynamics of treatment

[12] See C.W. Granger (1969). A lagged set of variables of x is said to Granger-cause y if it can be shown that lagged values of x add statistically significant information on future values of y even after a lag structure for y is included in the regression.

impact. Ideally, both practitioners and researchers would like to understand how *long* a given impact takes to become fully realized within a subject population, and changes in the impact of the treatment over time. Movies contain more information than photographs.

In our methodology, we first carry out a household survey within a pool of microfinance borrowers that creates a backcast panel of discrete historical events regarding major dwelling changes. We combine this backcast panel with historical variation in the timing at which different households had access to the treatment in order to estimate impacts. If the program was introduced in a fashion orthogonal to the outcome variable, our technique measures causality in the standard sense and is analogous to measuring the Treatment Effect on the Treated under random rollout. If the sequence in which people had access to a program (credit) or took advantage of an opportunity (remittances) is endogenous, we can no longer infer standard causality from this study but we can still measure important information about the degree to which interventions tend to precede changes in outcomes. Similar approaches have been carried out, for example, in ascertaining the impact of microfinance on fertility decisions (Morduch, 2004).

Our cross-sectional survey is of households from a series of villages surrounding Quetzaltenango, Guatemala who began participation in the microfinance program in at different times, beginning in the early 1990s until a year before the survey in 2005. Ideally one would like to obtain a random sample of program participants starting after a specified time in order to mitigate problems of "attrition bias," in which long-term participants may show treatment effects than the average participant (see Karlan, 2002). However, as we show in this paper, even when only current program participants are surveyed, it is relatively straightforward to check, and even account for, attrition bias in the impact estimations. Carrying out Chow tests for significant differences between old and new borrowers in the portfolio, we find none in our estimations.

The cross-sectional survey in our technique is oriented around discerning the timing of "unforgettable" events in the history of a household. For example, a study on the impact of a pre-natal health program on miscarriage and infant mortality could accurately collect recall data on miscarriages, births, and deaths of children, which are "unforgettable" events to any mother, but probably not on minor childhood illnesses. In our application to microfinance, we focus on the timing of "unforgettable" upgrades in dwelling structure such as the upgrade of a home's walls from adobe to cement or a home's floor from dirt to cement or tile. However, historical questions on changes in revenues and profits in an informal sector enterprise, and perhaps even changes in the number of employees or capital goods would likely be infeasible since their timing and precise quantities may be difficult for subjects to recall. Thus our technique can be used only with discrete and psychologically significant dependent variables. In our study, we took care to ascertain the timing of these qualitative events by referencing them off the ages of children, and other key events in the life of the household and village. From this data, we create a history for each household consisting of these discrete dwelling changes over time along with the timing of borrowing as well as initial credit access back to the time of occupation of the dwelling unit. The sum of these recreated household histories across households forms an (unbalanced) panel data set from which estimations are carried out.

Along with its ability to be implemented ex-post to program implementation, another advantage of this methodology is that it allows for an estimation of possible treatment effects without the use of an overt control group. In a statistical sense, the differential timing of program participation allows households in the sample who access the program at different times to act as mutual controls. The idiosyncratic influences of the regional economy over different years are controlled for through time fixed-effects. The idiosyncratic differences between households are dealt with through household-level fixed effects. Identification is carried out through noting changes in the estimated probability of an event around the time window of treatment. If the likelihood of a given type of discrete event consistently increases in the several years *after* treatment (and not before), given that treatments occur in different years, we can ascribe causality (in the Granger sense) stemming from the treatment.[2]

Identification of impacts is achieved through the existence of a "counterfactual," *i.e.* what *would* have happened in the absence of a particular treatment. The counterfactual in a randomized experiment is formed by comparing changes in the outcomes observed among those who received the treatment to an equivalent group that did not receive it. In contrast, the counterfactual in our methodology is identified by differences in the timing of receiving treatment, and so we use changes among those who have not yet (but will) receive credit as counterfactuals for those receiving credit at a moment in time. We include individual- and time-level fixed effects in the estimation. From a practitioner standpoint, it is extremely attractive to be able to form counterfactuals strictly within a program's own client base.

The practicalities of this general approach must be balanced by an important caveat, which is that, a set of exogeneity assumptions must be made on the sequencing of rollout and the sequencing of uptake. When exogeneity fails, we can only say that the intervention *precedes* the outcome (Granger causality) and not that it *causes* it. Yet even in cases when the decision may be endogenous, although we cannot infer standard causality though there may still be important information present in this relationship. In such a case, one cannot make the theoretical prediction that every household who receives a given treatment will observe the estimated impact (which would constitute a *sufficient* condition for realizing the impact). However, one may still be able to argue that the impact is not observed in households that have not undertaken the action, and so that the treatment is at least a *necessary* condition for the impact.

The greater the degree to which explanatory variables (in our example, microfinance borrowing or the receipt of remittances) are exogenous to the impact variables under study (the decision to undertake dwelling upgrades), the more one can ascribe the words "impact" or traditional "causality" to post- vs. pre-treatment differences.

Here, we attempt not to use words such as "impact" or "causality" lightly, and there is a substantial role that theory must play in the interpretation of results. However, what allows us to identify impact with more precision in our particular case is the existence of a sub-sample of approximately 83 of our 218 borrowers who obtained credit shortly after the introduction of the credit program in their village. The fact that these borrowers did not have access to the credit institution before they took their first microfinance loan helps us to statistically identify a counterfactual regarding the probability of major dwelling upgrades in the years before credit access when households *did not have access* to the credit institution.

By utilizing the timing of village-level program access, we eliminate the endogeneity of the timing of individual decisions, and now obtain identification off of the rollout process itself. Under the assumption that this institutional rollout is exogenous to housing upgrade decisions, we can infer standard causality from this relationship if borrowing consistently precedes increases in the probability of these upgrades. Thus to the extent that the treatment is both exogenous to individual decisions and orthogonal to unobserved determinants of timing, such as with the rollout of the credit program into different villages, then this relationship would imply causality in the standard sense rather than just the Granger sense.

All of this being said, we find no statistically significant difference in the probability of pre-borrowing dwelling upgrades between borrowers with and without program-based credit access in the four years before they took their first microfinance loan.

For this reason, our estimated pre- and post- treatment parameters are likely to be reliable reflections of the impact of microfinance borrowing on dwelling changes, though we cannot move beyond claims of Granger causality in our analysis of the changes in dwellings occurring after remittances.

The next section provides a brief review of other impact studies of microfinance and remittances on dwelling changes. Section 3 considers our field research context, methodology, and econometric model. Section 4 presents our results, and Section 5 concludes with suggestions for applications and a discussion about the appropriateness of our approach to other contexts.

2. Studies on Microfinance and Remittance Impacts

Research that has attempted to ascertain the impact of microfinance has taken a number of different approaches. The first approach is often undertaken by field researchers who seek to measure the impact of microfinance by comparing the performance of microfinance borrowers to a random sample of non-borrowers. There is one particularly large problem with this naïve approach to impact estimation: Borrowers are typically a self-selected group who are likely to possess characteristics that differ from the population norm. For example, entrepreneurial drive is likely to be much stronger among those seeking microfinance loans than a typical entrepreneur. As a result, problems with omitted variable bias are likely to cause an overestimation of treatment effects from microfinance.

One method of overcoming such problems has been to compare new borrowers to old borrowers. This has been the approach undertaken by some development researchers, such as carried out by USAID in its AIMS research project. But as Karlan (2001) and Karlan and Alexander-Tedeschi (2006) point out, this kind of approach can lead to an "attrition bias" in which the performance of old borrowers may exceed those of new borrowers because of a hidden qualities in old borrowers that have allowed them to remain in the program. Only a subset of new borrowers is likely to share these qualities, and hence the impacts observed by a researcher will be biased by this unobserved difference.

Other studies have undertaken a "quasi-experimental" approach to ascertaining microfinance program impact, which is related to the use of instrumental variables techniques. The most well-known the work of this kind uses the fact that programs are often implemented in a staggered fashion, or utilize participation rules that can be exploited by researchers to analyze program impact. Wydick (1999), for example, uses the staggered nature of the introduction of lending in different areas to help identify the degree of credit access granted to Guatemalan borrowers in estimating the effects of microfinance on child labor. Using the staggered entry of a credit institution into different areas along with gross sales as instrumental variables for quantity of borrowing, credit effects on school enrollment can be obtained.

Coleman (1999) is able to obtain a measure of microfinance impact in 14 villages in Thailand by using borrowers who would receive microfinance loans in the future as a control group for borrowers that were actually granted credit access. By including a dummy variable for credit participation by both those that seek credit in the control villages and those with access to credit in the treatment villages, he controls for self-selection issues. Using this methodology, Coleman finds the impact of microfinance to be small, yet cautions that the impact may be diluted in his study based on the relatively high degree of wealth and widespread credit access of the borrowers throughout his sample population.

Pitt and Khandker's (1998) study examines the impact of microfinance among a population of households who were located in areas served by the three largest microfinance institutions in Bangladesh, the Grameen Bank, RD-12, and BRAC. They exploit the program participation rules of the microlenders, which limit participation to poor households who owned less than 0.5 acres of land. Identification of impact from their study comes from looking at changes in consumption and other variables by borrowers marginally on either side of this participation rule. They find that consumption by households increased when loans were granted to women by about 18% of the amount borrowed.

The third approach is the use of randomized field experiments to measure microfinance impact. There is been little published work in this area to this point, partly because of the difficulty associated with denying credit to borrowers who are intent on seeking it, as well as ethical concerns with holding off the poor to a potentially welfare-improving treatment. New work in this area is being undertaken in Hyderabad, India by Duflo and Banerjee, who have worked with a microfinance program to provide credit access to a randomly selected group of neighborhoods out of a total of 120 chosen for the study. The purpose of the study is to compare treatment neighborhoods with non--treated neighborhoods after one year of credit access. Results are still pending in their research.

There is evidence suggesting that microfinance is associated with improvements in housing. Halder and Husain (1998), for example, find that borrowers with the BRAC institution in Bangladesh with loans given specifically for housing, used loans to improve their housing condition relative to a control group. The authors note that in home values and per capita floor space, performance of BRAC members was significantly better. Differences were particularly strong among younger borrowers between BRAC members and the control group in terms of value of living houses and per capita living space. However, these loans were given for the specific purpose of housing improvement, while housing improvements in our study should ostensibly be the result of increased profits from loans granted for enterprise expansion.

It is likely, however, that increased profits from microloans should result in housing changes since for a long time economists have understood that housing differs from other goods in that it not only represents a consumption good, but also a major store of wealth and a measure of prestige. This is no less true in developing countries. Tax (1953), for example, observed in Guatemala that social status among rural Mayans is often reflected in the quality and size of homes and land. For this reason, improvements in houses and land are typically among the first changes rural households make when family income begins to increase. In rural Guatemala this is particularly important, because in rural areas homes are rarely bought and sold, but rather are inherited by offspring who continue to reside on the same plot of land.

It is also possible that microloans intended for enterprise capitalization may be diverted into use for welling improvements. An anonymous Bolivian MFI estimates that 20 percent of its "microenterprise" loans go for home construction and expansion.[3] As a response to this phenomenon, some MFIs are now interested in developing a new line of micro-credit specifically to finance housing (Ferguson, 2004).[4] Nevertheless, some research has pointed out that investing in dwellings may not necessarily represent a complete diversion of credit, since such improvements increase the income-generating potential of home-based activities (Harvard University CUDS, 2000).

But the lack of financing specifically available for housing has created a tendency, particularly noted in Latin America, for the process of housing improvement to be very slow. Houses in Latin America often take 5 to 15 years to be completed, appearing perennially unfinished to the foreign eye. Much of this delay is caused by lack of income generation and credit constraints. Because of the length of time used in housing construction, the dynamics of microfinance impact are particularly important with housing. In a study such as Pitt and Khandker (1998) that focuses on increases in consumption,

[3] The Center for Urban Development Studies Harvard University Graduate School of Design (2000). Housing Microfinance Alternatives, Synthesis and Regional Summary: Asia, Latin America and Sub-Saharan Africa.
[4] In Guatemala for example, Genesis Empresarial, a Guatemala City-based MFI, has a small portfolio of borrowers with home improvement loan products that carry average terms of two years.

changes to consumption variables may appear relatively soon after credit access. Consequently, consumption impacts may be obtainable more easily with instrumental variable techniques and randomized field experiments that analyze loan impact only over a short window relative to a control group. With dwelling changes, the length of time a household has participated in a microfinance program is much more likely to matter, a dynamic which our study attempts to capture.

Immigrants and Remittances in Central America

Anecdotal evidence has suggested that remittances play an equal or greater role in housing improvements than microfinance in Guatemala, and we seek to measure the effects of microfinance borrowing relative to those from foreign remittances. The U.S. Census (2000) estimates that there are 480,665 Guatemalans legally living in the United States. This ranks second among Central American countries behind El Salvador (817,336) and ahead of Honduras (282,852). The number of illegal immigrants is thought to be perhaps at least one third higher than these numbers. A great number of immigrants retain families back in their home country, sending them cash earned from legal or illegal work in the U.S. Studies such as Woodruff and Zenteno (2001) indicate a strong relationship between reception of remittances and poverty alleviation. In almost every case where households had begun to receive remittances in our survey, male migrants had settled in the United States, with the majority working in construction, landscaping, or in restaurants in Los Angeles, Houston, or Miami.

Adams (2004) with a large data set from a national representative survey in Guatemala, finds that households receiving remittances have significantly higher income than those not receiving remittances. His study also shows that 25% of households in Guatemala receive remittances, a very similar figure to what was found in our sample. With only one exception, he finds that both internal and international remittances reduce the level, depth, and severity of poverty in Guatemala. However, he finds that remittances have a greater effect on reducing the severity as opposed to the level of poverty in Guatemala. Adams finds that the squared poverty gap, measuring the severity of poverty, decreases 21.1% with domestic remittances and by 19.8% when international remittances are included in such income. He finds that households in the bottom deciles of poverty receive between 50 and 60 percent of their total income from remittances, thus making up an enormous share of their income. The effects of foreign remittances are even evident at the macro level; Rapoport and Docquier (2005) found foreign remittances to have "an overall positive effect on an origin country's long-run economic performance." Our study seeks to ascertain the micro-level welfare changes associated with microfinance borrowing in comparison with the changes that occur after access to remittance flows within our sample of the Guatemalan households.

3. Methodology

The context for our field survey was rural western Guatemala in several villages surrounding the cities of Quetzaltenango and Mazaltenango. In Guatemala, the majority of the population lives in rural areas, a relatively high figure even by Latin American standards. Virtually all of those in our survey were Mayan Indian households living in subsistence agriculture on plots of land in which the household grows corn, beans, coffee, and sometimes plots of vegetables.

Our empirical estimations are taken from data collected during the summer of 2005 in Guatemala, during a survey of 218 rural households located in 14 different villages. The sample selection was coordinated with the help of *Fe y Alegria* (trans. *Faith and Joy*), a medium-sized Jesuit-run microfinance institution in Guatemala that has operated since 1993, who grants microloans to around 3000 clients per year. For the purpose of this study, borrowers were selected from two major regions serviced by *Fe y Alegria*, the predominantly rural regions around the city of Quetzaltenango and in and around Mazatenango. Quetzaltenango is part of the western highlands, with villages ranging between 7000 and 8500 feet above sea level, where nights are cold and daytime temperatures rarely exceed 85 degrees Fahrenheit. Mazatenango lies near the coast with a warmer and more humid climate. The sample was taken from a list of current borrowers of *Fe y Alegria* in both regions. All borrowers were engaged in microenterprise activity, including tailoring, furniture, and other light manufacturing, while others where commercial venders in markets and small shop owners.

The purpose of these microloans, as specified by *Fe y Alegria*, is to help micro-entrepreneurs acquire working capital, fixed assets, and microenterprise infrastructure. In other, words loans are not intended directly for new home improvements. Clearly, one of the goals of such loans is that increased profits from new borrowing result in these and other positive changes that improve the welfare of households.

The questionnaire was intended to measure changes in our different categories of dwelling improvement: upgrades to walls, roofs, floors, plumbing, and increases in land. Each borrower was asked about changes in these variables during the history of the household, and the timing of these changes. For example, we asked households how long they had lived in that specified location. If a household had cement walls, we asked them if a different kind of wall structure existed since they had lived in that location. If prior to the cement walls, the house had had adobe walls, we asked what year the upgrade had taken place. We tried to pin down the exact year carefully by referencing the relative ages of children at the time of the change and by referencing changes to important local events. In like manner we constructed a time series of changes in each dwelling category since the time the borrower lived in the given location.

Clearly, a substantive concern with this kind of survey method is the problem of inaccuracy in recall data. Our survey method seeks to mitigate this problem by asking subjects only to recall discrete, major changes in the history of the home. Because, for example, the upgrade of floors from dirt to cement poses such a major augmentation in quality of living standards for a family, there was relatively little problem with the recall of such events and their timing by year.

From the survey we then create an unbalanced panel data set. The unbalanced nature of the panel data arises because the model considers the number of years the head of household or borrower has been living in the present site as the defining number of years used in the time series for each household. Our estimations were carried out on data beginning in 1990, but some households had resided in a particular locale only after 1990.

Estimation Technique

A variety of estimators have been used to estimate the probability of right-hand-side independent variables on a binomial dependent variable. Probit and logit models are now most commonly used today for such estimations, but in panel data estimations, the linear probability model has become increasingly used, since as a linear estimator it produces more robust estimates when implemented with fixed-effects estimations on panel data (Chamberlain, 1980). Our model first estimates the probability of one of our households upgrading from a low quality material to a high quality material in the structure of the house. For walls this is from either adobe to finished adobe, or adobe (finished or not) or wood to cement. For roofs this is from either palm leaves or corrugated iron to either cement or tile. For floor upgrades, the changes we analyze are from dirt to cement, cement to tile, or dirt directly to tile. With changes in toilet, our upgrade is from an outhouse to indoor plumbing. Lastly, we consider increases in landholdings based on land purchases in *cuerdas* (approximately 25 square meters). Estimations are conditional, of course, upon a household not previously having made the particular type of dwelling upgrade.

The two-way fixed-effects model we estimate is the following:

$$y_{it} = h_i + \alpha_t + \sum_{i,t-\bar{t}}^{t-\bar{t}=k} \tau_{i,t-\bar{t}} T_{i,t-\bar{t}} + u_{it}\,, \qquad (1)$$

where y_{it} is a bivariate dependent variable that is equal to 1 if household i upgrades walls in year t. (And similarly for separate estimations on roof, floor, toilet, and land.) For the independent variables, h_i is a household-level fixed effect, α_t is a year-level fixed effect, and u_{it} is an error term. The third term is an estimation on a sequence of treatment dummy variables, $T_{i,t-\bar{t}}$, that encompasses a "treatment window" of length n years representing a sequence of lags and leads surrounding year t for household i. The treatment dummy variable is equal to 1 if household i first received a microfinance loan (or began receiving remittances) $t-\bar{t}$ periods "ago," and zero otherwise. If $t-\bar{t}$ is negative, it means that household i receives credit $t-\bar{t}$ years *forward* from time t. For a symmetric treatment window of width n around the time of treatment, then the summation in the third term of the model includes $k = (n-1)/2$ years of leading treatment dummies, $k = (n-1)/2$ years of lagged treatment dummies, as well as the contemporaneous dummy for when $t = \bar{t}$, for the year in which the household first received microfinance. For example, consider a treatment window of $n = 5$ for a household i who initially received microfinance in 2001. For the observation of household i in the year 2000, the data in the backcast panel then contains a vector of treatment dummy variables--0, 1, 0, 0, 0--which correspond to estimated coefficients $\tau_{i,-2}, \tau_{i,-1,} \tau_{i,0,} \tau_{i,+1,} \tau_{i,+2}$. For the observation of household i in the year 2003, the vector of dummies would be 0, 0, 0, 0, 1.

From this estimation we are able to identify whether microfinance borrowing Granger causes dwelling upgrades. We follow the well-known Sims (1972) test for Granger causality which says that the prediction of dwelling upgrades from past lagged treatments should not be improved if variables representing treatment in years ahead are included in the estimation. In other words, if lagged years of treatment significantly help to predict a current-year dwelling upgrade, while variables representing treatment in future years are jointly insignificant (based on an *F*-test), then the treatment "Granger causes" a particular type of dwelling upgrade.

We also obtained data on gender, education, and other time-invariant individual-level variables, but these drop out of a fixed-effects estimation and so we cannot report the effects of these on dwelling changes. Only 28% of the borrowers in the sample attended secondary school, and borrowers have an average of 4.2 children. Their average age is approximately 39 years old, and in our sample 65% are female borrowers. Almost exactly 50% of borrowers referred to themselves as Evangelicals, while the other 50% identify themselves as Catholic.

Many upgrade homes took place during the surveyed history of our households, our key set of dependent variables. At the time the current borrower began residing in the household, 109 of our houses had been constructed with the inferior wood or adobe walls (86 adobe, 23 wood). During the history of the household, 61 of these houses had upgraded to cement block. Similarly, 193 of the houses initially had either dirt or cement floors (97 dirt, 96 cement). During the history of the household, 68 had upgraded, either from dirt to cement, dirt to tile, or cement to tile. With respect to roofs, 139 roofs were initially of corrugated iron or palm leaves (137 corrugated iron, 2 palm leaves), and 25 had been upgraded to either cement or tile. In our survey, 133 of our households initially had access to only an outhouse, and 52 of these households installed indoor plumbing at some point in the current household's history. In land purchases, 49 households had realized changes in landholdings, with 44 acquiring more land and 5 selling land.

The goal of our estimation is to show how changes in these variables are influenced around the treatment window of either microfinance borrowing or access to remittances from abroad. Within the treatment window we should be able to observe a positive effect after credit or remittances by looking at changes in the estimated $\tau_{i,t-\bar{t}}$'s within the window. Our test for significance of credit and remittance effects is a straightforward *F*-test, in which the null hypothesis is that the $\tau_{i,t-\bar{t}}$'s within the treatment window before treatment are jointly equal to the $\tau_{i,t-\bar{t}}$'s after treatment.

We analyze different-sized treatment windows because there are advantages and disadvantages to each. A large window allows for a longer analysis of the dynamic effects of the treatment variable. However, because some households began to receive credit (or remittances) only one or two years prior to the survey, a larger window means that these observations are not included along with observations from older borrowers. A shorter window allows for uniformity in estimated coefficients, but obviously provides a shorter window for understanding the dynamics of changes in the dependent variables after treatment.

4. Econometric Results

We present basic tabulations from our household survey in Table A as shown by lagged values at one year before microfinance borrowing and the initial flow of remittances and one year after. Along with presenting a picture of dwelling characteristics, this represents a crude look at very short-term changes around initial microfinance borrowing and remittances. The figure in parenthesis in the "Pre-Credit" columns excludes exclude borrowers receiving credit in 2005 who do not appear in final columns for ease of comparison.

The first part of Table A shows changes in wall structure from approximately one year before and one year after credit. Before credit, houses with block walls constitute 51.9% the sample. Houses with (inferior) wood (7.0%), adobe (30.8%) and walls made of adobe finished with lime whitewash (10.8%) round out the sample. The changes appear to be uniformly positive in the window around initial microfinance borrowing: the percentage of houses with concrete block walls increases from 96 to 113 (51.9% to 61.1%) while the number of houses with adobe walls decline from 57 to 45 (30.8% to 24.3%). Wood-wall houses also decline from 13 to 9 (7.0% to 4.8%). Changes within the window before and after remittances are negligible. However, since we are not yet controlling for time via year-level fixed-effects, it is impossible to tell if these changes are the result of a general time trend or if they are influenced by the respective independent variables.

We see a similar story with changes in roofs. Concrete roofs increase from 22 to 31 (11.9% to 16.8%) while corrugated iron roofs decrease in the sample from 115 to 109 (62.2% to 58.9%). Clearly there is some movement from both corrugated iron and tile roofs to concrete, but again without accounting for year fixed-effects it is impossible to attribute such changes to credit. One year differences after first remittances are virtually nil.

Table A shows very similar patterns with changes in floors and toilets. One year after credit, both tile and concrete floors increase a few percent in the sample compared to one year before credit, while dirt floors decrease commensurately. Only one household upgraded floors one year after remittances. Houses indoor plumbing increases from 87 to 99 pre- to post-credit in the sample, while houses only having an outhouse decline from 99 to 82, a shift in about 5% of the households, a seemingly high rate of change within only (approximately) two years. Again, remittances show no raw change in this variable over the narrow treatment window.

While there is virtually no change in the aforementioned dwelling variables the year before and after access to remittances, changes in landholdings are greater around first access to remittances than around credit, from a mean of 3.301 to 3.588 cuerdas (though statistically insignificant (p-value = 0.147), compared with a change pre- and post credit from 2.940 to 3.041 cuerdas (p-value = 0.61).

The results of our estimations on changes in dwelling walls are given in Table 1. Column (1) gives a treatment window n = 9, with estimations on the four leading years before microfinance borrowing (FYRCREDITPLUS1, FYRCREDITPLUS2…etc.) the year of initial microfinance borrowing, and the four lagging years after initial microfinance borrowing (FYRCREDITMINUS1, FYRCREDITMINUS2…etc.) along with the same set of leads and lags for remittances. Point estimates show an increase in the probability of a wall upgrade of about 5 percentage points, but the increase in probability for wall upgrades begins one year prior to credit, and thus it is especially difficult to attribute causality to this particular post-credit increase in probability. Remittances (FYRREMITTPLUS1, etc.) seem to yield very little change in the probability of new walls in the first years after initial access to remittances, but then display a large effects in the fourth year after. Figure 1 plots the point estimates from the estimation in Column 1 comparing the results of credit with the results from remittances. In the remaining columns we carry out a similar estimation for a 5-year treatment window (two leads, two lags) in Column (2), and an estimation with a 7-year window only on credit in Column (3), and an estimation with a 7-year window on only remittances in Column (4).

Our main test for the significance of post-treatment changes in the dependent variable is an *F*-test, with the null hypothesis being that the coefficients on the leading variables in the estimation are equal to those on the lagged variables within the treatment window. If credit has a genuine effect on the probability of a major dwelling change, then the joint probability on the coefficients after the treatment should be significantly greater than the probability on the coefficients before treatment within the window. This test forms the basis for our judgment about whether credit positively influences the probability of dwelling improvements in our sample.

On the "new walls" estimations, the results for credit from the *F*-test show significant changes in wall upgrades within the 9-year treatment window in Column (1) only at a 21.5% level of confidence (*p*-value 0.215). In the narrower 5-year treatment window, however, the *p*-value on the *F*-test falls to 0.119 as the probability of a wall upgrade in the first year after credit is statistically significant, showing an increase of about 6 percentage points in that year. For remittances, an *F*-test for a significant difference between the coefficients on the post-treatment variables compared to coefficients on the pre-treatment variables rejects the null of no difference in the coefficients for the 9-year treatment window (p-value = 0.077), but cannot reject the null for the more narrow treatment windows. In the fourth year after receiving remittances, the probability of house conversion to block walls increases by an estimated 17.2 percentage points, a substantial increase as seen in Figure 1.

We carry out Sims' (1972) test for Granger causality through an F-test on the variables representing future years before borrowing and remittances, and find no statistical evidence (at the 95% confidence level) that these "leading" variables provide any additional explanatory power on dwelling upgrades for walls, roofs, floors, toilets, and landholdings. Thus it is appropriate to identify the significance of lagged treatment variables with Granger causality.

Our study does not suffer from the same kind of attrition bias described in Karlan (2001), because in identification our methodology relies on the specific *timing* of dwelling changes after microfinance borrowing rather than the simple differences in dependent (impact) variables between old and new borrowers. However, it remains conceivable that old borrowers could represent a group that exhibits different responses to credit than newer borrowers, since some borrowers (for whom the impact of loans could be greater or smaller) may have dropped out of the pool from an old cohort. Thus, while ideally for this type of study one would like to have a random sample of borrowers that includes former borrowers so that estimations are carried out on all recipients of credit after a given year, our estimations yield changes in the dependent variables among all borrowers within the current portfolio. The test for whether these coefficients are significantly different from those in Column (2) is easily carried out by first interacting a dummy variable for "new" borrowers (those in the portfolio who borrowed post 2001) with the post-credit treatment variables. It is then straightforward to carry out a Chow test on the joint significance of the coefficients on the interacted variables. For the "new wall" estimations--and on all other estimations on dwelling improvements in Tables 2 through 5--Chow tests failed to reject the null hypothesis that credit has the same effect on new borrowers within the portfolio as old borrowers.

Another possibility is that the 37 households with U.S. migrants (who make up a subset of our sample of microfinance borrowers) might have special "pro-active" qualities which exaggerate the impact on dwelling changes from remittances relative to credit. Thus in Column (5) we interact a dummy variable for migrant household status with each of the two years after initial microfinance borrowing (MDUM*CREDPS1 and MDUM*CREDPS2). The coefficient on this variable represents the added probability in the change of a dwelling upgrade attributed to households that have migrants over and above the standard household. The significant point estimate for change in new walls is estimated at an increase in 4.8 percentage points for the whole sample. However, column (5) of Table 1 shows that the increase in probability of a wall upgrade after credit is over *three times as high* for households with migrants than other households (even controlling for the effect of access to remittances), 11.8 percentage points versus about 3.7 percentage points for non-migrant households. This magnitude is reasonably close to the 17.2 percentage point increase realized by the same group after remittances. It seems thus that migrant households are likely to be characterized by pro-active qualities that magnify the effects of *any* income-augmenting access--whether it be credit or remittances.

Table 2 presents our estimations on changes in the probability of upgrades to roofs. In the roof estimations, credit does indeed appear to have a significant association with dwelling upgrades. In the second year after credit, the probability of an upgrade from either palm leaves or corrugated iron to either tile or concrete increases by almost exactly 2.7 percentage points in all of our specifications, and a significant at the 95% level both individually and in our *F*-test of the joint post-credit coefficients. Interestingly, virtually all of the changes occurring after the first year credit on roofs appears to function through the households with migrants, who show an increase of 8.4 percentage points in the first year of a roof upgrade as seen in Column (5) compared to virtually nil for others. Remittances appear to have very little effect again until the fourth year in which the probability of the roof upgrade increases by an estimated 16.2 percentage points (with the *F*-test on the nine-year treatment window significant at the 10% level). Thus, remittances appear to take longer to exert an influence on dwelling improvements, but when they do their effect (in this estimation) is approximately four times larger for a given year. The dynamics of changes in the probability of roof upgrades within the nine-your treatment window are seen in Figure 2.

Table 3 shows changes on the probability of floor upgrades, from either dirt to cement, cement to tile, or dirt to tile. The point estimates for the increase in probability of a for upgrade in each of the three years after credit in Column (3) are approximately 2.0-2.2% (with *t*-values close to one), but the credit treatment coefficients are jointly insignificant by our *F*-test, partly because the probability also increases in the year before credit by an estimated 2.9 percentage points, thus presenting an especially murky case for causality. Remittances yielded statistically significant changes to floor upgrades again in the fourth year after credit, increasing the probability of a floor upgrade by a (significant) 17.7 percentage points, however, our *F*-test on the significance of post-remittance coefficients in the nine-year treatment window cannot reject the null hypothesis of zero-difference relative to pre-remittance coefficients. Figure 3 shows the point estimates of changes in the probability of floor upgrade.

For our toilet estimations in Table 4, neither credit nor remittances appears to have any significant association with toilet and plumbing upgrades. Credit in fact would appear to be negatively associated with toilet and plumbing upgrades, for reasons that remain unclear. The only group within which credit may be associated with higher probabilities of floor upgrades is within the 37 migrant households, who show a probability increase of 5.4 percentages points in the second year, though not highly significant. There is a spike in increased probability of a plumbing upgrade after remittances such that the point estimate gives an increased probability of an upgrade by 9.1 percentage points (t-value = 2.22), but because the other post-remittance coefficients are negative, the joint F-test cannot reject the null hypothesis of no significant post-treatment difference.

Table 5 present our estimations on changes in land holdings. Here, increases in land holdings after access to remittances are large, and much larger than the modest changes realized after credit. Point estimates on remittances show land holdings on average increasing by 0.25, 0.38, 0.49, and 0.18 cuerdas sequentially in the four years after the commencement of remittances, implying a total increase in landholdings of about one-quarter of a hectare over four years after the initial receipt of remittances. (See Figure 5.) This magnitude seems to square well with our field observations; those receiving remittances in western Guatemala invariably invest in land as a secure asset and this kind of increase in average plot size for those receiving U.S. remittances from a family member is entirely plausible.

In contrast, the point estimates on increases in land holdings after microfinance borrowing are smaller--0.06, 0.14, 0.13, 0.14--and jointly insignificant due to fairly large standard errors. But the estimated magnitudes of the coefficients are quite stable and highly plausible, implying an increase in landholdings equal to about one-eighth of a hectare in the four years post-credit. This also seems to fit with our observations from the field that microfinance produces modest increases in land asset holdings, but perhaps only a fraction of the magnitude realized through access to remittances. As seen in Column (5), the bulk of this effect from credit appears to be within the migrant households.

Indeed much of the difference between the changes witnessed after remittances and microfinance borrowing seem to be due to the sub-sample of households receiving remittances from migrant family members, who may be more inclined toward positive dwelling changes. To test for the overall effect of these differences we carried out Chow tests on the joint significance of these interacted variables. We find statistical confirmation of this idea: post-credit changes in our dwelling variables are significantly stronger for households with migrants for some of our dwelling changes, especially for roof upgrade and new landholding estimations at the 95% confidence level or above.

In our estimation in Table 6, we analyze the effect of microfinance *access* (as opposed to actual microfinance borrowing) on dwelling upgrades. The sequential rollout of *Fe y Alegria's* microcredit program into different villages in different years allows us to investigate this possibility. These estimations may add value to our understanding of the effects of microfinance because the introduction to credit in different villages is more likely to be exogenous to dwelling upgrades that the timing of the household-level decision to take a loan. We denote the introduction of credit into a particular village as the year in which the first borrower from that village began borrowing. In the estimations in Table 6 the credit *access* dummy variable typically switches on one to three years before actual borrowing, though some cases it occurs contemporaneously, and in other cases many years after. In short we find that mere access to microfinance within one's village has no statistically significant influence on the probability of future upgrades to dwellings that we can identify.

In our final set of estimations in Table 7, we utilize the exogeneity of rollout of the credit program in our estimation to discern causality in the microfinance borrowing/dwelling upgrade relationship: First, we create a dummy variable indicating whether or not the credit program had been introduced into the village of household *i* at time *t*. We interact this dummy variable with the variables representing the years for each household prior to treatment and include it in the estimation along with the other pre-treatment time dummies. We then estimate the equation

$$y_{it} = h_i + \alpha_t + \sum_{i,t-\bar{t}}^{t-\bar{t}=k} \tau_{i,t-\bar{t}} T_{i,t-\bar{t}} + \sum_{i,t-\bar{t}}^{t-\bar{t}=-1} \delta_{i,t-\bar{t}} T_{i,t-\bar{t}} d_{i,t-\bar{t}} + u_{it}\,, \qquad (2)$$

where there are $n = 2k + 1$ leading and lagged treatment dummy variables in the first summation and $k - 1$ leading treatment dummy variables in the second summation interacted with a dummy equal to 1 if the microfinance program was *un*available and zero otherwise, where $d_{i,t-\bar{t}}$ is the interacted dummy representing the absence of a credit program. Identification comes in a nine-year treatment window from the 83 households who accessed credit within three years after the introduction of the program.[5]

This estimation allows us to better ascertain the pre-credit probabilities of major dwelling changes, since now we can utilize data from a subset of 83 households who took credit in the years immediately subsequent to program introduction in a village. The sum of the coefficients on the raw pre-treatment dummy variables and the dummy variables interacted with the "microfinance constraint" dummy provides a stronger counterfactual, yielding estimates of the probability of a dwelling upgrade in one of our treatment households before borrowing *and* when credit was unavailable.

Significance of the $\delta_{i,t-\bar{t}}$ coefficients in (2) could reflect endogenous borrowing in the following ways. On one hand it is conceivable that microenterprise entrepreneurs might choose to borrow when, for example, prices happen to be high for their particular product, in order to take advantage of economic opportunity. A jump in prices for an entrepreneur's product could thus initiate borrowing, but also cause high profits by themselves and thus cause dwelling upgrades. Failing to correct for lack of program access would thus *overestimate* the difference between post-credit and pre-credit treatment variables, *i.e.* our *F*-test would be biased *upwards*, biasing the test toward Type I errors (rejecting a null hypothesis that there is no significant change in probability of dwelling upgrades yielded after credit). In the presence of endogenous

[5] In our sample, 28 borrowers obtained credit in the first year a the program was introduced into a village (allowing of an observation on the probability of a dwelling upgrade one year before credit when there was no credit access as well as two, three, and four years before), 14 obtain credit one year after (allowing for an observation when there was no credit access on two, three, and four years before), 18 two years after access (three and four years before), and 23 three years after access (only on four years before).

borrowing based on positive economic opportunity, we would thus expect the $\delta_{i,t-\bar{t}}$'s to be positive. The true change in probability of dwelling upgrades for a pre-credit year would not be $\tau_{i,t-\bar{t}}$, but rather $\tau_{i,t-\bar{t}} + d_{i,t-\bar{t}}$.

Another source of endogeneity between borrowing and dwelling upgrades could be from an opposite phenomenon: Microenterprise entrepreneurs might choose to systematically borrow when prices happen to be low for their particular product (or economic times are hard) in order to smooth negative shocks. Here, failing to correct for lack of pre-credit program access would *underestimate* the difference between post-credit and pre-credit treatment variables, making our *F*-test biased *downwards* and inclined toward Type II errors, accepting the null of no significant change in probability of dwelling upgrades yielded after credit. With this type of endogeneity, we would expect the $\delta_{i,t-\bar{t}}$'s to be *negative* since with the unavailability of credit, negative shocks would further reduce the probability of dwelling upgrades.

Our strategy then is the following: To test for systematic endogeneity in the timing of borrowing decisions, we carry out a test for the significance of the $\delta_{i,t-\bar{t}}$'s on whether they are jointly different than zero. If these interactive dummies are jointly *significant* by an *F*-test, our new test for the effect of microfinance borrowing on dwelling upgrades would then become the significance of differences between the post-credit treatment $\tau_{i,t-\bar{t}}$'s and the sum of the pre-credit $\tau_{i,t-\bar{t}} + d_{i,t-\bar{t}}$'s within the symmetric treatment window. If the interacted variables are jointly *insignificant*, then we are less worried about endogeneity in the timing of borrowing decisions with respect to our dwelling impact variables, and we are able to ascribe a higher level of causality to our original estimations.

As seen in Table 7, we find little statistical evidence for the joint significance of these interacted dummy variables. In none of our five dwelling changes are the interacted variables on the raw pre-treatment dummy variables and the microfinance constraint dummy (NOPROGCREDMINUS1, NOPROGCREDMINUS2, NOPROGCREDMINUS3, NOPROGCREDMINUS4) jointly significantly different from zero at even the 10% level. We must qualify the power of these tests, since with a treatment window of 9 years, they rely on a subset of 83 households who took credit either the year that the program was introduced in a village or up to three years after. Nevertheless, even a sub-sample of this size is likely to pick up significant endogeneity between the timing of credit choice and the timing of dwelling upgrades. Because of the insignificance of the interacted pre-treatment and microfinance constraint dummy variables, we are more confident in ascribing causality to our estimates and Tables 1 through 5.

5. Summary and Implications for Future Research

In short, our results indicate that microfinance borrowing is associated with relatively modest, but in some cases positive and significant, changes in dwelling improvements, especially with upgrades to roofs and possibly land. This significance appears only subsequent to *receipt* of credit; the *availability* of credit has no significant impacts on any home improvements. In a sample of people who will borrow eventually, the former measure gives a treatment effect on the treated (TET) and the latter a form of intention to treat effect (ITE).[6] While in general the rollout of a program provides a more exogenous source of variation than the uptake subsequent to rollout, we test for differences in pre-treatment behavior between these two groups and find none. We thus conclude that in this case the uptake of credit provides an admissible source of identification. The timing of effects seen in the initial rollout of credit (ITE) and the subsequent uptake of credit (TET) is similar but the magnitude in the latter is larger. We attribute this

[6] That is, the impact of offering credit to a group when not all of them choose to take it *immediately*. The standard ITE is formed by measuring the impact of credit on a group wherein some choose *never* to take loans.

difference to the relatively low uptake of credit at the time of rollout, which typically causes the ITE to be closer to zero than the TET because it contains many 'non-compliers'.

Receipt of remittances, in comparison, precede substantial future upgrades to walls, roofs, and land (but are not significantly associated with upgrades to floors or toilets). Not only significance levels, but magnitudes of changes in probabilities of upgrades, are higher for remittances than microfinance. However, in the case of remittances we have no program rollout that can be used to identify a more exogenous source of variation. It is possible that the observed relationship between the timing of remittances and that of housing upgrades is due to households requesting that migrants send remittances once they have decided that they are ready to make such an upgrade. Nonetheless, there is important information present in the strong tendency of home improvements to follow on the heels of remittances. Our results confirm field observations and the widespread belief that remittances display a powerful effect on dwelling upgrades and construction of modern homes in rural Guatemala.

To carry out these estimations, our methodology involves the creation of a backcast panel for household participants in a microfinance program. This backcast panel re-creates a history of major changes in the household over time, the timing of these major changes then being compared to the timing of microfinance borrowing and the receipt of remittances from international migrants. Based on the timing of these changes within a treatment window, it becomes possible to analyze the subsequent changes in the probabilities of important variables correlated with economic development.

We think it may be helpful at this point to lay out a standardized approach to this kind of study, and to express both limits to and caveats with its implementation. First, to be able to fully attribute relative changes in post-treatment coefficients to program impact, a researcher seeking to implement this approach should try to identify instances in which a program has been phased in over time in a manner that is unrelated to impact variables (changes in dwelling units, health, capitalization of an enterprise, etc). This requires that the program rollout not be directly sequenced based on changes in the outcome of interest, and that there be no shocks which drive both program placement and shifts in outcomes. When implementation of the program is exogenous to household impact variables, we can interpret panel impacts of the availability of the program as a form of intention to treat effect.. If the adoption of a treatment is very high at the moment it is introduced the effect measured at the time when the program became available offers a clean ex-post measure of causal impacts.

Second, a cross sectional survey is carried out on a random sample of current and former program participants who received the treatment after a given time in the past. It is possible to carry out the survey, as we have done, only on current borrowers, but if there is a difference in impact between new and old borrowers within the portfolio, interpretation of coefficients becomes can become problematic. (Fortunately, in our case we find no statistical basis for such differences.)

Third, when using recall survey questions, it is important to focus primarily on ascertaining the timing of major discrete changes to impact variables. Changes in variables such as profit, revenue, and so forth are difficult for subjects to remember, and are often imprecise by their very nature in informal-sector enterprises even when trying to be ascertained in the present. Major diseases, deaths, school enrollments, and major asset purchases are the kinds of variables best used within this framework. In many respects, this may not represent a disadvantage since what researchers (and households in development countries as well) often view as "development" may be closely associated with these kinds of major changes anyway.

Lastly, after carrying out fixed effects estimations on the data, care in the interpretation of coefficients is critical. This interpretation may be in formed by theory, field observation, and common sense. The enthusiasm with which a researcher ascribes causality to statistical significance in estimations must reflect the degree to which either theory dictates causality and alternative "third variable" stories seem unlikely, or program access can be identified as exogenous to the relevant dependent variables in some way. The latter is most useful when treatment adoption is nearly instantaneous with access. An example might be a randomly assigned vaccination program in public schools which is either mandatory or for which the benefits are so clearly manifest that everybody immediately chooses treatment. Another example might be in the introduction of a clean water system; everyone prefers the clean water to what existed before.

It is also important to keep in mind that there are different "types" of causality. Microfinance, for example, is a treatment that is always a household choice representing a means to an end, and end that may include improving living conditions via higher enterprise profits. Microfinance does not *cause* dwelling improvements *per se*, but may represent a door, perhaps a necessary door in some cases, that a household can pass through to realize welfare improvements through releasing credit or liquidity constraints. What truly *causes* these improvements, is a particular household's *desire* for them, which in sequence may "cause" the household to take a microfinance loan and subsequently utilize enterprise profits for dwelling improvements. Thus microfinance, or other similar treatments, may constitute an intermediate step that is either necessary or a best option to the desired realization of an impact variable such as a dwelling upgrade. Some may describe microfinance as "causing" the dwelling improvement, but the nature of this causality is different, for example, than the case of de-worming drugs, which "cause" the body to expel dangerous parasites. Clearly in our study, collecting remittances from household migrants abroad would constitute another such vehicle for improvement in household welfare.

The benefits of random assignment for the estimation of causal impacts are well-established, but the circumstances in which this technique can be implemented are limited by practical considerations. This paper outlines a method that can be pursued using entirely ex-post data from a single survey wave, utilizing the sequencing of a program's rollout as a natural experiment. We outline the circumstances and provide an example from our own data under which statistical significance can be attributed to standard causality, suggesting that even where conditions for strict exogeneity are not met, that useful information can be recovered. We find that certain dwelling improvements are more likely to occur after a household starts receiving credit than before and that by exploiting differential timing in the rollout of the credit program, there is reason to believe that we can infer causal effects from our estimations. Yet the increased probability of these dwelling changes after credit is smaller than those after remittances. These results appear to conform with evidence from other contexts and anecdotal evidence from the field. Our study also illustrates the value of using "unforgettable events" to create backcast panels for impact estimation.

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Table A: Frequencies of Dwelling Type
(Pre- and Post- Credit/Remittances)

Walls	--- Pre-Credit ($\bar{t}-1$) ---		--- Post-Credit ($\bar{t}+1$) ---	
	obs.	percent	obs.	percent
block	106 (96)*	52.5 (51.9)	113	61.1
finished adobe	22 (19)	10.9 (10.3)	18	9.7
adobe	61 (57)	30.2 (30.8)	45	24.3
wood	13 (13)	6.4 (7.0)	9	4.8
total**	202 (185)	100.0 (100.0)	185	100.0
	--- Pre-Remittances ($\bar{t}-1$) ---		--- Post- Remittances ($\bar{t}+1$) ---	
block	15	40.5	15	41.7
finished adobe	7	18.9	7	19.4
adobe	13	35.1	12	33.3
wood	2	5.4	2	5.6
total	37	100.0	36	100.0
Roof	--- Pre-Credit ($\bar{t}-1$) ---		--- Post-Credit ($\bar{t}+1$) ---	
concrete	27 (22)	13.4 (11.9)	31	16.8
tile	51 (46)	25.2 (24.9)	44	23.8
corrugated iron	122 (115)	60.4 (62.2)	109	58.9
palm leaves	1 (1)	0.5 (0.5)	1	0.5
total	202 (185)	100.0 (100.0)	185	100.0
	--- Pre-Remittances ($\bar{t}-1$) ---		--- Post- Remittances ($\bar{t}+1$) ---	
concrete	5	13.5	5	13.9
tile	10	27.0	10	27.8
corrugated iron	20	54.1	19	52.8
palm leaves	1	2.7	1	2.8
total	37	100.0	36	100.0
Floor	--- Pre-Credit ($\bar{t}-1$) ---		--- Post-Credit ($\bar{t}+1$) ---	
tile	25 (23)	12.4 (12.4)	26	14.05
concrete	118 (108)	58.3 (58.4)	114	61.6
dirt	58 (53)	28.7 (28.6)	45	24.32
total*	202 (185)	100.0 (100.0)	185	100
	--- Pre-Remittances ($\bar{t}-1$) ---		--- Post- Remittances ($\bar{t}+1$) ---	
tile	2	5.4	3	8.3
concrete	26	70.2	25	59.3
dirt	8	21.6	7	19.4
total	37	100.0	36	100.0

* values in parenthesis exclude borrowers receiving credit in 2005 who do not appear in final columns
** totals may not equal category sum due to unrecorded observations for individual categories

Table A: Frequencies of Dwelling Type, con't
(Pre- and Post- Credit/Remittances)

Toilet	**--- Pre-Credit ($\bar{t}-1$) ---**		**--- Post-Credit ($\bar{t}+1$) ---**	
	obs.	**percent**	**obs.**	**percent**
indoor plumbing	97 (87)	48.0 (47.0)	99	53.51
outhouse	99 (92)	49.0 (49.7)	82	44.3
total	202	100.0	185	100.0
	--- Pre-Remittances ($\bar{t}-1$) ---		**--- Post- Remittances ($\bar{t}+1$) ---**	
indoor plumbing	15	40.5	14	38.9
outhouse	21	56.8	21	58.3
total	37	100.0	36	100.0
Land	**--- Pre-Credit ($\bar{t}-1$) ---**		**--- Post-Credit ($\bar{t}+1$) ---**	
		mean and std. dev		**mean and (std. dev)**
mean: cuerdas**	195 (178)	2.962 (2.940)	178	3.041
standard deviation		3.85 (3.73)		3.72
	--- Pre-Remittances ($\bar{t}-1$) ---		**--- Post- Remittances ($\bar{t}+1$) ---**	
mean: cuerdas	35	3.301	34	3.588
standard deviation		3.42		3.59
** equals approximately 25 x 25 meters				

Table 1: Probability of Wall Upgrade†

	(1)	(2)	(3)	(4)	(5)
	New walls	New walls	New walls	New walls	New walls
FYRCREDITPLUS4	0.001				
	(0.042)				
FYRCREDITPLUS3	-0.005		-0.003		
	(0.036)		(0.032)		
FYRCREDITPLUS2	0.032	0.036	0.035		0.020
	(0.032)	(0.026)	(0.028)		(0.030)
FYRCREDITPLUS1	0.056*	0.059**	0.058**		0.037
	(0.029)	(0.024)	(0.026)		(0.027)
FYRCREDIT	0.034	0.047**	0.045*		0.048**
	(0.028)	(0.024)	(0.025)		(0.024)
FYRCREDITMINUS1	0.047*	0.055**	0.057**		0.056**
	(0.027)	(0.024)	(0.025)		(0.024)
FYRCREDITMINUS2	-0.040	-0.031	-0.029		-0.030
	(0.027)	(0.024)	(0.025)		(0.024)
FYRCREDITMINUS3	-0.011		-0.001	**MDUM*CREDPS2**	**0.056**
	(0.027)		(0.025)		**(0.049)**
FYRCREDITMINUS4	-0.034			**MDUM*CREDPS1**	**0.081***
	(0.026)				**(0.046)**
FYRREMITTPLUS4	0.172***				
	(0.066)				
FYRREMITTPLUS3	0.033			0.023	
	(0.055)			(0.054)	
FYRREMITTPLUS2	0.026	0.012		-0.003	0.005
	(0.051)	(0.049)		(0.050)	(0.049)
FYRREMITTPLUS1	-0.027	-0.041		-0.043	-0.046
	(0.046)	(0.045)		(0.045)	(0.045)
FYRREMITT	0.004	-0.016		-0.015	-0.020
	(0.046)	(0.044)		(0.045)	(0.045)
FYRREMITTMINUS1	-0.005	-0.016		-0.013	-0.025
	(0.046)	(0.044)		(0.045)	(0.045)
FYRREMITTMINUS2	-0.054	-0.067		-0.066	-0.069
	(0.046)	(0.044)		(0.045)	(0.044)
FYRREMITTMINUS3	-0.001			-0.004	
	(0.046)			(0.046)	
FYRREMITTMINUS4	0.023				
	(0.049)				
Observations	1341	1341	1341	1341	1341
Number of *i*	109	109	109	109	109
R-squared	0.04	0.03	0.03	0.02	0.04
F-test,credit impact	1.53	2.43	0.92		**F-test signif. of**
(significance level)	p = 0.215	p = 0.119	p = 0.339		**being migrant:**
F-test,remitt impact	3.14*	0.40		0.30	2.02
(significance level)	p = 0.077	p = 0.530		p = 0.582	p = 0.1328

Standard errors in parentheses †adobe to finished adobe, adobe to block, adobe to concrete, or wood to concrete
* significant at 10%; ** significant at 5%; *** significant at 1%

Figure 1: Estimated Treatment Coefficients--Walls

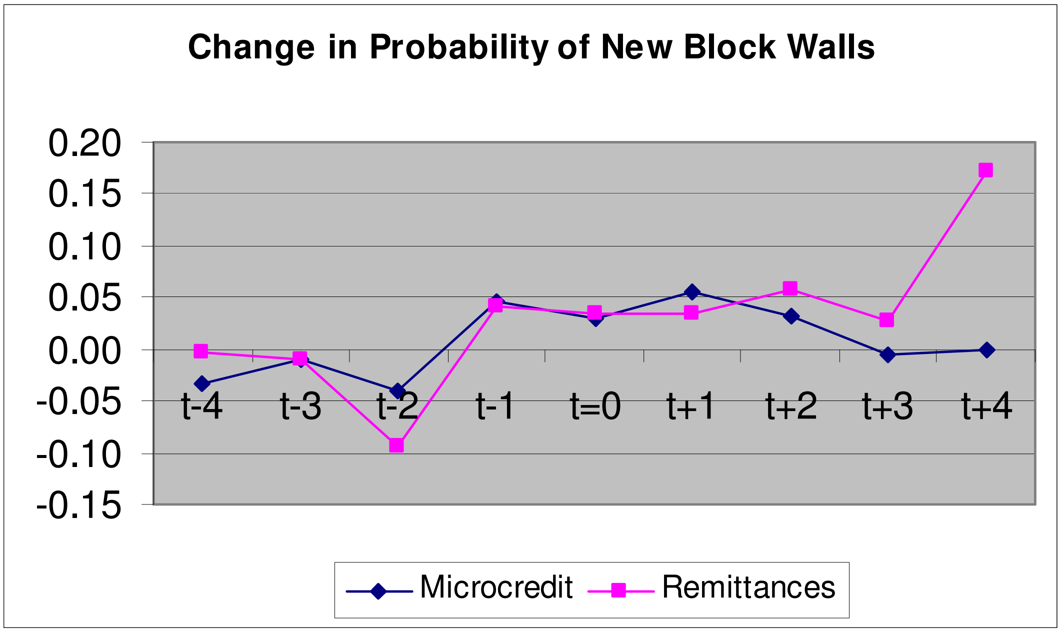

Table 2: Probability of New Roof (Palm Leaves/Corrugated Iron to Cement/Tiles)

	(1)	(2)	(3)	(4)	(5)
	New roof	New roof	New roof	New roof	New roof
FYRCREDITPLUS4	-0.002				
	(0.025)				
FYRCREDITPLUS3	-0.017		-0.017		
	(0.021)		(0.018)		
FYRCREDITPLUS2	0.007	0.010	0.007		0.027
	(0.019)	(0.015)	(0.017)		(0.018)
FYRCREDITPLUS1	0.028	0.027**	0.026*		0.003
	(0.017)	(0.014)	(0.015)		(0.016)
FYRCREDIT	0.013	0.015	0.014		0.011
	(0.017)	(0.013)	(0.015)		(0.014)
FYRCREDITMINUS1	-0.004	-0.010	-0.007		-0.008
	(0.016)	(0.013)	(0.014)		(0.014)
FYRCREDITMINUS2	-0.002	-0.006	-0.002		-0.004
	(0.016)	(0.014)	(0.015)		(0.014)
FYRCREDITMINUS3	0.015		0.016	**MDUM*CREDPS2**	**-0.049***
	(0.016)		(0.014)		**(0.029)**
FYRCREDITMINUS4	0.001			**MDUM*CREDPS1**	**0.084*****
	(0.015)				**(0.028)**
FYRREMITTPLUS4	0.162***				
	(0.041)				
FYRREMITTPLUS3	-0.051			-0.065*	
	(0.038)			(0.037)	
FYRREMITTPLUS2	-0.045	-0.047		-0.059*	-0.045
	(0.033)	(0.032)		(0.032)	(0.029)
FYRREMITTPLUS1	-0.034	-0.034		-0.043	-0.039
	(0.028)	(0.028)		(0.028)	(0.027)
FYRREMITT	-0.029	-0.031		-0.038	-0.026
	(0.028)	(0.027)		(0.027)	(0.027)
FYRREMITTMINUS1	0.004	0.004		-0.002	0.000
	(0.028)	(0.028)		(0.028)	(0.027)
FYRREMITTMINUS2	-0.044	-0.044		-0.051*	-0.036
	(0.028)	(0.028)		(0.028)	(0.026)
FYRREMITTMINUS3	-0.044			-0.049*	
	(0.030)			(0.030)	
FYRREMITTMINUS4	-0.033				
	(0.031)				
Observations	1567	1567	1567	1567	1341
Number of i	139	139	139	139	109
R-squared	0.04	0.02	0.02	0.02	0.03
F-test,credit impact	0.01	4.29**	0.06		**F-test signif. of**
(significance level)	p = 0.906	p = 0.038	p = 0.80		**being migrant:**
F-test,remitt impact	2.80	0.55		0.84	6.63***
(significance level)	p = 0.094	p = 0.451		p = 0.358	p = 0.0014

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Figure 2: Estimated Treatment Coefficients--Roof

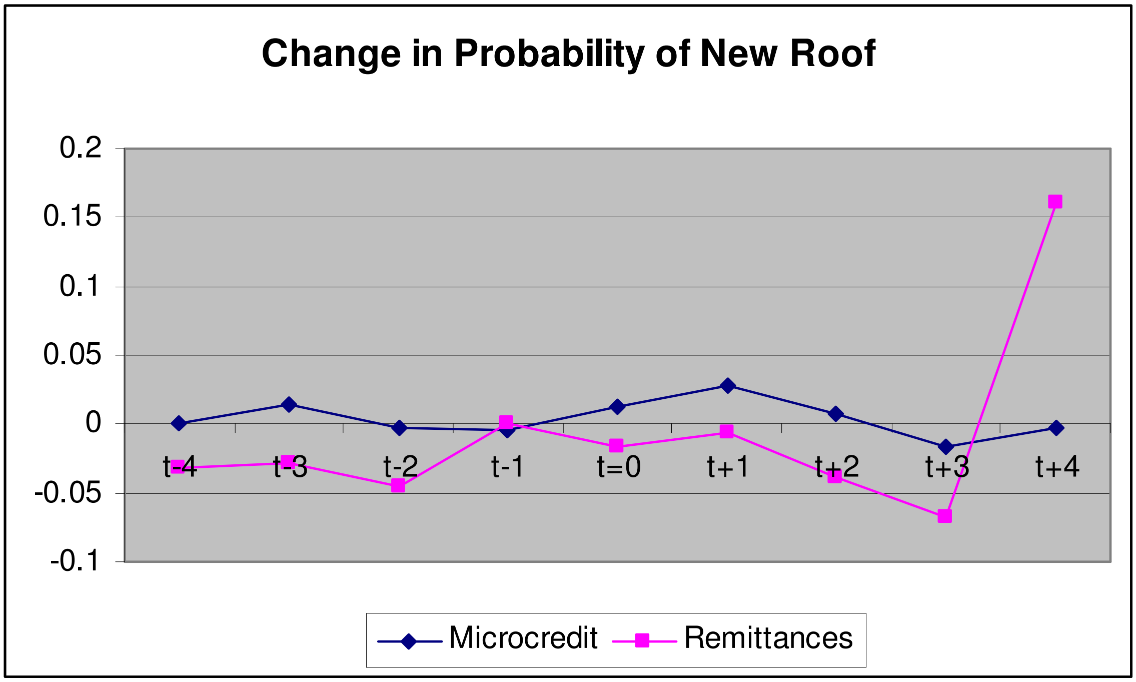

Table 3: Probability of New Floor (Dirt to Cement, Cement to Tile, or Dirt to Tile)

	(1)	(2)	(3)	(4)	(5)
	New floor	New floor	New floor	New floor	New floor
FYRCREDITPLUS4	-0.024				
	(0.033)				
FYRCREDITPLUS3	0.012		0.020		
	(0.028)		(0.024)		
FYRCREDITPLUS2	0.013	0.012	0.022		0.040
	(0.026)	(0.020)	(0.022)		(0.029)
FYRCREDITPLUS1	0.018	0.014	0.022		-0.002
	(0.023)	(0.018)	(0.020)		(0.027)
FYRCREDIT	-0.006	-0.007	-0.000		-0.000
	(0.022)	(0.018)	(0.019)		(0.023)
FYRCREDITMINUS1	0.029	0.023	0.029		0.019
	(0.021)	(0.018)	(0.019)		(0.023)
FYRCREDITMINUS2	-0.011	-0.015	-0.010		-0.009
	(0.021)	(0.018)	(0.019)		(0.023)
FYRCREDITMINUS3	0.007		0.005	**MDUM*CREDPS2**	**-0.035**
	(0.021)		(0.019)		**(0.048)**
FYRCREDITMINUS4	0.009			**MDUM*CREDPS1**	**0.063**
	(0.020)				**(0.045)**
FYRREMITTPLUS4	0.177***				
	(0.054)				
FYRREMITTPLUS3	-0.037			-0.048	
	(0.050)			(0.049)	
FYRREMITTPLUS2	-0.027	-0.040		-0.049	-0.066
	(0.044)	(0.043)		(0.043)	(0.048)
FYRREMITTPLUS1	-0.025	-0.031		-0.032	-0.049
	(0.037)	(0.036)		(0.037)	(0.044)
FYRREMITT	0.010	0.002		-0.000	-0.016
	(0.037)	(0.036)		(0.036)	(0.044)
FYRREMITTMINUS1	0.016	0.008		0.009	-0.011
	(0.037)	(0.036)		(0.037)	(0.044)
FYRREMITTMINUS2	-0.031	-0.037		-0.041	-0.059
	(0.038)	(0.037)		(0.037)	(0.043)
FYRREMITTMINUS3	0.015			0.014	
	(0.040)			(0.039)	
FYRREMITTMINUS4	-0.025				
	(0.041)				
Observations	1567	1567	1567	1567	1341
Number of i	139	139	139	139	109
R-squared	0.03	0.02	0.02	0.02	0.03
F-stat,credit impact	0.04	0.27	0.434		**F-test signif.**
(signif level)	p = 0.84	p = 0.644	p =0.61		**bng migrant:**
F-stat,remitt impact	0.91	0.33		1.37	1.34
(signif level)	p = 0.33	p = 0.568		p = 0.243	p = 0.263

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Figure 3: Estimated Treatment Coefficients--Floor

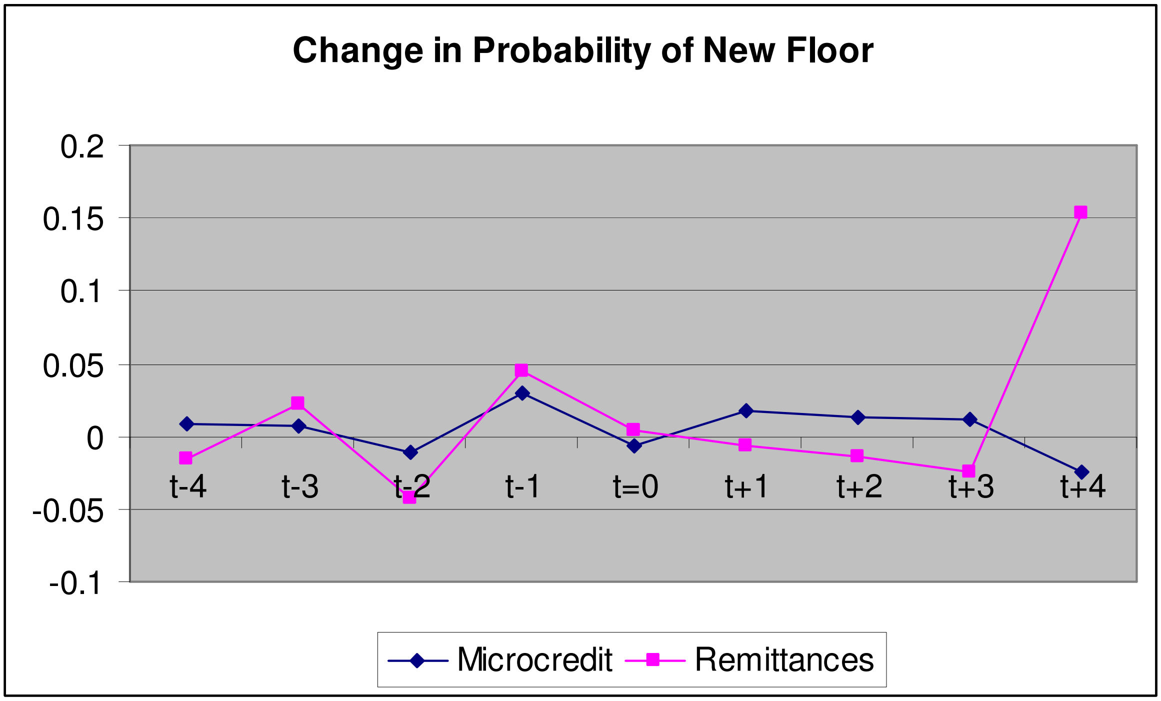

Table 4: New Toilet (From Outhouse to Indoor Plumbing)

	(1)	(2)	(3)	(4)	(5)
	New toilet	New toilet	New toilet	New toilet	New toilet
FYRCREDITPLUS4	-0.075*				
	(0.040)				
FYRCREDITPLUS3	-0.035		-0.010		
	(0.034)		(0.028)		
FYRCREDITPLUS2	-0.032	-0.004	-0.012		-0.009
	(0.030)	(0.023)	(0.025)		(0.022)
FYRCREDITPLUS1	-0.038	-0.012	-0.016		-0.005
	(0.027)	(0.021)	(0.023)		(0.020)
FYRCREDIT	0.002	0.022	0.018		-0.015
	(0.026)	(0.020)	(0.022)		(0.018)
FYRCREDITMINUS1	-0.006	0.010	0.005		0.004
	(0.025)	(0.020)	(0.022)		(0.018)
FYRCREDITMINUS2	0.001	0.015	0.014		0.008
	(0.024)	(0.020)	(0.022)		(0.018)
FYRCREDITMINUS3	-0.012		-0.008	MDUM*CREDPS2	**0.054**
	(0.023)		(0.021)		**(0.037)**
FYRCREDITMINUS4	0.007			MDUM*CREDPS1	**-0.020**
	(0.022)				**(0.035)**
FYRREMITTPLUS4	-0.060				
	(0.050)				
FYRREMITTPLUS3	-0.053			-0.048	
	(0.045)			(0.044)	
FYRREMITTPLUS2	0.091**	0.093**		0.091**	0.079**
	(0.041)	(0.040)		(0.040)	(0.037)
FYRREMITTPLUS1	-0.039	-0.034		-0.039	-0.021
	(0.038)	(0.036)		(0.037)	(0.034)
FYRREMITT	-0.055	-0.049		-0.052	-0.046
	(0.038)	(0.036)		(0.037)	(0.034)
FYRREMITTMINUS1	-0.007	-0.001		-0.009	0.019
	(0.038)	(0.037)		(0.037)	(0.034)
FYRREMITTMINUS2	-0.009	-0.003		-0.004	0.016
	(0.039)	(0.038)		(0.038)	(0.033)
FYRREMITTMINUS3	0.008			0.011	
	(0.040)			(0.039)	
FYRREMITTMINUS4	0.008				
	(0.042)				
Observations	1600	1600	1600	1600	1341
Number of i	133	133	133	133	109
R-squared	0.03	0.02	0.02	0.02	0.03
F-stat,credit impact	3.73	1.11	0.75		**F-test signif. of**
(significance level)	p = 0.057	p = 0.293	p = 0.386		**being migrant:**
F-stat,remitt impact	0.30	0.77		0.01	1.36
(significance level)	p = 0.586	p = 0.379		p = 0.946	p = 0.258

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Figure 4: Estimated Treatment Coefficients--Toilet

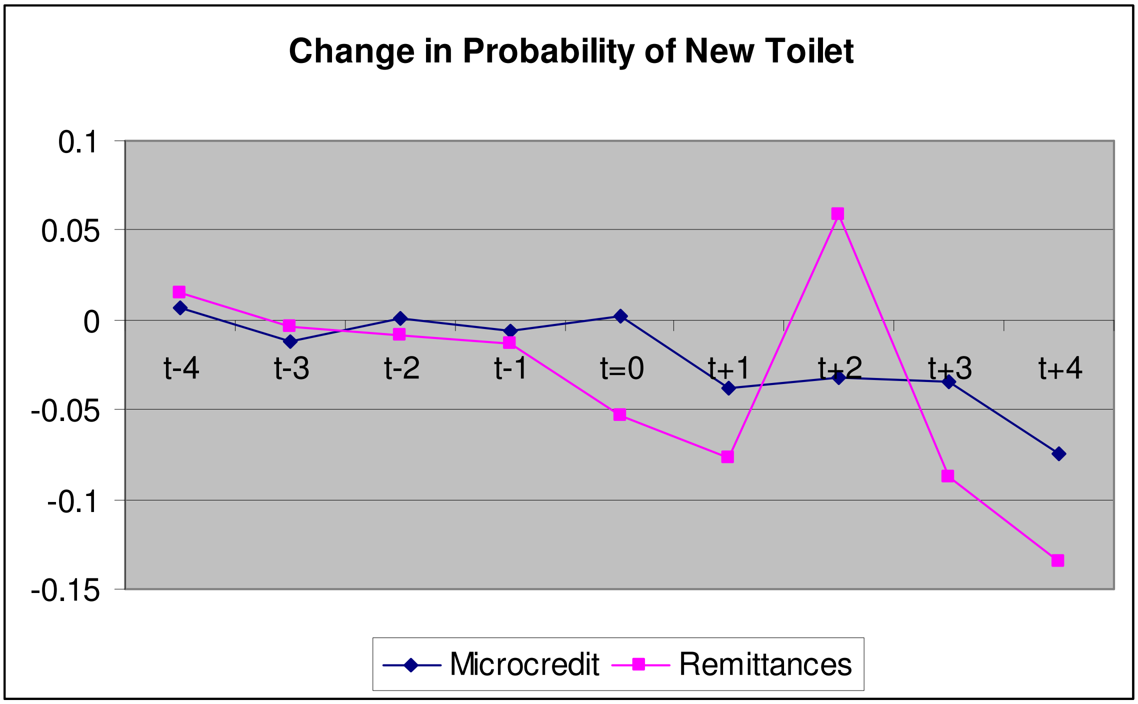

Table 5: Impact on Land Holdings

	(1)	(2)	(3)	(4)	(5)
	Cuerdas	Cuerdas	Cuerdas	Cuerdas	Cuerdas
FYRCREDITPLUS4	0.140				
	(0.180)				
FYRCREDITPLUS3	0.136		0.072		
	(0.153)		(0.133)		
FYRCREDITPLUS2	0.140	0.065	0.076		-0.169
	(0.137)	(0.110)	(0.120)		(0.209)
FYRCREDITPLUS1	0.057	-0.014	-0.001		-0.171
	(0.123)	(0.098)	(0.108)		(0.190)
FYRCREDIT	0.077	0.015	0.027		0.048
	(0.116)	(0.094)	(0.103)		(0.165)
FYRCREDITMINUS1	0.071	0.028	0.026		0.063
	(0.112)	(0.094)	(0.101)		(0.164)
FYRCREDITMINUS2	-0.007	-0.050	-0.044		-0.006
	(0.111)	(0.094)	(0.101)		(0.164)
FYRCREDITMINUS3	0.010		-0.008	M DUM*CREDPS2	**0.846****
	(0.107)		(0.099)		**(0.340)**
FYRCREDITMINUS4	0.015			M DUM*CREDPS1	**0.394**
	(0.105)				**(0.323)**
FYRREMITTPLUS4	0.178				
	(0.285)				
FYRREMITTPLUS3	0.489*			0.504**	
	(0.251)			(0.247)	
FYRREMITTPLUS2	0.380*	0.382*		0.392**	0.531
	(0.227)	(0.221)		(0.223)	(0.336)
FYRREMITTPLUS1	0.253	0.259		0.278	0.392
	(0.201)	(0.195)		(0.197)	(0.305)
FYRREMITT	0.068	0.075		0.099	0.050
	(0.198)	(0.192)		(0.194)	(0.305)
FYRREMITTMINUS1	0.122	0.132		0.144	0.175
	(0.201)	(0.195)		(0.197)	(0.305)
FYRREMITTMINUS2	0.140	0.153		0.169	0.285
	(0.200)	(0.195)		(0.197)	(0.304)
FYRREMITTMINUS3	-0.254			-0.218	
	(0.207)			(0.204)	
FYRREMITTMINUS4	-0.290				
	(0.217)				
Observations	2387	2387	2387	2387	1287
Number of i	205	205	205	205	103
R-squared	0.07	0.06	0.06	0.06	0.08
F-test, cred imp	0.92	0.16	0.41		**F-test signif. of**
(signif level)	p = 0.338	p = 0.690	p = 0.523		**being migrant:**
F-test, remit imp	6.66***	0.86		4.78**	3.62**
(signif level)	p = 0.010	p = 0.355		p = 0.0289	p = 0.027

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Figure 5: Estimated Treatment Coefficients--Land

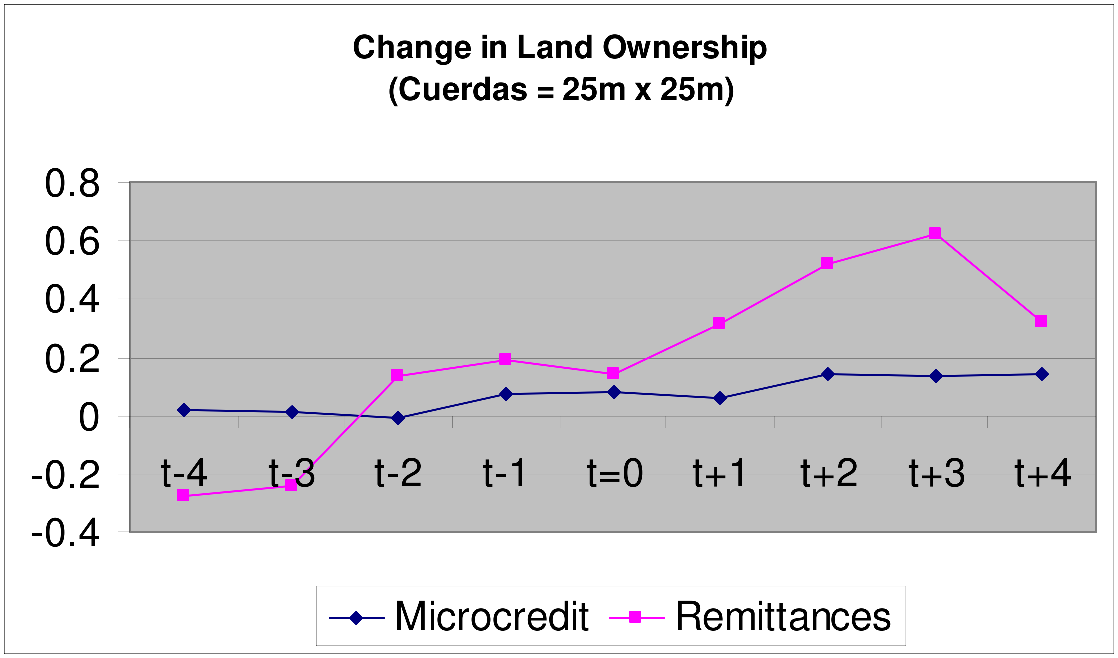

Table 6: Credit Access as Treatment Variable

	(1)	(2)	(3)	(4)	(5)
	New block	New roof	New floor	New toilet	Cuerdas
IVFYRCREDITPLUS4	0.014	-0.002	0.005	0.001	-0.014
	(0.016)	(0.010)	(0.017)	(0.015)	(0.110)
IVFYRCREDITPLUS3	0.007	-0.004	0.004	0.021	-0.080
	(0.017)	(0.010)	(0.017)	(0.015)	(0.113)
IVFYRCREDITPLUS2	0.029	-0.010	0.015	-0.001	-0.187
	(0.017)*	(0.010)	(0.017)	(0.016)	(0.116)
IVFYRCREDITPLUS1	0.015	0.007	-0.001	0.007	-0.145
	(0.017)	(0.010)	(0.017)	(0.016)	(0.116)
IVCREDYEAR	0.023	-0.007	-0.005	0.004	-0.144
	(0.018)	(0.010)	(0.018)	(0.016)	(0.117)
IVFYRCREDITMINUS1	0.031	0.012	0.017	-0.001	-0.139
	(0.019)*	(0.011)	(0.018)	(0.016)	(0.121)
IVFYRCREDITMINUS2	-0.008	-0.008	0.003	0.032	-0.065
	(0.018)	(0.011)	(0.018)	(0.017)*	(0.123)
IVFYRCREDITMINUS3	-0.006	0.014	0.034	-0.008	0.010
	(0.019)	(0.011)	(0.018)*	(0.016)	(0.122)
IVFYRCREDITMINUS4	-0.011	-0.006	-0.010	-0.008	-0.110
	(0.020)	(0.011)	(0.019)	(0.017)	(0.128)
FYRREMITTPLUS4	0.103	0.100	0.090	-0.043	0.190
	(0.043)**	(0.026)***	(0.043)**	(0.039)	(0.284)
FYRREMITTPLUS3	0.021	-0.031	-0.035	-0.045	0.504
	(0.038)	(0.023)	(0.038)	(0.034)	(0.251)**
FYRREMITTPLUS2	0.083	-0.029	-0.005	0.078	0.385
	(0.034)**	(0.021)	(0.034)	(0.031)**	(0.226)*
FYRREMITTPLUS1	-0.023	-0.023	-0.032	-0.031	0.270
	(0.030)	(0.018)	(0.030)	(0.027)	(0.200)
FYRREMITT	0.004	-0.021	-0.012	-0.041	0.089
	(0.029)	(0.018)	(0.029)	(0.026)	(0.197)
FYRREMITTMINUS1	-0.003	0.004	-0.001	-0.008	0.132
	(0.030)	(0.018)	(0.030)	(0.027)	(0.200)
FYRREMITTMINUS2	-0.037	-0.026	-0.034	-0.010	0.149
	(0.031)	(0.018)	(0.030)	(0.027)	(0.200)
FYRREMITTMINUS3	0.030	-0.024	-0.006	0.002	-0.238
	(0.031)	(0.019)	(0.031)	(0.028)	(0.207)
FYRREMITTMINUS4	0.008	-0.023	-0.038	0.010	-0.283
	(0.033)	(0.019)	(0.033)	(0.029)	(0.217)
Observations	2313	2456	2456	2456	2387
Number of i	213	213	213	213	205
R-squared	0.02	0.02	0.02	0.02	0.07
F-test, credit impact	0.12	0.48	0.15	0.07	0.12
(significance level)	0.725	0.489	0.696	0.784	0.724

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Estimations with Pre-Credit Credit Constraints
(Coefficients on remittances not included; NOPROGCREDMINUSX
is interacted pre-credit/no credit program access variable)

	(1)	(2)	(3)	(4)	(5)
	New block	New roof	New floor	New toilet	Cuerdas
FYRCREDITPLUS4	0.010	-0.001	-0.000	-0.043*	0.140
	(0.027)	(0.016)	(0.027)	(0.024)	(0.180)
FYRCREDITPLUS3	-0.003	-0.013	0.030	-0.018	0.138
	(0.023)	(0.014)	(0.023)	(0.021)	(0.153)
FYRCREDITPLUS2	0.015	0.002	0.022	-0.016	0.146
	(0.021)	(0.012)	(0.021)	(0.019)	(0.138)
FYRCREDITPLUS1	0.031	0.016	0.015	-0.018	0.069
	(0.018)*	(0.011)	(0.019)	(0.017)	(0.124)
FYRCREDIT	0.020	0.005	-0.000	0.008	0.094
	(0.018)	(0.011)	(0.018)	(0.016)	(0.117)
FYRCREDITMINUS1	0.027	-0.007	0.016	0.006	0.080
	(0.019)	(0.011)	(0.018)	(0.016)	(0.121)
FYRCREDITMINUS2	-0.029	-0.013	-0.005	-0.006	0.035
	(0.019)	(0.011)	(0.019)	(0.017)	(0.124)
FYRCREDITMINUS3	0.019	0.004	-0.018	0.008	0.078
	(0.020)	(0.012)	(0.020)	(0.017)	(0.129)
FYRCREDITMINUS4	-0.011	-0.006	-0.022	0.025	0.146
	(0.023)	(0.013)	(0.022)	(0.020)	(0.144)
NOPROGCREDMINUS1	-0.012	0.006	-0.033	-0.026	0.084
	(0.035)	(0.021)	(0.035)	(0.032)	(0.236)
NOPROGCREDMINUS2	0.024	0.036*	0.004	0.048*	-0.098
	(0.031)	(0.018)	(0.031)	(0.028)	(0.205)
NOPROGCREDMINUS3	-0.045	0.009	0.033	-0.035	-0.153
	(0.029)	(0.017)	(0.028)	(0.025)	(0.188)
NOPROGCREDMINUS4	0.002	0.012	0.053*	-0.034	-0.242
	(0.029)	(0.016)	(0.028)	(0.025)	(0.184)
F-test, no-program (δ's)	0.20	2.33	0.66	0.58	0.79
(significance level)	p = 0.655	p = 0.127	p = 0.418	p = 0.445	p = 0.375
Observations	2313	2456	2456	2456	2387
Number of i	213	213	213	213	205
R-squared	0.03	0.03	0.02	0.02	0.07

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%