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RETURNS TO CAPITAL IN MICROENTERPRISES: EVIDENCE FROM A FIELD EXPERIMENT

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Abstract

Small and informal firms account for a large share of employment in developing countries. The rapid expansion of microfinance services is based on the belief that these firms have productive investment opportunities and can enjoy high returns to capital if given the opportunity. However, measuring the return to capital is complicated by unobserved factors such as entrepreneurial ability and demand shocks, which are likely to be correlated with capital stock. We use a randomized experiment to overcome this problem, and to measure the return to capital for the average microenterprise in our sample, regardless of whether or not they apply for credit. We accomplish this by providing cash and equipment grants to small firms in Sri Lanka, and measuring the increase in profits arising from this exogenous (positive) shock to capital stock. After controlling for possible spillover effects, we find the average real return to capital to be 5.7 percent per month, substantially higher than the market interest rate. We then examine the heterogeneity of treatment effects to explore whether missing credit markets or missing insurance markets are the most likely cause of the high returns. Returns are found to vary with entrepreneurial ability and with measures of other sources of cash within the household, but not to vary with risk aversion or uncertainty.

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1. Introduction

Small and informal firms are the source of employment for large sectors of the labor force in many developing countries. A key question of interest is whether these firms hold the potential for income growth for their owners, or whether they merely represent subsistence sources of incomes for low-productivity individuals unable to find alternative sources of work. Evidence that *some* firms have high marginal returns is seen in the very high interest rates paid by some individuals to moneylenders, and by literature which identifies the effect of credit shocks on those who apply for credit (see Banerjee and Duflo, 2005 for an excellent recent summary). However, the sample of firms who apply for credit or who belong to microfinance organizations may not represent the full universe of firms for any number of reasons.[1] In this paper we use a randomized experiment to identify the marginal return to capital for all firms, irrespective of whether or not they choose to apply for credit at market interest rates. We then examine the heterogeneity of returns in order to test which theories can explain why firms may have marginal returns well above the market interest rate.

We accomplish this by surveying microenterprises in Sri Lanka and providing small grants to a randomly selected subset of the sampled firms. The grants were either 10,000 Sri Lankan rupees (about $100), or 20,000 rupees (about $200). Half the grants were given in the form of cash, and the other half in the form of equipment or inventories selected by the entrepreneurs. We purposely restricted our survey to small firms with less than 100,000 rupees (about $1000) in capital other than land and buildings. The median level of capital investment among firms in the sample is about 18,000 rupees. The capital shocks provided were therefore large relative to the size of the firms, with the larger shock equal to more than 100 percent of the median level of invested capital.

An accurate estimate of returns to capital in small enterprises is important for understanding both the limitations and the potential of microenterprises to play a role in increasing income levels in developing countries. If returns are very low at low levels of capital stock, then this would indicate that capital is an important barrier to opening a

[1] These reasons include both selection among entrepreneurs as to whether or not to apply for credit, determined by factors such as their attitudes to risk, access to alternative sources of finance, perceptions of the returns on investment, and expectations of getting a loan; as well as selection on the part of the lender as to which firms to accept as clients.

business. Low returns would suggest that individuals without access to a sufficient amount of capital would face a permanent disadvantage. That is, they would fall into a poverty trap. The theoretical literature on occupational choice has posited a minimum scale below which returns to capital are very low or even zero.[2]

If, on the other hand, returns are high at low levels of capital stock, then an entrepreneur would be able to open a business and grow by reinvesting profits. In this case, firms might remain at a sub-optimal size for some period of time, but entrepreneurs would not be permanently disadvantaged. With time, they would be able to bootstrap their way up to an optimal firm size. In the absence of minimum scale, we might expect returns to be high. Because small scale entrepreneurs lack access to credit at market interest rates, even risk-adjusted returns may well be higher than market interest rates.

Given the importance of this issue, there are an increasing number of estimates of the returns to capital in small scale productive activities in developing countries (see Banerjee and Duflo, 2005). Some of the estimates come from broad cross-sections of producers, and others from the subset of firms exposed to some shock. Among the former, McKenzie and Woodruff (2006) estimate returns to capital among the smallest urban microenterprises in Mexico, those with less than $US200 invested, of around 15 percent per month. Monthly returns in the Mexican data fall to around 3-5 percent above the $500 range, equivalent to 40-80 percent annually. Udry and Anagol (2006) estimate returns to capital in a sample of small scale agricultural producers in Ghana to be 50 percent per year among those producing traditional crops on a median-sized plot and 250 percent per year among those producing non-traditional crops on median sized plots. Banerjee and Duflo (2004), on the other hand, take advantage of a panel of firms in India during a period in which changes in laws forced banks to make preferential loans to certain groups of firms. The changes in the laws allows for the identification of changes in access to finance among the firms. Banerjee and Duflo derive estimates of returns for this set of firms of between 74 and 100 percent per year.

The central challenge in estimating returns to capital is that the optimal level of capital stock is likely to depend on attributes of entrepreneurial ability which are difficult

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[2] Banerjee and Newman (1993) and Aghion and Bolton (1997) are seminal papers in this literature. Barrett and Swallow (2004) discuss the effects of minimum scale on investment in rural Africa..

to measure. Banerjee and Duflo point to one way around this problem—exploiting an exogenous shock which is uncorrelated with entrepreneurial ability. But this approach has a downside, in that the sample is limited to firms or entrepreneurs exposed to the shock. In the case of Banerjee and Duflo's estimates, the estimates apply only to those firms applying for credit. This resolves one problem at the cost of creating another: While estimated returns are less subject to ability bias among this subsample of firms, self-selection of the subsample suggests the returns likely overstate those for the full spectrum of firms. Low returns to capital are only one of several possible reasons entrepreneurs may fail to apply for loans. Thus, we should be interested in returns in the broader sample of firms.

The difficulty of obtaining an unbiased estimate of returns to capital across all firms in a sector is the motivation for the field experiment on which the data used in this paper are based. We use random grants of either cash or equipment to generate changes in capital against which changes in revenues and profits can be measured. Randomized experiments have quickly become an important part of the toolkit of development economics (Duflo, Glennerster and Kremer, 2006), although this is the first experiment we are aware of involving payments to firms, rather than households or schools. The random allocation of the grants ensures that the changes in capital stock are uncorrelated with entrepreneurial ability, demand shocks, and other factors associated with the differences in the profitability of investments across firms. However, a further condition for randomization to consistently identify the return to capital is that there are no spillover effects from treated to untreated firms. We use GPS location data to control for the number of firms in the same industry within a given radius of each firm. We find evidence for spillover effects of the treatment of firms in the bamboo industry, but not for the remaining firms.

We first use the data to estimate the impact of the random capital shocks on the profits of the firms, and estimate a real marginal return on capital for the average firm in our sample after controlling for spillover effects. We next use the two different sizes of treatment to test whether returns are linear, increasing, or decreasing in the size of the treatment amount. We estimate the average real return to capital to be around 5.7 percent per month, much higher than the market interest rate, and cannot rule out linearity in returns.

We then set out a model consistent with our data which can be used to investigate the proximate causes of such high returns to capital. The two main theories we consider are imperfect credit markets and imperfect insurance markets. Examination of the heterogeneity of treatment effects leads us to find that returns to capital are higher for entrepreneurs with higher ability and with fewer other wage workers in the household. We do not find significant interaction effects consistent with risk aversion or uncertainty limiting investment in the enterprise. This suggests that the reason for high returns to capital is missing credit markets rather than missing insurance markets, and that the household internal capital market is a binding constraint.

We begin by describing the experiment. We follow in Section 3 with a description of the data. Section 4 estimates the treatment effect and Section 5 the return to capital. Sections 6 and 7 consider robustness to spillovers, attrition, reporting differences, different treatments, and nonlinearities. In Section 8, we develop a simple model of household investment which incorporates both the riskiness of investment and factors affecting the shadow value of capital. The model provides several testable implications, which we confront with the data. Section 9 concludes.

2. Description of Experiment

Selection of the participants

We carried out a baseline survey of microenterprises in April 2005 as the first wave of the Sri Lanka Microenterprise Survey (SLMS).[3] Four additional waves of the panel survey were then conducted at quarterly intervals. The survey took place in three Southern and South-Western districts of Sri Lanka: Kalutara, Galle and Matara. The survey is designed to also study the process of recovery of microenterprises from the December 26, 2004 Indian Ocean tsunami, and so these districts were selected as ones where coastal areas had received some tsunami damage. We set out to draw a sample of firms with invested capital of 100,000 rupees (about US$1000) or less, excluding

[3] Fieldwork was carried out by ACNielsen Lanka (Pvt) Ltd.

investments in land and buildings. This size cut-off was chosen in order that our treatments (described below) would be a large shock to business capital.

We began by using the 2001 Sri Lankan Census to select 25 Grama Niladhari divisions (GNs) in these three districts. A GN is an administrative unit containing on average around 400 households. We used the Census to select GNs with a high percentage of own-account workers and modest education levels, since these were most likely to yield enterprises with invested capital below the threshold we had set. The GNs were stratified according to whether the area was directly affected, indirectly affected, or unaffected by the tsunami. A door-to-door screening survey was then carried out among households in each of the selected GNs. This survey was given to 3361 households, with less than 1 percent of households refusing to be listed. The screening survey identified self employed workers outside of agriculture, transportation, fishing, and professional services who were between the ages of 20 and 65 and had no paid employees.

The full survey was given to 659 enterprises meeting these criteria. After reviewing the baseline survey data, we eliminated 41 enterprises either because they exceeded the 100,000 rupee maximum size or because a follow-up visit could not verify the existence of an enterprise. The remaining 618 firms constitute the baseline sample. For the purposes of this paper we exclude the firms which were directly affected by the tsunami, since the recovery of assets damaged by the tsunami might affect returns to capital. This gives a baseline sample of 405 firms which were not directly affected by the tsunami. The remainder of the paper will focus only on these firms.

The 405 firms are almost evenly split across two broad industry categories, with 203 firms in retail sales and 202 in manufacturing/services. Firms in retail sales are typically small grocery stores. The manufacturing/services firms cover a range of common occupations of microenterprises in Sri Lanka, including sewing clothing, making lace products, making bamboo products, repairing bicycles, and making food products such as hoppers and string hoppers.

The Experiment

The aim of our experiment was to randomly provide firms with a positive shock to their capital stock, and measure the impact of this on business profits. Firms were told before the initial survey that as compensation for participating in the survey, we would conduct a random prize drawing, with prizes of cash or equipment for the business. The prize consisted of one of four grants: 10,000 Sri Lankan rupees (~$100) in equipment for their business, 20,000 rupees in equipment, 10,000 rupees in cash, or 20,000 rupees in cash. In the case of equipment grants, the equipment was selected by the enterprise owner, and purchased by research assistants working for the project.[4]

After the first round of the survey, 124 firms were randomly selected to receive a treatment, with 84 receiving a 10,000 treatment and 40 receiving a 20,000 treatment. The randomization was done within district (Kalutara, Galle, and Matara) and zone (unaffected and indirectly affected by the tsunami). A second lottery was held after the third round of the survey, with an additional 104 firms selected at random from among those who didn't receive treatment after the first round: 62 receiving the 10,000 treatment and 42 the 20,000 treatment. In each case half the firms receiving a treatment amount received cash, and the other half equipment. Allocation to treatment was done *ex ante*, and as a result, there were an additional seven firms who were assigned to receive treatment after round 3, but who had attrited from the survey by then.

The 10,000 rupee treatment was equivalent to about three months of median profits reported by the firms in the sample, and the larger treatment equivalent to six months of median profits. The median initial level of invested capital, excluding land and buildings, was about 18,000 rupees, implying the small and large treatments correspond to approximately 55 percent and 110 percent of the median initial invested capital. By either measure, the treatment amounts were large relative to the size of the firms.

Although the amount offered for the equipment treatment was either 10,000 rupees or 20,000 rupees, in practice the amount spent on inventories and equipment sometimes differed from this amount. Among those receiving equipment, only a handful of firms spent less than the amount we offered. Only four of the 116 firms receiving equipment treatments spent as much as 50 rupees ($0.50) less than the amount we

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[4] In order to purchase the equipment for these entrepreneurs receiving equipment treatments, research assistants visited several firms in the evening to inform them they had won an equipment prize. The winning entrepreneurs were asked what they wanted to buy with the money, and where they would purchase it. The research assistants then arranged to meet them at the market where the goods were to be purchased at a specified time the next day. Thus, the goods purchased and the place/market where they were purchased were chosen by the entrepreneurs with no input from the research assistants.

offered. One of those spent 1200 less than we offered, one about 2500 less and the other two about 100 less than we offered. More commonly, the entrepreneurs contributed funds of their own to purchase a larger item. This occurred in 65 of the 116 equipment treatments. However, in most cases, the amount contributed by the entrepreneurs was trivial. In two-thirds of these cases (44 out of 65), the extra spending was less than 500 rupees, or $5. In 11 percent of the equipment treatments (13), the entrepreneurs contributed 2000 or more rupees from other sources. In three of those cases, their contribution was more than 10,000 rupees. We use the amount offered rather than the amount spent in our analysis of the effects of treatment. We have both receipts and pictures of all of the goods purchased with the equipment grants. Approximately 57 percent of the goods purchased were inventories or raw materials, 39 percent machinery or equipment, and 4 percent construction materials for buildings.

Cash treatments were given without restrictions. Those receiving cash were told that they could purchase anything they wanted, whether for their business or for other purposes. In reality, the grant was destined to be unrestricted because we lacked the ability to monitor what they did with the funds, and because cash is fungible. We felt that being explicit about the lack of restriction was likely to lead to more honest reporting regarding use of the funds. In the survey subsequent to the treatment, we asked what they had done with the treatment.[5] Approximately 58 percent of the cash treatments were invested in the business between the time of the treatment and the subsequent survey. An additional 12 percent was saved, 6 percent was used to repay loans, 5 percent on expenditures for the household, 4 percent on repairs to the house, 3 percent on equipment or inventories for another business, and the remaining 12 percent on “other items.” Of the amount invested in the enterprise, about two-thirds was invested in inventories and one-third in equipment.

Verifying Randomization

Table 1 compares the baseline characteristics of firms ever assigned to treatment with those firms always in the control group. In general the randomization appears to

[5] Our question noted that some entrepreneurs had told us they had spent the money on furniture or other items for the household, some had spent it on food and clothing, and some had invested in their business. In fact, they had told us this during piloting of the round 2 survey.

have worked in creating groups which are comparable in terms of baseline characteristics. There is no significant difference in the baseline profits, revenue, capital stock, age, and level of informality of the treated and untreated firms. The age, education, parental background, and risk aversion are similar for owners in both groups. The only significant difference between the two groups occurs for a household durable asset index, with firm owners in the control group having slightly higher mean baseline assets. To test for joint significance of all these variables, we estimate a probit model of whether or not a firm is assigned to treatment as a function of the 16 baseline characteristics given in the Table. The p-value from the chi-squared test is 0.355, so that we can not reject the null hypothesis that these observable characteristics do not help predict whether or not a firm was assigned to treatment.

3. Data/Survey

The baseline survey gathered detail information on the firm and the characteristics of the firm owner. The main outcome variable of interest in this paper is the profits of the firm. Firm profits were elicited directly from the firm by asking:

> "What was the total income the business earned during March after paying all expenses including wages of employees, but not including any income you paid yourself. That is, what were the profits of your business during March?"

The reported mean and median profits in the baseline are 3900 rupees and 3000 rupees respectively. The survey also asked detailed questions on revenues and expenses. Profits calculated as reported revenues minus reported expenses are lower, around 2500 at the mean and 1350 at the median. Profits calculated in this manner are positively correlated with reported profits, with a correlation coefficient of 0.32. This is about the same level as one finds in other microenterprise surveys. In de Mel, McKenzie and Woodruff (2007), we analyze the measurement of profits in some detail, reporting on experiments conducted with different questions, bookkeeping, and monitoring of sales. The biggest reason reported and calculated profits differ is a mismatch of the timing of purchases and the sales associated with those purchases. Some of the expenses in one month are associated with sales the following month. Correcting for this mistiming increases the correlation with reported profits to around 0.70. We conclude from the more detailed analysis of measurement issues that the reported profit is the best measure of the firm's profitability, and we use those data for the remainder of this paper.

The baseline survey also gathered detailed information on the replacement cost of assets used in the enterprise, and whether they were owned or rented. 99 percent of assets excluding land and buildings are owned by the enterprises. The majority of assets owned by the enterprises are land and buildings. In the baseline sample, these average 121,000 rupees ($1200), though about a sixth (15 percent) of firms report they own no assets in this category. The firms also reported an average of 14,400 ($145) rupees worth of machinery and equipment and 13,000 ($130) in inventories.

In each subsequent round of the survey, we then asked firms to report on the purchase of new assets, the disposition of assets by sale or damage, and the repair and return to service of assets. These are used to calculate a change in assets over the quarter. Combined with the data from each previous quarter, these data allow us to estimate investment levels by category for each quarter of the survey. Note that the data are not adjusted for depreciation of machinery and equipment. Given the relatively short period over which the data are gathered and the high percentage of assets invested in inventories, we do not see this as a major concern. The survey also asks the current value of inventories of raw material, work in progress and final goods each quarter.

We began with 405 firms in the baseline survey, and still had 384 of the firms in the survey by the fifth wave, an attrition rate of only 5.2 percent. However, only 388 of the 405 firms reported profits in the first wave, 390 in the second wave, 381 in the third, 366 in the fourth, and 366 in the fifth. We concentrate our analysis on the unbalanced panel of 383 firms reporting at least three waves of profit data. Fifteen of the firms assigned to the control group and 7 of the firms assigned to the treatment group attrited from the sample before reporting at least three waves of profit data. We show our results are robust to this attrition.

4. Estimation of the Mean Treatment Effect

We begin by assuming each enterprise is characterized by a linear production function, and that the treatments have homogeneous effects on the enterprises. We later relax both of these assumptions. Estimating the mean effect of treatment on business profits requires estimating for firm *i* in period *t*:

$$PROFITS_{i,t} = \alpha + \beta AMOUNT_{i,t} + \sum_{s=2}^{5}\delta_s + \varepsilon_{i,t} \tag{1}$$

Where $AMOUNT_{i,t}$ is an indicator of how much treatment firm *i* had received at time *t*, coded in terms of 10,000 rupees. *AMOUNT* therefore takes value 1 if the firm received the 10,000 rupee treatment, 2 if the firm received the 20,000 rupee treatment, and 0 otherwise. The δ_s are period effects. Table 1 showed that the randomization appears to have worked, giving treatment and control groups which are comparable in terms of observable baseline characteristics. Hence the expected correlation between the error term $\varepsilon_{i,t}$ and treatment status is zero. We can therefore estimate equation (1) using random effects, which assumes $\varepsilon_{i,t}$ to be uncorrelated with treatment status, but allows it to be correlated over time. Alternatively, we can use fixed-effects estimation, which will improve the precision of the estimates by capturing any time-invariant characteristics of the firm which affect business profits.

The first row of Table 2 presents the resulting treatment effects on nominal profits. Column 1 shows the random effects estimate and Column 2 the fixed effects estimate. The coefficients show a 10,000 rupee treatment increased monthly profits by 489 to 569 rupees, a monthly nominal return on the treatment of 4.9 to 5.7 percent, with these treatment effects significant at the 5 percent level. The profits data are noisy, and so Columns 3 and 4 present results after trimming outliers. In Column 3 we trim firms with monthly nominal profits below 200 rupees or above 25,000 rupees, corresponding to the 1^{st} and 99^{th} percentiles of the nominal profits distribution. This increased the estimated nominal return on the treatment to 6.1 percent, now significant at the 1 percent level. A firm that always has profits above 25,000 rupees is likely to just be a very productive firm, whereas a firm whose profits change from 1500 to 30,000 to 2000 could be one in which misreporting occurred. Column 4 therefore presents our preferred specification, which trims firms on the basis of extremely large *changes* in profits. We trim firms where the change in monthly profits from one quarter to the next was less than -90 percent or greater than 962 percent, corresponding to the 1st and 99th percentiles of the distribution of changes in profits. The estimated nominal return on the treatment is 5.9 percent per month after this trimming.

Inflation in Sri Lanka was low over the period studied, with point-to-point inflation of 4.0 percent between March 2005 and March 2006. The second row of Table 2 examines the treatment effect on real profits, where the monthly Sri Lanka Consumers' Price Index was used to deflate nominal profits.[6] After trimming based on extreme changes in profits, we see that the real monthly increase in profits was 5.7 percent of the treatment amount.

Impact on Labor Hours

This treatment effect represents the increase in real monthly profits from our treatment. The treatment increased capital stock. However, labor hours may have also have been affected if firm owners adjusted their own labor hours, or if the resulting rise in profits caused owners to substitute towards leisure. We therefore empirically investigate the impact of the treatment on labor hours by estimating equation (1), using own labor hours and family worker labor hours as the dependent variables.[7] The treatment is found to have a positive and significant impact on own labor hours: a 10,000 rupee treatment increases own weekly hours by 3.4 hours (t=3.23, p=0.001). There is a small and insignificant negative impact on family labor hours (a -0.7 hour per week effect, p=0.467).

Recall that business profits include the earnings of the firm owner. Hence the increase in real profits from the treatment reflects both the return to the additional capital, and the return to the owner of the additional labor hours worked. To isolate the impact of capital, we need to subtract the implicit wage earned by the firm owner. We estimate the marginal return to own labor by using the baseline data to regress profits on capital stock (exclusive of land and buildings), age of the owner, owner's education, a dummy for the

[6] Source: Sri Lanka Department of Census and Statistics, http://www.statistics.gov.lk/price/slcpi/slcpi_monthly.htm [accessed February 17, 2007].

[7] Recall that none of the firms had paid non-family workers in the baseline, and less than 5 percent of firms had non-family workers at any time during the panel survey.

owner being female, and the owner's monthly labor hours. The coefficient on owner's labor hours is 5.43 (p=0.003), implying a 5.43 rupee per hour marginal return to an hour of work. Valuing a labor hour at 5.43 rupees implies that approximately 11 percent of the firm-period real net profit observations are negative.

The third row of Table 2 then examines the impact of the treatment on real net profits, using this adjustment for labor hours. The treatment effect is still positive and significant in all specifications. The fourth column shows that after trimming outliers, the treatment effect corresponds to a 5.0 percent real increase in monthly net profits.

Finally, in the fourth row of Table 2, we carry out an extreme adjustment of profits for labor hours. The median profit per hour worked in the baseline is 14.1 rupees per hour. Taking this as the implicit wage rate assumes that there is no return to capital in the baseline, only to labor, and therefore we consider this as an extreme upper bound on the implicit wage. Indeed, 37 percent of the firm-period observations on net profits are negative after this adjustment, which appears too high. Even after using this extreme adjustment, Columns 3 and 4 of Table 2 still show a positive treatment effect, significant at the 10 percent level. The real return is 3.9 to 4.1 percent per month, interpreted as the return on the treatment *after* accounting for the return on additional labor hours induced by the treatment.

Overall, Table 2 shows the treatment had a significant and positive effect on firm profits, increasing real monthly profits by 5.7 percent, and increasing real monthly net profits by 5.0 percent after valuing the increase in labor hours at 5.43 rupees per hour. For the remainder of our analysis we will use this real net profit as our dependent variable. Similar results are obtained if we use real profits.

5. Estimation of the Return to Capital

The above results show the return to the experiment of giving cash or equipment to firm owners. While such an experiment may be informative of the likely impact of a government welfare program akin to conditional cash transfer programs used for education, our main interest lies in using the experiment to measure the return to capital. This requires estimating:

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

where the treatment amount offered to firms is used to instrument capital stock, $K_{i,t}$.

Table 3 reports the estimated return to capital from estimating equation (2). Columns 1 through 4 repeat the specifications used in Table 2, using random effects, fixed effects, trimming the level of profits, and trimming changes in profits. Columns 5 through 8 adjust for potential outliers in capital stock, by additionally trimming the top and bottom 1 percent of initial capital stocks. After trimming profits we get real returns to capital in the 6.8-6.9 percent per month range, which increase to 7.3-7.4 percent once we trim outliers in initial capital stock.

The lower panel of Table 3 shows the coefficient on the treatment amount in the first stage regression for capital stock. The treatment amount is a strongly significant predictor of changes in capital stock (F-statistic always above 41), with each 1000 rupees of treatment increasing capital stock by 744-800 rupees. This explains why the return on capital is higher than the treatment effect, since the change in capital stock is less than the treatment amount.

6. Controlling for potential Treatment Spillovers

The key condition for randomization to provide us with a valid estimate of the treatment effect and of returns to capital is the standard unit treatment value assumption, which requires that the potential outcomes for each firm are independent of its treatment status, and of the treatment status of any other firm (Angrist, Imbens and Rubin 1996). As Miguel and Kremer (2004) and Duflo, Glennerster and Kremer (2006) note, the presence of spillovers can cause this assumption to be violated, leading to biased estimates of the treatment effect. It is therefore important to test for spillover effects arising from our grants to firms.

We collected the GPS coordinates of each firm in our survey, taking advantage of improvements in precision and technology which allow location to be measured accurately to within 15 meters, 95 percent of the time (Gibson and McKenzie, 2007). This allows us to construct a measure of the number of treated firms in the same industry at any given point in time within a given radius of each firm. In our baseline survey, the median firm reported that 80 percent of its revenue came from customers within 1 kilometer of the business. With this in mind, we examine the effects of treatments provided to firms in a radius of either 500 meters or 1 kilometer from each firm. After the second set of treatments, the median firm in our sample has one firm in its industry treated within 500m, and also one firm treated within 1km. The means are 1.8 firms within 500m and 3.1 firms within 1km.

We then estimate the treatment effect regression as:

$$PROFITS_{i,t} = \alpha + \beta AMOUNT_{i,t} + \lambda N_{i,t}^{d} + \sum_{s=2}^{5} \delta_s + \varepsilon_{i,t} \qquad (3)$$

where $N_{i,t}^{d}$ is the number of treated firms in the same industry within radius d of firm i at time t. The average overall treatment effect on profits for treated firms is then $\beta + \lambda \overline{N}^{d}$ where $\overline{N}^{d}$ is the average number of treated firms in neighborhood of distance d of a treated firm. We likewise augment the returns to capital regression in equation (2) to include this spillover effect. The estimated returns to capital will be just the coefficient β on capital, which gives the marginal impact on profits of a change in capital, controlling for any firms getting treated nearby. Because the mean number of treated firms within 500 meters is identical in the sample of treated and untreated firms, each treatment negatively affects other treated and control firms in an identical manner, implying that β remains the estimated return on changes in capital stock.

Table 4 then reports the results of estimating (3). Columns (1) and (3) show a negative and significant spillover effect when estimating the treatment effect and return to capital respectively. Each treated firm within 500 meters lowers profits by 138-151 rupees. This reduces the overall treatment effect from 504 to 294 rupees for the mean treated firm. However, even after controlling for the number of firms treated within the neighborhood of a firm, the estimated return to capital is very close to that estimated in Table 3: 7.5 percent. The spillover effects are insignificant when we consider a neighborhood of radius 1 kilometer around the firm, although are similar in size when taken at the mean.

The distribution of the number of firms within a neighborhood of a treated firm is highly skewed. When we examine this by industry, we find that the bamboo industry is an outlier. All of our 29 bamboo product firms are located in two adjacent G.N.s, and the median (mean) bamboo firm has 12 (10) treated bamboo firms within 500 meters by round 5 of our survey. In contrast, all other industries have mean and median number of treated firms of 3 or less in wave 5. Columns 5 through 8 of Table 4 repeat the spillover analysis on a sample excluding the bamboo industry. The spillover effect shrinks in magnitude and is no longer statistically significant. This exclusion does cause the treatment effect and the estimated return to capital to fall, although the real return to capital is still significant at the 5 percent level and estimated to be 5.7 percent per month.[8]

The significant spillovers therefore seem confined to the bamboo industry. These firms produce items such as baskets, mats, and food covers out of bamboo. There are restrictions imposed by the government on the harvesting of bamboo, limiting the supply of raw materials. Several of the firms wished to buy raw materials with the equipment treatments we provided, but were not able to buy 10,000 or 20,000 rupees of bamboo at once from suppliers. They therefore paid the suppliers as advance payment for future supplies. In doing so, it is likely that they crowded out the supplies of other firms in the same industry. Combined with the heavy density of firms within one area, this may explain the significant negative spillover effects in this industry.

These results suggest that the firms in the bamboo industry are different from the remaining firms in the sample. We therefore exclude these 29 firms for the remainder of the analysis. What happens to our main treatment effects and estimated return to capital once we exclude bamboo, limiting the sample to the firms without significant treatment spillovers? Table 5 repeats our preferred specifications in Tables 2 and 3 after excluding bamboo. The treatment effect is estimated to result in a 381-409 rupee increase in real adjusted monthly profits. The real return to capital is estimated to be 5.7 percent per month. This effect is the same as seen in columns 7 and 8 of Table 4, and is therefore robust to controlling for the statistically insignificant spillovers.

[8] A spillover radius of 100 meters produces results which are similar in all respects to the radius of 500m. Spillovers are significant for the full sample, but insignficant once the bamboo sector is excluded. The coefficient is larger, reflecting the fact that many fewer firms have a treated enterprise within 100m. These results are available from the authors upon request.

7. Further Robustness

Robustness to Attrition

The attrition rate in the SLMS was remarkably low for a five-wave panel survey. Nevertheless, the combination of attrition, missing profit data, trimming capital stock, and trimming firms with large changes in profits reduces our sample of non-bamboo firms from 376 to 325 firms. Of the 51 firms which are dropped, 24 were assigned to treatment and 27 to the control group. This corresponds to a broad attrition rate (including both attritors and those dropped for missing or noisy data) of 11.1 percent for the treated, and 16.9 percent for the control group. 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.

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 that would not have dropped out if they had remained in the control group.[9]

To construct the Lee (2005) bounds we then trim 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. In our application, this requires trimming the upper or lower 6.5 percent of the adjusted profits distribution for the group assigned to treatment. Doing this then gives a lower bound for the treatment effect of 348 rupees and an upper bound of 399 rupees. The returns regressions

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[9] Of course attrition from our estimation sample also arises from an outlier change in profits. Conceivably the treatment might cause a large increase in profits, making attrition for this reason more likely. However, it would have to increase profits by more than 962 percent in order to do this, and if this were the case, dropping such observations will only lower the estimated treatment effect.

corresponding to these treatment effects are 627 rupees and 584 rupees respectively.[10] Thus attrition can at most lower our treatment effect from 3.8 percent to 3.5 percent, with this only occurring in the unlikely event that it is the most profitable control firms attriting. Moreover, even in this case, our estimated return to capital is still 5.7 percent. Thus the estimated return to capital appears robust to corrections for attrition.

Robustness to Reporting Effects

Our main outcome of interest is the profits reported by the firm – one item in a series of questions about income and expenditure asked midway through our questionnaire. A potential concern is that firms change their reporting of profits in the survey as a result of the treatment, in which case the measured returns do not correspond to actual returns. There are two potential elements of this concern. The first is that firms might deliberately overreport profits after receiving the treatment. The second is that treatment makes firms trust us more, and therefore makes them less likely to underreport. We address each in turn.

Deliberate overreporting of profits in response to treatment is likely to be a concern in evaluation of business loans or business training programs, where firms who wish to receive more help from the program in the future wish to show that they are benefiting from the treatment they have received. We believe this to be less of a concern in our experiment for several reasons. The first is that the treatment was presented to the firm as a "prize" received for participating in a survey, not as a loan or program offered to them. Firms were told these prizes were awarded randomly, and had no reason to think future prizes would be forthcoming on the basis of how they used the present prize. Secondly, if firms were deliberately overreporting how much use the prize had been to their business, we would expect them to overreport the share of the cash prize which they had invested in their business. However, as noted above, on average firms report only investing 58 percent of the cash treatment in the business, and saving an additional 12 percent. When we estimate the returns to capital just for the cash treatment, the first stage

[10] Trimming treated firms in the top or bottom 6.5 percent of profits also leads to a change in the first-stage coefficients. Trimming the most profitable firms lowers the first-stage coefficient on amount from 0.80 to 0.75. This more than counteracts the reduction in the treatment effect, leading to an estimated return to capital which is higher than that which occurs when we trim the least profitable firms (where the first-stage coefficient is 0.81).

coefficient on the cash amount is 0.76. Thus the 70 percent of the cash treatment that owners say they invest in the business or save is approximately the amount we find capital stock to increase by, and certainly does not appear an overstatement. Therefore it appears that firm owners are responding honestly to the question of how they invest the cash, making it likely they are also not deliberately overreporting profits.

The second potential concern is that the receipt of a prize makes firms trust our survey team more, and alleviate concerns about the possibility of the survey somehow being affiliated with tax collection.[11] Our survey provides two checks on whether reporting changed with treatment. As a first check, we can use the fact that firms were asked for March 2005 sales twice: once in the baseline sample before any treatments were administered, and again in the second round, after the first group of firms had received treatment. We take the ratio of the round 2 report of sales to the round 1 report, and test whether this differs according to whether firms received treatment between the two rounds. The mean ratio is 1.296 for treated firms and 1.306 for untreated firms, with a t-test of equality having a p-value of 0.963. Thus in both cases firms increased their report of sales compared to the first round, but this had no relationship to treatment status.

As a second check on whether reporting changed with treatment, we can use the fourth round questionnaire, taken in January 2006, which asked about sales for October, November, and December 2005. The second round of treatments occurred in November 2005. We can then test whether the January 2006 reporting of October sales differs for firms treated in November compared to never-treated firms. Mean sales for October are 17,818 rupees for the treated group, compared to 17,764 for the untreated group. A t-test of equality has a p-value of 0.980. Thus again there is no evidence that reporting changed with treatment.[12]

[11] The main taxes affecting the firms in our sample are registration and assessment fees, if they register with the municipal government. Income taxes only apply for annual income above 300,000 rupees, which only applies to approximately 5 percent of our firms.

[12] Alternatively, we can look at the growth of sales between September, reported in the October survey before the round 3 treatment, and November, reported in January after the treatment. Again, we find no significant difference between the treated and untreated enterprises.

Together these results strongly suggest that the estimated returns are indeed true returns, and not merely a consequence of firms changing reporting of profits in response to treatment. We now turn to examining how the returns vary by type of treatment.

Cash vs. Equipment Treatments and Linearity of Returns

The analysis thus far has pooled together the treatments provided as cash and equipment, and the 10,000 and 20,000 rupee treatment levels. Table 6 examines whether the impact of the equipment treatment varies from that of the cash treatment. The first column repeats the overall treatment effect in row 3, Column 4 of Table 2. Column 2 then separates the treatment into two variables, one representing the equipment treatment amount and the other representing the cash treatment amount. We see that the coefficients on the equipment and cash treatments are nearly identical, 370 on equipment and 393 on cash. Since we can not reject equality of the two treatment effects, we will continue to pool the two treatments in the remaining analysis.

The third column of Table 6 uses two separate dummy variables to examine the impact of the 10,000 and 20,000 rupee treatments, with the sample excluding firms in the bamboo industry and trimmed at the 1^{st} and 99^{th} percentile of changes in profits. The 10,000 treatment is estimated to raise net profits by 480 per month, compared to a 721 increase for the 20,000 treatment. However, the standard errors in both point estimates are high. We can reject neither the hypothesis that the effect of the 20,000 treatment is twice that of the 10,000 treatment, nor the hypothesis that the two treatments have the same effect.

Column 4 of Table 6 provides an alternative test of non-linearity of treatment effects by interacting the amount of the treatment with the baseline level of capital stock, exclusive of land and buildings. We also interact the period effects with this initial level of capital stock to allow for different growth paths of firms according to their initial capital stock levels. Column 4 shows the results after 1 percent trimming of firms on the basis of changes in profits. The interaction term is negative, which would suggest that the return to capital is decreasing, but is not statistically significant.

As a further check as to whether the treatment effect varies with firm size, we use propensity score matching to match each treated firm which received the 10,000 rupee treatment to its four closest non-treated firms based on baseline characteristics.[13] For each treated firm we can then calculate the difference between its change in profits from the baseline period and the change in profits of its four closest matches in each round after treatment. Figure 1 then plots these differences against the initial capital stock of the firm, using lowess for smoothing, along with a bootstrapped 95 percent confidence interval. A horizontal line is plotted at 381 rupees, the treatment effect in column 2 of Table 5. The figure shows the point estimate of the treatment effect is always above zero, and are reasonably flat, hovering close to the 381 line.

Taken together, these results suggest that returns to capital are either linear or decreasing in the size of the firms. We find no evidence consistent with increasing returns at the range of capital stock represented by our firms.

8. Explaining the high returns to Capital

We find that the treatment increased real monthly business profits by 5.7 percent. Even if all these additional profits are consumed by the household and not compounded by reinvestment in the business, this would still give a real annual return of 68 percent. This greatly exceeds the market interest rate on loans being charged by banks and microfinance institutions. Typical *nominal* market interest rates are 16-24 percent per annum for two year loans. Assuming a 4 percent inflation rate, this equates to an effective annualized real rate of 12-20 percent per annum. The presence of these high marginal returns well in excess of the market interest rate therefore raises the question of why firms are underinvesting and not taking advantage of these high returns, an issue we address in this section.

8.1. A Model of Heterogeneous Returns

In the baseline survey, 78 percent of firm owners reported that their business was smaller than the size they would like. When asked what they view as constraints to the growth of their business, the most prevalent constraint reported is lack of finance, which

[13] Firms were matched using on the basis of the entrepreneur's age, sex, education, ability, religion, risk aversion, uncertainty, household size, number of workers in the household, household assets, and the firm's industry, November 2004 profits, round 1 profits, legal registration status, location, and round 1 capital stock.

93 percent of firms say is a constraint. The second most prevalent constraint, lack of inputs, which 53 percent of firms list as constraint, is also likely to reflect in part liquidity constraints, as firms said that they couldn't afford to buy all the inputs they would need. The perception of financial constraints is supported by the relatively rare use by firms of formal finance. Only 3.1 percent of our firms have a bank account for business use, and 89 percent of firms got *no* start-up financing from a bank or microfinance organization. Formal credit is scarcely used at all for financing additional equipment purchases. Instead the major source of both start-up and ongoing funds is personal savings of the entrepreneur and loans from family. On average 69 percent of start-up funds came from this source, and 71 percent of firms relied entirely on own savings and family for start-up funds.

After finance, the second most common set of constraints to growth according to the firms themselves can be broadly interpreted as reflecting uncertainty among firms about realizing the gains from investment. The possibility of lack of demand for products (which 34 percent of firms say is a constraint), lack of market information (16 percent say is a constraint), and economic policy uncertainty (15 percent say is a constraint) all suggest that the riskiness of returns could be important. Relatively few firms view the legal and regulatory framework as constraints to growth: 16 percent say lack of clear ownership of land, 6 percent say legal regulations, and only 1 percent say high tax rates is a constraint. Few firms view infrastructure as a constraint.

These perceptions suggest that missing markets for credit or for insurance against risk could be important factors in explaining the high marginal returns to capital. These are two of the most common explanations considered in the literature.[14] We provide a simple model of microenterprise production to illustrate how these missing markets can give rise to marginal returns in excess of the market interest rate, and which can be used along with evidence of the heterogeneity of returns from our treatment to test whether missing credit or insurance markets is the more important factor driving high returns.

[14] See Banerjee and Duflo (2005) for an excellent recent review of different explanations. Missing credit and insurance markets appear the most important for our setting among the different theories they summarize.

Consider a simple one-period model in which the enterprise owner supplies labor inelastically to the business.[15] The household has an endowment of assets A, and allocates the number of other working age adults in the household, *n*, to the labor market, where they are paid a fixed wage *w*. The household can finance capital stock (*K)* through the formal credit market by borrowing (*B*), and through its internal household capital market, by allocating A_K of household assets and I_K of household labor income to financing capital stock.

The household's problem is then to choose the amount of capital stock, *K*, to invest in the business, subject to its budget and borrowing constraints:

Max EU(c)

{K, B, A_k, I_k}

Subject to:

$$c = \varepsilon f(K,\theta) - rK + r(A - A_K) + (nw - I_K) \quad (1)$$

$$K \le A_K + I_K + B \quad (2)$$

$$B \le \overline{B} \quad (3)$$

$$A_K \le A \quad (4)$$

$$I_K \le nw \quad (5)$$

Where ε is a random variable with positive support and mean one, reflecting the fact that production is risky, and *r* is the market interest rate. The production function of the firm, f(.) depends on the level of capital stock, and on θ, the ability of the entrepreneur.

With well-functioning credit and insurance markets, households will choose *K* to maximize expected profits and as a result, households choose *K* such that:

$$E[\varepsilon f'(K,\theta)] = r \quad (6)$$

Since E(ε)=1, this yields

$$f'(K,\theta) = r \quad (7)$$

That is, households will choose capital stock so that the marginal return to capital equals the market interest rate. In this case, the marginal return to capital will be the same for all firms, and will not depend on the characteristics of the owner or household.

The more general solution to the household's first-order condition for *K* is:

[15] This simple model is an adaptation of the agricultural household model set out in Bardhan and Udry (1999).

$$f'(K,\theta) = \frac{1}{1 + \frac{Cov(U'(c),\varepsilon)}{EU'(c)}} \left[r + \frac{\lambda}{EU'(c)} \right] \qquad (8)$$

where λ is the lagrange-multiplier on condition (2), and is a measure of how tightly overall credit constraints bind. We can consider two sub-cases:

a): *perfect insurance markets, missing credit market.* With perfect insurance, risk and uncertainty do not matter, and (5) reduces to $f'(K,\theta) = r + \lambda$. That is, the marginal return will exceed the market interest rate by the shadow cost of capital. Solving the first-order conditions for the optimal choices of *B*, I_K and A_K yields:

$$\lambda = \mu_B = \mu_A + r = \mu_I + 1 \qquad (9)$$

where μ_B, μ_A, and μ_I are the lagrange-multipliers on constraints (3), (4) and (5) respectively. Credit constraints will therefore be binding if and only if both the external (formal) and internal (household) credit markets are binding. Given the lack of access to bank finance seen in our firms, it therefore appears that the critical determinant of whether or not credit constraints bind will be the shadow cost of capital within the household.

In our model λ will then depend on the amount of internal capital available, which is increasing in household assets *A* and in the number of workers *n*. However, it will also depend on what the firms' unconstrained level of capital will be. If ability θ and capital are complements, then higher ability individuals will desire more capital, and so will be more likely to be constrained for a given level of assets and workers.

As a result, if credit constraints are the reason for high returns, we predict that the marginal return to capital will be higher for firms with greater ability, lower for households with more workers, and lower for households with more liquid household assets. We will test for this by examining whether the effect of our treatments varies with these factors.

b): *perfect credit markets, missing insurance market.*

An alternative explanation for the high marginal returns could be that credit markets function well, but that households are risk averse and insurance markets are missing, In this case equation (8) simplifies to:

$$f'(K,\theta)Cov(U'(c),\varepsilon) = [r - f'(K,\theta)]EU'(c) \qquad (10)$$

Since consumption increases with ε, Cov(U'(c), ε)<0. Since U'(c) <0 this implies that $r < f'(K,\theta)$. The size of the gap between the market interest rate and the marginal return to capital will be increasing in the level of risk in business profits, and in the level of risk aversion displayed by the household. We test this by interacting the treatment effect with measures of the risk aversion of the entrepreneur, and the perceived uncertainty they have in their profits.

8.2. Estimation of Heterogeneous Treatment Effects and Measurement of Factors Determining Heterogeneity.

The above theory shows that the pattern of heterogeneity of treatment effects can inform us about the reasons why returns are so high and exceed market interest rates. We allow for heterogeneity in treatment effects through estimation of variants of the following fixed effects regression:

$$net\ profits_{i,t} = \beta Amount_{i,t} + \sum_{s=1}^{S} \gamma_s Amount_i * X_{s,i}$$
$$+ \sum_{t=2}^{5} \phi_t \delta_t + \sum_{s=1}^{S} \left(\sum_{t=2}^{5} \phi_{s,t} \delta_t * X_{s,i} \right) + \alpha_i + \varepsilon_{i,t}$$

The parameter γ_s then shows how the effect of the treatment amount varies with characteristic *s*.[16] Since the evolution of profits over time may vary with $X_{s,i,t}$ even in the absence of treatment, we allow the wave effects δ_t to also differ with individual characteristics.

The theoretical model suggests that the heterogeneity of returns could vary with the number of workers in the household, household wealth, entrepreneurial ability, risk aversion, and uncertainty. We measure each of these factors as follows:

i) The *number of workers in the household* is the number of paid wage workers in the household in the pre-treatment baseline survey.

ii) *Household wealth* is difficult to measure accurately, especially since liquid wealth should be more important for funding the household budget than

[16] The upper limit of 100,000 rupees of capital stock may result in an interaction between ability and some of the characteristics we are interacting with the treatment. For example,an entrepreneur of a given ability level may stay within the sample criteria if there are no additional workers in the household, but grow beyond that limit if there are additional workers. The presence of additional workers would then be negatively correlated with entrepreneurial ability in the sample. This should be kept in mind when interpreting the coefficients on the interaction effects.

illiquid forms of wealth such as building quality. Following Filmer and Pritchett (2001) we proxy household wealth with the first principal component of a set of indicators of ownership of durable assets (such as fans, radios, cameras, televisions, bicycles, refrigerators and motorcycles). We again take this from the baseline survey, so that it is predetermined at the time of treatment.

iii) *Entrepreneurial ability* is a more nebulous concept, and so we explore several proxy measures. The first, and most standard in the literature (e.g. Paulson and Townsend 2004), is to use the number of years of education of the enterprise owner. Secondly, we follow Djankov et al. (2005) and use the score of the entrepreneur on a digit span recall test. Thirdly, we use the first principal component of years of education, digit span recall, time taken to solve a maze, and self-assessed efficacy in ability to carry out business tasks such as keeping accounts, making a sale to a new customer, and identifying market opportunities. This latter measure is a broad measure of entrepreneurial ability, and has a correlation of 0.64 with digit span and 0.79 with education.

iv) *Risk aversion* is measured from lottery experiments played with real money with each firm owner. Firm owners were given the choice between 40 rupees for certain, or a gamble with X percent chance of 10 rupees and 100-X percent chance of 100 rupees. A 10-sided dice was used to vary the odds of the higher payment from 10 percent up to 90 percent. The probability threshold at which an individual switches from the safe payment to the risky gamble then provides a measure of risk aversion, and can be used to derive the coefficient of relative risk aversion for a CRRA money utility function, which we take as our measure of risk aversion.

v) *Uncertainty* is measured by eliciting from firm owners their subjective distribution of firm profits (Manski, 2004; Lybbert et al, 2005). In the October 2005 survey, firm owners were given a sheet with different ranges of profits, such as <500 rupees, 500-1000 rupees, 1000-1500 rupees, etc. They were then asked to place 20 marks in cells, representing how many firms just like theirs they think would have profits in each range in December 2005. We use these subjective distributions to estimate the coefficient of variation of expected profits, which we take as our measure of the riskiness of firm profits as perceived by the firm owner.

8.3. Results on Treatment Effect Heterogeneity

Table 7 presents the results from estimating equation (11) allowing for different forms of heterogeneity in the treatment effects. We estimate the return from the treatment, because risk and credit constraints could affect the proportion of cash treatments which were invested in the enterprise. Column 1 presents the overall treatment effect, repeating row 3, column 4 of Table 2. Columns 2 through 6 examine whether returns are higher when credit constraints bind more tightly. About half of the households report having at least one paid wage worker We expect the shadow value of capital to be lower in households with wage workers, as the wages generate a source of funds for investing in the enterprise. We do find that having paid wage workers in the household is associated with lower returns from the treatment, an effect which is significant at the 5 percent level. We also tested whether having other enterprises owned by household members might be associated with higher returns, as these enterprises provide competing uses for funds. In results not reported on table 7, we find that the presence of other enterprises is associated with higher returns, but the effect is not statistically significant.

Column 4 shows a negative interaction effect with the household durable asset index, which is also consistent with a model of credit constraints. However, the coefficient is statistically insignificant. We note that households rely primarily on own savings rather than selling household assets when financing the business. Unfortunately we are not able to measure liquid savings of the household, preventing us from testing whether households with more liquid assets have lower returns.

The most significant interaction effects are found for our various measures of entrepreneurial ability. Columns 5, 6 and 7 show that the interaction of the treatment amount with years of schooling, a digit span recall test, and our index of broad entrepreneurial ability. Each of our measures of entrepreneurial ability is positive and significant, education and the digit span test at the 5 percent level, and the broad index at the 10 percent level. These results imply that the treatment has a larger effect on more able entrepreneurs. This is again consistent with credit constraints, since it implies that the return deviates further from market interest rates for more able entrepreneurs. The interaction predicts negative or zero return on the treatment for the 20 percent of firm owners with 6 or fewer years of schooling, 2.2 percent monthly returns at the 25th percentile of 8 years schooling, 5.0 percent monthly returns at the median education of 10 years schooling, and 6.4 percent monthly returns at the 75th percentile of 11 years schooling. The other measures of ability also give zero or slightly negative returns for the bottom of the ability distribution, and higher returns for the more able. For example, 15 percent of firm owners could only recall 4 digits or less: they have negative estimated returns. The median digit span recall is 6 digits, corresponding to an estimated return of 4.8 percent. At the top of the distribution, 11 percent of firm owners could recall 8 or more digits, resulting in predicted returns of 13.6 percent per month.

Column 8 shows no significant interaction of the treatment amount with risk aversion. Moreover, the sign of the coefficient is negative, suggesting that, if anything, firm owners which are more risk averse have lower returns. Column 9 finds a negative coefficient on uncertainty of firm profits, which is significant at the 10 percent level. This would suggest firm owners facing more uncertainty have lower returns. This is evidence against the high marginal returns being caused by risk averse entrepreneurs operating with missing insurance markets, as this would predict that both coefficients would be significantly positive.

Finally in Column 10 we consider the interactions together. The results are very similar to the regressions examining each effect individually. The interactions with the number of paid wage workers in the household and education are still positive and significant at the 5 percent level, confirming that the returns are highest for owners with more limited liquidity and more ability. Risk aversion is insignificant, and the measure of sales uncertainty has the unexpected sign and is significant at the 10 percent level.

Taken together, the heterogeneity of returns supports the view that the high marginal returns from our treatments reflect credit constraints rather than missing insurance markets. Credit constraints are more binding, and thus marginal returns are higher, for more able entrepreneurs and for entrepreneurs with a high shadow cost of capital within the household, measured by the presence of fewer other paid wage workers.

9. Conclusions:

We find returns to capital for the average microenterprise firm in our sample to be very high, around 5.7 percent per month, or at least 68 percent per year. Returns appear to be flat or decreasing – we do not find evidence of increasing returns over our sample range. Marginal returns are highest for entrepreneurs with more ability and with fewer other workers in the household. These are the individuals who, all else equal, we would expect to be most credit constrained. In contrast, returns are not found to differ with risk aversion of the entrepreneur, or with the perceived uncertainty of the entrepreneur about his or her profits. This suggests that missing credit markets rather than missing insurance markets is the main constraint on enterprise growth.

The results have important implications for the way we think about both microenterprises and microfinance markets. The high returns are very low levels of capital stock imply that non-convex production sets are unlikely to lead to permanent poverty traps. The high returns also suggest that there will be demand for loans from microlenders, even at interest rates which are high enough to cover the administrative costs of making small loans.

The fact that the returns exceed interest rates charged by microlenders in Sri Lanka by such a large margin also presents something of a puzzle. Sri Lanka has an active microfinance sector, with participation by both NGOs and government-run banks. Initial loan sizes are in the 10,000 rupee range, the same as our smaller treatment level. Do these enterprises lack credit because they have been screened out, in spite of the high returns? Or do they lack credit because they of a lack of demand, either because they are unaware of potential sources of credit or because they are unaware of the returns to incremental investments in the enterprises? These questions are left as motivation for future research.

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Table 1: Descriptive Statistics and Verification of Randomization

Baseline Characteristic	Total number of observations in R1.	Full sample Mean	Full sample Median	Means by Treatment: Assigned to Treatment	Means by Treatment: Assigned to Control	T-test p-value
Profits March 2005	388	3869	3000	3957	3748	0.539
Revenues March 2005	405	12250	7000	11936	12683	0.621
Total invested capital March 2005	405	147090	81500	156406	134213	0.330
Total invested capital excluding land and buildings March 2005	405	26652	18000	25963	27605	0.520
Own hours worked March 2005	405	52.7	50	52.0	53.7	0.440
Family hours worked March 2005	405	17.0	0	18.0	15.6	0.379
Age of entrepreneur	405	41.9	41	41.9	41.9	0.979
Age of firm in years	403	9.3	5	9.6	8.8	0.411
Proportion female	405	0.467		0.438	0.506	0.179
Years of schooling of entrepreneur	405	9.0	10	9.0	9.1	0.579
Proportion whose father was an entrepreneur	405	0.385		0.370	0.406	0.468
Proportion of firms which are Registered	405	0.225		0.243	0.200	0.312
Number of household members working in wage jobs	405	1.2	1	1.2	1.2	0.704
Household asset index	405	0.000	0.078	-0.178	0.246	0.018
Number of Digits recalled in Digit Span Recall test	375	5.8	6	5.8	5.8	0.684
Implied coefficient of relative risk aversion from lottery game	400	0.139	0.065	0.197	0.055	0.375
P-value from Chi-squared (16) test of joint significance in probit of treatment on these variables:						0.355

Table 2: Effect of Treatment on Profits

Impact of Treatment Amount on:	Random Effects (1)	Fixed Effects (2)	FE with 1% level trim (3)	FE with 1% change trim (4)
Nominal Profits	488.8**	568.7**	614.4***	593.2***
	(203)	(235)	(162)	(198)
Real Profits	469.9**	542.8**	585.7***	567.8***
	(198)	(231)	(158)	(193)
Real Profits less 5.43*own hours	407.0**	476.3**	519.3***	498.1***
	(196)	(230)	(157)	(193)
Real Profits less 14.1*own hours	307.6	370.1	413.4**	386.9*
	(197)	(233)	(162)	(197)
Number of firms	383	383	382	360
Number of firm*time observations	1857	1857	1834	1749

Notes: All regressions also include period effects
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 3: Return to Capital from Experiment
Dependent Variable: Real Profits less 5.43*own hours

	IV-RE (1)	IV-FE (2)	IV-FE with 1% level trim (3)	IV-FE with 1% change trim (4)	IV-RE (5)	IV-FE (6)	IV-FE with 1% level trim (7)	IV-FE with 1% change trim (8)
Capital stock	537.5**	621.5**	680.5***	686.3**	611.1**	670.7**	725.5***	738.8***
(excluding land & buildings)	(262)	(316)	(224)	(286)	(250)	(312)	(220)	(281)
1% trimming of initial capital	No	No	No	No	Yes	Yes	Yes	Yes
First-stage								
Coefficient on Treatment Amount	0.744	0.763	0.766	0.732	0.800	0.792	0.796	+0.762
F-statistic	41.99	46.10	45.83	45.56	48.86	50.41	50.27	50.69
Observations	1798	1798	1775	1691	1764	1764	1742	1657
Number of sheno	383	383	382	360	376	376	375	353

Notes: All regressions also include period effects
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 4: Testing for Treatment Spillovers
Dependent Variable: Real Profits less 5.43*own hours
Columns 1-4 all firms; Columns 5-8 exclude bamboo industry

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	FE	FE	IV-FE	IV-FE	FE	FE	IV-FE	IV-FE
Treatment Amount	504.6***	506.4***			385.1*	385.0*		
	(192)	(193)			(206)	(206)		
Capital stock			751.4***	751.8***			573.9**	574.7**
(excluding land & buildings)			(281)	(281)			(278)	(278)
Number of Firms in Industry Treated								
Within 500m	-150.9**		-138.0*		-99.23		-105.9	
	(65.3)		(70.9)		(138)		(146)	
Within 1km		-60.34		-58.54		-49.80		-59.90
		(43.2)		(47.1)		(82.0)		(87.4)
Constant	2744***	2744***	768.7	767.0	2719***	2719***	1089	1087
	(173)	(173)	(757)	(757)	(183)	(183)	(802)	(803)
Observations	1749	1749	1657	1657	1605	1605	1518	1518
Number of sheno	360	360	353	353	331	331	325	325

Notes: All regressions also include period effects
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 5: Treatment effect and Returns to Capital after excluding Bamboo industry

	(1)	(2)	(3)	(4)
	FE with	FE with	IV-FE with	IV-FE with
	1% level trim	1% change trim	1% level trim	1% change trim
Treatment Amount	408.9**	380.9*		
	(166)	(206)		
Capital stock			565.0***	565.6**
(excluding land & buildings)			(212)	(278)
Constant	2669***	2718***	1055*	1110
	(146)	(183)	(616)	(803)
First-stage				
Coefficient on Treatment Amount			0.842	0.805
F-statistic			47.20	46.92
Observations	1690	1605	1603	1518
Number of sheno	353	331	347	325

Notes: All regressions also include period effects
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
Columns 3 and 4 trim the top and bottom 1% of initial capital stocks.

Table 6: Cash Vs. Equipment and Linearity of Effects
Dependent Variable: Real Profits less 5.43*own hours, trimming at 1/99% on change in profits

	(1)	(2)	(3)	(4)
	FE	FE	FE	FE
Treatment Amount	380.9*			
	(206)			
Equipment Treatment Amount		369.6		
		(262)		
Cash Treatment Amount		392.6		
		(266)		
Treated Amount 10,000 Rs.			479.7	
			(364)	
Treated Amount 20,000 Rs.			720.7*	
			(437)	
Treatment Amount (0, 1, 2)				544.4*
				(312)
Treatment Amount * Initial Capital Stock				-58.4
				(77.4)
Constant	2718***	2717***	2714***	2718***
	(183)	(183)	(183)	(183)
F-test of equality of treatment effects p-value		0.944	0.627	
F-test p-value: 2*10,000 treatment = 20,000 treatment			0.751	
Firm-Period Observations	1605	1605	1606	1605
Number of Firms	331	331	331	331

Notes: All regressions also include period effects
Column 2 excludes firms ever receiving cash treatments, Column 3 excludes firms ever receiving equipment treatments. All other columns are for all firms.
Data were trimmed to remove firms with percentage change in profits less than -90%, or above +962%.
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 7: Treatment Effect Heterogeneity
Dependent Variable: Real Profits less 5.43*own hours

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	FE	FE	FE	FE	FE	FE	FE	FE	FE
Treatment Amount	380.9*	724.9***	404.2**	-879.5	-2148*	348.6*	368.3*	997.1*	252.1
	(206)	(272)	(215)	(656)	(987)	(212)	(212)	(553)	(882)
Interaction of Treatment Amount with:									
Number of Wage Workers		-522.3**							-579.0**
		(261)							(282)
Household Asset Index			-107.8						-87.6
			(134)						(148)
Years of Education				137.9**					157.0**
				(68.3)					(71.3)
Digitspan Recall					438.2**				
					(167)				
Broad Entrepreneurial Ability						353.8*			
						(187)			
Risk Aversion							-12.64		4.2
							(130)		(131)
Uncertainty								-1191.7*	-1703*
								(878)	(911)
Constant	2717***	2718***	2718***	2714***	2708***	2702***	2721***	2734***	2733***
	(183)	(183)	(183)	(183)	(186)	(189)	(184)	(184)	(184)
Firm-period observations	1605	1605	1605	1605	1580	1549	1605	1587	1587
Number of firms	331	331	331	331	324	316	331	327	327

Notes: All regressions include period effects, the interaction of period effects with the variables being interacted with treatment amount, and firm fixed effects.
Data were trimmed to remove firms with percentage change in profits less than -90%, or above +962%.
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

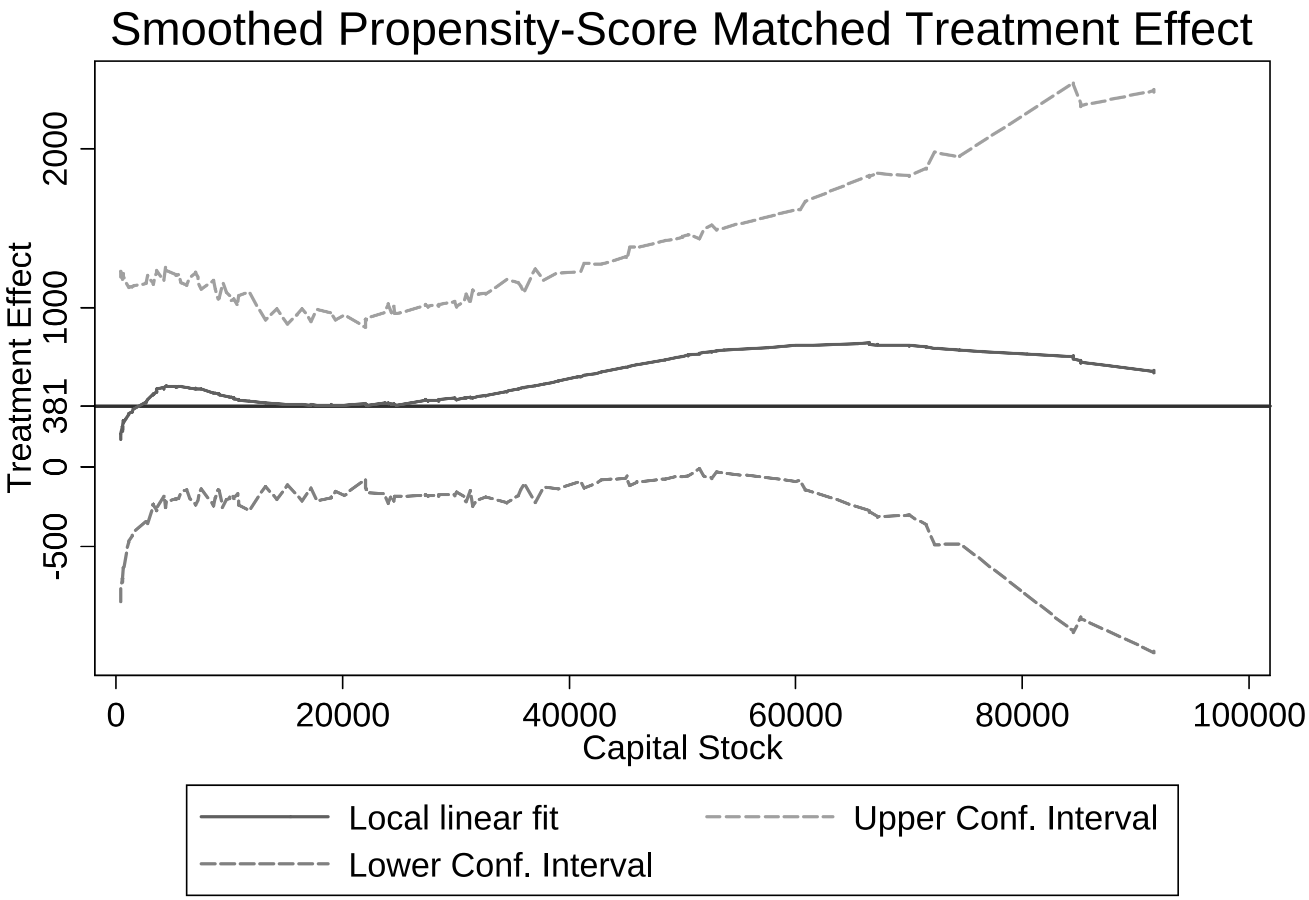

Figure 1: Returns to Treatment Estimated Using Four Nearest Neighbors