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Business Training and Female Enterprise Start-up, Growth, and Dynamics: Experimental evidence from Sri Lanka[*]

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Abstract

We conduct a randomized experiment among women in urban 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. We work with two representative groups of women: a random sample of women operating subsistence enterprises and a random sample of women who are out of the labor force but interested in starting a business. We track impacts of two treatments – training only and training plus a cash grant – over two years with four follow-up surveys and find that the short- and medium-term impacts differ. For women already in business, training alone leads to some changes in business practices but 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 entry but leads to no increase in net business ownership by our final survey round. Both profitability and business practices of the new entrants are increased by training, suggesting training may be more effective for new owners than for existing businesses. We also find that the two treatments have selection effects, leading to entrants being less analytically skilled and poorer.

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

JEL codes: O12, J16, L26, M53.

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1. Introduction

Self-employment accounts for a large share of female employment in most developing countries, and it 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 remain out of the labor force. These facts frame the central question addressed in this paper: Can business training, by itself or combined with a grant, raise the income of women in self employment? We examine this question in two different samples of women in urban Sri Lanka. One group was engaged in low-earning self-employment activities at baseline, and the other was out of the labor force but contemplating re-entering. For the first group, we examine the effect of training on income, and for the second, the effect of training on the decision to re-enter the labor force, and the earnings and type of job (wage work vs. self-employment) conditional on re-entry.

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 is not enough to raise the incomes of subsistence-level female businesses. One 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). These women may have a very low optimal efficient scale. A common policy response is to attempt to improve the skill level of these women through business training. As a result, business training programs have been widely used as a development policy for more than 30 years.

We evaluate the impact of the most common training course in developing countries, the International Labor Organization (ILO)'s Start-and-Improve Your Business (SIYB) program. Using a randomized design, we test whether the impact of training alone differs from that of training coupled with access to capital in the form of a grant. We work with two samples. The first sample consists of 624 women operating a business at the time of the project baseline and earning an income of less than $2 per day.[1] 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. We refer to the first sample as "current business owners" and the second as "potential

[1] We describe the manner in which the sample was selected below. The median earnings in this sample was 4000 Sri Lanka Rupees (Rs), or just over $1 per day. About 7 percent of the sample reported income the previous month above 7000 Rs, which is about $2 per day.

business owners." Each sample is randomized into three groups: a control group, a group invited to attend training, and a group invited to receive training and a grant of around $130 conditional on completing training. Individuals are not told whether they are eligible to receive the grant until the completion of training, so that the grant does not differentially alter the composition of who attends training between the two groups.

Among the sample of current business owners, we find that training alone leads to some changes in business practices, but has no effect on business profits, sales or capital stock. In contrast the combination of training and a grant leads to large and significant short-run improvements in business performance for the current enterprises, but these gains appear to have dissipated two years after completion of the training.

Among the sample of potential business owners, training – and especially training combined with a grant – speeds up the process of starting a business and has longer-term effects on the type of women who enter. Women assigned to either training treatment are more likely to be operating a business four months and eight months after the training course, but this entry effect disappears 16 months after training. Instead, later surveys show that training – either with or without the grant – causes women with lower initial analytical ability to open a businesses; the combination of training and the grant leads to entry by women with lower levels of household assets at baseline. Even with these selection effects, we find that the potential entrants assigned to training have higher profits and better management practices two years after completion of training. Business training therefore appears to enable more successful businesses to be started.

This study contributes to an existing and concurrent literature of 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 involve microfinance clients – most of them existing business owners – whose progress is evaluated by a single follow-up survey measuring outcomes six months to one year after training. 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 helps improve sales in bad weeks or bad months, even if there is no impact on the average month.

Our study builds on and differentiates itself from this extant literature in several key respects. First, ours is the only study of which 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 solely on existing microfinance clients. This is important for generalizability, as well as for understanding the potential demand for such training among people who are not implicitly or explicitly compelled to participate by their lender. Second, our intervention employs the ILO's SIYB program, which has been used to train over 4.5 million people in more than 95 countries worldwide. While the content of many of the training courses used in other studies appears similar in broad terms, each has some element of customization that makes comparability across studies difficult. Given that the SIYB program is the most widely applied microenterprise training program, evaluating its effectiveness is of particular interest. Third, we measure outcomes at four points in time post-training over the course of two years. 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.[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 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 were self-employed, mostly in small and marginally profitable enterprises. Our own representative survey of 795 female enterprises in 2008 in the 31 largest urban areas found median profits of only 5000 Rs per month

[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).

(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 female-dominated industries of apparel manufacture (sewing) and processed food production sold in local markets. Given this context, our goal was to investigate the effectiveness of business training to increase female labor force participation rates and to raise the income levels of low-earning women already in business. The first step in this process therefore involved putting together samples of these two groups.

2.1 Generating a sample

In selecting the sample we balanced the goals of creating a representative sample with logistical concerns. We chose to sample in the two largest urban areas in Sri Lanka – greater Colombo and greater Kandy.[3] Within each of the two urban areas we selected 5 D.S. divisions in urban and semi-urban areas. We located a training venue in each of the ten D.S. Divisions, thus minimizing the required travel time for project participants. Within each D.S. division we then conducted a door-to-door screening exercise in selected G.N. Divisions.[4] The short screening survey gathered employment information on females aged 25 to 45 living in the household.

For self-employed women, our screening survey identified women 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 business owner* sample.

The second group of interest was women who were out of the labor force, but who were likely to enter the labor force within the next year. For females out of the labor force and between 25 and 45 years of age, the screening survey 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 start. As a signal of seriousness of intent, we sampled only women who were able to identify the type of business that they planned to start. 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 any children younger than five years of age. Those with children

[3] The project area includes seven of Sri Lanka's 25 districts: Colombo, Kalutara, Gampaha, Kandy, Matale, Kegalle and Kurunegala.

[4] 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 Niladari (GN) Divisions (14,008).

under five years of age were included in the sample only if they had access to other arrangements for child care. We refer to this as our *potential business owner* sample.

Based on this screening exercise, we selected a sample of 628 current business owners and 628 potential business owners. This sample was stratified to take approximately 63 of each type in each 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.)

Table 1 provides summary statistics on the two subsamples. The current business owner sample consists of women in a wide range of industries typical of female self-employment in Sri Lanka, such as tea (snack) shops, beauty shops, bag and mat manufacturing, tailoring and sewing, selling fruit and vegetables or groceries, making and selling lunch packets and string hoppers, baking cakes, and flower shops. The typical owner in our sample is 36, married, with ten years of schooling, and has been running the business for just over six years. Mean monthly business income is around 4000 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 business owners. At baseline few enterprises were implementing the types of business practices taught in the SIYB training program. 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 better management practices (see appendix 1). The mean firm has a baseline score of only 4.6 out of a possible 29. Only 17 percent of firms keep written records, only 4 percent had done any advertising in the last six 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 business training, and the majority of this was technical training. Fewer than 4 percent of owners had ever taken a training course which covered record-keeping, marketing, customer and supplier relations, or management of employees.

Among the potential business owners, 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 current business owners, but are otherwise similar in many respects. In particular, the potential and current business owners 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. The household income of the potential business owners is about 1100 Rs less per month than the households of current business owners, and they are a bit less likely to own refrigerators or sewing machines. In terms of household assets that could potentially also be used for business purposes, 53 percent live in households with a sewing machine and 41 percent in households with a refrigerator, but only 25 percent have a gas cooker and only 7 percent an oven.

2.2 Randomization into treatment and control groups

Our goal is to determine the impact of business training for these women, and to see whether this training has more effect when coupled with a capital grant. We therefore randomly selected 400 current owners and 400 potential owners to be offered business training; half of these were pre-selected to receive a grant of 15,000 Rs (US$129) conditional on finishing the training. We wanted to use the possibility of receiving a grant as an incentive to attend training but did not want the selection of people into training to be affected by the grant. Therefore at the time of offering the business training, individuals were told only that half of those who completed 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 owners, we dropped four enterprises with either missing profits or profits above 10,000 Rs.[5] Randomization to treatment was then stratified by D.S. (10) and by whether or not there were children under age 18 for whom there was no other person to look after them while the mother worked (54%). Within each stratum we then formed triplets of firms on the basis of baseline profits, and randomly allocated two out of three 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. being chosen to be offered training.[6]

For the potential business owners, 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).

[5] 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.

[6] 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.

Within the 38 non-empty stratum we then randomly allocated one-third to training only, one-third to training with a grant conditional on completion, 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 the authors 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 is designed to meet the needs of small-scale entrepreneurs in developing countries, and started in Eastern Africa in 1977 (ILO, 2003). More than 4.5 million people in 95 countries have participated in SIYB training.[7] 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 three day training course designed to help potential entrepreneurs decide if they should 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 works with potential entrepreneurs who want to start their own business and already have a feasible business idea. The training is a five day course covering the main aspects of starting a business, including selection of products and pricing, organization of staff, purchasing of equipment and other inputs needed to get started, and financial planning. Participants produce a business plan that they can use as a step-by-step guide to starting their business.
- Improve Your Business (IYB) is a five day course which trains existing business owners who want to develop their business. The modules covered are marketing, buying, costing, stock control, record keeping, and financial planning.

In addition to these core modules, we added one day of training of a more technical nature. The purpose of this was to provide exposure to some relatively high-return sectors in which it is

[7] See: http://www.ilo.org/empent/areas/start-and-improve-your-business/WCMS_159435/lang--fr/index.htm.

perceived to be socially acceptable for women in Sri Lanka to work. These included food manufacturing, beauty culture (hair dressing, beauty treatments and bridal dressing), sewing clothes, plant nursery, and soap manufacturing. Based on participant interest, at least two of these modules were offered at each training location and participants could select their preferred technical training option. The most popular choices were 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.[8] SLBDC introduced 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 SLBDC had eight years experience delivering SIYB to the local market. 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. As such, they are all certified by the SIYB Association of Sri Lanka as SIYB Trainers. 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 sent a letter explaining the details of the course and the offer. Project staff also visited them in-person visit and made phone calls to make sure they had received and understood the training offer. The potential owners were offered a package consisting of the GYB and SYB courses, followed by the technical training. The resulting course was nine days, with seven hours of instruction per day. The current business owners were offered a package consisting of a compacted refresher GYB course to help them think about new products they might offer or sectors they might work in, followed by the IYB course and technical training. Overall to they received seven days of training, with seven 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 business owner and $133-$140 per potential business owner. The training was offered to participants for free, which mimics the approach used by many NGOs and microfinance institutions. In the baseline, 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,

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

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 even the 80th percentile response is an order of magnitude lower than what the market cost of the training.

Most existing evaluations of business training programs have targeted microfinance clients. Since group members meet regularly, and since the training is something that their lending organization often explicitly encourages them to do, such experiments are typically able to achieve quite high take-up of the training offered. Whether the average potential or existing business owner will show up for training if offered is a much more open question.

We began by offering the training to an initial group of 40 of the potential enterprises in two 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 seven out of the 40 women who were offered this training showed up on the first day. The remainder gave reasons such as illness, no one to look after their children, or lack of interest. We therefore revised our offer to 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. We explained this in personalized visits to each individual in the treatment group. After finalizing our financing for the grants, we also told the treated sample that half of those completing training would be randomly chosen for a 15,000 Rs grant. This information was then provided to all individuals in this first training group, and to individuals selected for later training groups as well.[9]

3.2 Take-up of Training

Training took place in April and May 2009 and the cash grants were dispersed 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

[9] The potential business owners in this training group were later offered a second chance to attend training with the per diem payment and the understanding that half of those completing the training would receive a grant. The rates of take-up in the two D.S. Divisions involved in the first training offer do not appear to have been affected significantly.

owners (70.5%) offered training attended at least one training session, and 261 (65.3%) received completion certificates. Among the 400 current business owners offered training, 279 attended at least one session (69.8%) and 268 individuals (67.0%) received completion certificates.

We asked those who did not attend the main reason for their absence. The most common answer among current business owners was that they or a family member was sick (25%), or that they had no one to look after their business (17%) or their children (16%). Only 4.5% said they did not think the training would be valuable and 3.6% said they could not 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. 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 owners attended the training amongst those receiving the offer. Column 1 shows married and more educated women, and those running younger firms, were more likely to attend the training. Contrary to our prior expectation, having no one else to care for young 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, and there is no association between baseline elicited willingness to pay for such training and actual attendance. The opportunity cost of time seems to matter: 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 (urban) 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 central Colombo, compared to 69 to 80 percent completion rates in the other districts. These Colombo firms earned higher profits. Controlling for district fixed effects eliminates the predictive effect of profits, but does not change the size or significance of the other variables.

The last three columns of Table 2 examine take-up of training for potential owners. We find that take-up increases with the age of the woman, and with the score on a Raven non-verbal reasoning test. But take-up 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 lowest in Colombo.

In both qualitative interviews and direct survey questions, those who attended appeared satisfied with the course. Among current enterprise owners surveyed in round two who had taken the course, 78 percent said they would strongly recommend, and 17 percent said they would somewhat recommend, the course to a friend currently running a business; 86 percent said that the course was more helpful than they had expected. Similarly, 81 (17) percent of potential owners said they would strongly (somewhat) recommend the training to someone starting a business, and 85 percent 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 - corresponding to 3-4 months, 7-8 months, 15-16, and 24-25 months after the training. We refer to these as rounds 2, 3, 4 and 5 surveys, respectively. The 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. Appendix 3 addresses the possibility that training affected how profits were reported; we find that this is not driving any of our results.

Overall attrition was low – of the 624 (628) current owners (potential owners) 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 – a test of equality of attrition rates by round 5 has p-value of 0.085 for this group. Appendix 2 shows that the results from the sample of potential owners 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 available for 97 percent of firms.

As a final point of context, it is worth noting that on May 18, 2009, towards the end of our training sessions, the Sri Lankan government formally declared the end of a 25-year civil war. GDP growth was 8.0 percent in 2010 and 8.3 percent in 2011, the highest growth level since Sri Lanka's independence.[10] Growth was widespread through manufacturing and services. As such, our interventions took part in an environment in which the economy was growing and there may have been possibilities for firms to start-up and/or expand.

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

Training and grants may have effects on both the intensive and extensive margins. That is, they may affect business outcomes conditional on operating a business, and also affect the likelihood that an individual of a given type operates a business. In the context of our project, the effects at the extensive margin are particularly likely in the sample of potential entrants. To clarify these selection effects, we sketch a simple model of occupational choice.[11]

We start by assuming that 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. Individuals not starting an enterprise can earn w as a wage worker or opt out of the labor market. In the latter case, we assume they obtain wage-equivalent utility of w from leisure and household production.[12] 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 individuals with sufficient wealth and ability find starting a business worthwhile. Let e denote the entry choice, with $e=1$ if the individual starts a firm, and $e=0$ otherwise. The individual chooses e and k to maximize:

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

[10] http://www.statistics.gov.lk/national_accounts/Press%20Release/PRESS%20NOTE%20%202011%20Annual%20English.pdf [accessed 23 May, 2012].

[11] 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.

[12] 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.

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

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 just enough wealth to pay the start-up costs but not buy any capital *k* do not want to enter. We assume that the marginal return to invested capital exceeds r even for those with zero ability. This implies that there is a well defined minimum level of wealth *A***, which makes it worthwhile to start a firm with zero ability. *A***is defined by $w + rA^{**} = f\ A^{**} - A^*, 0$.

Figure 2 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 by choice – they can 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 what the model tells us about the impact of business training on business entry. Suppose business training builds business skills by amount *T*, so that individuals now only need talent θ^*-T to enter with zero wealth. Moreover, they now need only $A_1 < A^*$ to be able to enter with innate talent 0, where $x(T)=A_1$, and would prefer self-employment to wage work when they have zero innate talent and wealth of at least A_2, where $w + rA_2 = f\ A_2 - A_1, T$. $A_2 < A^{**}$ for two reasons – training reduces the entry cost, leaving more wealth to be invested as productive capital, and training increases the earnings at any given level of invested capital.

Figure 3 shows that training lowers the innate talent required to start an enterprise. As a result, we should expect training to increase entry among individuals whose ability level was previously too low given their wealth to start a business. That is, training will induce entry by 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).[13] Whether the average innate

[13] If training were offered on a mass scale, then we would need to worry about general equilibrium effects as wage workers enter self employment. Our sample is dispersed enough that we can safely ignore these effects.

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 in addition to training will reduce the initial assets required to start a business, rotating the two curves down so that they cross the wealth index at lower points. This will draw into business individuals who were previously too poor given their abilities. Thus, we should also expect the training and grants combination to affect the selection of who operates a business.[14]

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 the individual's credit constraint. 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 other assets. If capital and skills are complements in production, then the trainees will wish to invest more, and so $k^{**} > k^*$ and the 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. There is then a possibility that profits actually decrease as a result of the training, though in most cases the net effect of the higher level of effective labor and the lower capital stock will still result in higher profits.

Taken as a whole, business training should therefore change the selection among potential business owners of who open a business. There is some theoretical ambiguity in the impact of training on average profits for those who do open a business due to the selection effect and also some uncertainty in the effect for those who would have opened a business anyway.

Finally, consider existing firm owners in this context. They have already paid the start-up cost *x*, and so business training works only through increasing the productivity of a given capital stock and by causing the firm to change its optimal capital stock. As with the entrants, if capital and labor are substitutes for one another it is possible to construct production functions which

[14] The model is a one-period model for simplicity of exposition. But allowing individuals to save their wage earnings over multiple periods would not have much effect on the predictions with respect to training and grants. The long-run impact on entry would 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, in the longer-term, the impact on entry would come from individuals for whom it is profitable to run a business with skill level θ+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.

result in a decrease in profits following the training. However, if skills and capital are complements then profits will increase.

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

We want to estimate the impact of being assigned to training only, or to training plus a cash grant, on business outcome $Y_{i,t}$ for firm *i* in follow-up time period *t*. We estimate the following Ancova model using the follow-up surveys:

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

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 includes the lagged outcome variable and so is possible only for the current enterprises (since potential owners did not have baseline business outcomes). 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). We estimate equation (1) round-by-round to determine how the treatment effects vary over time.

In addition, we can estimate the average treatment effect over all follow-up rounds. In doing so we gain additional power by pooling together several rounds of follow-up surveys. We estimate the following regression:

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

Where q 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. Under the assumption that the offer of training does not affect the outcomes of interest for those

who do not take up the training, we can also estimate the treatment-on-the-treated (TOT) effect – the effect of training for those who take it up. The required assumption seems plausible if we define take-up as going to at least one training session, but could be violated if we try to measure 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 attended at least one training session. Since almost all individuals who attended at least one session completed the training, in practice the distinction will not matter much. We estimate the TOT by instrumenting receipt of some training 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

In Table 3 we examine whether the training led to any improvements in business practices taught in the course. These practices were measured in the baseline, and again in the round 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 (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 improvement in practices slightly higher for those receiving the training plus grant treatment.

The magnitude of the increase is large relative to the low baseline levels, with the TOT showing an increase of two to three total practices relative to a baseline mean of five practices. However, given the maximum possible practices score is 29, the treated firms are still clearly a long way from implementing all the practices taught in the SIYB training. Columns three through seven 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 do not appear to have affected which firm survived: treatment status is unrelated to

observable characteristics among surviving firms. Although a part of the training was intended to encourage women to expand into other lines of business, in the round 5 data we find only a small and statistically insignificant impact of the training or training plus grant on change in product lines, introduction of new products or services, or starting a second business since baseline.[15] Given these findings, we proceed to analyze business outcomes for the group of surviving firms.

Table 4 examines whether the improvements in business practices following the training, along with the cash grants, 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). Panel D examines changes in hours worked in the business by the owner. For each outcome we first show the results using all follow-up rounds pooled together to achieve greatest power, and then examine the effects round-by-round. 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. The second, in column (3) takes the log of the outcome of interest. In panel A, we report both the ITT and the TOT for profits. For sales, capital stock, and hours we just report the ITT estimates. Approximate TOT estimates can be obtained by dividing the ITT by the proportion of firms attending some training (0.70). Finally, Figures 4A and 4B show the cumulative distribution functions of profits by treatment status for round 2 and round 5 respectively. These allow us to better assess in which parts of the distribution effects occur.

The results show that business training alone does not improve profits, sales, or capital stock of current firm owners, or change the number of hours the owners spend working in their businesses. 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 firms assigned to training only using significantly more, or significantly less, capital stock, with the point estimates small relative to the mean. The fact that profit does not increase suggests the owners are not substituting skills for capital. Instead, they appear to be using the same amount of capital to generate the same level of sales and profits.

[15] The point estimates are 5.7-6.7 percentage point increases in the likelihood of changing or adding product lines relative to a control group mean of 16.9 percent. An F-test that the two treatment effects are jointly zero has p-value 0.22, and neither is individually significant at standard significance levels.

These conclusions hold in each of the follow-up rounds, suggesting that this is not a case of training taking time to show impact.

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 amount as the grant for those who completed the training and received the 15,000 Rs. There is a significant increase in profits when using the pooled sample with truncated or log profits. This effect comes from the round 2 and 3 data. However, it appears that this increase in profits is only temporary; the point estimate on profits is much smaller by the rounds 4 and 5 surveys.[16] This is seen also in Figure 4B, where the CDF of profits in round 5 almost on top of that of the other two groups. The sales data are noisier than the profits data, and we generally see a positive, but not significant, impact on sales. Hours worked are higher in the round 2 survey, but not significantly different from hours worked in the control group in subsequent rounds.

The combination of training and a grant therefore have immediate effects which appear to dissipate 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 was not possible to tell who would complete training, precluding the inclusion of a treatment group receiving only cash conditional on being the sort of person who would complete 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. The control for capital stock nets out both the liquidity effect of the grant and the effect of training on the optimal capital stock that might arise, for example, through learning about how to access credit, or through re-investing profits. Under a sequential ignorability assumption (Imai et al, 2010), controlling for capital therefore gives us a lower bound on the impact of training among the sample of individuals receiving the grant.

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 as much as two-thirds of the effect is due to the grant. Looking just at the round 2 profits, the control for

[16] However, a test of equality of the impact on truncated profits over rounds 2 and 3 and the impact over rounds 4 and 5 has p-value 0.16 (p=0.0998 for log profits), so we can at best marginally reject no decline in profits.

capital stock reduces the coefficient on the training / grant treatment from 1758 to 971, suggesting that as much as half of the initial effect is 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 speeding up convergence to a steady state, but not dramatically changing the trajectory of the enterprise.

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 affect the likelihood that an individual enters self-employment or engages in wage work 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 operates a business at the time of the round 2 survey, increasing to 56 percent by the round 4 survey and 57 percent by the round 5 survey. This high rate of start-ups may reflect both the screening of our sample, and the rapid growth of the economy during this period, improving the environment for starting a new business.

The combination of cash and training results 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 the gap closes completely in rounds 4 and 5. This suggests that the treatment sped up entry markedly, but had no longer term effect. Those receiving the cash and training treatment are 10 percentage points more likely to have opened a business at any point since baseline, and 9 percentage points more likely to have opened a business and then closed it – with these two effects netting out to give the near zero and insignificant effect on round 5 business ownership.

Training alone leads to a smaller, but still significant, 12 percentage point increase in the business ownership rate in round 2 for those attending training. The impact falls in magnitude and loses statistical significance 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. As with self-employment rates, however, 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 had no long-term impact on ownership rates. But 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 the effect of ability on selection by graphing lowess lines of business ownership against Raven score by treatment group. The figure shows that entry increases in the Raven score throughout the distribution for the control group, in line with our theoretical model. Business ownership rates are relatively constant across skill levels for the two treatment groups both because of higher entry rates among those with the lowest baseline ability, as predicted by our model, and because of lower entry rates among those with the highest baseline ability.

That more skilled women have lower rates of business ownership by the last round survey in the treated groups compared with the control is something of a puzzle. The last four columns of Table 6 examine this from two perspectives. First, columns 5 and 6 look at heterogeneity in terms of whether individuals have ever started a business and have closed a business by round 5. Neither interaction between treatment and ability is statistically significant when we look at the two treatment effects separately, but we do find an effect on business closure which is significant at the 10 percent level if we pool the two treatments ($p= 0.085$). The results suggest that skilled women are no less likely to have ever started a business if they are trained, but are more likely to have closed a business. The last two columns of Table 6 show that

higher ability treated women are no more likely to be wage workers, but are more likley to be out of the labor force. This is particularly true among those eligible for both training and cash. 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. Plausibly, business training is more of a substitute for the types of business skills analytical reasoning would otherwise be employed for.

Panel C of Table 6 examines heterogeneity with respect to baseline household wealth. We find 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 that 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 induced poorer individuals to start a business – a pattern consistent with the model - but also to have reduced business ownership in round 5 among individuals from richer households. The last four columns provide some suggestive evidence that the cash grant led women from higher wealth households to be more likely to close a business and be out of the labor force in round 5, but neither effect is significant at the 10 percent level (the interaction with the cash treatment has p-value 0.138 for being out of the labor force).

5.4 Impact on Business Outcomes for Businesses Started by Potential Owners

The treatments led to more business entry in the short-run. In the medium-term, the effect of treatment on the entry rate disappears, but treatment appears to affect the characteristics of entrants. This complicates the comparison of the business outcomes for treatment and control groups, and so we employ different 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) combined. This income is zero for women who are not working.[17] By this definition, income is not subject to issues of selection into employment, and we can obtain experimental estimates as we did 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

[17] Thus, in this analysis we ignore the utility from household production or other non-market activities.

medium-term effect. Column 1 shows that despite the two treatments having significant impacts on the likelihood of self-employment in the short-term, the treatments have no significant effect on total earnings. In part this reflects the lower likelihood of wage employment. The point estimates are positive, but are equivalent to only about 7 percent of the control group's income. In contrast, the impacts are larger in the medium-term, with increases in work income of 1494 Rs for the training only group (significant at the 10 percent level), and 697 Rs (not significant) for the training plus cash group. We also can not reject that the possibility that the two treatments have effects which are equal in magnitude. The medium-run point estimates reflect increases in income of 14 to 30 percent relative to the control group mean. The estimates suggest that the training-only treatment costs of approximately 18,600 Rs per person would be repaid after 12 months of the medium-term treatment effect, but the training plus cash cost would take 48 months of the medium-term treatment effect to be repaid.

The remainder of panel A looks at the impact of the treatments on business profits, sales, capital stock, and business practices. These results are all conditional on running a business. We asked the potential business owners about business practices only in rounds 4 and 5. 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, the impact on business practices is similar in magnitude for the two treatments, and the point estimate of the effect on capital stock is larger for the cash plus training treatment. 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 and a 40 percent increase in sales, both relative to the control mean.

There is no difference in the rates of business ownership by rounds 4 and 5 of the survey, but our analysis has shown that both treatments led to women with lower Raven scores starting businesses and the cash plus training treatment led to women with lower wealth starting businesses. We would expect this selection to bias the OLS estimates downwards, since lower ability and poorer individuals might be expected to earn lower profits. In this sense, the estimates in panel A might be considered lower bounds for the treatment effect.

How sensitive our results are to this selection? To provide some indication, we use the sample of business owners to estimate a multinomial logit for the probability of being in each of our three treatment groups. We use the set of variables that were used in Table 2 to predict training take-up, including 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.[18] Since the initial sample was randomized and the selection was not extreme, the GPS distributions look quite similar across the three treatment groups; trimming dissimilar observations removes only a few observations.

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

Taken as a whole, the results for the potential business owner sample suggest that the interventions sped up the process of starting a business, and that the firms that were created employed slightly more business practices and are somewhat 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 the intervention.

5. Discussion and Conclusions

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 that women completing training undertake a few more of the practices taught by the training in their enterprises, but this is generally found 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 following training, but these enterprises are no more profitable

[18] 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.

than the control group two years after training. These results highlight the challenge in getting subsistence-level female-owned microenterprises to grow, and suggest that the binding constraints on their growth may lie outside the realm of capital and skills. One 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 addressing constraints to female participation in wage work, as these labor market failures are potentially the reason that many women are operating businesses in the first place (Emran et al, 2007).

In contrast, our results are more encouraging for using business training to help women who are out of the labor force start enterprises more quickly, and for improving the management and profitability of these businesses. 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 find that giving microfinance to poor women with a high propensity for business ownership leads to some new business start-ups. Taken together, these results suggest getting women to start a subsistence businesses is easier than getting 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.

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Appendix 1: Business Practices Score

The *total score* – the composite business practice score -- ranges from a minimum of -1 to a maximum of 29. The total is the sum of the 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 among potential business owners they are marginally lower for the training only group than they are for the training plus grant and control groups. 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 bounds are reasonably narrow and the conclusion that there is a large treatment effect in the short-term which closes in the medium-term is robust to this attrition.

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.

Appendix 3: Are the results due to business training changing measurement?

A key challenge for evaluations of business training programs is the possibility that training causes owners to better understand the finances of their firm, leading to changes in how profits and other financial outcomes are reported in surveys. We use two approaches to explore the robustness of our results to this concern.

The first approach is to control directly for the record-keeping practice score in our treatment regression. Columns 1 and 2 of appendix Table 2 shows our treatment impacts on truncated profits for the combined round 4 and round 5 data without and with this control, respectively. We do find that better record keeping is positively and significantly associated with higher profits. This might reflect causation in either direction – faster growing firms may use more record-keeping, or record-keeping may help firms earn more profits or to report a larger share of profits. However, controlling for record keeping does not change our conclusions for the impacts of the treatments on profits for the current or potential firm groups. This suggests that the failure to find an increase in profits among the current enterprises is not due to improvements in record keeping leading them realize profits are lower than they thought.

Similarly, the finding of a significant positive effect for potential enterprise owners does not appear to be caused by better record-keeping making them realize profits are higher than they thought.

The second approach is to check whether training affects the difference between self-reported profits (elicited via a direct question), and business revenue minus expenses. De Mel et al. (2009b) discuss several reasons why these two measures of profits may differ, such as mismatches in the timing of input purchases and sales based on those purchases. Nevertheless, if the training causes individuals to keep better track of their finances and start calculating profits differently with greater reference to revenue and expense records, we would expect training to reduce the absolute difference between these two measures. Column 3 of appendix Table 2 shows that this is not the case – training leads to an insignificant increase in the absolute difference between the two measures.

Taken together, these two approaches suggest that our results are not being driven by changes in reporting of profits.

Appendix Table 2: Are Impacts Being Driven by Better Record-Keeping?

	Truncated Profits in rounds 4 and 5		Absolute difference between revenue-expenses and reported profits (R4 and R5)
Panel A: Current Enterprises			
Assigned to Cash if finish Training	274.6	-230.1	1,892
	(809.0)	(797.5)	(1,371)
Assigned to Training only	-543.9	-769.2	1,432
	(868.8)	(872.4)	(1,409)
Record-keeping Score		493.5***	
		(169.1)	
Observations	1017	1017	1016
Panel B: Potential Enterprises			
Assigned to Cash if finish Training	804.7	743.1	839.9
	(830.2)	(827.3)	(858.5)
Assigned to Training only	2,244**	2,020**	1.067
	(975.9)	(940.8)	(785.9)
Record-keeping Score		718.4***	
		(171.4)	
Observations	675	675	675

Notes: Regressions in columns 1 and 2 also control for baseline profits and baseline record-keeping score for the current enterprises. Standard errors in parentheses, clustered at the firm level. *, **, and *** indicate significance at the 10, 5 and 1 percent levels respectively. Absolute difference between profits and revenue minus expenses truncated at the 99th percentile.

Figure 1: Timeline

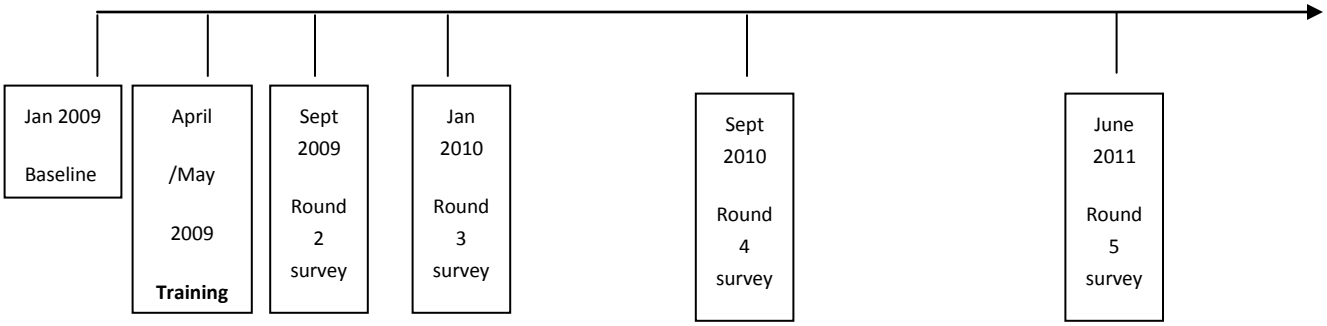

Jan 2009
Baseline
April
/May
2009
Training
Sept
2009
Round
2
survey
Jan
2010
Round
3
survey
Sept
2010
Round
4
survey
June
2011
Round
5
survey

Figure 2: Occupation choice by entrepreneurial ability and wealth

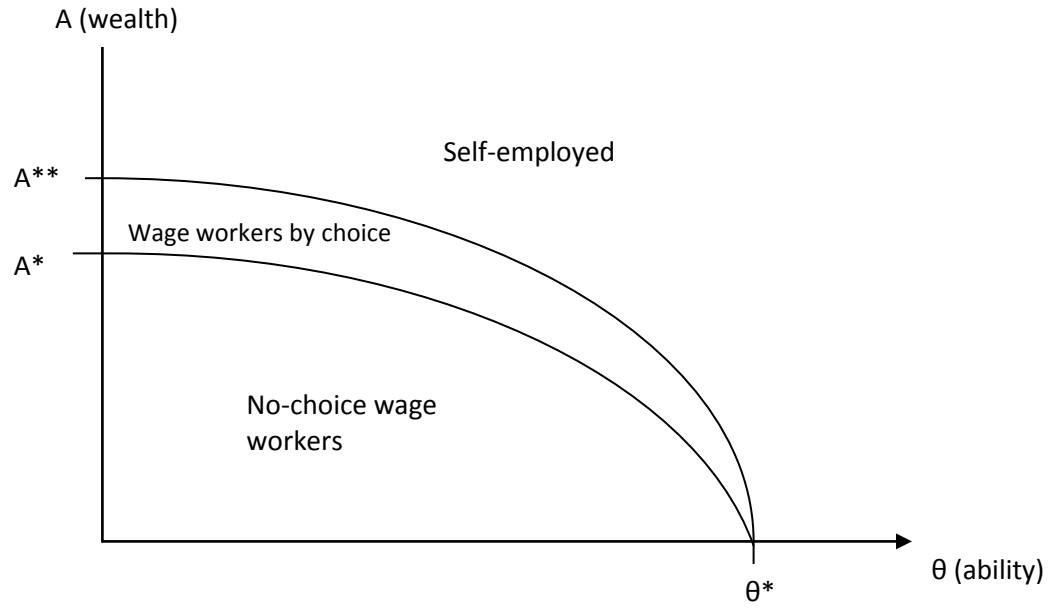
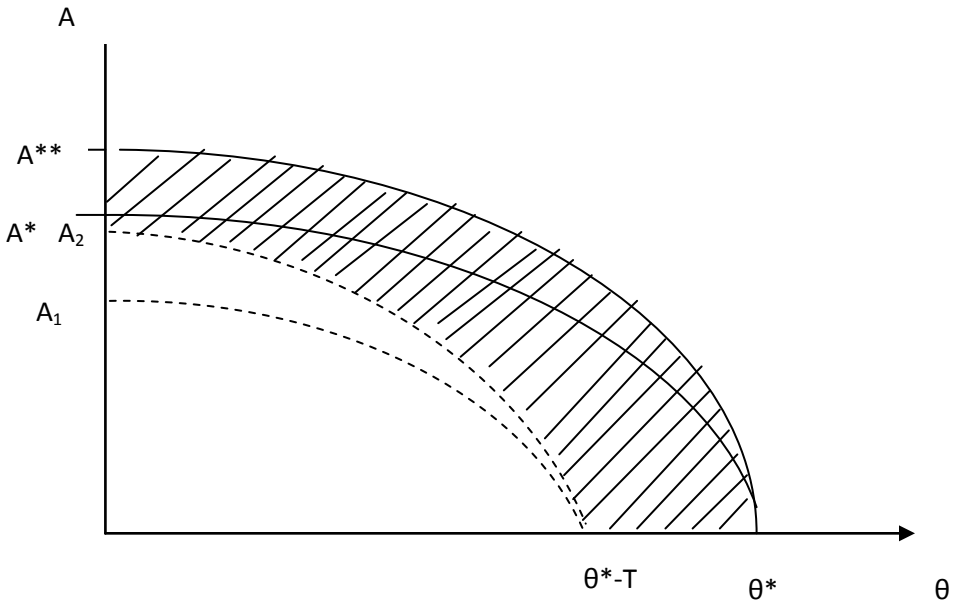


Figure 3: Impact of business training on occupational choice



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

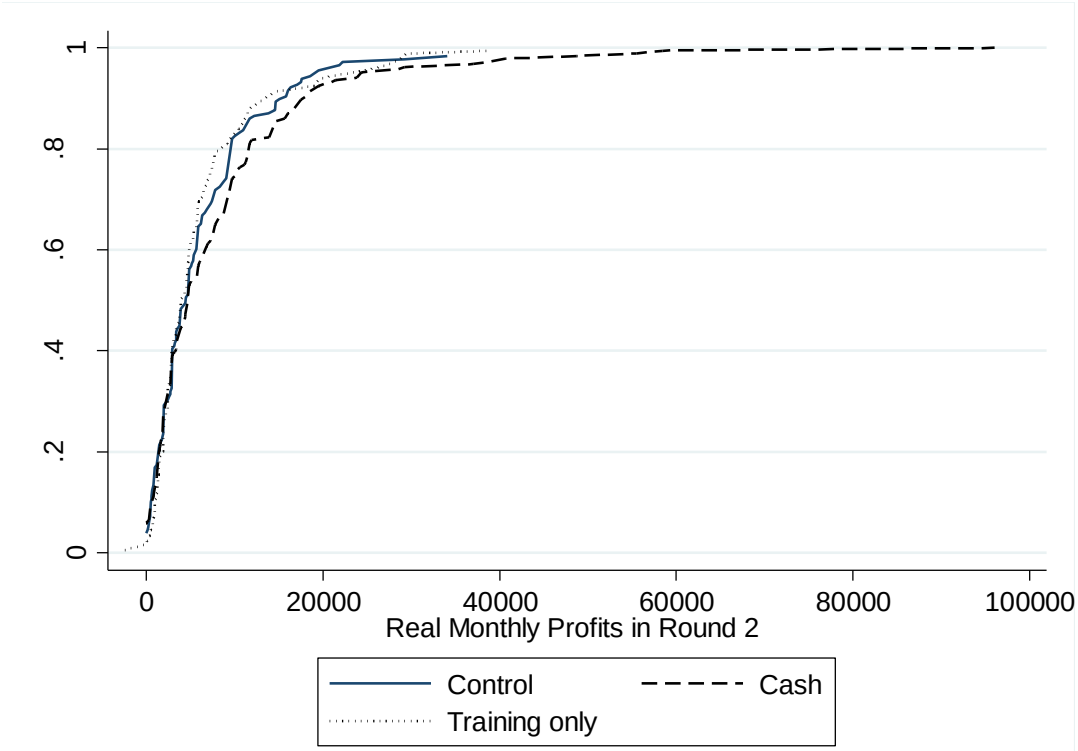


Figure 4B: CDF of Monthly Profits of Current Enterprises at Last follow-up Survey

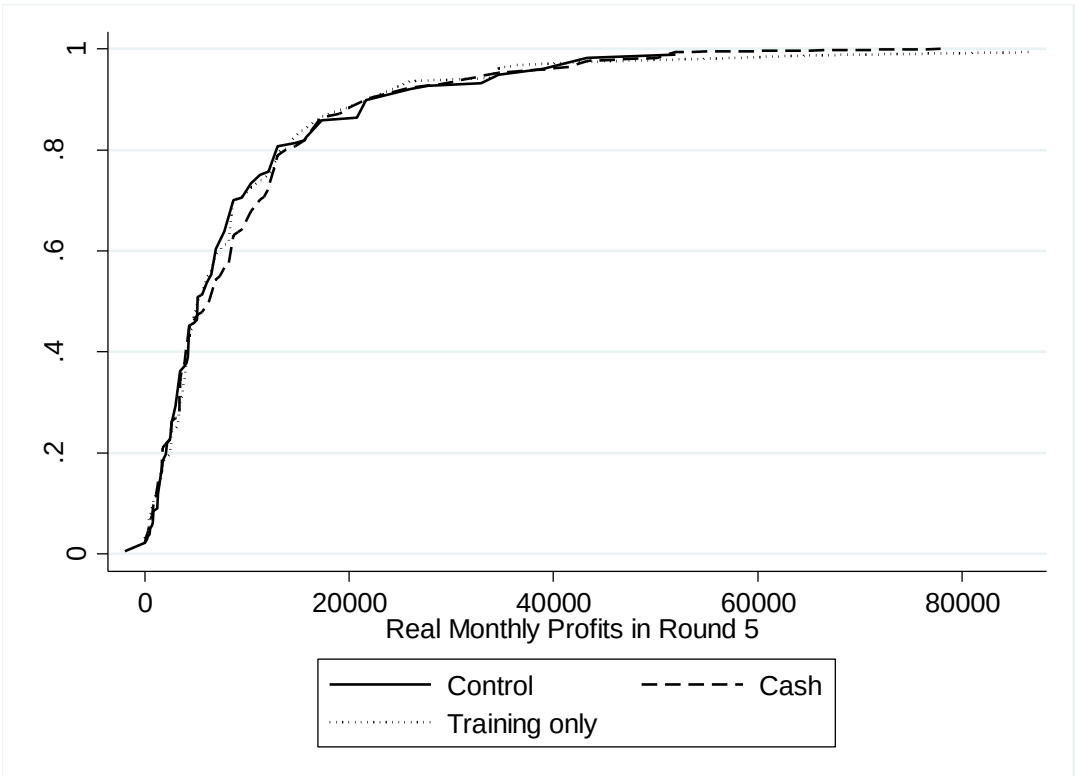
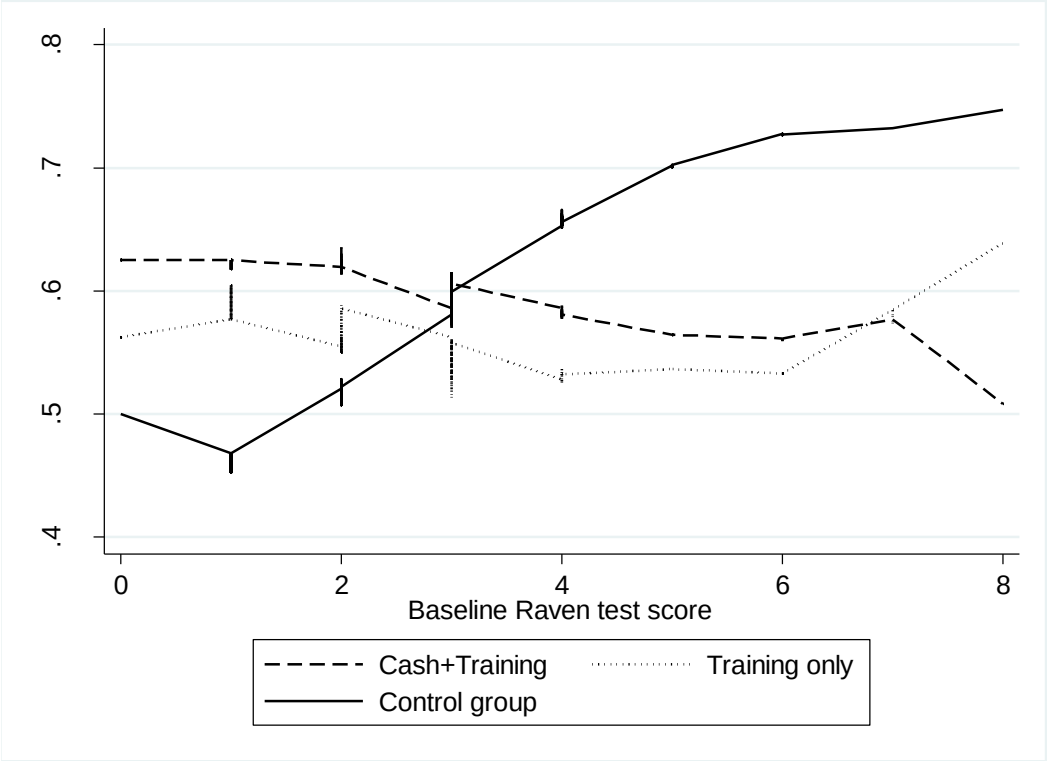
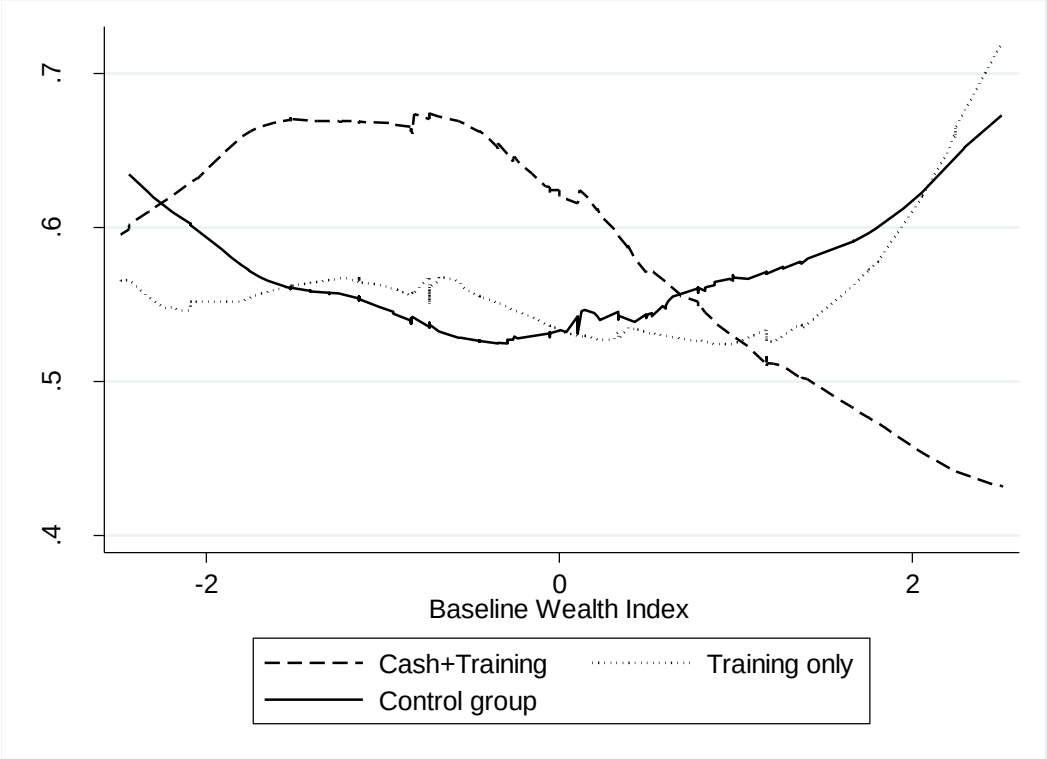


Figure 5A: Round 5 Business Ownership – Ability Profile by Treatment Status



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
Panel D: Hours Worked in Last Week							
ITT Effects							
Assigned to Cash if finish Training	1.932	1.751	-0.0211	4.537*	1.855	1.120	-1.717
	(1.433)	(1.382)	(0.0320)	(2.411)	(2.504)	(2.579)	(2.701)
Assigned to Training only	1.685	1.889	-0.0130	3.337	4.130	0.561	-2.038
	(1.436)	(1.399)	(0.0340)	(2.534)	(2.802)	(2.570)	(2.687)
Baseline Mean:	43.5	43.4	4.0	43.3	43.5	43.5	43.6
p-value for equality of treatment effects	0.866	0.921	0.799	0.621	0.414	0.826	0.904
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				Ever Own	Closed Business	Wage Work	Not working
	Round 2	Round 3	Round 4	Round 5	Round 5	Round 5	Round 5	Round 5
Panel A: Interaction with Raven score								
Assigned to Cash if finish Training	0.232***	0.149*	0.0956	0.161*	0.0846	-0.0224	0.0406	-0.195**
	(0.0840)	(0.0892)	(0.0916)	(0.0869)	(0.0769)	(0.0711)	(0.0550)	(0.0822)
Assigned to Training only	0.0640	0.0163	0.0450	0.101	0.0320	-0.0503	-0.0645	-0.0187
	(0.0835)	(0.0857)	(0.0894)	(0.0860)	(0.0799)	(0.0633)	(0.0550)	(0.0841)
Assigned to Cash * Raven Score	-0.0111	-0.00344	-0.0246	-0.0492*	-0.00500	0.0308	-0.0147	0.0586**
	(0.0254)	(0.0274)	(0.0269)	(0.0253)	(0.0221)	(0.0211)	(0.0144)	(0.0242)
Assigned to Training * Raven Score	0.0105	0.0188	-0.00248	-0.0451*	-0.00708	0.0246	0.0180	0.0209
	(0.0261)	(0.0271)	(0.0267)	(0.0255)	(0.0241)	(0.0178)	(0.0176)	(0.0240)
Panel B: Interaction with Digitspan recall								
Assigned to Cash if finish Training	0.220	0.0472	-0.147	-0.141	-0.117	0.0699	0.0640	0.0762
	(0.229)	(0.234)	(0.244)	(0.232)	(0.218)	(0.185)	(0.147)	(0.222)
Assigned to Training only	0.189	0.244	0.163	0.168	0.0560	-0.115	-0.105	-0.0766
	(0.224)	(0.240)	(0.249)	(0.238)	(0.222)	(0.199)	(0.137)	(0.237)
Assigned to Cash * Digitspan recall	-0.00315	0.0153	0.0283	0.0273	0.0310	-0.00106	-0.0107	-0.0178
	(0.0371)	(0.0379)	(0.0393)	(0.0375)	(0.0353)	(0.0297)	(0.0234)	(0.0360)
Assigned to Training * Digitspan recall	-0.0167	-0.0304	-0.0212	-0.0316	-0.00728	0.0221	0.0144	0.0197
	(0.0365)	(0.0395)	(0.0408)	(0.0390)	(0.0364)	(0.0327)	(0.0222)	(0.0389)
Panel C: Interaction with household wealth index								
Assigned to Cash if finish Training	0.198***	0.137***	0.0198	0.0177	0.0696	0.0684*	-4.25e-06	-0.0249
	(0.0468)	(0.0487)	(0.0500)	(0.0484)	(0.0438)	(0.0386)	(0.0314)	(0.0454)
Assigned to Training only	0.0855*	0.0652	0.0344	-0.0226	0.0121	0.0174	-0.0187	0.0423
	(0.0466)	(0.0492)	(0.0500)	(0.0492)	(0.0447)	(0.0368)	(0.0305)	(0.0473)
Assigned to Cash * Household Wealth	-0.0369	-0.0276	-0.0594**	-0.0540*	-0.0144	0.0356	0.0185	0.0385
	(0.0293)	(0.0295)	(0.0289)	(0.0282)	(0.0254)	(0.0227)	(0.0184)	(0.0259)
Assigned to Training * Household Wealth	-0.0515*	0.00990	-0.0290	-0.0142	-0.00663	0.00817	0.00815	0.00904
	(0.0302)	(0.0309)	(0.0299)	(0.0298)	(0.0258)	(0.0222)	(0.0179)	(0.0283)
Number of Firms	607	601	582	609	620	609	606	606

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

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

			Outcomes Conditional on Operating a Business						
	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

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.