

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

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## ABSTRACT

Microcredit seeks to promote business growth and improve well-being by expanding access to credit. We use a field experiment and follow-up survey to measure impacts of a credit expansion for microentrepreneurs in Manila. The effects 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. Overall, borrowing households substitute away from labor (in both family and outside businesses), and into education. 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 works broadly through risk management and investment at the household level, rather than directly through the targeted businesses.

Keywords: microfinance, microcredit, microentrepreneurship, risk sharing, formal and informal finance

JEL codes: O1, D1, D2, G2

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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)<sup>1</sup>

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 is an increasingly common weapon in the fight to reduce poverty and promote economic growth. Microlenders typically target women operating small-scale businesses and traditionally uses group lending mechanisms. But as microlending has expanded and evolved into what might be called 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.<sup>2</sup>

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 sector access could crowd-out relatively efficient informal credit and insurance mechanisms (see the second quote above). The often high cost of microcredit (60% APR in our setting) means that high returns to capital are required for microcredit to produce improvements in tangible outcomes like household or business income.<sup>3</sup> 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 risks.<sup>4</sup>

We generate clean variation in access to microcredit by working with a lender to randomly approve some microenterprise loans within a pool of marginally creditworthy, first-time applicants. We then use an extensive follow-up survey to measure a wide range of impacts on households and their businesses.

The setting for our study is very much second generation microcredit: individual liability loans,

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<sup>1</sup> [http://www.microcreditsummit.org/index.php?/en/about/microfinance\\_advocacy/](http://www.microcreditsummit.org/index.php?/en/about/microfinance_advocacy/).

<sup>2</sup> See Karlan and Morduch (2009).

<sup>3</sup> There is also some evidence that psychological biases can lead to “overborrowing” that does more harm than good; see Zinman (2009) for a brief review.

<sup>4</sup> Prior studies have used various methodologies to address endogeneity problems; see, e.g., Coleman (1999), Kaboski and Townsend (2005), McKernan (2002), Morduch (1998), Pitt et al (2003), and Pitt and Khandker (1998). One newer study of note examines the intensive impact margin of a government program, by using a program in Thailand that delivered a fixed amount of money to a village regardless of the number of individuals in the village (Kaboski and Townsend 2009).

delivered by First Macro Bank (“FMB,” or the “Lender”), a for-profit lender that operates in the outskirts of Manila and receives implicit subsidies to expand access to microentrepreneurs from a USAID-funded program (e.g., in the form of technical assistance).<sup>5</sup> Our study is the first randomized evaluation with such a firm, and complements a contemporaneous randomized evaluation of group lending in urban Indian slums by the non-profit microfinance institution Spandana (Banerjee et al. 2009), and our earlier study of expanding access to consumer loans in South Africa (Karlan and Zinman forthcoming).

The expansion we study 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.

The impacts of FMB’s credit expansion on more ultimate outcomes are varied, diffuse, and surprising in many respects. 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 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. The likelihood of other household members working (either in family or outside businesses) falls, as does the likelihood of someone working overseas. 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). The likelihood of a household member attending school increases. We find no evidence of improvements in measures of self-reported 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 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

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<sup>5</sup> The program is administered by Chemonics, Microenterprise Access to Banking Services (MABS).

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 seems to work broadly through risk management and investment at the household level, rather than directly through the targeted businesses.

A final set of key findings suggests that treatment effects are stronger for groups that are not typically targeted by microcredit initiatives: male, and relatively high-income, borrowers. The gender split is interesting because although microlenders typically target female entrepreneurs, recent evidence finds higher returns to capital for men (de Mel, McKenzie, and Woodruff 2008; de Mel, McKenzie, and Woodruff forthcoming). The income split is interesting because many consider poverty targeting an important criteria for microfinance (e.g., USAID has a Congressional requirement to allocate a proportion of funding to programs that reach the poor). Although we do not address the question of whether microcredit can help the poorest of the poor — our sample frame are microentrepreneurs, but wealthier than average for the Philippines — the fact that we find little evidence of effects on those with lower-income within our sample frame does not bode well for arguments that impact is biggest on those who are poorer. The overall picture of our results also 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 (Karlan and Zinman forthcoming finds direct evidence that salaried workers benefit from microloans).

## **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,<sup>6</sup> 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

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<sup>6</sup> 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.

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 are better educated and wealthier than average. 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 and monitoring, but substantial relative to borrower income. For example, the median loan size made under this experiment, 10,000 pesos (US\$400) 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%.<sup>7</sup>

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. 19.0% of the loans in our sample paid late at some point, and 4.6% 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

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<sup>7</sup> The Lender also requires first-time borrowers to open a savings account and maintain a minimum balance of 200 pesos.

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.<sup>8</sup> The branches are located in the provinces of Rizal, Cavite, and the National Capital Region. The Lender maintained normal marketing procedures by having loan officers canvass public markets and hold group information sessions for prospective clients.

Our sample frame is comprised of 1,601 marginally creditworthy applicants, nearly all (1,583) of whom were first-time applicants to the Lender. Table 1 provides some summary statistics, from application data, on our sample frame. The table shows that our sample is largely female (85%), has a typical household size, and is relatively well-educated (93% graduated high school or more) and wealthy compared to local and national averages (average household income of \$770 per month). The most common business is a sari-sari (small grocery/convenience) store. Other common businesses are food 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 we use for measuring impact of access to microcredit; these means 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, N =256) and high (scores 46-59, with 85% probability of approval, N = 1,345). 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.

Table 2 corroborates that the random treatment assignments generated observably similar treatment and control groups. In total, 1,272 applicants were assigned to the treatment (loan approval) group,

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<sup>8</sup> One branch was removed from the study when it was discovered that loan officers had eliminated the randomization component of the credit scoring software.

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 best, by controlling the flow of their more marginal clients in terms of creditworthiness.

## ii. 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 (summarized in Figure 1).

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 (nevertheless in all cases we use the software's *initial* assignment, from Step 2, to estimate treatment effects). 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 a survey of all 1,601 applicants in the treatment and control groups.<sup>9</sup> 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, 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:

$$(1) \quad Y_i^k = \alpha + \beta^k \text{assignment}_i + \delta \text{risk}_i + \phi \text{APP\_WHEN}_i + \gamma \text{SURVEY\_WHEN}_i + \varepsilon_i$$

$k$  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).  $\text{Assignment}_i = 1$  if the individual was initially *assigned* to treatment (regardless of whether

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<sup>9</sup> Midway through the survey effort, Innovations for Poverty Action staff replaced the survey firm’s management team but retained local surveyors.

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.<sup>10</sup> We estimate (1) using ordinary least squares (OLS) unless otherwise noted.

## 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 each row an outcome or summary index of related outcomes, and each column either the full sample or a subsample. 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).

### 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

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<sup>10</sup> This could occur if control applicants were harder to locate (e.g., because we could not provide updated contact information to the survey firm), and had poor outcomes compared to the treatment group (e.g., because they did not obtain credit).

affected by the treatment: loans from the Lender, or from close substitutes.<sup>11</sup> 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 significant effect on any reported borrowing in the month before the survey,<sup>12</sup> 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 get 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.<sup>13</sup> 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.

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<sup>11</sup> 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.

<sup>12</sup> The survey also collects some, albeit less detailed, information on borrowing over the last 12 months. We present these results in Appendix Table 1.

<sup>13</sup> Appendix Table 1 shows a statistically significant decrease in the likelihood of any informal sector loan over the last 12 months.

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.<sup>14</sup> Prior evidence suggests that this level underreporting of unsecured debt is common in household surveys (Copestake et al. 2005; Karlan and Zinman 2008; Jonathan Zinman 2009). Debt underreporting will bias the treatment effects on borrowing outcomes downward if underreporting is more severe in levels in the treatment than in the control group.<sup>15</sup>

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, but not statistically significant: a roughly \$50 US increase, compared to a sample mean of about \$500.<sup>16</sup> Dropping the top and bottom percentile

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<sup>14</sup> Conversely, only 3% of households reported having a loan outstanding from the Lender that did not appear in the Lender's administrative data.

<sup>15</sup> This will happen even if both groups underreport in the same proportion, so long as the treatment group obtains more loans 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.

<sup>16</sup> 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

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.<sup>17</sup>

The fact that microfinance often targets women, and the results in de Mel, McKenzie, and Woodruff (2008), 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 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.<sup>18</sup> Taken together the results in Table 4 and 5 suggest that returns to capital are higher for higher-income borrowers.

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,<sup>19</sup> 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

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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.

<sup>17</sup> We do not find any significant correlations between treatment status and (non)response to the profit question.

<sup>18</sup> 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.

<sup>19</sup> 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.

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 relating to risk management suggest an explanation that we discuss below (in sub-section G., and in the Conclusion).

#### *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 a large but insignificant decrease in 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, more children 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 children, 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 either.

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 by replacing worn-out roofs/walls/floors with lower-quality materials. 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.

#### *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: savings, 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.

#### *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 for male and higher-income respondents.<sup>20</sup>

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 turn for formal and informal credit in a pinch, and consequently rely less on formal sector insurance

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<sup>20</sup> 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).

(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) can be interpreted in a similar vein, as reduced reliance on diversification.

#### *H. Household Income and Consumption, Table 10*

Table 10 examines whether any business profit increase (Table 5) translates into income and consumption changes. 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 and perhaps related expenditures), thus leading to no change in total household income or consumption.

#### *I. Well-Being, Table 11*

Table 11 presents treatment effects on nine different measures of the self-reported well-being, based on responses to standard batteries of questions on optimism, calmness, (lack of) worry, life satisfaction, work satisfaction, 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.<sup>21</sup> Overall, nearly all of the point estimates are negative, however, and aggregating the nine outcomes into a summary index (Karlan and Zinman forthcoming; Kling, Liebman, and Katz 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

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<sup>21</sup> Fernald, Hamad, Karlan, Ozer, and Zinman (2008) also find that increased access to produces higher stress, in South Africa.

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.

The first key result is that 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 from somewhere else. This expanded use of credit then drives our results on more ultimate outcomes.

The first surprising result is that marginally creditworthy microentrepreneurs who randomly receive credit *shrink* their businesses relative to the control group. The treatment group also reports increased *access* to informal credit to absorb shocks (contrary to theories where formal credit may unintentionally crowd-out risk sharing arrangements by making it difficult to for those with better formal access to commit to reciprocation, e.g. see Conning and Udry (2005)). We also find that access to credit substitutes for formal insurance.

We find two other noteworthy results. First, following de Mel et al (2008, forthcoming), 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). Second, we find no evidence that increased access to credit improves well-being, as many microcredit advocates claim; rather, we find some evidence of a small *decline* in self-reported well-being.

The results here have several implications. They provide tests of broad classes of theories, as noted above. They call into question the wisdom of microcredit policies that target women and microentrepreneurs to the exclusion of men and wage-earners. They highlight the importance of replicating tests of theories and programs across different settings. And 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.

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