Expanding Microenterprise Credit Access: Using Randomized Supply Decisions to Estimate the Impacts in Manila[*]

Dean Karlan
Yale University
Innovations for Poverty Action
M.I.T. Jameel Poverty Action Lab

Jonathan Zinman
Dartmouth College
Innovations for Poverty Action
M.I.T. Jameel Poverty Action Lab

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ABSTRACT

Microcredit seeks to promote business growth and improve well-being by expanding access to credit. We use an innovative, replicable experimental design that randomly assigns credit, through credit scoring, to identify impacts of a credit expansion for marginal microentrepreneurial borrowers in Manila. The canonical case for microcredit-- that access increases profits, business scale, and household consumption-- is not supported on average. Instead the impacts are diffuse, heterogeneous, and surprising. Although there is some evidence that profits increase, the mechanism seems to be that businesses *shrink* by shedding unproductive workers. We also find substitution away from formal insurance, along with increases in access to informal risk-sharing mechanisms. Our treatment effects are stronger for groups that are *not* typically targeted by microlenders: male and higher-income entrepreneurs. In all, our results suggest that microcredit may work broadly through risk management and investment at the household level, rather than directly through the targeted businesses.

JEL Codes: microcredit, microfinance, credit scoring, impact evaluation, randomized evaluation

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Microfinance is a proven and cost-effective tool to help the very poor lift themselves out of poverty and improve the lives of their families. (Microcredit Summit Campaign)[1]

It is easy to construct examples where… the mere possibility that a new outsider might enter the market can crowd-out existing local contracting, leading to the possibility of a decline in welfare. (Conning and Udry 2005)

I. Introduction

Microcredit— broadly speaking, the provision of loans to very small businesses-- is an increasingly common weapon in the fight to reduce poverty and promote economic growth. The motivation for the continued expansion of microcredit, or at least for the continued flow of subsidies to both nonprofit and for-profit lenders, is the presumption that expanding credit access is a relatively efficient way to fight poverty and promote growth. Yet despite often grand claims about the effects of microcredit on borrowers and their businesses (e.g., the first quote above), there is relatively little convincing evidence in either direction. In theory, expanding credit access may well have null or even negative effects on borrowers. Formal options can crowd-out relatively efficient informal mechanisms (see the second quote above). The often high cost of microcredit means that high returns to capital are required for microcredit to produce improvements in tangible outcomes like household or business income. Behavioral biases may induce some to "overborrow" and do themselves more harm than good.

"Traditional" microlenders target women operating small-scale businesses and use group lending mechanisms. But as microlending has expanded and evolved into its "second generation," it often ends up looking more like traditional retail or small business lending: for-profit lenders, extending individual liability credit, in increasingly urban and competitive settings. For example, recent estimates suggest that about one-half of microfinance institutions are individual liability lenders, and about one-quarter are for-profits or cooperatives (Cull et al. 2007; 2009).[2]

We conduct the first randomized evaluation of second-generation microcredit by working with First Macro Bank (FMB) to implement a novel, replicable experimental design that uses credit scoring to randomly assign individual liability loans.[3] FMB is a for-profit lender that makes small, 3-month loans at 60% annualized interest rates to microentrepreneurs in the outskirts of Manila and receives implicit

[1] http://www.microcreditsummit.org/index.php?/en/about/microfinance_advocacy/ .

[2] See Karlan and Morduch (2009) for additional details. Also note that the definition of "microcredit" is often debated, but typically includes loans to microentrepreneurs that are small but sufficiently large to provide meaningful support to small vendors, convenience stores or production facilities. Standard definitions often exclude "consumer" loans made to salaried individuals.

[3] Banerjee et al (2009) is the first randomized evaluation of group-liability microentrepreneurial microcredit, and Karlan and Zinman (forthcoming) is the first randomized evaluation of consumer lending.

subsidies from a USAID-funded program.[4] Non-randomized empirical evaluations of microcredit impacts are typically complicated by classic endogeneity problems; e.g., client self-selection and lender strategy based on critical unobserved inputs like client opportunity sets, preferences, and aptitude.

Our study here is the first to use the random assignment via a credit scoring model as a source of exogenous variation in credit access. We worked with the lender to build a quantitative model that distinguishes creditworthy or uncreditworthy applicants from marginal ones. Marginal applicants then get approved for a loan with some pre-assigned probability. This method provides lenders with a way to take systematic, controlled risks when refining its underwriting. And it provides researchers and policymakers with a source of exogenous variation in access to credit that may be used, in conjunction with follow-up data (e.g., on business and household outcomes), to help identify the impacts of microcredit from a change in the screening criteria of existing lenders. This method is transferrable to many different types of lenders and settings.

The ability to transfer an evaluation method to a range of contexts is particularly important given the unsettled state of evidence on microfinance impacts. Prior studies have used various methodologies to address endogeneity problems and found varied impacts or lack thereof.[5] Is the variation in estimated impacts across studies due to methodology, and/or to true underlying heterogeneity in borrower characteristics and market conditions?

Here we take some first steps toward addressing this external validity issue by demonstrating our methodology's viability and internal validity. The expansion we study here changed borrowing outcomes, despite the existence of other formal and informal borrowing options in the markets where the expanding lender operates. "Treated" applicants (those randomly assigned a loan) significantly increase their formal sector borrowing. There is no evidence of significant effects on informal borrowing, but the point estimates are negative. The effects on total borrowing (sum of all types of formal and informal) are not significant but consistent with effect sizes on the order of the increases we find in our more precise estimates on formal borrowing.

We also add new pieces to the muddled puzzle of evidence on the impacts of credit expansion on more ultimate outcomes: our findings here are varied, diffuse, and surprising in many respects. Treatment effects are stronger for groups that are *not* typically targeted by microcredit initiatives: male and relatively high-income borrowers (we use an ex-ante measure of income for estimating

[4] The program is administered by Chemonics, Microenterprise Access to Banking Services (MABS).

[5] See Karlan and Morduch (2009) for a detailed discussion of the field experiments cited above and several other important microcredit evaluations using various methodologies (Morduch 1998; Pitt and Khandker 1998; Coleman 1999; McKernan 2002; Pitt et al. 2003; Kaboski and Townsend 2005; Kaboski and Townsend 2009; Roodman and Morduch 2009).

heterogeneous treatment effects).[6] Business investment does not increase; rather, we find some evidence that the size and scope of treated businesses *shrink*. We do find some evidence that profits increase, at least for male borrowers, and the mechanism seems to be that treated businesses shed unproductive employees. One explanation is that increased access to credit reduces the need for favor-trading within family or community networks. This hypothesis is consistent with some other treatment effects that are consistent with less short-term diversification and hedging, better access to risk-sharing, and more long-term investment in human capital. There is some evidence, at least among male borrowers, that household members withdraw from work, and are more likely to attend school.[7] The use of formal insurance falls, while trust in one's neighborhood and access to emergency credit from friends and family increase; i.e., microcredit seems to *complement*, not crowd-out, informal mechanisms. We find no evidence of improvements in measures of subjective well-being; if anything, the results point to a small overall *decrease*.

In all, we find that increased access to microcredit leads to less investment in the targeted business, to substitution away from labor and perhaps into education, and to substitution away from insurance (both explicit/formal, and implicit/informal) even as overall access to risk-sharing mechanisms increases. Thus although microcredit does have important— and potentially salutary— economic effects in our setting, the effects are not those advertised by the "microfinance movement." Rather the effects seem to work through interactions between credit access and risk-sharing mechanisms that are often viewed as second- or third-order by theorists, policymakers, and practitioners. At least in a second-generation setting, microcredit may work broadly through risk management and investment at the household level, rather than directly through the targeted businesses. An alternative explanation for some but not all of the results is that borrowers end up reducing investments (in businesses, in insurance) by constraint and not by choice, perhaps because they suffer from behavioral biases that produce mistaken intertemporal choices.[8]

The overall picture of our results questions the wisdom of targeting microentrepreneurs to the exclusion of "consumers." Money is fungible, and we find that entrepreneurs do not necessarily invest loan proceeds in growing their businesses. Limiting microcredit access to entrepreneurs may forgo opportunities to improve human capital and risk-sharing for non-microentrepreneurs.[9]

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[6] Additional motivation for the gender split is recent evidence finding higher returns to capital for men (de Mel et al. 2008; 2009).
[7] The finding that males are more likely to use liquidity to invest in schooling is strikingly at odds with prior findings that females are more likely to invest in their children; see, e.g., Duflo (2003).
[8] See Zinman (forthcoming) for a brief review of such biases in financial decision making.
[9] Karlan and Zinman (forthcoming) finds direct evidence that salaried workers benefit from microloans.

An important caveat in interpreting the evidence from this study is that it applies only to marginal applicants, who comprised 74% of first-time applicants in our setting. Our estimates do not necessarily apply to applicants who are clearly above or below the bar of creditworthiness, or to existing borrowers.

II. Market and Lender Overview

Our cooperating Lender, First Macro Bank (FMB), has operated as a rural bank in the Metro Manila region of the Philippines since 1960. Filipino “microlenders” include both for-profit and nonprofit lenders offering small, short-term, uncollateralized credit with fixed repayment schedules to microentrepreneurs. Interest rates are high by developed-country standards: FMB charges 63% APR on its standard product for first-time borrowers. There is also a similar market segment for consumer loans.

Most Filipino microlenders operate on a small scale relative to microfinance institutions (MFIs) in the rest of Asia,[10] and our lender is no exception. FMB maintained a portfolio of approximately 1,400 individual and 2,000 group borrowers throughout the course of the study. This portfolio represents a small fraction of its overall lending, which also includes larger business and consumer loans, and home mortgages.

Microloan borrowers typically lack the credit history and/or collateralizable wealth needed to borrow from traditional institutional sources such as commercial banks. This holds for our sample—which is only marginally creditworthy by the standards of a microlender, as detailed in Section III—despite the fact that our subjects have average income and education levels. Table 1 provides some demographics on our sample frame, relative to the rest of Manila and the Philippines.

Casual observation suggests that many microentrepreneurs in our study population face binding credit constraints. Credit bureau coverage of microentrepreneurs in the Philippines is quite thin, so building a credit history is difficult for poor business owners and consumers. Informal credit markets and serial borrowing from moneylenders charging 20% per month or more is common (e.g., more than 30% of our sample reported borrowing from moneylenders during the past year). Trade credit is quite uncommon. There are several microlenders operating in Metro Manila, but most MFIs operate on a small scale (as noted above) and charge high rates (see below).

The loan terms granted in this experiment were the Lender’s standard ones for first time borrowers. Loan sizes range from 5,000 to 25,000 pesos, which is small relative to the fixed costs of underwriting

[10] In *Benchmarking Asian Microfinance 2005*, the Microfinance Information eXchange (MIX) reports that Filipino microlenders have the lowest outreach in the region – a median of 10,000 borrowers per MFI.

and monitoring, but substantial relative to borrower income. For example, the median loan size made under this experiment 10,000 pesos, US$220 was 37% of the median borrower's net monthly income. Loan maturity is 13 weeks, with weekly repayments. The monthly interest rate is 2.5%, charged over the declining balance. Several upfront fees combine with the interest rate to produce an annual percentage rate of around 60%.[11]

The Lender conducted underwriting and transactions in its branch network. At the onset of this study, FMB changed its risk assessment process from one based on weekly credit committee meetings to one that utilized computerized credit scoring.

Delinquency and default rates are substantial. One-third of the loans in our sample paid late at some point, and 7.4% were charged off.

III. Methodology

Our research design uses credit scoring software to randomize the approval decision for marginally creditworthy applicants, and then uses data from household/business surveys to measure impacts on credit access and several classes of more ultimate outcomes of interest. The survey data is collected by a firm, hired by the researchers, that has no ties to the Lender.

A. Experimental Design and Implementation

i. Overview

We drew our sample frame from the universe of several thousand applicants who applied at eight of the Lender's nine branches between February 10, 2006 and November 16, 2007.[12] The branches were located in the provinces of Rizal, Cavite, and the National Capital Region. The Lender maintained normal marketing procedures by having account officers canvass public markets and hold group information sessions for prospective clients.

Table 1 provides some summary statistics, *from ex-ante application data*, on our sample frame of 1,601 marginally creditworthy applicants, nearly all (1,583) of whom were first-time applicants to the Lender. The table shows that our sample is largely female, has a typical household size, and has educational attainment and household income in line with averages for Metro Manila. The most common business is a sari-sari (small grocery/convenience) store. Other common businesses are food

[11] The Lender also requires first-time borrowers to open a savings account and maintain a minimum balance of 200 pesos.

[12] One branch was removed from the study when it was discovered that one account officer had found the underlying files saved by the credit scoring software and altered both the assignment to treatment and data recorded from the application. This was discovered in audits of the proportion assigned to treatment, as well as audits to verify that the handwritten application from the client matched the data entered into the credit scoring software. No other branches had problems revealed by such audits.

vending, and services (e.g., auto and tire repair, water supply, tailoring, barbers and salons). Table 1 does *not* contain sample means for each dependent variable of interest; these can be found in the tables on treatment effects.

The Lender identified marginally creditworthy applicants using a credit scoring algorithm that places roughly equal emphasis on business capacity, personal financial resources, outside financial resources, personal and business stability, and demographic characteristics. Credit bureau coverage of our study population is very thin, and our Lender does not use credit bureau information as an input into its scoring. Scores range from 0 to 100, with applicants scoring below 31 rejected automatically and applicants scoring above 59 approved automatically. Our 1,601 marginally creditworthy applicants fall into two randomization "windows": low (scores 31-45, with 60% probability of approval) and high (scores 46-59, with 85% probability of approval). Only the Lender's Executive Committee was informed about the details of the algorithm and its random component, so the randomization was "double-blind" in the sense that neither loan officers (nor their direct supervisors) nor applicants knew about assignment to treatment versus control. In total, 1,272 applicants were assigned to the treatment (loan approval) group, leaving 329 in the control (loan rejection) group.

The motivation for experimenting with credit access on a pool of marginal applicants is twofold. First, it focuses on those who are targeted by initiatives to expand access to credit. Second, (randomly) approving some marginally creditworthy applicants generates data points on the lender's profitability frontier that will feed into revisions to the credit scoring model. This allows the lender to manage risk by controlling the flow of their more marginal credits.

ii. Internal Validity

Table 2 corroborates that treatment assignment is uncorrelated with follow-up survey completion (column 2), and that pre-treatment characteristics are similar across treatment and control groups in the full sample (columns 1 and 3). Pre-treatment characteristics are also balanced across treatment and control in our male and female sub-samples (columns 4 and 5), which is critical given the heterogeneity in treatment effects by gender reported below.

iii. Details on Experimental Design and Operations

Our sample frame and treatment assignments were created in the flow of the Lender's three-step credit scoring process (Figure 1 summarizes this flow). This process is replicable because it is relatively easy to administer operationally. And it can be augmented to introduce random assignment into other elements of loan contracting besides the approve/reject decision: pricing, loan amount, maturity, etc.

First, loan officers screened potential applicants on the "Basic Four Requirements": 18-60 years old; in business for at least one year; in residence for at least one year if owner, or at least three years if renter; and daily income of at least 750 pesos. 2,158 applicants passed this screen.

Second, loan officers entered household and business information on those 2,158 into the credit scoring software, and the software then rendered its application disposition within seconds. 391 applications received scores in the automatic approval range. 166 applications received scores in the automatic rejection range. The remaining 1,601 applicants had scores in one of the two randomization windows (approve with 60% or 85% probability), and comprise our sample frame. 1,272 marginal applicants were assigned "approve", and 329 applicants were assigned "reject". The software simply instructed loan officers to approve or reject— it did not display the application score or make any mention of the randomization. Neither loan officers, branch managers, nor applicants were informed about the credit scoring algorithm or its random component.

The credit scoring software's decision was contingent on complete verification of the application information, so the third step involved any additional due diligence deemed necessary by the loan officer or his supervisor. Verification steps include visits to the applicant's home and/or business, meeting with neighborhood officials, and checking references (e.g., from other lenders). If loan officers found discrepancies, they updated the information in the credit scoring software, and in some cases the software changed its decision from approve to reject. In other cases applicants decided not to go forward with completing the application, or completed the application successfully but did not avail the loan.

In all, there were 351 applications assigned out of the 1,272 assigned to treatment that did *not* ultimately result in a loan. Conversely, there were 5 applications assigned to the control (rejected) group that *did* receive a loan (presumably due to loan officer noncompliance or clerical errors). Table 3 shows all of the relevant tabs, separately for each randomization window.

In all cases we use the *original* treatment assignment from Step 2 to estimate treatment effects; i.e., we use the random *assignment* to loan approval or rejection, rather than the ultimate disposition of the application, and thereby estimate intention-to-treat effects.

As detailed in Section II, the loans made to marginal applicants were based on the Lender's standard terms for first-time applicants. Loan repayment was monitored and enforced according to normal operations.

B. Follow-up Data Collection and Analysis Sample

Following the experiment, we hired researchers from a local university to organize to survey all 1,601 applicants in the treatment and control groups.[13] The stated purpose of the survey was to collect information on the financial condition and well-being of microentrepreneurs and their households. As detailed below, the surveyors asked questions on business condition, household resources, demographics, assets, household member occupation, consumption, subjective well-being, and political and community participation.

In order to avoid potential response bias in the treatment relative to control groups, neither the survey firm nor the respondents were informed about the experiment or any association with the Lender. Surveyors completed 1,113 follow-up surveys, for a 70% response rate. Table 2, Column 2 shows that survey completion was not significantly correlated with treatment assignment.

Ninety-nine percent of the surveys were conducted within eleven to twenty-two months of the date that the applicant entered the experiment by applying for a loan and being placed in the pool of marginally creditworthy applicants. The mean number of days between treatment and follow-up is 411; the median is 378 days; and the standard deviation is 76 days.

C. Estimating Intention-to-Treat Effects

We estimate intention-to-treat effects for each individual outcome Y using the specification:

$$(1)\quad Y^k_i = \alpha + \beta^k assignment_i + \delta risk_i + \phi APP\_WHEN_i + \gamma SURVEY\_WHEN_i + \varepsilon_i$$

k indexes different outcomes— e.g., number of formal sector loans in the month before the survey, total household income over the last year, value of business inventory, etc.— for applicant i (or i's household). $Assignment_i$ = 1 if the individual was *assigned* to treatment (regardless of whether they actually received a loan). $Risk_i$ captures the applicant's credit score window (low or high); the probability of assignment to treatment was conditional on this (set to either 0.60 or 0.85, depending on their credit score), and thus it is necessary to include this as a control variable in all specifications. *APP_WHEN* is a vector of indicator variables for the month and year in which the applicant entered the experiment and *SURVEY_WHEN* is a vector of indicator variables for the month and year in which the survey was completed. These variables control flexibly for the possibility that the lag between application and survey is correlated with both treatment status and outcomes.[14] We estimate (1) using ordinary least squares (OLS) unless otherwise noted.

[13] Midway through the survey effort, Innovations for Poverty Action staff replaced the management team but retained local surveyors.

[14] This could occur if control applicants were harder to locate (e.g., because we could not provide updated contact

IV. Results

A. Reading the Treatment Effect Tables

Tables 4 through 11 present our key estimated treatment effects on borrowing, business outcomes, and other outcomes. Each table is organized the same way, with a different outcome in each row, and different sample or sub-sample in each column. Each cell presents the intention-to-treat effect on that outcome or index, i.e., the coefficient on a variable that equals one if the applicant was randomly *assigned* to receive a loan. We also present the (sub)-sample mean for the outcome in each cell, in brackets, for descriptive and scaling purposes.

Each column presents results for a different (sub)-sample. Column 1 uses the full sample, and columns 2 through 5 use sub-samples based on gender and income, since these characteristics are commonly used for targeting microcredit. For the income sub-samples we use a measure taken by the Lender at the time of application (i.e., at the time of treatment, not at the time of follow-up outcome measurement, to avoid endogeneity).

B. Impacts on Borrowing Levels and Composition, Table 4

Table 4 presents the estimated treatment effects on various measures of borrowing. The key questions here are whether being randomly assigned a loan from our Lender affects overall borrowing, and borrowing composition. Ex-ante the impacts are not obvious, given the prevalence of other lenders in the market as described in Section II.

The first panel of Table 4 shows large increases in borrowing on loan types plausibly most directly affected by the treatment: loans from the Lender, or from close substitutes.[15] The probability of having any such loan in the month before the survey increases by 9.6 percentage points in the treatment relative to control group, on a sample mean of only 14.5 percentage points. The total original principal amount of loans outstanding increase 2,156 pesos. This is a large effect in percentage terms (83% of the sample mean) and equates to about $50 US or 10% of our sample's monthly income. The number of loans increases by 0.11, a 72% increase of the sample mean of 0.15.

The second panel of Table 4 presents results on overall formal sector borrowing. There is no

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information to the survey firm), and had poor outcomes compared to the treatment group (e.g., because they did not obtain credit).

[15] We define "close substitutes" to the treating lender as loans in the amount of 50,000 pesos or less (since the treating lender did not make loans larger than 25,000 pesos to first-time borrowers), from formal sector lenders with no collateral or group requirements that listed as either a rural bank or microlender by the MIX Market and/or Microfinance Council of the Philippines.

significant effect on any reported borrowing in the month before the survey,[16] but amount borrowed and the number of loans increase by roughly the same amount as in the first panel. This suggests that increases in formal sector borrowing are driven entirely by loans like the Lender's, and that the treatment did not crowd-in other types of formal sector borrowing like collateralized loans. This could be due to credit constraints, or because unsecured and secured loans are neither complements nor substitutes for our sample. Note that we again ignore loans larger than 50,000 pesos (thereby throwing out the largest 1% of formal sector loans), and here this restriction has some effect on the results: Appendix Table 2 shows that including all formal sector loans flips the sign and eliminates the significant treatment effect on loan amount. The effect on the number of loans gets a bit weaker but remains significant at the 90% level.

The third panel of Table 4 presents results on informal loans: those from friends and family, moneylenders, and borrowing circles. The point estimates are all negative, but do not indicate statistically significant decreases in informal debt outstanding in the month before the survey.[17] As discussed below, any reduction in informal borrowing seems to be the result of borrower choice rather than market constraints: Table 9 provides evidence that the treatment actually sharply *increased* access to informal borrowing.

The final panel of Table 4 presents results on overall borrowing. Relative to the formal sector categories, the standard errors increase, and the point estimates decrease, so there are no statistically significant results. This is most likely due to a lack of precision (caused in part by adding noise from unaffected loan types), rather than a true null result of not finding statistically or economically meaningful increases in overall borrowing.

Indeed, all of the above estimated treatment effects on borrowing are probably biased downward by borrower underreporting. More than half of respondents known, from the Lender's data, to have a loan outstanding from the Lender in the month before the survey do not report having a loan from the Lender (Appendix Table 3). Nearly half do not report *any* outstanding formal sector loan.[18] Prior evidence suggests that this level of underreporting is common in household surveys (Copestake et al. 2005; Karlan and Zinman 2008). Debt underreporting will bias these treatment effects on borrowing

[16] The survey also collects some, albeit less detailed, information on borrowing over the last 12 months. We present these results in Appendix Table 1.

[17] Appendix Table 1 shows a statistically significant decrease in the likelihood of any informal sector loan over the last 12 months.

[18] Conversely, only 3% of households reported having a loan outstanding from the Lender that did not appear in the Lender's administrative data.

outcomes downward if underreporting is more severe in levels in the treatment than in the control group.[19]

In all, the results on borrowing outcomes suggest that the treatment had some meaningful effects on borrowing. There is robust evidence that households who were assigned loans from the Lender shifted their borrowing composition towards formal sector loans like those offered by Lender. There is some evidence that this shift produced an overall increase in formal sector borrowing. We cannot rule out significant increases in overall borrowing, and our ability to detect (larger) effects on all of the borrowing outcomes are probably biased downward by respondent underreporting of debt. We find some evidence that borrowing increases are larger for males than for females, and for lower-income than for higher-income households.

C. Business Outcomes and Inputs, Table 5

As discussed at the outset, the theory and practice of microcredit posit a broad set of treatment effects that are of more ultimate interest than those on borrowing. Given that most microlenders (including ours) target microentrepreneurs, we start with measures of business activity.

Panel A presents intention-to-treat-effects on business "outcomes". Profit is arguably the most important outcome, as it is arguably the closest thing we have to a summary statistic on the success of the business and its ability to generate resources for the household. The full sample point estimate on last month's profits is positive and nontrivial in magnitude a roughly $50 US increase, compared to a sample mean of about $500.[20] Dropping the top and bottom percentile of profit reports from the sample (including 96 zeros) leaves the point estimate essentially unchanged, and reduces the standard error so that the p-value drops to 0.123. The point estimate on log profits is 0.05, but with standard error 0.10.[21]

The fact that microfinance often targets women, combined with the results in de Mel, McKenzie, and Woodruff (2008; 2009), suggest that it is important to explore gender differences in profitability. Our Columns 2 and 3 in Table 5 show some evidence that is broadly in lines with de Mel et al. Profits

[19] This will happen even if both groups underreport in the same proportion, so long as the treatment group obtains more loan in actuality. This is easiest to see by considering the limiting cases. Say 50% of the treatment group and 0% in the control group obtain loans. If only half of those obtaining loans report them, the true treatment effect is 50 percentage points, but the estimated treatment effect is only 25 percentage points. Now say 100% of the treatment group and 50% of the control group obtains loans. If only half of those obtaining loans report them (as assumed in the first case), then the true treatment effect is 100-50=50 percentage points, while the estimated treatment effect will be only 100*0.5-50*0.5 = 25 percentage points.

[20] We measure profits using the response to the question: "What was the total income each business earned during the past month after paying all expenses including wages of employees, but not including any income or goods paid to yourself? In other words, what were the profits of each business during the past month?" Including salary paid to the owner/operator does not materially change our measure of profits (this measure is correlated 0.97 with the measure based only on the profits question), or our estimates of treatment effects thereon.

[21] We do not find any significant correlations between treatment status and (non)response to the profit question.

increase for men, but less so and not statistically significantly for women. Each of the three profit point estimates for men are large, and statistically significant with at least 90% confidence. Each of the three point estimates for women are smaller and not statistically significant. However, if analyzed in one regression with an interaction term on female and treatment, the differences between the male and female profitability estimates are not significant at 90%. Furthermore, the small sample does not permit us to analyze whether the difference in returns for men and women is driven by social status, household bargaining, occupation/entrepreneurial choice, etc. Lastly, note that Table 4 suggests that larger profits may be an indicator of larger treatment effects on borrowing, rather than of higher returns to capital, for men.

The results by income suggest that effects on profits may be larger for those with relatively high incomes (Column 4 vs. Column 5). This is noteworthy in part because Table 4 suggests that treatment effects on borrowing are actually larger for lower-income households.[22] Taken together the results in Table 4 and 5 suggest that business returns to capital are relatively high for higher-income borrowers,, compared to alternative uses of loan proceeds. Lower-income borrowers may have lower returns to capital, and/or relatively high returns on household investments or consumption smoothing (although the results below provide little support for that story).

Table 5 Panel A also presents results on another key business outcome, total revenues. The point estimates for all three functional forms are negative, but imprecisely estimated.

Table 5 Panel B presents results for several measures of business "inputs" that, along with sales, we think of as proxies for the level and scope of business investment. The point estimates on inventory are imprecisely estimated, and sensitive to functional form. The other results here are surprising in that they point to decreases in the number of businesses,[23] and in the number of helpers in businesses owned by the household. The reduction in helpers is driven by paid (and non-household-member) employees.

In all, Table 5 suggests that treated microentrepreneurs used credit to re-optimize business investment in a way that produced smaller, lower-cost, and more profitable businesses. Profits increase in an absolute sense, suggesting that many microentrepreneurs employ workers with negative net productivity, and raising the question of why (and in particular, of why access to credit led them to reduce employment and increase profits). The various results below relating to risk management suggest an explanation that we discuss below (in sub-section G., and in the Conclusion).

[22] Appendix Table 3 suggests that the larger effects on borrowing for relatively low-income households may be due in part to more severe debt underreporting by relatively high-income households.

[23] The likelihood of any reported business activity in the household is quite high, 0.93 in the full sample, which is not surprising since the sample frame is composed entirely of people who had been in business for at least one year at the time of application. We do not find any treatment effect on the likelihood of any business activity.

D. Human Capital and Occupational Choice, Table 6

Table 6 presents estimated treatment effects on various types of human capital. The first row indicates no effect on the likelihood that the owner/operator has a second job. The second row shows no effect on the likelihood that a household member helps in a family business. The next two rows show that household member employment in other businesses drops (significantly and sharply for households with a male applicant). The last row suggests that instead of work, individuals are now in school: the likelihood of enrollment increases significantly (p-value = 0.061) in the male sub-sample. In all, the results suggest that (male) microentrepreneurs use loan proceeds to invest in human capital of their kids, rather than in capital specific to their businesses.

E. Non-Inventory Fixed Assets, Table 7

The possibility remains that our focus on inventory and labor inputs has overlooked fixed-capital investments in the business. Table 7 helps examine this, and does not find evidence of such investments. The first two rows present estimated treatment effects on the purchase or sale of many different types of non-inventory assets. We did not ask surveyors or respondents to distinguish between assets used for business or household production, given the nature of the non-inventory assets (computers, stoves, refrigerators, vehicles), and the closely-held nature of the businesses being studied. We do not find any significant effects in the full sample. The next rows present estimated treatment effects on surveyor observations of proxies for other types of investment. We find no full sample effects on building materials (wall, ceiling, or floor). The surveyor also recorded whether she observed a phone on the premises, and we do not find an effect on that.

Again, however, the absence of full sample effects should not obscure some potentially important heterogeneity. The quality of building materials drops significantly for treated males compared to controls. This suggests the treated males were reducing capital investment by deferring maintenance, or replacing roofs/walls/floors with lower-quality materials.[24] Similarly, lower-income treated applicants have lower-quality roof material (the point estimates on the other two materials are also negative), and are also significantly less likely to have a phone. In all these results suggest that increased access to credit may lead some microentrepreneurs to re-optimize into lower level of capital inputs into their businesses.

[24] It could be that entrepreneurs sold the higher-quality materials, or used them in their residence. Unfortunately we lack data on these potential channels.

F. Other Household Investments and Risk Management, Table 8

Table 8 presents treatment effects on the use of formal insurance, and on two other precautionary "investments" that plausibly relate to risk management: saving and sending remittances.

The results on formal insurance suggest that increased access to credit induces changes in risk management strategies. The effect on the likelihood of having health insurance is negative and insignificant in the full sample, with large and significant decreases in the male and higher-income sub-samples. The treatment effect on having any other insurance (life, home, property, fire, and car) is negative and significant in the full sample, with no evident differences across the sub-samples. The reductions in formal insurance are consistent with credit and formal insurance being substitutes, and/or with formal and informal insurance being substitutes; as documented directly below (Table 9), we find evidence of positive treatment effects on access to informal risk-sharing.

We do not find any significant effects on savings and remittance outcomes, although our confidence intervals include large effects on either side of zero. Note that although the bank does require savings deposits along with the loan, deposits may be withdrawn after a loan is paid off, and most of the treatment group had paid off their loans by the time of the follow-up survey (Appendix Table 3).

G. Informal Risk-Sharing: Trust and Informal Access, Table 9

Table 9 presents treatment effects that plausibly relate to informal risk-sharing.

The first four outcomes are measures of local trust (Cleary and Stokes 2006). The point estimates are positive on three out of the four measures (indicating more trust), and the increase on "trust in your neighborhood" is significant. Effects again seem to be stronger for males and higher-income applicants.

The next set of results point to increased access to financial assistance from friends or family in an emergency. We find no effects on the extensive margin (on a very high likelihood of being able to get any assistance: 0.9), but large and significant effects on the intensive margin: the ability to get 10,000 pesos of, or unlimited, assistance. Again, the effects are largest, and only significant for, male and higher-income respondents.[25]

In all, this table suggests that increased access to formal sector credit complements, rather than crowds-out, local and family risk-sharing mechanisms. Treated microentrepreneurs have more places to

[25] Our results on other subjective questions suggest that the positive effects on trust and perceived access to financial assistance are *not* due to the treatment group being artificially sanguine in response to subjective questions. The average treatment effect on subjective well-being is *negative* (Table 11). Another counterexample is the lack of a significant treatment effect on whether the respondent perceives lack of capital as a main business challenge; we discuss this finding further in the Conclusion.

turn for formal and informal credit in a pinch, and consequently rely less on formal sector insurance (Table 8). They may also rely less on informal insurance; the reduced likelihood of employing unproductive workers suggested by Table 5 may be an indicator of this. The drop in outside employment at the household level (Table 6) could be interpreted in a similar vein, as reduced reliance on diversification.

H. Household Income and Consumption, Table 10

Table 10 examines whether any profit increase translates into income and consumption changes (along with the increase in education investment suggested by Table 6). We look at four different functional forms of total household income and do not find any evidence that it increases, although our confidence intervals are wide. Nor do we find any significant effects on two key measures of consumption: food quality, and the likelihood of not visiting a doctor due to financial constraints. These "non-results" could be due to a combination of the earlier noted effects: business profits increased, but outside employment decreased (with an increase in school attendance), thus leading to no change in total household income or consumption.

I. Subjective Well-Being, Table 11

Table 11 presents treatment effects on nine different measures of the subjective well-being, based on responses to standard batteries of questions on optimism, calmness, (lack of) worry, life satisfaction, work satisfaction, (lack of) job stress, decision-making power, and socio-economic status (see Karlan and Zinman (forthcoming) for more details on these questions and their sources). In all cases higher values indicate better outcomes. We find no evidence of significant treatment effects on any of the individual measures. Examining sub-samples, we find only one effect: an increase in stress (i.e., a negative point estimate) for men. This coincides with results in Fernald et al (2008) from South Africa in which stress also increased as a result of getting access to credit. Overall, nearly all of the point estimates are negative, and aggregating the nine outcomes into a summary index (Kling et al. 2007) leads to a marginally significant (p-value = 0.079) decrease for the full sample. The implied effect size is small: a 0.06 standard deviation decrease in the average well-being outcome.

V. Conclusion

Theories marshaled in support of microcredit expansion assume that small businesses are credit constrained, and predict that expanding access to microcredit will lead to business growth. Other theories show that expanding access to formal credit may have indirect but potentially important effects on risk-management strategies and opportunities. We test these theories, and estimate a broader set of impacts of a microcredit expansion, using a randomized trial implemented by a bank in Metro Manila.

We also introduce quantitative credit scoring as a vehicle for generating exogenous variation in credit access, by introducing randomness into the approve/reject decision for applications with scores that fall within a predefined "gray area" range. This approach is cheap operationally, allows lenders to take controlled risks as they refine underwriting criteria, and holds promise for testing other margins of contracting (loan size, maturity, pricing, etc.) as well. The main disadvantage of using credit scoring as a randomization tool is that it only identifies treatment effects on the marginally creditworthy.

We find several noteworthy results. First, individuals assigned to the treatment group did borrow more than those in the control group, i.e., those rejected by this lender did not simply borrow elsewhere. This expanded use of credit then drives our results on more ultimate outcomes. Many of these results are quite surprising. Marginally creditworthy microentrepreneurs who randomly receive credit *shrink* their businesses relative to the control group. Nevertheless, following de Mel et al (2008; 2009), we find some evidence that expanding access to capital (credit in our case) increases profits for male but not for female microentrepreneurs. Males seem to use these increased profits to send a child to school (and we find an accompanying decrease in household members employed outside the family business). Overall, the treatment group also reports increased *access* to informal credit to absorb shocks, contrary to theories where formal credit crowds-out risk sharing arrangements by making it difficult to for those with better formal access to commit to reciprocation. We also find that access to credit substitutes for formal insurance. And we find no evidence that increased access to credit improves subjective well-being, as many microcredit advocates claim; rather, we find some evidence of a small *decline* in subjective well-being.

The results here have several implications. They provide tests of broad classes of theories, as noted above.[26] They call into question the wisdom of microcredit policies that target women and microentrepreneurs and exclude men and wage-earners. They support the hypothesis that the household financial arrangements in developing countries are complex (Collins et al. 2009), and hence that it is important to measure impacts on a broad set of behaviors, opportunity sets, and outcomes. Business outcomes are not a sufficient statistic for household welfare, nor even necessarily the locus of the biggest impacts of changing access to financial services.

Above all, our results highlight the importance of replicating tests of theories and interventions across

[26] It also bears mentioning that behavioral models could explain some but not all of the results. E.g., such models can explain why borrowers end up might needing to shrink their businesses-- because borrowing was an *ex-ante* bad decision reached as the result of some psychological bias— but struggle to explain why profits and school attendance increase among male borrowers.

different settings. Our findings add to a very muddled picture on the impacts (or lack thereof) of microcredit. At this point it remains to be seen whether different studies arrive at different estimates due to true underlying heterogeneity across settings, or to differences (and flaws) in some methodologies. One approach to solving this puzzle is to replicate research designs across settings. Random assignment via credit scoring is a viable tool for doing this, as it provides a win-win for lenders looking for an effective way to improve operations, and for other constituencies (researchers, donors, investors, and policymakers) looking for an effective way to measure impacts of expanding access to microcredit.

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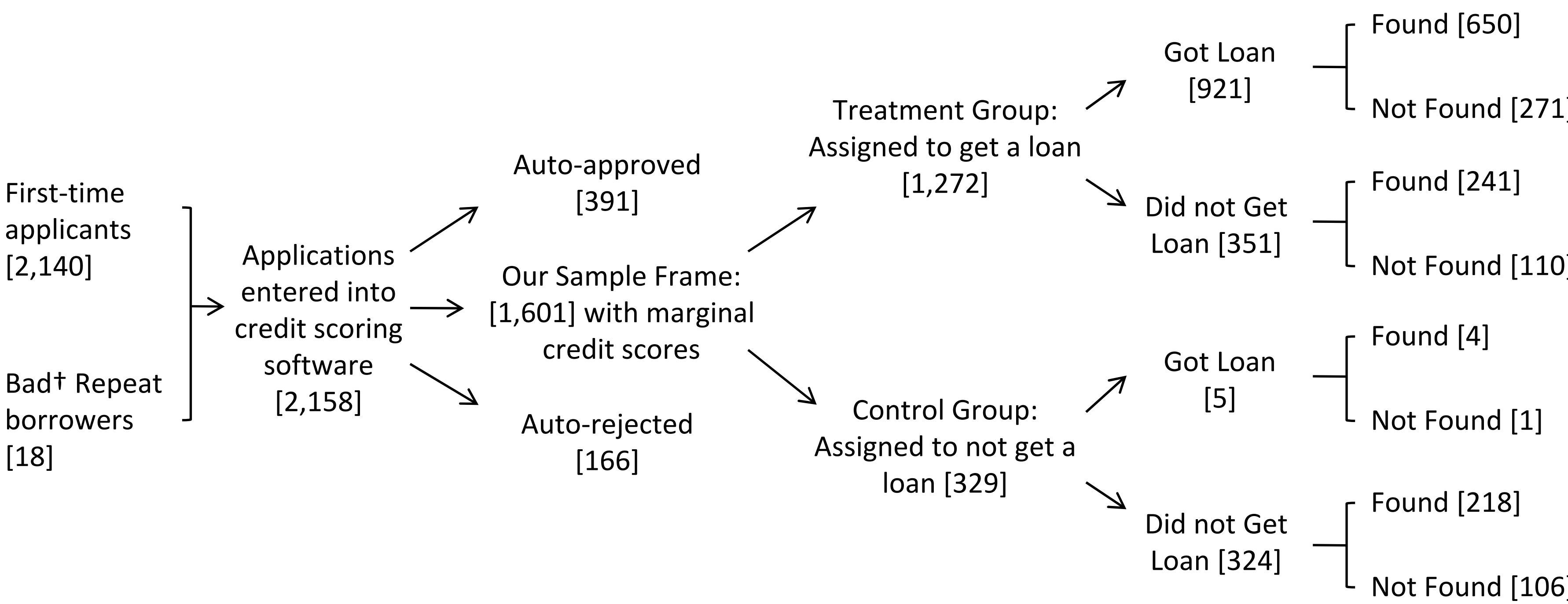
Figure 1. Sample Construction



† "Bad" defined as too many unexcused missed payments.

Possible Reasons for "Did not Get Loan" if Assigned to Treatment Group:

- Could not find suitable co-borrower;
- Discrepancies between self-provided application information and reality;
- Simply chose not to avail a loan at last minute;
- Prevented from availing loan by Account Officer (deemed unlikely due to anecdotal evidence and structure of incentive scheme).

Table 1. Demographics

	Our Sample Frame						Metro Manila		Philippines	
	All		Applicants with 60% chance of approval		Applicants with 80% chance of approval					
	Mean	Median	Mean	Median	Mean	Median	Mean	Median	Mean	Median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Applicant is female	85%	-	86%	-	85%	-				
Applicant is married	78%	-	53%	-	82%	-				
Age of applicant	42.1	42.0	41.8	42.0	42.1	42.0				
Education level of applicant†										
Elementary	11%	-	19%	-	10%	-	12%		33%	
High school	44%	-	49%	-	43%	-	42%		37%	
Post-secondary or college	45%	-	32%	-	47%	-	47%		31%	
Household size	5.1	5.0	5.0	5.0	5.1	5.0	5.0		5.0	
Number of dependents	2.28	2	2.29	2	2.28	2				
Applicant owns a sari-sari (corner) store	49%	-	55%	-	48%	-				
Monthly household income (Filipino pesos)††	24,920	17,245	19,524	14,150	25,826	17,800	25,917	18,333	14,417	9,250
Monthly household income per capita (Filipino pesos)	5,301	3,540	4,193	3,191	5,488	3,569	5,183	3,667	2,883	1,850
Number of businesses owned by household	1.15	1	1.20	1	1.14	1				
Applicant's business has employees	25%	-	17%	-	26%	-				

Sample frame data taken from Lender's application data unless otherwise noted. Per capita figures for Manila and the Philippines assumes average household size of 5.0 people. Source: http://www.census.gov.ph/data/quickstat/index.html.

† Education data on sample frame taken from the follow-up survey, where 97% of the sample frame is aged 20-59. Education data on Manila and the Philippines, restricted to Filipinos aged 20-59, taken from: http://www.census.gov.ph/data/sectordata/2003/fl03tabA.htm.

†† Monthly household income data on sample frame taken from the following questions from the follow-up survey: "How much was the total income (including remittances) earned by your household in the past month (gross calculation before expenses)?" less the sum total of "How much did each household business spend on each of the following categories of business expenses during the past month: [inventory, utility bills, wages and salaries for helpers, rent for machinery and equipment, rent for building and land, taxes, maintenance and general repairs, business-related transportation, and other expenses]?" Monthly household income data on Manila and the Philippines taken from: http://www.census.gov.ph/data/sectordata/2006/fies0607r.htm where, according to the Family Income and Expenditures Survey, "total family income includes primary income and receipts from other sources received by all family members ... and net receipts derived from the operation of family-operated enterprises/activities."

Table 2. Orthogonality of Treatment to Applicant Characteristics

Dependent Variable:	*1 = Loan Assigned*	*1 = Surveyed*		*1 = Loan Assigned*	
sample:	frame	frame	surveyed=1	surveyed=1, female	surveyed=1, male
Mean *(dependent variable)*	0.80	0.70	0.80	0.81	0.75
	(1)	(2)	(3)	(4)	(5)
Male	0.0576**		0.0492		
	(0.0286)		(0.0372)		
Marital status -- Married	-0.00487		-0.0181	0.0348	-0.224*
	(0.0376)		(0.0491)	(0.0560)	(0.120)
Marital Status -- Widowed / separated	-0.000186		0.0444	0.0846	-0.0157
	(0.0454)		(0.0575)	(0.0624)	(0.246)
Number of dependents	-0.00397		-0.00247	-0.00446	0.0103
	(0.00653)		(0.00753)	(0.00851)	(0.0198)
Age of applicant	0.000138		0.000512	0.000513	0.00590
	(0.00125)		(0.00152)	(0.00156)	(0.00541)
Education -- Some college	0.00234		-0.0243	-0.0157	-0.0710
	(0.0252)		(0.0307)	(0.0331)	(0.0904)
Education -- Graduated high school	-0.0172		-0.00900	-0.0130	0.0195
	(0.0245)		(0.0292)	(0.0307)	(0.111)
Education -- Some high school or less	-0.00576		0.0318	0.0259	-0.0261
	(0.0486)		(0.0525)	(0.0540)	(0.297)
Primary business location -- Poblacion	0.0150		0.0379	0.0271	0.101
	(0.0278)		(0.0335)	(0.0364)	(0.0957)
Primary business location -- Public market	-0.00625		0.0157	-0.0104	0.133
	(0.0335)		(0.0414)	(0.0444)	(0.106)
Primary business property arrangement -- Lease	-0.00103		0.0191	0.0108	-0.0604
	(0.0412)		(0.0550)	(0.0564)	(0.150)
Primary business property arrangement -- Rent	-0.0150		-0.0258	-0.0309	0.0281
	(0.0266)		(0.0331)	(0.0363)	(0.0967)
Primary business type -- Small grocery/convenience store	-0.0252		0.0111	0.00794	0.00447
	(0.0279)		(0.0331)	(0.0353)	(0.107)
Primary business type -- Wholesale	0.0278		0.0195	0.0317	-0.0164
	(0.0435)		(0.0608)	(0.0631)	(0.231)
Primary business type -- Service	0.00887		0.0301	0.0215	0.0900
	(0.0347)		(0.0432)	(0.0487)	(0.101)
Primary business type -- Manufacturing (not food processing)	-0.164**		-0.176*	-0.231**	0.0817
	(0.0801)		(0.0998)	(0.111)	(0.217)
Primary business type -- Food vending	-0.0239		-0.0123	0.000275	-0.0301
	(0.0385)		(0.0471)	(0.0497)	(0.164)
No regular employees in primary business	0.0198		0.0155	0.0147	-0.00344
	(0.0316)		(0.0387)	(0.0438)	(0.0955)
One regular employee in primary business	-0.0346		-0.00141	0.0378	-0.172
	(0.0311)		(0.0388)	(0.0416)	(0.118)
Log of years primary business in business	0.0103		-0.0206	-0.0151	-0.0591
	(0.0138)		(0.0165)	(0.0177)	(0.0533)
Log of net weekly cash flow	-0.00190		-0.00273	0.00135	-0.00617
	(0.0126)		(0.0152)	(0.0161)	(0.0465)
Randomized loan decision		0.0098			
		(0.0299)			
P-value on joint F-test: all RHS variables listed above > 0?	0.81		0.82	0.90	0.55
Number of Observations	1598	1601	1113	948	165

OLS with Huber-White standard errors in parentheses -- * significant at 10%; ** significant at 5%; *** significant at 1%. Sample frame contains 1,601 marginal applicants eligible for the treatment (i.e., for loan approval). Other regressors (not shown) are the randomization conditions (credit score cut-offs), appication month, application year, survey month, and survey year. 'Single' is the omitted marital status category. 'College graduate' is the omitted educational attainment variable. 'Barangay [neighborhood]' is the omitted primary business location variable. 'Own' is the omitted primary business property arrangement. 'Other retail)' is the omitted primary business type variable.

Table 3. Treatment Assignment and Treatment Status

Panel A. Entire Sample of Randomized Subjects

	Full sample			60% treatment probability		85% treatment probability	
	Loan Made?	Frequency	"Compliance" rate	Frequency	"Compliance" rate	Frequency	"Compliance" rate
Randomizer says to:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reject	no	324		114		210	
Reject	yes	5	0.98	1	0.99	4	0.98
Total assigned Reject		329		115		214	
Approve	yes	921		81		840	
Approve	no	351	0.72	60	0.57	291	0.74
Total assigned Approve		1272		141		1131	
Total attempted to survey		1601		256		1345	

Panel B. Those Subjects Reached for Survey

	Full sample			60% treatment probability		85% treatment probability	
	Loan Made?	Frequency	"Compliance" rate	Frequency	"Compliance" rate	Frequency	"Compliance" rate
Randomizer says to:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reject	no	218		72		146	
Reject	yes	4	0.98	1	0.99	3	0.98
Total assigned Reject		222		73		149	
Approve	yes	650		50		600	
Approve	no	241	0.73	38	0.57	203	0.75
Total assigned Approve		891		88		803	
Total reached for survey		1113		161		952	

Sample includes everyone reached for follow-up survey (Table 2 shows that being reached is uncorrelated with treatment assignment). "Compliance" rate does not have normative meaning: it simply refers to the proportion of application dispositions that matched the random assignment. Noncompliance with "approve" assignment was due to one of two unobservable reasons: 1) branch did *not* approve the loan despite the credit scoring software's instruction to approve; 2) branch *did* approve the loan, but the applicant ultimately chose not to take it.

Table 4: Intention-to-Treat Effects on Borrowing in Month Before Survey

	All	Female	Male	Above median income	Below median income
FORMAL SECTOR LOANS FROM TREATING LENDER OR CLOSE SUBSTITUTES	(1)	(2)	(3)	(4)	(5)
Any outstanding loan <= 50,000 pesos	0.096***	0.078***	0.163***	0.105***	0.084***
	(0.022)	(0.026)	(0.045)	(0.034)	(0.030)
	[0.145]	[0.149]	[0.122]	[0.150]	[0.139]
Level loan size for loans <=50,000 pesos	2,155.95***	1,790.57***	3,107.73***	2,911.40***	1,172.90***
	(435.58)	(490.89)	(988.21)	(741.68)	(404.95)
	[2,585.90]	[2,529.72]	[2,908.54]	[3,188.07]	[1,983.73]
Number of loans <=50,000 pesos	0.108***	0.090***	0.164***	0.121***	0.088***
	(0.024)	(0.028)	(0.046)	(0.038)	(0.030)
	[0.151]	[0.155]	[0.128]	[0.157]	[0.145]
ALL FORMAL SECTOR LOANS					
Any outstanding loan <= 50,000 pesos	0.015	0.003	0.089	-0.003	0.048
	(0.038)	(0.043)	(0.088)	(0.056)	(0.054)
	[0.408]	[0.419]	[0.341]	[0.394]	[0.421]
Level loan size for loans <=50,000 pesos	2,344.58**	1,979.24*	4,321.26**	1,968.02	3,006.18***
	(920.87)	(1,056.14)	(1,675.83)	(1,553.80)	(946.55)
	[7,202.26]	[7,371.87]	[6,228.05]	[7,706.51]	[6,698.01]
Number of loans <=50,000 pesos	0.094**	0.081	0.151*	0.070	0.132**
	(0.045)	(0.052)	(0.086)	(0.069)	(0.060)
	[0.445]	[0.466]	[0.323]	[0.427]	[0.463]
ALL INFORMAL SECTOR LOANS					
Any outstanding loan <= 50,000 pesos	-0.036	-0.036	-0.025	-0.064	-0.006
	(0.035)	(0.039)	(0.084)	(0.053)	(0.049)
	[0.246]	[0.241]	[0.274]	[0.253]	[0.239]
Level loan size for loans <=50,000 pesos	-786.03	-570.26	-1,296.70	-1,345.64	-390.37
	(728.76)	(777.11)	(2,224.04)	(1,255.21)	(692.15)
	[3,161.48]	[2,891.83]	[4,710.37]	[3,907.78]	[2,415.19]
Number of loans <=50,000 pesos	-0.011	-0.008	-0.013	-0.052	0.032
	(0.042)	(0.046)	(0.103)	(0.061)	(0.057)
	[0.273]	[0.268]	[0.305]	[0.284]	[0.262]
ALL LOAN TYPES					
Any outstanding loan <= 50,000 pesos	0.003	-0.008	0.045	-0.048	0.061
	(0.039)	(0.044)	(0.094)	(0.056)	(0.056)
	[0.538]	[0.550]	[0.470]	[0.528]	[0.548]
Level loan size for loans <=50,000 pesos	1,525.85	1,367.62	3,024.56	590.88	2,625.81**
	(1,236.80)	(1,392.07)	(2,954.26)	(2,099.08)	(1,202.42)
	[10,456.78]	[10,372.93]	[10,938.41]	[11,778.12]	[9,135.44]
Number of loans <=50,000 pesos	0.066	0.053	0.138	-0.009	0.164*
	(0.066)	(0.075)	(0.138)	(0.098)	(0.089)
	[0.733]	[0.752]	[0.628]	[0.734]	[0.732]
Number of Observations	1106	942	164	553	553

OLS with Huber-White standard errors in parentheses -- * significant at 10%; ** significant at 5%; *** significant at 1% -- followed by the mean of the dependent variable in brackets. Each cell presents the estimate intention-to-treat effect (i.e., the result on the treatment assignment variable) for the borrowing outcome in that row, and the (sub)-sample in that column. All results are conditional on the randomization conditions (credit score cut-offs), appication month, application year, survey month, and survey year. "Formal" sector loans are defined as loans from commercial, thrift, and rural banks (including mortgages), lending organizations, NGOs, cooperatives, and employers (including salary advances). "Informal" sector loans are defined as loans from paluwagans (savings groups), bombays (moneylenders), 5-6ers (borrow 5, repay 6), family, and friends. "All" loan types are defined as formal and informal sector loans, plus loans from pawnshops. "Close substitutes" to the treating lender are defined as formal sector lenders with no collateral or group requirements, listed as either a rural bank or microlender by the MIX Market and/or Microfinance Council of the Philippines.

Table 5: Intention-to-Treat Effects on Business Outcomes and Inputs

	All	Female	Male	Above median income	Below median income
	(1)	(2)	(3)	(4)	(5)
Panel A. Business Outcomes					
Total Profit in All Household Businesses in Month Before Survey: Profit Directly Reported	2,482.57	2,225.37	12,665.61*	4,795.85	680.30
	(2,114.02)	(2,407.01)	(7,642.53)	(3,700.34)	(2,338.35)
	[17,074.62]	[16,622.81]	[19,610.35]	[21,807.33]	[12,341.91]
	1,058	898	160	529	529
Total Profit in All Household Businesses in Month Before Survey: Profit Directly Reported (trim top and bottom percentiles)	2,340.28	2,623.66	7,363.89*	4,488.16**	126.14
	(1,515.42)	(1,787.34)	(3,792.71)	(2,215.95)	(2,163.64)
	[16,945.48]	[16,725.04]	[18,167.06]	[19,543.59]	[14,211.52]
	942	798	144	483	459
Log of Total Profit in All Household Businesses in Month Before Survey: Profit Directly Reported	0.052	0.054	0.370*	0.115	0.017
	(0.096)	(0.110)	(0.205)	(0.130)	(0.147)
	[9.178]	[9.142]	[9.378]	[9.349]	[8.996]
	952	807	145	490	462
Total Sales in All Household Businesses in Month Before Survey	-4,312.06	-756.70	-10,083.69	-2,471.65	-3,689.99
	(7,008.00)	(7,811.85)	(16,312.34)	(11,417.31)	(8,192.00)
	[57,319.51]	[56,822.15]	[60,148.28]	[72,459.45]	[42,065.95]
	1,070	910	160	537	533
Total Sales in All Household Businesses in Month Before Survey (trim top and bottom percentiles)	-3,025.70	1,885.65	-16,803.87	-244.54	-4,502.10
	(6,333.55)	(6,843.71)	(16,173.31)	(9,011.81)	(9,064.00)
	[56,691.95]	[55,597.53]	[62,977.26]	[66,293.45]	[46,499.27]
	971	827	144	500	471
Log of Total Sales in All Household Businesses in Month Before Survey	-0.017	0.045	-0.076	-0.008	0.005
	(0.101)	(0.111)	(0.228)	(0.150)	(0.134)
	[10.389]	[10.361]	[10.551]	[10.531]	[10.237]
	981	836	145	509	472
Panel B. Business Inputs					
Total Current Market Value of Inventory in All Household Businesses	-10,913.01	-12,789.48	1,742.90	-29,714.05	5,628.68
	(15,736.38)	(18,852.11)	(28,541.58)	(30,374.65)	(10,374.67)
	[43,572.77]	[39,185.56]	[69,395.46]	[59,300.97]	[27,534.96]
	1,026	877	149	518	508
Total Current Market Value of Inventory in All Household Businesses (trim top and bottom percentiles)	788.92	3,748.44	9,397.44	4,037.93	-5,227.20
	(7,072.32)	(6,118.18)	(29,748.70)	(11,287.38)	(7,902.10)
	[36,594.43]	[30,894.93]	[70,158.13]	[47,344.36]	[25,440.75]
	868	742	126	442	426
Log of Total Current Market Value of Inventory in All Household Businesses	0.039	0.077	0.207	0.090	-0.059
	(0.152)	(0.166)	(0.469)	(0.226)	(0.206)
	[9.278]	[9.243]	[9.483]	[9.525]	[9.019]
	878	751	127	450	428
Number of Businesses in Household	-0.102*	-0.062	-0.277	-0.073	-0.139
	(0.060)	(0.061)	(0.172)	(0.073)	(0.100)
	[1.282]	[1.287]	[1.255]	[1.282]	[1.282]
	1,113	948	165	556	557
Number of Helpers in All Household Businesses	-0.261*	-0.156	-0.645	-0.451**	-0.111
	(0.134)	(0.137)	(0.411)	(0.223)	(0.140)
	[1.051]	[1.022]	[1.212]	[1.298]	[0.805]
	1,104	939	165	551	553
Number of Paid Helpers (not Including In-kind Contributions) in All Household Businesses	-0.273**	-0.248*	-0.276	-0.397*	-0.181
	(0.123)	(0.130)	(0.321)	(0.208)	(0.124)
	[0.698]	[0.659]	[0.921]	[0.953]	[0.443]
	1,113	948	165	556	557
Number of Unpaid Helpers (not Including In-kind Contributions) in All Household Businesses	0.028	0.106*	-0.367	-0.059	0.097
	(0.071)	(0.058)	(0.290)	(0.115)	(0.082)
	[0.312]	[0.315]	[0.297]	[0.291]	[0.334]
	1,113	948	165	556	557

* p<0.10, ** p<0.05, *** p<0.01. Each cell presents the OLS estimate on the variable for 1= assigned a loan. Huber-White standard errors in parentheses. Mean of the dependent variable in brackets. Number of observations is listed below mean. All regressions include controls for the probability of assignment to treatment (60% or 85%), survey month, survey year, application month, and application year. All sample restrictions based on application data. To determine profits, we asked: "What was the total income each business earned during the past month after paying all expenses including wages of employees, but not including any income or goods paid to yourself? In other words, what were the profits of each business during the past month?"

Table 6: Intention-to-Treat Effects on Other Human Capital and Occupational Choice

	All	Female	Male	Above median income	Below median income
Business Owner/Operator has Second Job Outside the Business	-0.006	-0.001	-0.065	-0.025	0.011
	(0.029)	(0.031)	(0.085)	(0.043)	(0.039)
	[0.176]	[0.160]	[0.267]	[0.178]	[0.174]
	1,113	948	165	556	557
Any Household Member Helping in Family Business	-0.058	-0.058	-0.001	-0.066	-0.036
	(0.039)	(0.044)	(0.096)	(0.054)	(0.056)
	[0.525]	[0.505]	[0.636]	[0.588]	[0.461]
	1,113	948	165	556	557
Any Household Member Employed Outside the Family Business	-0.047	-0.022	-0.230**	-0.078	-0.019
	(0.039)	(0.044)	(0.096)	(0.055)	(0.056)
	[0.527]	[0.540]	[0.455]	[0.480]	[0.575]
	1,113	948	165	556	557
Any Overseas Foreign Workers in Household	-0.013	-0.004	-0.060	0.002	-0.028
	(0.019)	(0.021)	(0.050)	(0.023)	(0.033)
	[0.058]	[0.062]	[0.036]	[0.043]	[0.074]
	1,113	948	165	556	557
Any Students in Household	-0.014	-0.043	0.168*	-0.051	0.017
	(0.033)	(0.035)	(0.089)	(0.045)	(0.049)
	[0.758]	[0.763]	[0.733]	[0.764]	[0.752]
	1,113	948	165	556	557

* $p<0.10$, ** $p<0.05$, *** $p<0.01$. Each cell presents the OLS estimate on the variable for 1= assigned a loan. Huber-White standard errors in parentheses. Mean of the dependent variable in brackets. Number of observations is listed below mean. All regressions include controls for the probability of assignment to treatment (60% or 85%), survey month, survey year, application month, and application year. All sample restrictions based on application data. Lower randomization window corresponds to a 60% probability of assignment to treatment. Higher randomization window corresponds to 85% probability of assignment to treatment.

Table 7: Intention-to-Treat Effects on Non-Inventory Fixed Assets

	All	Female	Male	Above median income	Below median income
Purchased Any Assets in 12 Months Prior to Survey	0.023	0.034	-0.033	0.088*	-0.037
	(0.033)	(0.037)	(0.080)	(0.047)	(0.048)
	[0.245]	[0.252]	[0.207]	[0.265]	[0.226]
	1,104	940	164	551	553
Sold Any Assets in 12 Months Prior to Survey	-0.014	-0.021	0.032	-0.020	-0.013
	(0.020)	(0.022)	(0.057)	(0.029)	(0.028)
	[0.070]	[0.068]	[0.085]	[0.062]	[0.078]
	1,095	931	164	546	549
Wall Material is Finished Concrete (omitted: semi- or unfinished concrete, wood, plain GI sheet, salvaged or scrap materials, and bamboo)	0.014	0.044	-0.155*	0.072	-0.059
	(0.039)	(0.043)	(0.091)	(0.056)	(0.055)
	[0.536]	[0.531]	[0.570]	[0.558]	[0.515]
	1,113	948	165	556	557
Floor Material is Marble or Finished Concrete (omitted: ceramic or vinyl tiles, unfinished concrete, wood, earth, sand, and bamboo)	-0.013	0.038	-0.219***	0.032	-0.071
	(0.036)	(0.040)	(0.076)	(0.051)	(0.052)
	[0.687]	[0.684]	[0.709]	[0.701]	[0.673]
	1,113	948	165	556	557
Roof Material is Concrete Slab or Metal Sheet (omitted: tiles, salvaged or scrap, and other)	-0.010	0.021	-0.138***	0.041	-0.080**
	(0.027)	(0.031)	(0.052)	(0.039)	(0.036)
	[0.872]	[0.868]	[0.891]	[0.879]	[0.864]
	1,113	948	165	556	557
Owns a Phone (landline and/or cell phone)	-0.040	-0.041	0.019	0.016	-0.090**
	(0.029)	(0.031)	(0.066)	(0.041)	(0.041)
	[0.828]	[0.826]	[0.838]	[0.838]	[0.817]
	1,079	919	160	544	535

* p<0.10, ** p<0.05, *** p<0.01. Each cell presents the OLS estimate on the variable for 1= assigned a loan. Huber-White standard errors in parentheses. Mean of the dependent variable in brackets. Number of observations is listed below mean. All regressions include controls for the probability of assignment to treatment (60% or 85%), survey month, survey year, application month, and application year. All sample restrictions based on application data. Lower randomization window corresponds to a 60% probability of assignment to treatment. Higher randomization window corresponds to 85% probability of assignment to treatment.

Table 8: Intention-to-Treat Effects on Other Household Investments and Risk Management

	All	Female	Male	Above median income	Below median income
Any Health Insurance	-0.035	-0.014	-0.185**	-0.117**	0.039
	(0.038)	(0.043)	(0.092)	(0.052)	(0.057)
	[0.644]	[0.645]	[0.636]	[0.640]	[0.648]
	1,112	947	165	555	557
Any Other Type of Insurance	-0.079**	-0.070	-0.121	-0.101*	-0.071
	(0.039)	(0.043)	(0.095)	(0.056)	(0.055)
	[0.436]	[0.433]	[0.454]	[0.473]	[0.400]
	1,105	942	163	552	553
Any Savings in Household	0.002	-0.008	0.059	0.072	-0.088
	(0.039)	(0.043)	(0.096)	(0.055)	(0.054)
	[0.600]	[0.597]	[0.616]	[0.656]	[0.545]
	1,108	944	164	552	556
Any Remittances Sent by Household	0.009	-0.015	0.096	-0.033	0.050
	(0.034)	(0.038)	(0.073)	(0.049)	(0.047)
	[0.235]	[0.237]	[0.226]	[0.245]	[0.225]
	1,106	942	164	554	552

* $p<0.10$, ** $p<0.05$, *** $p<0.01$. Each cell presents the OLS estimate on the variable for 1= assigned a loan. Huber-White standard errors in parentheses. Mean of the dependent variable in brackets. Number of observations is listed below mean. All regressions include controls for the probability of assignment to treatment (60% or 85%), survey month, survey year, application month, and application year. All sample restrictions based on application data. Lower randomization window corresponds to a 60% probability of assignment to treatment. Higher randomization window corresponds to 85% probability of assignment to treatment.

Table 9: Intention-to-Treat Effects on Trust & Informal Access

	All	Female	Male	Above median income	Below median income
Ordered Probit					
Trust that you would not be taken advantage of you	-0.060	-0.087	0.056	0.023	-0.163
(1= People would take advantage, 10= People	(0.082)	(0.092)	(0.183)	(0.113)	(0.118)
would be fair)	[7.596]	[7.584]	[7.665]	[7.569]	[7.623]
	1,107	943	164	552	555
Trust in your neighborhood	0.209**	0.186*	0.382*	0.511***	-0.036
(-4 = No trust, -1 = Complete trust)	(0.090)	(0.099)	(0.215)	(0.132)	(0.119)
	[-2.147]	[-2.153]	[-2.109]	[-2.141]	[-2.153]
	1,110	945	165	554	556
Trust in people you know personally	0.036	-0.007	0.255	0.219*	-0.121
(-4 = No trust, -1 = Complete trust)	(0.093)	(0.102)	(0.222)	(0.133)	(0.130)
	[-1.899]	[-1.903]	[-1.879]	[-1.917]	[-1.881]
	1,110	945	165	554	556
Trust in your business associates	0.101	0.066	0.300	0.157	0.048
(-4 = No trust, -1 = Complete trust)	(0.089)	(0.101)	(0.188)	(0.131)	(0.122)
	[-2.179]	[-2.178]	[-2.184]	[-2.185]	[-2.173]
	1,105	942	163	551	554
OLS					
Could get financial assistance from family or	0.010	-0.005	0.087	0.020	0.002
friends in an emergency.	(0.027)	(0.030)	(0.068)	(0.037)	(0.042)
	[0.895]	[0.889]	[0.934]	[0.902]	[0.889]
	995	844	151	499	496
Could get 10,000 pesos-worth of financial	0.102***	0.084*	0.168*	0.168***	0.047
assistance from family or friends in an emergency.	(0.040)	(0.044)	(0.100)	(0.058)	(0.055)
	[0.447]	[0.447]	[0.450]	[0.517]	[0.377]
	995	844	151	499	496
Could get unlimited financial assistance from	0.090**	0.071*	0.161*	0.130**	0.057
family or friends in an emergency.	(0.035)	(0.040)	(0.086)	(0.053)	(0.048)
	[0.322]	[0.322]	[0.318]	[0.351]	[0.292]
	995	844	151	499	496

* p<0.10, ** p<0.05, *** p<0.01. For the first four outcome measures -- all relating to trust -- each cell presents the ordered probit estimate on the variable for 1=assigned a loan. For the last two outcome measures and the summary index, each cell presents the OLS estimate on the variable for 1= assigned a loan. Huber-White standard errors in parentheses. Mean of the dependent variable in brackets, followed by the number of observations. All regressions include controls for the probability of assignment to treatment (60% or 85%), survey month, survey year, application month, and application year. All sample restrictions based on application data. Lower randomization window corresponds to a 60% probability of assignment to treatment. Higher randomization window corresponds to 85% probability of assignment to treatment.

Table 10: Intention-to-Treat Effects on Household Income and Consumption

	All	Female	Male	Above median income	Below median income
Total Income in Household in Last Month	-3,574.05	-1,043.00	-11,652.46	551.72	-5,238.25
	(7,287.46)	(8,282.06)	(14,542.11)	(12,647.67)	(7,951.41)
	[64,447.18]	[65,302.00]	[59,505.25]	[79,993.06]	[49,269.42]
	1,085	925	160	536	549
Total Income in Household in Last Month (trim top and bottom percentiles)	-35.03	3,591.43	-14,941.27	7,087.84	-5,590.14
	(5,966.72)	(6,589.83)	(15,227.09)	(8,800.49)	(8,208.28)
	[60,569.57]	[60,556.82]	[60,642.29]	[71,342.96]	[50,276.78]
	1,052	895	157	514	538
Log of Total Income in Household in Last Month	-0.078	-0.046	-0.188	-0.051	-0.104
	(0.086)	(0.095)	(0.208)	(0.134)	(0.110)
	[10.525]	[10.515]	[10.576]	[10.676]	[10.378]
	1,062	905	157	524	538
Household Is Above Poverty Line	0.019	0.020	0.036	0.019	0.007
	(0.024)	(0.027)	(0.056)	(0.036)	(0.033)
	[0.904]	[0.900]	[0.925]	[0.909]	[0.898]
	1,078	919	159	530	548
Any Remittances Received by Household	-0.001	-0.024	0.037	0.054	-0.061
	(0.038)	(0.042)	(0.089)	(0.052)	(0.055)
	[0.346]	[0.359]	[0.268]	[0.338]	[0.354]
	1,107	943	164	554	553
Food Quality Has Improved in the Last 12 Months	0.002	-0.006	0.104	-0.016	0.020
	(0.040)	(0.044)	(0.103)	(0.057)	(0.056)
	[0.497]	[0.491]	[0.533]	[0.514]	[0.479]
	1,113	948	165	556	557
No One in Household Prevented from Visiting Doctor Due to Financial Constraints	-0.002	0.009	-0.095	0.002	0.002
	(0.032)	(0.036)	(0.074)	(0.047)	(0.042)
	[0.793]	[0.790]	[0.809]	[0.789]	[0.797]
	1,088	926	162	540	548

* p<0.10, ** p<0.05, *** p<0.01. Each cell presents the OLS estimate on the variable for 1= assigned a loan. Huber-White standard errors in parentheses. Mean of the dependent variable in brackets. Number of observations is listed below mean. All regressions include controls for the probability of assignment to treatment (60% or 85%), survey month, survey year, application month, and application year. All sample restrictions based on application data. Lower randomization window corresponds to a 60% probability of assignment to treatment. Higher randomization window corresponds to 85% probability of assignment to treatment.

Table 11: Intention-to-Treat Effects on Subjective Well-Being Measures

	All	Female	Male	Above median income	Below median income
Optimism (Scale: 6-30, low-to-high)	-0.123	0.073	-0.809	-0.296	0.033
	(0.229)	(0.251)	(0.555)	(0.317)	(0.340)
	[22.216]	[22.164]	[22.515]	[22.437]	[21.996]
	1,105	940	165	551	554
Calmness (Scale: 1-6, low-to-high)	-0.075	-0.105	0.150	-0.063	-0.067
	(0.095)	(0.107)	(0.237)	(0.140)	(0.136)
	[2.499]	[2.473]	[2.644]	[2.488]	[2.509]
	1,095	932	163	543	552
No Worry (1 = Has not had a month in the past year during which respondent felt mostly worried)	-0.007	0.001	-0.084	0.060	-0.050
	(0.038)	(0.043)	(0.094)	(0.055)	(0.054)
	[0.566]	[0.561]	[0.595]	[0.565]	[0.567]
	1,094	931	163	542	552
Life Satisfaction (Scale: 1-4, 1=Not at All, 4=Very)	0.016	-0.034	0.159	0.073	-0.034
	(0.063)	(0.068)	(0.162)	(0.093)	(0.088)
	[2.827]	[2.830]	[2.806]	[2.880]	[2.773]
	1,108	943	165	552	556
Job Satisfaction (Scale: 1-10, low-to-high)	-0.012	-0.068	0.169	-0.022	0.007
	(0.137)	(0.149)	(0.355)	(0.191)	(0.201)
	[6.615]	[6.599]	[6.709]	[6.658]	[6.573]
	1102	937	165	549	553
Job Stress (Scale: -12 to 0 : 0 = no stress, -12 = always stressed, tired, prevented from giving time to family/partner)	-0.190	0.042	-0.993*	-0.369	-0.024
	(0.227)	(0.257)	(0.524)	(0.313)	(0.325)
	[-6.829]	[-6.845]	[-6.738]	[-6.432]	[-7.221]
	1,062	902	160	528	534
Decision Making Power (Scale: 0-26, low-to-high)	-0.233	-0.290	-0.033	-0.367	-0.033
	(0.302)	(0.360)	(0.530)	(0.437)	(0.438)
	[10.384]	[10.480]	[9.902]	[10.453]	[10.317]
	797	665	132	393	404
Place on Socio-Economic Ladder Compared to Others in Village (1-10)	-0.077	-0.050	-0.285	-0.058	-0.065
	(0.101)	(0.112)	(0.259)	(0.146)	(0.144)
	[5.690]	[5.684]	[5.727]	[5.803]	[5.578]
	1110	945	165	553	557
Place on Socio-Economic Ladder Compared to Others in Philippines (1-10)	-0.162	-0.186	-0.146	-0.177	-0.127
	(0.122)	(0.134)	(0.319)	(0.170)	(0.180)
	[4.947]	[4.942]	[4.976]	[5.067]	[4.828]
	1110	945	165	553	557
Summary Index of above outcomes; coefficients in standard deviation units of average outcome	-0.053*	-0.046	-0.108	-0.050	-0.042
	(0.030)	(0.033)	(0.084)	(0.043)	(0.043)
	[-0.034]	[-0.043]	[0.022]	[0.017]	[-0.084]
	1,113	948	165	556	557

* p<0.10, ** p<0.05, *** p<0.01. Each cell presents the OLS estimate on the variable for 1= assigned a loan. Huber-White standard errors in parentheses. Mean of the dependent variable in brackets. Number of observations is listed below mean. All regressions include controls for the probability of assignment to treatment (60% or 85%), survey month, survey year, application month, and application year. All sample restrictions based on application data. Lower randomization window corresponds to a 60% probability of assignment to treatment. Higher randomization window corresponds to 85% probability of assignment to treatment. "Has Employees" corresponds to having one or more full time salaried employees. Sample for decision making power scale is all individuals that are either "married & living with partner" or "not married, but living with partner" (excludes: "single," "divorced/separated," "not living with partner, but married," and "widowed"). Six respondents with nonresponse on all decision making questions are excluded.

Appendix Table 1. Intention-to-Treat Effects on Borrowing Over the Last 12 Months (compare to Table 4)

	In Last 12 Months Before Survey				
	Full Sample	Gender		Income	
		Female	Male	High	Low
ALL FORMAL SECTOR LOANS					
Any outstanding loan	0.014	-0.005	0.110	0.004	0.038
	(0.035)	(0.037)	(0.103)	(0.050)	(0.050)
	[0.741]	[0.760]	[0.636]	[0.740]	[0.743]
Number of loans	0.278*	0.294*	0.051	0.178	0.409**
	(0.153)	(0.157)	(0.424)	(0.239)	(0.196)
	[1.991]	[2.077]	[1.494]	[1.994]	[1.987]
ALL INFORMAL SECTOR LOANS					
Any outstanding loan	-0.087**	-0.085*	-0.101	-0.093	-0.068
	(0.040)	(0.044)	(0.100)	(0.057)	(0.057)
	[0.445]	[0.436]	[0.494]	[0.450]	[0.439]
Number of loans	-0.016	0.045	-0.452	-0.777**	0.761*
	(0.286)	(0.311)	(0.614)	(0.360)	(0.429)
	[1.571]	[1.580]	[1.519]	[1.408]	[1.732]
ALL LOAN TYPES					
Any outstanding loan	-0.036	-0.041*	-0.020	-0.060**	-0.005
	(0.024)	(0.025)	(0.068)	(0.028)	(0.039)
	[0.887]	[0.895]	[0.846]	[0.898]	[0.877]
Number of loans	0.208	0.308	-0.587	-0.649	1.133**
	(0.331)	(0.352)	(0.821)	(0.445)	(0.483)
	[3.647]	[3.745]	[3.080]	[3.521]	[3.772]
Attempted to avail a loan but was denied	-0.064**	-0.058*	-0.107	-0.099**	-0.035
	(0.029)	(0.031)	(0.065)	(0.043)	(0.037)
	[0.051]	[0.049]	[0.220]	[0.080]	[0.068]
Number of Observations	1102	940	162	549	553

OLS with Huber-White standard errors in parentheses -- * significant at 10%; ** significant at 5%; *** significant at 1% -- followed by the mean of the dependent variable in brackets. Each cell presents the estimate intention-to-treat effect (i.e., the result on the treatment assignment variable) for the borrowing outcome in that row, and the (sub)-sample in that column. All results are conditional on the randomization conditions (credit score cut-offs), appication month, application year, survey month, and survey year. "Formal" sector loans are defined as loans from commercial, thrift, and rural banks (including mortgages), lending organizations, NGOs, cooperatives, and employers (including salary advances). "Informal" sector loans are defined as loans from paluwagans (savings groups), bombays (moneylenders), 5-6ers (borrow 5, repay 6), family, and friends. "All" loan types are defined as formal and informal sector loans, plus loans from pawnshops. "Close substitutes" to the treating lender are defined as formal sector lenders with no collateral or group requirements, listed as either a rural bank or microlender by the MIX Market and/or Microfinance Council of the Philippines. Survey did not collect loan amount for loans obtained in last 12 months, only for loans outstanding in the last month (see table 4 and Appendix Table 2).

Appendix Table 2. Intention-to-Treat Effects on Borrowing, Including Loans >50,000 Pesos (compare to Table 4)

		In Month Before Survey			
	Full Sample	Gender		Income	
		Female	Male	High	Low
FORMAL SECTOR LOANS FROM TREATING LENDER OR CLOSE SUBSTITUTES					
Any outstanding loan	0.095***	0.073***	0.162***	0.100***	0.089***
	(0.023)	(0.027)	(0.047)	(0.037)	(0.030)
	[0.152]	[0.155]	[0.134]	[0.163]	[0.141]
Level loan size for loans	2,028.08***	1,223.35	3,052.13*	2,667.23**	1,172.90***
	(727.01)	(817.43)	(1,556.42)	(1,344.72)	(404.95)
	[3,098.55]	[2,924.63]	[4,097.56]	[4,213.38]	[1,983.73]
Number of loans	0.107***	0.085***	0.163***	0.116***	0.094***
	(0.025)	(0.030)	(0.049)	(0.041)	(0.031)
	[0.158]	[0.161]	[0.140]	[0.170]	[0.146]
ALL FORMAL SECTOR LOANS					
Any outstanding loan	-0.012	-0.035	0.098	-0.041	0.033
	(0.039)	(0.043)	(0.089)	(0.056)	(0.055)
	[0.447]	[0.458]	[0.384]	[0.454]	[0.439]
Level loan size for loans	-12,897.48	-19,209.21	9,141.22**	-32,661.53	7,169.05
	(11,914.05)	(14,686.34)	(3,671.51)	(24,393.09)	(5,942.06)
	[17,375.86]	[18,382.48]	[11,593.90]	[21,778.84]	[12,972.88]
Number of loans	0.080*	0.044	0.215**	0.044	0.129**
	(0.048)	(0.054)	(0.094)	(0.073)	(0.063)
	[0.496]	[0.515]	[0.390]	[0.503]	[0.490]
ALL INFORMAL SECTOR LOANS					
Any outstanding loan	-0.031	-0.033	-0.022	-0.054	-0.006
	(0.036)	(0.039)	(0.084)	(0.053)	(0.049)
	[0.255]	[0.251]	[0.280]	[0.269]	[0.241]
Level loan size for loans	-185.48	-123.23	-745.53	-37.57	-286.94
	(964.36)	(1,049.37)	(2,538.67)	(1,761.03)	(705.85)
	[4,147.02]	[3,889.70]	[5,625.00]	[5,770.34]	[2,523.69]
Number of loans	-0.004	-0.001	-0.009	-0.039	0.034
	(0.042)	(0.046)	(0.103)	(0.062)	(0.057)
	[0.285]	[0.280]	[0.311]	[0.304]	[0.266]
ALL LOAN TYPES					
Any outstanding loan	-0.008	-0.033	0.086	-0.066	0.056
	(0.039)	(0.043)	(0.095)	(0.055)	(0.056)
	[0.577]	[0.590]	[0.590]	[0.591]	[0.564]
Level loan size for loans	-13,115.67	-19,373.79	8,395.69*	-32,730.60	6,892.10
	(11,950.08)	(14,717.77)	(4,957.05)	(24,470.65)	(5,984.25)
	[21,615.91]	[22,381.42]	[17,218.90]	[27,713.02]	[15,518.81]
Number of loans	0.059	0.023	0.206	-0.023	0.163*
	(0.067)	(0.076)	(0.142)	(0.101)	(0.091)
	[0.797]	[0.813]	[0.701]	[0.830]	[0.763]
Number of Observations	1106	942	164	553	553

OLS with Huber-White standard errors in parentheses -- * significant at 10%; ** significant at 5%; *** significant at 1% -- followed by the mean of the dependent variable in brackets. Each cell presents the estimate intention-to-treat effect (i.e., the result on the treatment assignment variable) for the borrowing outcome in that row, and the (sub)-sample in that column. All results are conditional on the randomization conditions (credit score cut-offs), appication month, application year, survey month, and survey year. "Formal" sector loans are defined as loans from commercial, thrift, and rural banks (including mortgages), lending organizations, NGOs, cooperatives, and employers (including salary advances). "Informal" sector loans are defined as loans from paluwagans (savings groups), bombays (loan sharks), 5-6ers (borrow 5, repay 6), family, and friends. "All" loan types are defined as formal and informal sector loans, plus loans from pawnshops. "Close substitutes" to the treating lender are defined as formal sector lenders with no collateral or group requirements, listed as either a rural bank or microlender by the MIX Market and/or Microfinance Council of the Philippines.

Appendix Table 3. Debt Underreporting

	IN MONTH BEFORE THE SURVEY					
	Loan From Lender			Number of Loans From Lender		
	Proportion Borrowing from Participating Lender, Self-report from Survey	Proportion Borrowing from Participating Lender, Administrative Data	T-test for Difference and (Standard Error) for Comparison of Proportions	Mean Number of Loans from Participating Lender, Survey Self-report	Mean Number of Loans from Participating Lender, Administrative Data	T-test for Difference (Standard Error)
All	0.114	0.243	0.129	0.116	0.279	0.164
	[1,106]	[1,106]	(0.012)	[1,106]	[1,106]	(0.014)
Above Median Income	0.127	0.289	0.163	0.130	0.325	0.195
	[553]	[553]	(0.019)	[553]	[553]	(0.022)
Below Median Income	0.101	0.197	0.096	0.101	0.233	0.132
	[553]	[553]	(0.015)	[553]	[553]	(0.018)
Male Respondent	0.122	0.183	0.061	0.122	0.195	0.073
	[164]	[164]	(0.031)	[164]	[164]	(0.033)
Female Respondent	0.113	0.254	0.141	0.115	0.294	0.179
	[942]	[942]	(0.013)	[942]	[942]	(0.016)
Male Surveyor	0.119	0.232	0.112	0.122	0.266	0.144
	[730]	[730]	(0.015)	[730]	[730]	(0.018)
Female Surveyor	0.105	0.263	0.159	0.105	0.304	0.199
	[372]	[372]	(0.020)	[372]	[372]	(0.024)
Gender Matched: Respondent and Surveyor	0.113	0.238	0.126	0.113	0.276	0.163
	[453]	[453]	(0.018)	[453]	[453]	(0.021)
Gender Mismatched	0.116	0.245	0.132	0.119	0.280	0.162
	[649]	[649]	(0.016)	[649]	[649]	(0.019)
Male Respondent and Male Surveyor	0.131	0.164	0.033	0.131	0.180	0.049
	[122]	[122]	(0.035)	[122]	[122]	(0.038)
Male Respondent and Female Surveyor	0.098	0.244	0.146	0.098	0.244	0.146
	[41]	[41]	(0.066)	[41]	[41]	(0.066)
Female Respondent and Female Surveyor	0.106	0.266	0.160	0.106	0.311	0.205
	[331]	[331]	(0.022)	[331]	[331]	(0.025)
Female Respondent and Male Surveyor	0.117	0.245	0.128	0.120	0.283	0.163
	[608]	[608]	(0.016)	[608]	[608]	(0.020)

Huber-White standard errors in parentheses. Four observations are dropped for surveyor gender-related measures due to missing information.