BUSINESS LITERACY AND DEVELOPMENT: EVIDENCE FROM A RANDOMIZED CONTROLLED TRIAL IN RURAL MEXICO*

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Abstract

A large share of women in developing countries run small enterprises often earning low incomes. This paper explores whether these poor performances are due to a lack of basic business skills. We randomly offered female entrepreneurs in rural Mexico 48 hours of business skills training at no cost. We find that those assigned to treatment earn higher profits, have larger revenues, serve a greater number of clients, and are more likely to use formal accounting techniques. Indirect treatment effects on those entrepreneurs randomized out of the program, yet living in treatment villages, are economically meaningful, yet imprecisely measured. We present a simple model of experience and learning that helps interpret our results, and consistent with the theoretical predictions, we find that "low-quality" entrepreneurs are the most likely to quit their business post-treatment, and that the positive impacts of the treatment are increasing in the quality of the entrepreneurs.

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1. Introduction

Self-employed, non-agricultural workers make up about 45 percent of the labor force in lower income countries, and private sector led growth is often stressed as an engine of creating jobs and spurring growth (World Development Report 2013). A persistent puzzle, however, is the observation that micro-entrepreneurs, females in particular, in developing countries are not efficiently running their businesses; for example, through the misallocation of capital and labor in the firm (see Mckenzie and Woodruff (2012) for a review of this literature). Given the importance of entrepreneurship in the development process, especially amongst women, it is of utmost importance to understand both how business decisions are made and if poor decisions are caused by a lack of business literacy and managerial knowledge.

In response to this perceived underperformance of poor female entrepreneurs, a considerable number of NGOs around the world provide business training. However, there is yet little evidence that this type of intervention is needed or effective. Among economists, there is an increasing interest in understanding the links between the variation in firm profits and financial and managerial practices in developing countries (see de Mel, Mckenzie and Woodruff (2009*a*); Karlan and Valdivia (2011); and Bloom et al. (2013)). On the other hand, more research is required to understand the way poor entrepreneurs make their investment decisions (de Mel, McKenzie and Woodruff, 2008).

In this paper we analyze the effects of providing business training to small and micro female entrepreneurs in rural Mexico through a Randomized Controlled Trial. There are three distinctive characteristics of our intervention. First, the pedagogy focuses on the practical application of the skills and topics in the entrepreneurs' own businesses. Second, the training is intensive, with a total of 48 hours of classes over 6 weeks. Compared with other training programs, the course is relatively long and intensive; for example many programs associated with microfinance organizations last only 30 minutes, added on to weekly or monthly borrower meetings (see Mckenzie and Woodruff (2012) for an extensive review of the literature). Third, the entrepreneurs in our sample do not receive any other treatment, for example, none are involved in micro-finance or other targeted business interventions.[1] This feature is important because it allows us to isolate the independent effect of business training, something that is not possible with much of the existing literature (e.g., Field, Jayachandran and Pande (2010), Karlan and Valdivia (2011), Drexler, Fischer and Schoar (2011)). Indeed, de Mel,

[1] In fact, only 4.5 percent of our sample had received a loan from a microfinance institution or the government in the previous 12 months.

Mckenzie and Woodruff (2012) find substantial complementarities between business training and the availability of credit amongst female entrepreneurs in Sri Lanka.

We designed and implemented a two-stage randomized controlled trial that allows us to uncover both the causal effect of the business training classes on participants and spillover and general equilibrium effects on non-participants in program villages (Miguel and Kremer (2004) and Angelucci and De Giorgi (2009)). In the small, partially closed markets (villages) that we study, it is possible that the treatment could generate within-market knowledge spillovers or (local) general equilibrium effects as treated entrepreneurs interact with those randomized out of the program. It is unclear whether these indirect treatment effects should be positive or negative. For example, treated subjects may implement better business practices and capture market share at the expense of non-treated entrepreneurs. Or, treated subjects may share their knowledge with non-treated subjects, intentionally through conversation or unintentionally if the new business practices are observable (such as new menus, changes to the product mix, or changes in prices).

The experiment was conducted in the poor, rural Mexican state of Zacatecas. Our sample includes 17 communities and about 900 entrepreneurs. We completed a pre-intervention survey in the summer of 2009. Then the business training classes began in late October 2009 for the eligible participants. The entrepreneurs included in the study are engaged in many different enterprises, such as making and selling food, making craft items, or small re-sale shops. We then re-surveyed the sample in the summer of 2010 and the spring of 2012, this allows us to look at short and medium run outcomes as well as increasing the statistical power of our testing strategy.

Specifically, our research will answer two questions: (i) Is the policy intervention of classroom training effective in improving business outcomes? (ii) Can we shed light on the possible mechanisms?

We find that the intervention raises profits, revenues, and the number of clients served for those women who were invited to the treatment. Importantly the use of formal accounting techniques increases, as well as the likelihood of registering with the relevant agencies, as a result of the intervention. We also find that the effects are present both in the short and the medium-run, that is, after 7 to 8 months and after about 2.5 years. Furthermore, we find that treated firms are able to reduce their costs and change the mix of products sold with an increase in the number (and plausibly variety) of items sold, while dropping higher cost, lower price goods and adding to their portfolio lower costs higher price goods. At the same time, and consistently with our conceptual framework and Karlan, Knight and Udry (2012), we find that low performing entrepreneurs (measured pre-treatment) quit their businesses as a result of the training, while the largest positive effects are recorded amongst the "best" entrepreneurs.

In terms of profits we detect negative, although not statistically significant, spillover effects to those businesses who were not invited to the treatment but operate in treatment villages. Such result seems to arise from an increase in costs rather than a fall in revenues. This last result, together with the cost reduction for the treated group, suggests that the control and treated women purchase their inputs from different suppliers (more costly for the control group in treatment villages) or that suppliers have latitude to set differentiated prices.

Our paper contributes to the growing literature on the effects of business literacy training on firms profitability. For example, empirical evidence is presented by Field, Jayachandran and Pande (2010) in India, Karlan and Valdivia (2011) and Valdivia (2011) in Perú, Drexler, Fischer and Schoar (2011) in the Dominican Republic, Berge, Bjorvatn and Tungodden (2011) in Tanzania, Bruhn and Zia (2011) in Bosnia-Herzegovina, and Giné and Mansuri (2011) in Pakistan, and Fairlie, Karlan and Zinman (2012) in the United States. Nyshadham (2013) provides theoretical arguments on the effects of business literacy training on entrepreneurial decision making. There is at the same time a growing literature on the effects of management services in developing countries (Bloom et al. (2013); Bruhn, Karlan and Schoar (2012); Karlan, Knight and Udry (2012)).

The remainder of the paper is as follows: Section 2 describes the business literacy training and our experimental design; Section 3 describes the data and the sample; 4 presents the empirical methodology and discusses the main effects of the intervention; Section 5 develops and empirically tests a conceptual framework to help interpret the main findings; and Section 6 concludes.

2. Description of the Business Literacy Training and Experiment

2.1 The business literacy classes

In 2009, we partnered with the NGO *CREA* to develop and implement a business literacy training program for small, female headed firms in the retail or production sector. CREA operates in small villages in the Mexican state of Zacatecas, a high-altitude, dry, and agricultural region. While there is good road access to all villages CREA operates in, the inhabitants are none-the-less isolated in most of their daily activities, as villages are geographically isolated, separated by farms and arid land.

The training program consists of two four-hour classroom meetings per week and runs for six weeks, for a total classroom time of 48 hours. The classes were designed to be small and inclusive, with two instructors and a class size of no more than 25 students; all instructors are experienced local university professors, graduate and undergraduates students. Furthermore, the program is free to invitees. In fact, CREA offers participants several incentives to further encourage participation, including: a completion certificate from CREA, the Institute for Women of Zacatecas (a government agency), and the Autonomous University of Zacatecas (the local university); in-class raffles for small prizes (e.g., a CREA hat or stationary supplies) each week conditional on attendance and homework completion; and the promise of acceptance in future CREA courses conditional on regular attendance to the current one.

The business literacy course covers six main topics, each taught in separate weekly modules. The first consists of understanding costs (e.g., the difference between unit, marginal, fixed, and total costs) and how they should be measured. The second covers how to optimally set prices. In this module, emphasis is placed on the concepts of profit maximization and pricing to reflect marginal costs, rather than average or fixed costs, as well as the concepts of demand and competition. The third module reviews the basic legal rights and obligations of small business owners. Since the vast majority of participants own informal businesses, this module includes a discussion of the costs and (potential) benefits of formally registering a business with the government. The fourth module covers general business organization and the choice of products to produce or sell. The fifth covers marketing, including concepts related to knowing and responding to competition. The final module is a discussion of how to be an effective salesperson.

The content and teaching style of CREA's course is intentionally simplified in order to be understandable to the population at hand, the majority of whom have low levels of formal educational. As such, classes emphasize practical examples and encourage students to relate the concepts to their own businesses. For each module, students received a 30 page "textbook" which discusses (1) the importance of the concept at hand, (2) the definition of the concept, (3) examples of how to compute or use the concept (e.g., how to do basic business accounting or compute unitary costs), (4) in-class exercises, and (5) exercises for homework. In-class instruction follows this structure, first introducing the main concepts, then applying those concepts to simple examples that are relevant to the participants' own businesses.[2]

Possible effects of the classes To fix ideas, we briefly describe the potential effects of this intervention and how they motivated our experimental design, saving a formal derivation of a model of entrepreneurial learning and choice for Section 5 below. Attendance at CREA's business literacy classes should inform women about how to properly run a small business. Importantly, this information may in fact make some entrepreneurs realize that their current business is unprofitable or that running her business is not an optimal choice. For example, a woman selling ready-to-eat food learns that she should separate her business and household accounts, and doing so discovers that in fact she is losing money. Or, upon learning the principle that an enterprise should include as a cost the opportunity cost of one's time, an entrepreneur may find that her time is better spent in other endeavors.

Given that business literacy classes may affect both how an entrepreneur runs her business and its likelihood of existing, it is ambiguous what the average effect of the classes will be on observable business-related measures, such as profits, revenues, or the number of clients served. As such, our working hypotheses are that the business classes might make some businesses more efficient through better accounting and management skills, leading to a positive effect on business-related outcomes, while at the same time some entrepreneurs might not have the skills to successfully implement the new technologies and procedures, leading to a negative effect.

Furthermore, in small, rural economies like the ones we study, it is likely that novel business practices will be shared, either intentionally amongst individuals in a social network or unintentionally through observable actions taken by business owners (e.g., posting prices or using advertising). To the extent that the economy is somewhat closed, any intervention that affects some businesses will have general equilibrium and spillover effects market wide, and thus impact the non-treated enterprises in a treated market. The experimental design we describe next was designed to capture such effects.

2.2 Experimental design and population of study

Our experimental design contains two-stages, the first assigning villages to either treatment or control, and the second assigning entrepreneurs within treatment villages to receive an invitation to attend the classes. This design allows us to estimate the direct effect of the program, by comparing invitees in treatment villages to entrepreneurs in control villages, as well as the indirect effects of the program, by comparing those not invited to attend classes in treatment village to entrepreneurs in

[2] An in-class example and exercise can be seen in Apendix Figure 1.

control villages.

Working with CREA, we selected a sample of entrepreneurs by first choosing villages, and then conducting a census of the female entrepreneurs in those villages who produced or sold goods. Our original sample frame included all villages in the state of Zacatecas that met three criteria: that they (i) had between 100 and 500 female entrepreneurs who sold goods or provided services, as identified by the 2005 Mexican census, (ii) are within a two hour drive from the City of Zacatecas, and (iii) had less than 1500 households (also identified by the 2005 Mexican census).[3] This selection process identified 25 villages and, in order to accommodate our survey budget as well as CREAs institutional capacity, we randomly drew a sample of 17 villages from this set of 25 to be included in the study.

Within chosen villages, we identified female entrepreneurs that produced and/or sold goods as follows: First, we contacted the elected village leader (the *diputado* or *comisario*, a mayor-like position) and asked him/her to introduce us to at least three knowledgable local women. Second, we asked this group to list all of the women in the village that (i) work for themselves and (ii) sell a good. This process yielded about 50 female entrepreneurs per village, to whom we applied a pre-intervention questionnaire between July and September of 2009.[4] Unfortunately, we did not have the resources to survey male entrepreneurs, which limits our ability to estimate the full indirect effects of treatment (spillover and general equilibrium effects). However, our experience in these villages is that the majority of the goods that are sold by women are not also sold by men. Importantly, none of the entrepreneurs we surveyed report selling their goods outside of their own village, suggesting it is unlikely that there are program spillovers across villages.

In order to assign subjects to treatment, we used information on business activity and demographics from the pre-treatment survey to perform the random assignment at both the village and intra-village levels.[5] In early October 2009, eligible entrepreneurs were contacted in person by a CREA staff member informing them of their selection into the program; classes began in late October and ran through December 2009, with attendance being recorded by the teachers.

[3]The second criterion was necessary to ensure that the CREA instructors who lived in Zacatecas City would be able to reach participating villages.

[4]The remaining female entrepreneurs identified by the 2005 Mexican census were either in the service sector or were farmers who did not retail their produce.

[5]Our randomization algorithm involved first choosing a "seed" group of seven villages and then choosing 50 percent of the sample in each treatment village to be offered the program. We repeated this assignment 10,000 times so as to minimize the (squared) sum of the distances of predicted last day's profits between treated and control units.

3. Data and Sample

3.1 Data

Our data includes an array of indicators of business performance, entrepreneurial ability, and socio-economic characteristics. In addition to the pre-intervention survey, two waves of data were collected post-intervention, approximately 18 months apart (the first between July and September 2010 and the second between March and May of 2012). These multiple post-intervention waves allow us to both analyze longer run impacts and increase the statistical power to detect significant program effects. All interviews were conducted by local enumerators with the stated purpose of studying female-run micro enterprises; intentionally, no connection was established with CREA or the intervention.

Our main measures of business performance include self-reports of profits, revenues, and the number of clients served, all from the last day the entrepreneur worked. Many women do not work the full week or regular hours; as such, they might be better able to remember daily figures rather than compute figures from a longer time horizon.

While evidence from other developing countries suggests that self-reported measures of aggregate business activity are as accurate as formal accounting figures (de Mel, Mckenzie and Woodruff, 2009*b*), we nonetheless also collected data on the individual goods sold in the enterprise. We first asked the entrepreneur to list all of the goods that she sold (up to a maximum of 14).[6] We then asked for each good the number of units sold on the last day worked, the unit price, and the unit cost. Unfortunately, this data is missing for the second post-intervention survey round due to a mistake in that questionnaire.

As the goods reported on in each survey round represent the contemporaneous stock of goods for sale, this data is an unbalanced panel at the good level. As such, it contains three types of goods: new goods for sale, old goods that were no longer sold, and goods that were sold both pre- and post-intervention. From this data, we first calculate aggregate measures of the stock of goods an entrepreneur sold, including total revenue, total profit, the total number of goods sold, and the mean-across-all-goods of both unit cost and price. These aggregate measures are useful because they capture optimizing decisions in terms of product stock, which could have been affected by the intervention. For example, a woman may learn that one product is losing money and drop that product; she may

[6] Approximately six percent of the sample reported selling 14 goods; thus six percent of the sample could have had more than 14 different goods for sale, information which we do not capture.

also decide to sell a new product with a larger profit margin.

We also use the good-specific data to examine how the product mix changes over time in response to the business training. Specifically, we examine treatment effects on total revenues, total profit, mean unit cost, and mean price amongst (i) the goods that the entrepreneur decided to stop selling (dropped goods), (ii) the goods that she continued to sell over both rounds (kept goods), and (iii) the goods she decided to start selling in the first post-intervention round (added goods).

Several other outcomes will give us further insight into how the intervention affects the performance of the business, including: the number of employees (both paid and unpaid), the number of co-owners, and the average number of hours worked per week by the owner. In order to directly examine the effect of the training classes on our subjects' business-math knowledge, we administered a simple exercise related to production and sales.[7] This same question was applied both pre- and post-treatment. We score each of the four sections as either correct or incorrect, summing to create a total score for the exercise. Furthermore, we asked the entrepreneurs how they kept accounts for their business, whether through personal notes or a formal accounting method, or whether they did not keep any accounts.

Finally, to capture important heterogeneity in our sample pre-treatment, we collected data on the owner's age, education, asset ownership (e.g. type of dwellings and number of rooms), risk aversion, reservation wages, credit availability and the cost of credit, the type of activity the woman is engaged in, the age of the business in months, and the size of business investments.

3.2 Sample and summary statistics

Our working sample includes 17 villages - seven treatment and ten pure control - and a total of 875 entrepreneurs: 164 eligible for and offered the treatment, 189 controls in treatment villages, and 522 in pure control villages. Figure 1 contains the distribution of the types of goods a firm sold, pre-intervention. The majority of firms (about 65 percent) were involved in the sale of food, either prepared (e.g., cheese, bread) or ready-to-eat (e.g., tacos, hamburgers, gorditas); general grocery store owners and other re-sale comprise a little over 25 percent of the sample; and handicrafts and clothing sum-up to about 10 percent.

Table 1 contains mean pre-intervention characteristics by treatment group, along with p-values from F-tests of their equality, and suggests that the randomization was successful in that the pre-

[7]This exercise can be seen in Appendix Figure 2.

intervention characteristics are for the most part indistinguishable across groups. For one variable, there is a significant difference across groups at the 5 percent level: more businesses were registered with a government agency in the control group than the treatment group.

This data paints a sobering picture of the economic lives of these entrepreneurs. Daily profits average around 140 pesos (approximately $11 USD), with a large variation (the standard error of the mean is 16 pesos).[8] Revenues are about four times the size of profits, and it is interesting to note this is the same order of magnitude as found amongst firms in Sri Lanka by de Mel, Mckenzie and Woodruff (2009*b*).

Business owners are on average 46 years old and have about six years of education. Approximately one third have a temporary roof on their residence (e.g., thatch or cardboard), an indirect measure of permanent income. Owners work for about 40 hours per week on average, and the total value of the capital stock (the replacement value of business capital) is about $570. Interestingly, the entrepreneurs in our sample seem to have access to credit that would allow them to replace the business capital at a high (albeit common for this type of population) six percent monthly interest rate. Businesses are small: on average there are 1.6 workers including the owner, and employees work only about one quarter of the hours the owner works (about 10 hours per week). About 60 percent of businesses have no workers other than the owner. The average age of a firm is about seven years, again with large variation.

Importantly, the women in our sample know how to make basic calculations, but are less proficient at determining profits or optimally setting prices. For example, 93 percent said that they know how to make simple math calculations (not shown in the table), while the average score on the math exercise was 39 percent, or less than two out of the four questions answered correctly.[9] Furthermore, less than five percent of entrepreneurs (one percent in the treatment group and four percent in the control) keep formal business accounts.

3.3 Take-up of classes

Classes were offered to the randomly selected invitees by a CREA staff member in an in-person visit to the entrepreneur's home or business. Importantly, CREA made the intentional decision to not pre-screen invitees on the basis of the stated desire to accept the classes. As such, amongst the 164

[8] The dollar peso exchange rate in 2008-2009 was approximately 13 Mexican pesos to 1 U.S. dollar.

[9] Analyzing the questions of the math exercise separately, less than 50 percent could calculate profits correctly and only 18 percent could calculate the optimal price to set.

entrepreneurs who were offered the classes, about 35 percent (57 entrepreneurs) did not attend any classes. Amongst those who did attend at least one class, an average of six classes were attended out of the 12 offered. Take-up and attendance rates are similar in magnitude to other business literacy interventions in the literature (Mckenzie and Woodruff, 2012).

Appendix Table 1 compares the mean pre-intervention characteristics of entrepreneurs who attended classes and those who did not, and shows that only one variable is significantly different across groups at the five percent level. However, despite this lack of significant difference (partly driven by the small sample size), it does appear that attendees are less successful entrepreneurs with respect to non-attendees. For example, daily profits and revenues are about 50 percent higher for entrepreneurs who did not attend compared to those who attended. Again, such findings are consistent with the literature (see, for example, Drexler, Fischer and Schoar (2011) and de Mel, Mckenzie and Woodruff (2012)).

In order to investigate the effect of treatment (being offered the class) on the treated (class attendees), we can instrument attendance status (which is presumably endogenous) with treatment status (which is exogenous due to randomization). However, for parsimony and a cleaner interpretation of the intervention, we instead focus on the well-identified Intent to Treat parameter which compares eligible and ineligible entrepreneurs.

3.4 Attrition and quitting

Some entrepreneurs attrited from our sample between the baseline and the first and second followup surveys; importantly, however, attrition rates do not vary significantly across treatment groups. Specifically, at the time of the first post-intervention survey, sample attrition was 12.8 percent in the treatment compared to 15.3 percent in the control (p-value = 0.58). During the second followup survey, we were able to survey some of the attrited entrepreneurs from the first follow-up, while some new subjects attrited: relative to the baseline sample, attrition in the second followup was 16.5 percent in the control group compared to 18.3 percent in the treatment group (p-value = 0.77). Virtually all of the attrited entrepreneurs either moved out of the village or were not available on the day of the interview; only three subjects ever refused to participate.[10]

Amongst the non-attriters in the sample overall, 21.2 percent had stopped running their business

[10]Comparing entrepreneurs who ever attrited with those who did not reveals that, pre-intervention, attrited entrepreneurs have significantly lower revenues and profits, fewer workers, work more hours, and are less likely to produce goods rather than re-sell goods (see Appendix Table 2); these relationships hold equally in both the treatment and control groups (results available upon request).

by the first followup survey and 49.5 percent had stopped running their business by the second follow-up, implying we do not observe business-related outcomes for these entrepreneurs (such as profits and revenues). Quitting one's business can certainly be considered an outcome that could be affected by the intervention; for example, if the training made the entrepreneur realize that her business was operating at an economic loss. Comparing quit rates across groups, however, they are strongly indistinguishable in both the first follow-up (p-value = 0.70) as well as at the second follow-up (p-value = 0.47).[11]

Finally, in both post-intervention surveys, we asked quitters why they stopped running their business, and what their main activity was after quitting. Table 2 contains the distribution of responses for the treatment and the control group and it is clear that there is no significant difference across groups in these responses. About 42 percent of the quitters did so because their business was losing money, about 20 of quitters did so for reasons of health, and a smaller percentage needed to care for a family member. Less than two percent quit their old business to start a new one. Table 2 also shows the distribution of new main activities amongst the quitters; again, there is no significant difference in these activities across treatment and control. The vast majority of quitters (about 75 percent) were doing house work and/or taking care of children as their principle activity. The remainder of the activities included retirement or exit from the labor force, unemployment (looking for work), running a new business, or working for a salary.

4. Empirical Strategy and Results

To isolate the causal impact of the business training classes, we estimate a series of difference-in-differences models of the following form:

$$y_{it} = \quad \alpha + \beta T_i + \delta Post_t + \gamma (T_i * Post_t) + \lambda Wave2_t + \mathbf{X_i}\Omega + \varepsilon_{it} \tag{1}$$

where y is the outcome interest, T is an indicator for living in a treatment village, $Post$ is an indicator for the post-intervention period, $Wave_2$ is an indicator for the first post-intervention survey,

[11] Perhaps not surprisingly, there are significant differences between those who ever quit and those who did not (see Appendix Table 3): compared to non-quitters, quitters had lower pre-treatment profits and revenues than non-quitters, more education (an extra one-half of a year), are about three years younger, and are poorer in terms of the one indicator of wealth that we observe, whether the roof of their house is made out of a temporary material. Furthermore, these relationships hold equally in both the treatment and control groups (results available upon request).

$\mathbf{X}$ is a vector of pre-intervention business and demographic characteristics, and ε is an error term. Pre-intervention characteristics are included as covariates to increase precision, and we only include covariates that were used in the randomization algorithm; below, we demonstrate that results are robust to the exclusion of these controls.[12]

Several issues are of note: First, the direct effect of the offer of treatment, or the Intent to Treat (ITT) effect, is identified by γ when equation 1 is estimated on the sample of all entrepreneurs in control villages and entrepreneurs in the treatment villages who were offered the classes. The indirect effect of the offer of treatment, or the Indirect Treatment Effect (ITE), is identified by γ when equation 1 is estimated on the sample of all entrepreneurs in the control villages and entrepreneurs in the treatment villages who were *not* offered the classes.

Second, with two post-intervention survey waves, we are able to estimate models that permit different treatment effects over time. However, as shown below, estimated treatment effects do not differ significantly across the two post-intervention survey waves, and so we pool the post-intervention surveys together in order to increase statistical power, while including $Wave2$ to absorb any time-specific effects.

Finally, statistical inference is complicated by the small number of clusters (i.e., villages), implying that the standard (asymptotic) method for computing standard errors will be incorrect. We thus report both p-values representing asymptotic, clustered standard errors (at the village level) as well as p-values computed using the wild bootstrap of Cameron, Gelbach and Miller (2008).[13]

4.1 The direct effect of classes on main business related outcomes

Columns 1-3 of Table 3 contain $ITTs$, estimated by equation 1, for the logarithm of three main business outcomes: self-reported profits, revenues, and the number of clients served in the last day the entrepreneur worked. The ITT for the logarithm of last day's profits (column 1) is 0.216, implying the offer of the business literacy classes has a positive effect on daily profits of about 23 percent. This effect is significant at the six percent level when using asymptotic, clustered standard errors and significant at the nine percent level when using wild bootstrapped, clustered standard errors.

[12] These pre-intervention covariates include: the number of workers in the business; the age and sector of the enterprise; the replacement value of business capital; whether the entrepreneur states that she lacks business skills; whether she is risk averse; her age, education, and number of rooms in her home; and her score on the business skills exercise.

[13] Randomization inference (Rosenbaum, 2002) can also be used to construct hypothesis tests of treatment effects; however, because our treatment effects are large, the power of randomization inference can be low. Regardless, we have implemented permutation tests for a subset of outcomes, finding p-values that are similar in magnitude to wild bootstrap p-values.

The corresponding Treatment on the Treated Effect (not reported) is larger by a factor of about 1.5 ($= 1/0.65$). This effect of business training on profits is large (both the ITT and the TTE), yet comparable to other studies in the literature (Bruhn, Karlan and Schoar (2012); Mckenzie and Woodruff (2012)).

Columns 2 and 3 of Table 3 show that treatment effects on revenues and the number of clients served are on the same order of magnitude as for profits – the ITT for revenues is 0.248, significant at the five percent level with wild bootstrap p-values, while the ITT for clients served is 0.218, significant at the 12 percent level with wild bootstrap p-values. It appears that the increase in revenues and the number of clients served is at least part of the explanation for the observed increase in profits; we return to probe these mechanisms in more detail below.

To address concerns with multiple hypothesis testing (Romano and Wolf, 2005), we create a standardized measure of the three main business outcomes presented in Table 3: profits, revenues, and clients served in the last day worked. As in Kling, Liebman and Katz (2007), we first standardize each of the variables independently with respect to the baseline control group and then take the average across the standardized measures.

We note that it is not clear how to create a standardized measure when an observation has missing information for one of the component outcomes. One solution it to calculate the standardized measure as the sum of standardized outcomes, regardless of whether all component outcomes are observed; for example, it would contain the average of two standardized measures if the third outcome is not observed. Another solution drop observations completely if any of the component measures are missing. Column 4 of Table 3 contains a standardized measure calculated in this latter manner, which we believe to be more conservative, and shows that the standardized outcome increased by 0.153 standard deviations amongst those offered treatment, significant at the 5 percent level. Reassuringly, we find a very similar effect, both in magnitude and statistical significance, when using a standardized measure that includes observations even with when some of the outcome measures are missing (not shown).

It is also important to note that estimated treatment effects are of similar magnitude in both the short run (one year post-intervention) and the medium run (2.5 years after the intervention). Table 4 contains by-wave $ITTs$ estimated from a version of equation 1 that includes indicators for each post-intervention wave, and their interaction with the treatment indicator. In general, point estimates of the ITT in wave 3 are of similar magnitude as in wave 2, yet are more noisy, and we can not reject the hypothesis that the $ITTs$ are equal across waves. This latter result is rather important as it shows that the one time intervention appears to have long lasting positive effects which do not seem to decay 2.5 years after the intervention.

4.2 Robustness of the main results

Our estimated treatment effects are robust to various alternative specifications, as demonstrated in Table 5 for the main business outcomes. First, columns 1, 4, and 7 replicate the estimates in Table 3, but exclude pre-program covariates. Perhaps not surprisingly, given the randomization, the point estimates do not change meaningfully yet p-values increase as we lose precision.

Second, we test the robustness of the logarithmic transformation of the outcome when it equals zero in levels, i.e., when the entrepreneur has no revenue, no profit, or serves no clients. In columns 2, 5, and 8 of Table 5, we impute zero profits, revenues, and number of clients served with a small, strictly positive number (specifically, one peso of profit and revenue and 0.1 clients); again, point estimates and p-values are very similar to those in Table 3, indicating that there is little information lost by excluding those observations with zero profits, revenues, or clients served in the logarithmic specifications.

Third, columns 3, 6, and 9 contain $ITTs$ estimated using the level of the outcomes as opposed to the logarithm. While the level of these outcomes are not preferred as their distributions are skewed, the magnitude and sign of the estimated $ITTs$ are consistent with the logarithmic transformation. Specifically, the offer of business classes increased the last day's profits by 48.6 pesos, significant at the 10 percent level (wild bootstrap p-value); last day's revenues by an insignificant 65.2 pesos; and the number of clients served in the last day by an insignificant 1.6 clients. Finally, while the rates of attrition are not differential across treatment groups (see Section 3.4), we show in a final robustness check in Appendix A that our results are largely robust to potential differential attrition across treatment groups by applying Lee's Bounds (Lee, 2009).

4.3 Possible mechanisms driving the main results

Having established the large and significant effect of business literacy classes on business profits, we now turn to explore why these results arose. Two mechanisms were already presented in Table 3: self-reported revenues in the last day worked increased, as did the self-reported number of clients served in the last day worked. Our good-specific data provides a separate way to estimate treatment effects on profits and revenues, and $ITTs$ for these outcomes are presented in the first two columns of Table 6; recall, however, that we only observe these measures in the baseline and the first post-intervention surveys. We find that the log of the mean good-specific profit and revenue increased by an 16.6 and 23.7 log points, respectively. Although insignificant at conventional significance levels, these point estimates are similar in magnitude to those for self-reported profits and revenues.[14] Column 3 of Table 6 shows that entrepreneurs marginally increased the number of goods they sold as a result of the offer of classes: the ITT on the logarithm of the number of goods sold is 0.116 (approximately 1 extra good for sale), with a wild bootstrapped p-value of 0.155. Interestingly, it appears that the observed increase in profits is coming from reduced costs rather than increased prices: the ITT for the logarithm of the mean unit cost of items sold is -0.293 log points (column 4, wild bootstrap p-value = 7) while the ITT for the log of mean unit price is 0.004 (column 5, strongly insignificant).

It is also interesting to note that program invitees seem to be changing the composition of the goods they sell. In particular, Table 7 contains $ITTs$ for the outcomes calculated from the good-specific questionnaire, but restricts the sample to those goods that were either (i) dropped between the baseline and first post-intervention survey, (ii) kept across both surveys, or (iii) added in the first post-intervention survey. Although these results are somewhat only suggestive given the low-power of our tests at the good-by-good level, they suggest that entrepreneurs offered the treatment dropped goods with low profits, revenues, and prices; kept goods with high profits and revenues and low costs; and added goods with high revenues and low costs.

Finally, Table 8 contains several other business related outcomes of interest. Column 1 presents the ITT on the percent of correct answers on the business practices exercise, and it seems that the program did not necessarily make entrepreneurs more business savvy, with a large but insignificant ITT of 5.6 percentage points (on a pre-treatment mean of about 40 percent). However, it does appear that the offer of classes significantly and meaningfully increased the use of formal accounting practices: column 2 shows that 4.7 percentage points more entrepreneurs used formal accounting methods post-treatment (wild bootstrapped p-value = 0.07). This is a large effect, considering that only one percent of treated entrepreneurs (and four percent of control entrepreneurs) used formal accounting

[14]Having two measures of business profits and revenues - one self-reported and the calculated from the good specific data - allows us to test whether the extent of measurement error in these outcomes is systematically linked to the offer of classes. Specifically, we cannot reject the equality of the correlations in the two measures for either profits or revenues between the control and treatment groups in the ex-post period, nor in a difference-in-differences specification. These results are inconsistent with systematic measurement error being the main driver of the positive ITTs. We thank Rema Hanna for suggesting this testing strategy.

practices pre-intervention. Although the effect is insignificant, the large point estimate on the number of hours worked per week by the owner (2.6 hours per week, column 3) is consistent with higher returns from entrepreneurship. There does not appear to be a significant effect of the program on the size of the enterprise, as measured by the number of employees (column 3), or on the number of hours worked per week by employees (column 4).

Interestingly, invitees are 8.7 percentage points more likely to register their business with a government agency (column 6); again, this is a large effect, representing an increase over pre-intervention registration levels of about 40 percent. The CREA course included a thorough discussion of the pros and cons of registering ones business, and this positive point estimate suggests that, upon learning this information, registering is an optimal decision for some entrepreneurs. Finally, in column 7, we confirm with the difference in differences model our earlier statement that the offer of classes did not affect the (average) propensity to quit one's business (wild bootstrap p-value = 0.69).

4.4 Spillover and general equilibrium effects of business literacy classes

We now turn to estimates of the Indirect Treatment Effects, presented in Table 9. To the extent that villages are segmented, these estimates identify the local spillover and general equilibrium effects of the intervention. In interpreting these estimates, it is important to recall that we do not observe male entrepreneurs, and thus do not observe the entire market, however the sectors studied in the current paper are female dominated in the sampled villages. As mentioned above, ITEs are estimated by equation 1 on the sample that excludes any entrepreneurs who were invited to the classes.

It is clear that very few of the estimates in Table 9 are significantly different from zero, under either asymptotic or wild bootstrap standard errors. However, the magnitude of many of the estimates are large and economically meaningful. In particular, the ITE on the logarithm of self-reported last day's profit is negative and rather large in magnitude, implying a decrease in profits of about 11 percent for control entrepreneurs in treatment villages relative to entrepreneurs in control villages. This point estimate is about half of the increase in profits realized by treatment entrepreneurs in treatment villages (approximately 23 percent, Table 3), and suggests the overall effect of the program on the profits of female entrepreneurs in treatment villages is about 12 percent. $ITEs$ on the last day's revenue and the number of clients are positive (just as the direct treatment effects), yet small in magnitude, approximately one quarter to one third the magnitude of the direct treatment effects. Not surprisingly given the opposing signs on the point estimates, the ITE on the standardized measure is essentially zero (0.013, wild bootstrap p-value = 0.798).

Reassuringly, calculated profits and revenues from good-specific data yields very similar $ITEs$ to the self-reported measures, although these estimates are more precisely estimated (wild bootstrap p-values of 0.164 and 0.137, respectively). There does not appear to be an indirect effect on the number of goods for sale, but the ITE on the logarithm of the mean unit cost is 0.221 and close to marginal significance (wild bootstrap p-value = 0.139). Interestingly, this estimate is of similar magnitude to the direct effect (-0.293, Table 6), but of the opposite sign. It is not clear why these estimates should be so divergent, but perhaps if factor markets are not perfectly competitive, those offered treatment were able to purchase input materials from lower-cost suppliers, leaving those not offered the treatment to purchase inputs from higher-cost suppliers. It is theoretically ambiguous as to whether we would expect the indirect effect on prices of the control entrepreneurs to be positive or negative. The point estimate suggests a small, yet insignificant positive indirect effect of the treatment on the logarithm of the mean unit price (0.072, wild bootstrap p-value = 0.326).

It is reasonable to believe, given the small size of these villages, that treated entrepreneurs interact with non-treated entrepreneurs, perhaps sharing lessons learned in the business literacy classes. There do not appear to be spillover effects on business knowledge (as measured by our business practices exercise), but there does appear to be a large and statistically significant impact on the use of formal accounting methods: relative to the control villages, 5.7 percentage points more control entrepreneurs in treatment villages use formal accounting methods, significant at the 3 percent level (wild bootstrap p-value). This estimate is even larger than the positive direct effect of the treatment (a 4.7 percentage point increase, Table 8). However, unlike the direct effect, there is not a positive effect on the likelihood of being registered with a government agency; in fact, the ITE on this outcome is a negative 3.7 percentage points (wild bootstrap p-value = 0.337).

There is not a significant indirect effect on the number of employees, but there is a significant increase in the hours worked by the owner (3.9 hours per week, an increase of about 10 percent over baseline) and a large-in-magnitude but slightly less significant increase in the hours worked by employees (2.3 hours per week, an increase of about 20 percent over baseline). Perhaps the untreated entrepreneurs in treatment villages have increased the hours worked in order to compensate for the decrease in profits (note that no entrepreneurs in our sample stated that they subtract the opportunity cost of their time from revenues in calculating profits).

5. Entrepreneurial Experimentation and Business Literacy

5.1 A simple model

As mentioned above, it is possible that business literacy classes can affect both the way an entrepreneur runs her business, and the likelihood that she stays in business. In order to better understand how entrepreneurs react to the information provided by business literacy classes, we develop a simple model that will provide testable predictions we can bring to the data.

Our model builds off of Karlan, Knight and Udry (2012), modified to capture two key components of our intervention: (i) accounting practices and (ii) "business" skills; as well as allowing for the outside option of quitting one's business. We think of the entrepreneur in this context as an experimenter with a noisy signal of her productivity who faces the outside options of quitting her business. The business classes lower the cost of (or introduce) a new, more expensive, yet potentially profitable, technology for running one's business, i.e., a set of new managerial and accounting practices. The entrepreneur then decides whether to adopt this more productive and expensive technology. The technology, however, is risky and entrepreneurs are heterogeneous in their ability (or productivity), the technology is only profitable for those with high ability, and ability is only partially unobservable to the entrepreneur. Importantly, through the adoption of the new technology, irrespective of the outcome, the entrepreneur learns her own productivity which informs her decision to continue running the business and with which technology.

The programming problem is formulated as follows where the entrepreneurs maximize their lifetime consumption subject to the resource constraint:

$$\max_{c_{it}} V \equiv E_0 \sum_{0}^{\infty} \beta^t U(c_{it}, w) \tag{2}$$

$$s.t. \quad c_{it} \leq \pi_{it} \tag{3}$$

$$\text{where } \pi_{it} = f(x, \alpha_i) - x \text{ and } \pi_{i0} = w - x \tag{4}$$

where c_{it} is entrepreneur i's consumption in period t and w is her initial wealth. We assume no credit markets are available, so consumption can not exceed per period profits π_{it}. Revenues, $f(x, \alpha_i)$, are a function of the management technology an entrepreneur uses, x, and her productivity (i.e., her type), α_i. Costs, also denoted by x, are indexed directly to the choice of management technology. The entrepreneur receives no revenue in the initial period ($t = 0$), yet must incur the cost of her choice of management technology in that period.

For simplicity, we assume that there are only two types of technology, new and old, denoted by x_h and x_l respectively, which cost x_h and x_l respectively (with $x_h > x_l$). For the more productive types of entrepreneurs, the more expensive technology is more profitable than the less expensive technology, while for less productive types, the reverse is true: that is, $\pi(x_h) - x_h > \pi(x_l) - x_l$ only for entrepreneurs of above a certain productivity type, say, $\overline{\alpha}$. If no management technology is chosen, the entrepreneur quits her business and incurs no cost, in which case $x = 0$ and she receives the outside option pay-out π^0. As will become clear, we think of the business literacy classes as lowering the costs of or introducing the new management technology (x_h) for those who attend the classes.[15]

Reflecting the environment in our experimental setting, we assume that the entrepreneurs do not know their type with certainty ex-ante, but believe they are either a high productivity type with probability p_i^h, a low productivity type with probability p_i^l, and very low productivity type (the type that will quit her business) with probability p_i^0 ($\sum_{j=0,l,h} p_i^j = 1.$). Choosing the new technology, however, will reveal the type of the entrepreneur ex-post as follows: if the more expensive management process succeeds, it returns π^h and the entrepreneur knows she is of type α^h or greater; if it returns π^l the entrepreneur knows she is of type $[\alpha^l, \alpha^h)$; and if it returns profits that are low enough, the very unsuccessful entrepreneur realizes that her type is lower than α^l, and quits her business to receive the outside option, π^0. Thus, experimentation informs the entrepreneur whether she is: (i) a "good"; (ii) a "bad"; or (iii) a "non" entrepreneur.

The entrepreneur's value function is as follows:

$$\begin{aligned}
V \equiv \max_{x=x^l,x^h,0} = \quad & U(w-x) + \\
& \mathbf{1}[x=x^h]\beta\left(p^h V(\pi^h(x^h), \alpha \geq \alpha^h) + p^l V(\pi^l(x^h), \alpha^l \leq \alpha < \alpha^h) + p^0 V(\pi^0(x^h), \alpha < \alpha^l)\right) + \\
& \mathbf{1}[x=x^l]\beta V(\pi^l, \alpha) + \\
& \mathbf{1}[x=0]\beta V(\pi^0, \alpha < \alpha^l)
\end{aligned}$$

The entrepreneur will decide to invest in the new technology rather than sticking with the old tech-

[15] We assume that the entrepreneurs have sufficient initial wealth to experiment with the new technology if they so wish. Recall that there isn't any credit market available or alternatively that the technologies are not colleteralizable.

nology if the following condition holds:

$$
\begin{aligned}
u(c^l) - u(w - x^h) < p^h \frac{\beta}{1-\beta} u(c^h) + \beta p^l u(\pi^l(x^h)) + \beta p^0 u(\pi^0(x^h)) + \\
p^l \frac{\beta^2}{1-\beta} u(c^l) + p^0 \frac{\beta^2}{1-\beta} u(c^0) - \frac{\beta}{1-\beta} u(c^l)
\end{aligned}
$$

That is, she will choose to experiment if she is sufficiently optimistic about p^h.[16]

Importantly, the new technology has a (positive) option value; that is, it offers the opportunity to learn one's type and possibly increase profits (become a "good" entrepreneur) if her type is high enough. Because of the positive option value, the entrepreneur may in fact choose to experiment even if the first-period expected (net) return from adopting the new technology is lower than the net return of the old technology, i.e. $p_i^h \pi_i^h(x^h) + p_i^l \pi_i^l(x^h) + p_i^0 \pi_i^0(x^h) < \pi_i^l(x^l)$. The reason is that:

$$
u(c^l) - u(w - x^h) + \beta \left(u(c^l) - p^h u(c^h) - p^l u(\pi^l(x^h)) - p^0 u(\pi^0(x^h)) \right) < p^h \frac{\beta^2}{1-\beta} \left(u(c^h) - u(c^l) \right).
$$

The term on the left hand side is the option value. This relationship implies that even if the second term on the right hand side is positive and fairly large it could still be that the option value is large and positive.

Furthermore, if we maintain that high ability entrepreneurs are better off using the new technology, low ability entrepreneurs are better off sticking to the old technology, and the lowest ability types are best off by quitting, as follows:

$$
\begin{aligned}
V(x^0, \alpha \leq \alpha^l) > V(x^l, \alpha \leq \alpha^l) > V(x^h, \alpha \leq \alpha^l) \\
V(x^0, \alpha > \alpha^h) < V(x^l, \alpha > \alpha^h) < V(x^h, \alpha > \alpha^h) \\
V(x^l, \alpha^l < \alpha \leq \alpha^h) > V(x^h, \alpha^l < \alpha \leq \alpha^h) \\
V(x^l, \alpha^l < \alpha \leq \alpha^h) > V(x^0, \alpha^l < \alpha \leq \alpha^h).
\end{aligned}
$$

[16] A similar problem applies to the decision of adopting the old technology, i.e., the decision to become an entrepreneur. We do not investigate this decision here as our baseline sample are all currently entrepreneurs.

then some entrepreneurs will quit their businesses when they discover their type. These ex-post choices can be summarized graphically for a given set of parameter values, as in Figure 2. In that figure we have essentially that the value functions are ordered according to the inequalities above so that an entrepreneur would quit her business if her type is in the leftmost portion of the horizontal axis (α), would use the old technology for intermediate values of her type and employ the new technology in the right part of the graph.

Turning to our experimental setting, we can think of the business literacy classes as either introducing or lowering the cost of the new technology for invited women. Under the assumption that the probability of success is positively related to one's ability, i.e. p^h is positively related to α, the treatment will induce less optimistic entrepreneurs to try the new technology relative to the control. This implies that the average profits amongst the treated may be smaller than amongst the controls, as some of the treated are low ability types who are "trying out" the new technology. Thus, the model can not predict what the average effect of the treatment (i.e., offering business literacy classes) will be on firm profits, as we would require knowledge of the distribution of types and beliefs in the population, as well as the relative productivity gains the new technology offers. Ultimately it's an empirical matter whether:

$$E(\pi|T=1) - E(\pi|T=0) \gtreqqless 0, \tag{5}$$

where $T = 1$ for invited entrepreneurs in treatment villages, and 0 otherwise.

However, we do know that amongst the high ability entrepreneurs ($\alpha > \alpha^h$), mean profits should increase amongst the treated relative to the controls:

$$E(\pi|T=1, \alpha > \alpha^h) - E(\pi|T=0, \alpha > \alpha^h) > 0. \tag{6}$$

Furthermore, we also know that amongst the low ability entrepreneurs ($\alpha \leq \alpha^l$) we should see "excess" quitting amongst treatment group relative to the control group:

$$Pr(Quit|T=1, \alpha < \alpha^l) - Pr(Quit|T=0, \alpha < \alpha^l) > 0. \tag{7}$$

Testing these two predictions requires knowledge of α. As we do not observe productivity directly, we proxy productivity with pre-treatment profits, π_0. Thus, the two testable implications of this model are that the intention to treat effect on quitting should be decreasing in pre-treatment profits and the intention to treat effect on profits should be increasing in pre-treatment profits as follows:

$$\frac{\partial\{E(\pi|T=1)-E(\pi|T=0)\}}{\partial\pi_0} \geq 0 \tag{8}$$

$$\frac{\partial\{Pr(Quit|T=1)-Pr(Quit|T=0)\}}{\partial\pi_0} \leq 0. \tag{9}$$

5.2 Testing the empirical predictions

We now bring these predictions to the data and explore how the direct treatment effects vary as a function of baseline profits. For clarity of presentation, we split our sample into those above and below the median of the last day's pre-treatment profit, and present separate $ITTs$ estimated by equation 1.

Table 11 contains estimates for profits, revenues, and number of clients served in the last day worked. For all of these measures, as well as the standardized measure in columns 7 and 8, the results are quite striking, and show that by-and-large the positive effects of the intervention consistently arise from those above the median of pre-treatment profits. Although we cannot reject the equality of the effects between the top and bottom half of the baseline profits distribution, it is clear that the point estimates are economically quite different from each other, and the $ITTs$ are only statistically different from zero amongst those above the median of pre-treatment profits. For example, the ITT on last day's profits is 0.254 for those above the median and 0.053 for those below the median.

A similar, albeit less statistically precise, story emerges when we look at the outcomes constructed from the good-specifc data in Table 12. While none of these estimates are significantly different from zero at more than the five percent level, point estimates suggest economically meaningful differential impact consistent with the predictions of our model. Specifically, last day's profits increased by 34.3 log points for those above the median and fell by 11.2 log points for those below the median. Similarly, treatment effects on the last day's revenues and the number of goods for sale are larger in magnitude for those above the median than for those below. Mean unit cost appears to have fell for both those above and below the median of pre-treatment profits, while the null overall effect on the mean unit price is masked by a slight increase in price amongst those above the median and a slight decrease for those below the median.

Finally, Table 13 shows another striking result: the positive treatment effect on the use of formal accounting practices is concentrated completely amongst the most able entrepreneurs: the ITT for those above the median of pre-treatment profits is 0.09 compared to 0.007 for those below the median, although neither of these estimates are significantly different from zero. There is a small differential in terms of knowledge gains as measured by our business practices exercise, no differential in terms of hours worked per week by the owner, but a large differential in terms of hours worked per week by employees: close to a 6 hour increase for those above the median compared to a 5 hour decrease for those below the median. These effects on hours worked by employees seem to be driven by differential hiring practices. There is little differential effect in terms of registering with a government agency.

Differential likelihood of quitting by baseline profits

The last two columns of Table 13 speak to the prediction that entrepreneurs of lower ability should be more likely to quit their businesses upon trying the new management technologies taught by the CREA classes. Lowering the cost of adopting this technology induces lower ability (lower α) entrepreneurs to try the technology, and these "excessive" experimenters will rapidly realize they are not good entrepreneurs once they keep better accounting and try out different business practices. They therefore quit their enterprise.

While not statistically significant, an economically meaningful differential in terms of quitting is apparent: column 13 of Table 13 shows that those below the median of pre-treatment profits are 1.9 percentage points more likely to quit while those above the median (column 14) are 4.8 percentage points less likely to quit, relative to the control. This differential - 7.1 percentage points - is large, representing about 14 percent of the post-treatment quit rate amongst control firms.

We further explore the hypothesis that the treatment will induce the low ability entrepreneurs to quit by looking at the far left tail of the distribution of pre-treatment profits, the lowest ability entrepreneurs. First, we present the distributions of pre-treatment profits in the whole sample compared to the distribution of pre-treatment profits amongst those who did not quit post-treatment: Figures 3 and 4 present these distributions for the treatment and control group separately. It is clear that the survived sample (i.e., those who did not quit) is similar in terms of baseline profits to the whole sample in the control group (Figure 3). In the treatment group, however (Figure 4), the distribution of the survived sample is significantly shifted to the right consistent with the prediction that those with the lowest ability (pre-treatment profits) will be induced to quit upon learning they are in fact a low ability type.

6. Conclusions

A large literature on enterprises in developing countries finds that firms are often run inefficiently (see for example Bloom et al. (2013); Bruhn, Karlan and Schoar (2012)), this could have multiple causes from the lack of credit market, to goods market imperfections and so on. Amongst those reasons it could be that entrepreneurs lack the basic business skills required to run an enterprise, such as an understanding of costs, sales, profits, price setting, marketing, and competition.

Recent years have seen a series of interventions offering business or financial training to entrepreneurs. Our intervention is unique in several ways, and thus offers new insights into our understanding of the effect of business literacy classes on enterprise performance. First, the intervention is very intensive, lasting six weeks with two, four-hour classes per week for a total instruction time of 48 hours; this is more than double many of the prior studies in this literature (e.g., Drexler, Fischer and Schoar (2011) and Karlan and Valdivia (2011). Second, our experimental design involves offering classes to a random subset of the population of micro-enterprises while not providing any other intervention (such as credit) beside business literacy training. This implies our findings are valid for a broad class of businesses, and identify the effects of the classes uniquely. Third, our survey design includes two post-intervention surveys (one year and 2.5 years post-intervention), which allows us to explore both the short and medium run effects of the training. Fourth, we are able to detect village-level spillover and general equilibrium effects thanks to our experimental design.

Our results indicate that a basic training in business management and accounting is capable of significantly increasing profits. This increase appears to be driven by a combination of higher revenues, lower costs, more clients served, and an increased use of formal accounting methods. Importantly, knowledge gained through the intervention does not appear to fade, as we observe positive effects persisting into the medium run.

These positive program impacts, however, must be weighed against the costs of running the business literacy classes in order to justify the intervention. In fact, a simple comparison of costs and benefits shows the program is indeed very cost-effective. First, the cost of running the CREA classes is extremely low, as local teachers were hired for a modest wage, minimal materials were provided to the students, and community centers were used to hold classes at no-cost. Specifically (and using US dollars for convenience), each of seven treatment villages had two teachers who taught for a total of

48 hours and were paid \$10 per hour yielding \$6720 (=7 x 2 x 48 x \$10) in salaries. While only 65 percent of invitees came to class, the classrooms would have accommodated all invitees, so if CREA were to replicate the program, the appropriate per-invitee cost of teacher's salaries with 164 invitees is \$49.97 (=\$6720 / 164). Materials (photocopies of lessons, pens, paper, calculators, and CREA logo hats that were used as prizes) totaled about \$5 per participant; inflating the latter costs to the invitees, the total per-invitee cost of CREA's program is \$57.66 (=\$49.97 + \$7.79).

Second, a back-of-the-envelope calculation shows that the benefits in terms of increased profits far outweigh these costs: The ITT on the logarithm of daily profits is 0.216, which implies the offer of classes increased daily profits by 23.4 percent (=exp(0.216)). The mean pre-treatment daily profit in the treatment group was \$10.2, implying the offer of treatment increased daily profits by \$2.38 (=\$10.2 x 23.4%). Pre-treatment, entrepreneurs in the treatment group reported working an average of 5.17 days per week. We do not know how many weeks are worked per year, but given that some of the businesses are seasonal (such as selling certain handicrafts or seasonal foods), a conservative assumption is that the average entrepreneur works half the year, or 26 weeks. Using a seven percent annual discount rate, the present discounted value of the increased profits due to the program is \$4394.50 (= (\$2.38 x 5.17 x 26)/0.07). It should be clear that it would be difficult to find a scenario under which increased profits do not outweigh the program costs, even if we were to include the opportunity cost of missed work when taking the classes, or to count as a program cost the negative indirect treatment effect on the profits of control firms in treatment villages.

Furthermore, our results are consistent with the predictions derived from our simple model of entrepreneurial experimentation: that only high-quality entrepreneurs will benefit from the business training, while very low quality entrepreneurs quit their business once the training helps them realize they are ill-suited to entrepreneurship. This is an important result which might have important long-run implications in terms of firm and market dynamics, in particular if bad firms have negative effects on potentially good firms, e.g. pricing below cost. For example, the faster disappearance of bad firms might allow good firms to grow to a scale that is more efficient (Hsieh and Klenow (2009) and Hsieh and Klenow (2010)).

Finally, an important finding is that the large positive direct effect of the program on firm profits is mitigated by a large negative (albeit imprecisely estimated) indirect effect on the profits of control firms in treatment villages. The negative indirect effect seems to arise from input market imperfections so that if the policy were to be scaled up it wouldn't necessarily have negative spillover effects as long as there are enough suppliers of intermediate-production inputs. Our indirect treatment effects result do not suggest a large effect on the demand side for the untreated entrepreneurs in the treatment villages, therefore if the policy were to be scaled up, as long as the suppliers market doesn't react increasing prices, we should expect effects of similar magnitudes to the one estimated here. Also notice that the increase in profits for treated firms comes only partially from savings on production costs, while about 50% of the effect is explained by changes in managerial practices and goods menu. There naturally a few open questions such as: why is the supply market imperfect? Is there an alternative policy which would increase competition amongst suppliers and therefore reduce production costs?

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Figure 1: Sectors of micro-enterprise activity pre-treatment

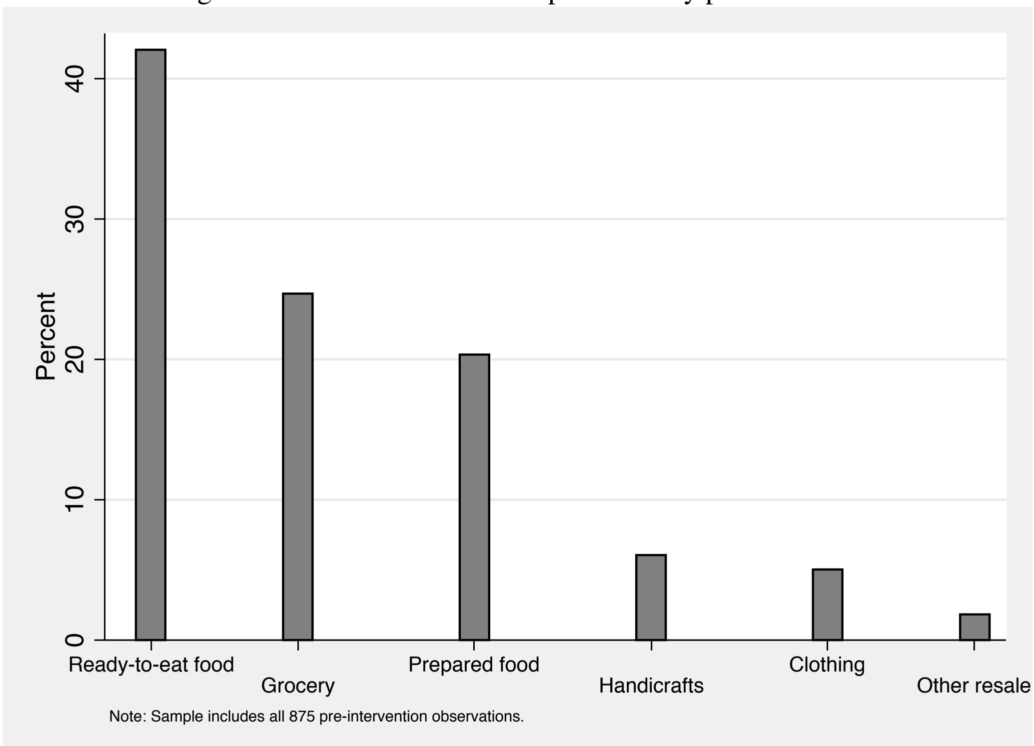

Figure 2: Entrepreneurial choice.

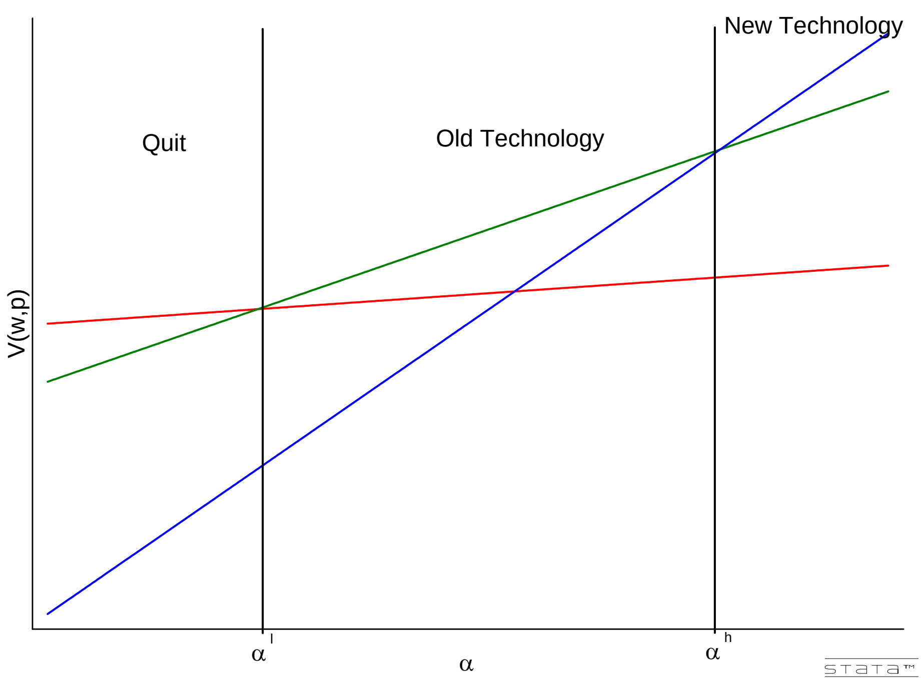

Figure 3: The distribution of baseline (log) daily profits amongst the whole and survived sample of control group enterprises

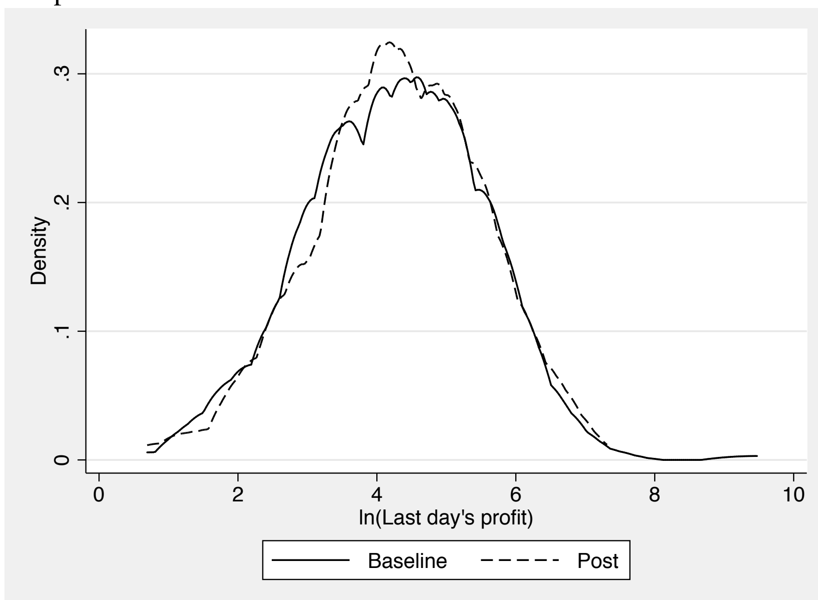

Figure 4: The distribution of baseline (log) daily profits amongst the whole and survived sample of treatment group enterprises

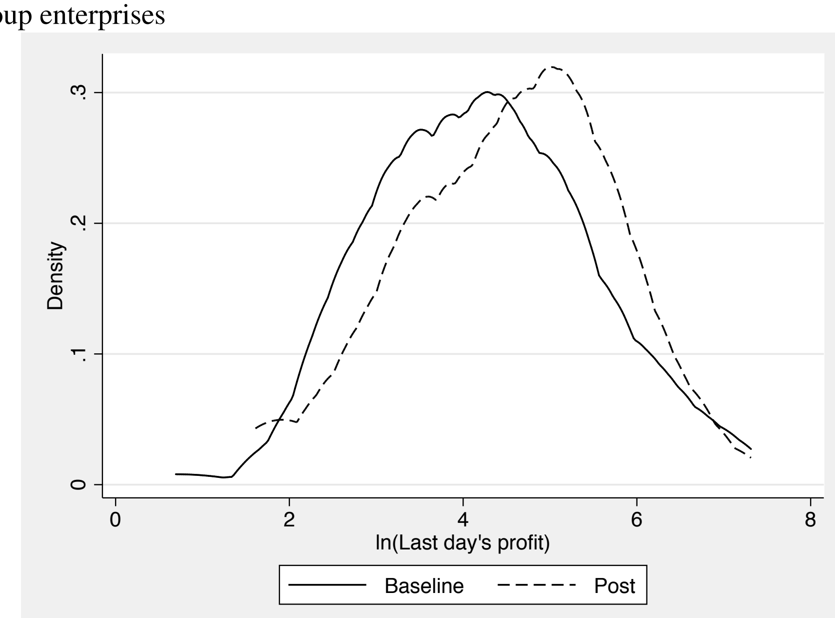

Table 1: Pre-treatment characteristics, by treatment group

	Treatment		Control		(1)=(3)	
	Mean	(s.e.)	Mean	(s.e.)	p-value	N
	(1)	(2)	(3)	(4)	(5)	(6)
Personal Characteristics						
Age	46.04	(0.48)	45.67	(0.53)	0.60	869
Years of education	5.96	(0.32)	6.07	(0.13)	0.72	846
Roof is made of temporary material	0.33	(0.09)	0.32	(0.05)	0.92	844
Score on math exercise (percent correct)	0.39	(0.04)	0.47	(0.03)	0.10	864
Keeps formal business accounts	0.01	(0.01)	0.04	(0.01)	0.09	873
Weekly hours worked in enterprise	39.43	(3.19)	39.19	(1.65)	0.95	866
Reservation wage, monthly	2,986.29	(92.06)	2,974.28	(140.90)	0.94	696
Maximum loan available if needed	8,703.94	(1,079.86)	9,016.38	(1,951.88)	0.89	689
Monthly interest rate on a potential loan	5.48	(0.62)	6.43	(0.32)	0.17	506
Business Characteristics						
Produces goods for sale	0.62	(0.03)	0.67	(0.04)	0.25	875
Last day's profit	132.24	(16.06)	154.92	(22.61)	0.42	760
Last day's revenue	456.16	(55.18)	405.96	(35.89)	0.49	840
Number of clients last day	14.03	(1.47)	14.43	(1.16)	0.83	808
Total number of workers, including owner	1.58	(0.05)	1.66	(0.03)	0.17	864
Weekly hours worked by employees	10.27	(2.27)	10.49	(0.84)	0.92	872
Age of business (years)	6.77	(0.84)	7.62	(0.65)	0.37	874
Replacement value of business capital	8,062.61	(1,009.51)	9,238.82	(1,023.20)	0.30	875
Registered with at least one gov't agency	0.18	(0.03)	0.26	(0.03)	0.04	847

Notes: Sample includes all subjects interviewed in the baseline survey. Asymptotic robust (s.e.) clustered at the village level. All monetary variable are measured in Mexican Pesos (~13 pesos / 1 U.S. dollar). Reservation wage is the minimum stated monthly wage a women would accept in order to quit her business.

Table 2: Reasons for quitting ones business and principle activity after quitting

	Treatment		Control		(1)=(3)	
	Mean	(s.e.)	Mean	(s.e.)	p-value	N
	(1)	(2)	(3)	(4)	(5)	(6)
Reason given for quitting the business:						
Business was losing money	0.41	(0.05)	0.44	(0.03)	0.66	333
For reasons of health	0.19	(0.02)	0.21	(0.02)	0.61	333
Found a salaried job that paid more	0.04	(0.02)	0.03	(0.01)	0.70	333
Needed to care for a family member	0.09	(0.03)	0.10	(0.02)	0.71	333
Started a new business	0.01	(0.01)	0.01	(0.01)	0.96	333
Other	0.26	(0.07)	0.21	(0.02)	0.49	333
Main activity, now that subject is not running the business:						
House work / taking care of children	0.74	(0.03)	0.78	(0.03)	0.28	333
Looking for work	0.08	(0.02)	0.06	(0.02)	0.47	333
Running a new business	0.06	(0.02)	0.05	(0.02)	0.70	333
Working for a salary	0.05	(0.02)	0.04	(0.02)	0.82	333
Retired or not looking for work	0.04	(0.01)	0.03	(0.02)	0.78	333
Other activity	0.04	(0.01)	0.04	(0.01)	0.92	333

Notes: Sample includes all subjects subjects who quit in either the first or second post-intervention survey. Assymptotic robust (s.e.) clustered at the village level.

Table 3: The effects of business training on main business outcomes

Outcome =	ln(Last day's profit)	ln(Last day's revenue)	ln(# clients last day)	Standardized
	(1)	(2)	(3)	(4)
Intention to Treat (ITT) effect	0.216	0.248	0.218	0.153
p-values, Asymptotic	(0.059)	(0.041)	(0.079)	(0.051)
p-value, Wild Bootstrap	(0.090)	(0.052)	(0.120)	(0.049)
Pre-program covariates	Yes	Yes	Yes	Yes
Observations	1,183	1,357	1,312	1,127

Notes: Sample excludes subjects who were not offered treatment in treatment villages. The standardized outcome is constructed as the mean of standardized z-scores (see text). Covariates include the following pre-program characteristics: number of workers, age of the enterprise, sector, replacement value, lack of business skills, risk aversion, age, education, number of rooms, and score on a business skills exercise. Both methods of calculating p-values allow for intra-village (cluster) correlation.

Table 4: The effects of business training by wave

Outcome =	ln(Last day's profit)	ln(Last day's revenue)	ln(# clients last day)	Standardized
	(1)	(2)	(3)	(4)
ITT wave 2	0.215	0.234	0.230	0.152
p-values, Asymptotic	(0.055)	(0.025)	(0.044)	(0.037)
p-value, Wild Bootstrap	(0.070)	(0.036)	(0.086)	(0.027)
ITT wave 3	0.215	0.282	0.195	0.159
p-values, Asymptotic	(0.309)	(0.149)	(0.328)	(0.223)
p-value, Wild Bootstrap	(0.340)	(0.182)	(0.402)	(0.249)
H0: ITT wave 2 = ITT wave 3, p-value, Asymptotic	0.999	0.765	0.835	0.950
Observations	1,183	1,357	1,312	1,127

Notes: Sample excludes subjects who were not offered treatment in treatment villages. The standardized outcome is constructed as the mean of standardized z-scores (see text). Covariates included (see text). Both methods of calculating p-values allow for intra-village (cluster) correlation.

Table 5: Robustness of the main effects

Variable =	Last day's profit			Last day's revenue			# clients last day		
Transformation =	Log	Log, imputed zeros	Level	Log	Log, imputed zeros	Level	Log	Log, imputed zeros	Level
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Intention to Treat (ITT) effect	0.163	0.203	48.579	0.226	0.250	65.177	0.251	0.205	1.625
p-values, Asymptotic	(0.128)	(0.062)	(0.166)	(0.067)	(0.031)	(0.533)	(0.068)	(0.093)	(0.435)
p-value, Wild Bootstrap	(0.180)	(0.112)	(0.101)	(0.083)	(0.090)	(0.535)	(0.080)	(0.121)	(0.476)
Pre-program covariates		Yes	Yes		Yes	Yes		Yes	Yes
Imputation indicator		Yes			Yes			Yes	
Observations	1,183	1,246	1,246	1,357	1,388	1,388	1,312	1,336	1,336

Notes: Sample excludes subjects who were not offered treatment in treatment villages. The standardized outcome is constructed as the mean of standardized z-scores (see text). Covariates included (see text). Both methods of calculating p-values allow for intra-village (cluster) correlation.

Table 6: Possible mechanisms

	Outcomes calculated from good-specific data				
Outcome =	ln(Last day's profit)	ln(Last day's revenue)	ln(# goods for sale)	ln(Mean unit cost)	ln(Mean unit price)
	(1)	(2)	(3)	(4)	(5)
Intention to Treat (ITT) effect	0.166	0.237	0.116	-0.293	0.004
p-value, Asymptotic	(0.460)	(0.142)	(0.138)	(0.037)	(0.938)
p-value, Wild Bootstrap	(0.496)	(0.170)	(0.155)	(0.074)	(0.922)
Observations	834	1,071	1,429	979	1,406

Notes: Sample excludes subjects who were not offered treatment in treatment villages. Covariates included (see text). Both methods of calculating p-values allow for intra-village (cluster) correlation.

Table 7: Effects on goods that were dropped across waves, kept across waves, and added post-intervention

Outcome:	*Goods that were:*	Intention to Treat (ITT) effect (1)	p-value, Asymptotic (2)	p-value, Wild Bootstrap (3)	Observations (4)
ln(Last day's profit)	Dropped	-0.476	(0.271)	(0.158)	120
	Kept	0.202	(0.567)	(0.684)	467
	Added	-0.035	(0.853)	(0.872)	97
ln(Last day's revenue)	Dropped	-0.364	(0.357)	(0.861)	129
	Kept	0.171	(0.600)	(0.734)	650
	Added	0.245	(0.211)	(0.220)	282
ln(Mean unit cost)	Dropped	-0.039	(0.802)	(0.917)	147
	Kept	-0.300	(0.090)	(0.045)	533
	Added	-0.072	(0.732)	(0.955)	109
ln(Mean unit price)	Dropped	-0.207	(0.277)	(0.464)	160
	Kept	0.009	(0.878)	(0.992)	732
	Added	-0.027	(0.827)	(0.980)	319

Notes: All sample excludes subjects who were not offered treatment in treatment villages. Dropped goods specifications use data from the pre-treatment wave only. Kept goods specifications use data from the pre-treatment wave and first post-treatment wave. Added goods specifications use data from the first post-treatment wave only. Covariates included (see text). Both methods of calculating p-values allow for intra-village (cluster) correlation.

Table 8: Possible mechanisms

Outcome =	% correct on business practices exercise		Uses formal accounting methods		Hours worked per week by owner		Hours worked per week by employees		# employees		Registered with government agency		Quit her business	
Sample = … median baseline profits	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Intention to Treat (ITT) effect	0.035	0.119	0.007	0.090	1.410	1.496	-5.019	5.814	-0.149	0.236	0.095	0.069	0.019	-0.048
p-values, Asymptotic	(0.541)	(0.192)	(0.774)	(0.039)	(0.655)	(0.632)	(0.092)	(0.386)	(0.140)	(0.285)	(0.130)	(0.306)	(0.754)	(0.271)
p-value, Wild Bootstrap	(0.544)	(0.240)	(0.750)	(0.214)	(0.662)	(0.618)	(0.204)	(0.224)	(0.124)	(0.306)	(0.174)	(0.298)	(0.748)	(0.298)
Observations	542	501	633	599	625	590	505	480	626	594	630	597	825	753

Notes: Sample excludes subjects who were not offered treatment in treatment villages. The standardized outcome is constructed as the mean of standardized z-scores (see text). Covariates included (see text). Both methods of calculating p-values allow for intra-village (cluster) correlation.

Table 9: The indirect effects of business training

Outcome:	Indirect Treatment Effect (ITE) (1)	p-value, Asymptotic (2)	p-value, Wild Bootstrap (3)	Obs. (4)
Main business outcomes				
ln(Last day's profit)	-0.110	(0.363)	(0.340)	1,250
ln(Last day's revenue)	0.056	(0.548)	(0.555)	1,430
ln(# clients last day)	0.073	(0.593)	(0.595)	1,371
Standardized	0.013	(0.846)	(0.866)	1,189
Outcomes calculated from good-specific data				
ln(Last day's profit)	-0.120	(0.423)	(0.164)	874
ln(Last day's revenue)	0.128	(0.137)	(0.137)	1,113
ln(# goods for sale)	0.015	(0.802)	(0.796)	1,495
ln(Mean unit cost)	0.221	(0.127)	(0.139)	1,031
ln(Mean unit price)	0.072	(0.294)	(0.326)	1,474
Other business outcomes				
Percent correct on business practices exercise	0.001	(0.987)	(0.942)	1,239
Uses formal accounting methods	0.057	(0.008)	(0.033)	1,501
Hours worked per week by owner	3.956	(0.050)	(0.078)	1,479
Hours worked per week by employees	2.289	(0.461)	(0.131)	1,194
Number of employees	0.016	(0.804)	(0.786)	1,485
Registered with government agency	-0.037	(0.313)	(0.337)	1,473
Quit her business	-0.029	(0.508)	(0.514)	1,907

Notes: Sample excludes subjects who were offered treatment in treatment villages. The standardized outcome is constructed as the mean of standardized z-scores (see text). Covariates included (see text). Both methods of calculating p-values allow for intra-village (cluster) correlation.

Table 10: Quitting and attrition by treatment and baseline profits

Outcome =	Quit the enterprise	Did not quit or attrite
	(1)	(2)
Treated	-0.012	-0.001
p-values, Asymptotic	(0.755)	(0.991)
p-value, Wild Bootstrap	(0.766)	(0.968)
Lowest 3% of last day's profits pre-treatment	0.003	0.112
p-values, Asymptotic	(0.971)	(0.155)
p-value, Wild Bootstrap	(0.974)	(0.128)
Treated x Lowest 3% of last day's profits pre-treatment	0.770	0.125
p-values, Asymptotic	(0.000)	(0.272)
p-value, Wild Bootstrap	(0.000)	(0.244)
Observations	404	553

Notes: *** p<0.01, ** p<0.05, * p<0.1
Sample uses a single cross section, and excludes subjects who were not offered treatment in treatment villages. Covariates included (see text). Both methods of calculating p-values allow for intra-village (cluster) correlation.

Table 11: Effects of training amongst those above and below the median of pre-intervention profits

Outcome =	ln(Last day's profit)		ln(Last day's revenue)		ln(# clients last day)		Standardized	
Sample = ... median baseline profits	Below	Above	Below	Above	Below	Above	Below	Above
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intention to Treat (ITT) effect	-0.053	0.254	0.036	0.276	0.073	0.447	0.004	0.246
p-values, Asymptotic	(0.739)	(0.074)	(0.872)	(0.068)	(0.675)	(0.009)	(0.969)	(0.006)
p-value, Wild Bootstrap	(0.776)	(0.058)	(0.848)	(0.070)	(0.672)	(0.020)	(0.888)	(0.006)
Observations	547	551	551	607	597	562	527	521

Notes: Sample excludes subjects who were not offered treatment in treatment villages. The standardized outcome is constructed as the mean of standardized z-scores (see text). Covariates included (see text). Both methods of calculating p-values allow for intra-village (cluster) correlation.

Table 12: Effects of training amongst those above and below the median of pre-intervention profits

	Outcomes calculated from good-specific data									
Outcome =	ln(Last day's profit)		ln(Last day's revenue)		ln(# goods for sale)		ln(Mean unit cost)		ln(Mean unit price)	
Sample = … median baseline profits	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Intention to Treat (ITT) effect	-0.112	0.343	-0.047	0.179	0.019	0.152	-0.235	-0.202	-0.119	0.075
p-values, Asymptotic	(0.749)	(0.161)	(0.857)	(0.330)	(0.860)	(0.088)	(0.313)	(0.361)	(0.219)	(0.589)
p-value, Wild Bootstrap	(0.261)	(0.275)	(0.207)	(0.235)	(0.894)	(0.098)	(0.326)	(0.450)	(0.226)	(0.618)
Observations	369	363	481	444	635	593	430	420	626	579

Notes: Sample excludes subjects who were not offered treatment in treatment villages. The standardized outcome is constructed as the mean of standardized z-scores (see text). Covariates included (see text). Both methods of calculating p-values allow for intra-village (cluster) correlation.

Table 13: Effects of training amongst those above and below the median of pre-intervention profits

Outcome =	% correct on business practices exercise		Uses formal accounting methods		Hours worked per week by owner		Hours worked per week by employees		# employees		Registered with government agency		Quit her business	
Sample = … median baseline profits	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Intention to Treat (ITT) effect	0.035	0.119	0.007	0.090	1.410	1.496	-5.019	5.814	-0.149	0.236	0.095	0.069	0.019	-0.048
p-values, Asymptotic	(0.541)	(0.192)	(0.774)	(0.039)	(0.655)	(0.632)	(0.092)	(0.386)	(0.140)	(0.285)	(0.130)	(0.306)	(0.754)	(0.271)
p-value, Wild Bootstrap	(0.544)	(0.240)	(0.750)	(0.214)	(0.662)	(0.618)	(0.204)	(0.224)	(0.124)	(0.306)	(0.174)	(0.298)	(0.748)	(0.298)
Observations	542	501	633	599	625	590	505	480	626	594	630	597	825	753

Notes: Sample excludes subjects who were not offered treatment in treatment villages. The standardized outcome is constructed as the mean of standardized z-scores (see text). Covariates included (see text). Both methods of calculating p-values allow for intra-village (cluster) correlation.

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APPENDIX

A. Bounds on Intention to Treat Effects

As discussed in Section 3.4 of the text, our sample of entrepreneurs shrinks overtime due to sample attrition. While the mean attrition rates do not differ significantly between the treatment and control groups, we nonetheless present in this appendix the results of bounding exercises that accounts for possible non-random attrition across groups.

We estimate bounds using a modified version of Lee's methodology (Lee, 2009) that allows us to maintain our difference-in-differences estimation strategy. Specifically, lower and upper bounds are calculated by first using Lee's methodology to trim each post-intervention period independently, and then estimating our difference in difference model with this trimmed data and the full pre-intervention sample. Table 4 contains both upper and lower bounds on the $ITTs$ calculated in this manner, for all of the main business-related outcomes. Perhaps not surprisingly, given the small and insignificant differential attrition across treatment groups, estimated bounds are tightly centered around estimated treatment effects.

Appendix Figure 1: An in-class example (Panel A) and an in-class exercise (Panel B) used in CREAs business literacy course.

Panel A

Supose that Belen has a store that sells beauty products. She sells makeup, hair products, and products for nails. Below is a list of articles that she sold today:

Belen's Beauty Products			
No.	**Article**	**Unit Price**	**Subtotal**
3	Nail files	$10	$30
1	Anti-dandruf shampoo	$30	$30
2	Eye shadow	$20	$40
		TOTAL	$100

As we can see in this bill of sale, Belen sold 3 nail files for 10 pesos each (3 x $10), generating a revenue of 30 pesos, 1 anti-dandruff shampoo for 30 pesos (1 x $30) gererating a revenue of 30 pesos, and 2 eye shadows for 20 pesos each (2 x $20) generating a revenue of 40 pesos. In total, Belen had revenue of 100 pesos today.

Panel B

Leticia has a business selling pineapple candy that she produces herself along with a small store in which she sells her candies and many other food items, from fruit and vegetables to cookies, flour, soda, etc. Leticia needs you to help her calculate her revenue from September 17th. Below is a list of products that she sold. Please calculate the revenue for each item and then calculate her total revenue.

Lety's Corner Store Sales on September 17th			
No.	**Article**	**Unit Price**	**Subtotal**
20	Pineapple candy	$3.50	
5	Kilos of tomatoes	$6	
10	Kilos of onion	$5	
4	Kilos of orange	$10	
6	Gansitos Marinela ®	$4	
8	Bottles of Coca-Cola ®	$5	
		TOTAL	

Appendix Figure 2: The applied math question given to entrepreneurs in the baseline and follow-up surveys

Section 10 Exercise

Now we are going to do an exercise, but I want to let you know that the numbers are invented, as is the example. If you have any questions, please ask me.

If they do no answer of don't want to answer, STOP, and leave the other parts blank.

Part 1: Imagine that you produce 5 tablecloths every week and that each tablecloth costs 10 pesos.

Suppose the first week you sell	1 tablecloth
The second week you sell	2 tablecloths
The third week you sell	2 tablecloths
and the fourth week you sell	5 tablecloths

a) How many tablecloths do you have left over at the end of the month?

b) What is your income for this month?

Part 2: Each week, you spend 5 pesos for cloth and 5 pesos in salaries in order to make tablecloths. Each month has 4 weeks.

c) How much are your profits at the end of the month? That is, how much money do you earn this month?

d) If your profits were to be zero for this month, what price should you have set for your tablecloths?

Appendix Table 1: Pre-treatment characteristics of treatment group entrepreneurs, by attendance status

	Attended		Did not attend		(1)=(3)	
	Mean	(s.e.)	Mean	(s.e.)	p-value	N
	(1)	(2)	(3)	(4)	(5)	(6)
Personal Characteristics						
Age	46.98	(0.91)	44.25	(1.80)	0.32	163
Years of education	6.07	(0.41)	5.76	(0.44)	0.57	161
Roof is made of temporary material	0.38	(0.11)	0.22	(0.07)	0.03	160
Score on math exercise (percent correct)	0.39	(0.05)	0.38	(0.06)	0.79	164
Keeps formal business accounts	0.01	(0.01)	0.02	(0.02)	0.72	164
Weekly hours worked in enterprise	37.84	(4.02)	42.43	(4.03)	0.36	162
Reservation wage, monthly	3,064.04	(140.02)	2,808.85	(271.85)	0.50	128
Maximum loan available if needed	8,479.91	(1,595.83)	9,190.24	(1,792.58)	0.79	130
Monthly interest rate on a potential loan	5.94	(0.64)	4.38	(1.07)	0.15	101
Business Characteristics						
Produces goods for sale	0.67	(0.02)	0.53	(0.08)	0.11	164
Last day's profit	110.83	(28.90)	177.91	(43.62)	0.34	141
Last day's revenue	337.85	(75.24)	690.53	(243.80)	0.28	158
Number of clients last day	13.76	(1.86)	14.55	(3.65)	0.86	152
Total number of workers, including owner	1.64	(0.06)	1.48	(0.13)	0.37	159
Weekly hours worked by employees	11.85	(2.86)	7.32	(3.21)	0.28	164
Age of business (years)	6.68	(0.66)	6.94	(1.63)	0.86	164
Replacement value of business capital	7,441.43	(1,310.72)	9,228.68	(1,819.19)	0.45	164
Registered with at least one gov't agency	0.20	(0.04)	0.15	(0.05)	0.50	160

Notes: Sample includes all women assigned to treatment who did not attrite post-intervention. Asymptotic robust (s.e.) clustered at the village level. All monetary variable are measured in Mexican Pesos (~13 pesos / 1 U.S. dollar). Reservation wage is the minimum stated monthly wage a women would accept in order to quit her business.

Appendix Table 2: Pre-treatment characteristics of entrepreneurs, by attrition status

	Ever attrited		Never attrited		(1)=(3)	
	Mean	(s.e.)	Mean	(s.e.)	p-value	N
	(1)	(2)	(3)	(4)	(5)	(6)
Personal Characteristics						
Age	44.89	(1.04)	46.04	(0.44)	0.29	869
Years of education	6.33	(0.21)	5.95	(0.14)	0.09	846
Roof is made of temporary material	0.28	(0.05)	0.33	(0.06)	0.23	844
Score on math exercise (percent correct)	0.43	(0.03)	0.46	(0.03)	0.37	864
Keeps formal business accounts	0.03	(0.01)	0.03	(0.01)	0.99	873
Weekly hours worked in enterprise	42.34	(2.42)	38.14	(1.47)	0.07	866
Reservation wage, monthly	3,076.29	(215.29)	2,942.19	(146.24)	0.62	696
Maximum loan available if needed	7,316.22	(1,004.10)	9,559.86	(2,112.11)	0.31	689
Monthly interest rate on a potential loan	6.66	(0.31)	6.10	(0.37)	0.21	506
Business Characteristics						
Produces goods for sale	0.62	(0.04)	0.68	(0.03)	0.02	875
Last day's profit	123.16	(11.78)	160.35	(23.28)	0.09	760
Last day's revenue	347.61	(20.98)	439.45	(38.20)	0.05	840
Number of clients last day	14.18	(1.21)	14.42	(1.05)	0.82	808
Total number of workers, including owner	1.56	(0.05)	1.68	(0.04)	0.09	864
Weekly hours worked by employees	10.35	(1.24)	10.48	(1.13)	0.94	872
Age of business (years)	6.55	(0.70)	7.79	(0.70)	0.17	874
Replacement value of business capital	7,298.10	(1,066.35)	9,628.18	(1,163.03)	0.15	875
Registered with at least one gov't agency	0.23	(0.03)	0.25	(0.03)	0.49	847

Notes: Sample includes all subjects interviewed in the baseline survey. A subject "ever attrited" if they were not surveyed in either the first or second post-treatment survey. Asymptotic robust (s.e.) clustered at the village level. All monetary variable are measured in Mexican Pesos (~13 pesos / 1 U.S. dollar). Reservation wage is the minimum stated monthly wage a women would accept in order to quit her business.

Appendix Table 3: Pre-treatment characteristics of entrepreneurs, by quitting status

	Ever quit		Did not quit		(1)=(3)	
	Mean	(s.e.)	Mean	(s.e.)	p-value	N
	(1)	(2)	(3)	(4)	(5)	(6)
Personal Characteristics						
Age	44.31	(0.58)	47.39	(0.68)	0.00	822
Years of education	6.22	(0.16)	5.85	(0.20)	0.12	799
Roof is made of temporary material	0.38	(0.06)	0.26	(0.05)	0.00	797
Score on math exercise (percent correct)	0.45	(0.03)	0.46	(0.03)	0.62	816
Keeps formal business accounts	0.02	(0.01)	0.04	(0.01)	0.10	825
Weekly hours worked in enterprise	35.91	(2.07)	42.18	(1.41)	0.01	818
Reservation wage, monthly	2,674.93	(137.15)	3,219.42	(188.06)	0.02	656
Maximum loan available if needed	8,883.73	(3,032.90)	9,378.14	(1,152.20)	0.88	651
Monthly interest rate on a potential loan	6.19	(0.45)	6.21	(0.35)	0.98	479
Business Characteristics						
Produces goods for sale	0.70	(0.04)	0.64	(0.03)	0.10	827
Last day's profit	126.03	(12.11)	174.10	(36.18)	0.23	722
Last day's revenue	378.42	(42.16)	456.31	(45.64)	0.25	793
Number of clients last day	14.12	(1.41)	14.70	(1.15)	0.72	763
Total number of workers, including owner	1.57	(0.03)	1.75	(0.06)	0.02	816
Weekly hours worked by employees	9.22	(0.97)	12.45	(1.39)	0.05	824
Age of business (years)	6.30	(0.71)	8.77	(0.71)	0.00	826
Replacement value of business capital	7,875.72	(1,113.79)	10,825.34	(1,137.04)	0.02	827
Registered with at least one gov't agency	0.18	(0.03)	0.33	(0.03)	0.00	800

Notes: Sample includes all subjects interviewed in the baseline survey that did not attrite. A subject "ever quit" if they were not running their business in either the first or second post-treatment survey. Asymptotic robust (s.e.) clustered at the village level. All monetary variable are measured in Mexican Pesos (~13 pesos / 1 U.S. dollar). Reservation wage is the minimum stated monthly wage a women would accept in order to quit her business.

Appendix Table 4: Lee's bounds on the Intention to Treat Effects

	Lower Lee bound on ITT	p-value, Wild bootstrap	Obs.	Upper Lee bound on ITT	p-value, Wild bootstrap	Obs.
Outcome:	(1)	(2)	(3)	(4)	(5)	(6)
Main business outcomes						
ln(Last day's profit)	0.154	(0.242)	1,178	0.340	(0.010)	1,177
ln(Last day's revenue)	0.165	(0.180)	1,349	0.359	(0.012)	1,350
ln(# clients last day)	0.145	(0.300)	1,305	0.337	(0.016)	1,301
Standardized	0.125	(0.096)	1,122	0.207	(0.010)	1,122
Outcomes calculated from good-specific data						
ln(Last day's profit)	0.104	(0.702)	832	0.313	(0.162)	831
ln(Last day's revenue)	0.143	(0.310)	1,067	0.334	(0.116)	1,067
ln(# goods for sale)	0.024	(0.716)	1,415	0.432	(0.008)	1,380
ln(Mean unit cost)	-0.361	(0.028)	976	-0.205	(0.192)	976
ln(Mean unit price)	-0.060	(0.284)	1,400	0.067	(0.300)	1,400
Other business outcomes						
Percent correct on business practices exercise	0.017	(0.792)	1,197	0.157	(0.052)	1,180
Uses formal accounting methods	-0.025	(0.078)	1,419	0.940	(0.004)	1,266
Number of employees	0.001	(0.974)	1,411	0.964	(0.026)	1,308
Hours worked per week by owner	-1.283	(0.640)	1,396	3.705	(0.232)	1,403
Hours worked per week by employees	-3.558	(0.406)	1,138	14.362	(0.078)	1,081
Registered with government agency	-0.144	(0.006)	1,349	0.626	(0.000)	1,276
Quit her business	-0.321	(0.002)	1,734	0.585	(0.000)	1,661

Notes: Sample excludes subjects who were offered treatment in treatment villages. Lower and upper bounds are calculated by first using Lee's methodology to trim each post-intervention period independently, and then estimating our difference in difference model with this trimmed data and the full pre-intervention sample. Covariates included (see text). Wild bootstrap p-values allow for intra-village (cluster) correlation.