Microcredit Impacts:
Evidence from a Randomized Microcredit Program Placement Experiment
by Compartamos Banco

June 2014

Manuela Angelucci, Dean Karlan, and Jonathan Zinman[*]

Abstract

We use a clustered randomized trial, and over 16,000 household surveys, to estimate impacts at the community level from a group lending expansion at 110 percent APR by the largest microlender in Mexico. We find no evidence of transformative impacts on 37 outcomes (although some estimates have large confidence intervals), measured at a mean of 27 months post-expansion, across six domains: microentrepreneurship, income, labor supply, expenditures, social status, and subjective well-being. We also examine distributional impacts using quantile regressions, given theory and evidence regarding negative impacts from borrowing at high interest rates, but do not find strong evidence for heterogeneity.

JEL Codes: D12; D22; G21; O12
Keywords: microcredit; microcredit impact; microentrepreneurship; Compartamos Banco

The initial promise of microcredit, including such accolades as the 2006 Nobel Peace Prize, has given way to intense debate about if and when it is actually an effective development tool. Expanded access to credit may improve the welfare of its recipients by lowering transaction costs and mitigating information asymmetries. Yet theories and empirical evidence from behavioral economics raise concerns about overborrowing at available rates, and microcredit debt traps have drawn much media and political attention in India, Bolivia, the United States, Mexico, and elsewhere. The possibility of positive or negative spillovers from borrowers to non-borrowers adds to the possibility of large net impacts in either direction.

Compartamos Banco (Compartamos) has been both praised (for expanding access to group credit for millions of people) and criticized (for being for-profit and publicly traded, and for charging higher interest rates than similar lenders do in other countries).[1] It is the largest microlender in Mexico and targets women who operate a business or are interested in starting one.[2] Using a clustered randomized trial that substantially expanded access to group lending through Compartamos Banco in north-central Sonora, Mexico, we provide evidence on impacts of expanded access to microcredit on credit use and a broad set of outcomes measured from household surveys and administrative data.

In early 2009 we worked with Compartamos to randomize its rollout into north-central Sonora State (near the Arizona border), an area it had not previously lent in. Specifically, we randomized credit access and loan promotion across 238 geographic "clusters" (neighborhoods in urban areas, towns or contiguous towns in rural areas). Treatment clusters received access to credit and door-to-door loan

---

[1] The rates are below average compared to both for-profit and non-profit microcredit market in Mexico, though they are higher than microlending rates in other countries and continents. For more on the public debate surrounding Compartamos, see http://www.businessweek.com/stories/2007-12-12/online-extra-yunus-blasts-compartamos.
[2] See http://www.compartamos.com/wps/portal/Grupo/InvestorsRelations/FinancialInformation for annual and other reports from 2010 onward.

promotion, whereas control clusters were not given access to credit and received no loan promotion. Compartamos verified addresses of potential loan recipients to maximize compliance with the experimental protocol.

The randomized program placement design used here (see also Attanasio et al. 2011; Banerjee et al. 2013; Crepon et al. 2011; Tarozzi, Desai, and Johnson 2013) has advantages and disadvantages over individual-level randomization strategies (e.g., Karlan and Zinman 2010; Karlan and Zinman 2011; Augsburg et al. 2012). Randomized program placement effectively measures treatment effects at the unit of randomization (in this case, the community level), which has the advantage of incorporating any *within*-community spillovers. These could in theory be positive (due to complementarities across businesses) or negative (due to zero-sum competition). Capturing spillovers with individual-level randomization is more difficult, but individual-level randomization can be done at lower cost, as it typically delivers a larger take-up differential between treatment and control, thereby improving statistical power for a given sample size.

Treatment assignment strongly predicts the depth of Compartamos penetration: according to Compartamos administrative data, 18.9 percent (1,563) of those surveyed in the treatment areas had taken out Compartamos loans during the study period, whereas only 5.8 percent (485) of those surveyed in the control areas had taken out Compartamos loans during the study.[3] Treatment assignment also predicts increased borrowing: there is a 5.1 percentage point (9 percent) increase in the likelihood of having any debt and a 1,157 pesos (18 percent) increase in outstanding debt. The likelihood of informal borrowing also increases modestly in treatment clusters (by 1.1 percentage point on a control group base of 5.1 percent).

---

[3] We surveyed likely borrowers, as detailed below in Section II.A. See footnotes in Section II.B for a description of why some control households gained access to loans.

This increased borrowing could plausibly produce mixed impacts in our setting. The market rate for microloans is about 100 percent APR, making concerns about overborrowing plausible. But existing evidence suggests that returns to capital in Mexico are about 200 percent for microentrepreneurs (McKenzie and Woodruff 2006; McKenzie and Woodruff 2008), and other studies find evidence suggesting high returns on investment in other household activities (Karlan and Zinman 2010; Dupas and Robinson 2013), making the hypothesis of business growth plausible.

Our outcome data comes from 16,560 detailed endline surveys of potential borrowers' households and businesses (see Section II for a description of the sample frame and Figure 1 for a timeline and sample frame summary). The average respondent assigned to treatment was surveyed 27 months after Compartamos began operations in her neighborhood; 90 percent of respondents assigned to treatment were surveyed between 17 and 35 months post-expansion.[4] Surveyors were employed by an independent firm with no ties to Compartamos or knowledge of the experiment. We estimate average intent-to-treat effects on 37 outcomes spanning six outcome families: microentrepreneurship (seven outcomes), income (four outcomes), labor supply (three outcomes), consumption (eight outcomes), social status (seven outcomes), and subjective well-being (eight outcomes).

Our results suggest that Compartamos' expansion had modest effects on some downstream outcomes. Twelve of the 37 estimated average intent-to-treat effects, adjusted for multiple hypothesis testing, are statistically significant with at least 90 percent confidence. We find evidence that households in treatment areas grow their businesses (both revenues and expenses increase), but we find no corresponding effects on business profits, entry, or exit. We find no evidence of statistically significant treatment effects on household income or labor supply.

---

[4] Exposure is defined as the length of time between the day that the first loan in the respondent's cluster was taken out and the day that the respondent was surveyed at endline.

Treatment effects on most measures of spending are not statistically significant (albeit noisily estimated), although we do find some evidence that asset and temptation purchases decline. This result is consistent with lumpy investment in businesses that require additional financing beyond that provided by marginal loans, or with a reduction in asset "churn".[5] We find evidence of modest increases in female intra-household decision-making power but no evidence of effects on intra-household conflict.

The economic magnitudes of even the statistically significant effects are likely less than transformative. Although scaling up our intent-to-treat to treatment-on-the-treated estimates requires some assumptions,[6] it seems plausible that our confidence intervals do not contain average effects on borrowers of larger than plus or minus 1 standard deviation, roughly speaking.[7] The confidence intervals on outcomes that are not statistically significant by and large do not contain effects on borrowers—again, roughly speaking—of larger than plus or minus 0.4 standard deviations. So although we cannot rule out some nontrivial (i.e., economically significant) effects, we do infer that the data cast doubt on the hypothesis of large average transformative positive or negative effects.

To examine the extent of heterogeneous effects, we estimate quantile treatment effects and show that for most outcomes, we do not find any noticeable pattern across the distribution. However, for revenues, profits, and household decision-

[5] Indeed, we find some evidence of a reduction in asset sales to service debt, suggesting that microcredit enables households to avoid costly fire sales.

[6] In particular, one ought to be concerned with violating the exclusion restriction due to externalities (e.g., through changes in risk-sharing, hiring, or informal lending from treated to untreated individuals in treatment communities). Furthermore, one ought to be concerned with treatment heterogeneity, wherein those who take-up have higher returns to capital than those who do not (Beaman et al. 2014).

[7] Figure 2 shows that, with one exception, the confidence intervals on our average intent-to-treat (AIT) estimates for 37 more-ultimate outcomes across six domains—self-employment, income, labor supply, expenditures, social, and other welfare—do not contain effect sizes >|0.125| standard deviations. Under the assumptions referenced in the footnote above, one can infer a treatment-on-the-treated effect by scaling an AIT by the reciprocal of the treatment rate between treatment and control: by 1/0.13, or about 8.

making power, we do find stronger effects at the upper end of the distribution. Treatment effects on happiness and on trust in people increase throughout their distributions. Importantly, there is limited evidence of negative impacts in the left tails of the outcome distributions, alleviating (but not dismissing) concerns that expanded credit access might adversely affect people with the worst baseline outcomes.

Our results come with several caveats. Many of the null intent-to-treat results have confidence intervals that include economically meaningful effect sizes, particularly if one were to scale up our intent-to-treat estimates to infer treatment-on-the-treated effects. The lack of highly precise nulls, despite the relatively large sample size (compared to many evaluations), is likely due to some combination of the modest take-up differential between treatment and control areas, heterogeneous treatment effects, and high variance and measurement error in outcomes. Cross-cluster spillovers could bias our estimates in an indeterminate direction. Focusing on mean impacts of expanded credit access ignores the potential existence of heterogeneous effects: our null results may be consistent with the hypothesis that some people benefit and others are hurt from access to loans. Finally, the external validity of our findings to other settings and lending models is uncertain: theory and evidence do not yet provide much guidance on whether and how a given lending model will produce different impacts in different settings, with varying demographics, competition, etc.; however, recent complementary randomized trials in microcredit are finding similar results (Attanasio et al. 2011; Augsburg et al. 2012; Banerjee et al. 2013; Crepon et al. 2011; Karlan and Zinman 2010; Karlan and Zinman 2011; Tarozzi, Desai, and Johnson 2013).

I. Background on the Lender, Loan Terms, and Study Setting

A.   *Compartamos and its Target Market*

The lender, Compartamos Banco, is the largest microlender in Mexico, with 2.3 million borrowers.[8] Compartamos was founded in 1990 as a nonprofit organization. It was later converted to a commercial bank in 2006 and went public in 2007. As of November 2012, it had a market capitalization of US$2.2 billion. In 2012, 71 percent of Compartamos clients borrowed through *Crédito Mujer*, the joint liability microloan product studied in this paper.

Crédito Mujer nominally targets women who have a business or self-employment activity or intend to start one. Empirically, we estimate that only about 51 percent of borrowers are "microentrepreneurs".[9] Borrowers tend to lack the income and/or collateral required to qualify for loans from commercial banks and other "upmarket" lenders.

B.   *Loan Terms*

Crédito Mujer loan amounts during most of the study range from 1,500- 27,000 pesos (12 pesos=US$1), with amounts for first-time borrowers ranging from 1,500-6,000 pesos (US$125-US$500) and larger amounts subsequently available to members of groups that have successfully repaid prior loans.[10] The mean loan amount in our sample is 6,462 pesos, and the mean first loan is 3,946 pesos. Loan repayments are due over 16 equal weekly installments and are guaranteed by the group (i.e., joint liability). There is no collateral associated with the loans. Interest rates on *Crédito Mujer* loans are about 110 percent APR during our study period.

---

[8] According to Mix Market, http://www.mixmarket.org/mfi/country/Mexico, accessed 22 August 2012.

[9] We define microentrepreneurship here as currently or ever having owned a business, and use our endline survey data, including retrospective questions, to measure it.

[10] Also, beginning in weeks 3 to 9 of the second loan cycle, clients in good standing can take out an additional, individual liability loan, in an amount up to 30 percent of their joint liability loan.

For loans of this size, these rates are in the middle of the market for Mexico (nonprofits charge comparable rates).[11]

C.   *Targeting, Marketing, Group Formation, and Screening*

Crédito Mujer groups range in size from 10 to 50 members. When Compartamos enters a new market, as in this study, loan officers typically target self-reported female entrepreneurs and promote the *Crédito Mujer* product through diverse channels, including door-to-door promotion, distribution of fliers in public places, radio broadcasts, and promotional events. In our study, Compartamos conducted door-to-door promotion only in randomly assigned treatment areas (see Section II). Once loan officers have a sufficient number of clients in new areas, they promote less frequently and rely more on existing group members to recruit other members.

When a group of about five women—half of the minimum required group size—expresses interest in *Crédito Mujer*, a loan officer visits the women at one of their homes or businesses to explain loan terms and processes. These initial women are responsible for finding other group members. Once potential members are identified, the loan officer returns for a second visit to explain loan terms in greater detail and complete loan applications for each individual. All potential members must be between the ages of 18 and 60 and present a proof of address and valid identification to qualify for a loan. Business activities (or plans to start one) are not verified; rather, Compartamos relies on group members to screen out poor credit risks. In equilibrium, potential members who express an interest and attend the meetings are rarely screened out by their fellow members, since individuals who would not get approved are neither approached nor seek out membership in the group.

---

[11] See http://www.cgdev.org/blog/compartamos-context for a more detailed elaboration of market interest rates in 2011 in Mexico.

Compartamos reserves the right to reject any applicant put forth by the group but relies heavily on the group's endorsement. Compartamos pulls a credit report for each individual and automatically rejects anyone with a history of fraud, but beyond that, loan officers do not use the credit bureau information to reject clients, as the group has responsibility for deciding who is allowed to join.

Applicants who pass Compartamos' screens are invited to a loan authorization meeting. Each applicant must be guaranteed by every other member of the group to get a loan. Loan amounts must also be agreed upon unanimously. Loan officers moderate the group's discussion and sometimes provide information on credit history and assessments of individuals' creditworthiness. Proceeds from authorized loans are disbursed as checks to each client.

D.   *Group Administration, Loan Repayment, and Collection Actions*

Together, each lending group decides where to meet, chooses the channel of repayment (e.g., local convenience store or agent bank), creates a schedule of fines for late payments, and elects leadership for the group, including a treasurer, president, and secretary. To promote group solidarity, Compartamos requires groups to choose a name for themselves, keep a plant to symbolize their strength, and take a group pledge at the beginning of each loan.

The treasurer collects payments from group members at each weekly meeting. The loan officer is present to monitor and assist the group but does not collect any money. If a group member does not make her weekly payment, the group president (and loan officer) will typically encourage "solidarity" pooling to cover the payment and keep the group in good standing. All payments are placed in a

plastic bag that Compartamos provides, and the treasurer deposits the group's payment at either a nearby bank branch or convenience store.[12]

Beyond the group liability, borrowers have several other incentives to repay. Members of groups with arrears are not eligible for another loan until the arrears are cured. Members of groups that remain in good standing qualify for larger subsequent loan amounts, and for interest rates as low as 2.9 percent monthly (compared to 3.89 percent on first loans).[13] Additionally, Compartamos reports individual repayment histories for each borrower to the Mexican Official Credit Bureau. Loans that are more than 90 days in arrears after the end of the loan term are sent to collection agencies. Nevertheless, late payments are common: using nationwide data from Compartamos, Karlan and Zinman (2014) find a 90-day group delinquency rate of 9.8 percent. However, the ultimate default rate is only about 1 percent.

E.   Study Setting: North-Central Sonora, 2009-2012

We worked with Compartamos to identify an area of Mexico that it planned to enter but had not yet done so. The bank selected the north-central part of the State of Sonora, which includes Nogales, Caborca, Agua Prieta, and their surrounding towns. The study area borders Arizona to the north, and its largest city, Nogales (on the U.S. border), has a population of roughly 200,000 people. The area contains urban, peri-urban, and rural settlements.

To understand the market landscape, we examine post-expansion data from our endline survey. We use the endline rather than the baseline because the endline covers our entire study area – we were only able to conduct successful baseline

---

[12] Compartamos has partnerships with six banks (and their affiliated convenience stores) and two separate convenience stores. The banks are Banamex (Banamexi Aquí), Bancomer (Pitico), Banorte (Telecomm and Seven Eleven), HSBC, Scotiabank, and Santander. The convenience stores are Oxxo and Chedraui.

[13] To determine the exact interest rate, Compartamos considers the number of group members, punctuality, willingness to pay, and group seniority.

surveys in the outlying areas of Nogales. 54 percent of respondents in the control group report that they, or a member of their household, received at least one loan in the previous two years, and 75 percent of their loan funds come from a formal institution. Average total borrowing in control group households is 6,493 pesos, or roughly US$541[14]. In control clusters, the most prevalent lenders are all considered close competitors of Compartamos: Bancoppel (12.1 percent of all loan funds, average loan size of 782 pesos), Banco Azteca (9.3 percent, 604 pesos) and Financiera Independencia (5.4 percent, 351 pesos). Moneylenders (0.7 percent, 44 pesos) and pawnshops (0.4 percent, 26 pesos) make up a small fraction of the market. Other prevalent sources of credit are the government (8.4 percent, 544 pesos) and trade credit (11.7 percent, 759 pesos).

II. Research Design, Implementation, and Data

A. Design Overview

Our analysis uses a randomized cluster encouragement design, with randomization of access to credit assigned by neighborhood (for urban areas) or by community (for rural areas). Our sample is composed of two frames: the "panel" sample frame contains 33 clusters in the outlying areas of Nogales and has both baseline and endline surveys, and the "endline-only" sample frame contains 205 clusters and has only endline surveys. Figure 1 depicts the timeline of surveying and treatment.

Household identification and selection was concurrent with surveying rounds: it occurred between April and June 2010 for the panel sample and between November 2011 and March 2012 for the endline only sample.

Both baseline and endline surveys were administered to potential borrowers: women between the ages of 18 and 60 who answered yes to any of three questions: (1) "Do you have an economic activity or a business? This can be, for

---

[14] See Appendix Table 2 for how total borrowing (labeled "Total amount" in Table 2b) is defined.

example, the sale of a product like cosmetics, clothes, or food, either through a catalogue, from a physical location or from your home, or any activity for which you receive some kind of income"; (2) "If you had money to start an economic activity or a business, would you do so in the next year?"; (3) "If an institution were to offer you credit, would you consider taking it?"

The endline survey was administered to 16,560 respondents, approximately 2-3 years after Compartamos' entry. We make only limited use of the baseline survey in this paper, using it to check whether baseline characteristics are orthogonal to treatment assignment and attrition, and to control for baseline outcomes when data is available (while controlling for missing values of the baseline outcome variable).[15]

B. Experimental Design and Implementation

In March 2009, the research team divided the study area into 250 geographic clusters, with each cluster being a unit of randomization. In rural areas, a cluster is typically a well-defined community (e.g., a municipality). In urban areas, we mapped clusters based on formal and informal neighborhood boundaries. We then grouped the urban clusters (each of which are located within the municipal boundaries of Nogales, Caborca, or Agua Prieta) into "superclusters" of four adjacent clusters each.[16] Half the clusters were randomly assigned to receive direct promotion and access of *Crédito Mujer*, while the other half would not receive any loan promotion or access until study data collection was completed. This randomization was stratified on superclusters for urban areas and on branch

---

[15] We will use the baseline more extensively in a companion paper on distributional and heterogeneous effects.

[16] We plan to use these superclusters to estimate spillovers from treatment to control in future work, by examining whether treatment versus control differences are smaller in high-intensity areas than in low-intensity areas.

offices in rural areas (one of three offices had primary responsibility for each cluster).[17]

Violence prevented both Compartamos staff and third-party surveyors from entering certain neighborhoods to promote loans and conduct surveys, respectively. We set up a decision rule that was agnostic to treatment status and determined solely by the survey team with respect to where they felt they could safely conduct surveys. The survey team dropped 12 clusters (five treatment and seven control), producing a final sample frame of 238 geographic clusters.

Table 1 verifies that our endline survey respondents are observably similar across treatment and control clusters, focusing on variables that do not change or are unlikely to have changed due to treatment, such as age and adult educational attainment. Column 2 presents tests of orthogonality between each variable and treatment status. Only one of the six variables, age, is significantly different across treatment and control. The difference, half a year, is substantively small. Column 3 reports the result of an F-test that all coefficients for the individual characteristics are zero in an OLS regression predicting treatment assignment. The p-value is 0.317. We find similar evidence of orthogonality in our panel sample (Appendix Table 1), which is smaller but has many more variables we can use to check orthogonality, given the availability of baseline survey data.

In April 2009, Compartamos began promoting loans and offering access to credit in treatment clusters on a rolling basis. Endline surveys were conducted between November 2011 and March 2012. For this study period, Compartamos established an address verification step that required individuals to live in treatment areas in order to get loans, and only actively promoted its lending in treatment clusters. This led to an 18.9 percent take-up rate in treatment clusters and a 5.8 percent

---

[17] In urban areas, branches are completely nested in superclusters; i.e., any one supercluster is only served by one branch.

take-up rate in control clusters.[18] All analysis will be intent-to-treat on respondents surveyed, not just on those who borrowed in the treatment clusters. Thus, while attrition may affect the external validity of our findings, it does not seem to bias the estimates of our treatment effects.

C. Partial Baseline and Full Endline

After an initial failed attempt at a baseline survey in 2008,[19] we capitalized on a delay in loan promotion rollout to 33 contiguous rural clusters (16 treatment and 17 control) on the outskirts of Nogales to do a baseline survey during the first half of 2010. For sampling, we established a target number of respondents to survey in each cluster, based on its estimated population of females between the ages of 18 and 60 (from Census data) who would have a high propensity to borrow from Compartamos: those who either had their own business, would want to start their own business in the following year, or would consider taking out a loan in the near future. We then randomly sampled up to the target number in each cluster, for a total of 6,786 baseline surveys.[20] After the baseline was completed, Compartamos began operations in these treatment clusters beginning in June 2010 (i.e., about a year after they entered the other treatment clusters).

---

[18] Control households that did borrow from Compartamos were likely able to because of ambiguous addresses or multiple viable addresses (e.g., using an address from someone in their extended family or using a work address).

[19] We were unable to track baseline participants successfully, and in the process of tracking and auditing discovered too many irregularities by the initial survey firm to give us confidence in the data. It was not cost-effective to determine which observations were reliable, relative to spending further money on an expanded endline survey and new baseline survey in areas still untouched by Compartamos. Thus we decided to not use the first baseline for any analysis.

[20] For the baseline, we conducted a census of each panel cluster, knocking on each door and surveying each woman that met our criteria. We returned several times if necessary in order to minimize non-response bias. For the endline, our sampling method was as follows: In Agua Prieta, assuming three surveys per block, we randomly chose blocks such that we would reach the target number of surveys for each cluster. Surveyors started in the northwest corner of the block and employed a skip-three-houses pattern, continuing the pattern if houses were not reached, until three surveys were completed per block. This was deemed inefficient in terms of time between surveys. Therefore, for the remaining regions (Caborca and Nogales), we changed the sampling strategy: every block was visited, and we varied the number of surveys per block according to population density, such that the target number of surveys per block was reached. Skips and substitution rules remained the same.

All targeted respondents were informed that the survey was a comprehensive socioeconomic research survey being conducted by a non-profit organization (Innovations for Poverty Action) in collaboration with the University of Arizona (the home institution of one of the co-authors at the time of the survey). Neither the survey team nor the respondents were informed of the relationship between the researchers and Compartamos.

The survey firm then conducted an endline survey between November 2011 and March 2012. This timing produced an average exposure to Compartamos loan availability of 16 months in the clusters with baseline surveys. In those clusters, we tracked 2,912 respondents for endline follow-up. In the clusters without baseline surveys, we followed the same sampling rules used in the baseline, and the average exposure to Compartamos loan availability was 28 months. In all, we have 16,560 completed endline surveys; 1,823 respondents have both baseline and endline surveys and make up our panel sample.

Our main sample is the full sample of 16,560 endline respondents. Their characteristics are described in Table 1, Column 1. Relative to the female Mexican population aged 18-60, our sample has a similar age distribution (median 37), is more educated (29 percent primary or less vs. 37 percent), rural (27 percent vs. 22 percent) and married (75 percent vs. 63 percent), and has more occupants per household (4.6 vs. 3.9).[21] Given the available endline variables conceivably unaffected by the treatment—age, education, marital status, and prior business and loan experience—we fail to predict loan take-up in our data (the adjusted R-squared is only 4.1 percent in the entire endline). Therefore, we do not attempt to predict take-up in order to create a smaller sample frame with higher participation rates.

---

[21] Source: Instituto Nacional de Estadìstica y Geografìa. 2010. "Demografìa y Poblaciòn." Accessed 22 March 2013 from http://www3.inegi.org.mx/.

D. Attrition

We use the panel sample to study attrition. When we embarked on the baseline for the panel, we purposefully surveyed a large number of households with the intent to use baseline data to predict who would borrow, and then oversample those with likelihoods above a certain threshold in the endline, thus increasing statistical power. As we described above, our baseline data do not predict take-up well. We thus reverted to drawing the endline using identical sampling strategy (i.e., a target number of surveys per cluster) in the endline-only and panel areas. For this reason, we attempted to track at endline 2,912 households out of the full baseline sample of 6,786 households. We identified 63 percent of this group, which comprises 1,823 households and yields an attrition rate of 37 percent. In Appendix Table 1, we use the 2,912 households that we attempted to track from the baseline to test whether attrition correlates with observed characteristics or differs by treatment assignment. After showing that the panel data are balanced at baseline (Columns 1-3) we show that, although attrition is not random—the probability of being in the endline is positively correlated with age, being married, and prior business ownership, and negatively correlated with income and formal account ownership (Column 4)—neither the rate of attrition (Column 5) nor the correlates of attrition (Column 6) systematically differ in control and treatment areas. The F-test of joint significance of treatment and baseline variables interacted by treatment produces a p-value of 0.179.[22,23]

---

[22] Unfortunately, we cannot directly address whether sample frame eligibility is affected by treatment status because we did not save data from respondents who were screened out on the eligibility criteria questions; i.e., if someone answered "no" to all of the three questions, then they do not show up as observations in our raw data. Therefore, we cannot estimate effects on the extensive margin of our sample frame. However, since someone is eligible for sampling if they answer yes to any of the three eligibility criteria questions, we can estimate treatment effects on the intensive margins of sample frame eligibility; i.e., on each of the variables individually. We do this on our panel sample by regressing each of the three eligibility criteria variables on treatment status, a survey iteration dummy (1= endline, 0=baseline), and the interaction between treatment status and survey iteration. The interaction terms have p-values of 0.31, 0.17, and 0.28.

[23] Note that we also have within-sample attrition, i.e., partially completed surveys. The number of missing observations is detailed in each column of the results tables. To address any concerns related to nonresponders being inherently different from responders, we test the hypothesis that

E. Estimating Average Intent-to-Treat Effects

We use survey data on outcomes to study the effect of providing access to *Crédito Mujer*. To do so, we estimate the average intent-to-treat (AIT) parameters of the following equation:

(1) $Y_{ics} = \alpha + \beta T_c + \mathbf{X}_s + \gamma Z_{ics} + e_{ics}$

The variable Y is an outcome (or summary index of outcomes, following Kling, Liebman, and Katz 2007), for person i in cluster c and supercluster s. We code Y's so that higher values are more desirable, all else equal. The Data Appendix details the survey questions that we use to measure each outcome. T is a binary variable that is 1 if respondent i lives in a treatment cluster c ("lives" defined as where she sleeps); $\mathbf{X}$ is a vector of randomization strata (supercluster fixed effects, where the superclusters are nested in the bank branches); and Z is the baseline value of the outcome measure, when available.[24] We cluster the standard errors at the cluster

---

treatment effects systematically differ for the 3,815 respondents with some missing outcome variables. To do that, we estimate a version of the equation below:

(2) $Y_{ics} = \beta_0 + \beta_1 T_c * Complete + \beta_2 T_c * Incomplete + \beta_3 Incomplete + \mathbf{X}_s + \beta_4 Z_{ics} + e_{ics}$

The variable Y is an outcome (or summary index of outcomes, following Kling, Liebman, and Katz (2007)), for person i in cluster c and supercluster s. T is a binary variable that is 1 if respondent i lives in a treatment cluster c ("lives" defined as where she sleeps); *Complete* is a dummy that takes the value of 1 if a respondent provides complete information for all outcomes of interest, and is 0 otherwise; *Incomplete* is a dummy that takes the value of 1 if a respondent does not provide complete information for all outcomes, and is 0 otherwise; $\mathbf{X}$ is a vector of randomization strata (supercluster fixed effects, where the superclusters are nested in the bank branches); and Z is the baseline value of the outcome measure, when available. If a control was added for the baseline value of the outcome, any missing values for the baseline observation of the outcome were coded as zero and a variable was added that is equal to one if the baseline value is missing and zero otherwise. Finally, we cluster the standard errors at the cluster level c, the unit of randomization. Under tests of equality (adjusted for critical values), β_1 and β_2 are statistically different from one another in 7 out of 55 cases at q < 0.10. This is roughly the number of significant differences we would expect due to chance (5-6). We therefore conclude that (non)response patterns do not bias our estimates.

[24] Adding controls for survey date does not change the results.

level c, the unit of randomization.[25]

F. Dealing with Multiple Outcomes

We consider multiple outcomes, some of which belong to the same "family" in the sense that they proxy for some broader outcome or channel of impact (e.g., we have several outcomes that one could think of as proxies for business size: revenues, expenditures, and profits). This creates multiple inference problems that we deal with in two ways. For an outcome family where we are not especially interested in impacts on particular variables, we create an index—a standardized average across each outcome in the family—and test if the overall effect of the treatment on the index is zero (see Kling, Liebman, and Katz 2007). For outcome variables that are interesting in their own right but plausibly belong to the same family, we present both unadjusted and adjusted p-values using the False Discovery Rate (FDR) approach (Benjamini and Hochberg 1995). In general, however, adjusting the p-values does not change the statistical significance of individual estimates.

III. Main Results

In tracking our results, note that sample sizes vary across different analyses due to item non-response or the use of sub-samples conditioned on the relevance of a particular outcome (e.g, decision power questions were only asked of married respondents living with another adult). The Data Appendix provides additional details.

We group outcomes thematically, by outcome "family". Tables 2-8 provide details on the results for each outcome family, while Figure 2 summarizes all the results. Before describing the average intent-to-treat (AIT) effect on any specific outcome, we note that, out of 37 parameters estimated on microenterpreneurship,

---

[25] When we cluster regressions at the supercluster level, several variables gain or lose a star, but few gain or lose significance as a whole. The exceptions to this are "Monthly household income from government subsidies or aid" (Table 5), which loses significance entirely, and "Good health status" (Table 8), which becomes significant at the 10 percent level.

income, labor supply, expenditures, social status, and subjective well-being, the AIT estimates are statistically significant at the 90 percent level (at least) for 12 outcomes. For these estimates, the upper bound of the 95 percent confidence interval is generally below 0.1 standard deviations, which, naively scaled up by a factor of 8, suggests that the average effect of the treatment on borrowers is small to medium. For the remaining 25 outcomes whose AIT estimates are not statistically significant, the confidence intervals often range between plus and minus 0.05 standard deviations. Multiplied by 8, in most cases they cannot rule out either positive or negative small effects (e.g. plus or minus 0.2 standard deviations), although they rule out medium and large effects (e.g. plus or minus 0.5 and 0.8 standard deviations).

A. Credit

Table 2a and the top panel of Figure 2 present AIT estimates for several measures of the extensive margin of borrowing. Panel A, Column 1 shows a 6.9 percentage point (pp) increase in the likelihood of borrowing from any MFI, on a control mean base of 13.8 percent. Panel A, Columns 2 and 3 show 11.5pp and 8.2pp increases in the likelihood of ever having borrowed from Compartamos, measured using administrative and survey data, respectively.[26]

Panel B, Columns 1-3 show no effects on measures of borrowing from other (non-Compartamos) formal sector sources. The 95 percent confidence intervals rule out effects that are large in absolute terms, but not effects that are about 10 percent changes from the control group means for other MFIs and banks. The lack of

---

[26] The administrative and survey measures of borrowing from Compartamos are not strictly comparable for several reasons. First, the recall period in the Compartamos data is different: longer in most cases and shorter in others (we could not get data prior to April 2009, meaning that some recalls are shorter than the two years used in the survey). Second, borrowing is underreported in surveys (Karlan and Zinman 2008): 22 percent of borrowers who we know, from administrative data, to have borrowed from Compartamos during the previous two years report no borrowing from Compartamos over the previous two years. Third, the Compartamos administrative data identifies only survey respondents, while the survey data includes borrowing by respondents and/or other household members.

crowd-out we observe may be attributed to the fact that Compartamos offers loans with interest rates comparable to other MFIs.[27]

Panel B, Column 4 shows a 1.1pp, or 21 percent, increase in the likelihood of any informal borrowing.[28] This is consistent with the Compartamos expansion not fully relaxing credit constraints, and hence crowding-in other borrowing to some extent, and/or with the uses of Compartamos loans not "paying for themselves"— not producing increased income—for some borrowers, who then need to borrow from other sources to pay off the Compartamos debt.

Panel B, Column 6 shows a 5.1pp increase in the likelihood that the household borrowed at all during the past two years (on a base of 0.537). Panel B, Column 7 shows a 1.1pp increase in the likelihood of making a late Compartamos loan payment (measured from administrative data). Note that this treatment effect includes non-borrowers and hence is driven mechanically by the greater likelihood of households in the treatment group borrowing from Compartamos.

Table 2b and the second panel of Figure 2 paint a similar picture with respect to loan amounts. These variables are not conditioned on having borrowed and hence are well-identified; the effects here combine the extensive and intensive margins of borrowing. We see a large and statistically significant increase in the amount borrowed from any MFI (574 pesos, se=101, on a base of 1,052) and from Compartamos (629 pesos, se=74, on a base of 280), no statistically significant

---

[27] We rely on David Roodman's calculations to compute the nominal APR for Mexican MFIs in 2009 (see http://www.cgdev.org/blog/compartamos-context). We derive this APR by converting the weekly rate before VAT to an annual interest rate. The ten largest MFIs by loan volume (which includes Compartamos) have a mean nominal APR of 132.4 percent. Compartamos, by comparison, has a calculated rate of 115.8 percent in the data. Note that the calculated rates of other MFIs assume that all loan terms are identical to Compartamos' *Crédito Mujer* product, with the exception of the interest rate.

[28] The survey prompted for money owed to specific informal lender types—moneylenders, pawnshops, and friends and relatives—so the low prevalence of informal borrowing in our sample is not simply due to respondent (mis)conceptions that money owed to these sources is not a "loan."

effects on borrowing from other formal sources (Columns 3-5), and some evidence of crowd-in overall (Column 8): the point estimate on total amount borrowed (1,157 pesos, se=456, on a base of 6,493) is nearly twice that of the point estimate on Compartamos borrowing, although the two point estimates are not statistically different from one another.

Overall, the results on borrowing suggest a statistically significant increase that is driven by Compartamos borrowing. There is some evidence of crowd-in, particularly with respect to informal borrowing (on the extensive margin), although the results on borrowing amounts do not rule out crowd-in of other formal sources.

B. Self-Employment Activities

Table 3 and the self-employment panel of Figure 2 show the AIT estimates for self-employment activities. The first two columns show growth in business size: revenues and expenses during the past two weeks increase by 121 pesos (27 percent) and 119 pesos (36 percent), respectively (se's 52 and 47).[29] Therefore, we find no effect on profits, although this null result is imprecisely estimated (see also Table 4, Column 1 for a null result on household business income). Columns 4-7 suggest that the growth in business size comes from growth in pre-existing businesses: we find no effect on the number of businesses, or on any of several extensive margins (has a business, has started a business within the last 12 months, no longer has a business).[30] The confidence intervals in Columns 4-7 rule out effect sizes that are large in absolute terms but do not rule out effects that are as large as 18 percent changes from the control group means.

---

[29] We ask about the last two weeks to minimize measurement error from longer recall periods.

[30] Respondents identified whether they currently had a business by responding to the following prompt: "How many businesses or economic activities do you currently have? It can be, for example, the sale of a product or food, either through catalogue, in an establishment or in your home." Fewer than 10 percent of owners have multiple businesses.

In all, the results on business outcomes suggest that expanded credit access increased the size of some existing businesses, but had no effect on business ownership or profits.

C. Household Income

Table 4 and the "Income" panel of Figure 2 examine additional measures of income, each elicited from questions about different sources of earnings during the prior month: business, labor, remittances, and aid. The motivation for examining these measures is twofold. Methodologically, any individual measure of income, wealth, or economic activity is likely to be noisy, so it is useful to examine various measures. Substantively, there is prior evidence of microloan access increasing job retention and wage income (Karlan and Zinman 2010), and there is reason to believe that credit access might increase self-reliance (which could reduce reliance on third-party aid) and/or finance investments in migration (which could pay off in the form of remittances).[31]

We do not find statistically significant effects on business income, labor income, and remittances, which have point estimates of 58, -30, and -19 pesos (se's=64, 128, 28). However, the confidence intervals cannot rule out large effect sizes on business income (upper bound of a 22 percent increase) and remittances (upper bound of a 23 percent decrease). Conversely, the bounds of the effects on labor income are smaller, around a 5 percent change over the mean in control areas. In Column 4, we do find a statistically significant reduction in income from government or other aid sources.[32] The point estimate is -16 pesos (se=7), a

---

[31] For example, Angelucci (2013) finds that giving cash transfers to poor households in rural Mexico increases international migration because the entitlement to the cash transfers increases access to loans by providing collateral.

[32] Specifically, the survey question asked about government aid and gave four examples, three of them potentially relevant for this population: Oportunidades, a conditional cash transfer program that targets based on poverty; Sumate, a regional program from 2009-2012 which provided construction materials for house improvements such as cement, or vouchers to affiliated supermarkets, office/paper supply stores, medical services or water utilities; 70 y Mas, a cash transfer to those over 70, and thus unlikely relevant based on our filter; and scholarships.

modest size relative to total household income but an 17 percent decrease relative to the control group mean.

Lastly, note that the effects in Columns 1-4 roughly sum to zero. In short, Table 4 suggests that any increase in business income may have been offset by reductions in income from other sources.

D. Labor Supply

To complement our analysis of impacts on income, Table 5 and the "Labor Supply" panel of Figure 2 report AIT effects on three measures of labor supply: any participation by the respondent in an economic activity, fraction of children 4-17 working, and number of family members employed in the respondent's business(es). We do not find any statistically significant treatment effects. The 95 percent confidence interval of the coefficient on treatment for participation in an economic activity ranges from -0.030 to 0.008. The confidence interval for fraction of children working has a minimum of -0.020 and a maximum of 0.005, ruling out even small positive effects on child labor. The confidence interval for the number of family member employees ranges from -0.014 to 0.024.

E. Assets/Expenditures

Table 6 and the "Assets and Expenditures" panel of Figure 2 report AIT effects on measures of household assets and recent spending over various time horizons. In theory, treatment effects on these variables could be either positive or negative. Loan access might increase recent expenditures through, e.g., income-generation that leads to higher overall spending.[33] On the other hand, loan access might lead to declines in spending through a number of pathways. If loans primarily finance short-term consumption smoothing or durable purchases, these loans may then be repaid at the expense of longer-term consumption. Marginal investments may

---

[33] Although we do not find effects on income above, it is important to keep in mind that the null results are noisy, so one might detect (income) effects on spending even in the absence of detecting effects on income itself.

require funding above and beyond what can be financed with Compartamos loans (lumpy investment), leading marginal borrowers to cut back on spending as well. Finally, loan recipients may "overborrow" on average, making bad investments (broadly defined) with the loan proceeds.

The first two columns of Table 6 present estimates of effects on fixed asset purchases (for home and/or business). Our survey only asks about whether and which types of assets were bought (or sold) during the previous two years, not the amount or value of those assets. This thus means that we cannot distinguish, for example, people buying fewer but larger assets versus more but smaller assets. To estimate asset values (Column 2), since we do not ask about value directly for all purchases, we instead use data on assets bought with a loan, when the respondent reported taking out a loan to pay for the item. We find the mean value of assets bought with a loan in each of six asset categories. We then sum across these category means to find a respondent's total value of assets.[34] The estimate assumes that no more than one asset was purchased from each category and that purchase prices do not vary with the use of borrowed versus non-borrowed funds. These are risky assumptions, and as such we view the asset value result as merely suggestive, and should be interpreted with caution. The most common assets we see purchased are furniture, electronics, and vehicles. Column 1 shows a statistically significant 9 percent decrease in the number of asset categories purchased from (of six) in the previous two years: a -0.047 change (se=0.022)

---

[34] When generating sum variables, there are two types of missing values we are concerned with: nonresponse and correctly skipped values. If the components of a sum variable are all nonresponse, then the sum variable is missing. If the components of a sum variable are all correctly skipped, then the sum variable equals 0. Both nonresponse and correctly skipped values are treated approximately as equivalent to 0: they do not alter the total. The exception to this is if the components are all missing and at least one is nonresponse: then the sum is missing.

By counting component nonresponse values as zeroes, we introduce nonclassical measurement error, as our point estimates are lower than the true effects (assuming all positive values, like for income or consumption). In order to examine the extent of nonclassical measurement error, we rerun our analysis but recode all sum variables to missing if at least one of their components is a nonresponse. In this supplementary analysis, no estimate went from significant to not, or vice versa, although a couple of estimates gained more stars.

from a control group mean of 0.502. Column 2 shows a statistically significant 18 percent drop in the value of purchased assets: a -1,534 pesos change (se=598) from a control group mean of 8,319 pesos. In addition to the mechanisms described above for negative treatment effects on spending, there is another mechanism to consider here: a reduction in asset "churn". We find some evidence consistent with this mechanism and discuss it in Section IV.

Columns 3-8 present results for six weekly expenditure categories: non-durables, food, medical, school, family events, and temptation goods (cigarettes, sweets, and soda). These are measured using questions with recall periods of one week (non-durables, food, and temptation goods), two weeks (food), one month (non-durables), or one year (medical, school, and family). The only statistically significant result is a small (6 pesos and 6 percent) reduction in temptation goods (cigarettes, sweets, and soda) purchased during the past week. Banerjee et al. (2009) attribute their similar finding to household budget tightening required to service debt (i.e., temptation spending is relatively elastic with respect to the shadow value of liquidity). Alternative explanations are that female empowerment (discussed below in Table 7) leads to reduced spending on unhealthy items, and/or that greater self-reliance and discipline in one domain (e.g., business investment) leads to greater willpower in other domains (Baumeister and Tierney 2011). The null results on the other spending categories are noisy, with the exception of food, where the upper bound of the confidence interval implies a change of less than 4 percent.

F.   Social Indicators

Table 7 examines treatment effects on indicators of family and social interactions and/or allocations. The first column shows a small increase in school enrollment for children aged 4 to 17, with an effect size of 0.009 (se=0.006) over a control group mean of 0.878.

The next three columns examine impacts on respondents' intra-household decision-making power for the subsample of women who are not single and not the only adult in their household.[35] These are important outcomes given claims by financial institutions, donors, and policymakers that microcredit empowers women by giving them greater access to resources and a supportive group environment (Hashemi, Schuler, and Riley 1996; Kabeer 1999). On the other hand, there is evidence that large increases in the share of household resources controlled by women threatens the identity of some men (Maldonado, Gonzales-Vega, and Romero 2002), causing increases in domestic violence (Angelucci 2008). Column 4 shows an increase on the extensive margin of female participation in household financial decision-making: treatment group women are 0.8pp more likely to have any say. This is a large proportional effect on the left tail—i.e., on extremely low-power women—since 97.5 percent of control group respondents say they participate in *any* financial decision-making; this effect represents an improvement for almost one-third of the 2.5 percent of respondents that otherwise had *no* financial decision-making. Column 2 shows a small but significant increase in the number of issues for which the woman has any say: 0.079 (se=0.030) on a base of 2.743.

Column 3 shows no increase in the amount of intra-household conflict. Note that the expected sign of the treatment effect on this final outcome and its interpretation is ambiguous: less conflict is more desirable all else equal, but all else may not be equal in the sense that greater decision power could produce more conflict.

---

[35] The dependent variable in Column 2, "# of household issues she has a say on," represents the number of household issues (of four) that the respondent either makes alone or has some say on when a disagreement arises if she makes the decision jointly. The dependent variable in Column 3, the "# of household issues in which a conflict arises," represents the number of household issues (of four) in which a disagreement sometimes arises if the respondent makes the decision jointly. The dependent variable in Column 4, "Participates in any financial decisions," is a binary variable equal to one if the respondent participates in at least one of the household financial decisions, and equal to zero if she participates in none of the decisions.

Columns 5-7 estimate treatment effects on measures of social cohesion. Column 5 shows that an index of trust in institutions (government workers, financial workers, and banks) is unaffected (-0.011, se=0.025). Column 6 shows that an index of trust in people (family, neighbors, personal acquaintances, people just met, business acquaintances, borrowers, and strangers) increases by an estimated 0.049 standard deviations, a statistically significant effect (se=0.027). This could be a byproduct of the group aspect of the lending product. Column 7 shows a statistically significant effect of -1.9pp on participation in an informal savings group, on a base of 22.8 percent. We lack data that directly addresses whether this reduction is by choice or constraint—constraints could bind if increased formal access disrupts informal networks—but the overall pattern of results is more consistent with choice: there is no effect on the ability to get credit from friends or family in an emergency (results not tabulated), and there remains the positive effect on trust in people reported in Column 6.

IV. Other Results

A. Well-Being Outcomes

Table 8 reports AIT effects on various other measures of proxies for well-being: depression, stress, locus of control, life and financial satisfaction, health status, and asset sales. These outcomes are important given claims by microcredit supporters that expanded access to credit improves subjective well-being. Social scientists have made considerable progress in measuring it (Kahneman and Krueger 2006; Stiglitz, Sen, and Fitoussi 2010; Deaton 2012) and measures of subjective well-being are increasingly standard components of impact evaluations (Kling, Liebman, and Katz 2007; Fernald et al. 2008; Karlan and Zinman 2010).

Unless mentioned otherwise, we create indices out of batteries of multiple questions, standardizing each index of well-being so that the control group mean is zero. As before, we create indices so that positive AIT effects mean that the

treatment has a beneficial effect on the outcome (e.g. we scale the depression index such that a positive AIT estimate means less depression).

Column 1 starts with perhaps our most important proxy for well-being, a measure of depression.[36] This outcome improves by 0.046 standard deviations (se=0.024), a small but statistically significant effect. Columns 2-6 show the AIT effects on indices of job stress, locus of control, satisfaction with one's life and harmony with others, satisfaction with economic situation, and index of good health. The confidence intervals contain effects that are at most plus or minus 0.07 standard deviations.

Columns 7 and 8 return to the question of whether the reduction in asset purchases (Table 6, Columns 1 and 2) is consistent with a reduction in costly "asset churn". If secondary markets yield relatively low prices (due, e.g., to a lemons problem), then reduced churn could actually be welfare-improving. Column 7 shows that treatment group households are 1pp less likely (se= 0.004) to sell an asset to help pay for a loan, a 20 percent reduction and a statistically significant result. This could indicate a reduction in costly "fire sales" and is a striking result, since the positive treatment effect on debt mechanically pushes against a reduction in fire sales (more debt leads to greater likelihood of needing to sell an asset to pay off debt, all else equal). The low prevalence of such sales— only 4.9 percent of households in the control group reported selling an asset to repay a loan in the previous two years—suggests that they are used as a last resort. In this case, the treatment might be beneficial for people in considerable

---

[36] The depression measure is an index of responses to questions about the incidence of the following: being bothered by things that do not normally bother you, having a poor appetite, not being able to shake off the blues even with support from friends and family, feeling just as good as other people, having trouble focusing, feeling depressed, feeling like everything required extra effort, being hopeful about the future, thinking your life was a failure, feeling fearful, having restless sleep, feeling happy, talking less than usual, being lonely, thinking people were unfriendly, having crying spells, enjoying life, feeling sad, thinking people dislike you, and feeling like you couldn't keep going on.

financial distress. Note, however, that we do not find a treatment effect on a broader measure of asset churn: Column 8 shows an imprecisely estimated increase in the likelihood that the household did not sell an asset under any circumstance over the previous two years (0.006, se=0.007).

In all, the results in Table 8 suggest that expanded access to credit has positive, albeit limited, effects on some aspects of subjective well-being. We do not find any evidence of adverse effects on average.

B. Quantile Treatment Effects

Looking only at mean impacts may miss important heterogeneity in treatment effects, as discussed previously. Quantile Treatment Effects (QTEs) provide further insight into how access to Compartamos credit changes the shape of outcome distributions; e.g., whether most of the changes in outcomes between the treatment and control groups are in the tails, in the middle, or throughout the distribution. QTEs also provide some information on the "winners and losers" question: if a QTE is negative (positive) for a given outcome in the tails, the treatment worsens (improves) that outcome for at least one household. But one cannot infer more from QTEs about how many people gain or lose without further assumptions.[37] We estimate standard errors using the block-bootstrap with 1,000 attempted repetitions.

---

[37] The QTEs are conceptually different than the effect of the treatment at different quantiles. That is, QTEs do not necessarily tell us by how much specific households gain or lose from living in treatment clusters. For example: say we find that business profits increase at the 25th percentile in treatment relative to control. This could be because the treatment shifts the distribution rightward around the 25th percentile, with some business owners doing better and no one doing worse. But it also could be the result of some people doing better around the 25th percentile while others do worse (by a bit less in absolute value); this would produce the observed increase at the 25th percentile while also reshuffling ranks. More formally, rank invariance is required for QTEs to identify the effect of the treatment for the household at the qth quantile of the outcome distribution. Under rank invariance, the QTEs identify the treatment effects at a particular quantile. However, rank invariance seems implausible in our setting; e.g., effects on borrowers are likely larger (in absolute value) than effects on non-borrowers.

Figure 3 shows QTE estimates for microentrepreneurship outcomes: revenues, expenditures, profits, and number of businesses. Revenues, expenditures, and profits increase in the right tail, although the increases in expenditures are not statistically significant at the estimated percentiles (Figures 3a through 3c).

Figure 4 presents QTEs for income outcomes. Many of these QTE estimates are imprecise, and none are significantly different from zero at the estimated percentiles.

Figure 5 presents QTEs for two labor supply outcomes: fraction of children aged 4-17 working and number of family members employed by respondent's business. None of the estimates are statistically different from zero at the estimated percentiles.

Figure 6 presents QTEs for expenditures. Although most individual QTEs are not statistically significant, the overall pattern suggests right-tail increases in several spending categories, including amount spent on nondurables, food, medical expenses, school expenses, and family events. One left-tail result of note is the statistically significant decrease in the amount spent on food at the 5th percentile for treatment respondents. For amount spent on temptation goods, there is statistically significant decrease at the 60th percentile; however, this result may be due to chance, as the QTEs along the remainder of the distribution remain close to zero. Figure 6a suggests that treated households are more likely to have bought zero new assets, and very nearly less likely to have bought any of the non-zero asset counts. This is consistent with the previously documented reduction in fire sales of assets.

Figure 7 presents QTEs for social status outcomes. There is a positive shift in the distribution in the trust in people index (Figure 7e) – estimates are statistically significant at the 50th and 60th quantiles. There is also a statistically significant

right-tail increase in the number of household issues the respondent has a say in (Figure 7b).

Figure 8 presents QTE estimates for subjective well-being outcomes. There is a positive, statistically significant shift in the distribution in the depression index (Figure 8a; recall that a positive shift is associated with less depression). Estimates at each quantile of the depression index are either significant or very nearly significant. In particular, the estimates at quantiles below the median are larger than those above the median. The point estimates for the satisfaction and harmony index are all zero (and often precisely estimated), excepting a significant increase at the 75th percentile (Figure 8d). Finally, there is a negative effect at the left-tail locus of control index that is nearly statistically significant (Figure 8c).

Overall, we glean three key patterns from the QTE estimates. First, there are several variables with positive and statistically significant treatment effects in the right tail: revenues, profits, and number of household issues respondent has a say on (several other outcomes have nearly significant positive QTEs at the 90th percentile or above). Second, we see positive effects on depression and trust throughout their distributions. Third, there are few hints of negative statistically significant (or nearly statistically significant) impacts in the left tail of distributions—with the exception of locus of control and amount spent on food—alleviating concerns that expanded credit access might adversely impact people with the worst baseline outcomes. However, as we discussed above, the results tell us relatively little about whether and to what extent distributional changes produced winners and losers.

V. Conclusions

Our results suggest modest effects on our sample of borrowers and prospective borrowers. We make five broad inferences. First, increasing access to microcredit increases borrowing and does not crowd-out other loans. Second, loans seem to be

used for investment—in particular for expanding previously existing businesses—and risk management (through a reduction in asset fire sales). Third, increasing access to microcredit leads to modest increases in business size, trust, and female decision-making, and decreases in depression and reliance on or need for aid. Fourth, there is little evidence of posited consequences from debt traps—such as asset sales or higher expenditures on temptation goods—as a result of access to credit. Fifth, the overall effects do not appear large or transformative. Although some of the intent-to-treat effects are economically large, and all of the statistically significant effects are likely large in treatment-on-the-treated terms, we find statistically significant effects on only 12 of the 37 outcomes we evaluate, and no large effects on income, consumption, or wealth.

We note several caveats, starting with those concerning internal validity. First, some of our treatment effects are imprecisely estimated, despite the large sample relative to most randomized trials in development. Second, our analysis assumes that there are no spillovers or general equilibrium effects beyond the unit of randomization - the "cluster" (neighborhood if urban, community if rural). This is a common assumption in the microcredit literature that we plan to test in future work, employing a similar strategy as Crepon et al. (2013). Third, our endline sampling strategy (specifically, not having a sample frame predetermined prior to the start of the intervention) is prone to migratory risk, in which the treatment leads some respondents to be more or less likely to migrate. Fourth, and finally, our panel survey attrition rate is 37 percent, which is high for a developing country survey.[38]

Regarding external validity, we highlight two sets of issues. First, broad economic, social, and political contexts may influence the effects of microcredit. For example, parts of our study area may be more transitory (due to cross-border migration) and more violent (due to drug-trafficking) than other settings. Second,

---

[38] We test whether this attrition is correlated with observables differentially for treatment and control areas, and do not find evidence of compositional changes as a consequence of treatment.

loan terms may also influence how proceeds are spent, and hence downstream impacts (e.g., see Field et al. 2013). Here we test one particular type of lending contract that has some typical features (group liability, fixed and equal periodic repayments over a term less than one year, larger loan sizes on loan subsequent to successful repayment) and some less-typical features (a triple-digit real APR[39] that is common in Mexico but less so elsewhere, dynamic pricing incentives).

These results, taken together with a paper showing strong price elasticities of demand for Compartamos credit (Karlan and Zinman 2014),[40] suggest that lowering interest rates would not lower profits, and could lead to larger social impact. One missing piece is evidence on heterogeneous treatment effects. If average impacts mask dispersion, in which some (potential) borrowers are much better off and others are worse off, this would have important implications for modeling and policy concerned with the effects of expanded access to credit on inequality. We suggest further research to identify the extent of heterogeneous treatment effects from expanded access to credit.

---

[39] Note that high interest rates would presumably select for borrowers with high-return investments or consumption smoothing opportunities; see Beaman et al. (2014) for some related evidence.
[40] One caveat is that the study areas in the two papers do not overlap; although Karlan and Zinman (2013) was nationwide, Compartamos had not yet expanded into the study site used in this paper.

References

Angelucci, Manuela. 2008. "Love on the Rocks: Domestic Violence and Alcohol Abuse in Rural Mexico." *B.E Journal of Economic Analysis and Policy* 8 (1).

———. 2013. "Migration and Financial Constraints: Evidence from Mexico." *Review of Economics and Statistics* forthcoming.

Attanasio, Augsburg, Britta Augsburg, Ralph de Haas, Fitz Fitzsimons, and Heike Harmgart. 2011. "Group Lending or Individual Lending? Evidence from a Randomised Field Experiment in Mongolia." *EBRD Working Paper* 136 (December).

Augsburg, Britta, Ralph de Haas, Heike Harmgart, and Costas Meghir. 2012. "Microfinance at the Margin: Experimental Evidence from Bosnia and Herzegovina." *Working Paper* (September).

Banerjee, Abhijit, Esther Duflo, Rachel Glennerster, and Cynthia Kinnan. 2013. "The Miracle of Microfinance? Evidence from a Randomized Evaluation". Working paper.

Baumeister, Roy F, and John Tierney. 2011. *Willpower: Rediscovering the Greatest Human Strength*. Penguin. com.

Beaman, Lori, Karlan, Dean, Bram Thuysbaert, and Christopher Udry. 2014. "Self-Selection into Credit Markets: Evidence from Agriculture in Mali." *Working Paper*.

Benjamini, Yoav, and Yosef Hochberg. 1995. "Controlling the False Discovery Rate: A Practical and Powerful Approach to Multiple Testing." *Journal of the Royal Statistical Society. Series B (Methodological)*: 289–300.

Crepon, Bruno, Florencia Devoto, Esther Duflo, and William Pariente. 2011. "Impact of Microcredit in Rural Areas of Morocco: Evidence from a Randomized Evaluation." *M.I.T. Working Paper* (March).

Crepon, Bruno, Esther Duflo, Marc Gurgand, Roland Rathelot, and Philippe Zamora. 2013. "Do Labor Market Policies Have Displacement Effects? Evidence from a Clustered Randomized Experiment." *The Quarterly Journal of Economics* 128 (2) (May 1): 531–580.

Deaton, Angus. 2012. "The Financial Crisis and the Well-Being of Americans 2011 OEP Hicks Lecture." *Oxford Economic Papers* 64 (1) (January 1): 1–26.

Dupas, Pascaline, and Jonathan Robinson. 2013. "Savings Constraints and Microenterprise Development: Evidence from a Field Experiment in Kenya." *American Economic Journal: Applied Economics* 5 (1) (January): 163–192.

Fernald, Lia C.H., Rita Hamad, Dean Karlan, Emily J. Ozer, and Jonathan Zinman. 2008. "Small Individual Loans and Mental Health: A Randomized Controlled Trial among South African Adults." *BMC Public Health* 8 (1): 409.

Field, Erica, Rohini Pande, John Papp, and Natalia Rigol. 2013. "Does the Classic Microfinance Model Discourage Entrepreneurship Among the Poor? Experimental Evidence from India." *American Economic Review* 103 (6) (October): 2196–2226.

Hashemi, Syed, Sidney Schuler, and Ann Riley. 1996. "Rural Credit Programs and Women's Empowerment in Bangladesh." *World Development* 24 (4): 635–53.

Kabeer, Naila. 1999. "Conflicts Over Credit: Re-Evaluating the Empowerment Potential of Loans to Women in Rural Bangladesh." *World Development* 29.

Kahneman, Daniel, and Alan Krueger. 2006. "Developments in the Measurement of Subjective Well-Being." *Journal of Economic Perspectives* 20 (1): 3–24.

Karlan, Dean, and Jonathan Zinman. 2010. "Expanding Credit Access: Using Randomized Supply Decisions to Estimate the Impacts." *Review of Financial Studies* 23 (1): 433–464.

———. 2011. "Microcredit in Theory and Practice: Using Randomized Credit Scoring for Impact Evaluation." *Science* 332 (6035) (June 10): 1278–1284.

———. 2014. "Long-Run Price Elasticities of Demand for Microcredit: Evidence from a Countrywide Field Experiment in Mexico."

Kling, Jeffrey, Jeffrey Liebman, and Lawrence Katz. 2007. "Experimental Analysis of Neighborhood Effects." *Econometrica* 75 (1) (January): 83–120.

Maldonado, Jorge, Claudio Gonzales-Vega, and Vivanne Romero. 2002. "The Influence of Microfinance on Human Capital Formation: Evidence from Bolivia." *Contributed Paper at 2002 LACEA Conference*.

McKenzie, David, and Christopher Woodruff. 2006. "Do Entry Costs Provide an Empirical Basis for Poverty Traps? Evidence from Mexican Microenterprises." *Economic Development and Cultural Change* 55 (1) (October): 3–42.

———. 2008. "Experimental Evidence on Returns to Capital and Access to Finance in Mexico." *The World Bank Economic Review* 22 (3) (October 22): 457–482.

Stiglitz, Joseph E., Amartya Sen, and Jean-Paul Fitoussi. 2010. *Mismeasuring Our Lives: Why GDP Doesn't Add Up*. The New Press.

Tarozzi, Alessandro, Jaikishan Desai, and Kristin Johnson. 2013. "On the Impact of Microcredit: Evidence from a Randomized Intervention in Rural Ethiopia." *UPF Working Paper*.

Table 1: Summary statistics and balance tests

| | Full Endline Sample Frame | | |
	Mean	Difference: Treatment - Control	Balance Test
	(1)	(2)	(3)
Female	1	0	
Age	37.664	0.504*	0.001*
	(0.086)	(0.286)	(0.001)
Primary school or none	0.290	-0.011	-0.026
(omitted: above high school)	(0.004)	(0.012)	(0.024)
Middle school	0.400	0.009	-0.006
	(0.004)	(0.010)	(0.019)
High school	0.235	-0.000	-0.011
	(0.003)	(0.012)	(0.016)
Prior business owner	0.213	0.005	0.001
	(0.003)	(0.008)	(0.009)
In urban area	0.726	0.038	0.300
	(0.003)	(0.068)	(0.283)
N	16560	16560	16014
Number of clusters	238	238	238
Share of sample in treatment group			0.500
pvalue of F test of joint significance of explanatory variables			0.317

* $p<0.10$, ** $p<0.05$, *** $p<0.01$. Respondents are Mexican women aged 18-60. Column 2 reports the coefficient on treatment assignment (1=Treatment, 0=Control) when the variable in the row is regressed on treatment assignment. Column 3 reports the results of balance tests. The cells show the coefficient for each variable when they are all included in one regression with treatment assignment as the dependent variable. Standard errors are in parentheses below the coefficients. All regressions include supercluster fixed effects and standard errors clustered by the unit of randomization.

Table 2a: Credit Access

Panel A: Net Borrowing Effects

Outcome:	Any loan from any MFI	Any loan from Compartamos - admin data	Any loan from Compartamos - survey data
	(1)	(2)	(3)
Treatment	0.069***	0.115***	0.082***
	$(0.009)^{AAA}$	$(0.009)^{AAA}$	$(0.008)^{AAA}$
Data source	Survey	Admin data	Survey
Baseline value controlled for	No	No	No
Adjusted R-squared	0.021	0.062	0.049
N	15876	16560	15845
Number missing	684	0	715
Control group mean	0.138	0.058	0.039

Panel B: Crowdout of Other Borrowing Effects

Outcome:	Any loan from other MFI	Any loan from other bank	Any loan from other formal institution	Any loan from informal entity	Any loan from other source	Any loan	Client was ever late on payments
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.002	0.002	-0.000	0.011**	0.003	0.051***	0.011***
	(0.005)	(0.010)	(0.004)	$(0.004)^{AA}$	(0.010)	$(0.011)^{AAA}$	$(0.002)^{AAA}$
Data source	Survey	Survey	Survey	Survey	Survey	Survey	Admin data
Baseline value controlled for	No	No	No	Yes	Yes	Yes	No
Adjusted R-squared	0.019	0.008	0.007	0.002	0.013	0.021	0.013
N	15844	15918	15820	15977	15987	16177	16560
Number missing	716	642	740	583	573	383	0
Control group mean	0.104	0.288	0.023	0.051	0.166	0.537	0.003

* p<0.10, ** p<0.05, *** p<0.01

^{A} q<0.10, ^{AA} q<0.05, ^{AAA} q<0.01 (adjusting critical values following the approach by Benjamini & Hochberg)

Specification: Standard errors, clustered by 238 geographic clusters (the unit of randomization), are in parentheses. Controls for randomization strata (i.e. 45 supercluster fixed effects and 3 branches) are included but not shown. Controls for the baseline value of the outcome (its value, and missing/non-missing value) are included when the outcome was measured in the baseline. If a control was added for the baseline value of the outcome, any missing values for the baseline observation of the outcome were coded as zero and a variable was added that is equal to one if the baseline value is missing and zero otherwise. Number missing includes both nonresponse and not applicable values.

Outcome(s): The dependent variables in Panel A, Column 2 and Panel B, Column 7 are from administrative data and refer to all the respondent's loans from Compartamos from April 2009 to February 2012. Panel A, Columns 1 and 3 and Panel B, Columns 1-6 are self-reported and refer to the 3 most recent loans of the last 2 years, first among the respondent's loans and then within the household. Panel B, Column 4 refers to loans from money lenders, pawnshops, relatives, and friends. Panel B, Column 5 includes merchandise not paid for in the amount of purchase and loans from employers and other sources. The adjusted critical values were calculated by treating Panel A, Columns 1 and 3 and Panel B, Columns 1-6 of this table and Column 7 of Table 7 as an outcome family.

Table 2b: Loan Amounts

Outcome:	Amount from any MFI	Amount from Compartamos - survey data	Amount from other MFI	Amount from other bank	Amount from other formal institution	Amount from informal entity	Amount from other source	Total amount
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	574***	629***	-55	197	-89	80	277	1157**
	(101)^AAA	(74)^AAA	(64)	(199)	(258)	(60)	(174)	(456)^AA
Baseline value controlled for	No	No	No	No	No	Yes	Yes	Yes
Adjusted R-squared	0.004	0.022	0.003	0.001	0.001	0.001	0.001	0.003
N	16154	16155	16156	16147	16157	16165	16159	16139
Number missing	406	405	404	413	403	395	401	421
Control group mean	1052	280	773	2906	919	308	1188	6493

* p<0.10, ** p<0.05, *** p<0.01

^A q<0.10, ^AA q<0.05, ^AAA q<0.01 (adjusting critical values following the approach by Benjamini & Hochberg)

Specification: Standard errors, clustered by 238 geographic clusters (the unit of randomization), are in parentheses. Controls for randomization strata (i.e. 45 supercluster fixed effects and 3 branches) are included but not shown. Controls for the baseline value of the outcome (its value, and missing/non-missing value) are included when the outcome was measured in the baseline. If a control was added for the baseline value of the outcome, any missing values for the baseline observation of the outcome were coded as zero and a variable was added that is equal to one if the baseline value is missing and zero otherwise. Number missing includes both nonresponse and not applicable values.

Outcome(s): All columns refer to the 3 most recent loans of the last 2 years, first among the respondent's loans and then within the household. Column 6 refers to loans from money lenders, pawnshops, relatives, and friends. Column 7 includes merchandise not paid for in the amount of purchase and loans from employers and other sources. The adjusted critical values were calculated by treating all outcomes in the table as one outcome family.

Table 3: Self-Employment Activities

Outcome:	Revenues in the last 2 weeks	Expenditures in the last 2 weeks	Profits in the last 2 weeks	Has a business	# of businesses	Has a business that was started in the last 12 months	No longer has a business
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	121**	119**	0	-0.004	-0.003	-0.007	0.001
	(52)^{AA}	(47)^{AA}	(39)	(0.009)	(0.010)	(0.005)	(0.007)
Baseline value controlled for	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.009	0.001	0.000	0.025	0.022	0.004	0.065
N	16095	16195	16005	16560	16560	16495	16558
Number missing	465	365	555	0	0	65	2
Control group mean	450	327	145	0.243	0.264	0.099	0.146

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

[A] $q<0.10$, [AA] $q<0.05$, [AAA] $q<0.01$ (adjusting critical values following the approach by Benjamini & Hochberg)

Specification: Standard errors, clustered by 238 geographic clusters (the unit of randomization), are in parentheses. Controls for randomization strata (i.e. 45 supercluster fixed effects and 3 branches) are included but not shown. Controls for the baseline value of the outcome (its value, and missing/non-missing value) are included when the outcome was measured in the baseline. If a control was added for the baseline value of the outcome, any missing values for the baseline observation of the outcome were coded as zero and a variable was added that is equal to one if the baseline value is missing and zero otherwise. Number missing includes both nonresponse and not applicable values.

Outcome(s): Business profits (Column 3) are calculated by subtracting responses for expenses from responses for revenues of the businesses. The adjusted critical values were calculated by treating Columns 1-3, 4-5, and 6-7 each as a separate family of outcomes. Two alternative families of outcomes gave the same results: (1) Columns 1-3 and 4-7 as separate families and (2) all columns as one family.

Table 4: Income

Outcome:	Household business income last month	Household income from salaried and non-salaried jobs last month	Monthly household income from remittances and other transfers	Monthly household income from government subsidies or aid
	(1)	(2)	(3)	(4)
Treatment	58	-30	-19	-16**
	(64)	(128)	(28)	(7)^A
Baseline value controlled for	Yes	No	No	Yes
Adjusted R-squared	0.021	0.010	0.000	0.018
N	15577	16155	16525	16292
Number missing	983	405	35	268
Control group mean	840	4541	327	93

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

^{A} $q<0.10$, ^{AA} $q<0.05$, ^{AAA} $q<0.01$ (adjusting critical values following the approach by Benjamini & Hochberg)

Specification: Standard errors, clustered by 238 geographic clusters (the unit of randomization), are in parentheses. Controls for randomization strata (i.e. 45 supercluster fixed effects and 3 branches) are included but not shown. Controls for the baseline value of the outcome (its value, and missing/non-missing value) are included when the outcome was measured in the baseline. If a control was added for the baseline value of the outcome, any missing values for the baseline observation of the outcome were coded as zero and a variable was added that is equal to one if the baseline value is missing and zero otherwise. Number missing includes both nonresponse and not applicable values.

Outcome(s): Income in Column 1 is calculated from a question asking an explicit, all-in question about household income from business or productive activity. Column 2 includes salaried jobs with a fixed schedule as well as jobs without a fixed salary. Column 3 includes gifts or help in the last month from a family member, neighbor, or friend that is not a member of the household; as well as remittances in the last 6 months, divided by 6 to adjust to monthly values. Column 4 is government subsidies or aid in the last 2 months, divided by 2 to adjust to monthly values. The adjusted critical values were calculated by treating all outcomes in the table as one outcome family.

Table 5: Labor Supply

	Outcome: Participated in an economic activity	Fraction of children 4-17 working	# of family members employed by respondent's business
	(1)	(2)	(3)
Treatment	-0.011	-0.007	0.005
	(0.009)	(0.006)	(0.010)
Baseline value controlled for	No	Yes	Yes
Adjusted R-squared	0.008	0.013	0.008
N	16560	12305	16560
Number missing	0	0	0
Control group mean	0.478	0.085	0.133

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

[A] $q<0.10$, [AA] $q<0.05$, [AAA] $q<0.01$ (adjusting critical values following the approach by Benjamini & Hochberg)

Specification: Standard errors, clustered by 238 geographic clusters (the unit of randomization), are in parentheses. Controls for randomization strata (i.e. 45 supercluster fixed effects and 3 branches) are included but not shown. Controls for the baseline value of the outcome (its value, and missing/non-missing value) are included when the outcome was measured in the baseline. If a control was added for the baseline value of the outcome, any missing values for the baseline observation of the outcome were coded as zero and a variable was added that is equal to one if the baseline value is missing and zero otherwise. Number missing includes both nonresponse and not applicable values.

Outcome(s): Anyone reporting having a job or a business is classified as participating in an economic activity (Column 1). Number of family employees in Column 3 is calculated by summing the number of family employees for each of 4 businesses of the respondent's. The adjusted critical values were calculated by treating all outcomes in the table as one outcome family.

Table 6: Assets and Weekly Expenditures

| Outcome:	# of asset categories bought item from	Value of assets	Amount spent on nondurable items other than food	Amount spent on food	Amount spent on medical expenses	Amount spent on school expenses	Amount spent on temptation goods	Amount spent on family events
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.047**	-1534**	-4	4	14	3	-6**	-1
	(0.022)^A	(598)^A	(11)	(15)	(17)	(3)	(3)^A	(2)
Baseline value controlled for	No	No	No	Yes	No	No	No	No
Adjusted R-squared	0.011	0.008	0.010	0.034	-0.001	0.010	0.009	0.001
N	16553	16553	16556	16497	15919	15573	16435	16373
Number missing	7	7	4	63	641	987	125	187
Control group mean	0.502	8319	502	874	37	33	98	17

* p<0.10, ** p<0.05, *** p<0.01

^{A} q<0.10, ^{AA} q<0.05, ^{AAA} q<0.01 (adjusting critical values following the approach by Benjamini & Hochberg)

Specification: Standard errors, clustered by 238 geographic clusters (the unit of randomization), are in parentheses. Controls for randomization strata (i.e. 45 supercluster fixed effects and 3 branches) are included but not shown. Controls for the baseline value of the outcome (its value, and missing/non-missing value) are included when the outcome was measured in the baseline. If a control was added for the baseline value of the outcome, any missing values for the baseline observation of the outcome were coded as zero and a variable was added that is equal to one if the baseline value is missing and zero otherwise. Number missing includes both nonresponse and not applicable values.

Outcome(s): The survey instrument did not include details about the value of assets bought and sold unless they were bought or sold in relation to a loan. Consequently, Column 1 reports the count of categories from which assets were purchased. Column 2 reports an approximate of the total value of assets purchased: for each asset category of purchase, the respondent's total includes the mean value of assets in the category purchased with a loan. The total assumes that no more than one asset was purchased from each category; see the Data Appendix for details. The amounts in Columns 3-8 are weekly. Column 3 includes cigarettes and transportation in the last week, as well as electricity, water, gas, phone, cable, and Internet in the last month, adjusted to weekly values. Column 4 is the sum of amount spent on food eaten out in the last week and amount spent on groceries in the last 2 weeks divided by 2. Columns 5-6 were asked for the last year and were adjusted to weekly values. Column 7 includes cigarettes, sweets, and soda from the last week. Column 8 refers to amount spent in the last year on important events such as weddings, baptisms, birthdays, graduations, or funerals, adjusted to weekly values. The adjusted critical values were calculated by treating all outcomes in the table and Columns 7-8 of Table 8 as one outcome family.

Table 7: Social Effects

Outcome:	Fraction of children 4-17 in school	# of household issues she has a say on (of 4)	# of household issues in which conflict arises (of 4)	Participates in any financial decisions	Trust in institutions index	Trust in people index	Member of informal savings group
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.009	0.079***	0.022	0.008***	-0.011	0.049*	-0.019***
	(0.006)	(0.030)^{AA}	(0.033)	(0.003)^{AA}	(0.025)	(0.027)^{A}	(0.007)^{AA}
Baseline value controlled for	Yes	No	No	No	No	No	Yes
Adjusted R-squared	0.015	0.011	0.016	0.001	0.009	0.027	0.023
N	12305	12185	12193	12183	16530	16558	16551
Number missing	0	11	3	13	30	2	9
Control group mean	0.878	2.743	1.537	0.975	0.000	0.000	0.228

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

^{A} $q<0.10$, ^{AA} $q<0.05$, ^{AAA} $q<0.01$ (adjusting critical values following the approach by Benjamini & Hochberg)

Specification: Standard errors, clustered by 238 geographic clusters (the unit of randomization), are in parentheses. Controls for randomization strata (i.e. 45 supercluster fixed effects and 3 branches) are included but not shown. Controls for the baseline value of the outcome (its value, and missing/non-missing value) are included when the outcome was measured in the baseline. If a control was added for the baseline value of the outcome, any missing values for the baseline observation of the outcome were coded as zero and a variable was added that is equal to one if the baseline value is missing and zero otherwise. Number missing includes both nonresponse and not applicable values.

Outcome(s): Columns 2-4 include only married respondents living with another adult. The issues in Columns 2 and 3 are: whether to buy an appliance or not for the home; in what way household members may work outside the home; whether to financially support family members; and whether to save for the future. Higher values in the indices in Columns 5-6 denote beneficial outcomes. In Column 5, institutions include government workers, financial workers, and banks. Trust in people in Column 6 includes questions about trust in family, neighbors, personal acquaintances, people just met, business acquaintances, people who borrow money, strangers, and a question about whether people would be generally fair. The adjusted critical values were calculated by treating Columns 2-4 as one outcome family and Column 7 of this table and Panel A, Columns 1 and 3 and Panel B, Columns 1-6 of Table 2a as another outcome family.

Table 8: Various Measures of Welfare

Outcome:	Subjective well-being						Assets	
	Depression index (higher = happier)	Job stress index (higher = less stress)	Locus of control index	Satisfaction (life and harmony) index	Satisfied with economic situation	Good health status	Did not sell an asset to help pay for a loan	Did not sell an asset
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.046*	-0.004	0.003	0.017	-0.009	0.012	0.010**	0.006
	(0.024)[A]	(0.025)	(0.024)	(0.024)	(0.011)	(0.008)	(0.004)[A]	(0.007)
Baseline value controlled for	Yes	No	No	No	No	Yes	No	No
Adjusted R-squared	0.033	0.004	0.009	0.009	0.007	0.025	0.002	0.006
N	16336	7656	16549	16553	16526	16556	16461	16553
Number missing	224	116	11	7	34	4	99	7
Control group mean	-0.000	0.000	-0.000	-0.000	0.458	0.779	0.951	0.863

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

[A] $q<0.10$, [AA] $q<0.05$, [AAA] $q<0.01$ (adjusting critical values following the approach by Benjamini & Hochberg)

Specification: Standard errors, clustered by 238 geographic clusters (the unit of randomization), are in parentheses. Controls for randomization strata (i.e. 45 supercluster fixed effects and 3 branches) are included but not shown. Controls for the baseline value of the outcome (its value, and missing/non-missing value) are included when the outcome was measured in the baseline. If a control was added for the baseline value of the outcome, any missing values for the baseline observation of the outcome were coded as zero and a variable was added that is equal to one if the baseline value is missing and zero otherwise. Number missing includes both nonresponse and not applicable values.

Outcome(s): Higher values in the indices denote beneficial outcomes. Column 1 consists of a standard battery of 20 questions that ask about thoughts and feelings in the last week. The feelings and mindsets include: being bothered by things that do not normally bother you, having a poor appetite, not being able to shake off the blues even with support from friends and family, feeling just as good as other people, having trouble focusing, feeling depressed, feeling like everything required extra effort, being hopeful about the future, thinking your life was a failure, feeling fearful, having restless sleep, feeling happy, talking less than usual, being lonely, thinking people were unfriendly, having crying spells, enjoying life, feeling sad, thinking people dislike you, feeling like you couldn't keep going on. In Column 2, the sample frame is restricted to just those that report participating in an economic activity; the index includes three questions about job stress. The index of locus of control in Column 3 includes five questions about locus of control. The adjusted critical values were calculated by treating Columns 7-8 of this table and Columns 1-8 of Table 6 as an outcome family.

Figure 1: Study Timeline and Survey Locations
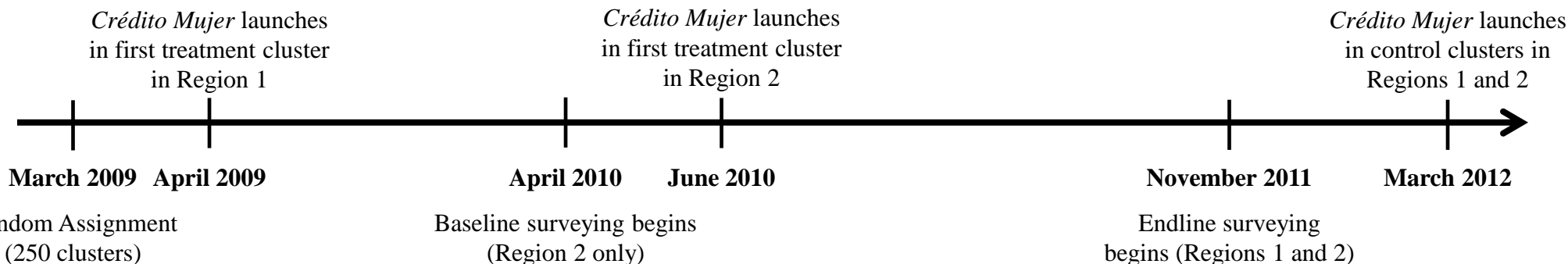
	Region 1 (Endline only): Caborca, Agua Prieta, and urban areas of Nogales	Region 2 (Panel sample): outlying areas of Nogales	Total[1]
# of Clusters			
Treatment	104	16	120
Control	101	17	118
# of respondents			
Baseline Survey	0	2,912[2]	2,912
Endline Survey	14,737	1,823	16,560
Average exposure[3]:	28 months	16 months	
Min exposure:	5 months	5 months	
Max exposure:	35 months	20 months	

[1] 12 Clusters (5T and 7C) were included in the original sample frame but were later deemed too dangerous for both surveyors and Compartamos to operate, and were therefore removed from the sample frame for surveying as well.

[2] We report the number of respondents we tracked for endline surveying.

[3] Exposure is defined as the length of time between the day that the first loan in the respondent's cluster was taken out and the day that the respondent was surveyed at endline.

Figure 2: Average Intent-to-Treat Effects for the Full Sample, at a Glance
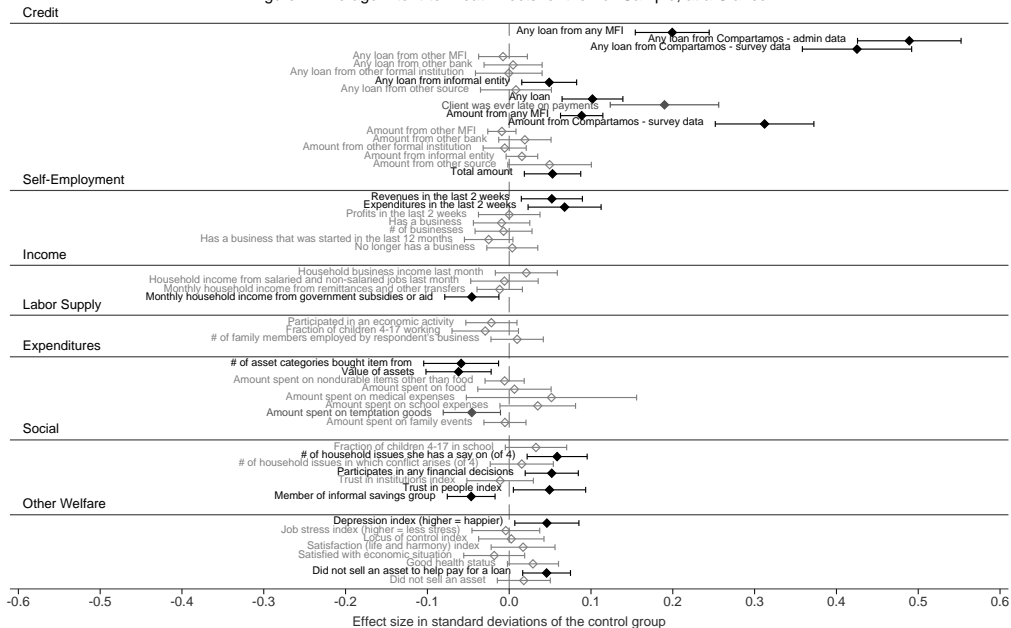
This figure summarizes the treatment effects presented in Tables 2-8. Here, treatment effects on continuous variables are presented in standard deviation units.
Each line shows the OLS point estimate and 90% confidence interval for that outcome.
For some outcomes, we adjust the critical level following the Benjamini and Hochberg approach. Appendix Table 4 lists the outcome families.
No treatment effects were significant at the unadjusted level but not significant after adjustment with alpha = .1.

Figure 3: Quantile Treatment Effects for Self-Employment Activities

For continuous variables, vertical lines show 90% confidence intervals for quantile treatment effects with standard errors block-bootstrapped by cluster with 1,000 replica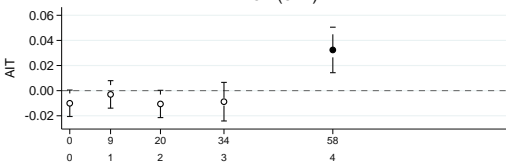tions. For count variables, vertical lines show 90% confidence intervals for the AIT estimate of the likelihood of treatment group respondents having the value on the x axis for that outcome relative to the control group respondents having that value. Standard errors are clustered by the unit of randomization. A + sign indicates that the value of the variable is at or above that number.

Figure 4: Quantile Treatment Effects for Income

4a. Household business income last month
Thousands of pesos
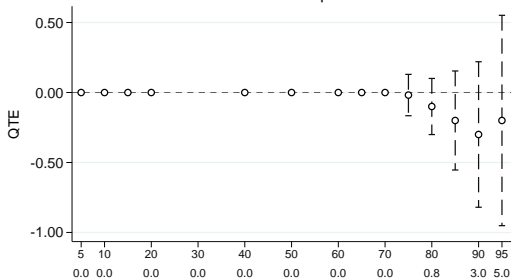
4b. Household income from salaried and non-salaried jobs last month
Thousands of pesos
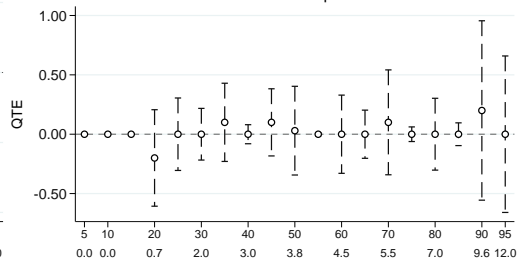
4c. Monthly household income from remittances and other transfers
Thousands of pesos
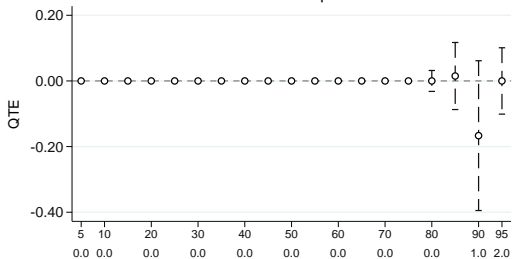
4d. Monthly household income from government subsidies or aid
Thousands of pesos
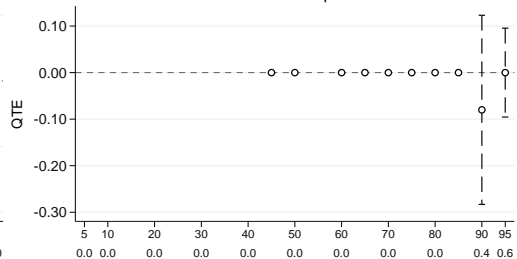
x axis shows the quantile (top row) and the control group value at that quantile (bottom row)

Vertical lines show 90% confidence intervals for quantile treatment effects with standard errors block-bootstrapped by cluster with 1,000 replications.

Figure 5: Quantile Treatment Effects for Labor Supply

5a. Fraction of children 4-17 working
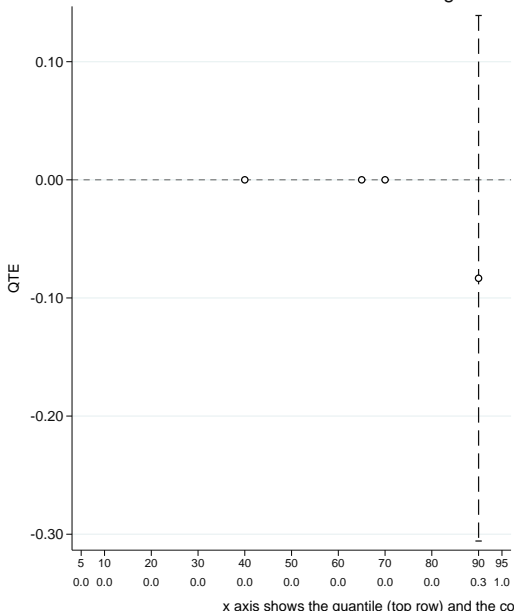
5b. # of family members employed by respondent's business
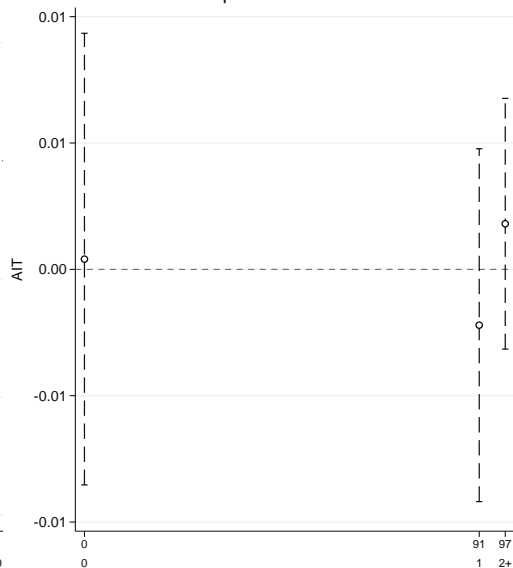
x axis shows the quantile (top row) and the control group value at that quantile (bottom row)

For continuous variables, vertical lines show 90% confidence intervals for quantile treatment effects with standard errors block-bootstrapped by cluster with 1,000 replications. For count variables, vertical lines show 90% confidence intervals for the AIT estimate of the likelihood of treatment group respondents having the value on the x axis for that outcome relative to the control group respondents having that value. Standard errors are clustered by the unit of randomization. A + sign indicates that the value of the variable is at or above that number.
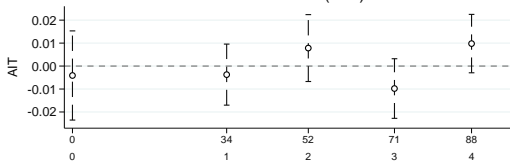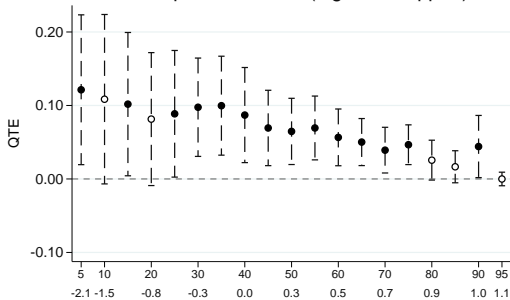
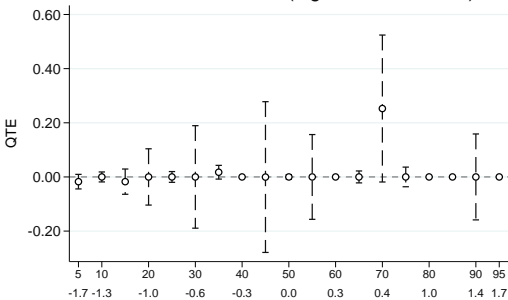Figure 6: Quantile Treatment Effects for Assets and Weekly Expenditures

For continuous variables, vertical lines show 90% confidence intervals for quantile treatment effects with standard errors block-bootstrapped by cluster with 1,000 replications. For count variables, vertical lines show 90% confidence intervals for the AIT estimate of the likelihood of treatment group respondents having the value on the x axis for that outcome relative to the control group respondents having that value. Standard errors are clustered by the unit of randomization. A + sign indicates that the value of the variable is at or above that number.

Figure 7: Quantile Treatment Effects for Social Effects

x axis shows the quantile (top row) and the control group value at that quantile (bottom row)

For continuous variables, vertical lines show 90% confidence intervals for quantile treatment effects with standard errors block-bootstrapped by cluster with 1,000 replications. For count variables, vertical lines show 90% confidence intervals for the AIT estimate of the likelihood of treatment group respondents having the value on the x axis for that outcome relative to the control group respondents having that value. Standard errors are clustered by the unit of randomization. A + sign indicates that the value of the variable is at or above that number.

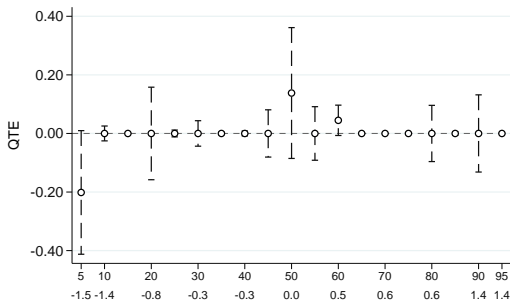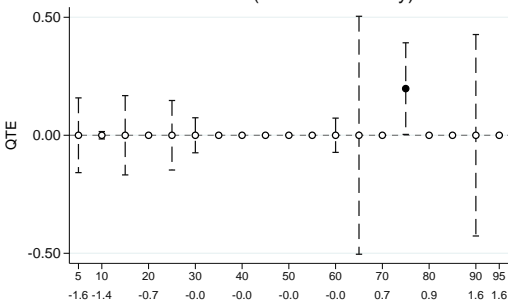Figure 8: Quantile Treatment Effects for Various Measures of Welfare

x axis shows the quantile (top row) and the control group value at that quantile (bottom row)

Vertical lines show 90% confidence intervals for quantile treatment effects with standard errors block-bootstrapped by cluster with 1,000 replications.