Preliminary and Incomplete: Please do not Quote

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

Suresh de Mel, David McKenzie and Christopher Woodruff[#]

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 most consistent effects come from a savings incentive program designed to allow owners to build capital in the business. Wage incentives lead to higher levels of employment, but not to higher profits. We find that while there are effects from the savings program through most of the distribution of firms, the effects of combined treatments are concentrated among a small percentage of firms experiencing substantial growth.

[#] University of Peradeniya, World Bank Research Group, and University of Warwick, respectively. Funding for this project was provided by the National Science Foundation (SES0820375), the World Bank, DFiD, the Knowledge for Change Trust Fund, the Diagnostic Facility for Shared Growth Trust Fund, and the Templeton Foundation. Matthew Groh provided excellent research assistance. The surveys and interventions were carried out with aplomb by the Kandy Consulting Group, without whose assistance we would not have been able to undertake the project.

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.[1] We then 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 then used a 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, but 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 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.

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, 559 enterprises in total to be assigned to the savings incentive treatment, 589 to be assigned to the business training treatment, and 845 to be assigned to the wage incentive treatment which we expected to have lowest take-up.[4] Within each stratum we then randomly assigned 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 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.

[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).

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 up savings. 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. 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.

The purpose of the wage subsidy program was to encourage 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

[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.

October for the full amount of the subsidy to be paid. The subsidy represents about half of a typical unskilled worker earnings.

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 and a short interview focused on the search and hiring process with the owner. We had research assistants make occasional unannounced visits to the enterprise to make sure the employee was working. In a few cases, we were unable to confirm that the employee was working full time. In all of these cases, after a few visits, the owner notified us that the employee was no longer working, and the subsidy was removed. We believe these spot checks were sufficient to root out any 'phantom' employees, though it is possible that a few deceptive owners avoided our screens.

Finally, the training program was 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 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 providers 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.

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

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

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 the wage incentives were in force during these two survey waves. We have conducted four follow-up surveys after all the treatments were completed, in October 2010 and 2011, and in April 2011 and 2012. These final four surveys are of particular interest because they allow us to examine effects that are fully post-treatment. [Note: Analysis of attrition to come. In the April 2012 survey, we were able to contact 86.8 percent of the control group and 85 percent of those receiving at least one treatment. From the perspective of business outcomes, there is further attrition caused by exit from self employment.]

Section 3: Results

Take-up

Training: 368 out of 587 offered the program (63 percent) attended at least one session. 341 completed the program and received a certificate. This represents 93 percent of those starting the program and 58 percent of the full sample.

Outcomes

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. 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:

$$Y_{i,t} = \alpha + \gamma_1 Assigned\ Training_i + \gamma_2 Assigned\ Savings_i + \gamma_3 Assigned\ WageSubsidy_i + \theta Y_{i,0} + \beta' X_i + \pi_{i,s} + \varepsilon_{i,t} \quad (1)$$

where $\pi_{i,s}$ are randomization strata dummies, $Y_{i,0}$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:

$$Y_{i,t} = \sum_{t=1}^{q} \delta_t + \gamma_1 Assigned\ Training_i + \gamma_2 Assigned\ Savings_i + \gamma_3 Assigned\ WageSubsidy_i + \theta Y_{i,0} + \beta' X_i + \pi_{i,s} + \varepsilon_{i,t} \quad (2)$$

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 nine. 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 only, or being assigned to receive training and cash.

Table 3 shows the effect of the three treatments on management practices, 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 ninth. The next six columns show the results for each round 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 eighth column shows the results of an ANCOVA

using rounds six to nine – all treatment rounds after the wage subsidies were removed. Finally, the ninth and last column shows the results of a fixed effects regression using the same sample of rounds six to nine.

We find that training has a modest but statistically significant positive effect on management practices. On average across all post treatment rounds, the management practices score increases by almost one point after training, or by about 10 percent of the mean for the control group. The round-by-round data show that the effect is largest in round 4, when it is 1.7 points, and drops steadily to about a half a point in rounds 8 and 9. 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).

Panel B of Table 3 shows the effect of the treatments on capital stock. We show results for investment in inventories, but the patterns are similar if we use all capital excluding land and buildings instead. Only the savings incentive treatment has a significant effect on capital investment. We see that inventory levels increase by an average of just over 15,000 LKR more for the savings treatment group than the control groups across the post-treatment rounds. This treatment effect is comparable to the average final balance of about 12,000 LKR among those assigned to the savings treatment. While the savings became available on 1 August, there is no significant effect on inventory investment until a full year later, in the October 2009 (round 6) survey. The effect of the program appears to be increasing even two years later, in rounds 8 and 9, suggesting perhaps that the proceeds from the additional inventories are being reinvested in the enterprises. Neither of the other treatments has any effect on investment levels.[7]

The final panel of Table 3 shows the effect of the programs on hired labor. We use the number of paid employees, winsorized at 1 and 99 percent. In effect, 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

[7] The results using all capital excluding land and buildings show coefficients of almost identical magnitudes, suggesting that the majority of the effect operates through inventory investments. The noisier data for the broader capital stock measure results in insignificance of most of the broader capital stock results.

surprisingly, then, we find the largest effects during these two periods. But even in rounds 6-9, one to two years after the subsidies were stopped, we find an effect of just over one-tenth of an employee, around 20 percent of the mean of the control group. The estimated effect jumps around a bit across the individual rounds, but there seems to be 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.

Perhaps more surprisingly, the savings incentive program also appears to have an effect on the use of hired labor. Indeed, the ANCOVA results suggest that savings program is as or even more effective in generating employment. 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 2000 LKR – about one quarter of an unskilled salary – while the measured effects for the other two treatments are around 600 LKR. None of the treatments have any effect on the reported number of hours worked by the owner of the enterprise. Coefficients of – 0.5 to -1.0 hours are estimated precisely enough that we can rule out a change of more than a couple of hours per week.

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. For now, we do this by examining the direct effect of the treatments on measures of enterprise outputs – revenues, profits and total household income.

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 by about 20 percent of the mean of the control group and by an

amount which is slightly larger 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 1.5. 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.

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. While the measured effect of incremental profits following the savings treatment is generally positive, in magnitude it is only about 4 percent of the change in sales. Here the gaps between sample average and the estimated marginal rations are large enough to have some confidence that they differ from one another. The lower incremental profit-to-sales ratios suggests that the incremental sales being generated are not as profitable as the baseline sales, or that the estimate. Whether this is because the owners have moved into new product lines that take time to develop, or whether it just reflect noise in the profits data is the subject of ongoing investigation.

The final panel of Table 5 presents an alternative measure of impact, reported household income from all sources. In some cases, owners told us that the employee they hired freed up time for them to work in a different business. For example, one owner of a fruit stand said that the employee allowed him to build up a wholesale business; another owner of a video rental store said that the employee allowed him to spend more time selling real estate. Hence, it is possible that income generated from the wage subsidy would accrue to other businesses rather than to the profits of the original business. However, the data provide no support for this story. On the contrary, the negative effect of the wage subsidies on income is now significant in a couple of cases – in the round nine data using ANCOVA and in the fixed effects regression using data from rounds six to nine.

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. 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 – 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 9 in all of the regressions.

Almost all of the interaction effects are negative, though only a few are significant. The exception is in the paid worker regression, where two of the three interactions are negative, though none of the three is significant. 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 0.3 workers, just over half the control group mean; the savings and training combination increases employment by about 0.2 workers. Both combinations involving the savings incentives also increase inventory investments and sales significantly. 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, which falls just 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.

Section 4: Heterogeneity of outcomes

While the primary purpose of the project is to examine effects in the overall sample, we explore heterogeneity of impacts in a couple of dimensions. First, we stratified on the retail sector because we expected retail activities would be relatively less responsive to shocks to the production function than manufacturing activities – especially where the latter involves sales to broader regional or national markets. Second, we previously examined ability and attitude differences between the self employed, wage workers and larger firm owners (de Mel et al 2010a). We constructed a 'species classification' to separate small firm owners into those more like large firm owners and those more like wage workers. We might expect the former to be more responsive to the stimulus treatment provides. In a future draft of the paper we will test this directly by using data from wage and large firms conducted in April 2008 contemporaneously with the baseline for this project. For now, we present suggestive results separating the sample using a smaller set of ability measures, as described below.

Table 6 shows results separately for the retail and non-retail sectors. Consistent with our expectations, the wage subsidies have a positive effect on employment only in the non-retail sector. However, the savings treatment appears to have positive effects of comparable magnitude on employment in both the retail and non-retail sectors. Moreover, training has a positive effect on savings only in the retail sector. We conclude that there are not consistent differences in the effects of the treatments across sectors.

In Table 7, we explore the heterogeneity of response according to measures of ability. We take the first principal component of years of schooling, a score on a Raven non-verbal test, and a score on a digitspan recall test. We split the owners into those above the median and those below, and run regressions that allow for interaction effects across treatments. Table 7 shows that there are very few significant treatment effects among those with below median ability and, perhaps surprisingly, that savings incentives alone generates the most significant impacts among those with above median ability. We see increases in inventory investments, sales and profits among those with higher ability levels. Paradoxically, combining savings incentives with either of the other treatments appears to reduce or eliminate the effects.

Species classification to follow…

Scaling up

The average of one-tenth of an employee following the wage incentive and savings treatments may be due to one-tenth of the firms hiring one extra worker, or a few of the firms becoming much larger. What do the data say about this? Table 8 shows the distribution of firms by employment size in rounds 8 and 9 of the survey for three different groups of firms: the control group, those offered the wage incentives and those offered the savings incentives. We show the percentage of enterprises in each group by number of paid employees, with the largest category including all enterprises with five or more employees. The treatments are associated with 3 to 4 percent fewer firms without any employees. In the case of wage subsidies, about half of that difference is found in firms with exactly one employee – perhaps the one our incentives induced them to hire. But the remaining 2 percentage points of the gap is found in enterprises with four or more employees. Moreover, essentially all of the 3 percentage point reduction in firms without employees in the savings treatment group have four of more employees. Thus, it appears that in the longer run, most of the enterprises hiring workers as a part of our wage incentives released those workers. But a small percentage have added many more employees. Perhaps more surprisingly, the same is true for an upper tail of firms offered the savings incentives. In either case, these represent only around 11 additional enterprises being in these categories, compared with the average of the control group.[8]

For sales and profits, we carry out a similar exploration by comparing the CDFs of the treatment groups compared with the control group. These are shown in Figures 1 (sales) and Figure 2(profits). Each graph shows three lines: the control group, the enterprises receiving the given treatment only, and the enterprises receiving the given treatment in combination with one of the other treatments. We use data from round 6 through 9. For sales, we see effects of the savings treatment everywhere above the lower quartile; for wage incentives and training, the effects are evident only in the upper half of the distribution. The negative effects of the combined treatments are evident for the savings treatment only in the middle of the distribution, while for training they are evident everywhere training appears to have an effect. For profits (Figure 2), the story is simpler: we see almost no separation anywhere in the distribution for the wage incentives or training treatments, either alone or in

[8] The treatment categories used in Table 8 are not exclusive. Hence the wage incentive group includes firms receiving one of the other treatments as well. Most of the increase in larger firms appears to come from the set of enterprises offered a combination of treatments.

combination with other treatments. For savings, there is separation everywhere above the lowest quintile for the savings incentive treatment by itself, but this effect is lost almost entirely throughout the distribution when the savings incentives are combined with other treatments.

The negative effects of combined treatments merit further investigation. One possibility is that by itself, savings induces owners to do more of what they are already doing, while in combination with either training or wage incentives, owners are encouraged to take more changes. Consistent with this story, we do see a crossover near the top of the distribution in the sales graphs for the combined treatments based on either savings or wage incentives. Both this and the employment size data in Table 8 are consistent with a few firms being induced to take off in new directions.

Section 5: Conclusions

To follow…

References (Incomplete):

Bruhn, Miriam and David McKenzie (2009) "In Pursuit of Balance: Randomization in Practice in Development Field Experiments", *American Economic Journal: Applied Economics*, 1(4): 200-232.

De Mel, Suresh, David McKenzie, and Christopher Woodruff (2008). "Returns to Capital: Results from a Randomized Experiment." *Quarterly Journal of Economics* 123(4): 1329–72.

De Mel, Suresh, David McKenzie, and Christopher Woodruff (2009a) "Are Women more Credit Constrained? Experimental Evidence on Gender and Microenterprise Returns." *American Economic Journal: Applied Economics* 1(3): 1-32.

De Mel, Suresh, David McKenzie, and Christopher Woodruff (2010a), "Who are the Microenterprise Owners?: Evidence from Sri Lanka on Tokman v. de Soto,", in *International Differences in Entrepreneurship*, Lerner and Schoar, eds., *University of Chicago Press, 2010.*

De Mel, Suresh, David McKenzie, and Christopher Woodruff (2010b) "Wage Subsidies for Microenterprises", *American Economic Review Papers and Proceedings*, 100(2): 614-18.

De Mel, Suresh, David McKenzie, and Christopher Woodruff (2012a) "The demand for, and consequences of, formalization among informal firms in Sri Lanka", *American Economic Journal: Applied Economics, forthcoming*.

De Mel, Suresh, David McKenzie, and Christopher Woodruff (2012b) "Business Training and Female Enterprise Start-up, Growth, and Dynamics: Experimental evidence from Sri Lanka," working paper, University of Warwick.

Fafchamps, Marcel, David McKenzie, Simon Quinn and Christopher Woodruff (2011) "Female Microenterprises and the Fly-paper Effect: Evidence from a Randomized Experiment in Ghana", Mimeo. World Bank.

Field, Erica, Rohini Pande, John Papp and Natalia Rigol, 2012, "Debt Structure, Entrepreneurship, and Risk: Evidence from Microfinance," Working paper Duke University.

McKenzie, David (2011) "Beyond baseline and follow-up: The case for more T in experiments", *Journal of Development Economics*, forthcoming.

Table 1: Assignment to Treatments

Intervention 2 \ Intervention 1	None	Savings	Training	Employment
None	287	112	141	250
Savings			150	297
Training		150		298
Employment		297	298	
Total	287	559	589	845

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	FE
VARIABLES	All rounds	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Rounds 6-9	Rounds 6-9
Assigned to Savings treatment	0.223	0.254	0.210	0.358	0.364	0.002	0.073	0.214	0.436
	(0.20)	(0.32)	(0.32)	(0.31)	(0.32)	(0.30)	(0.30)	(0.21)	(0.30)
Assigned to Wage subsidy treatment	-0.024	0.244	0.636**	-0.000	-0.489	-0.514*	-0.112	-0.275	-0.335
	(0.19)	(0.30)	(0.30)	(0.31)	(0.31)	(0.28)	(0.29)	(0.21)	(0.29)
Assigned to Training treatment	0.911***	1.710***	1.120***	0.696**	0.816**	0.515*	0.521*	0.644***	0.503
	(0.20)	(0.32)	(0.32)	(0.32)	(0.33)	(0.29)	(0.31)	(0.22)	(0.31)
Mean Dep Var Controls	8.67	10.62	8.28	9.29	8.88	8.11	6.62	8.23	8.23
Observations	7,551	1,305	1,284	1,242	1,272	1,241	1,207	4,962	6,540
R-squared	0.277	0.228	0.265	0.282	0.254	0.279	0.189	0.262	0.057
Number of sheno									1,525

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	FE
VARIABLES	All rounds	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Rounds 6-9	Rounds 6-9
Assigned to Savings treatment	15,593**	7,047	5,276	15,080*	16,556*	26,453**	24,126**	20,563***	28,454***
	(6,522)	(6,905)	(8,630)	(8,590)	(8,751)	(13,017)	(10,040)	(7,612)	(8,333)
Assigned to Wage subsidy treatment	7,392	10,219	10,352	11,135	1,529	11,893	-911	6,045	7,330
	(6,443)	(6,439)	(8,491)	(8,224)	(8,250)	(12,972)	(9,667)	(7,618)	(8,303)
Assigned to Training treatment	579	4,125	-2,865	3,456	-459	-776	-654	547	5,031
	(6,577)	(6,780)	(9,030)	(8,248)	(8,337)	(12,616)	(9,586)	(7,539)	(8,036)
Mean Dep Var Controls	345772	280685	313746	349471	359398	392458	402296	374938	374938
Observations	7,588	1,340	1,284	1,241	1,275	1,241	1,207	4,964	6,543
R-squared	0.282	0.354	0.315	0.322	0.336	0.228	0.263	0.269	0.013
Number of sheno									1525

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	FE
VARIABLES	All rounds	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Rounds 6-9	Rounds 6-9
Assigned to Savings treatment	0.134**	0.074	0.127*	0.197***	0.175***	0.098	0.134*	0.151***	0.125
	(0.05)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)	(0.08)
Assigned to Wage subsidy treatment	0.157***	0.265***	0.212***	0.081	0.175***	0.118*	0.076	0.114**	0.224***
	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.05)	(0.08)
Assigned to Training treatment	0.073	0.034	0.040	0.108	0.094	0.067	0.091	0.091	0.185**
	(0.05)	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)	(0.07)	(0.06)	(0.08)
Mean Dep Var Controls	0.53	0.48	0.53	0.56	0.45	0.60	0.59	0.55	0.55
Observations	7,591	1,340	1,285	1,244	1,275	1,241	1,206	4,966	5,827
R-squared	0.149	0.200	0.159	0.162	0.136	0.156	0.124	0.140	0.088
Number of sheno									1,438

Table 4: Effect of Treatments on Outputs

Panel A: Sales (Truncated at 99th percentile)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	FE
VARIABLES	All rounds	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Rounds 6-9	Rounds 6-9
Assigned to Savings	12,593**	8,780	12,721*	7,651	17,730*	20,664**	9,154	13,617**	15,627**
treatment	(5,753)	(6,065)	(7,117)	(6,889)	(9,062)	(9,751)	(10,216)	(6,819)	(7,317)
Assigned to Wage	1,453	8,552	6	-2,236	-978	8,455	-5,091	40	-5,325
subsidy treatment	(5,279)	(5,351)	(6,304)	(6,553)	(8,273)	(9,187)	(9,362)	(6,363)	(6,739)
Assigned to Training	5,064	-956	-5,722	9,218	12,078	3,683	12,757	9,558	10,578
treatment	(5,217)	(5,243)	(6,035)	(7,005)	(8,279)	(8,797)	(9,945)	(6,409)	(6,819)
Mean Dep Var Contro	81873	62378	75714	78686	80759	98067	98426	88923	88923
Observations	7,427	1,310	1,254	1,207	1,257	1,219	1,180	4,863	6,476
R-squared	0.198	0.233	0.197	0.225	0.163	0.247	0.172	0.194	0.073
Number of sheno									1,524

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	FE
VARIABLES	All rounds	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Rounds 6-9	Rounds 6-9
Assigned to Savings	295	-558	1,279	224	537	449	5	264	589
treatment	(738)	(849)	(1,314)	(982)	(1,077)	(1,109)	(1,491)	(835)	(966)
Assigned to Wage	-483	-117	-292	-930	626	16	-2,286	-626	-493
subsidy treatment	(688)	(793)	(1,152)	(931)	(1,030)	(1,071)	(1,437)	(803)	(932)
Assigned to Training	147	926	-694	-969	420	254	890	144	620
treatment	(706)	(819)	(1,133)	(930)	(1,061)	(1,081)	(1,533)	(835)	(954)
Mean Dep Var Contro	22215	17103	20465	21683	21812	25042	27890	24090	24090
Observations	7,483	1,317	1,254	1,216	1,269	1,234	1,193	4,912	6,450
R-squared	0.164	0.170	0.148	0.197	0.115	0.185	0.139	0.156	0.105
Number of sheno									1,522

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	ANCOVA	FE
VARIABLES	All rounds	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Rounds 6-9	Rounds 6-9
Assigned to Savings	60	-89	2,062	504	-446	277	-1,875	-429	1,048
treatment	(940)	(985)	(1,823)	(1,097)	(1,664)	(1,396)	(1,876)	(1,072)	(2,308)
Assigned to Wage	-1,010	-257	-76	-1,039	-1,164	-597	-3,256*	-1,460	-5,323**
subsidy treatment	(888)	(914)	(1,636)	(1,043)	(1,592)	(1,329)	(1,832)	(1,050)	(2,244)
Assigned to Training	-216	1,145	-1,991	-871	-425	160	604	-137	2,519
treatment	(896)	(938)	(1,512)	(1,043)	(1,580)	(1,343)	(1,966)	(1,079)	(2,291)
Mean Dep Var Contro	31631	22837	30505	27007	35040	33588	41932	34388	34388
Observations	7,398	1,305	1,250	1,206	1,249	1,215	1,173	4,843	6,426
R-squared	0.171	0.197	0.158	0.205	0.129	0.165	0.137	0.158	0.035
Number of sheno									1,518

Table 5: Interaction of Treatments

VARIABLES	(1) Paid workers		(2) Inventories		(3) Sales		(4) Profits	
Assigned to Savings treatment	0.122		65,236***		31,764*		2,866	
	(0.12)		(24,527)		(18,122)		(1,864)	
Assigned to Wage subsidy treatment	0.054		4,817		9,510		536	
	(0.08)		(10,901)		(9,797)		(1,396)	
Assigned to Training treatment	0.123		24,305*		18,460		146	
	(0.12)		(13,641)		(13,543)		(1,711)	
Assigned to Savings and Wage treatments	0.110	0.286***	-35,038	35,015***	-25,571	15,703*	-3,999*	-596
	(0.15)	(0.090)	(27,931)	(10,217)	(21,223)	(9,477)	(2,274)	(1,256)
Assigned to Savings and Training treatments	-0.071	0.175	-88,593***	947	-24,242	25,982**	-1,989	1,023
	(0.18)	(0.109)	(28,541)	(9,936)	(24,572)	(11,951)	(2,688)	(1,613)
Assigned to Wage and Training treatments	0.021	0.198**	-8,971	20,150*	-13,522	14,448*	-409	273
	(0.14)	(.087)	(18,692)	(11,205)	(16,527)	(8,227)	(2,164)	(1,247)
Observations	4,966		4,964		4,863		4,912	
R-squared	0.141		0.274		0.195		0.157	

Indicates level and significance of the indicated combination of treatmentsin the regression to the left.

Table 6: Treatment Effects by Sector

VARIABLES	(1) Paid workers	(2) Inventories	(3) Sales	(4) Profits	(5) Paid workers	(6) Inventories	(7) Sales	(8) Profits
	Retail Sector				Manufacturing / Services Sector			
Assigned to Savings treatment	0.135*	39,095**	36,232**	1,962	0.164**	11,265	3,377	-517
	(0.08)	(15,385)	(14,591)	(1,391)	(0.08)	(7,656)	(6,500)	(1,008)
Assigned to Wage subsidy treatment	-0.018	16,913	1,583	-1,746	0.200***	4,067	265	323
	(0.08)	(16,870)	(13,072)	(1,316)	(0.07)	(7,107)	(6,337)	(982)
Assigned to Training treatment	0.157*	15,902	16,547	1,633	0.056	-5,761	6,955	-582
	(0.08)	(16,406)	(12,789)	(1,383)	(0.08)	(6,655)	(6,573)	(1,022)
Observations	1,855	1,855	1,816	1,832	3,111	3,109	3,047	3,080
R-squared	0.103	0.303	0.169	0.181	0.173	0.186	0.211	0.164

Table 7: Treatment Effects by Owner's Ability Level								
	Panel A: Below Median Ability							
	(1)		(2)		(3)		(4)	
VARIABLES	Paid workers		Inventories		Sales		Profits	
	0.019		9,304		-3,290		-363	
Assigned to Savings treatment	(0.15)		(12,123)		(10,777)		(2,224)	
Assigned to Wage subsidy	0.170		10,297		15,925		2,564	
treatment	(0.13)		(14,907)		(11,676)		(2,257)	
	-0.072		29,963*		2,537		-1,637	
Assigned to Training treatment	(0.13)		(17,132)		(16,974)		(2,363)	
Assigned to Savings and Wage	0.036	0.225*	-9,114	10,487	-212	12,423	-2,426	-224
treatments	(0.20)	(0.125)	(19,044)	(10,159)	(18,301)	(13,369)	(3,175)	(1,768)
Assigned to Savings and	0.297	0.243	-37,040*	2,227	32,537	31,784	3,038	1,039
Training treatments	(0.23)	(0.166)	(21,102)	(11,680)	(26,848)	(19,527)	(3,517)	(2,190)
Assigned to Wage and	0.137	0.235	-24,432	15,828	-4,748	13,715	-694	223
Training treatments	(0.20)	(0.142)	(21,537)	(11,298)	(20,747)	(10,323)	(3,171)	(1,661)
Observations	2,258		2,258		2,211		2,235	
R-squared	0.152		0.271		0.194		0.146	
	Panel B: Above median ability							
	(5)		(6)		(7)		(8)	
VARIABLES	Paid workers		Inventories		Sales		Profits	
	0.195		104,975***		53,487*		5,160*	
Assigned to Savings treatment	(0.18)		(40,112)		(31,554)		(2,791)	
Assigned to Wage subsidy	-0.028		591		3,385		-955	
treatment	(0.10)		(15,349)		(15,584)		(1,799)	
	0.332*		22,755		34,041*		2,235	
Assigned to Training treatment	(0.19)		(20,622)		(19,920)		(2,396)	
Assigned to Savings and Wage	0.175	0.342***	-54,516	51,050***	-41,457	15,415	-5,304	-1,100
treatments	(0.21)	(0.130)	(45,143)	(16,212)	(35,068)	(13,506)	(3,247)	(1,745)
Assigned to Savings and	-0.379	0.148	-134,794***	-7,063	-65,773*	21,755	-6,472	923
Training treatments	(0.27)	(0.146)	(46,011)	(14,870)	(39,016)	(15,768)	(3,931)	(2,300)
Assigned to Wage and	-0.124	0.179	-2,315	21,031	-25,849	11,577	-1,093	187
Training treatments	(0.21)	(0.116)	(28,680)	(17,530)	(24,738)	(12,399)	(2,955)	(1,772)
Observations	2,708		2,706		2,652		2,677	
R-squared	0.143		0.293		0.207		0.169	
Ability measured bythe first PC of Raven, Digitspan and years of schooling								

Table 8: Distribution of Firm Size by Employment			
Number of paid employees	Control Group	Wage Incentives	Savings Incentives
0	71.1%	67.3%	68.0%
1	16.3%	18.7%	17.0%
2	7.2%	6.7%	6.9%
3	2.6%	2.7%	2.4%
4	1.3%	2.1%	2.8%
5 or more	1.6%	2.4%	2.9%
2 or 3 Employees	9.8%	9.4%	9.3%
4 or more Employees	2.8%	4.5%	5.7%
Number of enterprises	197	628	394
Sample of enterprises with no employees at baseline			

Figure 1: CDFs of Sales for rounds 6 through 9: Control vs. Treatments

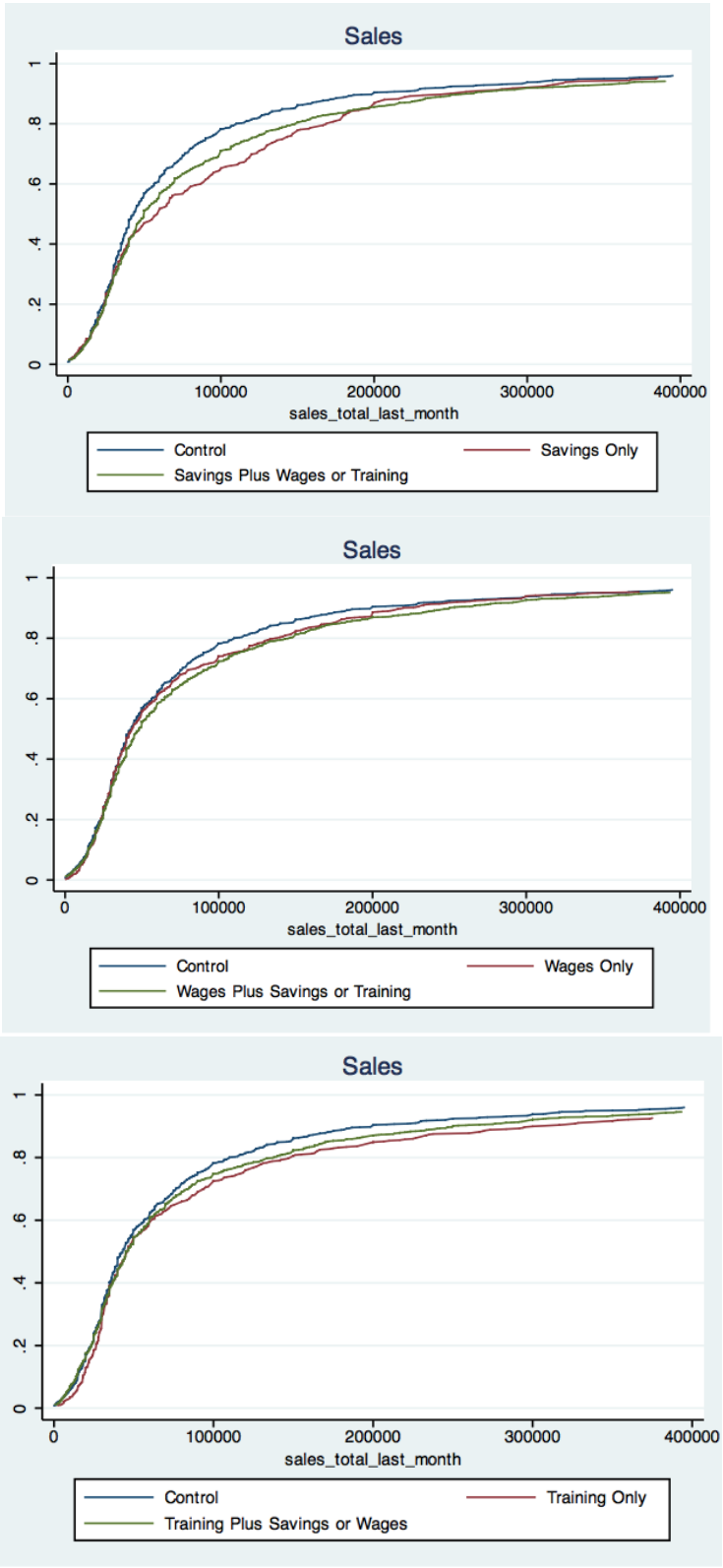

Figure 2: CDFs of Profits for rounds 6 through 9: Control vs. Treatments