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Preliminary

What Generates Growth in Microenterprises? Experimental Evidence on Capital, Labor and Training

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Abstract:

Previous research shows that capital injections lead to higher profits in microenterprises, but to little sustained growth. We conduct an experiment which provides overlapping treatments designed to provide capital, incentives to hire new employees and management training. Working with a sample of 1,525 Sri Lankan enterprises with two or fewer paid employees at baseline, we find that the treatments have largely temporary effects, suggesting that while they may speed convergence to a steady state, they do not appear to put firms on a different growth path. Wage incentives lead to higher levels of employment, but not to higher profits, suggesting that the typical firm does not face constraints to hiring which result in the marginal product of labor exceeding the market wage rate. We use data from surveys of wage workers and SME owners conducted at the same time as the baseline survey to estimate characteristics associated with entrepreneurial ability. We find that high-ability firms, if anything, benefit less from the treatments. The results are consistent with the view of the world illuminated by Lucas' 1978 model of firm size distribution.

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Recent experiments with microenterprises suggest that positive liquidity shocks translate into increases in enterprise profits which are sustained over the longer term (de Mel et al 2008, 2012; Field et al 2012). But there is little evidence that liquidity alone allows microenterprises to reach a different growth path which ultimately leads to an increase in scale such that additional employees are needed to run the business. In other words, capital leads to higher incomes, but not to employment generation.

The interest in employment generation from microenterprises comes from the vast number of self employed in low-income countries. If only a modest percentage of these firms were to scale up enough to hire a few employees, this would result in significant job creation. But is it possible to generate growth from microenterprises which leads to significant job creation, or are microenterprise owners run exclusively by 'types' of entrepreneurs who do not have interest in or the ability to scale up? De Mel et al (2012) present evidence that a small percentage of firms may be released to a higher growth path by formal registration. But they start with a sample of firms with two or more paid employees. The vast majority of the self employed in developing countries have no paid employees.

In this paper, we work with a sample of 1525 microenterprises which are mostly non-employers, and which hire at most two paid employees. In a randomized experiment, we relaxed three constraints: capital, labor and entrepreneurial skills. We used a matched savings program, wage subsidies to incentivize hiring additional employees, and entrepreneurship training based on the ILO's Improve Your Business (IYB) program – the most widely implemented entrepreneurship program in the world. The savings program required that funds accumulate for 9 months before being made available, leading to a lump of capital which became available just after owners completed training or learned that they were eligible to receive wage subsidies.

Enterprises in the sample received either zero, one or two of the interventions. The first group serves as a control group while those eligible to receive two interventions allow us to examine interaction effects. The three overlapping interventions give us instruments for each of the three primary factors in the production function of the enterprise.

The baseline survey for the project was conducted in April and October 2008. The saving incentive program began in November 2008, with funds made available in August 2009. Training sessions were held between May and July 2009, and owners

were told in July 2009 they were eligible for wage subsidies were they to hire an additional worker. We conducted a follow-up survey in April 2009 – while the savings program was ongoing but before the other two interventions had been carried out. We then conducted a further six follow up surveys at six month intervals in October 2009, 2010, and 2011 and in April 2010, 2011, and 2012. The multiple follow-ups are important not only because many of the outcomes of interest are very noisy (McKenzie 2011), but because they allow us to gain some sense of the timeline of effects.

We find that the strongest effects come from the savings incentive program. Wage incentives result more use of hired labor. The effect is largest while the subsidies are in place, but remains significant for two years after they are removed. On average, firms provided the incentives employ 0.1 to 0.2 additional workers one to two years after the incentives are removed. Training has effects which appear at all only 18 months or so after training, and are then marginally significant. While we still view the results as preliminary, the data consistently show that the marginal product of labor is very low. Neither profits nor household income increase significantly for any of the treatments, but the profits estimates are generally negative for the wage incentive treatment, and household income is significantly so in the last survey round. When we allow for treatment interactions, we find those to be almost always negative. In particular, providing capital alone appears to have positive effects on the scale of the enterprise by a number of measures; providing capital along with either training or wage incentives reduces the magnitude and significance of that effect. At least for the average firm in our sample, the challenge seems to be getting capital into the enterprises.

We proceed as follow: Section 2 describes the data and the experiment and section 3 presents the basic results. Section 4 then explores heterogeneity of outcomes using measures of ability and attitudes collected at baseline, and section 5 concludes.

Section 2: The sample and the experiment

We aimed to select a random sample of enterprises with two or fewer paid employees, owned by males aged 20 to 45 and operating in non-agricultural sectors. We chose to focus exclusively on male-owned enterprises because previous work showed that capital alone had a much larger effect on male-owned businesses (de Mel et al 2008, 2009). A separate project considers the effect of training and grants on a

sample of women (de Mel et al 2012). We work with a random sample because we want to understand the impact of the various constraints on the full spectrum of firms in the population, in order to provide a benchmark. Going forward, we believe that selecting on ability or aspirations may be important, and we use our data to explore the heterogeneity of outcomes based on ability and attitudes measured at baseline.

About half of our sample for this project comes from a larger panel survey which is representative of all urban areas in Sri Lanka outside the northern province. From this panel survey, we selected 717 male self employed workers with 2 or fewer paid employees in urban areas in Sri Lanka: Colombo, Kandy and the Galle-Matara area. This part of the sample was constructed through a listing exercise conducted in early 2008. We selected a total of 18 Division Secretariat (D.S.) Divisions in the three urban areas. Within each D.S. Division we then selected 10 (in Colombo and Kandy) or 5 (in Galle/ Matara) Grama Niladhara (GN) divisions and listed 50 households starting from a random point.[1] The listing collected information on each adult active in the labor force in each household. Because we needed a larger sample for the interventions, in October 2008 we selected a set of GNs neighboring those in the original panel survey. We used a similar screening survey to identify male self employed workers with fewer than 2 paid employees, boosting the sample by 808 individuals. Because of the way they are constructed, both subsamples are representative of the areas from which they are taken. However, there are some differences in the manner of constructing them, so we add a control for the enterprises in the booster sample in each of the regressions.[2]

After the baseline survey was conducted with those in the booster sample, we randomized the full sample into a control group or one of six treatment groups: savings incentives, wage subsidies, or training only, and the three combinations of two of these interventions.[3] Enterprises were stratified into six groups using

[1] The G.N. Division is the smallest of the four administrative levels in Sri Lanka: Provinces (9), Districts (25), Divisional Secretariat (DS) Divisions (324), and Grama Niladhari (GN) Divisions (14,008).

[2] We find no differences in the operating characteristics of the enterprises (sales, profits, etc.) but the owners in the original sample have about a half year less schooling and have been in business for about three-quarters of a year longer.

[3] The enterprises originally sampled in April 2008 had been surveyed twice when we conducted the randomization, while those added in the 'booster' sample in October 2008 had been surveyed only once. Before placing the enterprises into strata, we dropped all enterprises without profit or sales data in the October 2008 survey. We also dropped those from the April 2008 sample which had closed their business by October 2008. This produces a slightly different selection criteria in

geographic region – location in Colombo, Kandy and Galle/Matara – and whether they operated in the retail sector or were engaged in manufacturing / services. Within each of the six strata, we then randomly allocated enterprises to one of the six treatment groups or to the control group.

The number of enterprises assigned to each treatment cell is shown on Table 1. We decided to place more observations in treatment groups where we ex ante believed take-up would be lower, selecting a control group of 287, a savings incentive treatment group of 559, a business training treatment group of 589, and a wage incentive treatment group – which we expected to have lowest take-up – of 845.[4] We achieved these treatment / control group targets within each stratum by randomly assigning 18.6% (287/1535) to the control group, 7.3% to get only the savings program, 9.2% to get only the training program, 16.3% to get only the wage subsidy program, 9.8% to get the savings program and the training program, 19.3% to get the savings program and wage subsidy program, and 19.3% to get the training program and wage subsidy program.

Given this process, it was not possible to stratify further within our 6 basic geography*industry strata. In order to improve balance further on a set of key variables likely to be related to business outcomes we therefore employed a re-randomization procedure. We re-randomized 1000 times and in each randomization conducted an F-test for equality of means across the seven treatment groups for a set of 13 baseline variables listed in Table 2, including profits, management practices, employment, and assets. One potential pitfall for this approach can arise from outliers, so we also included dummy variables for profits and assets in the top or bottom 5 percent to reduce the possibility that balance on means was disguising large outliers. We then took the maximum F-statistic across these 13 variables, and then choose the random assignment from among the 1000 allocations that had the minimum maximum F-statistic. Table 2 shows that we achieved balance at baseline on a set of important variables. In all reported regressions, we control for the baseline measures

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the original April 2008 sample and the booster sample. We include a variable indicating the enterprise entered in the booster sample in all of the regressions.

[4] The unequal treatment reduces power slightly with regard to detecting differences between the control group and the various treatment groups, but by giving us more observations who take up the wage subsidy treatment, provides greater accuracy for the wage subsidy take-up regressions – early results on the wage subsidy take-up are provided in de Mel et al. (2010).

of these variables and for the full set of strata dummies, which Bruhn and McKenzie (2009) show gives the correct size and power after re-randomizing.

The treatments:

In November 2008 we notified those assigned to the savings treatment that they had been selected to participate in a program designed to encourage them to build savings balances. The participants were not told about the other two interventions in November even if they had been assigned to one of the other two treatments. As a part of the savings incentive program, we offered to make the initial deposit in a savings account at the National Savings Bank (NSB) and then to match deposits made into that account up to a certain limit each month and at a pre-announced match rate. The account would remain 'locked' until 1 August 2009. The initial match rate was set at 50 percent for deposits of up to 1000 Sri Lankan Rupees (LKR)[5] made by the end of December. The match rate was kept at 50 percent through July, but the maximum amount we would match was increased to 2000 LKR in January and to 4000 LKR in May, 2009. In July, we raised the match rate to 100% and the maximum to 5000 LKR. Finally, just before the accounts were unlocked, we added 5000 LKR to every account, regardless of previous deposit patterns. The participants received regular passbooks for the accounts, and deposits could be made at any NSB branch. But the accounts were all opened through a single branch in Gampola so that the branch manager there was able to ensure that money was withdrawn before 1 August only if the participant faced an emergency situation. After the accounts were unlocked on 1 August, the participants were free to move the accounts to any NSB branch, or to withdraw the money. At that point, we lost access the administrative data, and hence are unable to track when money was withdrawn.

A second treatment was a training program based on the International Labor Organization's *Improve Your Business* (IYB) program. IYB is a five day program intended to generate growth in microenterprises. The modules covered are marketing, buying, costing, stock control, record keeping, and financial planning. We asked that the training also include additional material on hiring and managing employees, as employment generation is a key outcome of interest in the project. The training was

[5] 1000 LKR was approximately US$8.75 in mid-2009, $8.85 in mid-2010, $9.14 in mid-2011, and $7.49 in mid-2012.

provided by the Sri Lankan Business Development Centre (SLBDC),[6] a Sri Lankan non-profit training institution established in 1984. SLBDC is the most experienced provider of ILO entrepreneurship programs in Sri Lanka, having offered the first training on the island in 2001. All of the SLBDC training staff involved in the project were university qualified and trained under the national-level SIYB training programs conducted by the ILO. Each had a minimum of five years experience delivering SIYB training. Therefore, any failure to find impacts should not be due to low quality trainers or inexperience with the materials. Those selected for training were offered a stipend of 1000 LKR and an additional bonus of 1500 LKR paid at the end if they attended all five days. The stipend was meant to cover transport and the opportunity cost of not working in the business on the training days.

The final treatment provided a temporary wage subsidy to firms with the purpose of encouraging owners to hire an additional full time employee. The April 2009 survey – taken before anyone was made aware of the wage incentive program – asked for information about each employee currently working at the enterprise. In early July, we notified those assigned to the wage incentive treatment that we would pay a flat amount of 4000 LKR per month for a period of six months if they hired an additional employee working at least 30 hours per week, and a flat amount of 2000 LKR per month for a further two months. The employee had to be someone living outside owner's household and could not be an immediate family member (spouse, parents, siblings, and children). Participants were told that payments would start in August and end in May 2010 regardless of when the worker was hired. In other words, workers had to be hired by 1 October for the full amount of the subsidy to be paid. The subsidy represents about half of the earnings of a typical unskilled worker.

Once we were notified by the participant that a worker had been hired, we sent a research assistant to conduct an interview with the new employee. We also conducted a short interview with the owner focused on the search and hiring process. Research assistants made occasional unannounced visits to the enterprise to make sure the employee was working. In a few cases, the research assistants were unable to confirm that the employee was in fact working full time. In all such cases, within a few visits the owner notified us that the employee was no longer working, and we removed the subsidy. We believe these spot checks were sufficient to root out any

[6] http://www.slbdc-lk.org/

'phantom' employees, though it is possible that a few deceptive owners avoided our screens.

Follow-up surveys

We conducted follow-up surveys at six month intervals in April and October, with operational data referring to March and September. The first follow-up was carried out in April 2009, during the accumulation portion of the savings incentive program and before enterprises had been notified of the training or wage incentive programs. The October 2009 and April 2010 surveys were conducted after the accumulated savings had been released and after the training was completed, but while the wage incentives were in force. We have conducted five follow-up surveys after all the treatments were completed, in October 2010, 2011, and 2012 and in April 2011 and 2012. These final five surveys are of particular interest because they allow us to examine effects that are fully post-treatment. We discuss attrition along with the results in the following section.

Section 3: A framework for understanding the effects of the treatments

Standard model with no capital or labor market constraints

The expected effect of the treatments will depend on the nature of constraints faced by firms. We explore the nature of the interaction between constraints and predicted effects in this section. We begin with the standard complete markets model of firm size of Lucas (1978), where differences in employment size among firms facing the same output production technology f(.) reflect differences in their management ability, θ. A firm facing a wage rate for workers *w*, and an interest rate on capital *r*, will choose capital, *K* and labor, *L* to maximize profits f(θ,K,L) – *w*L – *r*K. This yields the familiar first-order conditions in which the optimal levels of capital (K^*) and of labor (L^*) are chosen such that marginal products of labor and capital are equal to the wage rate and interest rate respectively[7]:

$f_L(\theta, K^*, L^*) = w$

$f_K(\theta, K^*, L^*) = r$

[7] For simplicity of exposition we assume the owner's own labor supply is inelastic here, but in our empirical work will also examine the labor supply response of the owner to our interventions.

If managerial ability is a complement, rather than a substitute for capital and labor, then in this model firms with zero workers are those with low managerial ability. Consider then our three interventions under this model. The matched savings program acts to increase the wealth or assets of the business owner. But given perfectly functioning credit markets, wealth does not affect the underlying production decision. Therefore, the savings incentive treatment should have no impact on employment, output or profits. The wage subsidy program temporarily lowers *w*, which leads to an increase in the employment of labor, output, and profits for the period of time the subsidy is in place. More capital may also be used if the capital and labor complement one another. However, once the subsidy ends, *w* returns to its previous level, and – so long as θ is unchanged by the intervention – output, profits, and employment return to their pre-subsidy levels. It is possible that the experience of supervising a worker makes a firm owner better at managing workers, which could increase θ. In that case employment, profits, and output may be higher than the pre-intervention level; but they still be lower than the level with the subsidy. Business training aims to increase managerial ability. If this management ability is a complement to labor and capital, then the result should be higher employment, profits and output, even in the long run. If instead better management techniques substitute for either capital or labor, then we should see profits increasing, but the overall impact on output and on employment will be uncertain. Scale effects will tend to increase the use of labor and capital, while substitution effects will tend to reduce them.

Capital or Labor Market Constraints

Now consider credit market constraints which limit the ability of firm owners to borrow to finance capital investments. Let *A* be the wealth of the business owner. This wealth can be leveraged in financial markets by some amount (b-1), with b≥1 being a measure of borrowing constraints. The capital constraint is then K≤b*A*. Then the new equilibrium levels of capital, K^{**}, and L^{**} solve:

$$f_L(\theta, K^{**}, L^{**}) = w$$
$$f_K(\theta, K^{**}, L^{**}) = r + \lambda b$$

Where λ is the Lagrange-multiplier on the borrowing constraint. In this set-up, equilibrium output and equilibrium capital are lower than in the no constraint case ($K^{**}<K^{*}$), but L^{**} may be greater than or lower than L^{*} depending on the shape of the production function: firms may substitute capital for labor and end up with more

employment than in unconstrained states, or they may find labor less productive without complementary capital and so hire less labor than in unconstrained states. However, in the absence of the intervention, credit-constrained firm owners should be able to reinvest earnings to build up capital over time, and so the long-run steady-state level of capital should still be K^*, with labor level L^*. In the presence of credit constraints, the savings treatment relaxes borrowing constraints, and so should increase output, capital stock, and profits in the short-run, with an uncertain impact on employment for the reasons mentioned. But there should be no long-run impact of the savings treatment, since the savings intervention should merely speed up convergence to the steady state level of capital K^* (Fafchamps et al, 2011). The business training and wage subsidy treatments should have somewhat similar impacts with credit constraints as in the no constraint case, except that the presence of credit constraints may limit the ability of the firm owner to adjust capital upwards to respond to more workers or better management.[8]

A second form of constraint may arise from the presence of either formal or informal minimum wages. Workers may be less productive in their first few months on the job while they learn the specifics of the job, with productivity increasing over time through on-the-job training. In the standard model above, the firm would pay a new worker his or her marginal product, so would pay a low (perhaps even zero or negative) wage at the beginning, and then a higher wage once productivity increases. However, poverty constraints, minimum wage laws, and social norms may limit the ability of workers to take low initial wages to compensate for their low initial productivity. This imposes the constraint $w \geq m$ on the optimization problem, where m is this lower bound on the wages that can be paid. Alternatively, firms may face fixed costs S of hiring a new worker, such as the costs of screening, interviewing, and testing the quality of the worker, but be constrained from being able to recoup this cost through paying workers less than their marginal product afterwards. Either of these cases can be modeled simply as increasing the effective wage that must be paid to the worker from w to $w+s$. The optimal choice of capital and labor then solves:

$$f_L(\theta, K^{***}, L^{***}) = w + s$$

[8] The business training also helps firm owners become more effective at accessing credit, for example if the training induces them to get their records in order and teaches them other steps needed to access bank credit. If this occurs, then training may have an additional positive impact through access to capital.

$$f_K(\theta, K^{***}, L^{***}) = r$$

Where $L^{***} < L^{*}$ and capital stock may be higher or lower than in the unconstrained equilibrium depending on whether firm owners use more capital to compensate for fewer workers, or find capital less productive when workers cannot be hired to use it.

In this setting, consider the wage subsidy intervention. The short-term subsidy can subsidize firms for the low productivity of workers during this training period, and for the fixed costs of hiring workers. If the productivity of workers increases during the period wages are subsidized (Bell et al, 1999), then they may be sufficiently productive after the subsidies end that firms are willing to pay them wage $w \geq m$ and keep them employed.[9] This yields the prediction that the short-term wage subsidy should have long-term impacts on employment and firm output and – because labor may complement or substitute for capital – an impact of uncertain direction on capital. However, since the subsidy induces hiring of labor at the margin where wages are equal to the value of marginal product, profits need not increase – the new workers may produce just enough extra to cover their wages. The savings treatment should have no impact if this labor market constraint is the only constraint, while the business training treatment should have similar impacts to the no constraint condition, though there may be less ability to adjust labor in response to the new managerial ability level.

Other constraints and theories

The savings treatment is predicted to have at most a short-term impact, speeding up convergence to the steady state equilibrium in either of the two models above. Fafchamps et al. (2011) show that this prediction that a grant has at most a temporary impact also holds with time-inconsistent preferences, with just the steady-state level of capital differing in this model. In contrast, a one-time grant can change the steady-state level of capital if there are multiple equilibria arising from production non-convexities coupled with credit constraints, or if it provides a way to overcome self-control problems. The fact that the savings treatment was offered through a new commitment savings technology might give the intervention a permanent effect among those with self control problems who otherwise would not have been able to

[9] Given enough friction in labor markets, firms may be able to recapture initial losses by paying wages below the marginal product of labor after workers become more productive. But movement of workers across firms may prevent this.

save up a lump of capital to invest in their business. In either the multiple equilibria or self control circumstances, the savings treatment could have long-term positive impacts on capital and output, with the impact on employment uncertain and depending on the degree of substitution between capital and labor.[10]

An alternative model could be that owners do not know their managerial ability, θ , especially with regard to their ability to manage labor, and only learn it when they hire a worker. Let θ^* be the managerial ability cutoff at which the unconstrained optimum is to hire a worker. Let $\tilde{\theta}$ be the belief a firm owner has about their own ability. If we consider a distribution of initial beliefs about own managerial ability, then all owners with initial beliefs $\tilde{\theta} \geq \theta^*$ will have tried hiring a worker before, and either found the worker to be productive and kept the worker, or not to have been productive and not have kept the worker. The pool of firm owners who have not previously hired a worker will then consist of owners with low actual managerial ability, as well as though with high actual ability but who believe they have low managerial ability. The wage subsidy then induces some of these owners to take on a worker while the subsidy is in effect. If this enables them to learn their ability type, then some of these firm owners will discover they were incorrect in their beliefs, and then keep the worker on after the subsidy ends. This is similar to the case where the wage subsidy actually increases θ , but here it just increases $\tilde{\theta}$.[11] In such a model the short-term wage subsidy could also have a long-term impact on employment and output.

Section 4: Results

Take-up of treatments

Participation in all three treatments was voluntary, and take-up rates were below 100 percent. The savings incentive program was offered to 559 individuals. The individuals were visited in person by project research assistants. The research assistants described the program, left an information sheet, and invited the participants to attend a meeting to discuss the program. At the meeting, one of us (de

[10] Because we don't know how long it takes to reach the steady state, if we find effects of the savings program after several years, we face a challenge of differentiating between these cases implying long-run effects and the more rapid movement to the steady state.

[11] A parallel argument can be made with respect to learning about demand for either new products of expanded production of existing products

Mel) and bank officials described the program and answered questions. The 162 participants who attended the meetings were able to complete the forms to open the required savings account during the meeting. In total, 455 individuals, or 81 percent of those offered the program, opened an account – 156 did so at the meeting, and the rest on their own at an NSB branch. For all of those opening an account, we paid the 500 LKR (about USD 4.50) initial minimum deposit required to open an account. Among the 104 individuals not taking up the program, the most common reasons given were a lack of interest in continuing with the survey (44, or 42 percent of those not taking up) or an inability of the project staff to explain the program to the individual (25, or 24 percent of those not taking up).

We matched deposits made by firms on nine separate occasions – on December 10, and then at the end of each month between December and July. Just over half (51 percent) of the firms made a deposit on at least one occasion, though only 6 percent of firms deposited each time and almost 15 percent deposited only once. Not surprisingly, more firms (190, or 42 percent of those who had opened an account) made a deposit at the end of July than at any other time. Indeed, the return on the deposits at the end of July was astronomical, since a deposit made on Friday was doubled and returned on Tuesday the following week. Given that more than half of the firms failed to make a deposit even suggests that perhaps some firms did not trust the scheme. However, 81 firms that failed to make the last deposit had previously deposited at least once.

A firm making the maximum deposits that would be matched would have had a balance of 41,750 LKR (plus a modest amount of interest) in August 2009.[12] The average balance was just over 14,500 LKR, implying a take-up rate of around 35 percent.[13] Note that even the deposit in December, matched at 50 percent, implied an annualized return of more than 75 percent. Participating once is positively correlated with the score on a Raven non-verbal reasoning test and the measure of management practices we describe below, but is not correlated with either measures of 1-month

[12] We matched 50 percent up to 500 LKR on 10 December, 1000 LKR end of December, 2000 LKR end of January through end of April, 4000 LKR end of May and end of June.

[13] This rate, however, includes the unconditioned deposits of 500 LKR to open the account and 5000 LKR at the end of the project for all of those opening an account. The maximum match required participants to make deposits of 22,500 LKR. On average, those opening account (all eligible participants) deposited just under 5000 LKR (4000 LKR). Calculated as the proportion of matchable deposits made by participants, the take-up rate is thus just under 18 percent.

discount rates nor with years of schooling or. The total balance at the time the accounts were released is significantly higher for those with better baseline management practices, a higher Raven score, and more schooling, and is negatively associated with short-term discount rates, though not significantly so. Participation did not vary across the three urban areas in the study. These results suggest that while trust in the product may have influenced the deposit decisions, there are also other reasonable correlates with deposit behavior.

Participants were notified of eligibility for the training program both by letter and an in-person follow-up visit. Of the 589 participants eligible for training, 368 (62 percent) attended at least one session and 341 (58 percent) attended at least four of the five days and received a certificate of completion. Completion of the training program was positively and significantly associated with the baseline management practices score and the age of the participant, and was negatively and significantly associated with the baseline sales level, baseline household wealth, and being in the retail sector. Participation was significantly higher in Colombo and Kandy than in the Galle / Matara area.

De Mel et al (2010) assesses the initial take-up of the wage incentive program. During the eight months the incentive program was active, 216 firms hired a worker under the program.[14] We find that baseline management practices and baseline sales are both strongly positively associated with hiring a worker. Owners in Colombo are less likely to hire a worker – though the effect is significant only at the .10 level – and households with higher wealth levels at baseline are also less likely to have hired a worker.

Outcomes: A,K and L

We begin by assessing whether any of the three treatments changed the inputs used by the enterprises. We expect training to have the most direct effect on management practices, savings incentives to have the most direct effect on capital stock, and wage subsidies to have the most direct effect on employment. We will then ask whether the interventions had an effect on enterprise outputs – profits and sales.

[14] This analysis is based on data obtained in the April 2009 survey. The actual number hired under the program is slightly higher because participants not respnding to that survey are not included.

We do this both by using the assignment to training directly in output regressions and by instrumenting for inputs using the assignment to treatment.

We begin by estimating the effect of being assigned to each of the three treatments, assuming there are no interaction effects in the treatments on business outcome for firm *i* in follow-up time period *t*. (We will later allow for interactions.) We estimate the following Ancova model separately for each of the post-treatment rounds:

$$Y_{it} = \alpha + \gamma_1 AssignedTraining_i + \gamma_2 AssignedSavings_i + \gamma_3 AssignedWageSubsidy_i + \theta Y_{i0} + \beta' X_i + \pi_{is} + \varepsilon_{it} \quad (1)$$

where π_{is} are randomization strata dummies, Y_{i0} is the baseline value of the outcome of interest, and X_i are the baseline values of the variables used in rebalancing. Ancova offers more power than either difference-in-differences or analysis using only the follow-up data, especially when looking at outcomes like microenterprise profits and sales which are not highly autocorrelated (McKenzie, 2011).

In addition, we can improve power further by pooling together several rounds of follow-up surveys and estimating the following regression:

$$Y_{it} = \sum_{t=1}^{q} \delta_t + \gamma_1 AssignedTraining_i + \gamma_2 AssignedSavings_i + \gamma_3 AssignedWageSubsidy_i + \theta Y_{i0} + \beta' X_i + \pi_{is} + \varepsilon_{it} \quad (2)$$

where δ_t are survey round dummies, q is the number of follow-up surveys, and the standard errors are clustered at the firm level. We estimate this pooled regression first by averaging over all post-treatment periods (survey rounds four through nine), and then secondly by pooling rounds six to ten. Round six is the first period after the wage subsidies were removed, and hence the latter sample shows longer term follow-ups from the treatments.

Estimation of equations (1) and (2) give the intention-to-treat (ITT) effect, which is the effect of being assigned to receive training, to receive the savings incentive program, or to receive the wage subsidies. Note that this specification does not allow for interactions in the training effects for firms that received two of the treatments. We examine those interactions later in the paper.

Table 3 shows the effect of the three treatments on management practices, inventories, fixed capital stock, and employment. For each dependent variable, the first column reports the results of an ANCOVA using all post-treatment rounds, the fourth through the tenth. The next seven columns show the results for each of rounds 4 through 10 separately. We aim to show the pattern of the treatment across time, though power is reduced when we use data from only a single round. The ninth column shows the results of an ANCOVA using rounds 6 to 10 – all treatment rounds after the wage subsidies were removed. Finally, the tenth column shows the results of a fixed effects regression using the same sample of rounds six to ten. Variables with a long right-hand tail – inventories, fixed capital stock and number of workers – are winsorized at the 1st and 99th percentile. For the inventories and fixed capital stock regressions, we also show (column 11) the results a hyperbolic sine transformation for the dependent variable.

We find that training has a modest but statistically significant positive effect on management practices. The effect is strongest in round 4, four months after the training sessions. It fades, but does not disappear into round 10. On average across all post treatment rounds, the management practices score increases by just under one point after training, or by about 10 percent of the mean for the control group. Perhaps not surprisingly, training is the only intervention with a consistent effect on management practices. Savings incentives never have a significant effect, and wage incentives have significant effects only in rounds 5 (positive) and 8 (negative).

Panels B and C of Table 3 shows the effect of the treatments on inventory investments (B) and fixed capital investments (C). Inventories are measured as a stock in each period, and data are deflated to April 2008 prices by the Colombo Consumer Price Index. Fixed capital stock excludes land and buildings and adjusts for both changes in market prices and inflation.[15] Only the savings incentive treatment has a significant effect on investment in inventories, with inventory levels increasing by an average of just over 13,000 LKR for the savings treatment group compared with the control groups across the post-treatment rounds. This treatment effect is comparable to the average final balance of about 14,500 LKR among those assigned to the savings

[15] Fixed capital stock in period t is measured as fixed capital stock in period t-1 increased by the rate of inflation, depreciated at an annual rate of 10 percent and adjusted for capital stock which was acquired or disposed of between survey rounds. The results are not sensitive to the particular depreciation rate or the assumption that market prices of used equipment change with the rate of inflation.

treatment. While the savings became available in August 2009, there is no significant effect on inventory investment until a full year later, in the October 2010 (round 6) survey. The effect is strongest in rounds 8 and 9, and disappears almost completely in round 10.

In contrast, the savings incentives have no significant effect on fixed capital investment. Instead, there is some indication that the training treatment increased fixed capital investment in rounds 9 and 10 of the survey. The effect is not strong enough to make the average effect of training on fixed capital investment significant across all post-treatment rounds. The wage subsidies have no effect on investment in either inventories or fixed capital stock. Neither of the other treatments has any effect on investment levels.

The final three panels of Table 3 show the effect of the programs on hired labor, the hours worked by the owner and the number of unpaid workers. The data for owner hours worked and number of paid employees are winsorized at 1 and 99 percent. For paid employees, this truncates the upper tail of the distribution at a level of five employees. The wage incentive program has a significant effect on the use of hired labor. Recall that the subsidies were in effect at the time of the round 4 and round 5 surveys. Not surprisingly, then, we find the largest effects during these two periods. The effect persists in rounds 7 and 8, but appears to be gone by round 10. On average in rounds 6-10, one to two years after the subsidies were stopped, we find an effect of just under one-tenth of an employee, around 15 percent of the mean of the control group. The estimated effect jumps around a bit across the individual rounds, but there is clearly a declining pattern. So while we might say the subsidies have a lasting effect, it is unclear whether they have a permanent effect. This is a case where the fixed effects regressions produce somewhat different results, with a measured effect of the subsidies almost twice as large and – though it is not shown on the table – a more consistent effect across the post-subsidy rounds – though even in the FE regressions, the effect diminishes substantially in round 10.

Perhaps more surprisingly, the savings incentive program also appears to have an effect on the use of hired labor. Indeed, the ANCOVA results produce level effects which are slightly (and not significantly) larger for the savings program. The coefficients on the training program are also positive, but are significant only in the fixed effects specification. We find very similar results for all three treatments using a 0/1 variable indicating the hiring of any employee or the total hours worked by paid

employees. But when we instead use the reported wage bill of the enterprise, we find that only the savings program is associated with a significant increase in the wage bill. The measured effect of the savings program indicates the wage bill increases by just over 1500 LKR – about one quarter of an unskilled salary – while the measured effects for the other two treatments are 600 LKR or less.

Neither the hours worked by the owner nor employment of unpaid workers was targeted directly by the incentives. But either might have been affected by other changes induced by the treatments. With regard to the hours worked by the owner and unpaid employees, none of the treatments have any significant effect on either of these two outcomes. Coefficients of – 0.3 to -0.7 hours are estimated precisely enough that we can rule out an increase of more than 1.5 hours per week in the number of hours worked by the owner. Similarly, the measured effect on the number of unpaid workers of -.02 to .03 allow us to rule out an increase of as much as 0.10 unpaid workers for any of the three treatments.

In sum, the treatments have the expected effects of changing a single element of the production function except for the savings program, which appears to increase the use of both capital and labor. We next ask whether the increases in inputs in the production function led to significant increases in outputs. We do this by examining the direct effect of the treatments on measures of enterprise outputs – revenues, profits and total household income.

Outcomes: Sales and profits

We find evidence of increases in the use of paid labor and capital following some treatments in at least some survey periods. Do the treatments also cause an increase in the output of the firms? We begin by considering the effect on sales and profits. We then discuss the possibility that the treatments may have led to changes in income in activities other than the business which is the focus of the survey. All of the sales, profits and other income data are deflated to April 2008 levels using the Colombo CPI.

Panel A of Table 4 shows the effect of the treatments on sales. The savings program has large and statistically significant effects on sales. The estimates by round are noisy, but the effect is statistically significant at least at the .10 level in rounds 5, 7, and 8, and when we combine the data from rounds six through nine. The savings treatment raises sales in rounds 6-10 by about 13 percent of the mean of the control

group and by an amount which is slightly smaller than the increase in inventories. For the full sample, the ratio of inventories to sales is about 0.7 to 1. Our estimates of the effects of the savings program indicate a marginal ratio of around 1.3. This may suggest that the incremental inventory investments have lower turnover, though the standard errors are large enough that we cannot rule out an inventory-sales ratio similar to the sample average. There are no significant effects on sales from the other treatments, though training shows consistently positive measured effects beginning in round 6, and a significant effect over rounds 6-10 in the fixed effects specification. Table 4 also shows the specification using the hyperbolic sine transformation, which indicates that none of the treatments has a significant impact on sales.

On average in the cross section, profits are around 25 percent of sales in most rounds. If marginal rates were comparable to average rates, then we should see a large increase in profits for those in the savings incentive group. In fact, the results in Panel B indicate that the change in profits is much smaller than the average profit-to-sales ratio would predict and is never significant. None of the three treatments significantly affect profits in any round or in any of the specifications. For the combined post-treatment rounds, the largest measured change in profits in the ANCOVA specification is for training, where it is only five percent of the change in sales. For training, the 10,143 LKR increase in sales implies an increase in profits of around 2500 LKR at average profit / sales ratios. We are able to rule out an increase in profits of this level at well below the .01 level.

The results on the sales and profits may be affected by differential attrition between the treatment and control groups. Attrition on outcomes related to the business (e.g., sales, profits, etc.) has three sources: exit from the survey, exit from self employment even for those remaining in the survey, and failure to report profits for those remaining in the survey and self employed. We find almost no difference between the control group and the treatment groups in the percentage of participants who are not surveyed. Across rounds 6 through 10, and average of 13.4 percent of the control sample and an average of 13.0 percent of the treatment samples are not surveyed in any given round. Some participants exit and then re-enter the sample; 94.7 percent of the control group and 93.3 percent of the treatment group are surveyed at least once in the post-treatment rounds, a difference which is not statistically significant ($p=0.36$). However, the treated firms are less likely to be surveyed but not self-employed. Among the controls, the average percentage of participants in this

state is 6.5 percent, compared with 4.0 percent of the treated sample. In rounds 10, the gap in those remaining in the survey but exiting self employment is 8.8 percent (controls) vs. 4.8 percent (treatment, p=0.01). There is also a slightly higher likelihood that a self-employed survey respondent in the control group will fail to report sales (3.5 percent over rounds 6-10, vs. 2.5 percent for the treatment groups.)[16]

Because the major cause of differences in attrition rates is exit from self employment, total household income and total personal labor income are much more balanced with respect to attrition. The average percentage of the original control sample not reporting personal labor income across rounds 6 through 10 is 17.1 percent, compared with an average of 15.6 percent in the treatment sample.[17] Hence, Panel C of Table 4 reports the treatment effects on total personal labor income, which combines the earnings from the enterprises with wage income earned by the respondents. A second advantage of total personal income as an outcome is that an intervention like training may have the effect of convincing some enterprise owners that they would be better off in wage work. However, Panel C shows no positive effects of any of the treatments on personal income. The effect is not significant in any treatment in any round. A largely similar result holds if we consider total household income (Panel D), though in this case we actually find negative and significant effects in round 9 using ANCOVA and in rounds 6-10 in the fixed effects specification.

Allowing for interactions

The regressions in Tables 3 and 4 use assignment to each training program as the independent variables. However, someone individuals were assigned to two treatments. A key feature of the project design is that we are able to ask whether the treatments are complementary to one another. There are reasons to think we might find complementarities. For example, the wage subsidy encourages a firm to hire an additional worker, but capital investments may be required to realize the full productivity of that worker. In Table 5, we include a set of dummies indicating assignment to multiple programs. We present results for four independent variables –

[16] Even when attrition rates are the same, there may be selection on attrition because the treatments may cause different types of individuals to attrit. We explore the heterogeneity of attrition in the appendix.

[17] Participants in the controls sample respond an average of 4.17 of the five rounds 6 through 10, compared with 4.22 rounds in the treatment group (p=0.63).

the number of paid workers, inventory investments, sales and profits. For each dependent variable, the table shows both the individual regression coefficients and the effect of joint treatments, which is the sum of the coefficients for the individual treatments and the interaction effects. We use rounds 6 through 10 in all of the regressions.

All of the interaction effects are negative except for those in the paid worker regression, though only a few are significantly negative. The data indicate that no single program is associated with a significant increase in employment. However, the combination of the wage subsidy program and either savings incentives or training does result in significant employment generation. The combination of savings incentives and wage subsidies increases employment by about a 0.23 workers, just under half the control group mean; the savings and training combination increases employment by about 0.16 workers. Both combinations involving the savings incentives also increase inventory investments significantly, while training combined with either of the other two increases sales. However, although the combinations of treatments appear to produce higher employment, investment and sales, there is no evidence that they produce higher profits.

The closest any of the estimates for profits comes to standard levels of significance is the savings incentive treatment by itself, though this still falls below the .10 threshold. Since the interaction effects are all negative in the investment and sales regressions, savings incentives alone also produce the highest levels of impacts for those variables. In sum, while individually and combined the treatments produce increases in business scale measured by employment, investment and sales, they do not appear to lead to higher rates of profits, at least within a two year window following treatment.

Comparing the results and the theories

We find short-run effects from some of the treatments, but no effects that appear to be sustained into the final survey round. What do the results suggest about the nature of credit and labor market constraints? The savings treatment leads to a short-run increase in inventory investment, employment of paid labor and sales. That all of these effects disappear by the last survey round suggests that liquidity may help speed convergence to the steady state, but does not change the longer-term steady state enterprise size. Notice that over the period covered by the study, the control

firms are growing quite robustly. The mean level of real sales, for example, grows by 49 percent between October 2009 and October 2012, and annual rate of 14 percent. The wage treatment has its largest effect while the subsidy is in place, and the effect is immediately cut in half when the subsidy is removed. This suggests that about half of the response is related to the reduction in the marginal cost of labor for these firms. The fact that the rest of the effect appears to have gone away by the last round suggests either that the wage incentives also merely sped up convergence to the steady state, or perhaps that social norms meant that firms took some time to release the excess workers after the subsidies ended. Training is actually the treatment that comes closest to showing signs of a longer term effect. There is a significant increase in fixed capital in the last two survey rounds, and sales show an increase in levels even in the later survey rounds – though the effect is only ever significant in the fixed effects specification.

Section 5: Heterogeneity and growth

The fact that the treatment effects appear to have short-term, but not long-term effects suggests that the firms in our sample are small in the steady state. In this section, we explore heterogeneity of the treatment effects along two dimensions. First, we ask simply whether the treatment had different effects for owners who were more constrained at baseline. We describe how we measure the baseline constraint level below. Second, we follow de Mel et al (2010a) and use separate samples of wage workers and larger firm owners to divide the sample used here into those more like wage workers and those more like SME owners. In effect, we are using the data on wage workers and SMEs to find characteristics associated with θ in the model. We would interpret ability as a combination of human capital and attitudes and aspirations which may affect the desire or ability to grow. We then examine whether the "high θ" owners grow faster even in the control group, and whether the treatments have differential effects on high- and low-θ owners. Not that all of the results in this section rely on division of the sample along lines we did not stratify on, and so should be interpreted as exploratory rather than definitive.[18]

[18] We did stratify the sample by whether the firm operates in the retail sector or in manufacturing / other services. We report results in the two sectors separately in the appendix. We find similar results across sectors for the wage subsidy treatment, but much

We begin by splitting the sample according to a single characteristic along which we expect the effect of treatment might vary. For savings, we use the baseline level of household wealth, measured as ownership of a series of household durable goods. Asset ownership is the best proxy available for credit constraints. For the wage incentive program, we divide the sample by whether the owner has previous experience with paid employees. From surveys we know whether owner has a current employee, whether he has hired someone in the past year, and whether he had someone at the time of start-up. Just over one-quarter of the sample has experience with a paid wage worker. Finally, for training, we split the sample at the median baseline management practices scores. The median baseline management practices score is seven, and we group those scoring seven or below and those scoring above seven.

We estimate regressions of the following form:

$$Y_{it} = \sum_{a=1}^{A} \gamma_a AssignedTreatment_i + \sum_{a=1}^{A} \gamma_a AssignedTreatment_i * Z_i + \sum_{t=1}^{q} \delta_t + \sum_{t=1}^{q} \delta_t * Z_i + \theta Y_{i0} + \beta' X_i + \pi_{is} + \varepsilon_{it} \quad (3)$$

where Z_i is a dummy representing either below median baseline household assets, a lack of any previous experience hiring a paid worker, or below median baseline management practices scores. Our prior is that each of these interaction terms will be positive for the particular treatment they relate to – that is, for example, training will have a larger effect on those with below median baseline practices scores. The specification pools the data across the post-treatment rounds, and allow for wave effects to differ for the two groups.

Table 6 shows the results for two outcomes: the number of paid workers and the winsorized sales level. For sales, the interaction terms are all positive, but none are statistically different from zero. For paid workers, on the other hand, the interaction terms are as often negative as positive, and significant for the specific treatment they are matched to only in the case of training. For example, there are no significant effect of any of the treatments on either outcome for those with above median household wealth. Compared with the wealthier households, those with below

stronger results for the savings and training treatments in the retail sector. The differences across sectors are significant for a few outcomes, but generally not significant.

median household wealth are significantly more likely to have hired workers as a result of the wage subsidy program and as a result of training. With regard to previous experience with a paid worker (Columns 2), the savings and training treatments – though curiously, not the wage incentives –have significant effects on the number of paid workers for those with previous employer experience, but not for those without previous experience. This result is inconsistent with the theory that the wage subsidies work primarily by subsidizing learning about managerial ability. Finally, those with above median management practices score are induced by each of the treatments to hire employees, but in comparison those with below median scores are significantly less likely to hire employees after training. In sum, though none of the interactions are significant, we find higher measured effects on sales in the low-wealth, low experience, low management skills groups. But we find the opposite with regard to employment of paid workers. With regard to employment, the effect is larger for the high-wealth, high experience, high practices group.

An alternative approach is to follow de Mel et al (2010a) and identify characteristics which are associated with owners of larger firms. In April 2008, when we conducted the baseline for the sample used in this project, we also sample random groups of wage workers and owners of firms with 5-50 employees. We use these two samples to identify characteristics of SME owners and wage workers. In the Lucas (1978) framework, development is associated with growth of firms managed by high-θ owners, and movement from self-employment to wage work of the low-θ owners. We think of this exercise as using the samples of wage workers and SME owners to estimate the characteristics of high-θ owners. We then classify our experimental sample into those more like the SME owners ("high-θ") and those more like the wage workers ("low-θ"). We can then look at whether the high-θ owners grow more quickly over our project period, and whether they benefit more from the treatments. We focus on employment growth in this part of the analysis.

We estimate θ using a sub-set of characteristics used by de Mel et al (2010a). There are three sets of characteristics: ability (education, digitspan recall and childhood leadership), family background (parents education and business experience, childhood income levels), and attitudes (risk, ambition, tenacity, etc.). The regression reported in the appendix shows that all three sets of characteristics separate wage workers from SME owners. In particular, SME owners have higher levels of

schooling (each additional year is associated with an extra 2 percentage point change of being and SME owner), and higher digitspan recall scores. SME owners are also significantly less likely to say that their family sometimes did not have enough to eat when they were 12 years of age, and to say that their father owned a business. With respect to attitudes, SME owners have higher levels of trust in strangers, like to juggle tasks, and place work as a more central in their life.

Using this analysis to classify those in our sample, we find that 29 percent of the experimental sample has characteristics more like the SME owners ("high-θ" types) and 71 percent has characteristics more like the wage workers ("low-θ" types). As a first check on the reasonableness of the classification, we compare the size of the two groups at baseline. Reassuringly, the "high-θ" types are significantly larger. They are more likely to have at least one employee (0.16 vs. 0.09, $p<.01$) and have a larger average number of employees (0.29 vs. 0.15, $p<.01$). They also have significantly higher sales and higher baseline management practices scores. Do the high-θ types also grow faster between baseline and round 10? We find results on sales, profits, and employment which suggest they might, though the differences are not significant. For example, among the control group firms, those in the low-θ add an average of 0.39 employees over the four years, while those in the high-θ group add an average of 0.63. But the difference is significant only at the 0.14 level.

Table 7 shows how the effects of the three treatments differs across the two groups. The main takeaway from the table is that the treatments have a larger positive impact on the low-θ types. Most of the interaction effects have a negative – though insignificant – coefficient. The two exceptions are that training has a significantly less positive effect on sales for the SME types, and that the wage subsidies have a significantly larger effect on inventories for the SME types. But our conclusion from this is that none of the interventions are particularly powerful in unleashing growth among the subset of owners with characteristics suggesting that they are most likely to grow.

Section 6: Discussion and Conclusions

The project can be viewed as a concerted effort to induce sustained growth of employment in a random cross-section of microenterprises. As such, the primary conclusion is that, for the typical firm in the sample, relaxing constraints on capital or

labor, or providing standard management training do not induce sustained growth. Though many of the outcomes, particularly sales and profits, are quite noisy, we find little evidence that the treatments have anything more than a temporary effect. As such, they appear to speed convergence to the steady state, but do not appear to be effective at inducing a higher rate of growth.

One factor which should to be taken into account when interpreting the results is that the period in which the study takes place is one of very rapid economic growth in Sri Lanka. By the fall of 2012, one-third of the firms in the control group who had no paid employees at baseline had at least one paid employee. Sales in the control group grew at an annual rate of around 14 percent over the period. The robust economic growth may have allowed even the control groups to grow by reinvesting profits.

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Table 1: Assignment to Treatments

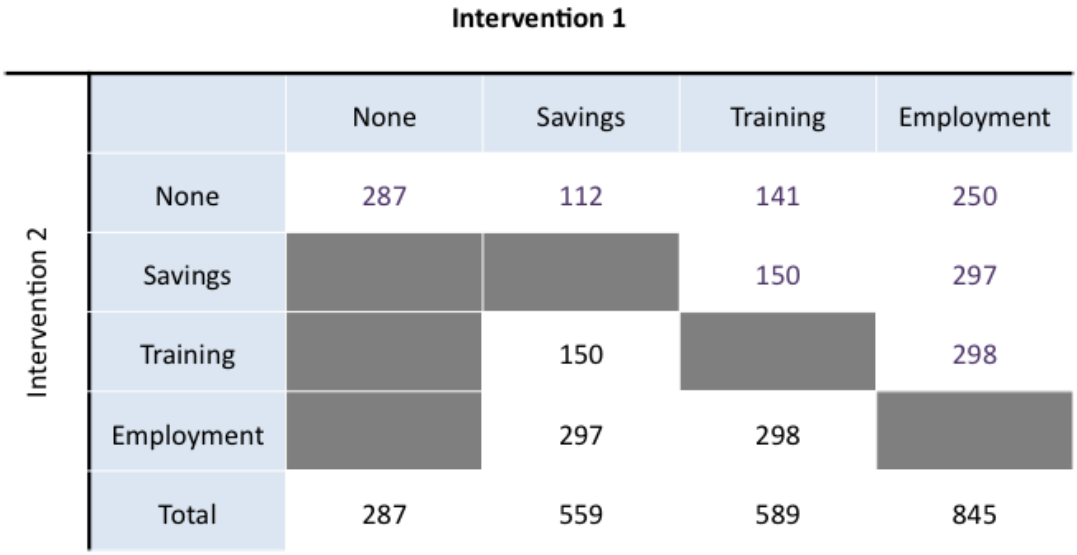

Table 2: Baseline Summary Statistics by Treatment Group

	Training	Savings	Wage Subsidy	Savings + Wage Subisdy	Savings + Training	Training + Wage Subsidy	Control
Number of Paid Workers	0.145	0.216	0.144	0.169	0.222	0.138	0.122
Any Paid Worker	0.092	0.098	0.098	0.119	0.111	0.090	0.108
Education (years)	11.11	10.45	10.66	10.60	10.28	10.99	10.39
Raven Test Score	3.30	3.06	3.02	3.03	3.06	3.04	3.32
Digitspan Recall Score	6.66	6.24	6.48	6.61	6.56	6.46	6.61
Total Assets	466020	386653	356336	452901	326184	670701	358800
Capital Stock	706277	664742	841692	453639	621318	1070599	543051
Sales	57149	36372	60544	58174	40842	47118	48876
Profits	14688	13795	15913	15057	14554	15293	15590
Business Practice Scores	9.24	8.35	8.76	8.24	8.18	8.59	8.29
Colombo	0.482	0.473	0.440	0.465	0.460	0.465	0.469
Galle	0.121	0.125	0.120	0.118	0.120	0.121	0.122
Kandy	0.475	0.473	0.476	0.478	0.480	0.475	0.476
Baseline Sample Size	141	112	250	297	150	297	286
Follow-up survey sample sizes							
Round 3	134	108	245	292	143	289	274
Round 4	134	99	235	272	136	268	268
Round 5	130	96	226	264	136	265	257
Round 6	124	91	220	261	132	259	249
Attrition Rate By Last Round	12.1	18.8	12.0	12.1	12.0	12.8	12.9

Table 3: Effect of Treatments on Inputs

Panel A: Management Practices Score

VARIABLES	(1) ANCOVA All rounds	(2) ANCOVA Round 4	(3) ANCOVA Round 5	(4) ANCOVA Round 6	(5) ANCOVA Round 7	(6) ANCOVA Round 8	(7) ANCOVA Round 9	(8) ANCOVA Round 10	(9) ANCOVA Rounds 6-10	(10) FE Rounds 6-10
Assigned to Savings treatment	0.225	0.262	0.220	0.379	0.341	0.023	0.065	0.201	0.212	0.433
	(0.19)	(0.32)	(0.32)	(0.31)	(0.32)	(0.29)	(0.30)	(0.33)	(0.21)	(0.30)
Assigned to Wage subsidy treatment	-0.047	0.245	0.627**	-0.005	-0.477	-0.520*	-0.101	-0.188	-0.256	-0.349
	(0.18)	(0.30)	(0.30)	(0.31)	(0.31)	(0.28)	(0.29)	(0.31)	(0.20)	(0.29)
Assigned to Training treatment	0.898***	1.718***	1.124***	0.720**	0.820**	0.541*	0.538*	0.711**	0.673***	0.467
	(0.20)	(0.32)	(0.32)	(0.32)	(0.33)	(0.29)	(0.31)	(0.33)	(0.21)	(0.31)
Mean Dep Var Controls	8.62	10.62	8.28	9.29	8.88	8.11	6.62	8.32	8.25	8.25
Observations	8,771	1,305	1,284	1,242	1,272	1,241	1,207	1,220	6,182	7,786
R-squared	0.269	0.228	0.265	0.282	0.255	0.279	0.189	0.209	0.250	0.047
Number of sheno										1,534

Panel B:Inventory levels (Winsorized at the 1st and 99th percentile)

VARIABLES	(1) ANCOVA All rounds	(2) ANCOVA Round 4	(3) ANCOVA Round 5	(4) ANCOVA Round 6	(5) ANCOVA Round 7	(6) ANCOVA Round 8	(7) ANCOVA Round 9	(8) ANCOVA Round 10	(9) ANCOVA Rounds 6-10	(10) FE Rounds 6-10	(11) Hyper Sine Rounds 6-10
Assigned to Savings treatment	10,923**	6,682	4,461	12,176	12,438*	19,760*	17,378**	3,027	13,047**	22,737***	0.3971**
	(5,430)	(6,543)	(7,942)	(7,560)	(7,247)	(10,733)	(7,888)	(8,653)	(5,961)	(7,019)	(0.193)

Assigned to Wage subsidy treatment	6,673	9,636	9,793	10,005	1,913	11,144	588	3,459	5,496	7,270	0.0766
	(5,475)	(5,989)	(7,715)	(7,235)	(6,904)	(10,870)	(7,604)	(8,639)	(6,163)	(7,006)	(0.188)
Assigned to Training treatment	1,480	3,796	-2,851	1,784	-772	-1,521	-170	8,785	1,778	6,832	-0.0421
	(5,596)	(6,388)	(8,174)	(7,382)	(7,017)	(10,557)	(7,605)	(8,957)	(6,122)	(6,837)	(0.193)
Mean Dep Var Controls	50422	46436	56893	49809	38187	48594	53518	59959	49935	49935	6.90
Observations	8,812	1,340	1,284	1,241	1,275	1,241	1,207	1,224	6,188	7,793	6,188
R-squared	0.285	0.354	0.315	0.319	0.341	0.230	0.273	0.289	0.273	0.008	0.313
Number of sheno										1,534	

Panel C: Fixed assets, excluding land and buildings (Winsorized 1st / 99th percentile

	(1) ANCOVA	(2) ANCOVA	(3) ANCOVA	(4) ANCOVA	(5) ANCOVA	(6) ANCOVA	(7) ANCOVA	(8) ANCOVA	(9) ANCOVA	(10) FE	(11) Hyper Sine
VARIABLES	All rounds	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Round 10	Rounds 6-10	Rounds 6-10	Rounds 6-10
Assigned to Savings treatment	2,981	895	-2,684	1,034	-5,142	-2,643	17,940	13,118	4,575	3,160	0.1939**
	(11,050)	(6,034)	(9,586)	(11,638)	(13,152)	(14,775)	(17,304)	(18,057)	(13,566)	(13,549)	(0.098)
Assigned to Wage subsidy treatment	820	2,688	838	2,261	3,199	-3,240	-1,752	2,087	423	-2,844	-0.0501
	(10,902)	(6,045)	(9,370)	(11,498)	(13,314)	(15,156)	(17,083)	(17,774)	(13,491)	(13,213)	(0.093)
Assigned to Training treatment	14,190	6,938	9,393	4,104	8,487	7,309	36,081**	32,181*	17,086	17,947	-0.0159
	(10,890)	(6,613)	(9,515)	(11,206)	(12,886)	(15,117)	(17,901)	(18,615)	(13,330)	(13,966)	(0.094)
Mean Dep Var Controls	256393	206987	226503	258025	270037	281574	281821	292012	276033	276033	11.87
Observations	8,088	1,311	1,244	1,173	1,151	1,104	1,063	1,042	5,533	7,078	8,088
R-squared	0.677	0.894	0.785	0.733	0.682	0.601	0.567	0.556	0.624	0.076	0.419
Number of sheno										1,454	

Panel D: Number of paid workers (Winsorized at the 1st and 99th percentile)

VARIABLES	(1) ANCOVA All rounds	(2) ANCOVA Round 4	(3) ANCOVA Round 5	(4) ANCOVA Round 6	(5) ANCOVA Round 7	(6) ANCOVA Round 8	(7) ANCOVA Round 9	(8) ANCOVA Round 10	(9) ANCOVA Rounds 6-10	(10) FE Rounds 6-10
Assigned to Savings treatment	0.124**	0.076	0.130**	0.201***	0.177***	0.100	0.127*	0.059	0.133**	0.111
	(0.05)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)	(0.08)
Assigned to Wage subsidy treatment	0.135***	0.265***	0.210***	0.078	0.174***	0.117*	0.082	0.002	0.091*	0.205**
	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.05)	(0.08)
Assigned to Training treatment	0.071	0.038	0.044	0.107	0.094	0.068	0.091	0.053	0.083	0.172**
	(0.05)	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.08)
Mean Dep Var Controls	0.57	0.48	0.53	0.56	0.45	0.60	0.59	0.79	0.60	0.60
Observations	8,816	1,340	1,285	1,244	1,275	1,241	1,206	1,225	6,191	7,078
R-squared	0.145	0.200	0.159	0.164	0.136	0.156	0.126	0.133	0.138	0.076
Number of sheno										1,454

Panel E: Number of hours worked by owner (Winsorized at the 1st and 99th percentile)

VARIABLES	(1) ANCOVA All rounds	(2) ANCOVA Round 4	(3) ANCOVA Round 5	(4) ANCOVA Round 6	(5) ANCOVA Round 7	(6) ANCOVA Round 8	(7) ANCOVA Round 9	(8) ANCOVA Round 10	(9) ANCOVA Rounds 6-10	(10) FE Rounds 6-10
Assigned to Savings treatment	-0.645	-3.031**	-0.225	-1.386	-0.257	0.720	-1.042	-1.277	-0.645	-1.243
	(0.90)	(1.38)	(1.51)	(1.45)	(1.60)	(1.39)	(1.36)	(1.30)	(0.90)	(1.57)
Assigned to Wage subsidy treatment	-0.672	-1.680	0.760	-1.843	-1.555	0.102	-0.208	0.089	-0.672	-1.249
	(0.88)	(1.31)	(1.44)	(1.39)	(1.53)	(1.32)	(1.33)	(1.27)	(0.88)	(1.49)

Assigned to Training treatment	-0.315	-0.043	1.418	-0.958	0.558	-1.449	-0.110	0.320	-0.315	0.754
	(0.91)	(1.33)	(1.46)	(1.42)	(1.58)	(1.38)	(1.37)	(1.30)	(0.91)	(1.52)
Mean Dep Var Controls	52.1	58.5	48.3	53.4	45.5	54.6	51.0	53.1	52.5	52.5
Observations	6,189	1,339	1,286	1,243	1,275	1,241	1,205	1,225	6,189	7,077
R-squared	0.073	0.083	0.090	0.093	0.051	0.076	0.053	0.087	0.073	0.042
Number of sheno										1,453

Panel F: Number of unpaid workers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	FE
VARIABLES	All rounds	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Round 10	Rounds 6-10	Rounds 6-10
Assigned to Savings treatment	0.032	0.011	0.029	-0.016	0.033	0.059	0.025	0.059	0.032	-0.077*
	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)
Assigned to Wage subsidy treatment	-0.023	-0.019	0.051*	-0.054	-0.049	0.014	-0.021	-0.002	-0.023	-0.047
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.04)
Assigned to Training treatment	0.004	0.025	-0.046	-0.053	-0.010	0.017	0.070**	-0.004	0.004	-0.053
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.02)	(0.04)
Mean Dep Var Controls	0.23	0.21	0.17	0.28	0.30	0.23	0.18	0.23	0.24	0.24
Observations	6,192	1,340	1,285	1,244	1,275	1,241	1,207	1,225	6,192	7,079
R-squared	0.033	0.057	0.062	0.053	0.030	0.049	0.042	0.038	0.033	0.015
Number of sheno										1,454

Table 4: Effect of Treatments on Outputs

Panel A: Real Sales (Truncated at 99th percentile)

VARIABLES	(1) ANCOVA All rounds	(2) ANCOVA Round 4 Oct-09	(3) ANCOVA Round 5 Apr-10	(4) ANCOVA Round 6 Oct-10	(5) ANCOVA Round 7 Apr-11	(6) ANCOVA Round 8 Oct-11	(7) ANCOVA Round 9 Apr-12	(8) ANCOVA Round 10 Oct-12	(9) ANCOVA Rounds 6-10	(10) FE Rounds 6-10	(11) Hyper Sine Rounds 6-10
Assigned to Savings treatment	10,146**	8,570	12,070*	6,593	14,843*	17,070**	7,023	5,593	10,143*	12,379**	0.0267
	(4,945)	(5,687)	(6,542)	(6,041)	(7,626)	(8,194)	(8,020)	(7,806)	(5,575)	(6,029)	(0.058)
Assigned to Wage subsidy treatment	1,586	7,804	-360	-2,094	-815	7,124	-3,916	3,297	721	-4,311	0.0432
	(4,552)	(4,988)	(5,779)	(5,759)	(6,954)	(7,645)	(7,309)	(7,269)	(5,231)	(5,584)	(0.055)
Assigned to Training treatment	4,708	-683	-4,890	7,679	10,167	3,173	10,572	7,404	7,882	9,307*	0.0222
	(4,503)	(4,908)	(5,544)	(6,173)	(7,004)	(7,365)	(7,929)	(7,382)	(5,272)	(5,649)	(0.055)
Mean Dep Var Controls	73253	57930	69018	68848	69593	84957	78661	86223	77581	77581	11.27
Median dep var Controls		28044	27377	32429	32567	36125	39518	39038			
Observations	8,631	1,310	1,254	1,207	1,257	1,219	1,180	1,204	6,067	7,716	6,068
R-squared	0.184	0.219	0.206	0.204	0.165	0.228	0.155	0.179	0.179	0.051	0.156
Number of sheno										1,532	

Panel B: Real Profits (Truncated at 1st and 99th percentile)

VARIABLES	(1) ANCOVA All rounds	(2) ANCOVA Round 4	(3) ANCOVA Round 5	(4) ANCOVA Round 6	(5) ANCOVA Round 7	(6) ANCOVA Round 8	(7) ANCOVA Round 9	(8) ANCOVA Round 10	(9) ANCOVA Rounds 6-10	(10) FE Rounds 6-10	(11) Hyper Sine Rounds 6-10

Assigned to Savings treatment	196 (623)	-466 (794)	1,278 (1,203)	104 (856)	439 (913)	303 (922)	-110 (1,176)	-41 (1,035)	106 (676)	565 (827)	0.0267 (0.058)
Assigned to Wage subsidy treatment	-333 (581)	-122 (737)	-336 (1,039)	-783 (815)	542 (861)	53 (892)	-1,710 (1,130)	-21 (964)	-377 (649)	-374 (800)	0.0432 (0.055)
Assigned to Training treatment	337 (608)	910 (772)	-533 (1,053)	-902 (816)	384 (893)	177 (905)	741 (1,214)	1,524 (1,079)	374 (685)	906 (824)	0.0222 (0.055)
Mean Dep Var Controls	19552	15926	18866	18914	18317	21069	22893	21421	20505	20505	10.23
Observations	8,700	1,317	1,254	1,216	1,269	1,234	1,193	1,217	6,129	7,693	6,068
R-squared	0.154	0.168	0.148	0.197	0.114	0.186	0.143	0.162	0.154	0.059	0.156
Number of sheno										1,531	

Panel C: Real Personal Income (Truncated at 1st and 99th percentile)

VARIABLES	(1) ANCOVA All rounds	(2) ANCOVA Round 4	(3) ANCOVA Round 5	(4) ANCOVA Round 6	(5) ANCOVA Round 7	(6) ANCOVA Round 8	(7) ANCOVA Round 9	(8) ANCOVA Round 10	(9) ANCOVA Rounds 6-10	(10) FE Rounds 6-10	(11) Hyper Sine Rounds 6-10
Assigned to Savings treatment	-143 (844)	-478 (883)	1,397 (1,695)	-874 (1,112)	409 (1,710)	283 (1,238)	-506 (1,791)	-1,188 (1,438)	-407 (928)	588 (1,153)	0.0217 (0.050)
Assigned to Wage subsidy treatment	-47 (776)	-224 (785)	98 (1,356)	-214 (1,065)	342 (1,484)	502 (1,228)	-927 (1,647)	-2 (1,489)	-47 (898)	-459 (1,103)	0.0108 (0.049)
Assigned to Training treatment	200 (839)	839 (876)	-1,860 (1,491)	-1,635 (1,016)	-321 (1,654)	249 (1,224)	1,206 (1,970)	2,812 (1,928)	452 (987)	775 (1,146)	0.0526 (0.049)
Mean Dep Var Controls	19202	15906	18595	18702	18499	20482	21980	20429	20019	20019	10.14
Observations	8,773	1,296	1,251	1,226	1,282	1,257	1,217	1,244	6,226	7,945	6,226
R-squared	0.127	0.202	0.200	0.179	0.091	0.173	0.122	0.094	0.115	0.022	0.062

Number of sheno										1,532	

Panel D: Household Income (Truncated at 1st and 99th percentile)

VARIABLES	(1) ANCOVA All rounds	(2) ANCOVA Round 4	(3) ANCOVA Round 5	(4) ANCOVA Round 6	(5) ANCOVA Round 7	(6) ANCOVA Round 8	(7) ANCOVA Round 9	(8) ANCOVA Round 9	(9) ANCOVA Rounds 6-10	(10) FE Rounds 6-10	(11) Hyper Sine Rounds 6-10
Assigned to Savings treatment	3	-18	1,933	385	-389	142	-1,549	-429	-403	1,115	-0.0076
	(775)	(920)	(1,664)	(955)	(1,400)	(1,167)	(1,478)	(1,230)	(844)	(2,119)	(0.032)
Assigned to Wage subsidy treatment	-853	-288	-121	-910	-962	-444	-2,525*	-984	-1,130	-4,902**	-0.0385
	(731)	(847)	(1,473)	(913)	(1,334)	(1,101)	(1,440)	(1,120)	(817)	(2,067)	(0.031)
Assigned to Training treatment	-65	1,145	-1,790	-844	-338	109	519	658	8	2,600	0.0022
	(753)	(878)	(1,411)	(922)	(1,336)	(1,121)	(1,555)	(1,239)	(854)	(2,123)	(0.032)
Mean Dep Var Controls	27105	21039	27922	23320	29393	27447	33149	28136	28293	28293	10.68
Observations	8,596	1,305	1,250	1,206	1,249	1,215	1,173	1,198	6,041	7,673	6,041
R-squared	0.160	0.195	0.160	0.205	0.128	0.167	0.141	0.178	0.154	0.027	0.086
Number of sheno										1,528	

Table 5: Interaction of Treatments

VARIABLES	(1) Paid workers		(2) Inventories		(3) Sales		(4) Profits	
Assigned to Savings treatment	0.082		45,592**		22,527		2,191	
	(0.12)		(18,339)		(14,162)		(1,469)	
Assigned to Wage subsidy treatment	0.022		5,056		7,828		596	
	(0.08)		(9,225)		(8,059)		(1,135)	
Assigned to Training treatment	0.096		27,220**		15,403		381	
	(0.11)		(11,754)		(11,392)		(1,370)	
Assigned to Savings and Wage treatments	0.134	0.238***	-22,527	28,122***	-17,509	12,846	-3,243*	-455
	(0.14)	(0.091)	(21,167)	(8,492)	(16,962)	(8,005)	(1,804)	(1,026)
Assigned to Savings and Training treatments	-0.027	0.151	-74,962***	2,150	-17,812	20,118**	-1,561	1,012
	(0.18)	(0.110)	(21,959)	(8,217)	(19,508)	(9,511)	(2,125)	(1,308)
Assigned to Wage and Training treatments	0.043	0.161*	-15,320	16,955*	-11,359	11,872*	-373	604
	(0.14)	(.087)	(15,555)	(9,213)	(13,852)	(6,889)	(1,759)	(1,036)
Observations	6,191		6,188		6,068		6,129	
R-squared	0.138		0.279		0.193		0.155	

Indicates level and significance of the indicated combination of treatmentsin the regression to the left.
Data from rounds 6-10; Inventories, sales and profits deflated by Colombo CPI and winsorized at the 1st and 99th percentile
Values in parethesis are standard errors

Table 6: Treatment Effects by baseline heterogenity

	(1) Wealth index <0		(2) No Exper w/ Paid worker		(3) Management Practices<7	
VARIABLES	Sales	Paid workers	Sales	Paid workers	Sales	Paid workers
Assigned to Savings treatment	**9,712**	**0.099**	**-6,232**	**0.262***	**7,608**	**0.168****
	(8,847)	(0.09)	(13,435)	(0.14)	(8,874)	(0.08)
Assigned to Wage subsidy treatment	**-5,919**	**-0.035**	**798**	**0.204**	**-4,467**	**0.156****
	(8,171)	(0.08)	(12,641)	(0.13)	(8,629)	(0.08)
Assigned to Training treatment	**7,196**	**-0.065**	**-2,796**	**0.311****	**5,953**	**0.184****
	(8,145)	(0.09)	(12,645)	(0.14)	(8,683)	(0.08)
Assigned to Savings *X	**454**	**0.058**	**22,363**	**-0.160**	**4,863**	**-0.064**
	(11,453)	(0.12)	(14,729)	(0.15)	(11,278)	(0.12)
Assigned to Wage * X	**12,085**	**0.219****	**353**	**-0.174**	**10,262**	**-0.127**
	(10,631)	(0.11)	(13,908)	(0.14)	(10,604)	(0.11)
Assigned to Training * X	**884**	**0.250****	**14,359**	**-0.295***	**3,648**	**-0.203***
	(10,561)	(0.12)	(13,938)	(0.15)	(10,467)	(0.11)
X	-24,019**	-0.340**	41,857***	0.494***	-8,129	0.172
	(12,046)	(0.15)	(14,674)	(0.14)	(13,732)	(0.13)
Observations	6,068	6,191	6,068	6,191	6,068	6,191
R-squared	0.194	0.142	0.199	0.193	0.193	0.140

Table 7: Allowing for interaction by species classification

	(1)	(2)	(3)	(4)
VARIABLES	Paid workers	Inventories	Sales	Profits
Assigned to Savings treatment	0.176**	15,166**	15,958**	698
	(0.07)	(7,343)	(7,359)	(881)
Assigned to Wage subsidy treatment	0.125*	-2,365	-3,781	-834
	(0.06)	(7,112)	(6,608)	(813)
Assigned to Training treatment	0.098	-2,754	15,208**	969
	(0.07)	(6,786)	(6,336)	(826)
Assigned to Savings treatment * SME	**-0.088**	**-4,307**	**-14,302**	**-1,981**
	(0.14)	(15,899)	(13,365)	(1,608)
Assigned to Wage subsidy treatment * SME	**-0.126**	**27,444***	**6,519**	**205**
	(0.14)	(16,514)	(12,819)	(1,639)
Assigned to Training treatment*SME	**-0.028**	**16,448**	**-31,350****	**-2,653**
	(0.14)	(16,975)	(12,787)	(1,709)
Observations	5,697	5,696	5,583	5,639
R-squared	0.147	0.274	0.193	0.155