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

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Abstract

A large share of the poor in developing countries run small enterprises, often earning low incomes. This paper explores whether the poor performance of businesses can be explained by a lack of basic business skills. We randomized the offer of a free, 48-hour business skills course to a broad group of female entrepreneurs in rural Mexico. We find that those assigned to treatment earn higher profits, have larger revenues, serve a greater number of clients, are more likely to use formal accounting techniques, and are more likely to be registered with the government. These effects last in both the short and medium run, and are larger for firms with higher profits pre-treatment. Indirect treatment effects on those entrepreneurs randomized out of the program, yet living in treatment villages, are economically meaningful but imprecisely measured. We present a simple model of experience and learning that helps interpret our results. 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 entrepreneurial quality.

JEL: C93, I25, O12, O14
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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 it is often believed that encouraging the development of small businesses will lead to job creation and economic growth (World Bank, 2013). As such, many governments and non-governmental organizations promote the use of business training programs to help grow small businesses. For example, the Start and Improve Your Business Programme - a leading business literacy program - has been introduced in more than 100 countries and has reached more than 4.5 million potential and existing entrepreneurs between 2003 and 2010 (ILO, 2012).

However, a recent review of the literature shows considerable heterogeneity in the effectiveness of business training programs (McKenzie and Woodruff, 2012). One interpretation of this heterogeneity is that not all entrepreneurs have the ability to increase their profits, let alone grow their small businesses into engines of economy-wide growth. The natural implication of this is that subsidies and training should be targeting those with the highest potential for growth.

In this paper, we study the impacts of a business literacy program targeted at the general population of poor female entrepreneurs in villages in rural Mexico. Our first goal is to assess the impact of the intervention on the population as a whole, taking as a sample frame all entrepreneurs who sell goods, either self-produced or as re-sale. We then look at the heterogeneous effects of the treatment as a function of the entrepreneur's pre-treatment ability and the intensity of the treatment within an entrepreneur's village-sector. Our sample contains about 900 small firms engaged in the production and sale of food, craft items, and other consumer goods in small shops. We offered a random subset of these entrepreneurs a free, 48-hour business training course focusing on the practical application of simple business concepts - concepts such as basic accounting, identifying unit costs, the importance of recording sales, and pricing to maximize profits.

Women who were offered the training have larger profits, revenues, and household income, and they also serve more clients. Treatment induced an increase in the use of formal accounting techniques and an increase in the likelihood of formally registering with the government, which requires paying taxes but also allows firms to issue legal bills of sale. Treated firms were also able to reduce their costs and change the mix of products they sold: specifically, they increased the number of items sold, dropping higher cost goods and adding lower cost ones. Importantly, we collected several distinct measures of business outcomes, such as profits and revenues: that treatment effects are consistent across these measures suggests that estimated impacts are not simply arising because treatment induced more precise accounting of self-assessed profits and revenues. Furthermore, we collected two rounds

of post-intervention data, one year and two and a half years post-program implementation, and find that the effects of the treatment do not fade out into the medium run.

One of the contribution of our study is that the randomization was conducted at both the village and intra-village levels, which allows us to study spillover effects on non-invitees in program villages (Miguel and Kremer, 2004; Angelucci and De Giorgi, 2009). It is ex-ante unclear whether the indirect treatment effects on business outcomes 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). Regardless, it is clear that the presence or absence of indirect treatment effects have important implications for policy.

In terms of profits, we find negative, although not statistically significant, spillover effects of the treatment, which seem to arise from an increase in costs (and not from a fall in revenues). This result, together with the finding that treated firms use lower cost inputs, suggests that the control and treated women purchase their inputs from different suppliers (which are more costly for the control group in treatment villages) or that suppliers have latitude to set differentiated prices.

Combining the direct and indirect effects, back-of-the-envelope calculations suggest that the intervention is extremely cost effective. Given the large return to this training, it is conceivable that firms would demand such a product. However, the lack of information by the rural entrepreneurs and credit constraints may well be important enough barriers so that business education to micro-enterprise is not provided by the market.

Finally, in order to better understand the heterogeneous impacts of treatment, we develop a simple conceptual framework. We think of our typical entrepreneur as an experimenter with a noisy signal of her productivity who faces the outside option of quitting her business. We also conjecture that entrepreneurs are overconfident about their own ability Burks et al. (2009) and if given the chance would be likely to try out new technologies. The offer of business classes lowers the cost of (or introduces) a new and 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 expensive technology. However, in our model the technology is risky, entrepreneurs are heterogeneous in their ability (or productivity), the technology is only profitable for those with high ability, and ability is only partially observable to the entrepreneur who has potentially an upward biased belief of her type. Through the adoption of the new technology including the accounting techniques, irrespective of the outcome, the entrepreneur learns her own productivity which informs her decision about whether to

continue running the business, and with which technology.

This model offers two testable implications: first, amongst treated entrepreneurs, the probability of quitting one's business should be higher for those with lower ability, and second, the effect of the treatment on profits should be larger for those with greater ability. Bringing these predictions to the data, and proxying for ability with the level of pre-treatment profits, we find some support for the fact that that low ability entrepreneurs are indeed more likely to quit their businesses as a result of the training, and strong evidence that the largest positive effects are recorded amongst the "best" entrepreneurs.

This 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 (2014) in the Dominican Republic, Berge, Bjorvatn and Tungodden (2011) in Tanzania, Bruhn and Zia (2013) in Bosnia-Herzegovina, and Giné and Mansuri (2014) in Pakistan, de Mel, McKenzie and Woodruff (2014) in Sri Lanka, and Fairlie, Karlan and Zinman (2012) in the United States.

At the same time, our intervention is distinct from this literature in several important dimensions. First, the pedagogy focuses on the practical application of the skills and topics in the entrepreneurs' own businesses. Second, compared with other training programs (McKenzie and Woodruff, 2012), the course is relatively long and intensive, with a total of 48 hours of classes over 6 weeks. 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 last feature increases the external validity of our results, and distinguishes them from other studies in this literature (e.g., Field, Jayachandran and Pande (2010), Karlan and Valdivia (2011), Drexler, Fischer and Schoar (2014)).[2]

This paper also relates to the work of Nyshadham (2014), who provides theoretical arguments on the effects of business literacy training on entrepreneurial decision making, and to the growing literature on the effects of management services in developing countries (Bloom et al. (2013); Bruhn, Karlan and Schoar (2013); Karlan, Knight and Udry (2012)).

Finally, we contribute to the understanding of the frictions that may constrain growth in emerging economies. According to the McKinsey Global Institute (2014) the share of employment in small firms (less than 10 employees) has gone from 32 to 42% between 1999 and 2009 while the productivity of these firms has gone down by 6.5 percentage points. It is possible that somewhat excessive self-

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

[2] Indeed, de Mel, McKenzie and Woodruff (2014) find substantial complementarities between business training and the availability of credit amongst female entrepreneurs in Sri Lanka.

confidence of one's ability for entrepreneurship (coupled with the wider availability of credit through microfinance) is an important friction explaining declining productivity and lack of business growth amongst small firms.

The remainder of the paper proceeds as follows: Section 2 describes the business literacy training and our experimental design, Section 3 describes the data and the sample, Section 4 presents the empirical methodology, Section 5 presents the results, Section 6 provides the simple theoretical framework for the interpretation of the results, and Section 7 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.[3] CREA operates in small villages in the Mexican state of Zacatecas, a high-altitude, dry, and poor agricultural region. While there is good road access to all villages in which CREA operates, the inhabitants are nonetheless isolated in most of their daily activities as villages are geographically separated by farms and arid land.

The training program consists of two four-hour classroom meetings per week and runs for six weeks - a total classroom time of 48 hours. The classes are designed to be small and inclusive, with two instructors and a class size of no more than 25 entrepreneurs; all instructors are experienced local university professors or university students (graduate and undergraduate). 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.

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 benefits of formally registering a business with the government. The fourth module covers general business organization and

[3] CREA excluded entrepreneurs in the service sector in an effort to focus the training context.

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 are intentionally simplified in order to be understandable to the population at hand, the majority of whom have low levels of formal education. 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, (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.[4]

2.2 Experimental design and population of study

Our experimental design contains two stages. In the first, villages were randomized into either treatment or control, and in the second, entrepreneurs within treatment villages were randomized to receive or not receive an invitation to attend the classes.[5] 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 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 sampling framework included all villages in the state of Zacatecas that met three criteria: that they (i) had between 100 and 500 female entrepreneurs, as identified by the 2005 Mexican census, (ii) are within a two hour drive from the City of Zacatecas, and (iii) had fewer than 1500 households (also identified by the 2005 Mexican census).[6] This selection process identified 25 villages. 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.

[4] An in-class example and exercise can be seen in Appendix Figure 1.

[5] Our randomization algorithm involved first choosing a "seed" group of potential treatment villages, and then choosing 50 percent of entrepreneurs in each seed village as potential treated individuals. We repeated this assignment 10,000 times as to minimize the (squared) sum of the distances of predicted profits between treated and control units. The approach we use is that of running a baseline regression of profits over a set of conditioning variables (number of workers, 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) which we then include in our analysis as controls.

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

Within chosen villages, we identified female entrepreneurs that produced and/or sold goods with a modified snowball sampling technique as follows: First, we contacted the elected village leader (the *comisario* or *presidente municipal*, a mayor-like position) and asked him/her to introduce us to at least three knowledgable local women (the "seeds"). Second, we asked this group to list all of the women in the village that (i) work for themselves and (ii) sell a good. None of the local seed women were entrepreneurs themselves, and enumerators emphasized to the seed women the importance of identifying as close to a census of women entrepreneurs as possible. This process yielded about 50 entrepreneurs per village, to whom we applied a pre-intervention questionnaire between July and September of 2009.[7]

We did not have the resources to survey male entrepreneurs, which limits our ability to estimate the full indirect effects of treatment (spillover 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, in which case we would indeed be capturing the entire market. 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 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, and attendance was recorded by the teachers.

2.3 Expected effects of the intervention

To fix ideas, we briefly describe the potential effects of this intervention and how they motivated our experimental design. Classes should inform women about how to properly run a small business. Importantly, this information may 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

[7] The difference between the 100 to 500 entrepreneurs identified in the census and the number of entrepreneurs identified in our sample is explained by the fact that we do not include farmers who did not retail their produce (the vast majority) nor those in the service sector.

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 discussed and potentially 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 both the treated and the non-treated enterprises in a treated market depending maybe on the number of treated competing firms. The experimental design we describe next was designed to capture such effects.

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 (McKenzie, 2012). 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 and interviewers did not know entrepreneurs' treatment status nor program participation decisions.

Our main measures of business performance include self-reports of profits, revenues, and the number of clients served reported for various time stretches. As some of the women do not work/sell everyday or at regular interval it seems appropriate to have several measures of business performance over different intervals. Furthermore, having several measures of profits and revenues allows us to combine them into standardized measures which likely have less measurement error. Importantly we also collected and analyze self-reports of total household income at baseline and in the first post-treatment wave, there are at least two main advantages in collecting household income in a study like our: firs, such measure doesn't suffer from firms' attrition (and closure) as it is recorded for the household rather than the firm; second, household income is potentially the relevant measure also in terms of overall household "welfare" in the absence of consumption measures.[8]

While evidence from other developing countries suggests that self-reported measures of aggregate

[8]Field, Pande and Rigol (2014) show the importance of collecting household level information on outcomes.

business activity are as accurate as formal accounting figures (de Mel, McKenzie and Woodruff, 2009), we nonetheless also collected data on the individual goods sold in the enterprise at baseline and in the first followup. We first asked the entrepreneur to list all of the goods that she sold (up to a maximum of 14).[9] We then asked for each good the number of units sold on the last day worked, the unit price, and the unit cost.

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 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 also decide to sell a new product with a larger profit margin. (They are also useful to help assuage concerns that the treatment simply teaches entrepreneurs to more accurately report their profits and costs, as we outline in the results section below).

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 revenues, profit, and mean unit cost 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). These outcomes help us understanding what changed in the daily operations of the treated businesses, and therefore allow us to look into some of the mechanisms.

Several other outcomes will give us more 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, the average number of hours worked per week by the owner, and whether the entrepreneur is registered with the government agency in charge of collecting taxes and regulating business activity, the Secretary of Finance and Public Credit (*Secretaria de Hacienda y Credito Publico, SHCP*). We administered a simple exercise related to production and sales (see Appendix Figure 2) in order to directly examine the effect of the treatment on business-math knowledge; this same exercise was applied both pre- and post-treatment. Each of the four sections are scored correct or not, and we sum to create a total score.

[9] 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.

Furthermore, we asked the entrepreneurs how they kept accounts for their business, whether through personal notes, a formal accounting method, or whether they did not keep any accounts.

Additional pre-treatment data includes: the owner's age, education, and asset ownership (e.g. type of dwellings and number of rooms), a measure of 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; and the replacement value of the firm's capital stock. Finally, in both post-intervention surveys, we elicited firm's survival by asking if the entrepreneur still sells any goods, and we define a firm as quitting accordingly.

3.2 Sample and summary statistics

Our 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; handicrafts and clothing sum-up to about 10 percent.

Table 1 contains pre-treatment summary statistics by village type and treatment group. The randomization appears to have been successful in that the mean pre-intervention characteristics are for the most part indistinguishable across groups: five comparisons out of 48 are significantly different at the 10 percent level, while two out of 48 differ at the five percent level.

This data paints a sobering picture of the economic lives of these entrepreneurs. Daily profits average around 140 pesos (approximately $11 USD).[10] 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).

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 their capital stock (the replacement value of business capital) is about $570. 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 and in

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

particular 25% of the firms are less than 1 year old with a median age of 4 years.

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.[11] Less than five percent of entrepreneurs keep formal business accounts, and only about one fifth of the sample is registered with the government.

3.3 Take-up of classes

Classes were offered to the selected invitees by a CREA staff member who visited 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 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 no variables are significantly different across groups at the five percent level. However, despite this lack of significant differences (partly driven by the small sample size), attendees appear on average to be less successful entrepreneurs than non-attendees. For example, daily profits and revenues are about 50 percent higher for entrepreneurs who did not attend classes. Again, such findings are consistent with the literature (see, for example, Drexler, Fischer and Schoar (2014) and de Mel, McKenzie and Woodruff (2014)).

The effect of treatment (being offered the class) on the treated (class attendees) can be estimated by instrumenting attendance status (which is presumably endogenous) with treatment status (which is exogenous due to randomization). However, we instead focus our empirical analysis on the Intent to Treat parameter for parsimony. In general, one can easily scale-up the Intent to Treat parameter to the (Local) Average Treatment Effect on the Treated by inflating the parameters presented below by the inverse of the probability of taking-up the treatment (0.65), or a factor of about 1.53 (=1/0.65).

3.4 Attrition

Some entrepreneurs attrited from our sample between the baseline and the first and second followup surveys; however, attrition rates do not vary significantly across treatment groups (on average).

[11] 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.

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 of the difference = 0.58). During the second followup survey, we were able to survey some of the attrited entrepreneurs from the first followup, 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.[12]

4. Empirical Strategy

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

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

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, $\mathbf{X}$ 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.[13]

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 (this identification strategy is immune from within-village spillover effects). 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 differ-

[12] Comparing entrepreneurs who ever attrited with those who did not reveals that, pre-intervention, attrited entrepreneurs have less education, have significantly lower revenues, employ fewer workers, 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).

[13] 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.

ent treatment effects over time. However, as shown below, estimated treatment effects do not differ significantly across the two post-intervention survey waves; therefore, we pool the post-intervention surveys together in order to increase statistical power and include an indicator for the first post-intervention period (*Wave*2) 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 hypothesis testing may be incorrect. We therefore use the Donald and Lang (2007) adjustment for all tests of statistical significance, which entails calculating p-values from tests with degrees of freedom equal to the number of clusters minus the number of group constant variables (in our case this is $17-2=15$ degrees of freedom).[14]

5. Results

5.1 The direct effect of business training

We first explore the effect of business literacy classes on firm survival. Column 1 of Table 2 shows the ITT on quitting one's business is an insignificant 1.6 percentage points, suggesting the offer of classes did not differentially induce entrepreneurs to quit their business (on average). Quit rates overall (in both treatment and control groups), however, were non-negligible: by the first and second followup surveys, 18.6 and 41.1 percent of the sample had stopped running their business, respectively.[15] Firm survival rates of this magnitude are typical for small businesses, but pose problems in longitudinal studies of entrepreneurs as business related outcomes are unobservable for those who quit, we note however that household income is immune from such an issue.[16] To account for potential selective quitting (and attrition) by treatment status, we estimate bounds on the treatment effects in Columns 2 and 3 of Table 2, using Lee's methodology (Lee, 2009) for continuous variables and Manski's methodology (Manski, 1990) for binary outcomes.

We next explore the effect of treatment on profits and revenues. All of the available measures of profit and revenue - from the last day, the last week, and the aggregate calculated from good specific last day and last month values - are self-reported and thus may be measured with error. As such, we normalize all individual measures of profit and revenue and calculate the mean standardized profit and the mean standardized revenue for each business (Kling, Liebman and Katz, 2007), we note that the use

[14]For a discussion on inferential problems with a small number of clusters see Wooldrige (2003), and Cameron and Miller (2015). We reach similar conclusions on the statistical significance of our results using the wild bootstrap method of Cameron, Gelbach and Miller (2008).

[15]Perhaps not surprisingly, there are significant differences between those who ever quit and those who did not (see Appendix Table 3); for example, compared to non-quitters, quitters were younger in age, worked fewer hours in their business, had fewer employees, and had been in business for less time. These relationships hold equally in both the treatment and control groups.

[16]For example, the five year survival rate for small businesses of similar age to our sample in the U.S. and other OECD countries is about 50-70% (Bartelsman, Scarpetta and Schivardi (2003) and U.S. Small Business Administration (2012))

of standardized measures of profits also addresses concerns due to multiple hypothesis testing across a series of outcomes (Romano and Wolf, 2005). For parsimony, we only present in our main analysis results on (the logarithm of) last day's profit and revenue as well as the standardized profit and revenue measures; estimates using the other available measures as outcomes are of similar magnitude and are included in Appendix Table 4.

Column 1 of Table 2 shows that the direct effect of the offer of classes is 22 log points (p-value < 0.1) on last day's profit and .209 standard deviations (p-value < 0.05) on standardized profit. The $ITTs$ on revenues are of a similar magnitude to profits: last day's revenue increased by 25.3 log points (p-value < 0.05) and standardized revenue increased by 0.209 standard deviations (p-value < 0.01). These effects are quite large, yet comparable to the impact of other business literacy courses in the literature (McKenzie and Woodruff (2012); Bruhn, Karlan and Schoar (2013)).

Importantly, given our concerns on attrition and quitting, we also present in Table 2 Lee's and Manski's bounds, which are in general quite conservative. We note that the lower bounds on all four of these profit and revenue measures are positive with fairly large magnitudes. Standard errors for last day's profit and revenue are large, but the lower bound on standardized profit and revenue are more precisely estimated and are significantly different from zero at standard confidence levels. The estimated bounds give us confidence that selective attrition and quitting are not likely to be driving these results.

The remainder of Table 2 presents a complementary set of outcomes which help us understand the mechanisms through which profits and revenues were impacted by the program. We first find a positive and significant effect on the number of clients in the last working day (22.4 log points or about four extra clients) and the number of goods for sale (20 log points, or approximately two extra goods for sale). We find this result rather important as it is bound to be measured with less error than profits and revenues, but also because it suggests that part of the overall effects on profits and revenues arises from larger quantities sold.

Household income - which was collected from all subjects regardless of whether they still ran their business and therefore not subject to missing information when the business closes down - increased significantly by 19.4 log points under the program. (Recall that household income was only collected in survey waves 1 and 2.) The similar magnitude of the increase in profits and household income suggests that the program did not have a meaningful income effect on overall household labor supply (for example, spouses or children of entrepreneurs do not appear to reduce their labor supply in response to the increase in entrepreneurial profits).

It appears that the observed increase in profits is being driven by reduced costs and increased

quantities sold rather than increased prices: unit costs fall by 27 log points (p-value $<.1$) and unit prices were not meaningfully impacted. Firms are also not changing the number of employees nor the hours worked by either the owner or employees.

Finally, we see evidence that entrepreneurs learned from the training: There is a 4.8 percentage points increase on the use of formal accounting (p-value $<.1$ and an increase of over 100% from baseline); a positive, but insignificant, effect on the share of correct answers in our simple business exercise; and an 8.6 percentage point increase in business registration with the government (p-value $<.01$, and a 40 percent increase over baseline). The CREA course included a thorough discussion of the pros and cons of registering ones business and it appears that upon learning this information registration was an optimal decision for some entrepreneurs.

While we focus mainly on the pooled effect of the treatment across survey waves, $ITTs$ are very similar magnitude in both the short run (one year post-intervention) and the medium run (two and a half years after the intervention). This can be seen in Table 4 which 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. (Note that this table only includes variables which we observed in both post-treatment waves). In general, point estimates for $ITTs$ in wave 3 have larger standard errors than those in wave 2 (in part because of the smaller sample size in wave 3), but we can not reject the hypothesis that the $ITTs$ are equal across waves. This finding is important in that the one time intervention appears to have long lasting positive effects which do not seem to decay two and a half years after the classroom training took place.

5.1.1 Robustness and validity of the main results

These treatment effects are robust to alternative specifications, as shown in Table 3. First, column 1 shows that excluding pre-program covariates does not change point estimates meaningfully, but increases standard errors as expected.

Second, we further explore the concern that differential quitting or attrition by treatment status is biasing our results. One important piece of evidence suggesting that our results are not being driven by selective observability of business outcomes is that our measure of household income is observable for *all* entrepreneurs, regardless of quit status, and we see in Table 2 that household income increased with a similar magnitude as did profits. Furthermore, we present results in column 2 of Table 3 which invoke a very strong assumption that can help bound estimates from below: that firms that quit or attrited had zero profits and revenues, served no clients, worked no hours, did not use formal accounting methods, and were not registered with the government. Applying this assumption (and using 0.1 clients and 1

peso in profits and revenues in logarithmic specifications), we see that treatment effects are of similar magnitude and less precisely estimated, but still suggest that differential quitting or attrition is not driving the results. For example, $ITTs$ on standardized profit and revenue (those measured with the least error) are still marginally distinguishable from zero with p-values of 0.103 and 0.07, respectively.

Thirdly, if we assume that spillover effects to control firms in treatment villages are non-existent and we include control firms in treatment villages in the analysis, therefore increasing the power of our design, we confirm our main results with slightly larger magnitudes and more precision given the substantial increase in sample size. As we believe this to be a questionable approach we just present those results for completeness in the robustness table but otherwise exclude control firms in treatment villages from the core analysis.

Finally, a consistency check of our estimated effects is possible given that we measure profits, revenues, unit prices, and unit costs. As profits and revenues increased by about 20% we expect aggregate costs to increase by roughly the same amount so that the ratios between revenues for the treated over the control group is equal to the ratio of aggregate costs for the two groups, i.e. $\frac{R^T}{R^C} \approx \frac{C^T}{C^C}$. This is because the increase in profits is mostly due to an increase in the quantities sold: One way to see this is that the number of clients served increases by roughly the same amount as profits. On the other hand, unit prices do not seem to change as a result of treatment, which suggests a scale effect is at play rather than a price effect. The only inconsistent result is that the ratio of unit costs between treated and control businesses should be close to 1: our estimated ratio of costs is about 0.73. However, as unit costs and prices are computed from the good-by-good analysis, and only for waves 1 and 2, we feel this result is plausible especially given that we cannot statistically reject the hypothesis that the unit cost ratio is indeed equal to 1.[17]

Are classes simply teaching entrepreneurs to more accurately report business outcomes?

One concern is that the intervention taught entrepreneurs to accurately measure business outcomes, but did not change actual outcomes themselves. Three pieces of evidence suggest this is not the case.

First, It is unlikely the treatment impacted how entrepreneurs measure revenue, the number of clients served, or the number of products sold. Second, we elicit unit costs directly in the good-by-good analysis, and use this to calculate aggregate profits. This measure is immune to mis-labeling of household expenses as business costs, which would tend to make business profits artificially low. Indeed, Appendix Table 4 shows that treatment effects on the good specific profits are of similar magnitude to those on reported overall profits, suggesting that there are real program effects on profits.

[17] We thank Luigi Guiso for pointing out this additional consistency check.

Third, the good-by-good analysis provides a set of alternative measures of business performance which help alleviate concerns that non-classical measurement error, or systematic downward bias in reporting, is driving our results. Having two classes of measures for business profits and revenues - one self-reported and one 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 the classes. Specifically, we cannot reject the equality of the correlations in the two measures, at the individual level, 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 treatment effects we find.[18]

5.2 Changes in the composition of goods for sale

We next explore further the mechanisms leading to the estimated effects, in terms of daily business operations. We show that the treatment affects the selection of goods offered by our entrepreneurs. The CREA training discussed how a business owner can increase profits by dropping goods that have negative profit margins and adding goods with positive margins. Using our good-level data, we estimate equation 1 amongst three distinct sets of goods: those that were (i) dropped between the baseline and first post-intervention survey, (ii) kept across both surveys, or (iii) added in the first post-intervention survey (we do not have good-level data in the second post-intervention survey). The $ITTs$ for selected outcomes are presented in Table 5.

As splitting the sample in this manner reduces sample sizes significantly, standard errors of the treatment effects are large. Regardless, we see that entrepreneurs who were 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. To conclude, despite the low power of these tests, there does appear to be suggestive evidence that the business literacy classes induced entrepreneurs to change their product mix in a manner consistent with profit maximization and with the training program.

5.3 Heterogeneity of the Effects on Business Outcomes

Intensity of the treatment within sector

In small, isolated villages, the effect of the treatment may be a function of how many direct competitors attend the business training course. For example, it is ex-ante hard to predict whether having more entrepreneurs in a given sector invited and potentially attend the classes would result in worse or better business outcomes on average for the treated. One can think of a simple story where hav-

[18] We thank Rema Hanna for suggesting this testing strategy.

ing multiple entrepreneurs in class would foster cooperation in those specific sectors, push owners to work harder, and lead to complementary effects on all businesses in the sector. At the same time, treatment may lead to greater competition with some firms improving their business performance at the expense of others: the average effect in a village-sector may be zero. While we can not sign the impact theoretically, we can use the exogenous variation in the number of firms in any given sector in treatment villages to document these effects empirically. We define a measure of treatment intensity as the number of treated businesses in a village-sector (there are $7 * 5 = 35$ village-sectors), and this measure varies between 1 and 21, with mean of 7 and median of 4.

Table 6 presents estimates of a version of equation 1 in which we include the intensity measure and its interaction with $T * Post$. On average, one extra treated business in one's sector significantly increases standardized profits and revenues by .03 of a standard deviation.[19] The number of clients served, per firm, falls significantly as the number of treated firms in a village-sector increases (4.5pp per extra treated firm). At the same time it seems that employees are working longer hours per week as the number of treated firms increases.[20]

Heterogeneity by pre-treatment profits

We next explore the hypothesis that that the effect of business literacy training is stronger for better performing enterprises at baseline (or higher ability entrepreneurs). This set of tests is the empirical counterpart of the simple model of entrepreneurship we discussed earlier in Section 2.3 and further detail more formally in Section 6. For ease of presentation, we split our sample into those above and below the median of the standardized pre-treatment profits, and present in Table 7 separate $ITTs$ estimates for above versus below the median of the baseline variable running equation 1. Comparing $ITTs$ in the samples above and below the median of pre-treatment standardized profits, we see quite striking differences: by-and-large the positive effects of the intervention consistently arise from those above the median of pre-treatment standardized profits, which can be seen as a proxy of entrepreneurial quality.

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 standardized profits is 0.236 (significant at the 5 percent level)

[19] It is unlikely that these results are reflecting the comparison of larger to smaller villages - which might simply differ in the size of the (potential) demand - as study villages are of a similar size in terms of population and we fixed the total number of treated in treatment villages ex-ante.

[20] We repeat a similar exercise for the spillover effects, but we find no significant interaction between the number of treated firms in a given village/sector and business outcomes for control firms in treatment villages.

for those above the median and 0.057 for those below the median (and largely insignificant). The difference between the two estimated parameters of 0.179 - while clearly economically non-negligible - is marginally significant with a p-value of 0.132. A similar story is present for our various measures of revenues and the number of clients served: good businesses benefit from the intervention more than bad businesses.

We also see that the positive treatment effect on the use of formal accounting practices is larger amongst the most able entrepreneurs: the ITT for those above the median of pre-treatment profits is 0.068 (significant at the 10 percent level) compared to an insignificant 0.042 for those below the median. These point estimates suggest that both high and (to a lesser extent) low quality entrepreneurs seem to adopt part of the new technology, but only high quality entrepreneurs are successful consistently with the theoretical discussion. There is also a small differential in terms of knowledge gains as measured by our business practices exercise.

One striking observation is the large and significant differential effect in terms of hours worked per week by the owner of almost 5 hours (or 13 percent over the baseline). We find an even larger 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 (with the difference of almost 12 hours significant at the 5 percent level). These effects on hours worked by employees seem not to be driven by differential changes in the number of employees. We also find a small differential effect in terms of registering with the government.

Conscious of the fact that treatment effects are by-and-large not statistically distinguishable between those with above and below the median of pre-treatment profits, we believe that the economically large differences in point estimates for many business-related measures is suggestive of the fact that only higher ability entrepreneurs benefit from this type of interventions.

5.4 Spillovers of business literacy classes

Our final analysis explores the Indirect Treatment Effects (ITE) which are estimated by equation 1 on the sample that excludes any entrepreneurs who were invited to the classes. To the extent that villages represent segmented markets, these estimates identify the local spillover (and general equilibrium) effects of the intervention.

Table 8 shows there is no evidence of a significant ITE on quitting one's business. Furthermore, it is clear that very few of the $ITEs$ on business related outcomes are significantly different from zero. 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 12 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 2), and suggests the overall effect of the program on the profits of female entrepreneurs in treatment villages is about 12 percent. $ITEs$ on our measures of revenues are generally positive, yet imprecise.

A few other estimates are of note. The ITE on unit cost is large – 14.5 log points – although not significant. One possible explanation for this positive indirect effect on costs is that factor markets are not perfectly competitive: those offered treatment may be now purchasing inputs from lower-cost suppliers, leaving those not offered the treatment to purchase inputs from higher-cost suppliers. The ITE on prices is also large – 10.6 log points – which could reflect a response to increased costs.

There is no evidence of spillover effects on demonstrated business knowledge (the score on the business practices exercise), but there is a large and significant impact on the use of formal accounting methods (a 5.7 percentage point increase). In light of the lack of strong evidence on improved business outcomes, it appears that this increased use of formal accounting methods was not as useful as for firms in the treatment group. Furthermore, unlike the direct effect, there is no indirect effect on the likelihood of being registered with the government. There is no indirect effect on the number of employees, but there owner of control firms in treatment villages work 3.9 hours per week more than in control village, an increase of about 10 percent over baseline. One possible explanation for the increase in hours worked is that firms are rationally responding to increased competition from treated firms.

6. A Simple Model of Entrepreneurial Experimentation and Business Literacy

To aid the interpretation of these findings we develop a basic model of entrepreneurial choices of managerial technology, where the entrepreneur has only partial information about her type and her ability to successfully adopt the new technology and scale up her business. This model is based on Karlan, Knight and Udry (2012) and captures two key components of our intervention: (i) accounting practices and (ii) "business" skills. At the same time we allow for the outside option of quitting one's business, distinct from Karlan, Knight and Udry (2012).

Entrepreneurs are assumed to maximize their lifetime consumption subject to the resource constraint in the following programming problem:

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

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

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

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, $f(x_i, \alpha_i)$, are a function of the management technology the entrepreneur uses, x_i, 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 (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, $\pi_i(x_h) - x_h > \pi_i(x_l) - x_l$ only for entrepreneurs of above a certain productivity type, say, α^h. If no management technology is chosen, the entrepreneur quits her business and incurs no cost, in which case $x_i = 0$ and she receives the outside option pay-out π_i^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.[21]

We assume that the entrepreneurs do not know their type with certainty ex-ante, but believe they are either of 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, with $\sum_{j=0,l,h} p_i^j = 1$. Choosing the new technology, however, will reveal (ex-post) own type to the entrepreneur as follows: if the more expensive management process succeeds, it returns π_i^h and the entrepreneur knows she is of type α^h or greater; if it returns π_i^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_i^0. Thus, experimentation informs the entrepreneur whether she is: (i) a "good"; (ii) a "bad"; or (iii) a "non" entrepreneur. Recall that all the subjects in our study are already entrepreneurs, so that their status quo is the us of the old technology in the model environment. Importantly, about a quarter of our entrepreneurs have been running their business for

[21] We assume that a non-empty set of entrepreneurs has sufficient initial wealth to experiment with the new technology if they so wish. Recall that there is no credit market available or alternatively that the technologies are not collateralizable.

less than one year, while about half of them have been in the current business for less than four years.

More formally, 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 technology 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 her productivity type being high, i.e. large p^h.[22]

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,

[22] A similar problem applies to the decision of remaining with the old technology, i.e., the decision to remain an entrepreneur.

low ability entrepreneurs are better off sticking to the old technology, and the lowest ability types are best off by quitting, as follows:

$$V(x^0, \alpha \leq \alpha^l) > V(x^l, \alpha \leq \alpha^l) > V(x^h, \alpha \leq \alpha^l)$$
$$V(x^0, \alpha > \alpha^h) < V(x^l, \alpha > \alpha^h) < V(x^h, \alpha > \alpha^h)$$
$$V(x^l, \alpha^l < \alpha \leq \alpha^h) > V(x^h, \alpha^l < \alpha \leq \alpha^h)$$
$$V(x^l, \alpha^l < \alpha \leq \alpha^h) > V(x^0, \alpha^l < \alpha \leq \alpha^h).$$

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. It is clear that the value functions are ordered according to the above inequalities, implying that an entrepreneur would quit her business if her type is in the leftmost portion of the horizontal axis (α), she would employ the old technology for intermediate values of her type (α), and she would employ the new technology in the right part of the graph.

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 more optimistic entrepreneurs to try the new technology relative to the control. This implies that the average difference between the treated and control groups in quit rates and profits cannot be signed ex-ante, as some of the treated are low ability types who are "trying out" the new technology. Thus, the average effect of the treatment (i.e., offering business literacy classes) is ambiguous on firm profits and quit rates, 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 is an empirical matter whether:

$$Pr(Quit|T=1) - Pr(Quit|T=0) \gtreqless 0$$
$$E(\pi|T=1) - E(\pi|T=0) \gtreqless 0,$$

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

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

$$E(\pi|T=1, \alpha > \alpha^h) - E(\pi|T=0, \alpha > \alpha^h) > 0. \quad (5)$$

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:

$$Pr(Quit|T=1, \alpha < \alpha^l) - Pr(Quit|T=0, \alpha < \alpha^l) > 0. \tag{6}$$

Testing these two predictions requires knowledge of α. As productivity and type are difficult to measure empirically, a potential proxy for productivity could be pre-treatment profits, π_0. Thus, the two testable implications of this model are that the intention to treat effect on quitting should be non-increasing in pre-treatment profits and the intention to treat effect on profits should be non-decreasing in pre-treatment profits:

$$\frac{\partial\{E(\pi|T=1) - E(\pi|T=0)\}}{\partial \pi_0} \geq 0 \tag{7}$$

$$\frac{\partial\{Pr(Quit|T=1) - Pr(Quit|T=0)\}}{\partial \pi_0} \leq 0. \tag{8}$$

The empirical support for the first hypothesis (Equation 7) was presented in Table 7, albeit - as discussed above - from tests with low power: the effect of treatment on profits, as well as other indicators of profitability, are larger amongst entrepreneurs with higher pre-treatment profits. We do not find empirical support for the second hypothesis in Table 7, in that the propensity to quit one's business in response to the treatment is not differential on average between those with higher and lower pre-treatment profits (a small differential in magnitude and strongly insignificant).

However, a closer look at the distribution of the propensity to quit one's business as a function of pre-treatment profits shows that the "excessive" experimenters are in fact located in the far left tail: it is precisely these entrepreneurs that have the lowest ability who are induced to quit their businesses. This can be seen in Figure 3 which presents - separately for treatment and control firms - the distributions of pre-treatment profits in the whole sample compared to the distribution of pre-treatment profits amongst those who did not quit by the second followup survey. It is clear that the survived sample (i.e., those who did not quit) is similar in terms of pre-treatment profits to the whole sample in the control group. In the treatment group, however, the distribution of the survived sample is significantly shifted to the right; this is consistent with the prediction that those with the lowest ability will be induced to quit upon learning they are in fact a low ability type. Kolmogorov-Smirnov tests for the equality of the distribution functions in Figure 3 yield p-values of 0.07 in the treatment group and 0.97 in the control group. We also test this prediction in a parametric framework (results available upon request). In the context of the model, the exercise we undertake amounts to searching for where α^l is located within

the distribution of baseline profits. We conduct a grid search over percentiles of the distribution of baseline profits by regressing an indicator for quitting (and attriting) by the second followup survey on a treatment indicator, an indicator for being a given percentile of the last day's profits pre-treatment, and the interaction of these two indicators. The interaction term is large and significant up to the 2nd percentile, and smaller in magnitude and insignificant - yet positive - up to the 5th percentile. Interaction terms for all percentiles greater than 5 are small in magnitude and insignificant. Thus, it appears α^l is around the 2nd percentile of pre-treatment profits.

A further test of the model's intuition on the effect of learning one's ability on quitting is that there is no indirect effect on quitting one's business, consistently with a learning story. If the mechanism at work were to be one of enhanced competition between firms in a given village, we should see higher exits in control firms in treatment villages than in control villages, and this does not appear to be the case empirically (see Table 2).

7. Conclusions

There is growing evidence suggesting that firms in developing countries are often run inefficiently. This paper focuses on whether a lack of entrepreneurial business skills is impeding business success, using data from a randomized controlled trial in Mexico that offered business literacy classes to poor women micro-entrepreneurs.

We find 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, a change in the composition of goods sold to higher profits ones, more clients served and quantities sold, and an increased use of formal accounting methods. Importantly, knowledge gained through the intervention does not appear to fade out overtime, as we observe positive effects persisting into the medium run.

We also find 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 point estimates seem to arise from input market imperfections, i.e. control firms in treatment market seem to see an increase in cost when compared to control firms in control markets. If the policy were to be scaled up it would not necessarily have negative spillover effects within the market as long as there are enough suppliers of intermediate-production whose prices will not be increased or who will not be able to discriminate among treated and control firms. Estimated indirect treatment effects 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 suppliers do not react by

increasing prices, we should expect effects of similar magnitudes to the one estimated here. Also, the increase in profits for treated firms comes only partially from savings on production costs, while about 50 percent of the effect is explained by changes in managerial practices and changes in the menu of goods.

In order to justify the intervention from a social point of view, these impacts must be weighed against the costs of offering the program. 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, each of seven treatment villages had two teachers who taught for a total of 48 hours and were paid about $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; conservatively assuming materials were purchased for all invitees, the total per-invitee cost of CREA's program is approximately $54.97 (=$49.97 + $5).

The program impacts were positive amongst the treated, but negative amongst the control in treatment villages: the direct effect on daily profits was a 23.4 percent increase (log(0.215) while the indirect effect - albeit imprecisely measured - was a 12.6 percent decrease (log(0.119)). Mean pre-treatment daily profit in the treatment villages were $10.68, which implies the program increased average net daily profits per entrepreneur by $1.15 (=$10.2 x (23.4% - 12.6%). 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 perpetuity of average increased profits is $2214.93 (= ($1.15 x 5.17 x 26)/0.07). Given this large disparity in program costs and benefits, it should be clear that it would be very 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.

We conclude with an important question: Why do we not observe private firms offering business training courses to micro-entrepreneurs? Given the large returns to training that we find, some entrepreneurs should demand the product at a price above cost. However, informational asymmetries and credit constraints in these poor, rural villages, may well be sufficiently large to impede the emergence of a market: business owners may not know the value, or even the existence, of better management skills,

further they might lack the initial capital (credit and savings constraint) for paying for such services upfront, while at the individual level those costs are substantially higher than at the group level due to fixed costs. It is also possible that potential suppliers of such services to small entrepreneurs may lack the capital required to build demand for the product through advertising or subsidized courses. Future research on the demand for business literacy training amongst entrepreneurs - and how demand evolves with knowledge of its effectiveness - would help governments and NGOs in deciding the optimal amount and type of subsidized intervention that should be provided.

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

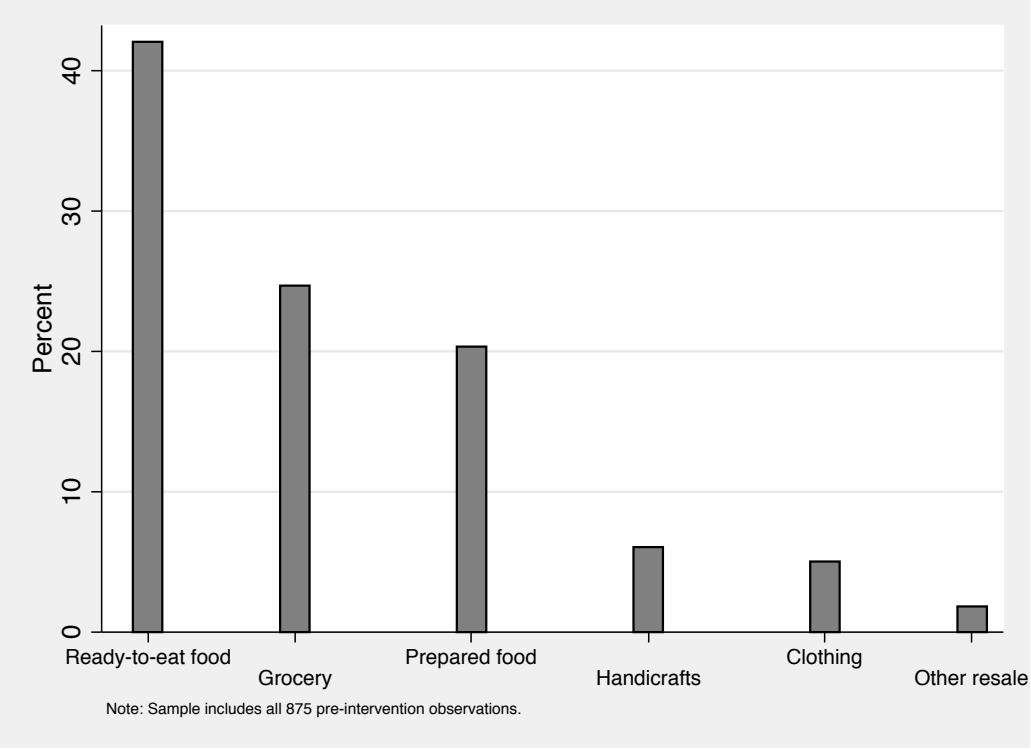

Figure 2: Entrepreneurial choice.

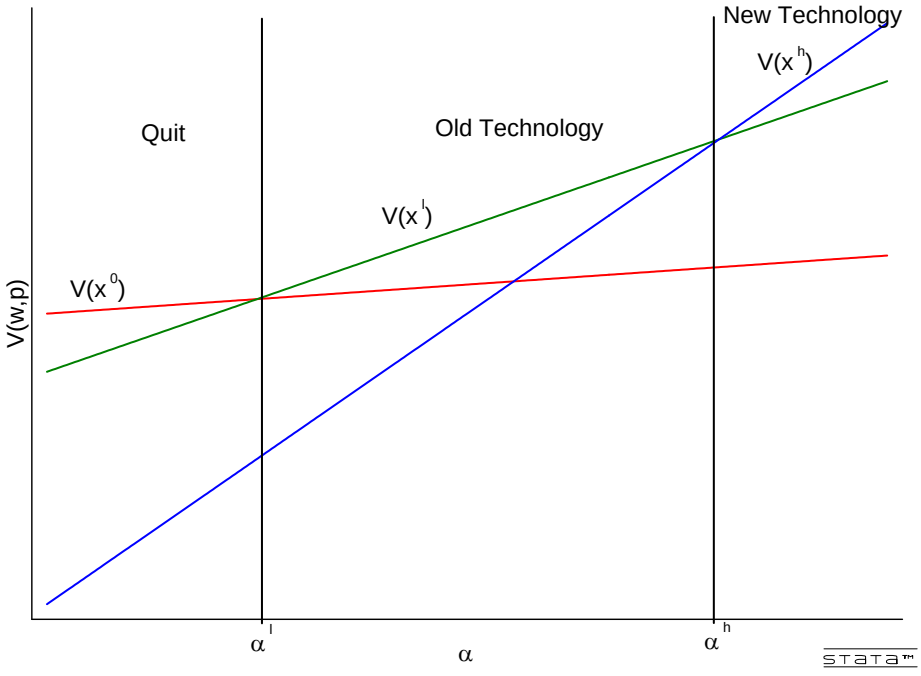

Figure 3: The distribution of baseline (log) daily profits amongst the whole and survived samples of the treatment and control groups

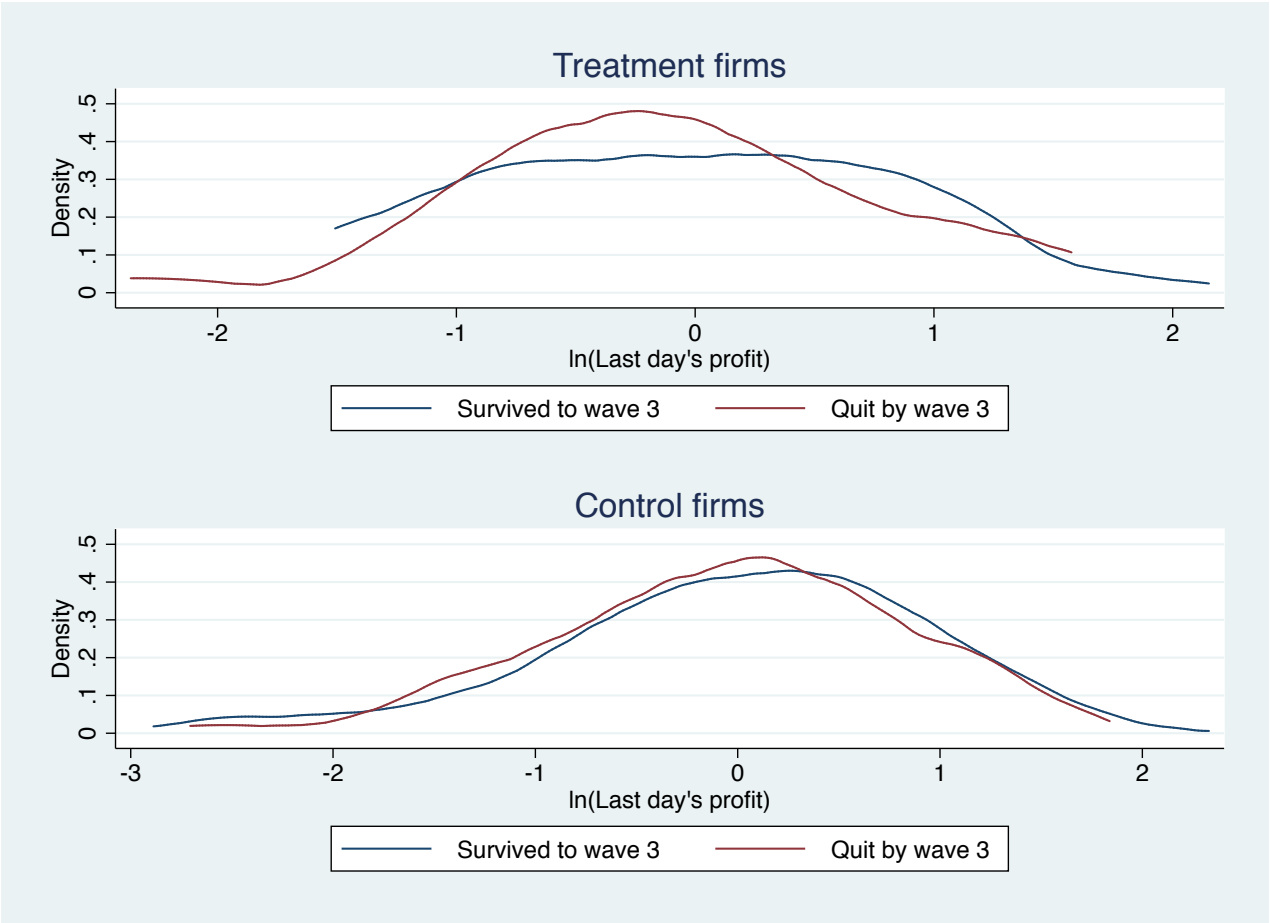

Table 1: Pre-treatment characteristics, by treatment group

	Treatment Villages		Control Villages	(1)=(2) p-value	(1)=(3) p-value	(2)=(3) p-value	Obs.
	Firms offered treatment	Control firms	All Firms				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Personal Characteristics							
Age	46.04	46.28	45.45	0.830	0.467	0.484	869
	(0.48)	(0.96)	(0.63)				
Years of education	5.96	6.05	6.08	0.666	0.743	0.895	846
	(0.32)	(0.21)	(0.15)				
Roof is made of temporary material	0.33	0.31	0.32	0.775	0.947	0.962	844
	(0.09)	(0.08)	(0.07)				
Score on math exercise (% correct)	0.39	0.44	0.48	0.114	0.134	0.511	864
	(0.04)	(0.03)	(0.04)				
Keeps formal business accounts	0.01	0.03	0.04	0.441	0.092*	0.537	873
	(0.01)	(0.01)	(0.01)				
Weekly hours worked in enterprise	39.43	35.82	40.40	0.196	0.803	0.088*	866
	(3.19)	(1.35)	(2.11)				
Household income, daily	158.71	173.24	182.96	0.500	0.508	0.778	826
	(18.86)	(14.99)	(30.32)				
Business Characteristics							
Produces goods for sale	0.62	0.69	0.66	0.024**	0.452	0.628	875
	(0.03)	(0.03)	(0.05)				
Last day's profit	132.24	145.54	158.52	0.553	0.458	0.717	760
	(16.05)	(17.29)	(30.50)				
Last day's revenue	456.16	404.74	406.42	0.341	0.508	0.976	840
	(55.14)	(28.09)	(48.34)				
Number of clients last day	14.03	15.70	13.95	0.488	0.971	0.469	808
	(1.47)	(1.88)	(1.41)				
Number of employees	0.49	0.64	0.52	0.138	0.539	0.255	874
	(0.03)	(0.09)	(0.04)				
Weekly hours worked by employees	10.27	12.42	9.79	0.341	0.846	0.205	872
	(2.26)	(1.79)	(0.86)				
Age of business (years)	6.77	7.17	7.79	0.496	0.402	0.596	874
	(0.84)	(0.77)	(0.83)				
Replacement value of business capital	8,062.61	10,714.60	8,704.49	0.031**	0.688	0.374	875
	(1,008.94)	(1,832.23)	(1,200.87)				
Registered with the government	0.15	0.20	0.22	0.242	0.085*	0.630	844
	(0.03)	(0.04)	(0.03)				
Assigned observations	164	189	522				

Notes: Sample includes all subjects interviewed in the baseline survey. Standard errors in parentheses clustered at the village level; p-values in columns 5 and 6 are calculated from F-tests of the equality of means with 15 degrees of freedom (see text). All monetary variable are measured in Mexican Pesos (~13 pesos / 1 U.S. dollar). Government registration is with the Secretary of Finance and Public Credit (SHCP). ***p<.01; **p<.05; *p<0.1

Table 2: The effects of business training on main business outcomes

	Intent to Treat Effect (ITT)			Lower bound on ITT		Upper bound on ITT			
	Mean	(s.e.)		Mean	(s.e.)	Mean	(s.e.)		Survey waves
Outcome:	(1)		Obs.	(2)		(3)		Obs.	included
Quit her business	0.016	(0.032)	1,836	-0.152***	(0.043)	0.168***	(0.032)	2,058	1,2,3
Measures of profit and revenue									
ln(Last day's profit)	0.213*	(0.110)	1,183	0.144	(0.123)	0.329***	(0.107)	1,177	1,2,3
Standardized profits	0.200**	(0.077)	1,322	0.152*	(0.079)	0.268**	(0.094)	1,317	1,2,3
ln(Last day's revenue)	0.253**	(0.115)	1,357	0.164	(0.116)	0.358***	(0.118)	1,350	1,2,3
Standardized revenue	0.232***	(0.067)	1,421	0.187**	(0.071)	0.297***	(0.071)	1,415	1,2,3
Other business outcomes									
ln(# clients last day)	0.220*	(0.121)	1,312	0.143	(0.120)	0.335**	(0.121)	1,301	1,2,3
ln(Household income, daily)	0.194*	(0.109)	1,080	0.138	(0.106)	0.314**	(0.122)	1,075	1,2
ln(# goods for sale)	0.204**	(0.089)	1,145	0.108	(0.086)	0.469***	(0.097)	1,118	1,2
ln(Mean unit cost)	-0.273*	(0.148)	907	-0.292*	(0.152)	-0.212	(0.142)	905	1,2
ln(Mean unit price)	-0.021	(0.085)	1,139	-0.053	(0.090)	0.047	(0.097)	1,135	1,2
% correct on business practices exercise	0.051	(0.063)	1,210	0.008	(0.062)	0.146**	(0.059)	1,180	1,2,3
Uses formal accounting methods	0.048*	(0.023)	1,432	-0.190***	(0.034)	0.262***	(0.060)	1,652	1,2,3
Hours worked per week by owner	2.686	(2.639)	1,411	-1.107	(2.800)	3.935	(2.724)	1,403	1,2,3
Hours worked per week by employees	0.756	(3.833)	1,143	-3.473	(3.652)	14.915**	(5.344)	1,081	1,2
Number of employees	0.091	(0.108)	1,419	-0.002	(0.109)	0.986***	(0.154)	1,308	1,2,3
Registered with the government	0.087***	(0.029)	1,399	-0.155***	(0.044)	0.298***	(0.048)	1,618	1,2,3

Notes: Samples include firms offered treatment in treatment villages and all firms in control villages. Coefficients are estimated by Equation 1, including an indicator for the first follow-up wave. Covariates include the pre-program covariates used for assigning treatment: 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. Standardized profits (revenues) are constructed as the mean of standardized z-scores of the four profit (revenue) measures. For continuous outcomes, 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. For binary outcomes, lower and upper bouds are calculated using Manski's methodology. Standard errors in parentheses clustered at the village level. P-values are calculated from t-tests with 15 degrees of freedom. ***p<.01; **p<.05; *p<0.1

Table 3: Robustness of the main effects

	Excluding Pre-treatment controls			Imputed values for quitters and attriters			Including control firms in treatment villages			Survey waves included
	ITT	(s.e.)	Obs.	ITT	(s.e.)	Obs.	ITT	(s.e.)	Obs.	
	(1)			(2)			(3)			
Quit her business	0.017	(0.032)	1,836	--	--	--	0.020	(0.027)	2,348	1,2,3
Measures of profit and revenue										
ln(Last day's profit)	0.163	(0.102)	1,183	0.109	(0.109)	1,637	0.254***	(0.083)	1,531	1,2,3
Standardized profit	0.159*	(0.080)	1,322	0.128	(0.075)	1,713	0.209***	(0.065)	1,699	1,2,3
ln(Last day's revenue)	0.226*	(0.115)	1,357	0.101	(0.124)	1,779	0.252**	(0.106)	1,699	1,2,3
Standardized revenue	0.194**	(0.076)	1,421	0.150*	(0.078)	1,812	0.209***	(0.057)	1,828	1,2,3
Other business outcomes										
ln(# clients last day)	0.251*	(0.128)	1,312	0.095	(0.114)	1,728	0.204*	(0.110)	1,690	1,2,3
ln(Household income, daily)	0.160	(0.105)	1,080	--	--	--	0.192*	(0.109)	1,403	1,2
ln(# goods for sale)	0.179*	(0.102)	1,145	--	--	--	0.195**	(0.079)	1,474	1,2
ln(Mean unit cost)	-0.394*	(0.194)	907	--	--	--	-0.320**	(0.113)	1,174	1,2
ln(Mean unit price)	-0.085	(0.087)	1,139	--	--	--	-0.050	(0.086)	1,467	1,2
% correct on business practices exercise	0.037	(0.065)	1,210	--	--	--	0.054	(0.052)	1,551	1,2,3
Uses formal accounting methods	0.045*	(0.021)	1,432	0.040**	(0.015)	1,825	0.032	(0.022)	1,844	1,2,3
Hours worked per week by owner	2.003	(3.069)	1,411	2.727	(2.762)	1,804	1.572	(2.388)	1,816	1,2,3
Hours worked per week by employees	0.512	(3.588)	1,143	0.706	(2.608)	1,536	-0.012	(3.311)	1,472	1,2,3
Number of employees	0.096	(0.095)	1,419	--	--	--	0.089	(0.105)	1,827	1,2,3
Registered with the government	0.077**	(0.031)	1,399	0.091***	(0.030)	1,790	0.092***	(0.024)	1,809	1,2,3

Notes: Samples in columns 1 and 2 include firms offered treatment in treatment villages and all firms in control villages, sample in column 3 also includes control firms in treatment villages. The estimates in column 1 exclude all pre-treatment control variables. The estimates in column 2 include control variables and impute outcomes for individuals who quit or attrited as follows: 0 for linear and binary outcomes, ln(0.1) for clients served, and ln(1) for other logarithmic outcomes. Standard errors in parentheses clustered at the village level. P-values are calculated from t-tests with 15 degrees of freedom, ***p<.01; **p<.05; *p<0.1

Table 4: The effects of business training by wave

Outcome:	ITT wave 2	(s.e.)	ITT wave 3	(s.e.)	H_0: ITT wave 2 = ITT wave 3, p-value	Obs.
Measures of profit and revenue						
ln(Last day's profit)	0.216*	(0.057)	0.208	(0.345)	0.971	1,183
Standardized profits	0.200**	(0.047)	0.198	(0.160)	0.986	1,322
ln(Last day's revenue)	0.240**	(0.027)	0.278	(0.160)	0.814	1,357
Standardized revenue	0.222***	(0.006)	0.249*	(0.053)	0.835	1,421
Other business outcomes						
ln(# clients last day)	0.237**	(0.044)	0.189	(0.355)	0.779	1,312
% correct on business practices exercise	0.037	(0.572)	0.127*	(0.090)	0.122	1,210
Uses formal accounting methods	0.030	(0.124)	0.078	(0.214)	0.477	1,432
Hours worked per week by owner	4.066**	(0.037)	0.237	(0.961)	0.322	1,411
Number of employees	0.178	(0.193)	-0.058	(0.692)	0.204	1,419
Registered with the government	0.073*	(0.054)	0.110**	(0.029)	0.520	1,399

Notes: Samples include firms offered treatment in treatment villages and all firms in control villages. Outcomes only include those observed in all three survey waves. Covariates included. Standardized profits (revenues) are constructed as the mean of standardized z-scores of the four profit (revenue) measures. Standard errors in parentheses clustered at the village level. P-values are calculated from t-tests with 15 degrees of freedom. ***p<.01; **p<.05; *p<0.1

Table 5: Effects on goods that were dropped across waves, kept across waves, and added post-intervention

Outcome:	*Goods that were:*	ITT	(s.e.)	Obs.
Standardized profit	Dropped	-0.040	(0.106)	489
	Kept	0.139	(0.176)	511
	Added	-0.080	(0.119)	138
Standardized revenue	Dropped	-0.094	(0.099)	519
	Kept	0.095	(0.132)	710
	Added	0.149	(0.104)	320
ln(Mean unit cost)	Dropped	0.167*	(0.086)	512
	Kept	-0.300*	(0.170)	533
	Added	0.018	(0.249)	145

Notes: Samples include firms offered treatment in treatment villages and all firms in control 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. Standard errors in parentheses clustered at the village level. P-values are calculated from t-tests with 15 degrees of freedom, ***p<.01; **p<.05; *p<0.1

Table 6: The effects of business training by sectoral density.

	# of firms in village-sector x Treat x Post		Treat x Post		# of firms in village-sector			Survey waves
Outcome:	Mean	s.e.	Mean	s.e.	Mean	s.e.	Obs.	included
Quit her business	0.011	(0.010)	-0.066	(0.064)	-0.003	(0.003)	1,836	1,2,3
Measures of profit and revenue								
ln(Last day's profit)	0.030	(0.020)	-0.021	(0.160)	-0.042	(0.027)	1,183	1,2,3
Standardized profits	0.028**	(0.011)	-0.016	(0.123)	-0.032**	(0.013)	1,322	1,2,3
ln(Last day's revenue)	0.025	(0.022)	0.059	(0.240)	-0.017	(0.026)	1,357	1,2,3
Standardized revenue	0.027*	(0.015)	0.032	(0.136)	-0.021	(0.015)	1,421	1,2,3
Other business outcomes								
ln(# clients last day)	-0.045**	(0.021)	0.546***	(0.137)	0.016	(0.017)	1,312	1,2,3
ln(Household income, daily)	0.016	(0.023)	0.073	(0.246)	-0.011	(0.018)	1,080	1,2
ln(# goods for sale)	0.025***	(0.007)	0.020	(0.120)	0.004	(0.014)	1,145	1,2
ln(Mean unit cost)	-0.047	(0.029)	0.122	(0.302)	-0.014	(0.045)	907	1,2
ln(Mean unit price)	-0.008	(0.033)	0.039	(0.264)	-0.031	(0.040)	1,139	1,2
% correct on business practices exercise	-0.002	(0.013)	0.062	(0.075)	-0.001	(0.008)	1,210	1,2,3
Uses formal accounting methods	-0.004	(0.004)	0.080	(0.049)	0.000	(0.003)	1,432	1,2,3
Hours worked per week by owner	-0.472	(0.451)	6.237	(4.041)	1.014*	(0.504)	1,411	1,2,3
Hours worked per week by employees	1.177**	(0.457)	-8.059	(5.199)	-2.111***	(0.584)	1,143	1,2
Number of employees	0.010	(0.019)	0.003	(0.166)	-0.020	(0.020)	1,419	1,2,3
Registered with the government	0.001	(0.009)	0.076	(0.074)	-0.002	(0.008)	1,399	1,2,3

Notes: Samples include firms offered treatment in treatment villages and all firms in control villages. The number of firms in a village-sector is calculated pre-treatment. Standard errors in parentheses clustered at the village level. P-values are calculated from t-tests with 15 degrees of freedom. ***p<.01; **p<.05; *p<0.1

Table 7: Heterogeneous treatment effects by pre-intervention profits

Sample =	Above median of standardized baseline profits			Below median of standardized baseline profits			(1)=(2) p-value	Survey waves
	ITT	(s.e)	Obs.	ITT	(s.e)	Obs.		
Outcome:	(1)			(2)			(3)	included
Quit her business	0.014	(0.029)	876	-0.016	(0.058)	912	0.700	1,2,3
Measures of profit and revenue								
ln(Last day's profit)	0.281*	(0.150)	609	0.042	(0.154)	561	0.240	1,2,3
Standardized profits	0.236**	(0.107)	669	0.057	(0.099)	640	0.132	1,2,3
ln(Last day's revenue)	0.338**	(0.131)	680	0.113	(0.188)	653	0.334	1,2,3
Standardized revenue	0.287***	(0.082)	703	0.107	(0.105)	688	0.187	1,2,3
Other business outcomes								
ln(# clients last day)	0.332**	(0.145)	646	0.150	(0.127)	644	0.272	1,2,3
ln(Household income, daily)	0.247	(0.182)	537	0.022	(0.162)	528	0.308	1,2
ln(# goods for sale)	0.193**	(0.084)	562	0.122	(0.094)	556	0.364	1,2
ln(Mean unit cost)	-0.151	(0.190)	459	-0.270	(0.239)	434	0.713	1,2
ln(Mean unit price)	0.090	(0.094)	557	-0.153	(0.116)	555	0.065*	1,2
% correct on business practices	0.058	(0.075)	583	0.035	(0.061)	598	0.468	1,2,3
Uses formal accounting methods	0.068*	(0.037)	707	0.042	(0.028)	692	0.581	1,2,3
Hours worked per week by owner	4.635	(3.409)	697	-0.283	(2.963)	682	0.087*	1,2,3
Hours worked per week by employees	6.786	(6.136)	565	-5.126*	(2.906)	552	0.046**	1,2
Number of employees	0.216	(0.180)	701	-0.044	(0.099)	685	0.106	1,2,3
Registered with the government	0.112**	(0.050)	690	0.082	(0.051)	678	0.606	1,2,3

Notes: Samples include firms offered treatment in treatment villages and all firms in control villages. Covariates included. Standardized profits (revenues) are constructed as the mean of standardized z-scores of the four profit (revenue) measures. Standard errors in parentheses clustered at the village level. P-values are calculated from t- and F-tests with 15 degrees of freedom. ***p<.01; **p<.05; *p<0.1

Table 8: The indirect effects of business training

	Indirect Treatment Effect (ITE)			Survey waves
	Mean	s.e.	Obs.	included
Quit her business	-0.018	(0.039)	1,907	1,2,3
Measures of profit and revenue				
ln(Last day's profit)	-0.119	(0.119)	1,250	1,2,3
Standardized profits	-0.034	(0.099)	1,388	1,2,3
ln(Last day's revenue)	0.053	(0.093)	1,430	1,2,3
Standardized revenue	0.096	(0.072)	1,491	1,2,3
Other business outcomes				
ln(# clients last day)	0.074	(0.134)	1,371	1,2,3
ln(Household income, daily)	0.009	(0.099)	1,128	1,2
ln(# goods for sale)	0.020	(0.060)	1,196	1,2
ln(Mean unit cost)	0.145	(0.175)	957	1,2
ln(Mean unit price)	0.106	(0.091)	1,190	1,2
% correct on business practices exercise	0.001	(0.055)	1,239	1,2,3
Uses formal accounting methods	0.057***	(0.019)	1,501	1,2,3
Hours worked per week by owner	3.907*	(1.870)	1,479	1,2,3
Hours worked per week by employees	2.224	(3.048)	1,194	1,2
Number of employees	0.016	(0.065)	1,485	1,2,3
Registered with the government	-0.025	(0.032)	1,472	1,2,3

Notes: Samples include control firms in treatment villages and all firms in control villages. Covariates included. Standardized profits (revenues) are constructed as the mean of standardized z-scores of the four profit (revenue) measures. Standard errors in parentheses clustered at the village level. P-values are calculated from t-tests with 15 degrees of freedom. ***p<.01; **p<.05; *p<0.1

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 followup 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

	Treated Firms		(1)=(2) p-value	Obs.
	Attended Classes	Did Not Attend Classes		
	(1)	(2)	(3)	(4)
Personal Characteristics				
Age	46.98	44.25	0.292	163
	(0.91)	(1.80)		
Years of education	6.07	5.76	0.555	161
	(0.41)	(0.44)		
Roof is made of temporary material	0.38	0.22	0.013**	160
	(0.11)	(0.07)		
Score on math exercise (% correct)	0.39	0.38	0.789	164
	(0.05)	(0.06)		
Keeps formal business accounts	0.01	0.02	0.717	164
	(0.01)	(0.02)		
Weekly hours worked in enterprise	37.84	42.43	0.335	162
	(4.02)	(4.03)		
Household income, daily	146.36	182.20	0.599	159
	(32.22)	(45.37)		
Business Characteristics				
Produces goods for sale	0.67	0.53	0.081*	164
	(0.02)	(0.08)		
Last day's profit	110.83	177.91	0.313	141
	(28.90)	(43.62)		
Last day's revenue	337.85	690.53	0.257	158
	(75.24)	(243.80)		
Number of clients last day	13.76	14.55	0.861	152
	(1.86)	(3.65)		
Number of employees	0.54	0.40	0.402	164
	(0.05)	(0.13)		
Weekly hours worked by employees	11.85	7.32	0.249	164
	(2.86)	(3.21)		
Age of business (years)	6.68	6.94	0.855	164
	(0.66)	(1.63)		
Replacement value of business capital	7,441.43	9,228.68	0.437	164
	(1,310.72)	(1,819.19)		
Registered with the government	0.16	0.11	0.250	158
	(0.04)	(0.03)		

Notes: Sample includes all women assigned to treatment who did not attrite post-intervention. Standard errors in parentheses clustered at the village level; p-values in column 3 are calculated from F-tests of the equality of means with 15 degrees of freedom (see text). All monetary variable are measured in Mexican Pesos (~13 pesos / 1 U.S. dollar). Government registration is with the Secretary of Finance and Public Credit (SHCP). ***p<.01; **p<.05; *p<0.1

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

	All Firms		(1)=(2) p-value	Obs.
	Ever Attrited	Never Attrited		
	(1)	(2)	(3)	(4)
Personal Characteristics				
Age	44.89	46.04	0.290	869
	(1.04)	(0.44)		
Years of education	6.33	5.95	0.085*	846
	(0.21)	(0.14)		
Roof is made of temporary material	0.28	0.33	0.228	844
	(0.05)	(0.06)		
Score on math exercise (% correct)	0.43	0.46	0.370	864
	(0.03)	(0.03)		
Keeps formal business accounts	0.03	0.03	0.986	873
	(0.01)	(0.01)		
Weekly hours worked in enterprise	42.34	38.14	0.072*	866
	(2.42)	(1.47)		
Household income, daily	143.40	187.40	0.069*	826
	(15.00)	(22.88)		
Business Characteristics				
Produces goods for sale	0.62	0.68	0.025**	875
	(0.04)	(0.03)		
Last day's profit	123.16	160.35	0.094*	760
	(11.78)	(23.28)		
Last day's revenue	347.61	439.45	0.048**	840
	(20.98)	(38.20)		
Number of clients last day	14.18	14.42	0.823	808
	(1.21)	(1.05)		
Number of employees	0.50	0.56	0.258	874
	(0.05)	(0.04)		
Weekly hours worked by employees	10.35	10.48	0.938	872
	(1.24)	(1.13)		
Age of business (years)	6.55	7.79	0.171	874
	(0.70)	(0.70)		
Replacement value of business capital	7,298.10	9,628.18	0.148	875
	(1,066.35)	(1,163.03)		
Registered with the government	0.20	0.20	0.882	844
	(0.04)	(0.02)		

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. Standard errors in parentheses clustered at the village level; p-values in column 3 are calculated from F-tests of the equality of means with 15 degrees of freedom (see text). All monetary variable are measured in Mexican Pesos (~13 pesos / 1 U.S. dollar). Government registration is with the Secretary of Finance and Public Credit (SHCP). ***p<.01; **p<.05; *p<0.1

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

	All Firms		(1)=(2) p-value	Obs.
	Ever Quit	Did Not Quit		
	(1)	(2)	(3)	(4)
Personal Characteristics				
Age	44.43	47.21	0.005***	822
	(0.59)	(0.61)		
Years of education	6.26	5.82	0.078*	799
	(0.17)	(0.20)		
Roof is made of temporary material	0.38	0.26	0.002***	797
	(0.06)	(0.05)		
Score on math exercise (% correct)	0.45	0.46	0.500	816
	(0.03)	(0.03)		
Keeps formal business accounts	0.02	0.04	0.120	825
	(0.01)	(0.01)		
Weekly hours worked in enterprise	36.14	41.83	0.012**	818
	(2.10)	(1.40)		
Household income, daily	156.13	195.15	0.260	781
	(12.74)	(32.92)		
Business Characteristics				
Produces goods for sale	0.70	0.64	0.078*	827
	(0.04)	(0.03)		
Last day's profit	124.85	174.21	0.196	722
	(12.48)	(34.82)		
Last day's revenue	375.42	457.68	0.234	793
	(43.74)	(45.28)		
Number of clients last day	14.02	14.78	0.638	763
	(1.42)	(1.15)		
Number of employees	0.48	0.64	0.063*	826
	(0.04)	(0.07)		
Weekly hours worked by employees	9.22	12.39	0.047**	824
	(0.94)	(1.40)		
Age of business (years)	6.33	8.69	0.004***	826
	(0.72)	(0.70)		
Replacement value of business capital	7,883.10	10,761.50	0.031**	827
	(1,112.81)	(1,178.51)		
Registered with the government	0.14	0.27	0.000***	797
	(0.02)	(0.03)		

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. Standard errors in parentheses clustered at the village level; p-values in column 3 are calculated from F-tests of the equality of means with 15 degrees of freedom (see text). All monetary variable are measured in Mexican Pesos (~13 pesos / 1 U.S. dollar). Government registration is with the Secretary of Finance and Public Credit (SHCP). ***p<.01; **p<.05; *p<0.1

Appendix Table 4: The effects of business training on all profit and revenue measures.

	Intent to Treat Effect (ITT)			Lower bound on ITT		Upper bound on ITT			
	Mean	s.e.		Mean	s.e.	Mean	s.e.		Survey waves
Outcome:	(1)		Obs.	(2)		(3)		Obs.	included
ln(Last day's profit)	0.213*	(0.110)	1,183	0.144	(0.123)	0.329***	(0.107)	1,177	1,2,3
ln(Last week's profit)	0.168	(0.110)	1,117	0.111	(0.097)	0.238*	(0.124)	1,113	1,2,3
ln(Good specific last day's profit)	0.178	(0.220)	807	0.103	(0.226)	0.298	(0.230)	805	1,2
ln(Good specific last month's profit)	0.191	(0.219)	834	0.130	(0.238)	0.343*	(0.193)	831	1,2
Standardized profits	0.200**	(0.077)	1,322	0.152*	(0.079)	0.268**	(0.094)	1,317	1,2,3
ln(Last day's revenue)	0.253**	(0.115)	1,357	0.164	(0.116)	0.358***	(0.118)	1,350	1,2,3
ln(Last week's revenue)	0.213*	(0.110)	1,258	0.151	(0.120)	0.319**	(0.110)	1,251	1,2,3
ln(Good specific last day's revenue)	0.248	(0.155)	1,071	0.156	(0.139)	0.349*	(0.166)	1,067	1,2
ln(Good specific last month's revenue)	0.400*	(0.190)	1,028	0.316	(0.183)	0.503**	(0.177)	1,024	1,2
Standardized revenue	0.232***	(0.067)	1,421	0.187**	(0.071)	0.297***	(0.071)	1,415	1,2,3

Notes: Samples include firms offered treatment in treatment villages and all firms in control villages. Coefficients are estimated by Equation 1, including an indicator for the first follow-up wave. Covariates include the pre-program covariates used for assigning treatment: 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. Standardized profits (revenues) are constructed as the mean of standardized z-scores of the four profit (revenue) measures. For continuous outcomes, 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. For binary outcomes, lower and upper bouds are calculated using Manski's methodology. Standard errors in parentheses clustered at the village level. P-values are calculated from t-tests with 15 degrees of freedom. ***p<.01; **p<.05; *p<0.1