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 study two groups of women: a random sample operating subsistence enterprises and a random sample 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. For women in business, training changes business practices but has no impact on business profits, sales or capital stock. The grant plus training combination increases business profitability in the first eight months, but this impact dissipates in the second year. Among potential startups, business training hastens entry – without changing longer-term ownership rates – and increases profitability. We conclude that training may be more effective for new owners.

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 (Banerjee and Duflo 2008; de Mel et al., 2009a; Kevane and Wydick, 2001). 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?

In previous experiments in Sri Lanka (de Mel et al., 2008, 2009a, 2012) and 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). A common policy response is to attempt to improve the skill level of these women through business training.

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. This program has been given to over 4.5 million people in more than 95 countries worldwide. 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 an unrestricted grant. We work with two samples. The first sample consists of 624 women operating a business and earning an income of less than $2 per day. 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 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.

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 training. Among the sample of potential business owners, training – and especially training combined with a grant – speeds up the process of starting a business. 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. However, training does appear to enable more successful businesses to be started, with potential entrants assigned to training having higher profits and better management practices two years after completion of training than businesses started by the control group.

This study contributes to a growing literature of business training experiments with women (or with men and women) in developing countries.[1] 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. As McKenzie and Woodruff (in press) discuss, the combination of sample size and sample heterogeneity leaves most of these studies underpowered. Because we select more homogeneous samples of women and conduct four follow-up survey rounds, the study we report here has sufficient power to detect comparatively modest changes in profits and sales. Unlike the previous literature, we are also able to look at the impact of training for potential owners, rather than only existing businesses.[2] The four survey rounds also enable us to measure the trajectory of impacts, which turns out to be very important in practice, especially for potential business owners.

The fact that we find little impact of training on the profitability of existing businesses, even with a representative sample of subsistence enterprise owners and with more power than these other studies suggests that the lack of impacts in most of the existing literature may not just be due to power issues, and that as a result business training programs are less effective for existing enterprises than proponents of such training would suggest. However, our results also suggest some potential for impacts on new owners.

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 our main results; and Section 5 concludes.

2. Context, the sample and randomization

The urban labor force participation rate for women aged 20 to 40 in Sri Lanka was 43% in 2009, compared to rates over 90% for prime-aged men (Department of Census and Statistics, 2010). Twenty-eight percent of women in paid work were self-employed, mostly in small and marginally profitable enterprises. 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.

2.1. Generating a sample

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 h 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 start working within the next year. The screening survey asked each female between 25 and 45 years of age whether she planned to enter self employment in the next year and, if so, 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. We refer to this as our *potential business owner* sample.

Based on this screening exercise, we selected a sample of 628 current business owners spread across 137 G.N.s and 628 potential business owners in 145 G.N.s. 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 Fig. 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% of firms kept written records, only 4% had done any advertising in the last six months, and only 3% had made a budget of what costs are likely to be over the next year. Fewer than 4% 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, 82% have worked before, but only 24% 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

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[1] Examples include Field et al. (2010) in India, Karlan and Valdivia (2011) and Valdivia (2011) in Peru, Drexler et al. (in press) in the Dominican Republic, Calderon et al. (2012) in Mexico, Berge et al. (2011) in Tanzania, Bruhn and Zia (2013) in Bosnia-Herzegovina, and Gine and Mansuri (2011) in Pakistan; and are discussed in detail in McKenzie and Woodruff (in press).

[2] Short-term results in the new work by Martinez et al. (2013) in Chile also suggest positive impacts for a combination of training and a grant on new business start-up.

[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 (D.S.) Divisions (324), and Grama Niladari (G.N.) Divisions (14,008).
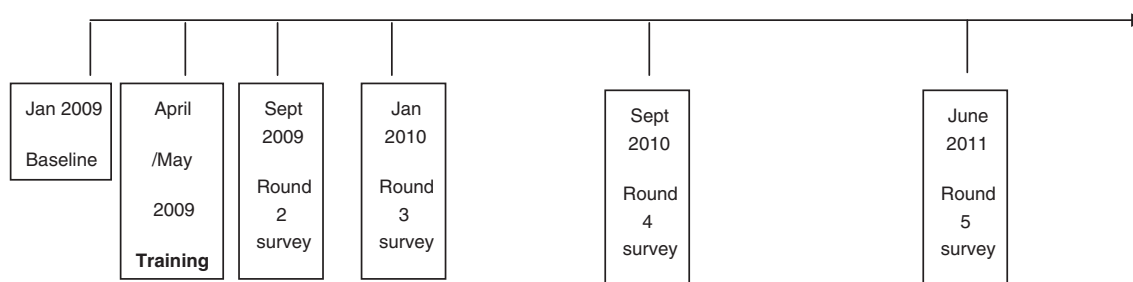
Fig. 1. Timeline.

potential business owners is about 1100 Rs less per month than the households of current business owners.

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 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. At the time of offering the business training, individuals were told only that half of those who completed the training would be randomly chosen to receive a 15,000 Rs grant. Those who completed training and had been allocated to the training plus grant treatment were then invited to a meeting at the training center where they were given a check for the 15,000 Rs. The grant was framed as compensation based on a random drawing for completing the program, and recipients were told that the funds could be used for any purpose.

For the current owners, we dropped four enterprises with either missing profits or profits above 10,000 Rs. Randomization to treatment was then stratified by D.S. (10) and by whether or not there were children under age 18 without another person to look after them while the mother worked (54%). Within each stratum we 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.[5]

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 ever worked before, even if only as an unpaid family worker (18% had not). 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

We use the International Labour Organization (ILO's) Start and Improve Your Business (SIYB) training program. SIYB is designed to meet the needs of small-scale entrepreneurs in developing countries, with materials customized to local languages and context of each country. Started in Eastern Africa in 1977 (ILO, 2003), the program has now reached more than 4.5 million people in 95 countries.[6] 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.
- 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 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.

We contracted the Sri Lanka Business Development Centre (SLBDC), a Sri Lankan non-profit training institution, to provide the business training. SLBDC introduced the SIYB program to the Sri Lankan market in 2001. All of the SLBDC training staff involved in the project were university qualified and trained under the national-level SIYB training programs conducted by the ILO. 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.

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 7 h 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 they received seven days of training, with 7 h 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 training cost us $126–$131 per current business owner and $133–$140 per potential business owner. The training was offered to participants for free, mimicking the approach used by many NGOs and microfinance institutions, who also often offer a transportation and

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[5] 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.

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

Table 1
Baseline characteristics of the sample by treatment group.

	Current enterprises				Potential enterprises			
	Control	Training only	Training + cash	Test of equality p-value	Control	Training only	Training + cash	Test of equality p-value
Variables stratified on								
Total monthly profits (Rs)	3987	3981	4001	0.995				
Have no children or have someone to look after children	0.55	0.54	0.55	0.983				
Colombo district	0.20	0.20	0.20	1.000	0.19	0.20	0.21	0.952
Kandy district	0.21	0.20	0.20	0.987	0.20	0.20	0.20	0.999
Has taken concrete steps to opening business					0.51	0.50	0.51	0.976
Has never worked before					0.18	0.17	0.19	0.859
Variables not stratified on								
Age	35.94	37.71	36.58	0.010	34.38	34.05	33.72	0.571
Married	0.89	0.86	0.80	0.030	0.84	0.91	0.89	0.140
Number of children under 18	1.55	1.47	1.40	0.287	1.40	1.47	1.59	0.164
Years of education	10.16	10.34	10.51	0.393	10.51	10.56	10.53	0.985
Risk-seeking score (0 to 10, 10 is highest risk-seeking)	6.81	6.87	6.53	0.172	6.73	6.82	6.75	0.880
Digitspan recall	6.00	6.04	6.01	0.955	6.03	5.93	6.06	0.542
Raven test score (out of 12)	2.58	2.75	2.68	0.652	2.76	2.59	2.81	0.465
Total household income from all sources	17,192	18,245	17,595	0.486	16,422	16,690	16,393	0.956
Wealth index (principal component)	0.01	0.05	0.28	0.211	−0.09	−0.12	−0.11	0.99
Household has a fridge	0.45	0.53	0.51	0.267	0.39	0.41	0.43	0.765
Household has a sewing machine	0.56	0.60	0.60	0.695	0.51	0.54	0.55	0.739
Household has an oven	0.08	0.08	0.12	0.381	0.09	0.05	0.08	0.196
Household has a gas cooker	0.25	0.23	0.30	0.257	0.28	0.24	0.24	0.612
Age of firm (years)	6.47	6.88	6.35	0.731				
Ever had a loan from financial institution	0.23	0.18	0.20	0.396				
Total monthly sales (Rs)	12,523	12,485	12,640	0.990				
Capital stock excluding land and buildings (Rs)	28,649	27,418	35,187	0.227				
Truncated capital stock (Rs)	28,649	27,418	34,997	0.234				
Business practices score (max. of 29)	4.59	4.99	4.98	0.464				
Number of firms	224	200	200		228	200	200	

food per diem allowance and/or compensation for the opportunity cost of attending (Mckenzie and Woodruff, in press). We included a 400 Rs per day attendance payment to cover basic transport cost (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, and also told them that half of those completing training would be randomly chosen for a 15,000 Rs grant.[7]

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 owners (70.5%) offered training attended at least one training session, and 261 (65.3%) received completion certificates. Of 400 current business owners offered training, 279 (69.8%) attended at least one session and 268 (67.0%) received completion certificates.[8] Those who attended expressed satisfaction with the course in qualitative interviews and direct survey questions: 78% (81) of current (potential) business owners said they would strongly recommend the course to others, and 86% said that the course was 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 months, and 24–25 months after the training. We refer to these as the round 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 the case of refusals or inability to conduct an interview in the fifth round, proxy reports and direct observation were also used to assess whether or not the owner was running a business, with information on this outcome available for 97% 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% in 2010 and 8.3% in 2011, the highest growth level since Sri Lanka's independence.[9] Growth was widespread through manufacturing and

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[7] As a result, our LATE estimates are the impact of training for those who would take it up when offered these incentives to attend. As noted, many organizations offer per diems/ transport costs. A number of government programs also offer the possibility of financing, including grants (e.g. Martinez et al. 2013).

[8] The working paper version of the paper examines the correlates of training take-up. Among existing firms, take-up is lower for those with higher opportunity costs of time (those earning higher profits, working more hours, or in Colombo), higher for manufacturing and retail firms than services firms, and not related to baseline business practices or elicited willingness to pay for such training. Among potential firms, take-up is lowest in Colombo, increases with age and the score on a Raven test of non-verbal ability, and is again unrelated to expressed willingness to pay for training.

[9] http://www.statistics.gov.lk/national_accounts/PressRelease/ PRESSNOTE2011AnnualEnglish.pdf [accessed 23 May, 2012].

Table 2
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
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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	1563	1563	1563	1563	1563
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	0.09	0.01	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 were used.

Total business practices score is a score of business practices used, with a maximum of 29, defined in Appendix 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.

   * p < 0.1.
  ** p < 0.05.
*** p < 0.01.

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. 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 model using the follow-up surveys:

$$Y_{i,t} = \alpha + \gamma_1 \text{ assigned training only}_i + \gamma_2 \text{ assigned training\&cash}_i + \theta Y_{i,0} + \pi_{i,s} + \varepsilon_{i,t} \quad (1)$$

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.[10] This Ancova specification includes the lagged outcome variable to increase power (McKenzie, 2012) and is possible only for the current enterprises; the potential owners did not have baseline business outcomes to include in Eq. (1). We estimate Eq. (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 by pooling together several rounds of follow-up surveys. We estimate the following regression:

$$Y_{i,t} = \sum_{t=1}^{q} \delta_t + \gamma_1 \text{ assigned training only}_i + \gamma_2 \text{ assigned training\&cash}_i + \theta Y_{i,0} + \pi_{i,s} + \varepsilon_{i,t} \quad (2)$$

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 Eqs. (1) and (2) gives 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 (which seems plausible for

those who attend no sessions), we can also estimate the treatment-on-the-treated (TOT) effect — the effect of training for those who take part in at least one session. 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.

4.1. Impact on the business practices of women who were already in business

In Table 2 we examine whether the training led to any improvements in business practices. The first three columns show that management practices improved in both the short-term (3–4 months post training), and medium-term (15–16 months after training and 24–25 months after training). The magnitude of the increase is 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% 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 that 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 five through eight pool together the different rounds and show that the training programs significantly improved each sub-component of the overall business practice score, with improvements in marketing, stock control, financial planning, and record-keeping.

4.2. 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 affect which firms survived: treatment status is unrelated to observable characteristics among surviving firms. Given these findings, we proceed to analyze business outcomes for the group of surviving firms.

Table 3 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

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[10] This specification assumes that there are no spillovers (such as competing for the same customers) among treated and control groups. 63% of current enterprises have no other firms in the same sector receiving treatment in their G.N. Controlling for the number of firms treated in the same sector within the G.N. does not affect our results, suggesting that spillover effects are not driving our results.

Table 3
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
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Monthly profits							
ITT effects							
Assigned to cash if finish training	724.9	1207**	0.168**	1758*	1910**	432.5	169.9
	(839.9)	(593.0)	(0.0716)	(932.6)	(898.5)	(1123)	(1099)
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)	(1148)	(1241)
TOT effects							
Received training & cash	1079	1786**	0.248**	2526**	2819***	642.0	249.6
	(1169)	(827.5)	(0.100)	(1032)	(1023)	(1233)	(1220)
Received training only	−912.9	−217.3	0.0333	6.206	−86.50	−605.9	−1042
	(1152)	(782.4)	(0.0949)	(882.4)	(924.0)	(1141)	(1276)
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	5171	4436	0.143	6818*	3284	3079	2129
	(4686)	(3500)	(0.0932)	(4020)	(5366)	(6534)	(6482)
Assigned to training only	−2941	−1786	−0.0414	−1718	−1519	−3884	−2248
	(4422)	(3512)	(0.0967)	(3845)	(5386)	(5993)	(7177)
Baseline mean:	12,659	12,659	9.09	12,198	12,520	12,600	12,548
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**	9535*	7270	12,195*	11,374**
	(7815)	(3583)	(0.0691)	(4893)	(4932)	(6379)	(5760)
Assigned to training only	−700.2	−490.7	−0.0671	−3476	−278.1	−4452	3389
	(5616)	(3338)	(0.0629)	(4192)	(4596)	(5921)	(6474)
Baseline mean:	31,272	31,272	9.48	30,137	30,359	30,538	30,350
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	2097	2097	2019	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 were used.
All specifications also include survey round dummies, baseline outcome value, and controls for randomization strata.
Truncated levels truncate at the 99th percentile.
   * $p < 0.1$.
  ** $p < 0.05$.
 *** $p < 0.01$.

(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, Fig. 2A and B shows 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. 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, an effect which holds 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;

A) CDF of Monthly Profits of Current Enterprises at First follow-up Survey
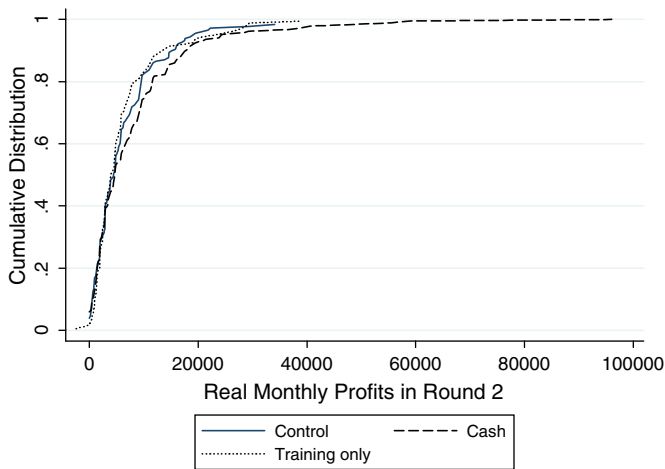
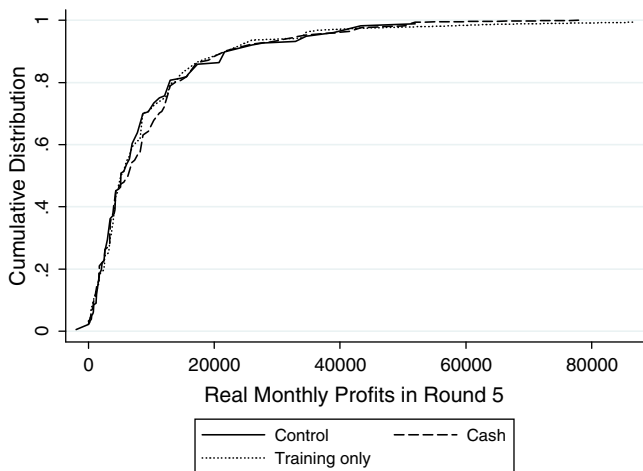## B) CDF of Monthly Profits of Current Enterprises at Last follow-up Survey

Fig. 2. A: CDF of monthly profits of current enterprises at first follow-up survey. B: CDF of monthly profits of current enterprises at last follow-up survey.

the point estimate on profits is much smaller by rounds 4 and 5.[11] This is seen also in Fig. 2B, where the CDF of profits in round 5 is 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 has immediate effects which appear to dissipate a year later. Our prior research with a comparable sample of existing female microenterprises in Sri Lanka found zero average return to capital alone (de Mel et al., 2009a, 2012). The combination of the findings that training alone has no effect, that grants alone have no effect in the comparable sample, and that training and grants combined do have effects immediately after the grants are provided suggests that there is a complementarity between training and grants in the short run. 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.[12]

4.3. Impact on starting a business among potential business owners

We now turn to the potential business owner group. Table 4 examines whether the treatments affect the likelihood that an individual enters self-employment or engages in wage work. We see that 38% of the control group operates a business at the time of the round 2 survey, increasing to 56% by the round 4 survey and 57% 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, 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 effect on 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 of Table 4 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. However, Appendix 4 shows that although the long-term rate of business ownership is unchanged, the treatments change the selection of who operates a business, with training alone leading to women who are, on average, less analytically skilled (as measured by a Raven test) operating businesses, and the combination of training and a grant leading to less analytically skilled and poorer women operating businesses.

4.4. 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 a business owner) and wages (if a wage worker) combined. We define this income as zero for women who are not working. 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 medium-term effect. We pool rounds to enable us to display all the key results on

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[11] 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 of 0.16 (p = 0.0998 for log profits), so we can at best marginally reject no decline in profits.

[12] We did not have a grant-only treatment in this project because of the prior work and a desire to maximize power for the treatments involving training. Moreover, if we wanted to be able to compare the impact of training and cash to that of training alone in exactly comparable groups, we would need to provide cash grants only to a subsample of women who would have completed training if offered the training under terms comparable to the training treatment groups. Since ex ante it was not possible to tell who would complete training, this precluded the inclusion of a treatment group receiving only cash conditional on being the sort of person who would complete training if offered it.

Table 4
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
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
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.
  * $p < 0.1$.
 ** $p < 0.05$.
*** $p < 0.01$.

Table 5
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	6043	7179	4215	0.999**
	(556.5)	(728.5)	(741.7)	(830.2)	(3059)	(3841)	(7324)	(7892)	(0.489)
Assigned to training only	211.5	1494*	484.9	2244**	397.5	6248*	−2293	−261	0.870
	(545.4)	(773.9)	(785.3)	(975.9)	(2989)	(3638)	(6885)	(7821)	(0.559)
Observations	1175	1119	615	675	616	675	615	718	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	0.566	0.82
Control group mean	3516	4940	5001	5209	14,739	15,292	25,489	34,033	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	5840	6820	6316		1.173**
		(692.6)	(846.0)	(3060)	(3698)	(7467)	(8.735)		(0.502)
Assigned to training only		374.3	2171**	−349.7	5950	−1664	2298		0.971*
		(772.0)	(1072)	(2979)	(3749)	(7336)	(7975)		(0.567)
Observations		590	651	591	651	590	693		652
Firms		345	380	345	380	344	372		381
p-Value for testing treatment equality		0.670	0.213	0.773	0.978	0.277	0.63		0.728

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.
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.
 * $p < 0.1$.
** $p < 0.05$.

outcomes for potential owners in the same table — results are similar when we look round by round.

Column 1 of Table 5 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% 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% level), and 697 Rs (not significant) for the training plus cash group. We also cannot reject 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% 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,[13] the

[13] This difference is not statistically significant, so it may just reflect chance. Alternatively, it might reflect the fact that the training plus grant led to the new businesses being started by ex ante poorer individuals on average than is the case with the training only treatment.

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% increase in profits and a 40% 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 both treatments led to women with lower Raven score starting businesses and the cash plus training treatment led to women with lower wealth starting businesses (see Appendix 4 for details). 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 are our results 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 predict these probabilities as a function of wealth, ability, risk attitudes, interest in attending training, interest in running a business, and personal characteristics — age, marital status and education. We use the predicted probabilities to form a generalized propensity score (Imbens, 2000), and re-run our treatment regressions, reweighting by the inverse of this generalized propensity score (GPS) and restricting the sample so that the GPS has a common range across the three treatments.[14] Since the initial sample was randomized and the selection was not extreme, trimming dissimilar observations removes only a few observations.

Panel B of Table 5 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.

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 business practices taught, but the training does not have measurable impacts on business profitability or employment levels. One response to this has been that these existing studies have been unable to detect impacts because they work with a selected subset of microfinance clients and have low statistical power. We conduct an experiment on a sample of women which is both broadly representative of low-income female microenterprise owners and more homogeneous than other studies. The homogeneity of the sample yields considerably more statistical power than other experiments. We conclude that training alone is not enough to generate growth in subsistence businesses run by women. Adding capital leads to a boost in profitability in the first year following training, but the effect is temporary; these enterprises are no more profitable than the control group two years after training. These results highlight the challenge in generating growth in subsistence-level female-owned microenterprises, and suggest that the binding constraints on 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% in female microenterprises in Peru. 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 with regard to the effect of business training for women who are out of the labor force. We find that training helps these women to start enterprises more quickly, and to improve the management and profitability of the businesses they start. The improvement in profitability comes in spite of the fact that training appears to induce more entry by women with lower measured cognitive skills, and more exit by women with higher cognitive skills. The impact on business start-ups is consistent with results from recent randomized experiments with microfinance (Attanasio et al., 2011; Banerjee 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, and with short-term results from Martinez et al. (2013). Taken together, these results suggest getting women to start 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.

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 *records 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,

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

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 to 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

    o Zero points for "never"
    o One point for "once a year or less"
    o Two points for "two or three times a year"
    o 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.

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 show 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 to realize that 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 that 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?

Appendix 4. Does business training change the selection of which potential owners operate businesses?

Table 4 shows that by round 5, there was no significant impact of either treatment on the overall rate of business ownership. In Appendix Table 3 we test whether training changed the skill and wealth composition of those operating businesses. We allow for heterogeneity in treatment effects by two measures of skill – Raven score and digitspan recall – and by our baseline household wealth index. 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 — that is, training increases the self-employment rate of lower skilled individuals relative to higher skilled individuals.

Appendix Table 1

	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*** (0.0455)	0.244*** (0.0456)	0.0953** (0.0469)	0.192*** (0.0472)	−0.0419 (0.0477)	0.111** (0.0483)	−0.00521 (0.0475)	0.0586 (0.0478)
Assigned to training only	0.0540 (0.0461)	0.119*** (0.0457)	0.0163 (0.0480)	0.106** (0.0478)	−0.0408 (0.0482)	0.108** (0.0477)	−0.0495 (0.0481)	0.00799 (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. **$p < 0.05$, ***$p < 0.01$.
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 Table 2

	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	1892
	(809.0)	(797.5)	(1371)
Assigned to training only	−543.9	−769.2	1432
	(868.8)	(872.4)	(1409)
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	2244**	2020**	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 5 and 1% levels respectively. Absolute difference between profits and revenue minus expenses truncated at the 99th percentile.

Columns 5 and 6 of Appendix Table 3 then 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% 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 appendix Table 3 show that higher ability treated women are no more likely to be wage workers, but are more likely to be out of the labor force. This is particularly true among those eligible for both training and cash. Panel B of the table 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

Appendix Table 3

	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
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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, and show ITT impacts.
Robust standard errors in parentheses. *p < 0.1, **p < 0.05, ***p < 0.01.
All specifications include randomization strata dummies and level effect of interacting variable.

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 Appendix Table 3 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. The cash and training treatment appears to have induced poorer individuals to start a business, 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% level (the interaction with the cash treatment has p-value 0.138 for being out of the labor force).

Appendix Table 3: Do the grants affect the selection of who owns a business?

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