Business Training and Female Enterprise Start-up, Growth, and Dynamics:

Experimental evidence from Sri Lanka[*]

Suresh de Mel, *University of Peradeniya*

David McKenzie, *World Bank*

Christopher Woodruff, *University of Warwick*

May, 2012

Abstract

We conduct a randomized experiment in Sri Lanka to measure the impact of the most commonly used business training course in developing countries, the Start-and-Improve Your Business (SIYB) program. In contrast to existing business training evaluations which are restricted to microfinance clients, we consider two more representative groups: a random sample of women operating subsistence enterprises, and a random sample of women who are out of the labor force but interesting in starting a business. Both samples are randomized into three groups: a control group, a group invited to attend training, and a group invited to receive training and who receive a cash grant conditional on completing training. We track impacts over four rounds of follow-up surveys taken over two years and find that the short- and medium-term impacts differ. For women already in business, we find that although training alone leads to some changes in business practices, it has no impact on business profits, sales or capital stock. In contrast the combination of training and a grant leads to large and significant improvements in business profitability in the first eight months, but this impact dissipates in the second year. For women interested in starting enterprises, we find that business training speeds up the process of opening a business, and changes the selection of who operates a business by making the entrants less analytically skilled, but leads to no increase in net business ownership by our final survey round. Receiving a grant results in poorer women opening businesses, but again does not increase net business ownership. Training appears to have increased the profitability and business practices of the businesses started up, suggesting it may be more effective for new owners than for enhancing existing businesses.

Keywords: Business training; female self-employment; randomized experiment; business start-up; trajectory of treatment effects.

JEL codes: O12, J16, L26, M53.

---

1. Introduction

Self-employment accounts for a large share of female employment in most developing countries, and is considered an important avenue for women's economic empowerment. However, the majority of female-owned enterprises are small in scale with commensurately low earning levels (Kevane and Wydick, 2001; Banerjee and Duflo 2008; de Mel et al, 2009a). Moreover, in much of South Asia and the Middle East, the majority of women are not even employed at all. These facts raise the central questions for this paper, which are whether business training, alone or combined with a grant, can raise the incomes of women in these low-earning firms, and whether it can allow women outside the labor force to start new businesses.

In previous experiments in Sri Lanka (de Mel et al, 2008, 2009a) and in Ghana (Fafchamps et al, 2011) we have found that physical capital alone has not been enough to raise the incomes of subsistence-level female businesses. One potential reason for this is that labor market imperfections may draw women with low levels of business skills into self-employment rather than wage work (Emran et al, 2007), resulting in their optimal efficient scale being very low. A common policy response is then to try and improve the level of skills these women have, through business training. As a result, business training programs have been used for more than 30 years as a development policy.

We evaluate the impact of the most commonly used training course in developing countries, the International Labor Organization (ILO)'s Start-and-Improve Your Business (SIYB) program, and test whether the impact of training alone differs from that of training coupled with access to capital in the form of a grant. We do this through randomized experiments in urban Sri Lanka with two samples of women. The first sample consists of 624 women who were operating a business at the time of the project baseline and earning a subsistence income of around $1 per day on average. The second sample consists of 628 women who were out of the labor force at baseline, but who expressed interest in starting a business within the next year. Both samples are randomized into three groups: a control group, a group invited to attend training, and a group invited to receive training and who receive a grant of around $130 conditional on completing training. Individuals are only told whether or not they have received the grant at the completion of training, so that the grant does not differentially alter the composition of who attends training between the two groups.

We find that although training alone leads to some changes in business practices for firms in business at the start of the survey, it has no impact on business profits, sales or capital stock. In contrast the combination of training and a cash grant leads to large and significant short-run improvements in business performance for these current enterprises, but these gains appear to have dissipated in the medium-term.

For women who were out of the labor force, training and especially training combined with a grant speeds up the process of starting a business. We find the women assigned to either training more likely to be operating a business 4 months and 8 months after the training course, but that this impact has disappeared by 16 months and 25 months. In addition to speeding up entry, the treatments affect the selection of who enters, with training causing women with lower initial analytical ability to start opening businesses, and the addition of the grant also resulting in poorer women opening businesses. Although no more businesses have opened on net by the end of two years, the businesses that are opened by the treated firms are employing more business practices that the training focuses on, and earning higher profits. Business training therefore appears to be enabling more successful businesses to be started.

This study contributes to an existing and concurrent literature which has conducted business training experiments with women (or with men and women) in developing countries. Examples include Field et al. (2010) in India, Karlan and Valdivia (2011) and Valdivia (2011) in Peru, Drexler et al. (2011) in the Dominican Republic, Berge et al. (2011) in Tanzania, Bruhn and Zia (2011) in Bosnia-Herzegovina, and Gine and Mansuri (2011) in Pakistan. These studies typically work with microfinance clients who are mostly existing business owners, and conduct a single follow-up survey to measure outcomes six months to one year after training occurred. The stylized finding is that training leads to improvements in business practices, but no detectable effects on profitability or employment, with Karlan and Valdivia (2011) and Drexler et al. (2010) finding some evidence to suggest training may help improve sales in bad weeks or bad months, even if there is no impact on the average month.[1]

Our study builds on and differentiates itself from this extant literature in several key respects. First, ours is the only study we are aware of to consider the impact of business training on a general population of female business owners, and on potential business owners, rather than

---

[1] Larger effects have been found for related studies that look at much more intensive interventions, which include one-on-one consulting (Bloom et al, 2011; Bruhn et al, 2012; Valdivia, 2011).

just on existing microfinance clients. This is important for generalizability, as well as for understanding the potential demand for such training in a general population who are not implicitly or explicitly compelled to participate by their lender. Second, while the content of many of these training courses seem similar in broad terms, it can be sometimes be difficult to judge the quality of customized training programs developed by microfinance organizations. In contrast, the ILO's SIYB program we evaluate has been used to train over 1.5 million people in more than 95 countries worldwide, so learning about its impact is of broad interest. Third, we measure outcomes at four points in time post-training. This enables us to examine the trajectory of impacts, which turns out to be very important in practice - especially for potential business owners for whom we would infer very different impacts of the training in the short-term vs medium-term. Finally, the use of grants along with training enables us to test whether the impact of business training differs with access to capital or not.[2]

The remainder of the paper is structured as follows: Section 2 describes the sample and randomization procedure; Section 3 the content of the training and determinants of training take-up; Section 4 provides a simple model for thinking about how training and grants might affect the selection of who operates a business; Section 5 provides our main results; and Section 6 concludes.

2. Context, the sample and randomization

The urban labor force participation rate for women aged 20 to 40 in Sri Lanka was only 38 percent in 2009, compared to rates over 90 percent for prime-aged men (Department of Census and Planning, 2010). Twenty-eight percent of women in paid work are self-employed, but most of these enterprises are small and have low earnings levels. Our own representative survey of 795 female enterprises in 2008 in the 31 largest urban areas found median profits only 5000 Rs per month (US$43), and showed that only 5 percent of female firms had any paid workers. The most common activities, accounting for 40 percent of the women earning below the median profits are the female-dominated industries of apparel manufacture (sewing) and making food products to sell to local markets. Given this context, our goal was to investigate the ability of business training to bring more women into the labor force, and its ability to raise the income

---

[2] Berge et al. (2010) also consider the impact of training plus grants. However, their sample is of microfinance clients, who may be less credit constrained in the first place, and the grants they give average only approximately 20 percent of monthly profits (compared to 375% of mean monthly profits in our case), making it unlikely that the grants they give are sufficiently large to have an effect (and they find none).

levels of low-earning women already in business. The first step in this process therefore involves putting together samples of these two groups.

2.1 Generating a sample

We chose greater Colombo and greater Kandy as the locations for this study, as the largest urban areas in Sri Lanka, and as places were the training organization had sufficient venues to provide the training.[3] Within these cities we chose 10 D.S. divisions (each the location of one training venue), and then employed a door-to-door screening survey in randomly selected G.N.s (the smallest sampling unit) to identify our sample.[4]

For women already self-employed, our screening survey consisted of identifying women who were aged 25 to 45, who worked more than 20 hours per week in self-employment, were involved in a sector other than seasonal agriculture or fisheries, and had monthly profits of 5000 Rs or less (the median in the general population of female self-employed). We refer to this as our *current enterprise* sample.

The second group of interest was women who were out of the labor force, but who were interested or likely to enter the labor force in the next year. We again focused on women aged 25 to 45, since we believed older women would be less likely to enter the labor market. The screening survey then asked directly whether the woman planned to enter self employment in the next year, as well as the nature of the business that she planned to get into. If the respondent was able to identify the type of business that she planned to commence she was included in the sample. Since the ability to participate in full day business training program was important for the proposed intervention, respondents were also asked about the availability of child care for those with children younger than 5 years of age. If they had access to other arrangements for child care they were included in the sample.[5]. We refer to this as our *potential enterprise* sample. Based on this screening exercise, we selected a sample of 628 current enterprises and 628 potential enterprises. This sample was stratified to take approximately 62 of each type of enterprise per D.S., in order to have equal sized groups at each training location. A baseline survey of these 1256 individuals was conducted in January 2009 (see timeline in Figure 1).

---

[3] The study includes Colombo, Kalutara, Gampaha, Kandy, Matale, Kegalle and Kurunegala districts.
[4] There are four administrative levels in Sri Lanka: Provinces (9), Districts (25), Divisional Secretariat (DS) Divisions (324), and Grama Niladari (GN) Divisiona (14,008).
[5] If the respondents were unmarried *or* married with no children *or* married with children aged 5 years or older (school going age) they were included in the sample. For those with children younger than 5 years of age they were asked about the availability of child care arrangements. If they had access to other arrangements for child care they were included in the sample. If they did not have access to other arrangements, they were screened out.

Table 1 provides summary statistics on the sample used for the experiment. The current enterprises sample consists of women in a wide range of industries typical of female self-employment in Sri Lanka, such as tea shops, beauty shops, bag and mat manufacturing, tailoring and sewing, fruit and vegetable selling, selling groceries, making and selling lunch packets and string hoppers, baking cakes, and flower shops. The typical owner in our sample is 36, married, with 10 years of education, and has been running the business for just over 6 years. Mean monthly business income is around 4,000 Rs (US$34, or around a dollar a day at market exchange rates), and constitutes about one-quarter of total household income for the median and mean firm owners. At baseline few enterprises are implementing the types of business practices that the IYB program is intended to get businesses to implement. Our business practices index measures the marketing, costing and record-keeping, stock control, and financial planning practices used in the firm, with higher scores indicating more practices (see appendix 1). The mean firm has a baseline score of only 4.6, when the maximum possible is 29. Only 17 percent of firms keep written records, only 4 percent had done any advertising in the last 6 months, only 9 percent have a sales target for the next year and only 3 percent had made a budget of what costs are likely to be over the next year.

Only 18 percent of these current business owners reported ever having had any form of previous business training, and the majority of this was technical training. Less than 4 percent of owners had ever taken a training course which covered record-keeping, marketing, customer and supplier relations, or management of employees. When asked why they did not use business training or consulting services more often, 61 percent said that not enough time was a moderately important or very important reason, and 58 percent said not being able to afford the cost was – compared to only 30 percent thinking it wouldn't be profitable and 26 percent thinking it involved too much risk in terms of the potential payoffs.

Among the potential enterprises, only 18 percent have never worked before, but only 8 percent have previously engaged in self-employment. Half of them claim to have taken concrete steps towards opening a business in the past year. These women are about two years younger on average than the women with current enterprises, but are otherwise similar in many respects. In particular, the potential and current enterprise women have similar levels of education, similar cognitive ability (as measured by digitspan recall and raven tests), similar attitudes towards risk, and similar numbers of children on average. Their households earn about 1,100 Rs less per

month than the households with women with current enterprises, and they are a bit less likely to have fridges or sewing machines in their houses. In terms of household assets that could potentially also be used for business use, 53 percent live in households with a sewing machine, and 41 percent in households with a fridge, but only 25 percent have a gas cooker and only 7 percent an oven.

2.2 Randomization into treatment and control groups

Our goal was to determine the impact of business training on these firms, and to see whether this training had more effect when coupled with a capital grant. We therefore randomly selected 400 current enterprises and 400 potential enterprises to be offered business training, and half of these to also receive a grant of 15,000 Rs (US$129) conditional on finishing the training. We wanted to use the grant as an incentive to attend training and did not want the selection of people into training to differ in the group assigned to receive a grant from those assigned to receive training only. Therefore at the time of offering the business training, individuals were told only that half of those who complete the training (evidenced by receiving the training completion certificate which requires 80% attendance) would be randomly chosen to receive a 15,000 Rs grant.

For the current enterprises we dropped 4 enterprises with either missing profits or profits above 10,000 Rs.[6] Randomization to treatment was then stratified by D.S. (10) and by whether or not they either had no children under 18 or if they had children under 18 they had someone who could look after them while they worked (54%). Within each stratum we then formed triplets of firms on the basis of baseline profits, and randomly allocated 2 out of 3 firms in each triplet to receive training, and one of these two to get a grant conditional on completing training, subject to a quota of 40 firms per D.S. getting chosen to be offered training.[7]

For the potential enterprises, in addition to D.S., we stratified on whether or not they had taken any steps in the past year towards starting a business (50% said they had), and whether they had never worked at all before, even as an unpaid family worker (18% had not). Within these 38 stratum we then randomly allocated one-third to training only, one-third to training and

---

[6] The screening survey asked about profits in a "normal" month, while the baseline survey asked about profits in a specific month. All women in the sample had stated that their profits were below 5000 Rs in the screening survey, but 17% of these reported profits above 5000 Rs in the more detailed baseline survey referring to December 2008. Most of these were in the 5000-7000 Rs range.

[7] This form of randomization is done to increase power (see Bruhn and McKenzie, 2009). We then always control for the randomization strata in our analysis.

a grant conditional on training, and one-third to control, again subject to a quota of 40 firms per D.S. getting chosen to be offered training.

Randomization was done by computer, so any differences are due to pure chance. Table 1 shows that randomization succeeded in generating groups with similar observable characteristics.

3. **The training and take-up**

3.1 Training content

The training program we use is the International Labour Organization (ILO's) Start and Improve Your Business (SIYB) program. This program was designed to meet the needs of small-scale entrepreneurs in developing countries, and started in Eastern Africa in 1977 (ILO, 2003). It currently has an estimated global outreach of 1.5 million trainees[8], and has been implemented in over 95 countries worldwide. In each country the materials are customized to local languages and context. We use the three main packages in the SIYB program:

- Generate Your Business (GYB) is a 3 day training course designed to assist potential entrepreneurs to find out if they are the right person to start a business, to generate feasible business ideas, and to choose the best idea from among this set of choices.

- Start Your Business (SYB) training deals with potential entrepreneurs who want to start their own business and already have a feasible business idea. The training is a 5 day course which aims to cover the main aspects needed to start a business, including what products to sell and how to decide on their prices, organizing the staffing, equipment and other inputs needed to get started, costing, deciding on the legal form of the business, and financial planning. The end result is a business plan that potential entrepreneurs can use as a step-by-step guide to starting their business.

- Improve Your Business (IYB) training is a 5 day course which deals with existing business owners who want to develop their business. It has modules on marketing, buying, costing, stock control, record keeping, and financial planning.

In addition to these core modules, we concluded the program with one day of training of a more technical nature. The purpose of this was to provide some exposure to, and training in, some relatively high-return sectors that are also socially acceptable for women to engage in. This included food manufacturing, beauty culture (which could include hair dressing, beauty

---

[8] http://www.ilo.org/empent/Whatwedo/Projects/lang--en/WCMS_143335/index.htm

treatments and bridal dressing), sewing clothes, plant nursery, and soap manufacturing. Based on participant interest, at least two of these were on offer at each training location on that day and participants could select their preferred technical training option. The majority attended food manufacturing (40%), sewing clothes (26%) or beauty culture (17%).

We contracted the Sri Lanka Business Development Centre (SLBDC), a Sri Lankan non-profit training institution established in 1984, to provide the business training.[9] SLBDC was involved in the introduction of the SIYB program to the Sri Lankan market in 2001, and is a leading partner organization of this program. At the time of our intervention it had 8 years of experience delivering this content to the local market. They typically provide the course to groups of participants brought to them by a variety of local and international partners. Examples included groups of tsunami-affected individuals, microfinance clients, beneficiaries of social foundations, and members of industry associations. All of the SLBDC trainers are university qualified (and some of them also have postgraduate level training). They have all been trained under the national-level SIYB Training programs conducted by the ILO and are registered with the SIYB Association of Sri Lanka as certified SIYB trainers. They have been involved in SIYB training for over 5 years. As such, any failure to find impacts should not be due to low quality trainers or inexperience with the materials.

The potential enterprises in our sample were offered a package consisting of the GYB and SYB courses, followed by the technical training. Those selected were sent a letter explaining the details of the course and the offer, which was followed up by in-person visits to each individual in the treatment groups and phone calls to make sure they had received and understood the training offer. The resulting course was 9 days, with 7 hours of instruction per day. The current enterprises were offered a package consisting of a compacted refresher GYB course to get them to think about new potential products they could make or sectors they could work in, followed by the IYB course and technical training. Overall this amounted to 7 days of training, with 7 hours of instruction per day. The training locations were very central and easy to find within each D.S., and close to public transport routes.

The cost of the training to us was between $126-$131 per current enterprise owner and $133-$140 per potential enterprise owner trained. This was offered to participants for free, which mimics the approach used by many NGOs and microfinance institutions. In the baseline

---

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

participants were asked their interest in subsidized business training and the amount they would be willing to pay to attend a general business training course. The majority (89% of potentials, 80% of current) said they were interested, but the expressed willingness to pay was low: the median was 500 Rs for both groups, and 82% of potentials and 85% of current business owners said the most they would pay would be 1000 Rs or less. Since we did not use an incentive-compatible revelation mechanism, this could well be an understatement of true willingness to pay, but it is still notable in that it is an order of magnitude much lower than what the market cost would be.

Most existing evaluations of business training programs have used microfinance clients as their target group. Since group members are used to meeting regularly, and since the training is something that their lending organization is explicitly suggesting that they do, such experiments are typically able to achieve quite high take-up of the training offered. It is a much more open question whether the average potential or existing business owner will show up for training if offered it.

We began by offering the training to an initial group of 40 of the potential enterprises in 2 D.S. locations. This offer did not mention the cash grants at all (since they were still being finalized at the time), participants were told the training was free, and that they only needed to pay transportation costs to the nearby training venues. Only 7 out of the 40 women who were offered this training showed up, with others giving reasons such as illness, no one to look after their children, or lack of interest. We therefore revised our offer to also include a 400 Rs per day attendance payment to cover the cost of basic transport (estimated at 50 Rs per day) and the opportunity cost of attending. This was explained in personalized visits to each individual in the treatment group, and with financing for the grants finalized, we also told them that half of those who complete training would be randomly chosen for a 15,000 Rs grant. This information was given to all individuals in this first training group, as well as to the other individuals selected for training.

3.2 Take-up of Training

Training then took place in April and May 2009, with the cash grants being given out in June 2009. There was very little drop-out among those who began the courses, and take-up rates were similar among the potential and current groups. Overall 282 of the 400 potential business owners (70.5%) offered training attended at least one training session, and 261 individuals

completed it (65.3%). Among the 400 current business owners offered training, 279 attended at least one session (69.8%) and 268 individuals completed it (67.0%).

When we directly asked those who did not attend the main reason why they didn't attend, the most common answer among current enterprise owners was that they or a family member was sick (25%), that they had no one to look after their business while doing training (17%), and that they had no one to look after their children (16%). Only 4.5% said it was because they didn't think the training would be valuable and 3.6% said they couldn't get transport to the location. Among the potential owners, the most common answers for not attending were again sickness of themselves or a family member (23%), not finding anyone to look after their children (20%), and having to help children with school work (17.6%), with less than 1% reporting it was due to transportation issues, to not thinking the training would be valuable, or to changing their mind about wanting to start a business. However, one should treat these responses with some skepticism, since people who are not interested may think it is easier to just say they were sick or had to look after others than to say they weren't interested. We therefore turn to probit analysis of take-up.

The first three columns of Table 2 examine which current enterprises attended the training amongst those offered it. Column 1 shows married and more educated women, running younger firms, were more likely to attend the training. Contrary to our prior expectation, whether or the firm owner has no children or has someone to look after children is not significantly associated with attendance. Owners of manufacturing and retail firms were more likely to attend than services firms. Baseline business practices do not predict take-up, so it is not that some firms are already doing all these practices and choose not to go. There is no association between baseline elicited willingness to pay for such training and whether women actually attend the training. The opportunity cost of time seems to matter, in that women earning higher profits at baseline are less likely to attend, as are women working more than 40 hours per week, and those from wealthier households. Column 2 then adds controls for being in Colombo or Kandy districts, while Column 3 adds fixed effects at the D.S. level for the 10 D.S.s. Only 34 percent of those invited completed training in Colombo, compared to 69 to 80 percent completion rates in the other districts. These Colombo firms earned higher profits, and controlling for district eliminates the predictive effect of profits, but does not change the size or significance of the other variables.

The last three columns examine take-up for potential entrants. We find that take-up increases in the age of the woman, and in the score on a Raven non-verbal reasoning test, but is unrelated to household wealth, expressed willingness to pay for training, marital status, or willingness to take risks. As with existing enterprises, take-up is lower in Colombo than in Kandy or other districts.

The individuals who attended the course appeared happy with it in qualitative interviews and in direct survey questions. In the round 2 follow-up, among those who had taken the course, 78% of current enterprise owners said they would strongly recommend it to a friend currently running a business and an additional 17% would somewhat recommend it, and 86% said that the course was more helpful than they had expected. Similarly 81% of potential owners would strongly recommend it to someone starting a business and 17% would somewhat recommend it, and 85% said it had been more helpful than they had expected.

3.3 Follow-up surveys

Four rounds of follow-up surveys were conducted in September 2009, January 2010, September 2010, and June 2011 - which corresponds to 3-4 months, 7-8 months, 15-16, and 24-25 months after the training occurred. We refer to these as the rounds 2, 3, 4 and 5 surveys respectively. These follow-up surveys asked detailed information about business outcomes, including the key performance measures of business profits in the last month, sales in the last month, and capital stock (including raw materials and inventories). Business profits were asked directly, following the recommendations of de Mel et al. (2009b). Nominal values were converted into real values using the Colombo consumer price index.

Overall attrition was low – of the 624 (628) current enterprises (potential enterprises) selected for the experiment, 584 (588) were interviewed in the second round, 591 (587) in the third round, 580 (560) in the fourth round, and 575 (556) in the fifth round surveys. We cannot reject equality of attrition rates across treatment groups by round 5 for the current enterprises (p=0.25), but attrition rates are slightly lower for the training only group amongst the potential enterprises so a test of equality of attrition rates by round 5 has p-value of 0.085 for this group. Appendix 2 shows our potential sample results are robust to this attrition. In addition, in the case of refusals or inability to conduct an interview in the fifth round, proxy reports and direct observation were used to assess whether or not the owner was running a business, with information on this outcome for 97 percent of firms.

4. Theory: how might business training and grants affect who runs a business and the profitability of these businesses?

Consider a simple model of occupational choice.[10] In this model, individuals are endowed with wealth A, and innate entrepreneurial ability, θ. If they pay a start-up cost $x(\theta)$, which is declining in θ, they are able to combine their single unit of labor, capital k, and their ability to produce according to the common production function $f(k, \theta)$. The production function is assumed to be strictly increasing in both arguments, and strictly concave. We assume there is no borrowing, so individuals must finance the start-up cost out of their initial wealth. Alternatively, if they do not set up an enterprise, they can work and earn wage labor w, or opt out of the labor market and obtain wage-equivalent utility of w from leisure and household production.[11] Any wealth not invested in the business earns return r, which reflects either the interest rate on savings or the opportunity cost of funds for household uses. We assume r is low enough that it is worthwhile starting a business for individuals with sufficient wealth and ability. Let e denote the entry choice, with $e=1$ if they start-up a firm, and $e=0$ otherwise. Then the individual's problem is to choose e and k to maximize:

$$ef(k, \theta) + (1 - e)w + r\left(A - e\left(k + x(\theta)\right)\right)$$

$$\text{s.t. } k + x(\theta) \leq A$$

Let θ^* denote a high-level of ability such that $x(\theta^*)=0$, and A^* denote a high-level of wealth such that $x(0)=A^*$. i.e. very high ability individuals can enter with zero wealth, and very high wealth individuals can enter with zero ability. Assume also that $f(0, \theta^*) > w$ so that talented individuals find it profitable to enter even if they have no wealth, but that $f(0,0) < w$ so that individuals who have no talent and only enough wealth to pay the start-up costs but not buy any capital k do not want to enter. Then let A^{**} be the minimum level of wealth to make it worthwhile entering with zero ability, defined by $w + rA^{**} = f(A^{**} - A^*, 0)$.

Figure 2 then shows the occupational choice by ability and wealth level. Individuals with relatively low levels of wealth or entrepreneurial ability are "no-choice" wage workers- they are unable to meet the start-up costs of opening a firm, and so must be wage workers (or household production workers). Workers with an intermediate range of ability and wealth are wage workers

---

[10] This model is related to that in Lloyd-Ellis and Bernhardt (2000), but one key difference is that we allow not just the start-up cost of operating an enterprise, but also the output produced conditional on opening, to depend on θ.

[11] Since we are focused on the decision of whether or not to operate a business, we abstract from the decision of whether to be in wage work versus exit the labor force.

by choice – they could afford to open a business, but find wage or household work more rewarding given that they do not have sufficient wealth or ability to earn large profits in any business they would open. Workers with either high wealth and/or high ability become self-employed.

Consider then the impact of business training on business entry in this model. Suppose business training builds business skills by amount T, so that now individuals only need talent θ^*-T to enter with zero wealth. Figure 3 shows that this lowers the innate talent required to enter entrepreneurship, and as a result we should see individuals who previously were of too low an ability level given their wealth to either be able to set up a business or to want to set up a business now starting a business. Since the new owners attracted into business include both relatively poor but high ability individuals (lower right of the shaded area), and relatively wealthy but low ability individuals (upper right of the shaded area), whether or not the average innate skill level of business owners will rise or fall will depend on the distribution of wealth and ability in the underlying population. Likewise giving individuals a cash grant on top of this training will reduce the amount of initial assets required to start a business, rotating the two curves down so that they cross the wealth index at lower points, drawing into business individuals who were too poor given their abilities. As a result we should expect the training and grants to likewise affect the selection of who operates a business.

This model is a one-period model for simplicity of exposition. If we allow individuals to save their wage earnings over multiple periods, then the long-run impact on entry will be less than the short-run impact, since credit-constrained high ability individuals should over time save their wage earnings and enter. As a result the longer-term impact on entry will come from individuals for whom it is profitable to run a business with skill level $\theta + T$, but who would prefer wage work if their skill level is θ. That is, training should bring into business ownership individuals with relatively low initial ability levels.

What about individuals who would have started businesses even in the absence of training? Initially these individuals would have chosen capital stock k^* such that $f'(k^*, \theta) = r + \lambda(A, \theta)$ where $\lambda(A, \theta)$ is the lagrange multiplier which determines how credit-constrained the individual is. Now these individuals will choose capital stock k^{**} given by $f'(k^{**}, \theta + T) = r + \lambda(A, \theta + T)$. Higher entrepreneurial skills reduces the start-up cost, giving them more money they could invest in either the business or at the opportunity cost of funds r. If capital and skills

14

are complements in production, then they will wish to invest more, and so $k^{**} > k^{*}$ and profitability of these firms who would have started anyway will increase. In contrast, if capital and skills are substitutes in production, individuals may invest less capital in the business and more in the outside option, and then whether or not business profits go up or down will depend on the shape of the production function.

Taken as a whole, business training should therefore change the selection among potential business owners of who opens a business, and the impact on average profits for those who do open a business is theoretically ambiguous due to this selection effect and an uncertain effect for those who would open a business anyway.

Finally, consider existing firm owners in this context. They have already paid the start-up cost x, and so business training has impacts by increasing productivity for a given capital stock level, and by causing the firm to change its optimal capital stock. Whether or not business profits increase or not will then depend on whether skills and capital are complements or substitutes, and how much training skills translate into higher productivity at a given capital stock level.

5. What was the impact of the training and grants?

To estimate the impact of being assigned to training only or to training plus cash on business outcome $Y_{i,t}$ for firm i in follow-up time period t, we estimate the following Ancova model:

$$Y_{i,t} = \alpha + \gamma_1 Assigned\ TrainingOnly_i + \gamma_2 Assigned\ Training\&Cash_i + \theta Y_{i,0}$$
$$+\pi_{i,s}+\varepsilon_{i,t} \tag{1}$$

where $Y_{i,0}$ is the baseline value of the outcome of interest, and $\pi_{i,s}$ is a dummy for being in randomization strata s. The Ancova specification, which includes the lagged outcome variable is only possible for the current enterprises (since potential owners did not have baseline business outcomes), and offers more power than either difference-in-differences or analysis just using the follow-up data, especially when looking at outcomes like microenterprise profits and sales which

are not very highly autocorrelated (McKenzie, 2011).[12] We estimate equation (1) round-by-round in order to determine how the treatment effects vary over time.

In addition, we can estimate the average treatment effect over all follow-up rounds with additional power gains by pooling together several rounds of follow-up surveys, and estimating the following regression:

$$Y_{i,t} = \sum_{t=1}^{r} \delta_t + \gamma_1 Assigned\ TrainingOnly_i + \gamma_2 Assigned\ Training\&Cash_i$$
$$+\theta Y_{i,0} + \pi_{i,s} + \varepsilon_{i,t} \tag{2}$$

Where r is the number of follow-up surveys, and the δ_t are survey round dummies. Standard errors are then clustered at the firm level.

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 conditional on receiving training. Under the assumption that the offer of training does not affect the outcomes of interest for those who do not take the training up, we can also estimate the treatment-on-the-treated (TOT) effect, which is the effect of training for those who take it up. This assumption seems plausible if we define take-up as going to at least one training session, but could be violated if we were to try and look at the effect of finishing the training course, since training may have an effect on businesses of owners who start training and drop-out. We therefore look at the TOT for having received some training, although since 267 of the 279 current owners who attended at least one session completed the training, in practice there are few drop-outs to worry about. We then estimate the TOT by instrumenting receipt of some training only with assignment to training only, and receipt of training plus cash with assignment to this treatment.

5.1 Impact on the business practices of women who were already in business

We then begin in Table 3 by asking whether the training led to any improvements in the types of business practices the training was meant to improve. These practices were measured in the baseline, and then the rounds 2, 4 and 5 surveys. The first three columns show that the use of these practices increased in both the short-term (3-4 months post training), and medium-term

---

[12] We typically get similar sized coefficients with larger standard errors using these other specifications. Results available upon request.

(15-16 months after training and 24-25 months after training), with the magnitude of the increase similar at all follow-up intervals. In the last survey round, and for all rounds combined, we can reject equality of the training and training plus cash treatments at the 10 percent level, with the increase in practices slightly higher for the joint treatment.

The magnitude of the increase is large relative to the low baseline levels, with the TOT showing an increase in 2 to 3 total practices relative to a baseline mean of 5 practices. However, given the maximum possible practices score is 29, the mean treatment firm which is implementing 9 or 10 practices in the follow-up round is still a long way from implementing all the practices the IYB training is intended to get them to do.[13] Columns 3 through 7 then pool together the different rounds, and show that the training programs significantly improved each subcomponent of the overall business practice score, with improvements in marketing, stock control, financial planning, and record-keeping.

5.2 Impact on the Business outcomes of existing business owners

Eighty-three percent of the current business owners remain in business by the round 5 survey, and we cannot reject that survival is unrelated to treatment status ($p=0.37$). Moreover, the treatments don't appear to have affected which firms survive, with treatment status unrelated to observable characteristics among surviving firms. We therefore proceed to analyze business outcomes for the group of surviving firms.

Table 4 examines whether these improvements in business practices from the training, along with the cash given to the cash plus training group, lead to improvements in business outcomes. We examine the impact of the two treatments on monthly profits (panel A), monthly sales (panel B), and capital stock (including inventories and raw materials but excluding land and buildings) (panel C). For each outcome we first show the results using all follow-up rounds pooled together to achieve greatest power, before then examining the round-by-round estimates. Profits, sales and capital stock are all noisy variables, and so in addition to showing the impact on the raw levels in column (1), we use two measures which are less susceptible to outliers. The first, in column (2), involves truncating the data at the 99th percentile of reported levels. Alternatively, in column (3) we report impacts on the log of the outcome of interest. In panel A, we report both the ITT and the TOT for profits. For sales and capital stock we just report the ITT

---

[13] This can be compared with the large increases in management practices that Bloom et al. (2011) find as a result of 5 months of individual consulting for large firms. It seems likely that high-intensity individualized follow-up consulting would lead to a greater increase in these practices being implemented (but also at higher cost).

estimates – approximate TOT estimates can be obtained by dividing these by the proportion of firms attending some training (0.70). In addition, Figures 4A and 4B show the cumulative distribution functions of profits by treatment status for the round 2 and round 5 surveys respectively, in order to better assess which parts of the distribution, if any, effects are occurring in.

The results show that the business training alone does not improve profits, sales, or capital stock of current firm owners. The CDF for profits of the training only group lies almost entirely on top of that of the control group, and the point estimates of the impact on profit are actually negative in most survey rounds. We do not find these firms using significantly more, or significantly less, capital stock, with the point estimates small relative to the mean. The lack of a profitability improvement thus does not reflect these firms substituting skills for capital. Instead, they appear to be using the same amount of capital, and not selling any more or earning any greater profits. These conclusions hold regardless of the follow-up period looked at, which suggests that this is not a case of it taking time for the training to show impact, or conversely, of training having a short-term impact, but then this effect dissipating over time.

In contrast, the combination of training and the cash grant does have positive and significant impacts on capital stock, which hold across survey rounds. Truncated capital stock increases by an ITT of 10,000 Rs, with the TOT for the pooled rounds of 15,357. Thus capital stock appears to have increased by the same size as the grant for those who completed the training and received the 15,000 Rs grant. There is a significant increase in profits when using the pooled sample with truncated or log profits, with this effect coming from the round 2 and 3 data. However, the point estimate on profits is much smaller by the rounds 4 and 5 surveys, and thus it appears that this increase in profits is only temporary. This is seen also in Figure 4B, where the CDF of profits is now almost on top of that of the other two groups. The sales data are noisier than the profits data, and we generally see positive, but not significant, impacts on sales.

The combination of training and a grant therefore had effects very shortly after training ended, which appear to have had dissipated a year later. Since we wanted to be able to compare the impact of training and cash to that of training alone, cash grants are only given to those who completed training. Ex ante it is not possible to tell who these individuals would be, precluding the inclusion of a treatment group which only gets cash conditional on being the sort of person who would get training if offered it. To learn whether the short-term impact on profits is due just

to the cash grants, or to the combination of the cash and training, we therefore re-estimate the impact on profits, controlling directly for capital stock. This nets out both the effect of the grant, as well as any effect of business training on ability to increase capital stock (such as through learning about how to access credit, or through re-invested earnings). Under a sequential ignorability assumption (Imai et al, 2010), controlling for capital will therefore give us a lower bound on the amount of impact the training has in addition to the grant for firms getting both.

Controlling for capital stock reduces the coefficient for the impact of the joint treatment on truncated profits from 1207 in the pooled sample to 425 (s.e. 544), suggesting that two-thirds of the effect is due just to the grant. Looking just at the round 2 profits, controlling for capital stock reduces the coefficient from 1758 without this control to 971 with it, suggesting that half of the effect is just due to the grant. As a final check, given the grant was 15,000 Rs, the TOT of 1786 Rs in the pooled sample would represent a monthly return on the grant of 11.9 percent if the effect was due to the grant alone. This is similar to the return on capital found for the average male microenterprise owner in Sri Lanka, but considerably higher than average returns for female business owners found in de Mel et al. (2008, 2009a). It therefore appears plausible that some of the short-term effect may be due to the combination of training and the cash grant, rather than just the cash grant alone. However, the fact that this effect appears to only be temporary suggests that the combination is only speeding up convergence to a steady state, rather than dramatically changing the production technology.

5.3 Impact on Starting a Business among Potential Business Owners

We now turn to the potential business owner group. Table 5 examines whether the treatments affected the likelihood that the individual entered self-employment or engaged in wage worker during our various survey rounds. We present ITT estimates from a regression of equation (1) with self-employment as the dependent variable (and no lagged dependent variable since none of this group operated a business at baseline), and TOT estimates instrumenting receipt of the treatment with random assignment.

We see that 38 percent of the control group ran a business at the time of the round 2 survey, with this increasing to 56 percent at the time of the round 4 survey and 57 percent at the time of the round 5 survey. The combination of cash and training resulted in a 29 percentage point increase in the likelihood of running a business in round 2 for those treated, and a 20 percentage point increase in the likelihood in round 3, but no significant increase in rounds 4 and

5. There is thus a temporary impact, with the business ownership rates for the control group then catching up over time. We also see that the cash and training treatment lead to treated individuals being 10 percentage points more likely to have ever opened a business since baseline, and 9 percentage points more likely to have opened a business and then closed it – with these two effects netting each other out so to give the near zero and insignificant effect on round 5 business ownership.

Training alone has a smaller, but still significant, 12 percentage point increase in the likelihood of operating a business in the round 2 survey for those attending training. The impact is then small and no longer significant in the subsequent survey rounds. The last columns show that the higher incidence of self-employment in rounds 2 and 3 comes in part from a lower incidence of wage work for both treatment groups, but that by rounds 4 and 5 there is no significant difference in wage employment rates by treatment status.

The treatments therefore seem to have sped up entry into business ownership, but not had any long-term impact on ownership rates. Our model predicts that in addition to affecting the rate of ownership, training and grants should also affect the selection of who owns a business, drawing in relatively less-skilled and/or poorer individuals. We examine this by allowing for heterogeneity in treatment effects by two measures of skill- Raven score, and digitspan recall; and by our baseline household wealth index. Table 6 shows the results of ITT estimation of the likelihood of having a business in each survey round. Panel A shows little in the way of selection according to ability as measured by the raven test in the early survey rounds, but a significant negative interaction by round 5 for both treatments. Figure 5A illustrates this by graphing lowess lines of business ownership against Raven score by treatment group. We see that there is an upward-sloping relationship for the control group, in line with our theoretical model, whereas business ownership rates are relatively constant across skill levels for the two treatment groups. The consequence is that training, whether or not accompanied by cash, has induced less innately skilled women to open businesses, but also appears to have deterred some of the more skilled women from opening them.

Panel B of Table 6 shows no significant heterogeneity with respect to our other measure of ability, the digitspan recall. The correlation between raven score and digitspan in this sample is only 0.18. The raven score is a measure of analytical reasoning, whereas the digitspan recall is a measure more closely related to short-term memory. It seems plausible that business training is

more of a substitute for the types of business skills analytical reasoning would otherwise be employed for, which might explain why there is heterogeneity for raven test and not for digitspan.

Panel C of Table 6 then examines heterogeneity with respect to baseline household wealth. We see that there is a negative interaction effect, with this being largest and most significant for the cash and training treatment in rounds 4 and 5. Figure 5B shows the pattern of business ownership in round 5 against baseline household wealth. We see the control group and training only group show similar patterns, with business ownership increasing in wealth for wealth levels above the median. In contrast, the pattern is reversed for the cash plus training group. The cash and training treatment appears to have drawn poorer individuals into opening up businesses, but also deterred individuals from richer households from having businesses in round 5. The inclusion of poorer individuals is consistent with the cash grant overcoming credit constraints to entry.

5.4 Impact on Business Outcomes for Businesses Started by Potential Owners

We have seen that in the short-run the treatments led to more business entry in the treatment groups than the control group, whereas in the medium-term the rate of business ownership is similar, but the characteristics of who owns a business has changed. This makes comparison of the business outcomes for treatment and control groups difficult, and so we employ several approaches to assessing impacts.

We begin by looking at the impact of the treatments on total work income from profits (if they own a business) and wages (if they work in wage work). This income is then zero for women who don't work. The advantage of this outcome is that it is not subject to selection into employment, and we can obtain experimental estimates as for the current enterprise sample. We pool together rounds 2 and 3 to give a short-term effect, and rounds 4 and 5 to give a medium-term effect. Column 1 shows that despite the two treatments having significant impacts on the likelihood of self-employment in the short-term, these individuals are not earning significantly more, which in part reflects the lower likelihood of wage employment. The point estimates are positive, but at 211-267 Rs are equivalent to only about 7% of the control group's income. In contrast, the impacts are larger in the medium-term, with an increase in work income of 1494 for the training only group (significant at the 10% level), and insignificant increase of 697 for the training plus cash group (which we also can't reject is equal in magnitude to the training only

group). These point estimates reflect an increase in income of 14 to 30 percent relative to the control group mean. In terms of cost-benefit, the training only intervention cost approximately 18,600 Rs per person, or 12 months of the medium-term treatment effect; whereas the training plus cash cost is the equivalent of 48 months of the medium-term treatment effect for this treatment – which it should be noted is also statistically insignificant.

The remainder of panel A then looks at the impact of the treatments on business profits, sales, capital stock, and business practices, conditional on running a business. The business practices were only asked for this group in the rounds 4 and 5 surveys. We can never reject equality of the two treatment effects, but the point estimates on profits and sales are higher for the training only treatments, while the impact on business practices is similar in magnitude for the two treatments, and the cash plus training treatment has larger magnitude point estimate for the effect on capital stock. The results for the training only intervention show significant impacts on profits and sales in the rounds 4 and 5 surveys, with the magnitudes equivalent to a 43 percent increase in profits relative to the control mean and 40 percent increase in sales.

However, while there is no difference in the rates of business ownership in the rounds 4 and 5 surveys, our analysis has shown there is a difference in the characteristics of who runs a business, with the treatments leading to women with lower Raven scores and, in the case of the cash plus training treatment, lower wealth, running businesses. To examine how sensitive our results are to this potential selection, we estimate a multinomial logit for the probability of being in each of our three treatment groups for the sample who own a business. The variables used in this multinomial are the same as those shown in Table 2 to predict training take-up, and include wealth, ability, risk attitudes, interest in attending training, interest in running a business, and personal characteristics like age, marital status and education. We use these predicted probabilities to form a generalized propensity score (Imbens, 2000), and re-run our treatment regressions after reweighting by the inverse of this generalized propensity score (GPS). We do this after restricting the analysis to the subsample such that the GPS is in a common range across the three treatments.[14] Since the initial sample was randomized and the selection was not that extreme, the GPS distributions look quite similar across the three treatment groups, and trimming dissimilar observations only removes a few observations.

---

[14] We look at the GPS distributions for each of the three treatment groups, and take the maximum of the 1st percentile, and the minimum of the 99th percentile over these three groups. We then trim observations which lie outside this range.

Panel B of Table 7 shows the resulting estimates. They are reasonably similar to those in panel A, suggesting that the observed selection on observed ability and wealth is not driving the estimates obtained. We still find the training only intervention to have a significant impact on monthly profits, both treatments have relatively large, but insignificant, impacts on sales, and both treatments result in better business practices.

Taken together, these results suggest that the interventions sped up the process of business start-up for firms in our sample, and the resulting firms that were created employed slightly more business practices and appear to be more profitable. This conclusion should be caveated by the lack of significance of some outcomes, and by the possibility that selection on unobservables could be driving the higher profitability of the firms created by individuals assigned to training. Nevertheless, the higher total work income for this group suggests that individuals are on average better off as a result of this intervention.

5. Conclusions and Discussion

Existing business training experiments have typically found rather limited impacts of business training on female microfinance clients who already own businesses. The stylized finding is for women undergoing training to undertake a few more of the practices taught by the training in their enterprises, but for this not to have measurable impacts on business profitability or employment levels. Our results with a representative sample of existing poor female business owners provide a similar picture, and suggest training alone is not enough to get subsistence businesses run by women to grow. Adding capital leads to a temporary boost in profitability in the first year after training, but this effect appears to be relatively short-lived and these enterprises are no more profitable two years after training than the control group. These results highlight the challenge in getting subsistence-level female-oriented microenterprises to grow, and suggest that the binding constraints on their growth may lie outside the realm of capital and skills. On option is more intensive (and expensive) one-on-one personalized mentoring and consulting, which Valdivia (2011) finds to increase sales by 18 percent. Another is to address constraints to their participation in wage work, with these labor market failures potentially the reason that many of these women are operating businesses in the first place (Emran et al, 2007).

In contrast, our results are more encouraging for the ability of business training to help women who are out of the labor force to start enterprises more quickly, and to make these

businesses better managed and more profitable. The impact on business start-ups is consistent with results from recent randomized experiments with microfinance (Banerjee et al, 2011; Attanasio et al, 2011) which have found giving microfinance to poor women with a high propensity to become business owners does lead to some new business start-ups. Taken together, these results suggest that it is easier to get women to start up subsistence businesses than it is to get these businesses to grow.

A final point is that our study highlights the importance of tracing out the trajectory of impacts and not just relying on a single follow-up survey. The impacts of business training vary over the time frame in which they are measured, particularly for potential enterprise owners.

References

Attanasio, Orazio, Britta Augsburg, Ralph de Haas, Emla Fitzsimons, and Heike Harmgart (2011) "Group lending or individual lending? Evidence from a randomized field experiment in Mongolia", BREAD policy paper no. 27.

Banerjee, Abhijit and Esther Duflo (2008) "What is middle class about the middle classes around the world?", *Journal of Economic Perspectives* 22(2): 3-28.

Banerjee, Abhijit, Esther Duflo, Rachel Glennerster and Cynthia Kinnan (2011) "The miracle of microfinance? Evidence from a randomized evaluation", Mimeo, MIT.

Berge, Lars Ivar Oppedal Kjetil Bjorvatn and Bertil Tungodden (2011) "Human and financial capital for microenterprise development: Evidence from a field experiment in Tanzania", Mimeo.

Bloom, Nicolas, Benn Eifert, David McKenzie, Aprajit Mahajan and John Roberts (2011) "Does management matter? Evidence from India", World Bank Policy Research Working Paper.

Bruhn, Miriam, Dean Karlan and Antoinette Schoar (2012) "The Impact of Consulting Services on Small and Medium Enterprises: Evidence from a Randomized Trial in Mexico", Mimeo.

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.

Bruhn, Miriam and Bilal Zia (2011) "Business and Financial Literacy for Young Entrepreneurs: Evidence from Bosnia-Herzegovina", Mimeo. World Bank.

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 (2009b) "Measuring microenterprise profits: Must we ask how the sausage is made?", *Journal of Development Economics* 88(1): 19-31

Drexler, Alejandro, Greg Fischer and Antoinette Schoar (2010) "Keeping it simple: Financial Literacy and Rule of Thumbs", mimeo. LSE.

Emran, M. Shahe, AKM Mahbub Morshed and Joseph Stiglitz (2007) "Microfinance and Missing Markets", Mimeo.

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, Seema Jayachandran, and Rohini Pande (2010) "Do traditional institutions constrain female entrepreneurship? A field experiment on business training in India", *American Economic Review Papers and Proceedings* 100(2): 125-29.

Gine, Xavier and Ghazala Mansuri (2011) "Money or ideas? A field experiment on constraints to entrepreneurship in rural Pakistan", Mimeo.

Imai, Kosuke, Luke Keele, Dustin Tingley and Teppei Yamamoto, (2011). "Unpacking the black box: Learning about causal mechanisms from experimental and observational studies", Mimeo. Princeton University.

Imbens, Guido (2000) "The Role of the Propensity Score in Estimating Dose-Response Functions", *Biometrika* 87(3): 706-710.

Karlan, Dean, and Martin Valdivia (2011). "Teaching Entrepreneurship: Impact of Business Training on Microfinance Institutions and Clients". *Review of Economics and Statistics*, 93(2): 510-27.

Kevane, Michael and Wydick, Bruce (2001), "Microenterprise Lending to Female Entrepreneurs: Sacrificing Economic growth for Poverty Alleviation?", *World Development*, Volume 29(7): 1225-1236.

Lloyd-Ellis, Huw and Dan Bernhardt (2000) "Enterprise, inequality, and economic development", *Review of Economic Studies* 67: 147-68.

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

Valdivia, Martin (2011) "Training or technical assistance for female entrepreneurship: Evidence from a field experiment in Peru", Mimeo.

Appendix 1: Business Practices Score

The *total score* – the composite business practice score -- ranges from a minimum of -1 to a maximum of 29. It is the sum of the firm's following component scores: the *marketing score*, the *stock score*, the *record s score*, and the *financial planning score*.

The *marketing score* ranges from 0 to 7, and it is calculated by adding one point for each of the following that the business has done in the last 3 months:

- Visited at least one of its competitor's businesses to see what prices its competitors are charging
- Visited at least one of its competitor's businesses to see what products its competitors have available for sale
- Asked existing customers whether there are any other products the customers would like the business to sell or produce
- Talked with at least one former customer to find out why former customers have stopped buying from this business

- Asked a supplier about which products are selling well in this business' industry
- Attracted customers with a special offer
- Advertised in any form (last 6 months)

The *stock score* ranges from -1 to 2, and it is calculated by subtracting one point

- If the business runs out of stock once a month or more

And adding one point for each of the following that the business has done in the last 3 months

- Attempted to negotiate with a supplier for a lower price on raw material
- Compared the prices or quality offered by alternate suppliers or sources of raw materials to the business' current suppliers or sources of raw material

The *records score* ranges from 0 to 8, and it is calculated by adding one point for each of the following that the business does

- Keeps written business records
- Records every purchase and sale made by the business
- Able to use records to see how much cash the business has on hand at any point in time
- Uses records regularly to know whether sales of a particular product are increasing or decreasing from one month to another
- Works out the cost to the business of each main product it sells
- Knows which goods you make the most profit per item selling
- Has a written budget, which states how much is owed each month for rent, electricity, equipment maintenance, transport, advertising, and other indirect costs to business
- Has records documenting that there exists enough money each month after paying business expenses to repay a loan in the hypothetical situation that this business wants a bank loan

The *financial planning score* ranges from 0-12, and it is calculated by adding up to three points for each of the following two questions

- How frequently do you review the financial performance of your business and analyze where there are areas for improvement
- How frequently do you compare performance to your target
  - Zero points for "Never"
  - One point for "Once a year or less"
  - Two points for "Two or three times a year"
  - Three points for "Monthly or more often"

And adding one point for each of the following that the business has

- A target set for sales over the next year
- A budget of the likely costs your business will have to face over the next year
- An annual profit and loss statement
- An annual statement of cash flow
- An annual balance sheet
- An annual income/expenditure sheet

Appendix 2: Bounding the Start-up Results among the Potential Sample

Attrition rates in our study are low, but they are marginally lower for the training only group amongst potential firms than they are for the training plus grant and control group. To assess the sensitivity of our results to this attrition, in Appendix Table 1 we provide conservative bounds for the impact of the treatments on business ownership. We create a lower bound for the treatment impact by assuming that all control group individuals whose ownership status is unknown had started businesses whereas all treated individuals with unknown status had not; an upper bound reverses this assumption. The conclusion of a large treatment effect in the short-term which closes in the medium-term is robust to this attrition, and the bounds are reasonably narrow.

Appendix Table 1: Bounding the Impact of Business Training on Business Start-up

Dependent Variable: Whether the individual owns a business in a given survey round

	Round 2		Round 3		Round 4		Round 5	
	Lower Bound	Upper Bound	Lower Bound	Upper Bound	Lower Bound	Upper Bound	Lower Bound	Upper Bound
Assigned to Cash if finish Training	0.157***	0.244***	0.0953**	0.192***	-0.0419	0.111**	-0.00521	0.0586
	(0.0455)	(0.0456)	(0.0469)	(0.0472)	(0.0477)	(0.0483)	(0.0475)	(0.0478)
Assigned to Training only	0.0540	0.119***	0.0163	0.106**	-0.0408	0.108**	-0.0495	0.00799
	(0.0461)	(0.0457)	(0.0480)	(0.0478)	(0.0482)	(0.0477)	(0.0481)	(0.0481)
Individuals	628	628	628	628	628	628	628	628

Notes: Results shown are for OLS regressions of outcome on treatment assignment. All specifications include randomization strata dummies. Robust standard errors in parentheses , clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1 Lower bound assigns all control individuals with missing current business status as business owners and all treatment individuals with missing status as non-business owners. Upper bound does the reverse.

Figure 1: Timeline

| Jan 2009 Baseline | April /May 2009 **Training** | Sept 2009 Round 2 survey | Jan 2010 Round 3 survey | Sept 2010 Round 4 survey | June 2011 Round 5 survey |
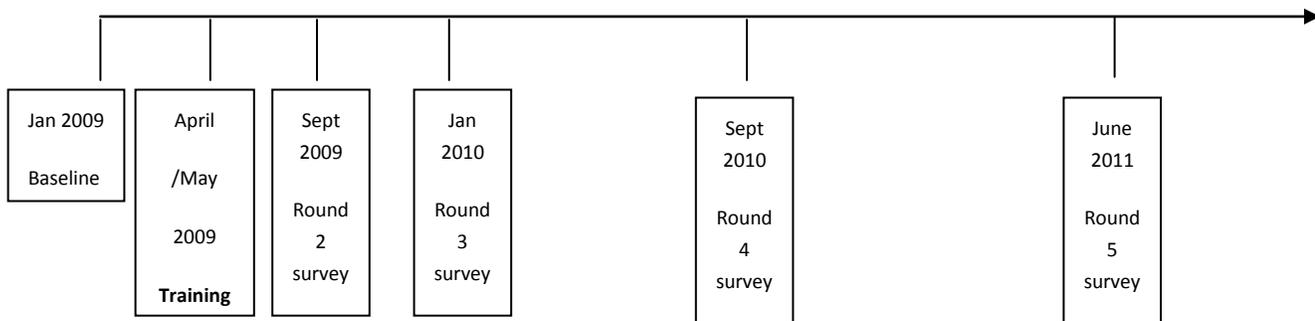
Figure 2: Occupation choice by entrepreneurial ability and wealth

A (wealth)

A**

Self-employed

Wage workers by choice

A*

No-choice wage
workers

θ*

θ (ability)
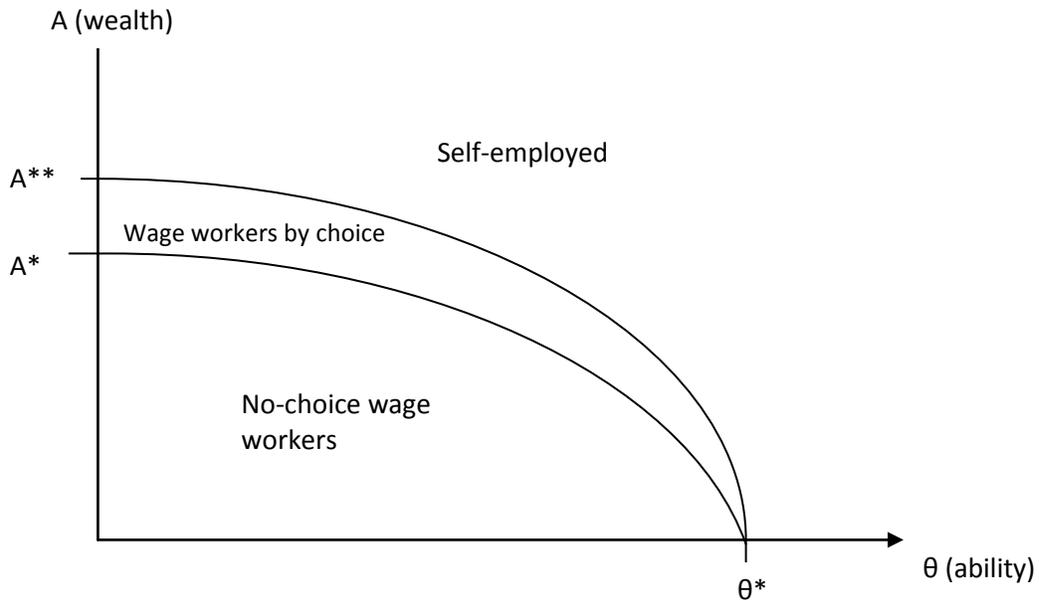
Figure 3: Impact of business training on occupational choice

A

A**

A*

θ*-T   θ*   θ
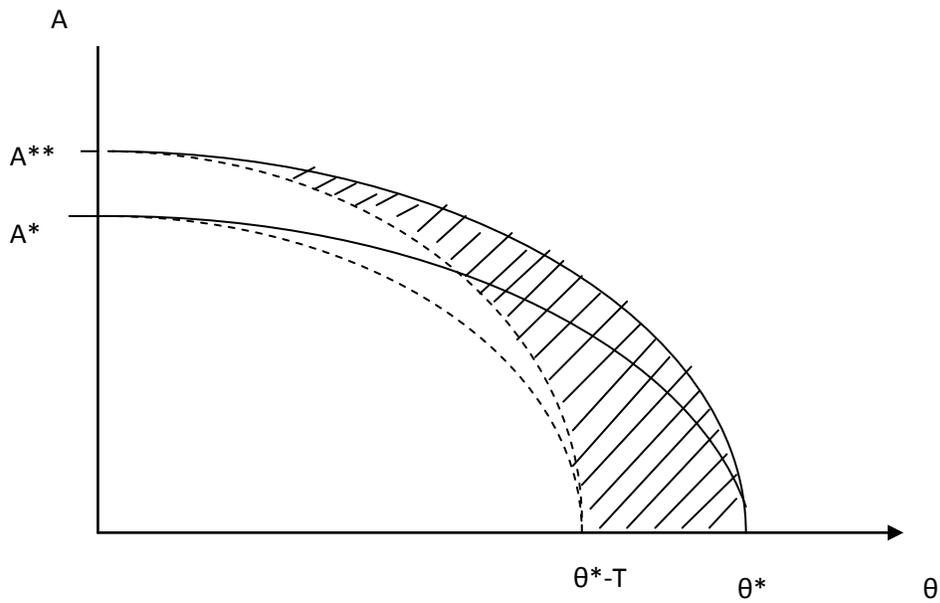
Shaded area shows individuals who newly become entrepreneurs because of business training.

Figure 4A: CDF of Monthly Profits of Current Enterprises at First follow-up Survey
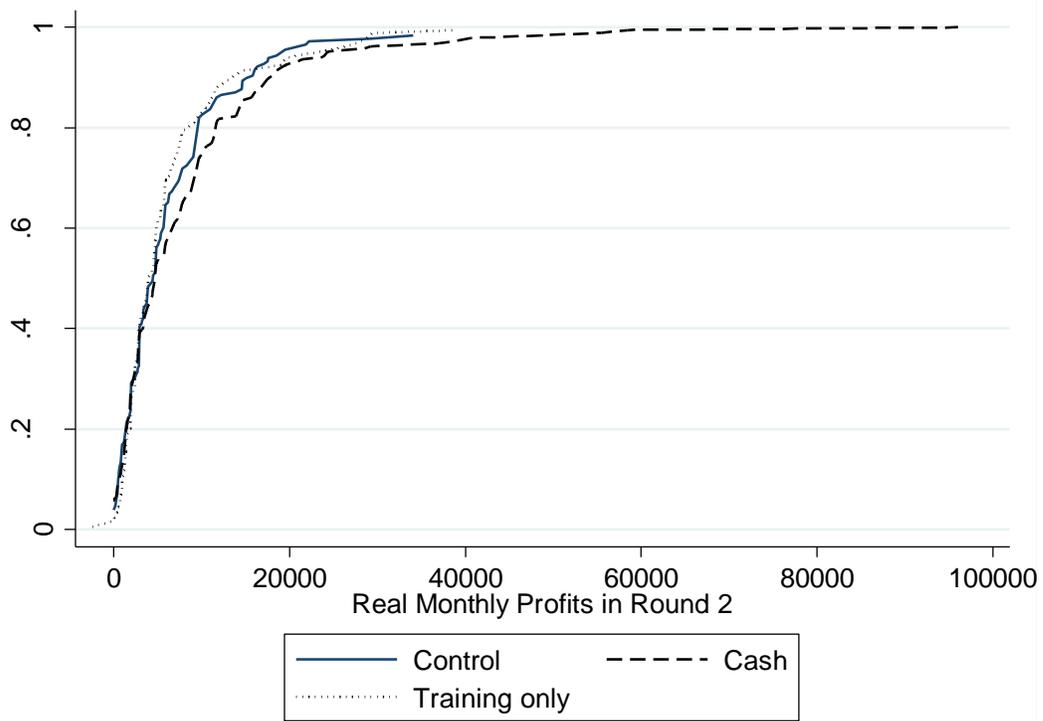
Figure 4B: CDF of Monthly Profits of Current Enterprises at Last follow-up Survey
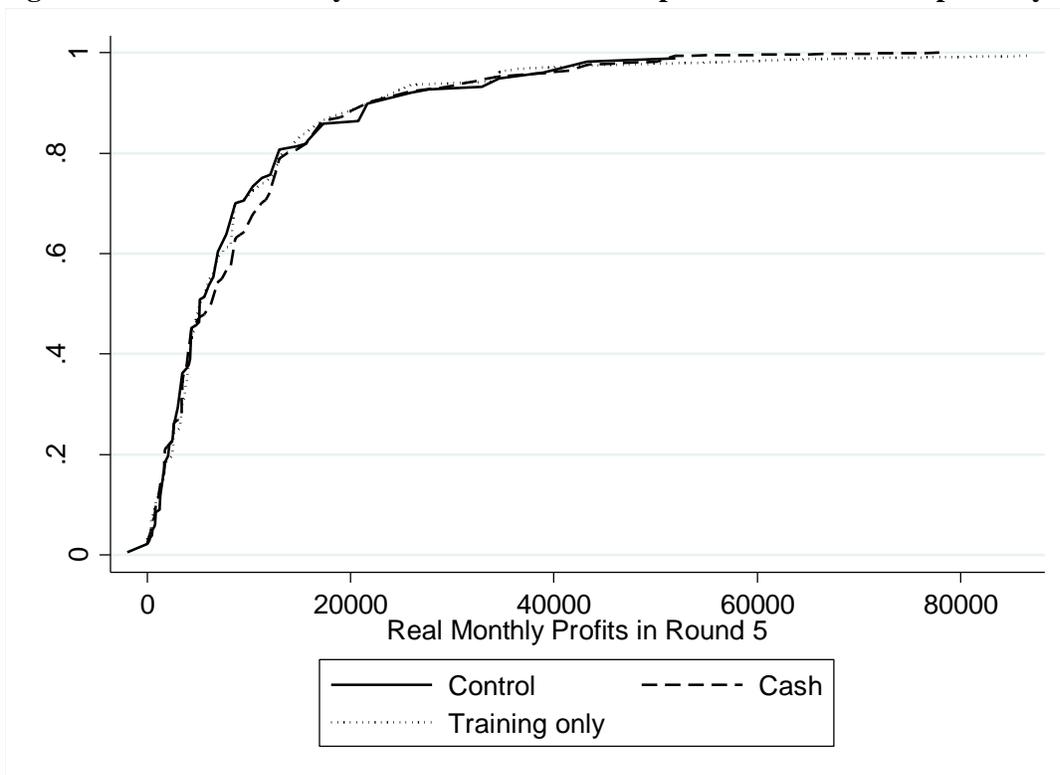
Figure 5A: Round 5 Business Ownership – Ability Profile by Treatment Status
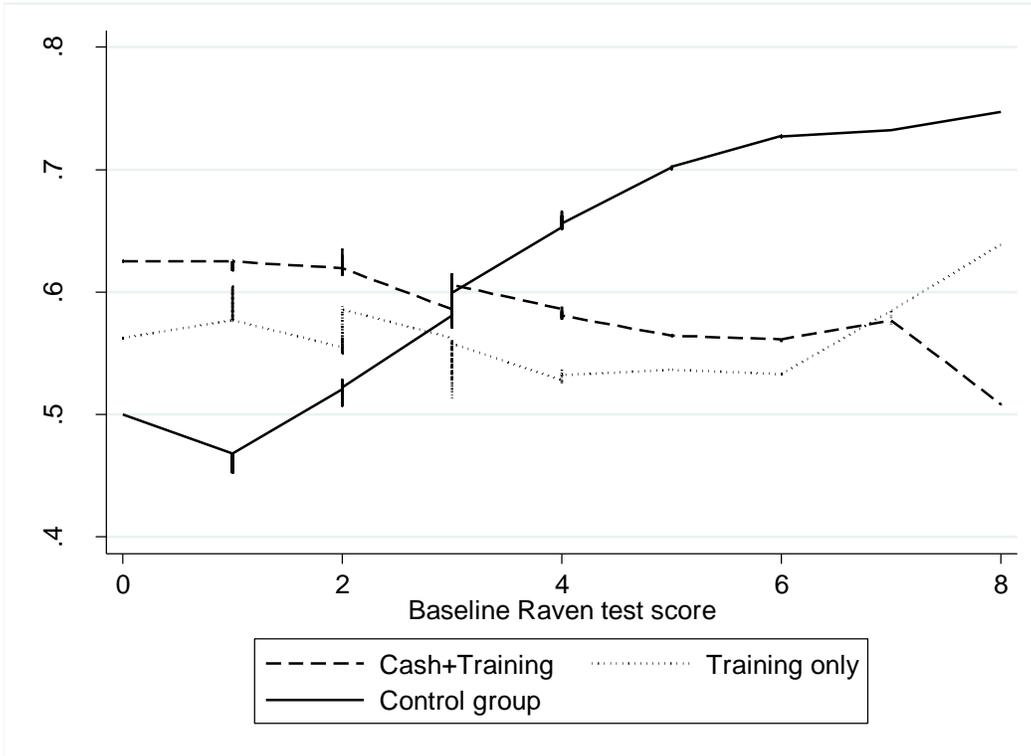
Lines shown are lowess best-fit lines; Graph shows up to the 99th percentile of Raven test score. Median score is 2.

Figure 5B: Round 5 Business Ownership – Wealth Profile by Treatment Status
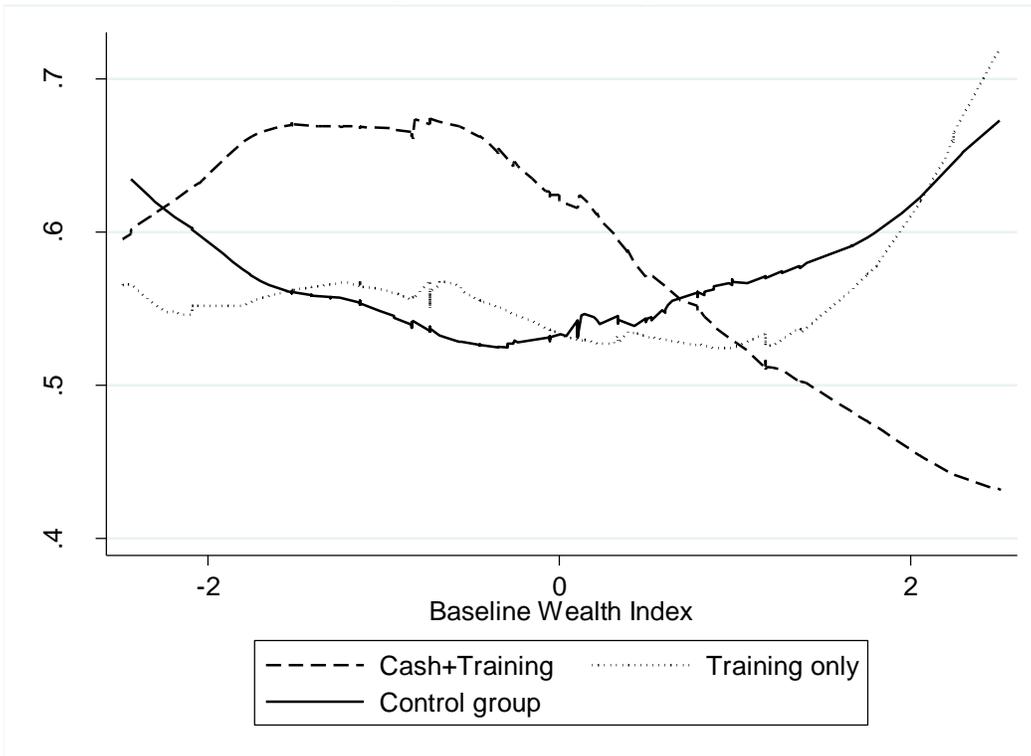
Lines shown are lowess best-fit lines; Graph shows 1st to 99th percentile of Baseline wealth. Median score is -0.1.

Table 1: Baseline Characteristics of the Sample by Treatment Group

	Current Enterprises			Potential Enterprises		
	Control	Training only	Training + Cash	Control	Training only	Training + Cash
Variables stratified on						
Total Monthly Profits (Rs.)	3987	3981	4001			
Have no children or have someone to look after them	0.55	0.54	0.55			
Colombo district	0.20	0.20	0.20	0.19	0.20	0.21
Kandy district	0.21	0.20	0.20	0.20	0.20	0.20
Has taken concrete steps to opening business				0.51	0.50	0.51
Has never worked before				0.18	0.17	0.19
Variables not stratified on						
Age	35.94	37.71	36.58	34.38	34.05	33.72
Married	0.89	0.86	0.80	0.84	0.91	0.89
Number of children under 18	1.55	1.47	1.40	1.40	1.47	1.59
Years of Education	10.16	10.34	10.51	10.51	10.56	10.53
Risk-seeking score	6.81	6.87	6.53	6.73	6.82	6.75
Digitspan Recall	6.00	6.04	6.01	6.03	5.93	6.06
Raven test score	2.58	2.75	2.68	2.76	2.59	2.81
Total household income from all sources	17192	18245	17595	16422	16690	16393
Wealth index (principal component)	0.01	0.05	0.28	-0.09	-0.12	-0.11
Household has a fridge	0.45	0.53	0.51	0.39	0.41	0.43
Household has a sewing machine	0.56	0.60	0.60	0.51	0.54	0.55
Household has an oven	0.08	0.08	0.12	0.09	0.05	0.08
Household has a gas cooker	0.25	0.23	0.30	0.28	0.24	0.24
Age of Firm (years)	6.47	6.88	6.35			
Ever had a loan from financial institution	0.23	0.18	0.20			
Total Monthly Sales (Rs.)	12523	12485	12640			
Capital Stock excluding land and buildings (Rs.)	28649	27418	35187			
Truncated Capital Stock (Rs.)	28649	27418	34997			
Business Practices Score	4.59	4.99	4.98			
Number of Firms	224	200	200	228	200	200

Table 2: Determinants of Training Take-up

Marginal effects from Probit estimation of Attending Training among those offered

	Current Enterprises			Potential Enterprises		
Owner characteristics						
Has no children or has someone to look after childre	0.0171	-0.0460	-0.0204	0.00373	-0.00589	0.0146
	(0.0475)	(0.0476)	(0.0493)	(0.0523)	(0.0522)	(0.0544)
Age	0.00614	0.00617	0.00403	0.00956**	0.00827**	0.00759*
	(0.00409)	(0.00408)	(0.00418)	(0.00395)	(0.00397)	(0.00398)
Married	0.128*	0.169**	0.152**	-0.0993	-0.0835	-0.0760
	(0.0658)	(0.0679)	(0.0693)	(0.0705)	(0.0734)	(0.0730)
Years of Education	0.0290***	0.0184	0.0171	0.0122	0.00820	0.00754
	(0.0109)	(0.0113)	(0.0118)	(0.0110)	(0.0113)	(0.0115)
Risk-seeking Attitude	-0.0189	-0.00868	-0.0111	-0.0105	-0.00195	-0.000607
	(0.0133)	(0.0133)	(0.0140)	(0.0132)	(0.0138)	(0.0141)
Raven test score	0.000541	0.000168	-0.00425	0.0282**	0.0310**	0.0312**
	(0.0141)	(0.0147)	(0.0149)	(0.0133)	(0.0135)	(0.0139)
Digit-span Recall	-0.0110	0.00665	0.00691	-0.0309	-0.0209	-0.0289
	(0.0200)	(0.0198)	(0.0213)	(0.0195)	(0.0200)	(0.0209)
Wealth index (principal component)	-0.0441***	-0.0381**	-0.0379**	-0.00556	-0.00337	0.00362
	(0.0168)	(0.0169)	(0.0174)	(0.0152)	(0.0153)	(0.0159)
Says would pay 500 Rs or more for a training course	-0.0115	-0.0239	-0.0492	0.0439	0.0367	0.0368
	(0.0496)	(0.0531)	(0.0540)	(0.0507)	(0.0505)	(0.0522)
Has taken concrete steps to opening a business				0.0635	0.0338	0.0472
				(0.0484)	(0.0502)	(0.0552)
Has never worked before				-0.0157	-0.0358	-0.0523
				(0.0625)	(0.0639)	(0.0666)
Firm Characteristics						
Log of monthly profits	-0.0677*	0.0267	0.0125			
	(0.0399)	(0.0385)	(0.0420)			
Firm is in Manufacturing	0.126*	0.119*	0.121*			
	(0.0666)	(0.0693)	(0.0697)			
Firm is in Retail Trade	0.0650	0.0356	0.0331			
	(0.0676)	(0.0737)	(0.0748)			
Works more than 40 hours a week at baseline	-0.0899*	-0.0889*	-0.0766			
	(0.0488)	(0.0503)	(0.0504)			
Baseline Business Practices Score	0.00670	0.00801	0.0116			
	(0.00681)	(0.00729)	(0.00790)			
Firm is younger than 5 years old	0.0969*	0.141***	0.136***			
	(0.0505)	(0.0527)	(0.0529)			
Geography						
Colombo District		-0.446***			-0.152**	
		(0.0676)			(0.0694)	
Kandy District		0.0762			0.0317	
		(0.0636)			(0.0610)	
D.S. (locality) fixed effects	No	No	Yes	No	No	Yes
Number of firms	400	400	400	399	399	399

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 3: Impact on Business Practices of Current Enterprises

	Total Practices Score				Marketing	Stock Control	Record keeping	Financial Planning
	Round 2	Round 4	Round 5	All rounds	All rounds	All rounds	All rounds	All rounds
Intent-to-Treat Effects								
Assigned to Cash if finish Training	2.530***	1.936***	2.109***	2.087***	0.379***	0.230***	0.872***	0.628***
	(0.555)	(0.567)	(0.570)	(0.326)	(0.109)	(0.0603)	(0.154)	(0.132)
Assigned to Training only	1.719***	1.708***	1.075*	1.524***	0.433***	0.125**	0.483***	0.535***
	(0.555)	(0.560)	(0.568)	(0.326)	(0.113)	(0.0638)	(0.148)	(0.140)
Treatment on the Treated								
Received Training & Cash	3.588***	2.790***	3.122***	3.059***	0.552***	0.338***	1.281***	0.917***
	(0.591)	(0.607)	(0.631)	(0.429)	(0.147)	(0.0819)	(0.204)	(0.174)
Received Training Only	2.192***	2.261***	1.489**	2.031***	0.574***	0.167**	0.645***	0.711***
	(0.540)	(0.546)	(0.580)	(0.389)	(0.136)	(0.0783)	(0.178)	(0.168)
Observations	544	513	506	1,563	1,563	1,563	1,563	1,563
Firms	544	513	506	573	573	573	573	573
p-value for testing two treatments equal	0.154	0.690	0.080	0.099	0.622	0.091	0.011	0.533
Baseline Mean:	4.96	5.02	4.98	4.96	1.66	0.53	2.10	0.64

Robust standard errors in parentheses clustered at the firm level when all rounds used, *** p<0.01, ** p<0.05, * p<0.1

All specifications also include baseline outcome value and controls for randomization strata, and columns 4-8 also include survey

round dummies. Business practices were not measured in round 3.

Table 4: Impact on Firm Performance for Current Enterprises

	All rounds pooled			Round 2	Round 3	Round 4	Round 5
		Truncated		Truncated	Truncated	Truncated	Truncated
	Levels	Levels	Logs	Levels	Levels	Levels	Levels
Panel A: Monthly Profits							
ITT Effects							
Assigned to Cash if finish Training	724.9	1,207**	0.168**	1,758*	1,910**	432.5	169.9
	(839.9)	(593.0)	(0.0716)	(932.6)	(898.5)	(1,123)	(1,099)
Assigned to Training only	-695.7	-171.3	0.0240	11.75	-76.47	-460.3	-760.6
	(920.7)	(626.2)	(0.0752)	(889.5)	(912.4)	(1,148)	(1,241)
TOT Effects							
Received Training & Cash	1,079	1,786**	0.248**	2,526**	2,819***	642.0	249.6
	(1,169)	(827.5)	(0.100)	(1,032)	(1,023)	(1,233)	(1,220)
Received Training Only	-912.9	-217.3	0.0333	6.206	-86.50	-605.9	-1,042
	(1,152)	(782.4)	(0.0949)	(882.4)	(924.0)	(1,141)	(1,276)
Baseline Mean:	4014	4014	8.14	4004	4023	4016	3994
p-value for equality of treatment effects	0.089	0.035	0.056	0.069	0.041	0.446	0.437
Panel B: Monthly Sales							
ITT Effects							
Assigned to Cash if finish Training	5,171	4,436	0.143	6,818*	3,284	3,079	2,129
	(4,686)	(3,500)	(0.0932)	(4,020)	(5,366)	(6,534)	(6,482)
Assigned to Training only	-2,941	-1,786	-0.0414	-1,718	-1,519	-3,884	-2,248
	(4,422)	(3,512)	(0.0967)	(3,845)	(5,386)	(5,993)	(7,177)
Baseline Mean:	12659	12659	9.09	12198	12520	12600	12548
p-value for equality of treatment effects	0.090	0.102	0.054	0.065	0.411	0.316	0.514
Panel C: Capital Stock							
ITT Effects							
Assigned to Cash if finish Training	17,221**	10,379***	0.155**	9,535*	7,270	12,195*	11,374**
	(7,815)	(3,583)	(0.0691)	(4,893)	(4,932)	(6,379)	(5,760)
Assigned to Training only	-700.2	-490.7	-0.0671	-3,476	-278.1	-4,452	3,389
	(5,616)	(3,338)	(0.0629)	(4,192)	(4,596)	(5,921)	(6,474)
Baseline Mean:	31272	31272	9.48	30137	30359	30538	30350
p-value for equality of treatment effects	0.017	0.003	0.001	0.005	0.113	0.009	0.257
Observations	2,097	2,097	2,019	538	542	512	505
Firms	587	587	581	538	542	512	505

Notes:

Robust standard errors in parentheses clustered at the firm level when all rounds used, *** p<0.01, ** p<0.05, * p<0.1

All specifications also include survey round dummies, baseline outcome value, and controls for randomization strata.

Truncated levels truncate at the 99th percentile.

Table 5: Impacts on Business Ownership and Wage Work for Potential Owners

	Currently Own a Business				Ever own a business	Closed Business	Currently a Wage Worker			
	Round 2	Round 3	Round 4	Round 5	Round 5	Round 5	Round 2	Round 3	Round 4	Round 5
ITT										
Assigned to Cash if finish Training	0.201***	0.140***	0.0261	0.0244	0.0708	0.0634*	-0.0309	-0.0587**	-0.00920	-0.000515
	(0.0465)	(0.0483)	(0.0501)	(0.0485)	(0.0436)	(0.0383)	(0.0282)	(0.0281)	(0.0325)	(0.0317)
Assigned to Training only	0.0904*	0.0628	0.0364	-0.0217	0.0123	0.0166	-0.0252	-0.0753***	-0.0210	-0.0180
	(0.0466)	(0.0490)	(0.0499)	(0.0488)	(0.0446)	(0.0365)	(0.0286)	(0.0275)	(0.0327)	(0.0311)
TOT										
Received Training & Cash	0.290***	0.203***	0.0384	0.0360	0.104*	0.0935*	-0.0449	-0.0862**	-0.0138	-0.000779
	(0.0628)	(0.0658)	(0.0704)	(0.0689)	(0.0614)	(0.0548)	(0.0392)	(0.0394)	(0.0465)	(0.0450)
Received Training Only	0.122**	0.0848	0.0495	-0.0298	0.0161	0.0219	-0.0338	-0.102***	-0.0286	-0.0244
	(0.0606)	(0.0638)	(0.0652)	(0.0643)	(0.0584)	(0.0478)	(0.0370)	(0.0357)	(0.0428)	(0.0406)
Firms	607	601	582	609	620	609	588	587	547	606
P-value for testing equality of treatments	0.020	0.113	0.842	0.357	0.173	0.244	0.830	0.470	0.714	0.584
Control group mean	0.381	0.477	0.555	0.573	0.688	0.155	0.096	0.116	0.116	0.115

Notes: Results shown are for OLS regressions of outcome on treatment assignment. All specifications include randomization strata dummies. TOT instruments receipt of treatment with random assignment. Robust standard errors in parentheses , *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6: Do the grants affect the selection of who owns a business?

	Currently Own a Business			
	Round 2	Round 3	Round 4	Round 5
Panel A: Interaction with Raven score				
Assigned to Cash if finish Training	0.232***	0.149*	0.0956	0.161*
	(0.0840)	(0.0892)	(0.0916)	(0.0869)
Assigned to Training only	0.0640	0.0163	0.0450	0.101
	(0.0835)	(0.0857)	(0.0894)	(0.0860)
Assigned to Cash * Raven Score	-0.0111	-0.00344	-0.0246	-0.0492*
	(0.0254)	(0.0274)	(0.0269)	(0.0253)
Assigned to Training * Raven Score	0.0105	0.0188	-0.00248	-0.0451*
	(0.0261)	(0.0271)	(0.0267)	(0.0255)
Panel B: Interaction with Digitspan recall				
Assigned to Cash if finish Training	0.220	0.0472	-0.147	-0.141
	(0.229)	(0.234)	(0.244)	(0.232)
Assigned to Training only	0.189	0.244	0.163	0.168
	(0.224)	(0.240)	(0.249)	(0.238)
Assigned to Cash * Digitspan recall	-0.00315	0.0153	0.0283	0.0273
	(0.0371)	(0.0379)	(0.0393)	(0.0375)
Assigned to Training * Digitspan recall	-0.0167	-0.0304	-0.0212	-0.0316
	(0.0365)	(0.0395)	(0.0408)	(0.0390)
Panel C: Interaction with household wealth index				
Assigned to Cash if finish Training	0.198***	0.137***	0.0198	0.0177
	(0.0468)	(0.0487)	(0.0500)	(0.0484)
Assigned to Training only	0.0855*	0.0652	0.0344	-0.0226
	(0.0466)	(0.0492)	(0.0500)	(0.0492)
Assigned to Cash * Household Wealth	-0.0369	-0.0276	-0.0594**	-0.0540*
	(0.0293)	(0.0295)	(0.0289)	(0.0282)
Assigned to Training * Household Wealth	-0.0515*	0.00990	-0.0290	-0.0142
	(0.0302)	(0.0309)	(0.0299)	(0.0298)
Number of Firms	607	601	582	609

Notes: Results shown are for OLS regressions of outcome on treatment assignment.
Robust standard errors in parentheses , *** $p<0.01$, ** $p<0.05$, * $p<0.1$
All specifications include randomization strata dummies and level effect of interacting variable.

Table 7: Impacts on Total Work Income and Business Outcomes for Potential Group

	Total Work Income		Profits		Sales		Capital Stock		Business Practices
	R2 and R3	R4 and R5	R2 and R3	R4 and R5	R2 and R3	R4 and R5	R2 and R3	R4 and R5	R4 and R5
Panel A: Experimental ITT Estimates									
Assigned to Cash if finish Training	266.7	696.7	-161.0	804.7	165.5	6,043	7,179	3,959	0.999**
	(556.5)	(728.5)	(741.7)	(830.2)	(3,059)	(3,841)	(7,324)	(8,255)	(0.489)
Assigned to Training only	211.5	1,494*	484.9	2,244**	397.5	6,248*	-2,293	1,048	0.870
	(545.4)	(773.9)	(785.3)	(975.9)	(2,989)	(3,638)	(6,885)	(8,326)	(0.559)
Observations	1,175	1,119	615	675	616	675	615	665	676
Firms	601	585	359	393	359	393	357	385	394
p-value for testing treatment equality	0.920	0.327	0.398	0.165	0.939	0.961	0.218	0.723	0.819
Control group mean	3516	4940	5001	5209	14739	15292	25489	34940	8.33
Panel B: Generalized Propensity Score Reweighted Estimates to account for selection into who operates a business									
Assigned to Cash if finish Training			59.12	767.2	512.5	5,840	6,820	6,083	1.173**
			(692.6)	(846.0)	(3,060)	(3,698)	(7,467)	(8,852)	(0.502)
Assigned to Training only			374.3	2,171**	-349.7	5,950	-1,664	3,341	0.971*
			(772.0)	(1,072)	(2,979)	(3,749)	(7,336)	(8,490)	(0.567)
Observations			590	651	591	651	590	641	652
Firms			345	380	345	380	344	372	381
p-value for testing treatment equality			0.6702	0.2127	0.7728	0.9784	0.2766	0.7533	0.7282

The "Profits", "Sales", "Capital Stock", and "Business Practices" columns fall under the heading **Outcomes Conditional on Operating a Business**.

Notes: Results shown are for OLS regressions of outcome on treatment assignment. All specifications include randomization strata dummies. Robust standard errors in parentheses , clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1

R2 and R3 denotes survey rounds 2 and 3, 4 and 8 months after training; R4 and R5 denote survey rounds 4 and 5, 16 and 25 months post-training.

All outcomes are truncated at the 99th percentile to reduce the influence of outliers.