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

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NOVEMBER 2014

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 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. 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
Keywords: Business literacy, development, entrepreneurship

*We thank Shauna Cozad, Marina Kutyavina, Paul Feldman, and especially José Maria (Chema) Gardoni, Alejandro Maza, and Carla Roa for excellent research assistance. We are especially indebted to Leticia Jaraegui and the staff of CREA. Helpful comments were made by Pascaline Dupas, Rema Hanna, Dean Karlan, Asim Khwaja, Neale Mahoney, Anant Nyshandham, Jon Robinson, Mark Rosenzweig, Fabiano Schivardi, Chris Udry, and seminar participants at UCLA, Harvard-MIT, Yale, USC, Cal-Poly San Luis Obispo, NY-Fed, IFPRI, the IDB, and the EIEF. We gratefully acknowledge funding from Stanford Center for International Development, the Freeman Spogli Institute, the Michelle R. Clayman Institute for Gender Research, the Social Science Research Council, the Graduate Research Opportunity (Studies and Diversity Program of the School of Humanities and Sciences, Stanford University), and SEED-Stanford University. Giacomo De Giorgi acknowledges financial support from the Spanish Ministry of Economy and Competitiveness, through the Severo Ochoa Programme for Centres of Excellence in R&D (SEV-2011-0075) and ECO2011-28822, the EU through the Marie Curie CIG grant FP7-631510. The project was granted IRB approval by the Naval Postgraduate School and Stanford University.

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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 poor female entrepreneurs in rural Mexico. Our first goal is to assess the impact of the intervention on the population as a whole as we surveyed the population of female entrepreneurs (not in service sectors) in the sampled villages. We, therefore, offered the training to a random set of entrepreneurs in our treatment villages while surveying the population of entrepreneurs both in treatment and control villages. We then look at the heterogeneous effects of the treatment based on the entrepreneur's pre-treatment ability. A simple model of entrepreneurial learning highlights how training can inform a naive entrepreneur of her actual ability to be profitable, and induce those who find they are of low ability to quit their business.

Our experimental sample comprises about 900 (small and micro) entrepreneurs engaged in the production and sale of food, craft items, as well as the sale of 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 identifying unit costs, the importance of recording sales, and pricing to maximize profits.

We find that those women who were offered the training have larger profits, revenues, and serve a larger number of clients at followup interviews. We also find, consistently with the intervention, 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. Furthermore, treated firms were 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. Furthermore, we collected two rounds of post-intervention data, one year and 2.5 years post-program implementation, and find that the effects of the treatment do not fade out into the medium run.

In order to understand the heterogeneous impacts of treatment, we develop a simple conceptual framework along the lines of Karlan, Knight and Udry (2012). We think of the entrepreneur in this context as an experimenter with a noisy signal of her productivity who faces the outside option of quitting her business. 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 productive and 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. Through the adoption of the new technology, 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 that low ability entrepreneurs are indeed more likely to quit their businesses as a result of the training, and the largest positive effects are recorded amongst the "best" entrepreneurs.

A further 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-participants in program villages (Miguel and Kremer (2004); Angelucci and De Giorgi (2009)). It is unclear whether the indirect treatment effects should be positive or negative. For example, treated subjects may implement better business practices and capture market share at the expense of non-treated entrepreneurs, or, treated subjects may share their knowledge with non-treated subjects, intentionally through conversation or unintentionally if the new business practices are observable (such as new menus, changes to the product mix, or changes in prices). Regardless, it is clear that the presence or absence of indirect treatment effects have important implications for the global effectiveness of development policies.

In terms of profits, we detect 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 choose lower costs inputs and goods, 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, a back-of-the-envelope cost and benefit analysis developed in Section 6 suggests that the intervention is extremely cost effective.

Our paper contributes to the growing literature on the effects of business literacy training on firm 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 ways. 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 is important because it allows us to isolate the independent effect of business training, something that is not possible with much of the existing literature (e.g., Field, Jayachandran and Pande (2010), Karlan and Valdivia (2011), Drexler, Fischer and Schoar (2014)).[2]

Our 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)).

This research also contributes to our 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-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

[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.

and our experimental design; Section 3 develops a conceptual framework to help interpret the main findings; Section 4 describes the data and the sample; 5 presents the empirical methodology and discusses the main effects of the intervention; and Section 6 concludes.

2. Description of the Business Literacy Training and Experiment

2.1 The business literacy classes

In 2009, we partnered with the NGO *CREA* to develop and implement a business literacy training program for small, female-headed firms in the retail or production sector. CREA operates in small villages in the Mexican state of Zacatecas, a high-altitude, dry, and 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 the choice of products to produce or sell. The fifth covers marketing, including concepts related to knowing and responding to competition. The final module is a discussion of how to be an effective salesperson.

The content and teaching style of CREA's course is intentionally simplified in order to be understandable to the population at hand, the majority of whom have low levels of formal 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.[3]

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. 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 who sold goods or provided services, as identified by the 2005 Mexican census, (ii) are within a two hour drive from the City of Zacatecas, and (iii) had fewer than 1500 households (also identified by the 2005 Mexican census).[4] 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.

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 female entrepreneurs per village, to whom we applied a pre-intervention questionnaire between July

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

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

and September of 2009.[5] 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.

3. A Simple Model of Entrepreneurial Experimentation and Business Literacy

To understand the potential effects of this intervention, we propose a simple theoretical framework which models entrepreneurs as experimenters with imperfect knowledge of their ability. 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:

$$\max_{c_{it}} V \equiv E_0 \sum_{0}^{\infty} \beta^t U(c_{it}, w_i) \tag{1}$$

$$s.t. \quad c_{it} \leq \pi_{it} \tag{2}$$

$$\text{where } \pi_{it} = f(x_i, \alpha_i) - x_i \text{ and } \pi_{i0} = w_i - x_i \tag{3}$$

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,

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

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.[6]

Reflecting the environment in our experimental setting, we assume that the entrepreneurs do not know their type with certainty ex-ante, but believe they are either a high productivity type with probability p_i^h, a low productivity type with probability p_i^l, and very low productivity type (the type that will quit her business) with probability p_i^0, with $\sum_{j=0,l,h} p_i^j = 1$. Choosing the new technology, however, will reveal the type of the entrepreneur ex-post as follows: if the more expensive management process succeeds, it returns π_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.

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:

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

[6] 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.

That is, she will choose to experiment if she is sufficiently optimistic about p^h.[7]

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

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

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

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

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

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

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

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:

$$E(\pi|T=1, \alpha > \alpha^h) - E(\pi|T=0, \alpha > \alpha^h) > 0. \tag{4}$$

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:

$$Pr(Quit|T=1, \alpha < \alpha^l) - Pr(Quit|T=0, \alpha < \alpha^l) > 0. \tag{5}$$

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

$$\frac{\partial\{E(\pi|T=1) - E(\pi|T=0)\}}{\partial \pi_0} \geq 0 \tag{6}$$
$$\frac{\partial\{Pr(Quit|T=1) - Pr(Quit|T=0)\}}{\partial \pi_0} \leq 0. \tag{7}$$

In our empirical analysis below, we first explore the overall effects of the program and we then test these implications of this model of entrepreneurial decision making.

4. Data and Sample

4.1 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.

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.[8]

While evidence from other developing countries suggests that self-reported measures of aggregate business activity are as accurate as formal accounting figures (de Mel, McKenzie and Woodruff, 2009), 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

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

[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.

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. At the same time, this information provides a set of alternative measures of business performance which help alleviate concerns that non-classical measurement error is driving our results.[10]

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

Several other outcomes will give us further insight into how the intervention affects the performance of the business, including: the number of employees (both paid and unpaid), the number of co-owners, 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*). In order to directly examine the effect of the training classes on our subjects' business-math knowledge, we administered a simple exercise related to production and sales (see Appendix Figure 2). This same question was applied both pre- and post-treatment. We score each of the four sections as either correct or incorrect, summing to create a total score for the exercise. Furthermore, we asked the entrepreneurs how they kept accounts for their business, whether through personal notes, a formal accounting method, or whether they did not keep any accounts.

To capture important heterogeneity in our sample pre-treatment, we also collected data on the owner's age, education, asset ownership (e.g. type of dwellings and number of rooms), risk aversion, reservation wages, credit availability and the cost of credit, the type of activity the woman is engaged in, the age of the business in months, and the replacement value of the firm's capital stock. Finally, in both post-intervention surveys, we elicited a firm's survival by asking if the entrepreneur still sells any goods. Naturally, we do not observe these business-related outcomes for firms who quit their enterprise.

[10]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 classes. Specifically, we cannot reject the equality of the correlations in the two measures for either profits or revenues between the control and treatment groups in the ex-post period, nor in a difference-in-differences specification. These results are inconsistent with systematic measurement error being the main driver of the positive treatment effects. We thank Rema Hanna for suggesting this testing strategy.

4.2 Sample and summary statistics

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

Table 1 contains mean pre-intervention characteristics by village type and treatment group, along with p-values from F-tests of their equality which suggest that the randomization was 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), with a large variation (the standard error of the mean is 16 pesos).[11] 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. [12] Businesses are small: on average there are 1.6 workers including the owner, and employees work only about one quarter of the hours the owner works (about 10 hours per week). About 60 percent of businesses have no workers other than the owner. The average age of a firm is about seven years, again with large variation.

Importantly, the women in our sample know how to make basic calculations, but are less proficient at determining profits or optimally setting prices. For example, 93 percent said that they know how to make simple math calculations (not shown in the table), while the average score on the math exercise was 39 percent, or less than two out of the four questions answered correctly.[13] Less than five percent

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

[12] Interestingly, the entrepreneurs in our sample seem to have access to credit that would allow them to replace the business capital at a high (albeit common for this type of population) six percent monthly interest rate.

[13] 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.

of entrepreneurs keep formal business accounts, and only about one fifth of the sample is registered with the government.

4.3 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 difference (partly driven by the small sample size), attendees appear 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 **?**).

In order to investigate the effect of treatment (being offered the class) on the treated (class attendees), we can instrument attendance status (which is presumably endogenous) with treatment status (which is exogenous due to randomization). However, for parsimony and a cleaner interpretation of the results, we instead focus on the Intent to Treat parameter to study the direct effect of inviting entrepreneurs to the training classes. 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).

4.4 Attrition

Some entrepreneurs attrited from our sample between the baseline and the first and second followup surveys; importantly, however, attrition rates do not vary significantly across treatment groups (on average). Specifically, at the time of the first post-intervention survey, sample attrition was 12.8 percent in the treatment compared to 15.3 percent in the control (p-value = 0.58). During the second followup survey, we were able to survey some of the attrited entrepreneurs from the first 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.[14]

5. Empirical Strategy and Results

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

$$y_{it} = \alpha + \beta T_i + \delta Post_t + \gamma(T_i * Post_t) + \lambda Wave2_t + \mathbf{X_i}\Omega + \varepsilon_{it} \quad (8)$$

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.[15]

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 8 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 8 is estimated on the sample of all entrepreneurs in the control villages and entrepreneurs in the treatment villages who were *not* offered the classes.

Second, with two post-intervention survey waves, we are able to estimate models that permit different treatment effects over time. However, as shown below, estimated treatment effects do not differ significantly across the two post-intervention survey waves, and so we pool the post-intervention surveys together in order to increase statistical power, while including *Wave*2 to absorb any time-specific effects.

Finally, statistical inference is complicated by the small number of clusters (i.e., villages), implying

[14] 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).

[15] 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.

that the standard (asymptotic) method for hypotheses testing will 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).[16]

5.1 The direct effect of classes on firm survival and main business related outcomes

5.1.1 Quitting, Profits and Revenues

We first explore the effect of business literacy classes on firm survival. In each post-intervention survey wave, an entrepreneur was asked if she still runs her business, and we define a firm as quitting accordingly. The first row of Table 2 shows the ITT on the likelihood of quitting one's business is an insignificant 1.6 percentage points, suggesting that the offer of classes did not differentially induced entrepreneurs to quit their business on average. However, many women did quit their businesses over our survey period implying we do not observe business related outcomes (such as profits and revenues). Specifically, by the first and second followup surveys, 18.6 and 41.1 percent of the sample had stopped running their business respectively.[17] This firm survival rate is typical for small businesses; 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% (see Bartelsman, Scarpetta and Schivardi (2003) and U.S. Small Business Administration (2012)). In order to understand the potential effect of selective quitting and attrition, we present bounds on the treatment effects in columns 3 to 6 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 our various measures of profits. The estimated $ITTs$ are positive and similar magnitude, around 20 log points, across all four measures of profits: last day's and last week's aggregate profits, and aggregate profits we calculated from reported good specific last day's and last month's profits. The effect is significant at the 10 percent level for last day's profit. As these measures are all self-reported, they likely contain significant measurement error. As such, we create a standardized profits measure by first normalizing each of our four measures of profits, and then calculating the mean across these four measures for each individual. The ITT on this standardized profit measure is a strongly significant (p-value=0.02) 0.2 standard deviations.

[16] In previous versions of this work, we used both the wild bootstrap method of Cameron, Gelbach and Miller (2008) and randomization inference (Rosenbaum, 2002) for statistical inference. As all three methods give very similar results, we choose to present here the simplest and most transparent - the alternative tests are available upon request.

[17] 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. Furthermore, these relationships hold equally in both the treatment and control groups (results available upon request).

Our revenue measures are defined analogously to the profit measures. The estimated $ITTs$ are large for all revenue measures, and significant at standard levels for three out of four. Overall, we find an increase in revenues of about 30 log points (about 35 percent), slightly larger than the effect of profits. Using a standardized measure, the effect on revenues becomes more precise (p-value=.004), with an effect of 0.23 standard deviations.[18]

The estimated upper bounds for our measures of profits and revenues are all positive and precisely estimated. The lower bounds are all positive with fairly large magnitudes, but we can not reject that they are equal to zero. However, the lower bounds on the standardized measures of both profits and revenues are significantly different from zero, giving us confidence that selective attrition and quitting are not driving our results.

It is also important to note that the estimated treatment effects are of similar magnitude in both the short run (one year post-intervention) and the medium run (2.5 years after the intervention). Table 3 contains by-wave $ITTs$ estimated from a version of equation 8 that includes indicators for each post-intervention wave, and their interaction with the treatment indicator. In general, point estimates of the ITT in wave 3 are of similar magnitude as in wave 2, yet are more noisy, and we can not reject the hypothesis that the $ITTs$ are equal across waves. This latter result is rather important as it shows that the one time intervention appears to have long lasting positive effects which do not seem to decay 2.5 years after the intervention.

Overall, the effects we find on business training on profits and revenues are large, yet comparable to other studies in the literature (McKenzie and Woodruff (2012);Bruhn, Karlan and Schoar (2013)).

5.1.2 Other business outcomes

We now turn to a complementary set of outcomes under the heading *Other business outcomes* in Table 2 which will help us understand why the large effects on profits and revenues occurred. We first find a positive and significant effect on the number of clients (22 log points) and the number of goods sold (20 log points, or approximately two extra goods for sale). We also find a significant program effect on household income of 19.4 log points. Importantly, household income was collected from all subjects regardless of whether they still ran their business. The effect on household income is of similar magnitude to that on profits, suggesting the program did not have large income effects onto household labor supply, i.e. spouses do not reduce their labor supply due to the increase in profits.

It appears that the observed increase in profits is coming from reduced costs rather than increased prices: unit costs fall by 27 log points while we detect no effect on unit prices. It does not appear that

[18]Note that the use of the standardized measures (Kling, Liebman and Katz (2007)) of profits and revenues also address concerns with multiple hypothesis testing (Romano and Wolf, 2005).

firms are changing the number of employees or the hours worked by either the owner or employees. Finally, we see evidence that entrepreneurs learned from the training: First, we see a large and significant positive effect (a 4.8 percentage points increase) on the use of formal accounting (with a baseline mean in the control group of 4 percent). Second, we see a positive but insignificant effect on the share of correct answers in our simple business exercise. And third, invitees are 8.6 percentage points more likely to register their business with a government agency; again, this is a large effect, representing an increase over pre-intervention registration levels of about 40 percent. The CREA course included a thorough discussion of the pros and cons of registering ones business, and this positive point estimate suggests that, upon learning this information, registering is an optimal decision for some entrepreneurs. As with profits and revenues, the effects on these other business outcomes are fairly persistent (albeit less precisely estimated) in the medium run (Table 3).

The program also seems to be changing the composition of the goods sold. In particular, Table 4 contains $ITTs$ for the outcomes calculated from the good-specific questionnaire, but restricts the sample to those goods that were either (i) dropped between the baseline and first post-intervention survey, (ii) kept across both surveys, or (iii) added in the first post-intervention survey. Although these results are only suggestive given the low power of our tests at the good-by-good level, they show 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. This analysis suggests that our entrepreneurs are endogenously changing their goods portfolio as a response to the business training, in a manner consistent to improving their overall performance, and as taught in the classes.

5.1.3 Robustness of the main results

Our estimated treatment effects are robust to various alternative specifications. First, excluding pre-program covariates does not changes point estimates meaningfully, but increases standard errors marginally, as expected (results available upon request). Second, we further explore whether differential quitting or attrition by firms is substantially biasing our results. A strong assumption that can help bound our estimates from below is that firms that quit or attrited had zero profits and revenues, served no clients, sold no goods, worked no hours, and had no other business activity. Applying this assumption (using 0.1 clients and 1 peso in profits and revenues in logarithmic specifications), treatment effects are smaller, as expected, and generally less precisely estimated (see Appendix Table 4). However, focusing on the standardized measures (those with the least measurement error) both standardized profits and revenues are positive and economically significant; the effect size on standardized

profits is 0.13 standard deviations (p-value=0.103), and the effect size on standardized revenues is of 0.15 standard deviations (p-value=0.07). All other results are qualitatively similar to those presented in the main table, albeit less statistically significant.

5.2 Spillover and general equilibrium effects of business literacy classes

We now turn to estimates of the Indirect Treatment Effects (ITE), estimated by equation 8 on the sample that excludes any entrepreneurs who were invited to the classes, and presented in Table 5. To the extent that villages represent segmented markets, these estimates identify the local spillover and general equilibrium effects of the intervention.

First, 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 in Table 5 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.

Interestingly, although it is not statistically significant, the point estimate on unit cost is a positive 14.5 log points. It is not clear why the indirect effect on costs would be positive, but if factor markets are not perfectly competitive, those offered treatment may be able to purchase input materials from lower-cost suppliers, leaving those not offered the treatment to purchase inputs from higher-cost suppliers. It is theoretically ambiguous as to whether we would expect the indirect effect on prices of the control entrepreneurs to be positive or negative. The point estimate suggests a small, yet insignificant positive indirect effect of the treatment on the logarithm of the mean unit price of 10.6 log points

It is reasonable to believe, given the small size of these villages, that treated entrepreneurs interact with non-treated entrepreneurs, perhaps sharing lessons learned in the business literacy classes. There do not appear to be spillover effects on business knowledge (as measured by our business practices exercise), but there does appear to be a large and statistically significant impact on the use of formal accounting methods: relative to the control villages, 5.7 percentage points more control entrepreneurs in treatment villages use formal accounting methods, significant at the 1 percent level, however given their business outcomes the ineligibles don't seem to act upon the adoption of formal accounting or simply could not properly perform such formal accounting. This estimate is even larger than the

positive direct effect of the treatment (a 4.8 percentage point increase. However, unlike the direct effect, there is not a positive 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. An increase in hours worked could be a rational response to increased competition from treated firms.

5.3 Testing the empirical predictions of the model of entrepreneurial experimentation

Average program impacts across the entire sample are useful for understanding the overall effect of the program; however, the model presented in section 3 offers further insight into how the program may differentially affect entrepreneurs. The predictions from this model are that the ITT on quitting should be non-increasing in pre-treatment profits and the ITT on profits should be non-decreasing in pre-treatment profits. We now test these predictions empirically. For ease of presentation, we split our sample into those above and below the median of the last day's pre-treatment profit, and present separate $ITTs$ estimated by equation 8.

Differential likelihood of quitting by baseline profits

Lowering the cost of adopting the new technology taught in the classes induces lower ability (lower α) entrepreneurs to try the technology, and these "excessive" experimenters will rapidly realize they are not good entrepreneurs once they keep better accounting and try out different business practices. They therefore quit their enterprise. This result can be seen in Figure 3, however already above the very low ability entrepreneurs we find no significantly different quitting rates between invitees and not invitees. This is consistent with the way we model the treatment as entrepreneurs will better know their ability but also could benefit from the new technology.

We further explore the hypothesis that the treatment will induce the low ability entrepreneurs to quit by looking at the distribution of pre-treatment profits. First, we present the distributions of pre-treatment (baseline) profits in the whole sample compared to the distribution of pre-treatment profits amongst those who did not quit by the second followup survey: Figure 3 contains these distributions for the treatment and control group separately. It is clear that the survived sample (i.e., those who did not quit) is similar in terms of baseline profits to the whole sample in the control group. In the treatment group, however, the distribution of the survived sample is significantly shifted to the right consistent with the prediction that those with the lowest ability (pre-treatment profits) will be induced to quit upon learning they are in fact a low ability type.[19]

[19] 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.

Second, we use a parametric framework (results available upon request) to statistically test whether treated entrepreneurs in the far left tail of pre-treatment profits indeed have a high propensity to quit (and attrit from the sample) than similar control entrepreneurs. 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. In practice, 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 overall significant for the 1st and 3rd percentiles, depending on the definition of quitting we adopt, and smaller in magnitude and insignificant for the 5th percentile. Thus, it appears α^l is around the 3rd percentile of pre-treatment profits. (Interaction terms for all percentiles greater than 5 are insignificant.) Furthermore, if we consider that attrition is a possible result of treatment, we can define successful businesses as those that did not quit or attrited by the second followup survey. A further test of the model 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.

Differential effects on profits by baseline profits

In Table 6 we perform a series of tests on the second set of predictions of the model: that the treatment effects on profits (as well as other business outcomes and intermediate steps to higher profits) should be increasing in the ability of the entrepreneur. Comparing $ITTs$ in the samples above and below the median of pre-treatment 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 profits, which we take 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) for those above the median and 0.057 for those below the median (and largely insignificant). The difference between the two estimated parameters of .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 concentrated completely 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 0.042 for those below the median. These point estimates are close enough to suggest that both high and low quality entrepreneurs seem to adopt part of the new technology, but only high quality entrepreneurs are successful. There is a small differential in terms of knowledge gains as measured by our business practices exercise.

One striking observation is the large and significant differential 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. There is little 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 lends support to the predictions of our theoretical model.

6. Conclusions

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

Recent years have seen a series of interventions offering business or financial training to entrepreneurs. Our intervention is unique in several ways, and thus offers new insights into our understanding of the effect of business literacy classes on enterprise performance. First, the intervention is very intensive, lasting six weeks with two, four-hour classes per week for a total instruction time of 48 hours; this is more than double many of the prior studies in this literature (e.g., Drexler, Fischer and Schoar (2014) and Karlan and Valdivia (2011). Second, our experimental design involves offering classes to a random subset of the population of micro-enterprises while not providing any other intervention (such as credit) beside business literacy training. This implies our findings are valid for

a broad class of businesses, and identify the effects of the classes uniquely. Third, our survey design includes two post-intervention surveys (one year and 2.5 years post-intervention), which allows us to explore both the short and medium run effects of the training. Fourth, we are able to detect village-level spillover and general equilibrium effects thanks to our experimental design.

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

These positive program impacts, however, must be weighed against the costs of running the business literacy classes in order to justify the intervention. In fact, a simple comparison of costs and benefits shows the program is indeed very cost-effective, once offered to a group of entrepreneurs at once, whether is the individual upfront cost or the lack informational preventing individual entrepreneurs from seeking out advice is however unclear. First, the cost of running the CREA classes is extremely low, as local teachers were hired for a modest wage, minimal materials were provided to the students, and community centers were used to hold classes at no-cost. Specifically (and using US dollars for convenience), each of seven treatment villages had two teachers who taught for a total of 48 hours and were paid \$10 per hour yielding \$6720 (=7 x 2 x 48 x \$10) in salaries. While only 65 percent of invitees came to class, the classrooms would have accommodated all invitees, so if CREA were to replicate the program, the appropriate per-invitee cost of teacher's salaries with 164 invitees is \$49.97 (=\$6720 / 164). Materials (photocopies of lessons, pens, paper, calculators, and CREA logo hats that were used as prizes) totaled about \$5 per participant; inflating the latter costs to the invitees, the total per-invitee cost of CREA's program is \$57.66 (=\$49.97 + \$7.79).

Second, a back-of-the-envelope calculation shows that the benefits in terms of increased profits far outweigh these costs: The ITT on the logarithm of daily profits is 0.215, which implies the offer of classes increased daily profits by 23.4 percent (=exp(0.215)). The mean pre-treatment daily profit in the treatment group was \$10.2, implying the offer of treatment increased daily profits by \$2.38 (=\$10.2 x 23.4%). Pre-treatment, entrepreneurs in the treatment group reported working an average of 5.17 days per week. We do not know how many weeks are worked per year, but given that some of the businesses are seasonal (such as selling certain handicrafts or seasonal foods), a conservative assumption is that the average entrepreneur works half the year, or 26 weeks. Using a seven percent annual discount rate, the present discounted value of the increased profits due to the program is \$4394.50 (= (\$2.38 x 5.17 x 26)/0.07). It should be clear that it would be 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. The policy would still be cost effective even if we were to count as a program costs the negative (but imprecisely estimated) indirect treatment effect on the (daily) profits of the control firms in treatment villages (Table 5).

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

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

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

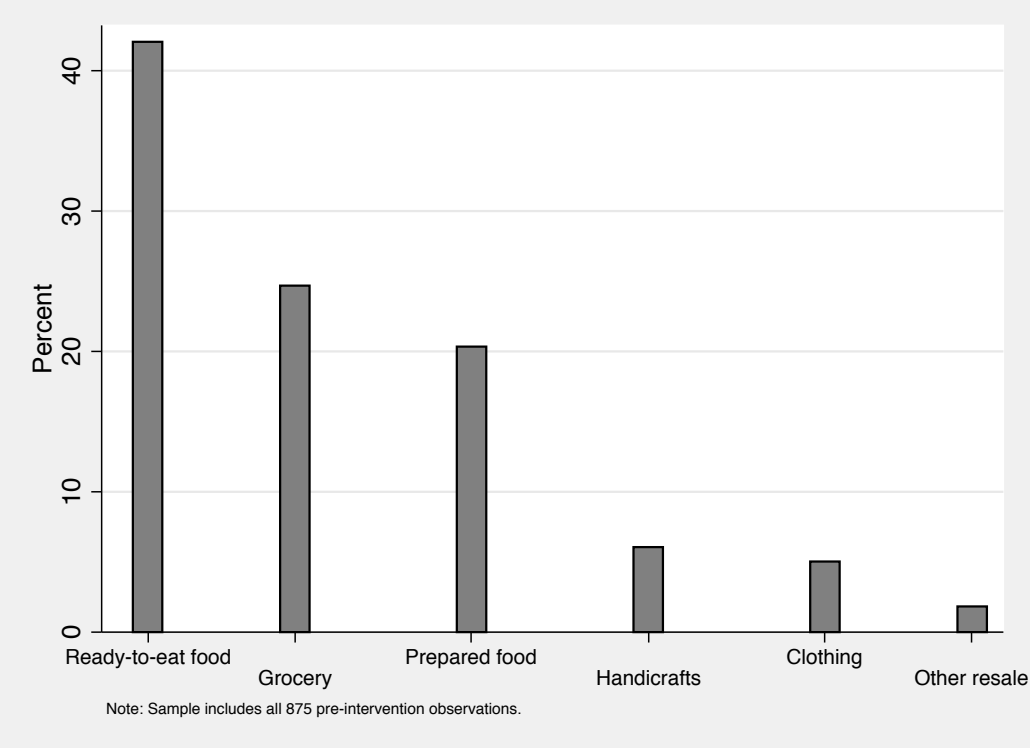

Figure 2: Entrepreneurial choice.

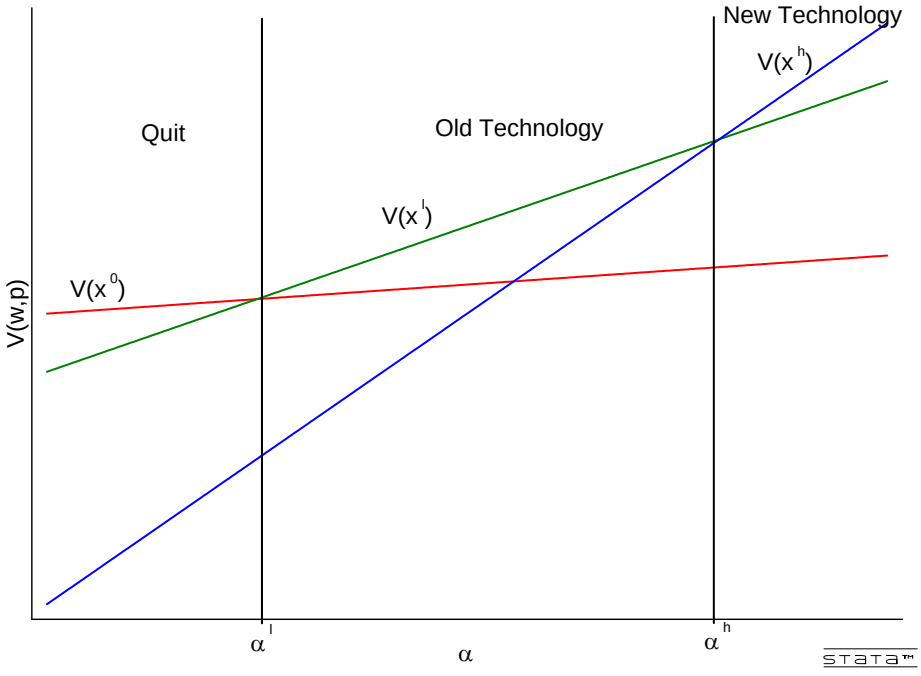

Figure 3: The distribution of baseline (log) daily profits amongst the whole and survived samples of the treatment and control groups

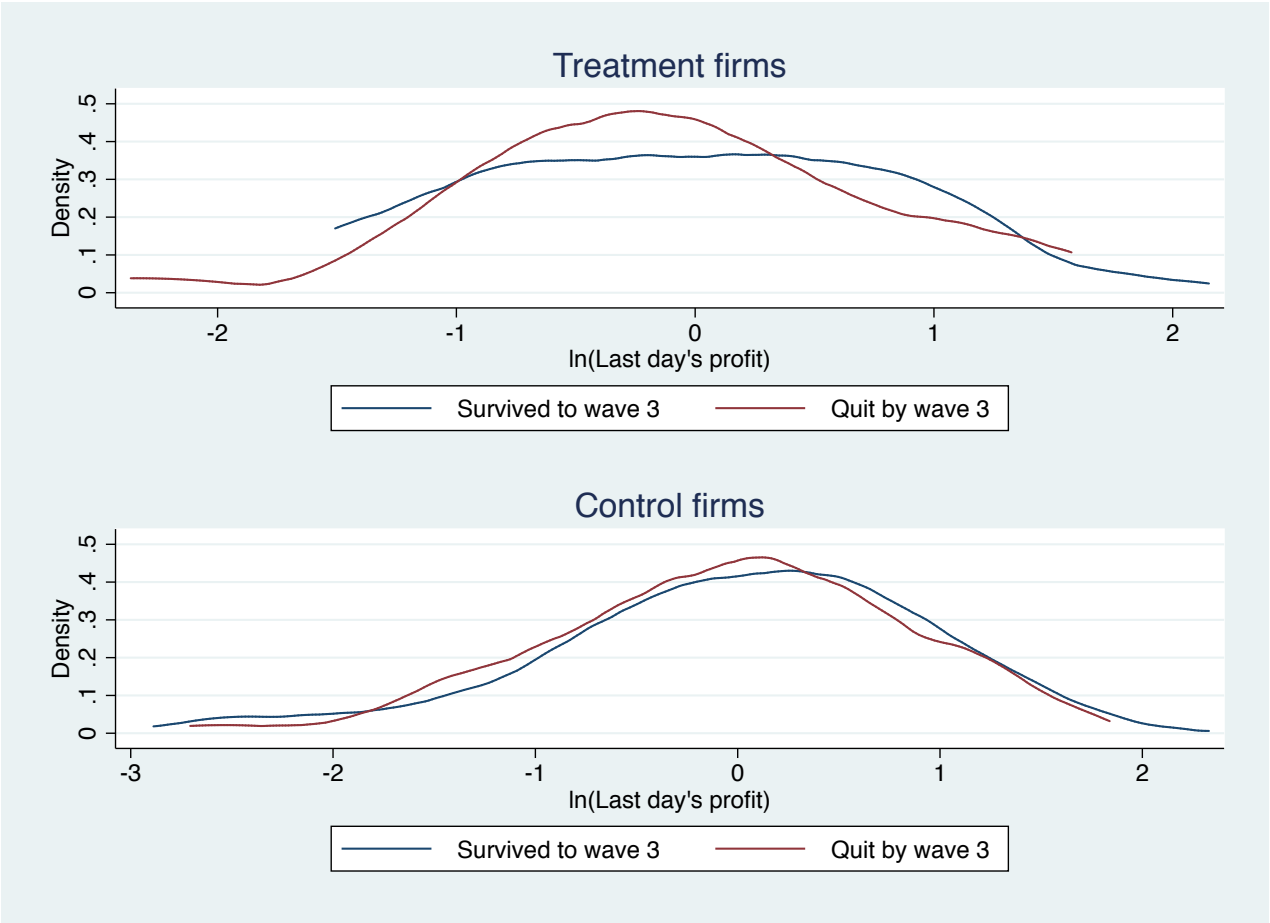

Table 1: Pre-treatment characteristics, by treatment group

	Treatment Villages		Control Villages	(1)=(2)	(1)=(3)	(2)=(3)	Obs.
	Firms offered treatment	Control firms	All Firms	p-value	p-value	p-value	
	(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	875				

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		Survey waves	
	Mean	s.e.	Mean	s.e.	Mean	s.e.	included	Obs.
Outcome:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Quit her business	0.016	(0.032)	-0.152***	(0.043)	0.168***	(0.032)	1,2,3	1,836
Measures of profit and revenue								
ln(Last day's profit)	0.213*	(0.110)	0.144	(0.123)	0.329***	(0.107)	1,2,3	1,183
ln(Last week's profit)	0.168	(0.110)	0.111	(0.097)	0.238*	(0.124)	1,2,3	1,117
ln(Good specific last day's profit)	0.178	(0.220)	0.103	(0.226)	0.298	(0.230)	1,2	807
ln(Good specific last month's profit)	0.191	(0.219)	0.130	(0.238)	0.343*	(0.193)	1,2	834
Standardized profits	0.200**	(0.077)	0.152*	(0.079)	0.268**	(0.094)	1,2,3	1,322
ln(Last day's revenue)	0.253**	(0.115)	0.164	(0.116)	0.358***	(0.118)	1,2,3	1,357
ln(Last week's revenue)	0.213*	(0.110)	0.151	(0.120)	0.319**	(0.110)	1,2,3	1,258
ln(Good specific last day's revenue)	0.248	(0.155)	0.156	(0.139)	0.349*	(0.166)	1,2	1,071
ln(Good specific last month's revenue)	0.400*	(0.190)	0.316	(0.183)	0.503**	(0.177)	1,2	1,028
Standardized revenue	0.232***	(0.067)	0.187**	(0.071)	0.297***	(0.071)	1,2,3	1,421
Other business outcomes								
ln(# clients last day)	0.220*	(0.121)	0.143	(0.120)	0.335**	(0.121)	1,2,3	1,312
ln(Household income, daily)	0.194*	(0.109)	0.138	(0.106)	0.314**	(0.122)	1,2	1,080
ln(# goods for sale)	0.204**	(0.089)	0.108	(0.086)	0.469***	(0.097)	1,2	1,145
ln(Mean unit cost)	-0.273*	(0.148)	-0.292*	(0.152)	-0.212	(0.142)	1,2	907
ln(Mean unit price)	-0.021	(0.085)	-0.053	(0.090)	0.047	(0.097)	1,2	1,139
% correct on business practices exercise	0.051	(0.063)	0.008	(0.062)	0.146**	(0.059)	1,2,3	1,210
Uses formal accounting methods	0.048*	(0.023)	-0.190***	(0.034)	0.262***	(0.060)	1,2,3	1,432
Hours worked per week by owner	2.686	(2.639)	-1.107	(2.800)	3.935	(2.724)	1,2,3	1,411
Hours worked per week by employees	0.756	(3.833)	-3.473	(3.652)	14.915**	(5.344)	1,2	1,143
Number of employees	0.091	(0.108)	-0.002	(0.109)	0.986***	(0.154)	1,2,3	1,419
Registered with the government	0.087***	(0.029)	-0.155***	(0.044)	0.298***	(0.048)	1,2,3	1,399

Notes: Samples include firms offered treatment in treatment villages and all firms in control villages. Coefficients are estimated by Equation 8, 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: The effects of business training by wave

	ITT wave 2	(s.e.)	ITT wave 3	(s.e.)	(1)=(2) p-value	Obs.
Outcome:	(1)	(2)	(3)	(4)	(5)	(6)
Quit her business	0.013	(0.806)	0.019	(0.739)	0.950	1,836
Measures of profit and revenue						
ln(Last day's profit)	0.216*	(0.057)	0.208	(0.345)	0.971	1,183
ln(Last week's profit)	0.181	(0.213)	0.137	(0.510)	0.867	1,117
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
ln(Last week's revenue)	0.160	(0.141)	0.319	(0.172)	0.500	1,258
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 4: Effects on goods that were dropped across waves, kept across waves, and added post-intervention

Outcome:	*Goods that were:*	ITT (1)	(s.e.) (2)	Obs. (3)
ln(Last day's profit)	Dropped	0.026	(0.182)	464
	Kept	0.253	(0.280)	480
	Added	-0.180	(0.299)	119
ln(Last month's profit)	Dropped	-0.127	(0.141)	447
	Kept	0.220	(0.342)	467
	Added	-0.074	(0.158)	133
Standardized profit	Dropped	-0.040	(0.106)	489
	Kept	0.139	(0.176)	511
	Added	-0.080	(0.119)	138
ln(Last day's revenue)	Dropped	-0.109	(0.172)	495
	Kept	0.085	(0.199)	667
	Added	0.149	(0.162)	294
ln(Last month's revenue)	Dropped	-0.179	(0.143)	472
	Kept	0.169	(0.323)	650
	Added	0.298*	(0.166)	300
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
ln(Mean unit price)	Dropped	0.065	(0.089)	536
	Kept	0.010	(0.057)	732
	Added	-0.019	(0.124)	327

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 5: The indirect effects of business training

	Indirect Treatment Effect		Survey	
	Mean	s.e.	waves	Obs.
Outcome:	(1)	(2)	(7)	(8)
Quit her business	-0.018	(0.039)	1,2,3	1,907
Measures of profit and revenue				
ln(Last day's profit)	-0.119	(0.119)	1,2,3	1,250
ln(Last week's profit)	-0.083	(0.132)	1,2,3	1,182
ln(Good specific last day's profit)	-0.038	(0.263)	1,2	849
ln(Good specific last month's profit)	-0.119	(0.145)	1,2	874
Standardized profits	-0.034	(0.099)	1,2,3	1,388
ln(Last day's revenue)	0.053	(0.093)	1,2,3	1,430
ln(Last week's revenue)	-0.013	(0.134)	1,2,3	1,328
ln(Good specific last day's revenue)	0.126	(0.083)	1,2	1,113
ln(Good specific last month's revenue)	0.272	(0.221)	1,2	1,073
Standardized revenue	0.096	(0.072)	1,2,3	1,491
Other business outcomes				
ln(# clients last day)	0.074	(0.134)	1,2,3	1,371
ln(Household income, daily)	0.009	(0.099)	1,2	1,128
ln(# goods for sale)	0.020	(0.060)	1,2	1,196
ln(Mean unit cost)	0.145	(0.175)	1,2	957
ln(Mean unit price)	0.106	(0.091)	1,2	1,190
% correct on business practices exercise	0.001	(0.055)	1,2,3	1,239
Uses formal accounting methods	0.057***	(0.019)	1,2,3	1,501
Hours worked per week by owner	3.907*	(1.870)	1,2,3	1,479
Hours worked per week by employees	2.224	(3.048)	1,2	1,194
Number of employees	0.016	(0.065)	1,2,3	1,485
Registered with the government	-0.025	(0.032)	1,2,3	1,472

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

Table 6: Heterogeneous treatment effects by pre-intervention profits

Sample =	Above median of baseline profits			Below median of baseline profits			(1)=(4) p-value
	ITT	(s.e)	Obs.	ITT	(s.e)	Obs.	
Outcome:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Quit her business	0.014	(0.029)	876	-0.016	(0.058)	912	0.700
Measures of profit and revenue							
ln(Last day's profit)	0.281*	(0.150)	609	0.042	(0.154)	561	0.240
ln(Last week's profit)	0.263	(0.203)	572	-0.070	(0.135)	532	0.223
ln(Good specific last day's profit)	0.321*	(0.163)	408	-0.003	(0.388)	394	0.379
ln(Good specific last month's profit)	0.148	(0.242)	426	0.068	(0.293)	404	0.720
Standardized profits	0.236**	(0.107)	669	0.057	(0.099)	640	0.132
ln(Last day's revenue)	0.338**	(0.131)	680	0.113	(0.188)	653	0.334
ln(Last week's revenue)	0.254	(0.168)	637	0.044	(0.158)	600	0.296
ln(Good specific last day's revenue)	0.203	(0.170)	530	0.056	(0.168)	522	0.325
ln(Good specific last month's revenue)	0.402*	(0.214)	500	0.177	(0.254)	509	0.453
Standardized revenue	0.287***	(0.082)	703	0.107	(0.105)	688	0.187
Other business outcomes							
ln(# clients last day)	0.332**	(0.145)	646	0.150	(0.127)	644	0.272
ln(Household income, daily)	0.247	(0.182)	537	0.022	(0.162)	528	0.308
ln(# goods for sale)	0.193**	(0.084)	562	0.122	(0.094)	556	0.364
ln(Mean unit cost)	-0.151	(0.190)	459	-0.270	(0.239)	434	0.713
ln(Mean unit price)	0.090	(0.094)	557	-0.153	(0.116)	555	0.065*
% correct on business practices exercise	0.058	(0.075)	583	0.035	(0.061)	598	0.468
Uses formal accounting methods	0.068*	(0.037)	707	0.042	(0.028)	692	0.581
Hours worked per week by owner	4.635	(3.409)	697	-0.283	(2.963)	682	0.087*
Hours worked per week by employees	6.786	(6.136)	565	-5.126*	(2.906)	552	0.046**
Number of employees	0.216	(0.180)	701	-0.044	(0.099)	685	0.106
Registered with the government	0.112**	(0.050)	690	0.082	(0.051)	678	0.606

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 F-tests of the equality of means 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	N
	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)		
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	N
	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)		
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: Robustness to imputing zeros for business outcomes of firms who quit or attrited.

	Intent to Treat Effect (ITT)	(s.e.)	Survey waves included	Obs.
Outcome (imputed values for quitters and attriters):	(1)	(2)	(3)	(4)
Measures of profit and revenue				
ln(Last day's profit)	0.107	(0.107)	1,2,3	1,637
ln(Last week's profit)	-0.023	(0.196)	1,2,3	1,577
ln(Good specific last day's profit)	0.168	(0.163)	1,2	1,216
ln(Good specific last month's profit)	-0.032	(0.152)	1,2	1,261
Standardized profits	0.129	(0.074)	1,2,3	1,713
ln(Last day's revenue)	0.097	(0.125)	1,2,3	1,779
ln(Last week's revenue)	0.115	(0.196)	1,2,3	1,714
ln(Good specific last day's revenue)	0.144	(0.165)	1,2	1,499
ln(Good specific last month's revenue)	0.340*	(0.188)	1,2	1,429
Standardized revenue	0.151*	(0.077)	1,2,3	1,812
Other business outcomes				
ln(# clients last day)	0.093	(0.115)	1,2,3	1,728
ln(Household income, daily)	0.204	(0.233)	1,2	1,482
ln(# goods for sale)	0.159	(0.099)	1,2	1,538
Uses formal accounting methods	0.040**	(0.016)	1,2,3	1,825
Hours worked per week by owner	2.718	(2.762)	1,2,3	1,804
Hours worked per week by employees	0.711	(2.582)	1,2,3	1,536
Number of employees	0.092	(0.074)	1,2,3	1,812
Registered with the government	0.091***	(0.030)	1,2,3	1,790

Notes: Samples include firms offered treatment in treatment villages and all firms in control villages. Outcomes for individuals who quit or attrited are imputed with 0 for linear outcomes, ln(0.1) for clients served, and ln(1) for other logarithmic outcomes. 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