Experimental Evidence on Returns to Capital and Access to Finance in Mexico

David McKenzie, and Christopher Woodruff[#]

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Abstract

A strong theoretical argument for focusing on access to finance is that financial market imperfections can result in large inefficiencies, with firms with productive investment opportunities underinvesting. Lack of access to finance is a frequent complaint of microenterprises, which account for a large share of employment in developing countries. However, assessing the extent to which a lack of capital affects their business profits is complicated by the fact that business investment is likely to be correlated with a host of unmeasured characteristics of the owner and firm, such as entrepreneurial ability and demand shocks. We report on the results of a randomized experiment which gave cash and in-kind grants to small retail firms, providing an exogenous shock to capital. We find that this shock generated large increases in profits, with the effects concentrated on firms which were more financially constrained. The estimated return to capital is found to be at least 20 to 33 percent per month, three to five times higher than market interest rates.

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[#] McKenzie, Finance and Private Sector Research Unit, Development Research Group, World Bank, IZA and BREAD; Woodruff, Graduate School of International Relations and Pacific Studies, UC San Diego. We thank the editor and three anonymous referees for their helpful comments on the paper, and thank Bob Cull for his insights on the Mexican financial sector and for his collaboration in this project. Matthias Schündeln and participants at the World Bank Access to Finance conference provided useful comments. The authors appreciate financial support for this project from the World Bank. Data OPM carried out the survey work on which the paper is based.

I. Introduction

Lively microenterprise sectors are a dominant feature in urban areas of low- and middle-income countries. As much as a third of the labor force in these economies is self-employed. Those involved in retail trade—street vendors and owners of small shops and restaurants—are a plurality of small scale enterprises. These vendors earn their living using their own labor and small amounts of capital. They generally lack access to loans from formal financial institutions, relying on their own savings and perhaps informal loans from family members or friends. Surveys indicate that the lack of access to finance is one of their most often mentioned complaints.

How much does the lack of capital affect the earnings from their enterprise? At least one previous study in Mexico suggests that the answer is a considerable amount. McKenzie and Woodruff (2006) use data from the Mexican National Survey of Microenterprises (ENAMIN) to estimate returns to capital. They find return in the range of 10-15 percent per month for the smallest firms, those with capital stocks of less than US$500. That is, with each additional $100 invested in the enterprise, earnings rise by $10-15 per month. The ENAMIN is a repeated cross section which was carried out every two years between 1992 and 1998. While provocative, the returns estimated by McKenzie and Woodruff are subject to several concerns. A primary concern is whether capital investment is correlated with unmeasured ability, resulting in conflation of returns to capital with returns to ability. McKenzie and Woodruff address this issue with several measures of ability, but each is imperfect. Note that standard panel data would not resolve all of the issues. Changes in capital stock between waves of a panel would be endogenous to unmeasured shocks to demand for a firm's output, among other factors.

To address these concerns, we designed a randomized experiment to generate data which allow a consistent measure of returns to capital in microenterprises. We collected data from a panel of microenterprises in the city of Leon, Guanajuato in Mexico over a period of five quarters. After the first through fourth rounds of the survey, we administered treatments in the form of either cash or equipment to randomly selected enterprises in the

sample. The treatments generate shocks to capital stock which are random, uncorrelated with either the ability of the enterprise owner or the prospects for the business.

We estimate that a 1500 peso ($140) treatment increased monthly profits by at least 292-487 pesos, representing a marginal return to capital of 20-33 percent. The estimates are found to be robust to controls for possible treatment spillovers and for attrition. These returns are in the upper end of the range estimated by McKenzie and Woodruff (2006) using non-experimental methods. We then interact the treatment effect with different measures of financial constraints and access to finance. We find the return is much higher (70-79 percent) for firms which report themselves as financially constrained, and much lower for firms that report that lack of finance is not a constraint. Indeed, we can not reject the possibility of no return for the latter group of firms. Finance is found to be less of a constraint for firms which are formal, have more educated owners, and have owners whose fathers have previously owned a business.

An unbiased estimate of returns to capital has important policy implications in several areas. First, the returns from investment determine the interest rates which borrowers are willing to pay to microlending organizations. Higher returns imply a higher likelihood of developing financially sustainable microlenders. There is considerable debate about the sensitivity of microfinance demand to higher interest rates (Morduch, 2000; Karlan and Zinman, 2007). Mexico has a very underdeveloped microlending sector relative to other low- and middle-income countries. High returns may suggest the scope for more lending. Second, if returns are low below some investment threshold, then these low returns may act as an entry barrier, preventing high ability entrepreneurs without access to capital from entering. If, on the other hand, returns to capital are high at very low levels of investment, then capital-constrained entrepreneurs should be able to enter and grow to a desired size by reinvesting profits earned in the enterprise. In that case, capital constraints will have short term costs, but fewer long term effects on outcomes. High returns at low very low capital stock levels suggest that credit constraints will not lead to poverty traps.

The recent literature has generally found high rates of return to capital in small scale enterprises. With one exception, the existing literature uses non-experimental approaches to estimate the return to capital. Banerjee and Duflo (2004) use changes in laws forcing Indian banks to make preferential loans to certain groups of firms, considerably larger than the firms we study here, to identify changes in access to finance among the firms. Banerjee and Duflo conclude that returns to capital are between 74% and 100% per year. Udry and Anagol (2006) estimate returns to capital among small scale agricultural producers in Ghana to be 50% per year among those producing traditional crops on a median-sized plot and 250% per year among those producing non-traditional crops on median sized plots. Closer to the type of enterprise we examine in this paper, Udry and Anagol calculate effective discount rates from the market for used taxi parts. Using data on prices and useful lives of used taxi parts, they estimate the discount rate among taxi drivers to be 60% per year, suggesting the shadow value of capital among taxi drivers is at least this high. Finally, Kremer, Lee and Robinson (2007) creatively use data on stock-outs and discounts for bulk purchases to estimate a return of at least 113% per year for rural retail shops in Kenya.

Randomized experiments have become increasingly popular in development economics as a way to overcome many of the identification concerns which can arise with non-experimental approaches. Many of the earliest applications were in education and health (e.g. Kremer, 2003), but several recent papers have used randomized experiments to examine some of aspects of access to finance, including the impact of consumer credit (Karlan and Zinman, 2008) and the role of joint versus individual liability in microfinance groups (Gine and Karlan, 2006). The only other study we are aware of which takes an experimental approach to relaxing capital constraints on firms is parallel work we have conducted in Sri Lanka (De Mel, McKenzie and Woodruff, 2007). Using a similar experimental design we find returns for similarly sized enterprises in both the retail and manufacturing sectors in the range of 5-7 percent per month, several times higher than market interest rates, with lower returns for females than males. The present paper focuses on male-owned firms in the retail trade industry, and shows that the high returns found in Sri Lanka for this group generalize to a very different country context.

Moreover, in this paper we consider the difference in returns according to prior access to finance, which we can do only indirectly in the Sri Lankan paper.

In the next section, we describe the manner in which the sample was selected, the relevant portion of the survey instrument, and the data. We then describe the experimental intervention, present and discuss the results, and conclude.

2. The Sample and the Data

We use data from a panel survey of microenterprises which we carried out in the city of Leon, in the state of Guanajuato, Mexico. Leon is the fifth largest city in Mexico, with a metropolitan area population of approximately 1.4 million. The city is the center of Mexico's shoe and leather industries, and is also home to a vibrant microenterprise sector covering the usual range of activities found throughout the developing world.

The initial survey was conducted in November 2005, reflecting data from October 2005. Subsequent surveys were administered quarterly, with the fifth and final survey conducted in November 2006. There were 207 firms in the first wave of the panel.[1] We resurveyed 182 firms in the first follow-up (February 2006), and 137 firms in the fifth wave. There are 114 firms who report profits in all five waves of the survey and 161 firms that report profits in at least three waves. Thus, attrition is of some concern in analyzing the results of the survey. We return to this issue later. In this section we describe how the sample was drawn, use data from the Mexican population census to benchmark the sample to the population, and describe the survey instruments and data.

The Sample

We set out to select a sample of enterprises with less than 100,000 pesos (approximately US$1000) in capital stock, excluding land and buildings. We placed the upper limit of capital stock because the results in McKenzie and Woodruff suggested returns were highest at very low levels of capital. While this truncates firms which have managed to

[1] We surveyed 220 firms in the first round, with the intention of forming a panel of 200 firms. We eliminated before the second round of the survey began 13 firms which did not meet the original sample criteria.

grow above a certain size, available data suggest that the majority of firms starting small in the retail sector never grow large. Moreover, the sample will also include young firms which might potentially grow above this threshold in the future. The project budget was sufficient to follow a sample of about 200 firms for five quarters. Cross sectional data on similar enterprises from the National Microenterprise Survey (ENAMIN) indicated variances in reported data on key statistics like profits and income were very high. To reduce the expected variance and increase the expected power of the estimates based on the data, we limited the sample to enterprises engaged in retail trade and owned by males aged 22-55. In order to cover only full-time work, we required the owners to be working 35 hours or more a week in the baseline period.

The retail trade sector covers a wide range of typical small-scale businesses found in many developing countries. Examples of the types of microenterprises found in our sample include men selling groceries, flowers, DVDs, shoes, small toys, batteries, balloons, tacos, newspapers, bread, fruit, watches, seafood, beer, wallets, leather bags, handicrafts, perfume and cosmetics, corn holders, chiles, juices, books, and clothing.

The sample frame was based on the 10% public use sample of the 2000 population census for the city of Leon. The census data indicate that 79 percent of the full-time self-employed are male and 80 percent of the male full-time self-employed are aged 22 to 55. Wholesale and retail trade is the most common industry among 22-55 year old males in Leon, accounting for 29.4 percent of full-time self-employment . The next most common industries are manufacturing (26 percent), personal services (12 percent), and construction (9 percent).

We examined data at the level of the smallest geographical unit available in the public sample, the UPM (unidad primaria de muestreo).[2] For each UPM, we calculated for males 22-55 years of age the average education level and the percentage self-employed in the retail sector. We also calculated the percentage of households in the UPM with a male household head present. Using these data, we selected 20 UPMs with high rates of retail

[2] An average UPM in Leon contains about 17,000 people and 3,400 males aged 22-55.

self employment and modest average levels of education. There were two reasons for restricting our attention to UPMs satisfying these criterion. The first was one of cost and logistics. The survey firm randomly selected blocks within these UPMs and administered a short screening survey in both households and small enterprises. By restricting the survey to UPMs with high incidences of self-employment, we could ensure that the screening survey would capture enough firms with characteristics fitting our criteria, and that the firms would not be so scattered throughout the city that it would be difficult to administer the experiment and follow-up surveys. The screening survey identified enterprises owned by males 22-55 years of age in the retail sector, operating without paid employees. Enterprises with paid employees are very likely to exceed our upper limit of 100,000 pesos of capital stock, so the lack of paid employees was used as an initial screen for capital stock. Where the screening survey was administered to the owners, we also asked for the value of the capital stock excluding land and buildings, measured at replacement cost. The second reason for focusing on UPMs with a high incidence of self-employment and modest average levels of education is that these are potentially the areas where microfinance or other access to finance interventions would be targeted – that is, areas where many low-income households depend on self-employment.[3]

Table 1 matches the distribution of the enterprise income and the owner's education and age from our sample to distributions of the same variables from the 2000 population census for the city of Leon. All of the census data in the table refer to the sample of males 22-55 years of age, or subsamples drawn from that sample. The top line of the table shows the distribution of monthly reported income in our sample of microenterprises, using the baseline data gathered in November 2005.[4] Between February 2000 and October 2005 (the reference month in the baseline survey), consumer prices increased by approximately 26% in Mexico. The second line of the table deflates the survey data to reflect 2000 price levels. The next two lines show the distribution of income reported in the census in for all workers and own-account workers, respectively. Just over 20 percent

[3] In contrast, if UPMs were chosen randomly, it is possible that by pure chance all the microenterprises in the survey could come from relatively wealthy areas where few other microenterprises operate, which is something we wished to avoid.

[4] The table shows the data for all firms in the baseline sample. The distributions of income, education and age are very similar for the sample of 114 firms reporting profit in all five rounds of the survey.

of all 22-55 year old male workers in Leon are own-account workers (self-employed without any employees).

Our survey data are clearly not representative of the full sample of own-account workers. The mean level of income in our sample is 2,606 pesos per month, compared with 5,671 pesos among own-account workers in the census. The difference in medians is somewhat more comparable, 2,277 vs. 4,281 pesos per month. The fifth line of the top panel shows the comparison is not altered when the census sample is limited to own-account workers in the retail sector. There are almost 12,000 prime-age males who are own-account workers in the retail trade sector in Leon, about 30 percent of the male own-account workers there.[5]

These findings are not surprising, given the upper limit we placed on our sample of 100,000 pesos in non real estate capital. We excluded the upper tail of the size of enterprises from our sample. In the sixth line, we eliminate the upper third of the income distribution from the census sample. When we do this, we find a distribution which is quite close the distribution from our sample. The mean income from the truncated census sample is 2,809 pesos per month, about 4% higher than the mean in our sample. The difference in medians is only a bit bigger, around 13%. The differences are biggest in the lower tail of the distribution.[6] Thus, our sample appears to represent the lower two-thirds of the income distribution of own-account workers in Leon.

The second panel of table 1 compares the years of schooling of the enterprise owners in our sample with the data from the 2000 population census in Leon. Our owners have lower schooling: 6.8 years on average, compared with 7.7 years among the census sample of self-employed. The differences are especially notable at the lowest schooling levels. In our sample, 22 percent of owners have three years or fewer of schooling, and 38

[5] The factor weights included in the census suggest there are almost 30,500 own-account workers in the city's retail sector, including both males and females of all ages. Our treated sample represents too small a number to be concerned about the general equilibrium effects of the treatments.

[6] About one-sixth of the own-account workers in the census report monthly incomes of exactly 4,286 pesos, or exactly 1000 pesos per week. The mean income of the census sample truncated just below this level (at the 5oth percentile) is 2,342 pesos. The mean of the census sample truncated just above this level is 2,797.

percent have 4-6 years. The comparable numbers among self-employed males of the same age in Leon are 17 percent and 30 percent, respectively. But once we remove the top third of the income earners from the census, the distribution of education matches that in our sample remarkably well: 23 percent of the self-employed in the truncated census sample have 3 years or less of schooling, and 36 percent have 4-6 years. We have a somewhat lower proportion with lower secondary schooling (13 percent vs. 24 percent) and a higher proportion with upper secondary schooling (20 percent vs. 12 percent).

Finally, the bottom panel on table 1 shows that the distribution of age is similar in our sample and any of the census samples. The age distribution is not much affected by the truncation of the census sample. Collectively, the data suggest that our sample represents well the bottom two-thirds of the sample of self-employed in Leon. Own-account workers are about 20 percent of the 22-55 year old male workforce in Leon. Hence, our sample represents a sizable portion of the male labor force, about 14 percent.

The Census suggests that our sample is representative of the income, education, and age distribution of the bottom two-thirds of the self-employed in Leon. We can also examine how the capital stock of our firms compare to those of other retail trade firms using the nationally representative ENAMIN. The mean (median) capital stock at the time of the baseline survey was 5600 (5100) pesos. The 1998 ENAMIN contains 609 own-account workers in the retail trade sector. Their enterprises have a median capital stock of 5,920 pesos, and a mean capital stock of just over 18,000 pesos. Adjusting our data for inflation between 1998 and 2005, our median enterprise has a capital stock which is about 66% of the average own-account enterprise. As with the population census and income, truncating the ENAMIN sample at the 67th percentile results in a distribution of enterprise capital stocks which is reasonably close to the distribution in our sample. The ENAMIN sample firms are smaller in the lower tail (700 pesos vs. 1742 pesos, deflated to 1998 pesos at the 25th percentile, for example) and larger in the upper tail (8,000 pesos vs. 6,511 pesos, deflated to 1998 pesos at the 75th percentile). But on average, enterprises in the truncated ENAMIN sample are only 12% larger than those in our sample.

The Data

The baseline survey was carried out in November 2005. The survey instrument was modeled after the ENAMIN survey. In the first round, we gathered detailed information on the capital invested in the enterprise, separated into tools, machinery and equipment, vehicles, real estate and buildings, and inventories and finished and unfinished goods. We also gathered operational data on the firm--revenues, expenses and profits—for the preceding month, and personal information about the owner. In each subsequent survey, we ask firms about changes in capital stock, either purchase of new assets or sales of existing assets, and operational data for another month of the survey.

The main outcome of interest is firm profits. Profits are measured by a single question of the following form "What was the total income the business earning during the month of March after paying all expenses including the wages of employees, but not including any income you paid yourself. That is, what were the profits of your business during March?" This wording is the same as in the ENAMIN survey. In De Mel, McKenzie and Woodruff (2008) we show on the basis of data collection experiments in Sri Lanka that directly asking for profits gives a more reliable measure than taking revenue minus expenses, with the mismatch in timing between when expenses are incurred and the revenues from these expenses are realized accounting for much of the difference between the two measures. Nominal profits were then deflated into real (October 2005) profits using the Consumer Price Index for León.[7]

The sample is limited to males aged 22-55 operating in the retail sector. The average enterprise has been operating for just over five years. Only 20 percent of the enterprises were started within a year of the baseline survey. Almost 20 percent are at least ten years old. Sales average 5,700 pesos per month, and profits 3,486 pesos per month. The median levels of sales and profits are similar, 5,000 and 3,000 pesos per month, respectively. We asked owners for profits before accounting for any compensation for their own time, so the profit levels should be viewed as including the opportunity cost of the time spent in

[7] http://www.banxico.org.mx/polmoneinflacion/estadisticas/indicesPrecios/indicesPreciosConsumidor.html [accessed March 17, 2008].

the enterprise by the owner. As a result of this, profits are never reported as being negative.

3. The Experimental Intervention

Our randomized experiment aimed to provide exogenous shocks to the capital stock of microenterprises. We accomplish this by giving grants to a randomly selected subset of the firms in our sample. We use grants instead of loans, since our aim is to determine the marginal return to capital for the average small microenterprise, not just the microenterprise which would take up a loan with particular terms if offered such a loan. The likelihood a microenterprise owners took a loan would be affected by factors such as credit constraints, risk aversion, ability, beliefs about future productivity, and past experiences with debt. By providing grants we can show whether greater access to capital has the potential to provide high returns to firms more generally. This can be of direct interest to governments contemplating grants for poverty alleviation, as well as showing whether firms not presently borrowing have the potential to earn sufficient returns to repay loans at relatively high interest rates.

Firms were told before the first round of the survey that the only compensation they would receive for participating in the survey was a chance of receiving either cash or capital through prizes to be given out after each survey round.[8] The prize was a grant of 1500 pesos (about US$140). After the first round of the survey, the authors used a single draw from a computer random number generator to randomly assign firms into treatment and control groups.[9] Among the firms assigned to treatment status, the random draw also determined the round they would be treated and whether or not they would receive their grant as cash or capital for their enterprise. The results of the initial random draw were

[8] It does not seem plausible that forward looking firms may have changed their behavior in expectation of compensation, since they were not told the odds of a prize, had no prior relationship with the survey firm and thus would be somewhat uncertain about whether the prizes would really be given out, and the gradual roll-out of the treatment meant that untreated firms had no reason to believe the probability of treatment was so large as to make decisions based on a high likelihood of receiving treatment.

[9] Since all firms were male, in a restricted age range, and operating in the same broad industry in the same city, we did not stratify when doing the randomization. Simulations on microenterprises in Bruhn and McKenzie (2008) suggest that with our sample size and with microenterprise data, the choice of randomization method is unlikely to matter much in terms of either balance or power.

kept private from both the survey company and from the firms in the sample. After each round, the survey company was given a list of firms of firms to hand the grants to. Each firm could receive a prize at most once, although this was not made explicit to the firms.

Half of the grants were provided in cash and half as equipment or inventories for the enterprise. This replicates the grant process used by de Mel et al. (2007) in Sri Lanka, and is intended to see whether conditional (in-kind) or unconditional (cash) grants have different impacts on the business. For the in-kind purchases, a member of the survey team accompanied each firm to purchase whatever inputs or equipment the enterprise owner chose, under the condition that it had to be for the enterprise. Cash was given without restrictions on its use. Owners were allowed to contribute funds of their own to purchase items costing more than 1,500 pesos, though in practice, none did. All but two of the firms provided with equipment grants purchased inventories or raw materials. Two purchased display cases for goods. A total of 87 firms received treatment after one of the rounds; the remaining firms did not receive any treatment. The 1500 pesos treatments represent just over a quarter of the mean or median baseline capital stock, and one half of median monthly profits for the enterprises. They are thus substantial shocks for the enterprises.

Table 2 compares baseline characteristics of firms assigned into treatment with those assigned to the control group, and shows that the randomization was successful in creating groups which are comparable in observable characteristics of the owner and his firm.[10] The average owner is 37 years old and has 6.7 years of education. The majority of firms are not registered and do not keep business records. Just under half of the firms have a traveling local, meaning that the business does not operate out of fixed premises.

Staggering of Treatment, Attrition, Non-compliance and Noise

The first two columns of Table 3 show the number of firms assigned to the treatment and control groups by survey round. Treatments were made between survey rounds, so, for

[10] We do not report p-values for testing differences between the two groups, since assignment to group was done randomly, so by definition any such differences are due to chance. See Bruhn and McKenzie (2008) for discussion of this point.

example, 13 firms were assigned to be treated between round 1 and round 2. The remaining columns of Table 3 show how many firms were actually treated, and the number of firms with non-missing profit data by treatment status. Non-compliers are firms which were assigned to receive a treatment, but which did not. The majority of these firms did not receive a treatment because they had dropped out of the survey by the time they were to receive treatment. However, some firms assigned to treatment remained in the survey, but were not given the treatment by the survey firm, generally because they could not locate the firm owner at the time of giving treatment. Five of these firms were then treated in a later round than which they were assigned to treatment.

Table 3 shows that we began with 198 of the 207 firms reporting profits in the baseline survey, but that the number of firms dropped in each subsequent round. Only 128 firms were in the survey and reported profits by the last round. Table 4 explores this attrition in more detail. The first two columns report the attrition rates as of each round by assignment to treatment. The attrition rates are very similar for firms assigned to control and to treatment, and are 15-20% at the start of the second round, climbing to 38-39% by the fifth round. Whilst some of the attrition was reported to be due to migration to the United States, the majority comes from refusals and from the inability of the survey firm to relocate some individuals in subsequent waves.[11] These rates of attrition are similar to those in the quarterly labor force panel surveys undertaken by the Mexican Government's statistical agency INEGI – Antman and McKenzie (2007) report an attrition rate of 35 percent over five quarters among 25 to 49 year old household heads with wage or self-employed earnings. The fact that the attrition rates are so similar for treatment and control groups means that under plausible assumptions (to be elaborated in the next section), attrition will not bias our estimates of the treatment effect. We will return to this issue after the main results.

Table 4 does show that attrition is much lower among the firms that actually received treatment, with only 18 percent of the treated firms having attrited by round 5. There are

[11] The business locations for many firms are at most semi-fixed, with low costs of moving to new locations to take advantage of market opportunities in a different part of the city.

several possible reasons for lower attrition among treated firms. The first is selection, since in order to receive a treatment, a firm assigned to treatment had to stay in the survey and be able to be located at the time of receiving treatment. Many of the firms assigned to treatment dropped out before they were scheduled to receive treatment. Secondly, treatment might increase the likelihood of firm owners agreeing to continue responding to the survey, through some sense of obligation or better appreciation of the survey after receiving a prize from it. Finally, the treatment may prevent some firms that would have gone out of business from doing so, keeping them in the sample longer.

We can address the selection effect by conditioning on individuals still in the sample. Selection accounts for the majority of the difference in attrition rates between those assigned and not assigned to treatment. A probit regression of the probability of attriting between time t-1 and time t, conditional on being in the survey at time t-1 gives a marginal effect of being treated of 0.08, with a p-value of 0.068. This suggests there is some effect of treatment on the likelihood of a firm staying in the sample. However, since we do not have complete detail on whether attritors attrit because of closing down their business versus because of refusal, we can not separate the latter two explanations.

In addition to attrition and non-compliance, the third factor we need to consider when estimating the effect of the treatment is the large volatility of the monthly profits data reported by the firms. Profits vary a lot both across firms, and for the same firm over time. In round 1, the mean of profits across firms is 3410 pesos, with a standard deviation of 2198, resulting in a coefficient of variation of 0.64. If we calculate the percentage change in monthly profits from one round to the next by firm, this ranges from -97.6% to +4110%. Fitting an AR(1) model with a constant to profits gives an auto-regressive coefficient of only 0.31 (p=0.000) on the quarterly lag, a surprisingly low degree of autocorrelation.

Some of this variation over time in the profits of a given firm is undoubtedly genuine, reflecting productivity shocks, seasonality, growth, and decline. However, a substantial amount of the variation is likely to be noise. Only 6.8% of the firms keep business

accounts by formal methods, 34% use personal notes, and 58% keep no business accounts at all. As a result, estimation of firm profits is based on recall and personal notes for almost all firms, and likely to be subject to some reporting error. We therefore carry out some of our estimation by eliminating from our sample firms which have a percentage change in profits in the top or bottom tails of the percentage chance in profits distribution. Trimming the top and bottom 5% means dropping firms with a change in monthly profits less than -75% or greater than 280% from one quarter to the next.[12]

The last two columns of Table 4 display the cumulative attrition rates by round once we take into account both firms attriting from the sample, and firms which are dropped through the above trimming exercises. Trimming the top and bottom 5% increases overall attrition by the fifth round to 55-58%. Again there is only a minor difference in the attrition level by assignment to treatment status. This trimming greatly increases the autocorrelation of profits. Estimating an AR(1) model with a constant for the subset of firms left after trimming the top and bottom 5% of percentage changes gives an autocorrelation coefficient of 0.62 (p=0.000) compared to 0.31 in the no trimming case.

Figure 1 displays the profits data for firms which had and had not been treated at each wave, plotting real profits in each of rounds 2 through 5 against baseline profits. The top panel of the figure shows the results with no trimming. The much larger vertical scale than horizontal scale shows that a few firms report extremely large changes in profits, and make it hard to see what is happening for the majority of the firms. Moreover, with the vertical scale going to 50,000, one can not visually see the 495 peso mean difference in treated and untreated firms, so the fitted line is not shown. The bottom panel shows the same figure after trimming the top and bottom 5% of changes in profits. This trimming is seen to remove the largest outliers, and the gap in fitted mean profits between treated and untreated firms can be clearly seen.[13] This difference in mean profits between those receiving treatment and those not receiving treatment is a naïve estimate of the

[12] Results are similar, but not as precise, when we trim the top and bottom 1%. Results available upon request.

[13] The fitted line is Real Profits (t) = a + b*Treated by Time t + c*Baseline Profits + e, where the error term is clustered at the firm level.

treatment effect. It will only be equal to the average treatment effect on the treated (ATT) if there is no selection into treatment amongst those assigned to treatment. In the next section we show how to estimate the ATT even when there is selection into the treatment amongst those randomly assigned to receive it.

4. Estimation of the Treatment Effect and Results

Estimation Strategy

We begin by estimating the mean effect of being assigned to treatment on business profits. This requires estimating for firm *i* in period *t*:

$$PROFITS_{i,t} = \alpha + \beta Z_{i,t} + \sum_{s=2}^{5} \delta_s + \varepsilon_{i,t} \qquad (1)$$

Where $Z_{i,t}$ is an indicator of whether firm *i* has been assigned to treatment at time *t*, and δ_s are period effects. We pool the cash and equipment treatments as a single treatment for greater power, but will later examine whether the impact varied according to the form the treatment took. The coefficient β captures the average effect of being assigned to treatment, also known as the intention-to-treat (ITT) effect. Randomization means that the expected correlation between the error term $\varepsilon_{i,t}$ and $Z_{i,t}$ is zero, implying that ordinary least squares can be used to estimate (1), clustering the error term at the firm level. However, since we observe multiple observations on the same firms, the error term $\varepsilon_{i,t}$ is likely to have a firm-specific component which we can control for through random-effects estimation. As a further check, we also carry out fixed-effects estimation. The advantage of fixed effects is that they will capture any time-invariant firm characteristics that affect profits. As we have seen in Table 2, randomization appears to have worked in providing comparable assignment to control and treatment, making this less necessary. A second possible use of fixed effects is to potentially increase the precision of the results, by lowering the residual variance. However, a potential disadvantage in our application is that since we believe the profit data to be subject to measurement error, using fixed effects may decrease the signal-to-noise ratio, thereby lowering precision.

The intention-to-treat effect shows the overall impact of the experiment. However, this interest of this parameter to policymakers may be limited, since the experiment we

undertook is not a program that has been implemented in practice. Given the success of the Progresa/Oportunidades program in using conditional cash transfers to reduce poverty, though, it is conceivable that governments may consider conditional cash transfers to poor firm owners as another form of poverty alleviation. Nevertheless, for most policy purposes we would like to know the impact of actually receiving the treatment. That is, we are interested in the parameter λ in the following equation:

$$PROFITS_{i,t} = \theta + \lambda TREAT_{i,t} + \sum_{s=2}^{5} \pi_s + \nu_{i,t} \quad (2)$$

Where $TREAT_{i,t}$ is an indicator of whether firm *i* actually received treatment by time *t.* We can estimate λ by using $Z_{i,t}$, whether or not a firm was assigned to treatment, as an instrument for receiving treatment. This estimate of λ is known as the local average treatment effect (LATE) and can be interpreted as the effect of our treatment on individuals who receive treatment after being assigned to treatment. Angrist (2004) demonstrates that in situations where no individuals who are assigned to the control group receive the treatment, as is the case here, the LATE is the same as the average treatment effect on the treated (TOT). As with estimation of (1), we estimate (2) allowing the error term to be clustered at the firm level, have a random effect, or have a fixed effect component.

Results

Table 5 presents the main results of the paper. Panel A provides estimates of the ITT effect, while Panel B provides the TOT effect. The first two columns provide OLS/2SLS estimates, the next two random effects/IV-random effects and the final two fixed effects/IV-fixed effects estimates. For each estimation method we first present the raw results, and then the results after trimming firms with percentage change in profits in the the top and bottom 5%.

Consider first the OLS and 2SLS results. The ITT estimates range from 543-603 pesos, and the TOT from 608-685 pesos. Comparing this with the treatment given to firms of 1500 pesos (approximately $140), we see that the resulting treatment effects are very large. With 5% trimming, the TOT effect is significant at the 10% level, and is equivalent

to a 46 percent return on the treatment. The random effects estimates show slightly smaller treatment effects, with the TOT ranging from 263 pesos to 527 pesos. After 5% trimming, the TOT is significant at the 5% level, and is equivalent to a 35% return on the treatment.

The raw results from fixed effects estimation are the only exception to the pattern of large treatment effects. The TOT effect is -19.4 pesos, with a standard error of 449. This appears to be a result of the noise in the data swamping any signal once the fixed effects are taken out. Indeed, as we trim the data and reduce the amount of noise, the fixed effects estimates also show positive treatment effects. The TOT effect with 5% trimming is 432 pesos, corresponding to a 28.8% return. The p-value for this is 0.142, close to a standard significance level even after having removed a large amount of the signal from the data.

The results from Table 5 therefore show treatment effects which are significant for 2SLS and IV-RE estimation, and which are marginally significant for IV-FE estimation, after 5% trimming. The estimated treatment effect ranges from 28.8% to 45.6%. This treatment effect is only well-identified for the subset of firms without really noisy profit data, who take up the treatment when assigned.

In order to interpret the treatment effect on the treated as a return to capital, we need to assume that the treatment only affected profits through changing capital stock. The firms in survey had no paid employees, and over the short time horizon, it seems unlikely that the treatment affected management ability or total factor productivity. Thus the only other channel through which the treatment may have affected profits is through adjustments in the own labor supply of the owner. Estimating equation (1) with own hours as the dependent variable gives an intent-to-treat effect of -3.7, with a p-value of 0.09. This falls to an effect of -2.1 and a p-value of 0.27 when we restrict hours to be above zero and below 100 per week. As such, ignoring changes in own hours is justified by the lack of significance after removing outliers, and leads our estimates to be a lower bound on returns to capital. Alternatively, an OLS regression on the baseline data

suggests the marginal value of one additional hour of own labor is 15 pesos. Valuing the fall at labor at this rate would raise the random effects treatment effect in column (4) of Table 5 from 527 pesos to 724 pesos, increasing the return to capital to 48 percent.

Pooling Cash and Equipment Treatments

Our analysis so far has pooled the cash and equipment treatments. The first two columns of Table 6 report the results of allowing the effect of the treatment to vary according to the form it is given in. Using random effects, we find a treatment effect on the treated of 600 pesos for the equipment treatment, and 436 for the cash treatment. We can not reject the null hypothesis that the two effects are equal in size, justifying pooling the treatments. Under fixed effects, we obtain a larger point estimate for the cash treatment than the equipment treatment, but again can not reject equality of the two treatment effects. Thus we do not have sufficient power to rule out the null hypothesis of no difference in effects between conditional and unconditional grants given to microenterprise owners.

Spillovers

The treatment effect is estimated by comparing firms randomly assigned to treatment with firms randomly assigned to the control sample. This will provide a valid estimate of the treatment effect provided that there are no spillover effects from the treatment to the control sample. The remainder of Table 6 investigates the validity of this assumption, by adding the number of other firms within the same census sampling cluster (UPM) as a firm that have either been assigned to treatment, or which actually received treatment. Over waves 2 through 5, the median firm had 1 firm receiving treatment in the same UPM, the mean had 2.1, and the 90^{th} percentile had 7 firms receiving treatment in the same UPM. The spillover effect is estimated to be small, positive and not statistically significant, and the coefficients on the treatment effects are very similar to those seen in Table 5. Therefore there does not seem to be any evidence of spillover effects, allowing us to interpret the treatment effects as pure treatment effects.

Attrition

One potential concern is whether the process of trimming combined with attrition could be biasing the results. Table 4 showed that attrition rates are similar for firms assigned to the control and treatment groups. However, after 5% trimming, attrition after 5 rounds is 58 percent for the control group compared to 55 percent for the group assigned to treatment. To examine the robustness of our results to this differential attrition, we use the bounding approach of Lee (2005) to construct upper and lower bounds for the treatment effect.[14]

The key identifying assumption required for implementing the Lee (2005) bounds is a monotonicity assumption which assumes that treatment assignment affects sample selection only in one direction. In our context, it requires assuming that there are some firms who would have attrited if they had not been assigned to treatment, but that firms do not attrit because of being assigned to treatment. This seems plausible in our context, since firms receiving treatment may stay in the sample when they would otherwise have attrited as a result of a business failure being prevented, or to an increased willingness to answer our survey question. It does not appear likely that receiving treatment would have caused some firms to drop out of the survey who would not have dropped out if they had remained in the control group.

Then to construct the Lee (2005) bounds, one trims the distribution of profits for the group assigned to treatment by the difference in attrition rates between the two groups as a proportion of the retention rate of the group assigned to treatment. A lower bound on the treatment effect is constructed by trimming the upper tail of the distribution, and an upper bound by trimming the lower tail of the distribution. In our application, this requires trimming the upper or lower 6.7 percent of the profits distribution for the group assigned to treatment.

[14] An alternative approach is to parametrically model attrition, and use the predicted model to reweight our data. However, we find attrition is not related to the age of the firm, education of the owner, whether or not the firm is registered, baseline profits, or the type of location of firm, or household size. A parametric correction for attrition by using weighted least squares to account for differences in probability of staying in the sample of different firms, increases the 2SLS estimate from 685 to 806. With a standard error of 425, we can not reject that there is no change in the coefficient from attrition, but if anything this parametric correction for attrition increases the estimated return to capital.

The last row of Table 5 then provides the Lee (2005) upper and lower bounds for the TOT effect after 5% trimming, adjusting for differential attrition between the groups assigned to control and treatment. As a result of the skewed distribution of profits, our point estimates in Table 5 are much closer to the upper bounds than the lower bounds. The lower bound for the treatment effect is 19.1% for the IV-FE estimate, 24.5% for the IV-RE estimate, and 31.6% for the 2SLS estimate. Thus in all cases, even the lower bound shows a large effect of our treatment.

5. Returns and access to finance

The returns we find are much higher than interest rates offered by banks and microfinance firms in Mexico. The leading explanation for such high returns is that many of our firms are credit constrained, causing them to operate below their efficient size. If this is the case, we should expect firms which are more credit constrained to have higher returns from the treatment. We explore this by interacting the treatment effect with different measures of whether or not a firm is credit constrained, via the following estimation equation

$$PROFITS_{i,t} = \theta + \lambda TREAT_{i,t} + \phi TREAT_{i,t} * UNCONSTRAINED_i + \sum_{s=2}^{5} \pi_s + \sum_{s=2}^{5} \delta_s * UNCONSTRAINED_i + \nu_{i,t} \quad (3)$$

where we allow the period effects to also vary with the measure of whether or not firm *i* is financially constrained. The level effect for being unconstrained is also included in this regression when we use random effects, but drops out when fixed effects are used. We use assignment to treatment and the interaction of assignment to treatment with being unconstrained as instruments for receiving treatment and its interaction with being unconstrained.

Our data provide several possible measures of access to finance and financial constraints from the baseline survey. The first is a question which asks firms whether lack of finance is an obstacle to the growth of their business. 64% of firms say it is a constraint, and 36% say it is not. Owners with more education and whose father owned a business are less

likely to say finance is a constraint. The other measures we use are based on whether or not firms had ever used formal finance or supplier credit at the time of the baseline survey. These have the advantage of being based on objective measures of use of finance, and firms which have had previous use of formal loans or supplier credit may be less financially constrained. 15.6% of firms had ever had a formal loan, and 31.7% had ever had supplier credit. Formally registered firms are more likely to have had a formal loan or supplier credit. However, the measures of use of finance are only very weakly positively correlated with saying finance is not a constraint: the correlation between saying finance is not a constraint and having previously had a formal loan is 0.036.

We again trim firms with percentage changes in profits below the 5th percentile or above the 95th percentile. Table 7 then provides the estimates of the treatment effects allowing for interactions between treatment and different measures of lack of financial constraints. Columns 1 and 2 show a large and strongly significant interaction effect between the treatment and whether a firm owner reports finance not to be a constraint to business growth. We can not reject that firms which report finance not to be a constraint have no increase in profits from the treatment (and the point estimate actually gives a decrease in profits). The treatment effect is then much stronger for the 64% of firms saying finance is a constraint: monthly profits increase by 1051-1192 pesos for these firms, a 70-79% return. We find similar, but less significant interaction effects with the measures of previous use of credit. We can not reject that there is no treatment effect for firms that have previously had formal loans or supplier credit, while the treatment effect for financially constrained firms is always positive, and is significant in all cases but one (firms that have not had a formal loan).

Finally, we combine the different measures to create a set of firms that say finance is a constraint to business growth, and which have never had a formal loan or supplier credit. We refer to the 38% of firms which fall into this category as "financially super-constrained". The final two columns of Table 7 interact this variable with the treatment. The treatment is estimated to result in an increase in profits of 1430-1515 pesos for these super-constrained firms – an incredible 100% return.

6. Discussion and Conclusion

Returns to capital are difficult to measure due to numerous problems well understood by the literature. Among the smallest firms, managed by their owners, the main issue is that the both the firm's profits and the level of capital stock may be correlated with the unmeasured ability of the owner and/or unmeasured factors affecting demand for the firm's products. We overcome these issues in a sample of small retail enterprises in Mexico by generating random shocks to capital stock in a field experiment.

Our data imply that profits increase anywhere from 300 to 1500 pesos (for super-constrained firms), which represents a 20-100% monthly return on the 1500 pesos that were given to treated firms. A skeptical reader might question whether such large estimates are plausible. First, we note that our analysis is limited to firms in the retail sector with replacement costs of capital less than US$900, whose owners work full time in the enterprise. Using a broader sample of Mexican enterprises, McKenzie and Woodruff (2006) find average monthly returns on capital of 10-20% for firms with capital stock below $200. For firms with capital stock of $500-$1000, they find returns on capital in the 5% range. Second, within our sample Table 8 indicates that the treatment effect is negligible for firms that report no financial constraints. Our results therefore reflect the experience of a relatively select group: firms with capital stock near $200 that report being financially constrained.

Borrowers from microfinance institutions might match these characteristics reasonably well. Being a client of an MFI is evidence that an individual felt financially constrained, and many are likely to operate microenterprises in sectors that require only a small amount of capital. *Compartamos*, the largest pure micro-lender in Mexico charges an average annual interest rate of 105% on loans made primarily to solidarity groups composed of 3-8 women.[15] The group is liable for any default, and thus members have a strong incentive to monitor and assist one another to ensure repayment. Without group

[15] The interest rate figure comes from Carlos Labarthe, co-CEO of *Compartamos*, as reported in Connie Bruck, "Millions for Millions," *The New Yorker*, October 30, 2006. When all costs are considered, the actual interest rate on some *Compartamos* loans is near 120%.

liability, we may expect lending rates to be higher.[16] Using the lower bound for our treatment effect (roughly 25% in Table 8), one of our treated firms could repay a 1500 peso loan in 4-5 months. Thus the interest rates charged by Mexican micro-lenders, though high by the standards of micro-lenders in other countries, are affordable to the segment of the population represented by our sample firms. If the annual rate on an individual loan were double that of a loan to a solidarity group, our estimates indicate that our sample firms would still, on average, be able to repay the loan from profits earned. That the rates charged by the largest micro-lender are roughly similar to our estimates provides support for the plausibility of our regressions.

Additional support for the notion that returns are especially high for the type of firms in our sample comes from comparisons with micro-lenders that focus on larger firms. The MIX Market, a web-based platform for the global exchange of information in the microfinance industry, provides data for four leading Mexican microlenders. The data suggest that return on equity and average loan size are negatively correlated for these four micro-lenders. *Caja Popular Mexicana* had an average loan size of $1,429 and a return on equity of 10.5% in 2005. *Compartamos* and *Finca México*, which focus on small borrowers (average loan sizes of $399 and $242, respectively), had much higher returns on equity (55.2% and 34.2%, respectively).[17] *FinComun* is intermediate by both measures, with loans averaging $635 and a return on equity of 25.2%. Thus, the available data indicate that returns are highest for microfinance institutions that make small loans. Moreover, those institutions have loan sizes substantially larger than the $140 received by treated firms in our experiment. Given the peculiarities of the Mexican context, the returns that we estimate are not implausible.

The high returns to capital at these very low levels of capital stock have several important implications. First, they suggest that there is no minimum investment threshold below

[16] Although the exact role of group liability compared to individual liability is still being debated in the literature, with Gine and Karlan (2006) suggesting that converting from group to individual liability does not affect repayment rates.

[17] *Finca México* is a so-called village lender, a group lending technology where each branch forms a single, large group and is given a degree of self-governance. This could explain why its return on equity is lower than that of *Compartamos* despite having a smaller average loan size.

which returns to capital are so low that entry into self employment is discouraged. This suggests that capital constraints operating through occupational choice is not a cause of permanent poverty traps. Capital constraints are still plausibly a source of some inefficiency. The finding that returns are highest among the most constrained enterprises supports this. There are a number of reasons why capital may not flow to these firms despite the high returns, including the cost of servicing small loans, lack of collateral and a weak contracting environment, and large information asymmetries (Morduch, 1999).

Secondly, the presence of high returns helps contribute to the debate on how to best increase access to financial services among the poor (Morduch, 1999; Armendáriz and Morduch 2007). One view is that it can only be profitable to do so with highly subsidized interest rates, either via a subsidized microfinance institution or through government banks. The contrasting view is that at least some of the poor have the capacity to repay loans at much higher rates, enabling microfinance institutions to be self-sustaining. Our results here show that the average financially-constrained microenterprise in the retail trade sector has very high returns, at a level sufficient for them to be able to repay high interest rates. But these high returns also present a puzzle. The enterprises in our sample are, on average, five years old. Given the high returns to capital in the enterprises, and the lack of any investment threshold, we might wonder why the owners have not grown on their own accord by reinvesting profits from the enterprise. This is a topic for future research.

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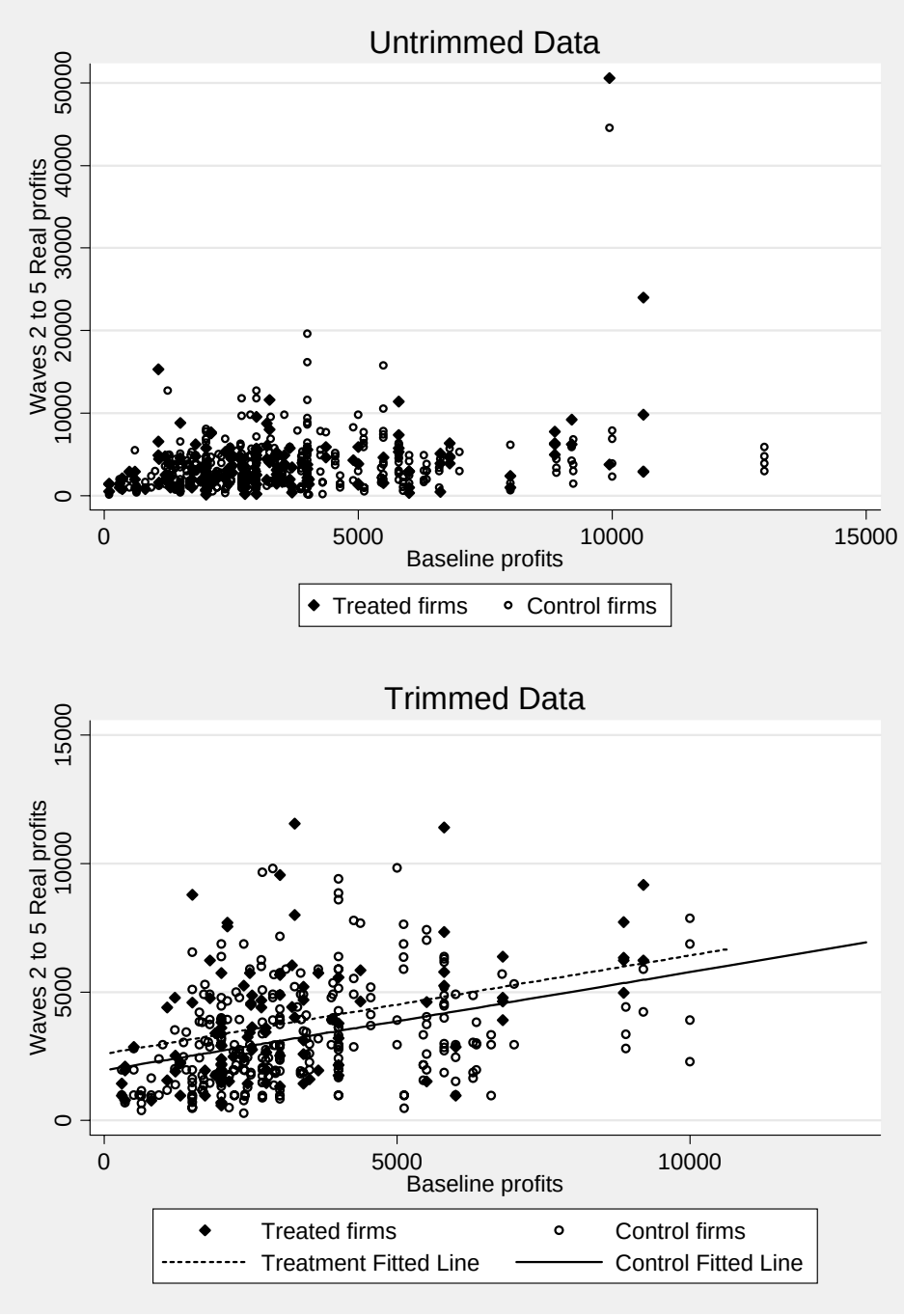
Figure 1: Real Profits in Follow-up Rounds Versus Baseline, before and after trimming large changes in profits.



Notes: Trimming removes firms with a change in profits above the 95th percentile of change in profits (>280%) or below the 5th percentile (<-75%). The fitted line in the bottom figure is an OLS regression of real profits on a dummy variable for treatment and on baseline profits. The coefficient on treatment is 643, with a standard error clustered at the individual level of 268 (p=0.018).

Table 1: Comparing our Sample to the General Population

		Panel A: Distribution of Income Data (pesos per month)					
		10th pctile	25th pctile	Median	75th pctile	90th pctile	Mean
Survey:	Sample (nominal)	965	1800	2800	4000	6150	3222
	Sample (2000 pesos)	763	1423	2213	3162	4862	2547
Census	Working 22-55	1714	2143	3000	4286	8000	4580
	Own Acct 22-55	1714	2357	4281	6429	12000	5671
	Own Acct retail	1714	2143	4286	6429	12857	6345
	Own Acct, trunc 67%	1286	2143	2571	4000	4286	2809

		Panel B: Distribution of education (years of schooling)						
		<3 years	4-6 years	7-9 years	10-12 years	13-16 years	>17 years	Mean
Survey:	Sample	22	38	13	20	6	1	6.8
Census:	Working 22-55	16	31	27	13	8	5	7.6
	Own Acct 22-55	17	30	24	14	8	7	7.7
	Own Acct retail	15	28	29	18	7	3	7.6
	Own Acct, trunc 67%	23	36	24	12	3	2	6.7

		Panel C: Age of Owner (years)					
		10th pctile	25th pctile	Median	75th pctile	90th pctile	Mean
Survey:	Sample	25	29	37	44	50	37
Census:	Working 22-64	24	27	34	42	49	35
	Own Acct 22-64	26	31	38	45	51	38
	Own Acct retail	26	31	38	45	51	38
	Own Acct, trunc 67%	26	30	38	45	51	38

Table 2: Baseline Characteristics of Treatment and Control Groups

	Assigned to Treatment	Assigned to Control
Age of owner	37.1	36.7
Age of business	5.2	5.8
Years of education	6.6	6.7
Mother's education	3.5	3.8
Father's education	4.1	4.1
Father owned a business	0.40	0.48
Profits in round 1	3433	3312
Sales in round 1	6063	6024
Non-land owned capital round 1	4342	4358
Travelling local	0.37	0.49
Unregistered	0.66	0.65
Doesn't keep business records	0.58	0.55
Owns house with title	0.46	0.48
Household Size	4.70	4.21
Number of working adults	1.17	1.30
Ever had supplier credit	0.32	0.31
Number of Firms	130	77

TABLE 3: Sample Size by Wave

	Ex-ante design		Actual Sample				
	Assigned	Assigned	Treated by	Observations in sample with non-missing profit			
Round	as control	as treatment	this round	Control	Treated	Non-compliers	Total
1	207	0	0	198	0	0	198
2	194	13	9	155	8	3	166
3	162	45	31	110	25	6	141
4	118	89	68	83	59	2	144
5	77	130	87	47	71	10	128

TABLE 4: Cumulative Attrition Rates by Wave

	Attrition rate				with 5% trimming	
	Assigned	Assigned	Received		Assigned	Assigned
Round	as control	as treatment	treatment	Non-compliers	as control	as treatment
2	0.20	0.15	0.11	0.25	0.49	0.46
3	0.32	0.31	0.19	0.57	0.52	0.56
4	0.30	0.31	0.13	0.90	0.49	0.53
5	0.39	0.38	0.18	0.77	0.58	0.55

Note: attrition measured as not in round, or in round with missing profits.

TABLE 5: TREATMENT EFFECTS
Dependent Variable: Real Monthly Profits (October 2005 pesos)

Panel A: Intention to Treat Effect

	OLS		Random Effects		Fixed Effects	
	Untrimmed	Trimmed	Untrimmed	Trimmed	Untrimmed	Trimmed
	(1)	(2)	(3)	(4)	(5)	(6)
Assignment to Treatment	543.1	602.9*	227.5	459.4**	-16.71	370.2
	(455)	(343)	(348)	(231)	(387)	(252)
p-value	0.234	0.081	0.514	0.047	0.966	0.142
Constant	3409***	3244***	3419***	3284***	3474***	3339***
	(176)	(183)	(282)	(193)	(225)	(146)

Panel B: Treatment Effect on the Treated

	2SLS		IV-Random Effects		IV-Fixed Effects	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	608.2	684.5*	263.8	526.5**	-19.37	431.8
	(503)	(383)	(398)	(266)	(449)	(293)
p-value	0.227	0.074	0.507	0.048	0.966	0.141
Constant	3409***	3244***	3419***	3284***	3474***	3338***
	(175)	(181)	(281)	(194)	(225)	(146)
Trimming	None	5%	None	5%	None	5%
Firm-Period Observations	715	504	715	504	715	504
Number of Firms	161	113	161	113	161	113
Implied Monthly Return	40.5%	45.6%	17.6%	35.1%	-1.3%	28.8%
Lee bounds for treatment effect		[31.6%, 42.9%]		[24.5%, 37.5%]		[19.1%, 31.3%]

Notes:
Standard errors are in parentheses and are clustered at the firm level for OLS and 2SLS. *** p<0.01, ** p<0.05, * p<0.1
Estimation is restricted to firms in the sample for three or more rounds. All regressions include period effects.
Assignment to Treatment is used as an Instrument for Receiving Treatment in Panel B.
See text for description of how Lee bounds are calculated.

Table 6: Robustness to Pooling Treatments and to Spillovers
Dependent Variable: Real Monthly Profits (October 2005 pesos)

	IV-RE	IV-FE	IV-RE	IV-RE	IV-FE	IV-FE
	(1)	(2)	(3)	(4)	(5)	(6)
Equipment Treatment	600.0**	264.2				
	(304)	(334)				
Cash Treatment	435.8	658.1				
	(373)	(413)				
Treatment			526*	529*	429	432
			(271)	(271)	(299)	(299)
# Neighbors Assigned to Treatment			5.3		3.8	
			(34.0)		(38.4)	
# Neighbors Which Received Treatment				15.3		11.4
				(43.6)		(50.5)
Constant	3283***	3341***	3298***	3298***	3354***	3353***
	(193)	(145)	(197)	(198)	(148)	(148)
Observations	504	504	495	495	495	495
Number of Firms	113	113	111	111	111	111

Note: Estimation trims the top and bottom 5% of changes in profits. Regressions also include period effects.
Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Assignment to Equipment and Cash Treatments are used as Instruments for Receiving Cash and Equipment in Columns 1 and 2. Assignment to Treatment is used as an Instrument for Receiving Treatment in Cols 3-6.

TABLE 7: ARE TREATMENT EFFECTS HIGHER FOR THOSE WHO ARE MORE CREDIT CONSTRAINED

	(1) IV-RE	(2) IV-FE	(3) IV-RE	(4) IV-FE	(5) IV-RE	(6) IV-FE	(7) IV-RE	(8) IV-FE
Treatment	1148***	1010***	633.3**	470.5	790.0**	611.7*	-56.87	-194.6
	(345)	(374)	(290)	(322)	(330)	(357)	(336)	(376)
Treatment*Says Finance not Constraint	-1662***	-1616***						
	(537)	(594)						
Treatment*Ever had a Formal Loan			-808.8	-457.4				
			(727)	(769)				
Treatment*Ever had Supplier Credit					-777.8	-566.9		
					(559)	(625)		
Treatment*Financially Super-Constrained							1528***	1585***
							(555)	(605)
Constant	3443***	3342***	3079***	3335***	3447***	3337***	3323***	3339***
	(238)	(145)	(213)	(145)	(235)	(145)	(252)	(145)
P-value for Chisquared-test of no effect for financially unconstrained firms	0.212	0.189	0.793	0.985	0.978	0.931	0.865	0.604
Firm-Period Observations	504	504	504	504	504	504	504	504
Number of Firms	113	113	113	113	113	113	113	113

Notes:results for sample with 5% trimming of firms based on percentage change in profits
All regressions include period effects, and interactions between period effects and the interaction variable.
Random effects regressions also include the level effect for the variable being interacted.
Financially super-constrained firms are defined as firms saying finance is a constraint, who have never had a bank loan or supplier credit.
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1